DUAL-TASK STUDIES ON THE CAPACITY LIMITS OF PERCEPTUAL ATTENTION AND WORKING MEMORY

By

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Dissertation
Submitted to the Faculty of the
Graduate School of Vanderbilt University
In partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY
In

Psychology

May, 2010

Nashville, Tennessee

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ACKNOWLEDGEMENTS

I owe an immense debt, academically, intellectually, and socially to all the members of the Marois lab that I've been fortunate enough to know during my time in the lab. With heroic patience, Rene Marois has helped me learn and correct my weaknesses. I will be forever grateful for his support. I wish to also specifically name the support of Jay Todd, Paul Dux, Michael Tombu and Chris Asplund. Whenever I had a question, they were always willing to listen, and whenever I got stuck, they always had suggestions. I thank my Dissertation committee, Dan Levin, Gordon Logan, and Amy Needham for their valuable advice that helped shape my research. I appreciate the help of Jurnell Cockhren for assistance in programming the gabor tracking task. I would also like to acknowledge the help of Janice Louie, Tristan Watkins, Senna Munnikhuysen, Tara Usakoski, Chris Suciu, and Xin Lu. They were excellent undergraduates and I enjoyed working with all of them.

Of course, graduate school is more than a period of academic growth, it is also a large chuck of one's social life. I owe the biggest thanks to those individuals that have kept me sane and provided happiness. I love my family and it has been difficult being separated from them for so long. Most importantly, I thank Olivia Cheung for things too numerous to enumerate here. She has taught me the importance of paying attention to the small things that happen every day.

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Preface

As we interact with our environment, we feel that we construct veridical and durable representations of our surroundings. However, despite the fact that the human brain contains billions of neurons and trillions of synaptic connections, there are striking limits on perceptual and working memory capacity—we are only consciously aware of a subset of sensory information and can only maintain a handful of fragile representations over short intervals. Indeed, it has been suggested that working memory is constrained by a single, amodal attentional capacity (Cowan, 2006). What is the source of limits in attention and working memory: Do they stem from a single, limited-capacity process, or are limits due to the interaction of multiple capacity-limited components? One important test of the specificity or generality of capacity limits is whether interference between two attention or two working memory tasks occurs irrespective of stimulus content. Here I use a dual-task methodology to measure the interference between auditory and visual stimuli. The use of auditory and visual stimuli provides a strong test as to whether the capacity of attention and working memory is restricted in a content-specific fashion. The perception of auditory and visual stimuli relies on distinct brain regions processing (DeYoe & Van Essen, 1988; Fritz, Elhilali, David, Shamma, 2007). Additionally, only minimal cross-modal interference is observed in some task combinations (Alais, Morrone, & Burr, 2006; Cocchini, Logie, Della Sala, MacPherson, & Baddeley).

This dissertation begins with a description and definition of capacity limits within perceptual attention and working memory. The motivation and importance of the research question is discussed. I then describe the dual-task methodology, which can be used to assess whether two tasks tap into an undifferentiated capacity, or tap into (partially) dissociable processes. In the final section of Chapter I, I discuss how this dual-task methodology has been previously applied to show that visual attention and visual working memory have partially dissociable capacity limits. The second chapter of the dissertation examines whether there are domain-general or domain-specific limits in maintaining perceptual representations in working memory. The third chapter asks whether our ability to track perceptual information is dependent on the modality of the attended information. The dissertation concludes by discussing the implications of the findings and how they may inform theoretical models of attention and working memory.

CHAPTER I

CAPACITY LIMITS IN ATTENTION AND WORKING MEMORY

Description of capacity limits in perceptual attention and working memory

Perceptual attention

In some theories attention is considered to be a limited-capacity resource capable of processing stimuli (e.g. Kahneman, 1973; Norman & Bobrow, 1975), while others describe attention as the selective component in our information processing architecture (e.g. Broadbent, 1958). Commonly, both metaphors of attention stress the need to select a subset of information for access to limited-capacity perceptual processing. At which stage of processing does selection occur? There is evidence that attention can affect early perceptual processing (Cherry, 1953; Mangun & Hillyard, 1991) or later processing stages (Osman & Moore, 1993). The strong support for both early and late selection has led to the proposal that there may be more than one stage of attentional selection (Lavie, Hirst, de Fockert, & Viding, 2004; Luck & Vecera, 2002; Posner & Boies, 1971; Posner & Peterson, 1990). As an example, one influential theory argues for separate alerting, orienting, and executive attention networks (Posner & Boies, 1971; Posner & Peterson, 1990). The alerting network controls the general state of responsiveness to sensory stimulation, the orienting network selects a subset of sensory information for limited-capacity processing, and executive attention selects among post-sensory representations to resolve conflict among competing responses. The focus of this dissertation is on attentional effects within the orienting network, here defined as perceptual attention (Johnston, McCann, Remington, 1995; Luck & Vecera, 2002; Pashler 1989, 1991, 1993; Vogel, Woodman, & Luck, 2005).

Several paradigms reveal limits in perceptual attention and demonstrate processing benefits for selected stimuli at the expense of unselected information. Spatial cuing paradigms show that performance is significantly improved if participants receive advance information about the location of a subsequent target (Downing & Pinker, 1985; Posner, 1980). Similarly, when two visual objects are presented concurrently under time pressure, participants can only report properties of one stimulus accurately (Bonnel & Miller, 1994; Norman & Bobrow, 1975; Sperling & Dosher, 1986). Research also reveals limits in selectivity. Divided attention paradigms such as dichotic listening and multiple-object tracking (MOT) paradigms reveal that participants can selectively attend to and process one or more task-relevant stimuli and ignore task-irrelevant items (Cherry, 1953; Moray, 1959; Pylyshyn & Storm, 1988; Sears & Pylyshyn, 2000). However, there are limits to the maximum number of stimuli that can be simultaneously attended. For example, participants have difficulty tracking more than four targets in a MOT task (Pylyshyn & Storm, 1988) and following more than one auditory passage in dichotic listening tasks (Cherry, 1953; Conway, Cowan, & Bunting, 2001).

Working memory

Whenever we have to jot down a phone number, solve a complex math problem, or engage in lengthy conversations, we rely on our capability to store information in a highly active and accessible state where it is available for other cognitive processes (Baddeley & Hitch, 1974). This ability, termed working memory (WM), is a fundamental aspect of cognition and is linked to other mental faculties such as attentional control, language acquisition, problem solving, task switching, and general intelligence (Baddeley & Hitch, 1974; Baddeley, 1986; Duncan, 1995; Engle, Tuholski, Laughlin, & Conway, 1999; Hasher & Zacks, 1988; Kane & Engle, 1997, 2002; Roberts, Hager, & Heron, 1994; Unsworth, Schrock, & Engle, 2004). Yet, while WM is critically important in our day-to-day lives, its capacity is surprisingly limited. People struggle to store more than a handful of representations, and often fail to detect rather salient changes between two display separated by a short temporal interval (Cowan, 2001; Cowan, Zhijian, & Rouder, 2004; Henderson, 1972; Hollingworth, 2004; Irwin, 1992; Luck & Vogel, 1997; Miller, 1956; Pashler, 1988; Rensink, 2000, 2002).

A common task used to assess the limits of WM storage capacity is to give participants a set of stimuli to remember (a sample display) over a short temporal interval. The number of representations that can be stored is measured by the set size of the sample and accuracy at determining whether a subsequent probe display is the same as or different from the sample (Cowan, 2001; Pashler, 1988). Miller (1956) famously suggested that 7 +/- 2 items may be stored in WM. This estimate has been revised by many researchers to suggest that the capacity of WM is 4 +/- 1 (Cowan, 2001; Cowan, Zhijian, & Rouder, 2004; Henderson, 1972; Luck & Vogel, 1997). However, WM performance is not simply limited by the number of items that need to be stored, there are also limits in the fidelity of representations (Awh, Barton, & Vogel, 2007; Magnussen & Greenlee, 1992, 1999; Wilken & Ma, 2004; Zhang & Luck, 2008), and WM task performance may depend on the information load of the stimuli that have to be stored (Alvarez & Cavanagh, 2004; Fougnie, Asplund, & Marois, submitted).

Nature of capacity limits: A single source or separate sources?

One influential proposal suggests that a single capacity-limited process underlies the striking limits in both attention and working memory (Cowan, 1995, 2001, 2006; Duncan 1994). Support for this proposal includes remarkably similar capacity estimates across attention and working memory tasks, particularly for visual stimuli (Cowan, 2001; Rensink 2002). However, interference between two visual working memory tasks has been found to be more severe than between a visual working memory and visual attention task (Fougnie & Marois, 2006). Based on this finding, I suggested that attention and working memory were constrained by multiple sources of capacity. Evidence against a single, amodal source of capacity that underlies attention and working memory is also found in dual-task studies that require participants to attend to or maintain two sets of stimuli from different modalities. Such studies often find little or no cost between modalities (e.g. Alais, et al., 2006; Duncan, Martens, & Ward, 1997; Soto-Faraco & Spence, 2002; Treisman & Davies, 1973; Cocchini, et al., 2002; Scarborough, 1972). However, as described in the following sections, such findings are not universal (e.g. Saults & Cowan, 2007) and do not sufficiently rule out potential confounds and alternate interpretations.

The issue of domain-general or domain-specific sources for working memory has been extensively studied with a typical finding being small but significant interference between concurrent auditory and visual WM loads (e.g. Cocchini et al., 2002; Fougnie & Marois, 2006; Scarborough, 1972), consistent with partially overlapping sources of WM capacity across sensory domains, but also consistent with additional content-specific sources of capacity. However, there is one finding suggesting that a dual-task paradigm can show evidence of complete capacity sharing between the two modalities (Saults & Cowan, 2007). Thus, there is no consensus on whether WM capacity is differentiated by modality. This is particularly true considering that previous findings of dual-task costs might also reflect non-mnemonic sources of interference such as increases in executive load, overlap in representational content (such as overlap in propositional coding between tasks), or perceptual interference (Cocchini, et al., 2002; Postle, D'esposito, & Corkin, 2005) rather than competition for a domain-general WM system.

This issue of domain-general or domain-specific capacity limits has also been extensively studied in perceptual attention tasks. Previous experimental findings generally show no interference between auditory and visual attention tasks (Alais, et al., 2006; Duncan, et al., 1997; Larsen, McIlhadda, Baert, & Bundesen, 2003; Soto-Faraco & Spence, 2002). In contrast, neurophysiological evidence shows that attending to one modality leads to reduced neural activity for stimuli in another modality (Johnson & Zatorre, 2005, 2006). One possible explanation for this apparent discrepancy in findings is that behavioral costs were not shown because previous studies may have allowed participants to time-share their attention across modalities. These studies used target detection tasks where the target-defining stimulus features were supplied in advance of the target. In those tasks, participants could perform well in the absence of constant attention if they set up target templates (Duncan & Humphries, 1989) that would act to summon attention in a stimulus-driven fashion whenever a task-relevant target appeared. This account is speculative, but raises the possibility that costs across modalities will occur in tasks that require participants to attend to sources of information where a pre-specified target is not supplied. The current studies required participants to constantly track feature properties of target stimuli as the features continuously and unpredictably changed over a sustained period of time. Cross-modal costs under such conditions would place limits on the conditions under which auditory and visual attention tasks don't interfere.

Summary of studies and predictions

In order to address the unitary or divisible nature of our capacity-limited cognitive architecture, this dissertation examines the degree to which performance limits in perceptual attention and working memory are differentiated by modality. The current work addresses weaknesses in past studies that prevent strong conclusions from being drawn.

Chapter 2 asks to what degree auditory and visual WM interfere with each other if the WM tasks are designed to minimize interference from non-mnemonic sources such as executive processing (Cocchini, et al., 2002; Postle, et al., 2005). In Chapter 3, I ask whether there is a common attentional system shared between auditory and visual attention networks using tasks that require participants to continuously track target information over a long duration.

What predictions can be made about the degree of interference across modalities in attention and working memory tasks? It is known that in some dual-task conditions it is possible to observe large costs between two tasks in different modalities. For example, costs are observed between cell-phone conversations and visual navigation tasks (Strayer, Drews, & Johnston, 2003; Strayer & Johnston, 2001). One possibility is that competition between auditory and visual tasks will depend on whether those tasks load on early or late stages of processing. Specifically, if two tasks compete for perceptual processing no costs are expected, as it is proposed that there are individual perceptual processing capacities for auditory and visual stimuli (Alais, et al., 2006; Duncan, et al., 1997; Larsen et al., 2003). However, if the tasks compete at later response stages, such as response selection, it is suggested that auditory and visual stimuli compete for a domain-general capacity (Jolicoeur, 1999b; Jolicoeur, & Dell'Acqua, 1998; 1999; Pashler, 1994; but see Mohr & Linden, 2005). Importantly, this predicts that dual-task costs will not be found between perceptual tracking tasks, even if those tasks require the constant tracking of stimulus features, as these tasks do not place concurrent demands on response selection. However, since there is no consensus on whether consolidation in memory draws on the same stage of attention involved in response selection (Baddeley & Logie, 1999; Blake & Fox, 1969; Dell'Acqua & Jolicoeur, 2000; Giesbrecht, Dixon, & Kingstone, 2001; Jolicoeur, & Dell'Acqua, 1998; 1999; Jolicoeur, 1999b; Pashler, 1991, 1993; Posner & Boies, 1971), few predictions can be made from this theory on dual-task costs in WM.

Alternatively, dual-task costs between auditory and visual stimuli may depend on whether the two tasks require the processing, updating, or manipulation of information, or whether the tasks are 'offline' in the sense that they require storage and not processing (Duff & Logie, 2001; McElree, 2001; Oberauer, 2002; Oberauer, Demmrich, Mayr, & Kliegl, 2001). Such frameworks suggest that representations can be maintained in WM through self-sustaining activity in domain-specific stores (even in the absence of top-down signals) (Hopfield, 1982; Washburn & Astur, 1998), whereas attention is considered to be a limited-capacity amodal resource (Baddeley & Logie, 1999; Duff & Logie, 2001; Oberauer & Kliegl, 2006). It may seem that failures to observe dual-task costs between auditory and visual attention tasks presents a problem for the processing versus storage distinction. However, it is possible that such failures occurred only because in such tasks attention can be shared between modalities. The attention tasks employed in the present dissertation, which require constant tracking of feature values, provide a stronger test for such frameworks. The processing versus storage distinction also predicts that dual-task costs between auditory and visual WM tasks will decrease as the tasks require less executive control to perform in tandem (Cocchini, et al., 2002).

Dual-task methodology

Before discussing individual experiments, it will be helpful to briefly outline the basic dual-task methodology used in the studies described here. In this paradigm, two tasks are to be concurrently performed separately or concurrently and the load of each task is varied. Dual-task costs can be measured by comparing single- to dual-task performance, or by the decrease in performance as task load increases. These costs can be compared to task combinations in which the tasks are known to tap into the same capacity limit (e.g. two visual WM tasks). Thus, the magnitude of interference relative to other task combinations provides a powerful tool to examine the overlap in processing between tasks. However, one must be careful that the tasks used are well equated for difficulty (Lee, Koch, & Braun, 1999) and that interference does not occur at stages of processing ancillary to the processing stage of interest, such as interference during encoding into or retrieval from WM (Cowan & Morey, 2007; Eng, Chen, & Jiang, 2005; Sligte, Scholte, & Lamme, 2008). The current experiments were designed to minimize these concerns and, where possible, address them with additional experiments or analyses.

Application of dual-task methodology: Relationship between attention and WM

To demonstrate the utility of the dual-task paradigm, I will describe its application to study whether attention and WM tap into the same capacity-limited process (Fougnie & Marois, 2006). Perceptual attention involves the selective maintenance of sensory representations for perceptual processing, while WM involves the selection of stored representations for limited WM capacity. Since both attention and WM involve the selection of task-relevant information at the expense of other information, it is prudent to consider the similarity of these two constructs. Indeed, several researchers have argued that attention is the capacity-limited process that constrains visual working memory (VWM) capacity (Cowan, 2001; Rensink, 2000, 2002). Before describing the dual-task study, I briefly review the evidence for a common attention and WM process below, including: 1) evidence that attention and WM have strikingly similar capacity limits, and 2) dual-task interference between VWM and attention.

Several researchers have noted the similarity in capacity limits for visual attention and VWM. Multiple object tracking tasks reveal that participants can only simultaneously attend to 3-4 stimuli at a time (Oksama & Hyönä, 2004; Pylyshyn & Storm, 1988; Sears & Pylyshyn, 2000). Similarly, estimates of the number of objects that can be stored in VWM also are in the range of 3-4 items (Pashler, 1988; Sperling, 1960; Vogel, Woodman, & Luck, 2001). This has led to the proposal that attention and VWM share the same capacity-limited processes (Cowan 2001; Rensink, 2002). However, the similarity in capacity limits may be mere coincidence: given that capacity for attentional tracking and VWM tasks depends on various stimulus parameters such as the speed of the objects that need to be tracked (Alvarez & Franconeri, 2007) or the complexity of the items that need to be remembered (Alvarez & Cavanagh, 2004), it is not difficult for capacity estimates of either attentional or VWM tasks to depart from the typically observed range of 3-4.

Dual-task experiments that combine attention and WM tasks can reveal costs when these two tasks are combined (Awh, Jonides, & Reuter-Lorenz, 1998; Oh & Kim, 2004; Woodman & Luck, 2004). This has been provided as evidence for a shared capacity limit. However, additive dual-task costs between WM and attention are not always observed (Logan, 1978, 1979; Woodman, Vogel, & Luck, 2001) and may depend on the overlap of features across tasks. Specifically, dual-task costs between attention and VWM occur when visual search tasks are paired with spatial WM tasks (Oh & Kim, 2004; Woodman & Luck, 2004) or when object-based attention tasks are paired with VWM for object features (Matsukura & Vecera, 2009), but not when visual search is paired with object or verbal WM (Logan, 1978; Woodman et al., 2001).

Thus, previous evidence is insufficient to determine whether interference between attention and VWM tasks reflects capacity sharing and leaves unanswered whether previous findings of dual-task costs signify a shared capacity-limit or merely partial overlap in processing across tasks. To address these questions, I recently conducted three experiments to measure dual-task costs between: visual attention and VWM, VWM and VWM, and auditory working memory (AWM) and VWM (Fougnie & Marois, 2006). The results showed that VWM and attention interfered with each other and showed increased interference as task load increased. However, the dual-task costs were less than when two tasks that tap into the same capacity-limited processes (VWM and VWM) were paired, but were greater than were found for a task pairing that typically shows little dual-task costs (VWM and AWM). Additional control experiments attempted to rule out the possibility that differences in dual-task costs were due to differences in featural overlap between tasks (Fougnie & Marois, 2006) or to interference between tasks at encoding or retrieval stages (Fougnie & Marois, 2009B). The results suggested that VWM capacity is set by the interaction of visuospatial attentional, central amodal, and VWM-specific sources of processing.

CHAPTER II

ARE THERE SEPARATE WM STORES FOR AUDITORY AND SPATIAL STIMULI?

Motivation for dual-task WM studies

According to the influential multi-component model of Baddeley and colleagues (Baddeley, 1986; Baddeley & Hitch, 1974; Baddeley & Logie, 1999) information is maintained in WM by slave systems specialized for a specific modality. According to Cowan's embedded processes model (Cowan, 1988; 1995), the capacity of WM is set by a single capacity-limited system that holds WM information in the focus of attention. A critical distinction between these theories is the degree to which WM capacity is domain-general. That is, is there a limited-capacity WM system that acts to store information regardless of the modality or content?

Evidence in dual-task WM studies is generally in support of at least partially dissociable auditory and visual WM systems. In a recent study (Fougnie & Marois, 2006; Expt. 3) we observed dual-task costs between an auditory and visual WM task only at high dual-task load. This is a common finding in the literature. Studies that combine a low AWM (auditory working memory) load with a VWM (visual working memory) load observe no dual-task costs (Luck & Vogel, 1997; Vogel et al., 2001), but when the AWM load is high the results show small, but significant dual-task costs (Cocchini, et al., 2002; Fougnie & Marois, 2006, Morey & Cowan, 2005; Scarborough, 1972). These costs seem to support the contribution of a domain-general store, and further suggest that WM performance has contributions from both amodal and modality-specific sources of capacity (Fougnie & Marois, 2006; but see Saults & Cowan, 2007). The finding of cross-modal interference in WM allows the potential for a neuroimaging experiment to locate the neural correlates of a domain-general storage system. Similar techniques have been used to identify the correlates of domain-specific storage of visual (Todd & Marois, 2004) or auditory (Zatorre, Evans, & Meyer, 1994) stimuli.

However, before accepting that dual-task costs between auditory and visual WM tasks provides evidence of domain-general store, it is important to consider potential alternative explanations. Can these costs be explained by the increased executive load of coordinating two simultaneous tasks in the dual-task condition (Cocchini, et al., 2002) or the use of tasks that overlap in representational content such as visual displays that are recoded in propositional or verbal forms (Postle et al., 2005)? To address this, I designed WM tasks to minimize overlap in non-mnemonic processes and to be as distinct as possible in the task-relevant information. In order to provide the strongest evidence that the interference between auditory and visual WM tasks represents competition for a domain-general storage system that is capable of storing any type of information, the auditory and visual WM task should be as distinct as possible. Previous studies typically had participants encode and store object properties of auditory and/or visual objects. A stronger test of a central WM store is to pair maintenance of auditory object features with maintenance of visuospatial locations, as there is evidence that our WM system may have distinct capacities and neural correlates for object and spatial features (Postle et al., 2005; Smith, Jonides, Koeppe, Awh, Schumacher, & Minoshima, 1995; Tresch, Sinnamon, & Seamon, 1993; Ungerleider, Courtney, & Haxby, 1998; Ventre-Dominey, Bailly, Lavenne, LeBars, Mollion, Costes, & Dominey, 2005). Compounding this issue, many previous studies used visual WM tasks with object properties that could have been easily verbalized or encoded as propositions (Postle, et al., 2005). For example, a participant might encode a propositional representation of the green square in Figure 5 as “green square on the right”. It is possible that previous evidence for dual-task costs was due to interference at a representational level, and would therefore not provide strong support for a domain-general storage system.

Second, many of the dual-task WM studies use stimuli that can be *chunked* or combined to form a higher order representation. This is particularly true of spatial WM tasks that require participants to encode the location of a series of dots or squares (e.g. Cocchini et al., 2002). Performance on this task may improve if participants chunk the individual locations into an object shape. Chunking operations load on the modality-independent central executive and could lead to dual-task costs. Finally, costs could also originate from having to perform two tasks concurrently, irrespective of the overlap in their storage capacity (Cocchini et al., 2002). In a dual-task setting, participants will have to encode two displays, make two responses, and prepare for both tasks. Having to coordinate performance for two tasks could load on the central executive, particularly if the task order is unpredictable (De Jong & Sweet, 1994).

To reduce these non-mnemonic sources of interference I used a spatial working memory (SWM) task where the higher order structure formed by the stimuli (a line) was constant across trials and therefore not informative. To further prevent grouping of items, sample stimuli never appeared in adjacent positions, and the probe stimulus either appeared in a matching or an adjacent location (Figure 1), requiring participants to memorize the exact spatial position of sample stimuli. Finally, I presented two samples and required two responses on all trials to minimize differences between single- and dual-task conditions. The task order was constant across trials to minimize changes in preparation across conditions (De Jong & Sweet, 1994). In later studies (Expts. 3-6) I excluded single-task conditions and manipulated task load so that all conditions involved performance of two tasks. Thus, the present studies were designed to minimize the likelihood of observing dual-task costs that are not from competition for a domain-general storage system.

Experiment 1

The first experiment compared the SWM task described above with an AWM task for spoken consonants.

Methods

Participants

Seventeen young adults participated for course credit or monetary reward.

Design

Participants performed single-task AWM (20% of trials), single-task SWM (20% of trials), and dual-task trials (60% of trials) randomly intermixed within four blocks of 60 trials. For each task there were three manipulations of set size. There were 16 trials per condition for each combination of dual-task and single-task load. To keep task order constant across trials I embedded the SWM task within the retention interval of the AWM task.

Participants were given bonus pay of 0-10$ based on their performance in the task. For each single-task trial that participants answered correctly and for each dual-task trial with both responses correct, participant’s bonus pay increased. Participants were instructed to maintain fixation throughout each trial, and to emphasize the two tasks equally. During practice blocks, participants were given response accuracy feedback during the 1000ms inter-trial interval

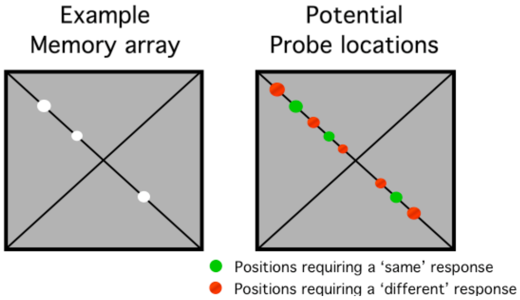

Figure 1: Example SWM display and potential locations for the SWM probe.

SWM Task

The SWM display consisted of 1, 3, or 5 white dots appearing along one of two black diagonal lines that crossed the screen to form an *X* (Figure 1). The use of two diagonal lines also served to create a fixation point at the center of the screen where the lines crossed. The stimuli always appeared on the same line within a block and were presented for 800ms. Whether the stimuli appeared on the line that runs from top-left to bottom-right, or vice-versa changed between blocks. Participants were instructed to memorize the spatial positions indicated by the white dots. A dot could occupy one of 12 possible positions on a line, with the restriction that two dots could not occupy adjacent positions. The spacing of positions was not constant but increased with increased distance from fixation. The two nearest positions were separated by .5°. With each step from fixation the spacing increased by 33% such that the spacing for the farthest adjacent dots was 2.77°. This spacing was shown to produce nearly equivalent WM performance at all positions in a pilot study with eight participants. The size of the dots increased with increased distance from fixation (.15° diameter near fixation to .3° diameter at the point farthest from fixation).

After a 2000ms retention interval the SWM probe appeared. The SWM probe consisted of a single dot presented at the same position as one of the sample dots, or at a position adjacent to a sample dot (50% probability). Participants made one of two key presses with their left hand to indicate whether the probe was the same as or different from the sample dot positions. Participants had 3000ms to respond before the display cleared and an incorrect response was registered. The SWM probe remained onscreen for 3000ms even if participants responded within that interval.

In AWM single-task trials, there were no dot positions to memorize for the SWM task. Instead, the SWM display consisted of block dots at each potential location. Participants were instructed to expect these trials and that their presence meant that they could ignore the SWM task. During the irrelevant SWM probe, participants were instructed to respond by pressing either of the two response keys.

AWM Task

The AWM task consisted of the presentation via headphones of 2, 6, or 10 consonants spoken in a female voice. In pilot testing I found that this set size produced similar levels of performance to that of the SWM task for set sizes 1, 3, and 5, respectively. The stimuli were presented sequentially at a rate of 300ms per consonant. Note that stimulus presentation time ranged from 600ms (set size 2) to 3000ms (set size 10). To control presentation time across set size, the inter-task interval ranged from 600ms (set size 10) to 3000ms (set size 2) for a total of 3600ms from the onset of the AWM sample to the onset of the SWM sample. During the AWM retention interval participants performed the SWM task (5800ms). The AWM probe followed the SWM probe and involved the presentation of a consonant that was the same or different from the auditory sample (50% probability). Participants made one of two key presses with their right hand to indicate whether the probe was the same as or different from the sample. Participants had 3000ms to respond before an incorrect response was registered.

In SWM single-task trials, the auditory sample consisted of the presentation of the vowel *E* to indicate that there was no AWM load. Participants were instructed to expect these trials and to ignore the AWM task during them. During the task-irrelevant AWM probe participants were instructed to respond by pressing either of the two response keys.

Results

To minimize confusion, I reserve the term set size to refer the number of stimuli for the currently analyzed task and the term load for the number of stimuli in the other task. For example, for SWM performance I will discuss the impact of SWM *set size* and AWM *load*. The following analyses are on the capacity (K) data. Note that there were no qualitative differences in the analysis of variance (ANOVA) results between K and change detection accuracy. The advantage of examining K values is that it allows us to determine whether the single-task set sizes tested were sufficiently high to exhaust capacity. The K results for all six experiments are shown in Figure 2. The percent correct results are shown in Figure 3.

Measuring Capacity

Change detection accuracy for the auditory and spatial WM tasks was entered into Cowan's (2001) K formula (K = [hit rate – false alarm rate]N) to measure the number of items stored for each task in each condition (Pashler, 1988; Cowan, 2001; Cowan, Johnson, & Saults, 2005). Single-task K values for intermediate set sizes were higher than for low set sizes in both conditions [paired t-tests; AWM task, *t(16)* = 10.5, *p* < .001; SWM task, *t(16)* = 2.92, *p* = .01] suggesting that the low set size was insufficient to exhaust WM capacity. Importantly, there was no difference in K scores between high and intermediate set sizes, [AWM task, *t(16)* = 1.27, *p* = .22; SWM task, *t(16)* =.94, *p* = .36] suggesting that participant's auditory and spatial WM capacity was exhausted at intermediate and high set sizes. Thus, a failure to find dual-task costs cannot be attributable to insufficient task demands.

SWM performance

K values for the SWM task (Figure 2A top) were analyzed with a 4 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, *F*(2, 48) = 2.72, *p* < .05, no main effect of AWM load, *F*(3, 32) = .93, *p* = .43, and a marginal interaction, *F*(6, 96) = 1.97, *p* = .06.

AWM performance

K values for the AWM task (Figure 2A bottom) were analyzed with a 4 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, *F*(2, 48) = 18.51, *p* < .001, no main effect of SWM load, *F*(3, 32) = .44, *p* = .73, and no interaction, *F*(6, 96) =.76, *p* = .64.

Analysis excluding set size 1

The prior analyses show no main effect of secondary task load on performance. However, one concern is that statistical power may have been reduced by including set size 1 trials. Performance on those trials may have been near ceiling (accuracy in single-task trials with only 1 item averaged across auditory and visual tasks was 92.6%), thus lowering variability. In the following analysis, responses for stimulus displays with only one item were excluded to potentially increase the power of the ANOVA analyses. This analysis showed no effect of AWM load on SWM performance, *F*(3, 32) = 2.1, *p* = .16, and no interaction between SWM set size and AWM load, *F*(3, 32) = 1.36, *p* = .26. Similarly, there was no effect of SWM load an AWM performance, *F*(3, 32) = .52, *p* = .52, and no interaction between AWM set size and SWM load, *F*(3, 32) = .9, *p* = .45. Thus, the current study shows no evidence for cross-modal interference in WM even when the analysis excludes responses for displays of only 1 item.

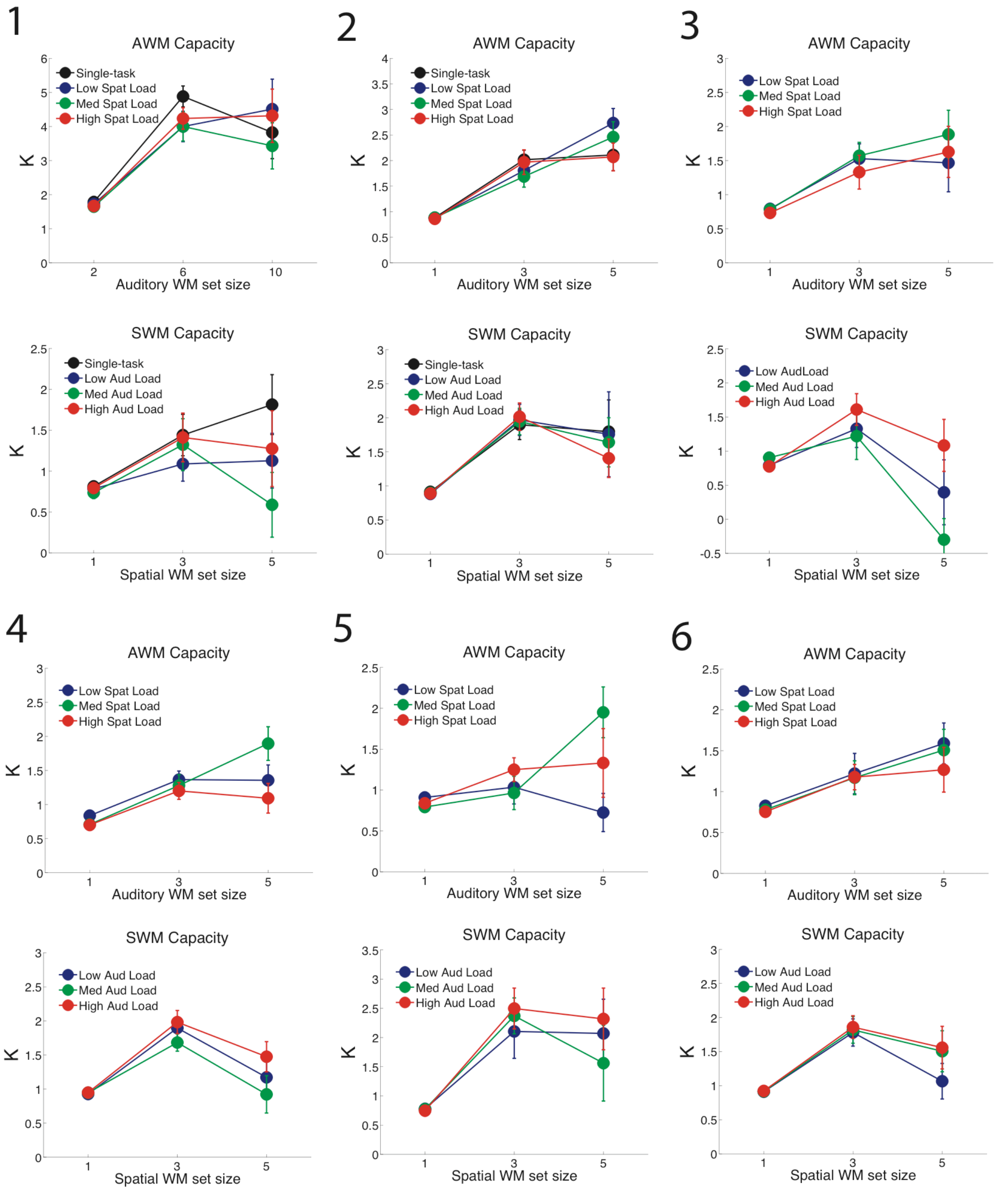

Figure 2: Capacity data (K) for the AWM (above) and SWM (below) tasks for Expts. 1-6 (represented in panels 1-6, respectively) as a function of task set size and secondary task load. Error bars represent between-subject standard error of the mean.

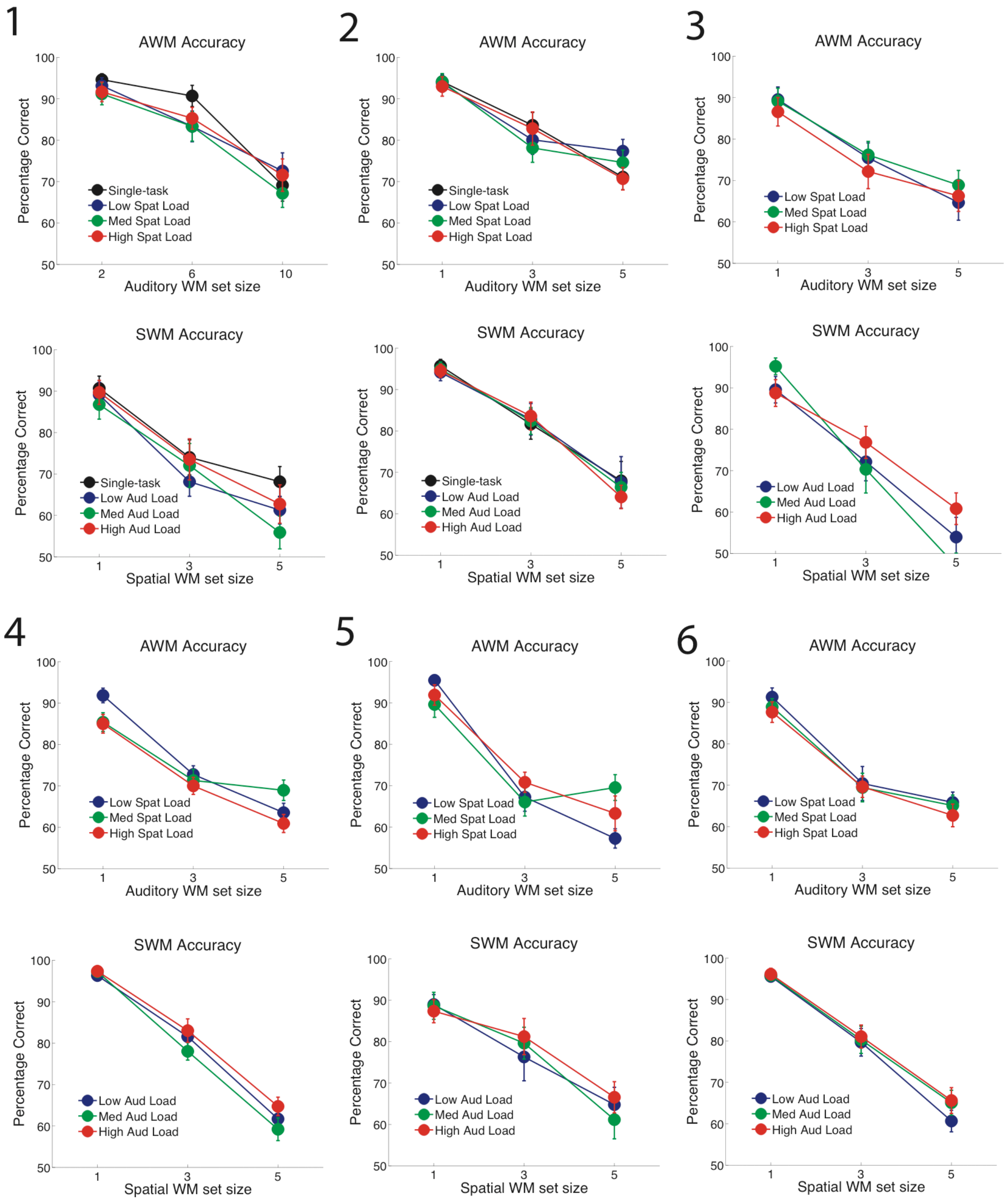

Figure 3: Percent correct for the AWM (above) and SWM (below) tasks for Expts. 1-6 (represented in panels 1-6, respectively) as a function of task set size and secondary task load. Error bars represent between-subject standard error of the mean.

Discussion

For both AWM and SWM, K values were strongly affected by task set size, suggesting that the tasks were sufficiently demanding to exhaust processing capacity. However, there was no evidence for dual-task costs for either task. Even at the highest secondary task load WM performance is similar to that in the single-task condition. These results differ from previous studies that found significant costs when auditory and visual tasks are paired at high load (Fougnie & Marois, 2006; Cocchini et al., 2002; Scarborough, 1972). However, before I can conclude that the present study is evidence against the presence of a domain-general WM store, several concerns need to be addressed:

1) Will these findings generalize for different auditory WM stimuli?
2) Will the lack of interference effects occur even if an additional task ties up participants' articulatory loop?
3) Can these results be explained by proposing that long lasting sensory traces of the stimuli eliminate the need for storage in WM (Saults & Cowan, 2007)?

The following studies address these concerns and also show that the lack of costs between auditory and spatial stimuli is highly replicable in this paradigm.

Experiment 2

A possible explanation for the lack of dual-task costs could be that the use of a verbal WM load may allow participants to rehearse the AWM stimuli using the articulatory loop proposed by Baddeley (Baddeley, 1986). In Expts. 2 and 3 I ask whether stimuli that cannot be maintained in the articulatory loop show more evidence of dual-task costs.

Experiment 2 combines SWM with an AWM task for non-verbalizable synthesized sounds. A set of 12 sounds were generated to be as distinct from each other as possible and to be difficult to vocalize (to listen to these sounds visit http://sites.google.com/site/darylfougnie/audwmexamples). In a pilot study with ten participants I found that set sizes of 1, 3, and 5 sounds produced the same pattern of K as set sizes of 2, 6, and 10 for verbal WM in Expt. 1.

Methods

Participants

Sixteen young adults participated for course credit or monetary reward.

AWM task

An AWM task for 1, 3, or 5 non-verbalizable sounds (out of 12) replaced the verbal WM task from Expt. 1. The sounds were 300ms in duration and were presented at a rate of 500ms per item. Total stimulus presentation time lasted between 500ms (set size 1) and 2500ms (set size 5), and was followed by an inter-task interval (duration between offset of auditory presentation and onset of SWM sample) that varied between 2000ms (set size 5) and 4000ms (set size 1) such that the SWM sample was presented 4500ms after AWM sample onset. During the AWM task probe, a single sound was presented and participants indicated by key press whether the probe was one of the sample items. As in Expt. 1, during single-task SWM trials the auditory sample was replaced by presentation of the vowel *E*. Participants completed five blocks of sixty trials for a total of 20 trials per condition (up from 16 in Expt. 1). All other aspects of the study were unchanged from Expt. 1.

Results

Single-task capacity

Single-task K values for intermediate set sizes were higher than for low set sizes in both conditions [AWM task, *t(15)* = 6.73, *p* < .001; SWM task, *t(15)* = 4.64, *p* < .001]. However, there were no differences in K values between intermediate and high set sizes [AWM task, *t(15)* = .29, *p* = .78; SWM task, *t(15)* = .25, *p* = .80]. This suggests that the task loads used were sufficiently demanding to exhaust single-task capacity.

SWM performance

K values for the SWM task (Figure 2B top) were analyzed with a 4 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, *F*(2, 45) = 8.99, *p* < .001, no main effect of AWM load, *F*(3, 30) = .16, *p* = .93, and no interaction, *F*(6, 90) = .34, *p* = .92.

AWM performance

K values for the AWM task (Figure 2B bottom) were analyzed with a 4 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, *F*(2, 45) = 38.58, *p* < .001, no main effect of SWM load, *F*(3, 30) = .94, *p* = .43, and no interaction, *F*(6, 90) = 1.86, *p* = .11. One concern is that the null effect of SWM load is driven by performance in the AWM single-task condition, which is surprisingly low. Perhaps by mixing single- and dual-task conditions within blocks the single-task trials (which were less common) were more difficult because they were unexpected. However, there was still no effect of SWM load if single-task trials were excluded, *F*(2, 30) = 1.16, *p* = .33, and no interaction, *F*(6, 90) = 1.71, *p* = .13. Indeed, an analysis of dual-task performance at the highest AWM set size still found no effect of SWM load (*p* = .14).

Analysis excluding set size 1

An additional analysis was conducted to test whether the lack of load effects was influenced by reduced variability at set size 1. The lack of load effects shown in the full ANOVA results were replicated in an ANOVA analysis that excluded set size 1 conditions. There was no effect of AWM load on SWM performance, *F*(3, 45) = .15, *p* = .93, and no interaction between SWM set size and AWM load, *F*(3, 45) = .4, *p* = .75. There was no effect of SWM load an AWM performance, *F*(3, 45) = .93, *p* = .44, and no interaction between AWM set size and SWM load, *F*(3, 45) = 2.14, *p* = .11.

Experiment 3

No evidence of dual-task costs was observed in the previous study even with non-verbal AWM stimuli. While the sounds for Expt. 2 were difficult to verbalize, participants might have associated the sounds with conceptual labels that they could articulate. To further minimize the possibility that the lack of dual-task costs between auditory and spatial WM are due to participants being able to rehearse the auditory stimuli using a verbal or pre-verbal strategy, the AWM task for Expt. 3 used a set of stimuli drawn from a single category—difficult to identify birdcalls from birds not found in the United States (to listen to these sounds visit http://sites.google.com/site/darylfougnie/audwmexamples). During debriefing all participants were asked whether they recognized the birdcalls. No participant reported that they associated a birdcall with a specific bird. Thus, performance on this task likely depended on participants memorizing sound characteristics rather than memorizing a category label.

Methods

Ten young adults participated for course credit or monetary reward. The AWM task was changed to memorizing 1, 3, or 5 birdcalls (350ms duration, presented every 500ms) selected from a set of 12 possible birdcalls. Additionally, single-task trials were removed from the design so that participants performed two tasks on all trials. This was done to address concerns that the single-task conditions were more difficult than dual-task conditions due to the relatively rare occurrence of single-task trials. Participants performed six blocks of thirty-six trials for a total of 24 trials per condition.

Results

Single-task capacity

In Experiment 3 there were no single-task conditions. Therefore, to test whether the tasks used were sufficient to exhaust WM capacity I compared K values as a function of task set size during low load. K values for intermediate set sizes were higher than for low set sizes in both conditions [AWM task, $t(9) = 2.23$, $p = .05$; AWM task, $t(15) = 3.87$, $p < .005$]. A comparison of differences in K values between intermediate and high set sizes revealed no difference for the AWM task, $t(9) = .12$, $p = .91$, and a trend towards lower capacity at high than intermediate set sizes for the SWM, $t(9) = -2.0$, $p = .08$. This is consistent with previous studies that have found smaller K values at high than low set sizes (e.g. Rouder, et al., 2008). There have been several proposals to account for why K may decline at large set sizes. Rouder et al. (2008) suggested that participants might feel intimidated by large set sizes. Alternatively, performance could be limited, in part, by mutually suppressive interactions between WM representations (Johnson, Spencer, Luck, & Schöner, 2009) and mutual inhibition increases at higher set sizes. Regardless of why SWM Ks decline at the highest set size, the results suggest that the task loads used were sufficiently demanding to exhaust capacity.

SWM performance

K values for the SWM task (Figure 2C top) were analyzed with a 3 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, $F(2, 18) = 7.1$, $p < .005$, a main effect of AWM load, $F(2, 18) = 15.91$, $p < .005$, and an interaction, $F(4, 36) = 3.6$, $p = .01$. Thus, unlike the previous studies, the present study finds evidence that AWM load influences SWM K values. Was this effect consistent with competition for a shared capacity? Interference between tasks should reduce K values as task load is increased. However, SWM K values during high auditory load were greater than during low auditory load [linear contrast, $t(9) = -2.55$, $p = .03$]. The pattern of interference is not consistent with shared capacity across tasks.

AWM performance

K values for the AWM task (Figure 2C bottom) were analyzed with a 3 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, $F(2, 18) = 10.44$, $p < .005$, no main effect of SWM load, $F(2, 18) = .71$, $p = .51$, and no interaction, $F(4, 36) = .34$, $p = .85$. Thus, there is no evidence that SWM load interferes with AWM K values.

Analysis excluding set size 1

Similar results were found when set size 1 was not included. There was an effect of AWM load on SWM performance, *F*(2, 18) = 18.08, *p* < .001, but it went in the opposite direction than predicted. There was no interaction between set size and load, *F*(2, 18) = 1.72, *p* = .20. There was no evidence of SWM load affecting AWM performance, *F*(2, 18) = .72, *p* =.50, and no interaction between AWM set size and SWM load, *F*(2, 18) = .28, *p* = .76.

Experiment 4

To further eliminate the possibility that the lack of interference between the auditory and SWM tasks was due to participants verbalizing the auditory stimuli, in the current study participants were required to overtly repeat the word *the* at a rate of 4Hz. This articulatory suppression task should reduce participants' ability to verbalize stimuli.

Methods

Twenty young adults participated for course credit or monetary reward. One participant's data were not included in the analysis because they were not performing the articulatory suppression task on all trials, leaving 19 participants for analysis. This experiment was similar to Expt. 3 except that participants were instructed to perform articulatory suppression while the SWM lines were shown on screen. The SWM lines were removed during the 1000ms ITI and appeared 1000ms prior to the AWM sample.

Results

Single-task capacity

To test whether the tasks used were sufficient to exhaust WM capacity, I compared K values as a function of task set size during low load. K values for intermediate set sizes were higher than for low set sizes in both conditions [AWM task, *t(18)* = 4.41, *p* < .001; SWM task, *t(18)* = 6.57, *p* < .001]. A comparison of differences in K values between intermediate and high set sizes revealed no differences for the AWM task, *t(18)* = .04, *p* = .97, and evidence for lower capacity at high versus intermediate set sizes for the SWM task, *t(18)* = -2.75, *p* = .02 This suggests that the task loads used were sufficiently demanding to exhaust capacity.

SWM performance

K values for the SWM task (Figure 2D top) were analyzed with a 3 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, *F*(2, 36) = 25.73, *p* < .001, a main effect of AWM load, *F*(2, 36) = 3.27, *p* = .05, but no interaction, *F*(4, 72) = .88, *p* = .48. A linear contrast analysis was run to determine whether SWM Ks were greater during low AWM load than high AWM load. However, the results showed a non-significant pattern of greater Ks at higher secondary task load, *t*(18) = -1.11, *p* = .27. Thus, there is no evidence that increased AWM load competes with SWM capacity.

AWM performance

K values for the AWM task (Figure 2D bottom) were analyzed with a 3 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, *F*(2, 36) = 18.44, *p* < .001, a main effect of SWM load, *F*(2, 36) = 3.26, *p* =.05, and a marginally significant interaction, *F*(4, 72) = 2.42, *p* = .06. A linear contrast analysis found that AWM Ks were not greater during low AWM load than high AWM load, *t*(18) = 1.5, *p* = .14.

Analysis excluding set size 1

The full ANOVA results provide little evidence that WM performance depends on the load of a second task in a different modality. To provide a more sensitive test, an additional ANOVA analysis was conducted that excluded set size 1. There was a significant effect of AWM load on SWM performance, *F*(2, 36) = 3.37, *p* = .05, but no interaction between set size and load, *F*(2, 36) = .26, *p* = .78. There was also a marginal effect of SWM load on AWM performance, *F*(2, 36) = 3.12, *p* = .06, but no interaction between AWM set size and SWM load, *F*(2, 36) = 2.21, *p* = .12. Note that while both main effects of load found significant or nearly significant results, these effects went in opposing directions. For the SWM task, performance in high AWM load conditions was higher than in low AWM load conditions. In contrast, for the AWM task performance in low SWM load condition was higher than in high SWM load conditions. Overall, the present data does not find support for greater interference between SWM and AWM stimuli during articulatory suppression.

Experiment 5

In the previous studies I found no evidence for interference between spatial and AWM tasks, and found that these results generalized across three different AWM tasks. This is in contrast to the results of a paper by Saults and Cowan (2007) that claimed that dual-task costs between auditory and visual WM tasks signify that the two tasks share the same capacity-limited storage system. One possible explanation for this discrepancy is that performance in my experiments may be assisted by long-lasting sensory memory for auditory and visual stimuli. Sensory memory, which is distinct from WM, refers to the temporary persistence of sensory information after a stimulus has ceased. It is characterized as having an extremely large capacity but a brief duration. Typical estimates of the duration of sensory memory for visual information (iconic memory; 200-300ms) or auditory information (echoic memory; 1-2s) are too brief to assist performance in typical WM tasks (Averbach & Coriell, 1961; Broadbent, 1958; Crowder, 1982; Crowder & Morton, 1969; Rostron, 1974; Sperling, 1960). However, Cowan has argued that there are two phases of sensory storage: an initial transient phase with unlimited capacity, and a capacity-limited phase lasting several seconds (Cowan, 1988, 1995). It is possible that the WM tasks could be performed by this longer lasting component of sensory memory. This possibility was tested in Expts. 5 and 6. In Expt. 5, the retention interval for the SWM task (during which participants were also storing the AWM sample) was increased from 2000ms to 9000ms such that it would exceed even the longer estimates of sensory memory proposed by Cowan (1988, 1995).

Methods

Eleven young adults participated for course credit or monetary reward. This experiment was similar to Expt. 2 except that the retention duration for the SWM task was increased from 2000ms to 9000ms. As in Expt. 3 single-task trials were removed and participants performed six blocks of thirty-six trials for a total of 24 trials per condition.

Results

Single-task capacity

To test whether the tasks used were sufficient to exhaust WM capacity, I compared K values as a function of task set size during low load. Surprisingly, AWM K values for intermediate AWM set sizes were not higher than for low AWM set sizes, $t(10) = .64$, $p = .54$. SWM K values for intermediate SWM set sizes were higher than for low SWM set sizes, $t(10) = 3.01$, $p = .01$. A comparison of differences in K values between intermediate and high set sizes revealed no differences for both tasks [AWM task, $t(10) = 1.80$, $p = .11$; SWM task, $t(10) = .11$, $p < .91$]. This suggests that the task loads used were sufficiently demanding to exhaust capacity.

SWM performance

K values for the SWM task (Figure 2E top) were analyzed with a 3 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, $F(2, 20) = 22.11$, $p = .002$, no main effect of AWM load, $F(2, 20) = 1.45$, $p = .26$, and no interaction, $F(4, 40) = 1.88$, $p = .13$. Thus, there is no evidence of AWM load interfering with SWM capacity.

AWM performance

K values for the AWM task (Figure 2E bottom) were analyzed with a 3 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, $F(2, 20) = 4.79$, $p = .02$, no main effect of SWM load, $F(2, 20) = 1.9$, $p = .17$, and an interaction, $F(2, 20) = 4.06$, $p = .007$. The interaction appears to be driven by the unusual pattern of K across set size in the medium SWM load condition. If this level of load is removed, the interaction between AWM set size and SWM load is no longer significant (*p* = .25). The present data provide no evidence that SWM load interferes with AWM capacity.

Analysis excluding set size 1

Analyses that exclude set size 1 also show no main effect of secondary task load on performance. There was no effect of AWM load on SWM performance, *F*(2, 20) = 1.19, *p* = .30, and no interaction between set size and load, *F*(2, 20) = 2.26, *p* = .13. There was no effect of SWM load on AWM performance, *F*(2, 20) = 2.45, *p* = .11. However, as in the full ANOVA, there was an interaction between AWM set size and SWM load, *F*(2, 20) = 4.37, *p* = .03. Again, this interaction is likely driven by the unusual pattern of K as set size is increased in the medium SWM load condition.

Experiment 6

As a further control to remove potential contributions from sensory memory in the task, I used visual and auditory masks similar to that employed by Saults and Cowan (2007) to disrupt any echoic or iconic sensory traces of the sample displays.

Methods

Fifteen young adults participated for course credit or monetary reward. This experiment was similar to Expt. 3 except that auditory and visual sensory masks were presented during the SWM retention interval to eliminate sensory traces of the auditory and SWM samples. The masks were presented 1000ms after the offset of the SWM display. The SWM mask consisted of the presentation of white dots at each potential stimulus location (12 dots on each line) for 1000ms. For the AWM mask the 12 possible auditory stimuli were combined into one sound (350ms) that was played twice during the 1000ms mask interval. A 2000ms retention interval separated mask presentation and the presentation of the SWM probe.

Results

Single-task capacity

To test whether the tasks used were sufficient to exhaust WM capacity, I compared K values as a function of task set size during low load. There was a marginally significant trend towards higher AWM K values for intermediate AWM set sizes than for low AWM set sizes, $t(14) = 1.80$, $p = .10$. SWM K values for intermediate SWM set sizes were higher than for low SWM set sizes, $t(14) = 4.77$, $p < .001$. A comparison of differences in K values between intermediate and high set sizes revealed no differences for the AWM task, $t(14) = 1.39$, $p = .19$. For the SWM task I found higher capacity for intermediate than high set sizes, $t(14) = 3.97$, $p = .001$. This suggests that the task loads used were sufficiently demanding to exhaust capacity.

SWM performance

K values for the SWM task (Figure 2F top) were analyzed with a 3 (AWM load) x 3 (SWM set size) within-subjects ANOVA. There was a main effect of SWM set size, $F(2, 28) = 12.02$, $p < .001$, no main effect of AWM load, $F(2, 28) = 1.67$, $p = .21$, and no interaction, $F(4, 56) = 1.12$, $p = .36$. There is no evidence of AWM load interfering with SWM capacity.

AWM performance

K values for the AWM task (Figure 2F bottom) were analyzed with a 3 (SWM load) x 3 (AWM set size) within-subjects ANOVA. There was a main effect of AWM set size, *F*(2, 28) = 7.33, *p* = .003, no main effect of SWM load, *F*(2, 28) = .86, *p* =.43, and no interaction, *F*(4, 56) = .47, *p* = .76. The present data provide no evidence that SWM load interferes with AWM capacity.

Analysis excluding set size 1

As in the full ANOVA, an additional ANOVA analysis that excluded set size 1 also found no main effects of secondary task load and no interactions between load and set size. There was no effect of AWM load on SWM performance, *F*(2, 28) = 1.69, *p* = .20, and no interaction, *F*(2, 28) = .98, *p* = .39. There was also no effect of SWM load on AWM performance, *F*(2, 28) = .60, *p* = .56, and no interaction, *F*(2, 28) = 55, *p* = .58.

Discussion of Experiment 5 and 6

Expts. 5 and 6 tested the possibility that the lack of interference between AWM and SWM tasks was due to the use of sensory memory. In Expt. 5, the retention interval was increased to be longer than any estimate of the duration of sensory memory. In Expt. 6 sensory masks were presented to disrupt information from the sample stored in sensory buffers. Neither study found evidence for interference between AWM and SWM. Thus, the lack of interference between the two tasks cannot be explained by a role of sensory memory, inconsistent with Saults and Cowan (2007).

Pooling the data across the six studies

In six studies I found no evidence of dual-task costs between AWM and SWM. Might these have lacked the power to detect small but significant interference between tasks? To provide the most sensitive test for interference, I pooled the results of all studies (for a total of 88 participants) and examined the effect of secondary task load on task performance. Since experiments 3-6 did not include single-task trials, single-task conditions were not included. Additionally, since the set size varied across studies and K values scale with set size, the analysis is restricted to accuracy.

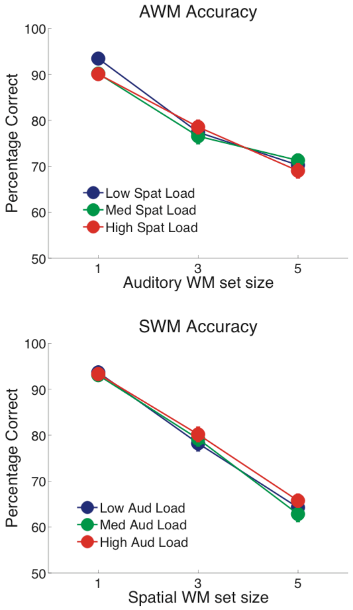

Figure 4: Task accuracy for the AWM (top) and SWM (bottom) tasks pooled across Expts. 1-6 (n=88).

SWM performance

SWM accuracy (Figure 4 top) was analyzed with a 3 (AWM load) x 3 (SWM set size) between-subjects ANOVA. There was a main effect of SWM set size, *F*(2, 174) =

393.74, $p < .001$, $\eta^2 = .38$, no main effect of AWM load, $F(2, 174) = 1.68$, $p = .20$, $\eta^2 = .08$, and no interaction, $F(4, 348) = 1.04$, $p = .30$. Pooling across all studies, there is no evidence of AWM load interfering with SWM capacity.

AWM performance

AWM accuracy (Figure 4 bottom) was analyzed with a 3 (SWM load) x 3 (AWM set size) between-subjects ANOVA. There was a main effect of SWM set size, $F(2, 174) = 232.63$, $p < .001$, $\eta^2 = .35$, no main effect of AWM load, $F(2, 174) = 1.37$, $p = .26$, $\eta^2 = .11$. There was a significant interaction, $F(4, 348) = 3.58$, $p = .002$. As with SWM performance, pooling across all six studies provided no evidence of dual-task costs.

Analysis excluding set size 1

The pooled analysis provides a powerful test for whether there is competition between auditory and spatial stimuli for storage in WM. However, it is possible that a significant effect of secondary task load might have been observed except that the lack of variability for set size 1 reduced the power of the statistical test. To show that this is not the case, an additional ANOVA analysis was performed that excluded set size 1. For SWM performance, there was a main effect of SWM set size, $F(1, 87) = 16.73$, $p < .001$, no main effect of AWM load, $F(2, 174) = 1.97$, $p = .14$, and a marginally significant interaction, $F(2, 174) = 2.26$, $p = .08$. For AWM performance, there was a main effect of AWM set size, $F(1, 87) = 5.41$, $p < .001$, no main effect of SWM load, $F(2, 174) = .29$, $p = .88$, and a significant interaction, $F(2, 174) = 2.89$, $p = .04$.

Bayesian tests for accepting or rejecting the null hypothesis

None of the six studies I conducted, nor the pooled data from those experiments, reveal any evidence of competition between auditory and spatial WM loads. These results are difficult to reconcile with the existence of an amodal WM system that has a limited capacity and can store information regardless of the modality (Baddeley, 2000; Cowan, 1995, 2006; Fougnie & Marois, 2006; Saults & Cowan, 2007). However, the evidence against an amodal store is a failure to reject the null hypothesis that auditory and spatial loads do not interfere. Traditional statistical tests using *t* tests and *F* tests do not allow for the statement of evidence for the null hypothesis. Fortunately, an alternative statistical test has been developed using Bayes factor analysis that allows one to state a preference for the null or alternative hypothesis (Rouder, Speckman, Sun, Morey, Iverson, 2009). However, use of the Bayes-factor is not possible in a factorial design. To provide the strongest case for the alternative hypothesis (that secondary task load effects primary task performance) I consider whether there is an effect of load (comparing low and high load) at performance at the highest set size. The *t* value for the effect of AWM load on SWM performance (t = -.92) corresponds to a Bayes Factor of 8.10, suggesting that the null hypothesis was about eight times more probable than the alternative hypothesis. Similarly, the *t* value for the effect of SWM load on AWM performance (t = .88) corresponds to a Bayes Factor of 7.84. These results strongly support the null hypothesis. Similar results are found if all set sizes are included, or if only intermediate and high set sizes are included.

Discussion of Expts. 1-6

The results of Expts. 1-6 are the first comprehensive examination of dual-task costs between AWM and SWM to find no evidence of interference between the tasks.

While this conclusion is based on null results, the large number of participants included in these studies and the high task loads involved suggest that if there is any potential for interference between these tasks it is small and therefore not consistent with the idea of a domain-general storage system (Cowan, 2001, 2006; Saults & Cowan, 2007).

Why did the present study find no evidence for interference across auditory and visual tasks in contrast to previous studies (Cocchini et al., 2002; Fougnie & Marois, 2006; Scarborough, 1972)? One possibility is that previous costs were due to representational overlap with the tasks, perhaps because the auditory and visual stimuli tapped into a common object WM system (Smith, et al., 1995; Tresch, et al., 1993; Ungerleider, et al., 1998; Ventre-Dominey, et al., 2005) or because the visual features were re-coded in a propositional or verbal form. Alternatively, costs may have reflected interference in the coordination of the tasks due to overloading the central executive (Baddeley, 1986; Baddeley & Logie, 1999; Duff & Logie, 2001) or to an inability to prepare for two tasks versus one task (De Jong & Sweet, 1994). Neither explanation is likely sufficient. Dual-task costs between spatial and auditory WM tasks have been observed (Cocchini et al., 2002). However, it cannot be ruled out that participants did not chunk the spatial locations in the displays used by Cocchini and colleagues into a non-spatial format. Also, past studies have tested for interference between auditory and visual WM tasks as a function of task load, rather than simply comparing single- and dual-task conditions (Fougnie & Marois, 2006; Morey & Cowan, 2005), but no previous study has controlled all of these factors, and each may contribute to producing small dual-task costs.

Examining the findings of Saults and Cowan (2007)

Saults and Cowan (2007) argued that auditory and visual WM tap into the same capacity-limited system based on evidence using a dual-task paradigm similar to the studies described above. They suggested that if sensory masks are presented to disrupt perceptual traces of sample stimuli then evidence for interference between AWM and VWM can be observed, and that such interference is sufficiently large to only be accountable by positing that the capacity limit of WM is due to a single shared capacity-limit between VWM and AWM. However, this claim was not supported by the results of my Expts. 5 and 6. Why were Saults and Cowan able to observe evidence of complete sharing of capacity between visual and auditory WM tasks whereas I was unable to find any evidence of dual-task costs? The answer, I believe, is partially due to the metric Saults and Cowan used to measure the amount of dual-task costs. They compared combined dual-task capacity to the *higher* single-task capacity (maximum capacity method). This analysis method may overestimate costs when there is a large disparity in single-task performance, as there was in Saults and Cowan's task—single-task VWM capacity (3.62) was more than twice that of single-task AWM capacity (1.41). This possibility was tested in Expts. 7-9.

To quantify dual-task costs when there is a large disparity in the single-task capacities I propose measuring the reduction in dual-task capacity relative to each task's single-task capacity, i.e. the proportional cost between single- and dual-task conditions. This result is averaged across tasks to estimate an aggregate measure of dual-task costs, termed ΔK.

$$\Delta K = \left(\frac{task1_{\sin gle} - task1_{dual}}{task1_{\sin gle}} + \frac{task2_{\sin gle} - task2_{dual}}{task2_{\sin gle}}\right)/2 \qquad (1)$$

Assuming that both tasks tap into separate resources, ΔK should be zero. If both tasks tap into the same limited-capacity system, ΔK should be 50%, or half of capacity (since that same capacity is shared across both tasks).

In the following experiments, I use a dual-task WM paradigm similar to that of Saults and Cowan (2007). Participants had to concurrently maintain two sets of WM stimuli: four spoken digits and four colored squares (Expt. 7). To eliminate contributions from sensory memory I presented pattern masks, as in Saults and Cowan (2007), during the WM retention interval. Participants completed separate blocks of single-task auditory, single-task visual, and dual-task WM trials. Capacity estimates (Ks) for each task were derived from participants' change detection accuracy. In Experiments 7 and 8, I demonstrate that the maximum capacity method (as used by Saults & Cowan, 2007) is modulated by the disparity in single-task capacity, while ΔK is not. When VWM capacity is twice that of AWM capacity, the maximum capacity method finds evidence for a single capacity-limit across tasks (Expt. 7). However, when single-task visual and AWM capacity are equivalent, the maximum capacity method shows evidence of *inter-modal savings* - combined dual-task capacity is greater than single-task capacity (Expts. 8a and 8b). The ΔK method found consistent evidence of only small dual-task costs across both studies. These findings reveal a problem with the maximum capacity method: the outcome depends on the ratio of single-task performance across tasks. In contrast, ΔK was robust to manipulations of single-task capacity. The results show that auditory and visual WM do not tap into the same limited-capacity process.

Experiment 7

Saults and Cowan (2007) had participants concurrently perform auditory and visual WM tasks and found that combined dual-task capacity was not greater than the higher single-task capacity. The goal of Experiment 7 was to replicate their findings including the disparity in single-task VWM and AWM capacity. I predicted that the maximum capacity method would show no inter-modal savings due to the disparity in single-task performance across tasks. If the lack of inter-modal savings in Saults and Cowan (2007) is due to the disparity in single-task performance, than ΔK should show evidence of inter-modal savings, since ΔK is resilient to differences in single-task capacity across tasks (Fougnie & Marois, 2006).

In the VWM task participants had to remember the color and location of briefly presented squares. Participants are generally able to store 3-4 items in this VWM task (Luck & Vogel, Todd & Marois, 2004; Vogel et al., 2001). For the AWM task, participants were presented with four digits, each spoken in a different voice (two distinct male, two distinct female voices), and asked to remember the value and speaker identity for each digit. A pilot study with eight participants found that this task had a capacity of about 2 items, similar to the single-task AWM task used by Saults and Cowan (2007).

Methods

Participants

Twelve young adults participated for course credit or monetary reward.

Stimuli

Colors for the VWM task were drawn from blue, green, red, or yellow without replacement. VWM squares subtended 1.4° and were presented at horizontal and vertical axes positions 2.9° from fixation (Figure 5). Visual pattern masks (1.4°) were formed by presenting a multicolored square (with 4 colored stripes, randomly assigned from the VWM color set) at the four VWM locations. Auditory WM stimuli were drawn from the digits 0-9 without replacement. Each digit was randomly assigned a distinct voice (from a set of two male and two female voices) with each audio file lasting 300ms. Masks for the AWM task were formed by layering the 40 audio files. The mask sound was 300ms in duration, and was presented four times during the 1200ms mask interval.

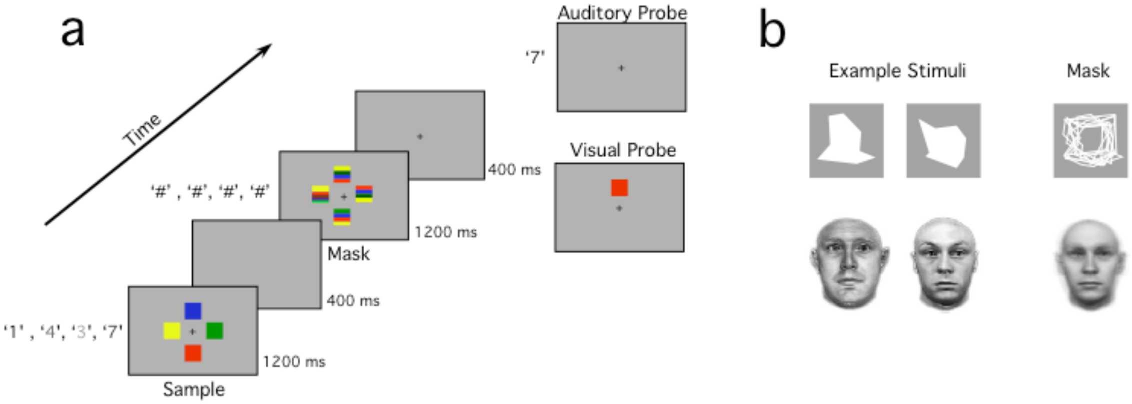

Figure 5: a) Trial timeline for Experiment 7. b) Example stimuli and sensory masks used in Experiments 8a and 8b.

Procedure

Prior to the main experiment, participants completed a practice session of 32 single- and dual-task trials. In the main experiment, participants performed one single-task auditory, one single-task visual, and two dual-task blocks, with forty trials in each block. Block order was counter-balanced across participants with the restriction that dual-task blocks were performed consecutively. In single-task trials, both the auditory and VWM samples and masks were presented to minimize perceptual differences during encoding. However, only the task-relevant modality was tested. In dual-task trials, participants were tested on either the auditory or visual WM sample. Since the tested modality was assigned on a per trial basis, participants had no way of knowing which modality would be tested, and therefore were required to maintain both samples until the probe appeared.

A trial began with the presentation of a black fixation dot against a gray background. Participants were instructed to keep their gaze centered on fixation for the duration of a trial. Presentation of the WM stimuli began 1000ms after fixation onset. Participants heard four digits over headphones spoken at a rate of 300ms/item. Concurrent with digit presentation, four colored squares were presented, and remained on screen for 1200ms. A retention interval lasting 2000ms followed WM sample presentation. Auditory and visual masks were presented 400ms into the retention interval, with a 1200ms duration. Following retention, a single-item change detection probe was presented. VWM probes were a colored square that was either in the same location as it was during the sample, or was shown in one of the other three locations. AWM probes were a digit from the sample either spoken in the same voice, or in one of the other three voices. Participants were to indicate whether the probe item exactly matched the sample (50% probability), by pressing one of two keys with their right hand. Accuracy was stressed and participants were under no time pressure to respond. VWM probes remained onscreen until a response was recorded. A 200ms ITI period separated trials.

Results

Task capacity (Figure 6a top) was estimated by measuring K from change detection accuracy (Cowan, 2001). ANOVA analyses were quantitatively the same whether K or change detection accuracy was the dependent measure. I report analyses on K consistent with Saults and Cowan (2007). K was entered into a within-subjects ANOVA with the factors of modality (visual or auditory) and single- or dual-task. There was a main effect of modality, $F(1,11) = 55.37$, $p < .001$, with VWM having a higher capacity than AWM. There was also a main effect indicating that single-task trials had higher capacity than dual-task trials, $F(1,11) = 22.31$, $p < .001$. There was no interaction between modality and single- or dual-task condition, $F(1,11) = .29$, $p = .60$. The ANOVA results show that AWM capacity was much lower than VWM capacity. Indeed, a paired t-test found that single-task VWM capacity (3.73) was significantly higher than single-task AWM capacity (2.23), $t(11) = 8.03$, $p < .001$.

These results replicate two major aspects of Saults and Cowan (2007). First, in this study the single-task capacity for VWM was nearly twice that of single-task AWM capacity. Second, the ANOVA results found clear evidence of dual-task costs. Two methods were employed to measure the amount of dual-task costs and determine whether they were indicative of a single shared capacity across modalities.

Maximum Capacity Method

The maximum capacity method tests whether combined auditory and visual dual-task capacity is greater than the single-task with the higher capacity. This was not the case. A paired t-test found that combined dual-task capacity (4.11) was not greater than single-task visual capacity, $t(11) = 1$, $p = .33$.

ΔK Method

Using equation 2 I measured the average percent decrease in each task's dual-task capacity relative to its single-task capacity (Fougnie & Marois, 2006). With this measure, if two tasks tap into the same capacity-limited process, a ΔK of 50% is expected. Indeed, a ΔK of 51% is found if equation 1 is applied to the concurrent VWM task of Fougnie and Marois (2006). However, the current study shows a ΔK of 34% (Figure 6a top) which is significantly lower than 50%, *t*(11) = 2.37, *p* = .04. Thus, measuring dual-task costs with ΔK reveals significant inter-modal savings.

Discussion

Significant dual-task costs were observed when participants had to concurrently perform an auditory and visual WM task. Do these costs indicate that a single amodal store underlies WM capacity (Cowan, 2001; 2006)? Two measures were applied to quantify the dual-task costs. The maximum capacity method (Saults & Cowan, 2007) found no evidence of inter-modal savings, consistent with the predictions of a single shared capacity across modalities. In contrast, a quantitative measure of dual-task costs using the ΔK method found that costs were less than predicted by a single shared capacity. To explain this discrepancy, I propose that the maximum capacity method is a biased measure of dual-task costs that overestimates costs when there is a large disparity between the single-task capacities of the two tasks. Critically, this predicts that altering single-task capacity to be equivalent across tasks will affect the maximum capacity method, but will not affect the ΔK method. Specifically, I predict that both measures will show evidence of inter-modal savings under such conditions. To test this, in Experiment 8 the AWM task of Expt. 7 was paired with VWM tasks that had a single-task capacity similar to the AWM task. VWM tasks that require storage of complex stimuli show reduced change detection performance and lower estimates of capacity (Alvarez & Cavanagh, 2004). The explanation for these lower capacity estimates is a matter of debate (Alvarez & Cavanagh, 2004; Awh, Barton, & Vogel, 2007; Barton, Ester, & Awh, 2009; Eng, Chen, & Jiang, 2005; Jiang, Shim, & Makovski, 2008; Luria, Sessa, Gotler, Jolicoeur, Dell'Acqua, 2009; Scolari, Vogel, & Awh, 2008). However, there is clear evidence that increasing the complexity of representations reduces VWM accuracy, and therefore measured K values. For example, Alvarez and Cavanagh (2004) found that WM capacity for colored squares was 4.4, while capacity for complex polygons was 2.0. In addition, there is evidence that WM capacity for faces is around two items (Eng, et al., 2005; Curby & Gauthier, 2007). Experiments 8a and 8b paired AWM for digits with VWM for polygons and faces, respectively.

Experiment 8a

Methods

A separate set of twelve young adults participated for course credit or monetary reward. The color WM task was replaced with a VWM task that required participants to memorize the shape of complex polygons. A set of ten eight-sided polygons were randomly generated such that their spatial extent did not exceed a 1.6° x 1.6° area. Polygons had a solid white color, and were presented against a gray background. None of these polygons resembled any familiar shapes. Every trial, four random polygons were assigned to one of the four VWM locations (see Expt 1) without replacement. Participants were instructed to remember the pairing of shape and location. For VWM probes a polygon from the sample array was presented at the correct location, or one of the other possible locations (equally likely). Participants made an unspeeded response to indicate whether the shape and location matched. Visual masks were constructed by layering the outline of the ten polygons (Figure 5b), and presenting this stimulus at all four VWM locations for the 1200ms mask duration. In all other respects, this experiment was the same as Expt. 7.

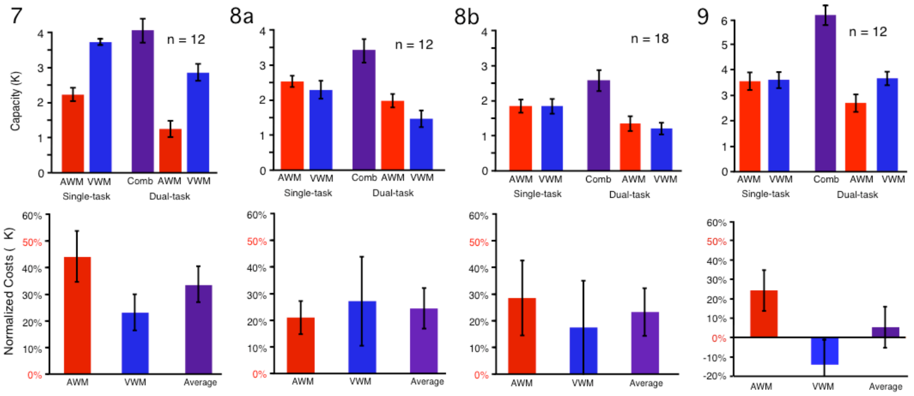

Figure 6: Top: WM capacity for AWM (red) and VWM (blue) tasks as a function of single- or dual-task condition for Experiments 7, 8a, 8b, and 9, respectively. The combined dual-task capacity (Comb; purple) was calculated by summing each participant's auditory and visual dual-task WM capacity. Bottom: Normalized dual-task costs (ΔK) for the AWM (red) and VWM (blue) tasks were combined to measure average costs (purple). Normalized costs of 50% would represent no inter-modal savings, whereas 0% indicates no interference across modalities.
Error bars represent standard error of the mean.

Results and Discussion

Capacity data (Figure 6b top) was entered into a within-subjects ANOVA with the factors of modality (visual or auditory) and single- or dual-task. There was a marginally significant effect of modality, *F*(1,11) = 3.31, *p* = .1, with AWM having a higher capacity than VWM. However, this effect appears to be influenced by dual-task performance. Indeed, there is no difference in single-task auditory (2.45) and visual (2.22) WM capacity (paired t-test, *p*=.34), suggesting that, unlike Expt. 7, single-task capacity is equated in the current study. The ANOVA revealed that single-task trials had a higher capacity than dual-task trials, *F*(1,11) = 18.76, *p* = .001. The interaction between modality and single- or dual-task condition was not significant, *F*(1,11) = .55, *p* = .47.

The maximum capacity method found evidence for inter-modal savings in the present study, in contrast to the results of Expt. 7. Paired t-tests found that combined dual-task capacity (3.33) was greater than both auditory and visual single-task capacity (*p*'s < .005). Inter-modal savings were also found when dual-task costs were quantified using ΔK. The ΔK for the current study (Figure 6b bottom, 24%) was significantly lower than 50%, *t*(11) = 3.16, *p* < .01. As predicted, when single-task WM capacity was equated, the maximum capacity method no longer shows evidence for a single shared capacity across modalities. In addition, now the maximum capacity and ΔK methods converge in showing evidence of inter-modal savings. This suggests that ΔK is a more reliable measure of dual-task costs since it is less affected by unequal single-task capacities. Additionally, these results suggest that auditory and visual WM tasks tap into at least partially dissociable storage systems.

Experiment 8a differs from Experiment 7 in two ways: the capacities for the two single-tasks were matched, and the VWM stimulus set involved complex polygons instead of colored squares. An additional experiment was conducted to show that the present findings generalize to a different stimulus set. Experiment 8b uses face stimuli for the WM task, as this stimulus set has also been shown to have a capacity of around two items (Eng et al., 2005; Curby & Gauthier, 2007).

Experiment 8b

Methods

A separate set of eighteen young adults participated for course credit or monetary reward. The VWM stimuli were ten male faces obtained from the Max-Planck face database (Troje & Bülthoff, 1996). These images were presented in grayscale, against a white background, and subtended 1.6° x 3.2° degrees of visual angle. Four random faces were selected per trial, without replacement, to occupy one of the four stimulus locations. A mask was formed by averaging the luminance values of each pixel across the ten face stimuli (Figure 5b). This mask was presented at each of the four stimulus positions for the duration of the 1200ms mask interval.

Results and Discussion

Capacity data (Figure 6c top) was entered into a within-subjects ANOVA with the factors of modality (visual or auditory) and single- or dual-task. There was no main effect of modality, $F(1,17) = .23$, $p = .64$. Both single-task auditory and visual WM capacity were 1.8 items. There was a main effect revealing that single-task trials had a higher capacity than dual-task trials, $F(1,17) = 21.07$, $p < .001$. The interaction between modality and single- or dual-task condition was not significant, $F(1,17) = .16$, $p = .69$.

The maximum capacity method found evidence for inter-modal savings in the present study. Paired t-tests found that combined dual-task capacity (2.51) was greater than both single-task auditory and visual WM capacity (both p's < .01). When dual-task costs were quantified with ΔK there was also evidence for inter-modal savings. The ΔK for the current study (24%; Figure 6c bottom) was significantly lower than 50%, $t(17) = 2.69$, $p = .01$. I found similar results in the current study as Experiment 8a, suggesting that the present findings generalize across stimulus sets. These results replicate the findings of Experiment 8a using a distinct stimulus set. The disparity in results between Experiment 7 and 8 is likely due to the change in ratio of single-task capacities, and not stimulus-specific factors.

The dual-task costs reported in the present study are smaller than those predicted by a single shared capacity-limit across modalities. However, they are quite substantial, and are larger than dual-task costs reported in Expts. 1-6, as well as in other previous studies (Cocchini et al., 2002, Morey & Cowan, 2005). In Expt. 9 I examine one potential factor that may determine the magnitude of dual-task costs: the amount of processing overlap shared by two tasks (Dutta, Schweickert, Choi, & Proctor, 1995; Navon, 1984, Navon & Miller, 1987). If two sets of stimuli have largely independent storage systems, having to perform both concurrently may still result in large dual-task costs if the tasks engage another capacity-limited process. One such candidate capacity-limited process is attention (Cowan, 1995), which may be particularly critical when a WM task has multiple task-relevant features that participants may try to bind together and store in an integrated fashion (Fougnie & Marois, 2009A). In Expts. 1 and 2 participants were required to remember color-location and digit-voice pairings. Similarly, in Saults and Cowan's (2007) experiments colors and digits could appear more than once in a WM sample, and therefore participants would be encouraged to maintain color-location and digit-voice bindings in order to identify all potential changes (Vogel et al., 2001). Therefore, it is possible that dual-task costs in these studies may have been influenced by limited attentional resources. To test this, here I measure the amount of dual-task costs for WM tasks for a single feature and therefore neither encourage nor require a binding of features in stored representations. The VWM task involves WM for the color of squares presented sequentially at fixation and the AWM task involves WM for the pitch of sequentially presented tones. WM stimuli differed from each other only in the task-relevant feature, eliminating any potential benefit for storing bound representations. A separate pilot study on eight participants found that single-task capacity for the auditory and visual WM tasks was greater than four, but no higher than six. Therefore, the set size for each task was increased from four to six. I predict that there will be significantly less dual-task costs in the current study than in Expt. 7.

Experiment 9

Methods

A separate set of twelve young adults participated for course credit or monetary reward. The VWM stimuli were six colored squares (1.4°) presented sequentially at fixation for 300ms/item. Sample display colors were randomly selected from blue, orange, purple, brown, dark green, black, white, yellow, light blue, pink, light green, or red without replacement. The study used multi-colored pattern masks, as in Expt. 7, except that the masks had 12 distinctly colored stripes. Participants were to indicate whether the single-item VWM probe was the same color as one of the sample items (50% likelihood). The AWM task consisted of a series of six tones (300ms) presented sequentially over headphones. The tones were selected without replacement from a set of 12 possible tones with frequencies varying from 220-1100 hz, in 80 hz steps. A tone mask was formed by layering all tone stimuli and was presented for the entire 1200ms mask interval. Participants were to indicate whether the probe tone was the same frequency as one of the sample items (50% likelihood). Note that the auditory and visual samples were presented concurrently, such that a colored square was on screen for the duration of a single tone stimulus. In other respects, this study was the same as Expt. 7.

Results and Discussion

Capacity data (Figure 6d top) was entered into a within-subjects ANOVA with the factors of modality (visual or auditory) and single- or dual-task. There was a marginally significant effect of modality, *F*(1,11) =.84, *p* = .07. However, this effect appears to be influenced by performance in dual-task conditions. Indeed, there is no difference between single-task auditory (3.53) and single-task visual (3.6) WM capacity (paired t-test, *p* = .89). Therefore, the auditory and visual WM tasks were matched for capacity.

There was no evidence for dual-task costs, unlike Experiments 7 and 8, as the ANOVA revealed that single-task trials did not have a higher capacity than dual-task trials, *F*(1,11) = 2.6, *p* = .13. There was also a significant interaction between modality and single- or dual-task condition, *F*(1,11) = 4.8, *p* = .05, driven by the fact that while auditory and visual single-task performance was equivalent, there was a difference in auditory (2.4) and visual (3.8) dual-task performance.

The maximum capacity method found evidence for inter-modal savings in the present study. Paired t-tests found that combined dual-task capacity (6.18) was greater than both auditory and visual single-task capacity, both *p*'s < .005. Inter-modal savings were also found when dual-task costs were quantified using the ΔK measure. The ΔK for current study (Figure 6d bottom; 5%) was significantly lower than 50%, *t*(11) = 5.05, *p* < .001 Indeed, since ΔK was not significantly above zero, *t*(11) = .49, *p* =.64, and the ANOVA results found no difference between single- and dual-task capacity, there is no evidence of dual-task costs in the present study.

It cannot be known for certain that removing the need to store bound representations can eliminate interference between a visual and an auditory WM task since the current results are a null finding which may have reached significance with a larger sample size. Note though that a Bayesian analysis applied to the test of whether ΔK is greater than zero (*t* = .49) found that the null hypothesis was 4.16 times more likely than the alternate hypothesis (Rouder et al., 2009). Additionally, I can conclude that there are significantly less dual-task costs in the current study relative to Experiment 7 (an independent samples t-test on ΔK values across studies found that ΔK was significantly lower in the current study; *t*(22) = 2.63, *p* = .01). Since the main distinction between these studies was the requirement to form and maintain integrated representations in Experiment 7, this suggests that the interference between auditory and visual arrays in that study may have been influenced by an attentional cost in the maintenance of feature bindings (Fougnie & Marois, 2009A; Wheeler & Treisman, 2002). Thus, caution is necessary in the use of a dual-task paradigm to assess interference between auditory and visual WM loads. Cross-modal costs may be overestimated if the tasks overlap in the attentional requirements of maintaining bound representations.

Chapter II Discussion

Together, the results of Expts. 1-9 provide strong support for the proposal that WM is comprised of modality-specific stores (Baddeley, 1986; Cocchini, et al. 2002; Scarborough, 1972) and are inconsistent with a domain-general view of WM. In the first six studies, no costs were observed between an auditory and spatial WM task even when the set size of each task was beyond task capacity. These results could not be explained by the use of an articulatory rehearsal strategy or by persisting sensory traces of the stimuli. These results were quite surprising, as past studies, including research I had previously conducted, had always found dual-task costs between auditory and visual WM arrays at high load. A goal of the current studies was to design tasks that would overlap as little as possible in representational content and to assess dual-task costs independent of executive and task preparation loads introduced with a dual-task design. Specifically, the current studies used a visuospatial WM task where the shape formed by the spatial stimuli was not informative. By pairing a spatial WM task with a WM task for auditory object features I attempted to pair very distinct tasks to provide the best test for a domain-general WM system. Additionally, dual-task costs in these studies were assessed by examining dual-task costs as secondary task load is *increased*, rather than simply comparing single- and dual-task conditions.

It is difficult for any WM model with a contribution from domain-general sources to account for these data (Baddeley, 2000; Cowan, 2006; McElree, 2001; Oberauer, 2002). If a domain-general source of WM capacity exists, why was it not contributing to performance in the tested tasks? I conclude that the current findings favor models of WM in which capacity is determined by modality-specific storage systems (Baddeley, 1986; Baddeley & Logie, 1999; Cocchini et al., 2001; Duff & Logie, 2001; Scarborough, 1972) or where performance is limited by content-specific interference (Dutta, et al., 1995; Navon & Miller, 1987; Oberauer & Kliegl, 2001, 2006).

These results stand in stark contrast to a recent finding arguing that auditory and visual stimuli compete for a single shared WM capacity (Saults & Cowan, 2007). Experiments 7-9 explored the cause of the discrepancy between this finding and the current studies. The results of these experiments suggest that Saults and Cowan's study overestimates dual-task costs when there is a large disparity between single-task capacities. These findings argue for the adoption of measures that are resilient to differences in single-task capacity across tasks. One such measure, ΔK, calculates the percent decrease in dual-task performance relative to each task's single-task performance. Adoption of this measure requires the assumption that dual-task costs will scale with the size of a task's single-task capacity. Under this assumption, normalizing dual-task costs to each task's single-task capacity weakens the influence of capacity differences across tasks. This helps to alleviate the issue of task-tradeoffs. Otherwise, if participants allocate more resources to the task with the higher single-task capacity, dual-task costs will be reduced, whereas they will be exaggerated if participants allocate more resources to the task with the lower capacity.

Overall, the data of Experiments 7-9 are consistent with the results of Expts. 1-6 and provide further support for domain-specific WM systems, as there was no evidence for dual-task costs (Expt. 9) when the need to use attention to maintain feature bindings was eliminated. The source of independent capacity for auditory and visuospatial information may be distinct neural regions in prefrontal or association cortex specialized for maintaining visual or auditory representations in working memory (Gruber & von Cramon, 2001, 2003; Rämä & Courtney, 2005; Romanski & Goldman-Rakic, 2002;

Schumacher et al., 1996; Smith, Jonides, & Koeppe, 1996). Perhaps more likely, modality-specific WM representations may be maintained by sustained activity in the sensory areas that encode the to-be-remembered information (Harrison & Tong, 2009; Serences, et al., 2009). For example, Serences and colleagues demonstrated that the color or orientation of gabors held in WM could be decoded from fMRI activity patterns in area V1. Sensory-recruitment during WM would help explain the lack of dual-task costs, since perception of auditory and visual stimuli engage separate cortical regions.

The present results are difficult to reconcile with theories that suggest the source of maintenance in WM is mediated by a top-down bias from amodal regions in pre-frontal cortex (Duncan, 1994). Instead the results support the view that storage in WM can happen in the absence of top-down signals (Washburn & Astur, 1998). Neurocomputational models of WM (Hopfield, 1982; Amit, Brunel, Tsodyks, 1994), based on the possibility that WM representations represent recurrent activity in cell assemblies (Hebb, 1949), demonstrate that such self-sustaining representations are possible and can be instantiated in the brain.

In conclusion, my studies demonstrate that performance in WM tasks is not due to a monolithic WM store, and that theories need to account for the distinct capacities of auditory and visuospatial tasks. These findings are not easily accommodated by proposals suggesting that attention is the source that constrains WM capacity. To account for the findings, one could suggest that independent capacities for auditory and visual WM are mediated by independent capacities for auditory and visual attention. In the next chapter I explore the degree to which capacity limits in perceptual attention are common or distinct across capacities. Is there a single shared capacity responsible for processing perceptual information, irrespective of the stimulus modality? Are there completely distinct perceptual processing resources for auditory and visual stimuli?

CHAPTER III

ARE THERE SEPARATE CAPACITIES FOR AUDITORY AND VISUAL PERCEPTUAL ATTENTION?

Motivation for dual-task tracking studies

There is more information in the environment than we can attend to and process at any given moment. For example, if participants are required to respond to two visual objects presented concurrently under time pressure, they can only report properties of one stimulus accurately (Bonnel & Miller, 1994; Norman & Bobrow, 1975; Sperling & Dosher, 1986). Such findings illustrate a limit in perceptual processing, here termed perceptual attention. Does this limit in perceptual processing reflect an undifferentiated source of capacity, or are processing limits dissociable for different types of stimuli?

Evidence for distinct capacity limits for auditory and visual perceptual attention is found in *dual-monitoring* studies that require participants to monitor two distinct sources, or ‘channels’, of information to detect or discriminate targets. When the two channels differ in modality (e.g. auditory and visual stimuli) the studies typically find that performance does not differ compared to monitoring one channel (Alais, et al., 2006; Duncan, et al., 1997; Larsen, et al., 2003; Treisman & Davies, 1973). However, significant costs are found for attending to two channels if channels share a modality (e.g. two visual stimuli) (Lee et al., 1999; Pastukhov, Fischer, & Braun, 2008).

Costs for attending to channels in different modalities has been shown in a few dual-monitoring studies (Arnell & Jolicoeur, 1999; Bonnel & Hafter, 1998; Jolicoeur, 1999a). However, such costs have been interpreted, not as costs in perceptual processing, but as costs at post-perceptual stages of processing. For example, Bonnel and Hafter (1998) found no cost between an auditory and visual monitoring task when the task involved target detection, but observed competition between modalities when participants were required to categorize the stimuli. They concluded that competition between the auditory and visual task emerges from the post-perceptual demands of categorization. Similarly, Arnell and Jolicoeur (1999) attributed their observed cross-modal attentional blink, or the transient impairment in detecting the second of two targets (Raymond, Shapiro, & Arnell, 1992), to interference in consolidating the targets for later report. Jolicoeur and colleagues have argued that consolidation draws on central / post-perceptual attention (Jolicoeur & Dell'Acqua, 1998; 1999). In sum, dual-monitoring studies are suggestive of independent *perceptual* processing capacities for auditory and visual stimuli.

However, dual-monitoring studies may not be the ideal paradigm to study the domain-specificity of perceptual processing, as performance can depend on stimulus-driven components of the task. For example, tasks that require participants to detect intermittent targets in a stream of distractors (e.g. Duncan et al., 1997b) can allow participants to rely on target templates (Duncan & Humphries, 1989) to summon attention when a task-relevant target appears. Setting up templates for the two sources of information may allow participants to employ strategies to time-share their attention across channels. Additionally, measures of psychophysical discrimination thresholds (Alais, et al., 2006; Bonnel & Hafter, 1998) may depend on how the visual or auditory sensory systems are configured. For example, if the task requires detecting whether a line of a certain orientation is presented, the gain of neurons that code for that orientation can be increased in advance of the target to improve discrimination thresholds. Thus, sources of modality-specific interference in dual-monitoring tasks may be due to factors other than limits in the capacity for rapidly processing perceptual information. Instead, costs may reflect an inability to maintaining two target templates in the same modality, or an inability to efficiently tune the sensory system for targets that share a modality.

In more complex tasks, dual-task costs can occur between tasks that differ in modality (Allen, McGeorge, Pearson, & Milne, 2006; Kunar, Carter, Cohen, & Horowitz, 2008; Strayer & Johnston, 2001). For example, talking on a cell phone can disrupt visual navigation tasks (Strayer, et al., 2003; Strayer & Johnston, 2001). As another example, tasks that require speeded responses to auditory tones (Tombu & Seiffert, 2008) or generation of words (Kunar et al., 2008) can interfere with participants' ability to track visual targets among distractors in a multiple object tracking task (Pylyshyn & Storm, 1988). However, it is believed that such costs occur because of the executive demands of the auditory tasks. For example, the interference from cell phone conversations is due to the central demands imposed by word generation rather than by auditory distraction (Kunar et al., 2008; Strayer & Johnston, 2001). Since these findings can be explained by interference at central stages of processing they do not provide strong evidence against domain-specialized perceptual attention.

However, there is reason to believe that strong links exist between auditory and visual perceptual attention. Studies have shown extensive interactions across modalities in the selection of perceptual information (Spence & Driver, 1996, 1997; Spence, Nicholls, & Driver, 2001). Valid exogenous and endogenous spatial cues from one modality (e.g. auditory) facilitate responses to stimuli that occur in a different modality (e.g. vision) at the cued location. There is also strong neurophysiological evidence for cross-modal links between auditory and visual selective attention (Eimer & Schroger, 1998; Ghazanfar & Schroeder, 2006; Hocherman, Benson, Goldstein, Heffner, & Hienz, 1976; Kayser, Petkov, Augath, & Logethetis, 2007). Visual evoked potentials have been found to be enhanced for visual stimuli that are presented at a location that was cued for an auditory target (Eimer & Schroger, 1998). Evidence for strong links across modalities provides support for domain-independent sources for the control of attention across tasks and modality (Chiu & Yantis, 2009; Shomstein & Yantis, 2004, 2006). Further evidence for a domain-general attention system is provided by studies showing that attending to stimuli in one modality reduces activity evoked by stimuli in another modality (Laurienti, et al., 2002; Shomstein & Yantis, 2004). In addition, activity in domain-specific sensory regions is reduced in bimodal conditions (attention split across modalities) relative to attending to a single stimulus (Johnson & Zatorre, 2005, 2006). These results match the predictions of an attentional commodity that has to be divided across modalities.

The neurophysiological evidence for a link between auditory and visual perceptual processing conflicts with the lack of convincing behavioral evidence for cross-modal interference. To explore this issue, the current studies employed tasks where the target-defining feature values constantly changed over time. This prevents strategies where the participants can rely on stimulus-driven components of the task to time-share their attention. Specifically, I use auditory and visual tracking tasks that require participants to individuate a target from a distractor item where the feature value(s) that differentiate the target from the distractor changes over the course of tracking. Tasks that require the tracking of feature values over time draw on perceptual attention via the need to constantly adapt to changes in the target's feature value (Alvarez & Cavanagh, 2005). In addition, they highlight the need for selective attention since the participant has to individuate the target from a similar distractor item (Pylyshyn & Storm, 1988). A lack of interference between such tasks would provide stronger evidence for distinct perceptual processing capacities across modalities. Alternatively, interference in this paradigm would place limits on the conditions in which auditory and visual processing tasks can be performed concurrently with no cost. This would suggest that previous findings are task specific, and do not generalize to tasks that require constant tracking of perceptual features.

Description of tracking tasks

Three tracking tasks (two visual and one auditory) were used in the current studies. In one visual tracking task (dot task) I required participants to track a target disc while ignoring a distractor disc (that was similar in appearance) by following the target's spatiotemporal identity. Such tracking tasks have been well studied and demonstrate limits in visuospatial attention (Alvarez & Cavanagh, 2005; Oksama & Hyönä, 2004; Pylyshyn & Storm, 1988; Sears & Pylyshyn, 2000). In a second visual tracking task (gabor task) I required participants to track one of two spatially overlapping gabors by attending to the gabor's color, orientation, and spatial frequency as both gabors' feature properties changed over time (Blaser, Pylyshyn, & Holcombe, 2000; see Neisser & Becklen, 1975, for a similar task). A novel task was developed for auditory tracking since previous tasks either did not require constant attention or required attention to semantic content. Several auditory attention tasks rely on detecting or classifying an infrequent target (e.g. Duncan, et al., 1997; Arnell & Jolicoeur, 1999). As mentioned, such tasks may allow strategies where attention can be time-shared across modalities by relying on target templates to attract attention to relevant targets. Another common auditory attention task is a dichotic listening task – participants hear two spoken passages presented at the same time (one to each ear) and are required to attend to or shadow one of the passages (Allport, Antonis, & Reynolds, 1972; Cherry, 1953; Conway, et al., 2001; Moray, 1959; Treisman, 1960). Vocal passages are often highly predictive (Treisman, 1960) such that participants can use semantic and content cues to recover tracking if attention is momentarily shifted to another task. This may afford an attention-switching strategy in which a pool of resources is effectively shared across two tasks (Broadbent, 1982; Cowan, 1995). Indeed, such a strategy has been suggested (Cowan, 1995) to account for past findings of perfect task sharing (after practice) between complex tasks such as reading and taking dictation (Hirst, Spelke, Reaves, Caharack, & Neisser, 1980; Spelke, Hirst, & Neisser, 1976). It is also possible that tasks with a semantic component may exacerbate costs due to the executive demands of language comprehension (Engle, 2002). Thus, auditory tracking tasks with a language or semantic component have severe problems of interpretability and are not useful for testing whether vision and audition share a common perceptual attention capacity. The novel auditory task employed in the current study requires participants to track a target tone as it changes in pitch and stereo position (i.e. left or right stereo space). Studies of auditory perceptual segregation demonstrate that it is possible to attend to and distinguish one of two concurrently presented tones if the tones are presented as a series of alternating beeps, a phenomenon termed the ‘streaming effect’ (Bregman, 1990; Bregman & Campbell, 1971). For the auditory system to perceive the beeps as two distinct sequences, the tones must differ either in pitch or stereo position (timbre can also be an effective cue for segregation but was not used in the current study).

These tracking tasks were combined in a dual-task setting in which participants either performed one or both tasks. Two types of task pairings were tested: multi-modal and uni-modal. The multi-modal condition paired the gabor tracking task with the tone tracking task. The uni-modal condition paired the gabor tracking task with the dot tracking task. In addition, there were two levels of difficulty for each task, with hard difficulty trials having a faster rate of feature change over time than easy difficulty trials. The tone and dot tracking tasks were not paired since both tasks required attention to spatial information. Therefore, interference between these tasks could have been larger than the other task combinations due to an additional source of interference at a representational level.

Predictions

A critical question is whether performance for tracking an auditory and visual target will resemble performance for tracking one target (predicted by proposals suggesting that there are independent perceptual capacities across modalities; Alais, et al., 2006; Duncan et al., 1997; Larsen et al., 2003) or performance for tracking two targets from the same modality (predicted by theories proposing a domain-general source of processing or attentional control; Chiu & Yantis, 2009; Cowan, 1995; Daneman & Carpenter, 1980; Johnson & Zatorre, 2005; Just & Carpenter, 1992; Kane et al., 2004; Shomstein & Yantis, 2004; Treisman, 1969; Wickens, 1984). The former hypothesis predicts no difference in performance between single-task and dual-task performance in the multi-modal condition. The latter hypothesis predicts that not only will there be strong dual-task costs in the multi-modal condition, but that they will be equivalent to those observed in the uni-modal condition. Intermediate results are also possible—dual-task costs in the multi-modal condition that are significantly less than that observed in the uni-modal condition—and are consistent with theories that allow both domain-general and domain-specific contributions to task performance (Kane et al., 2004; Treisman & Davies, 1973).

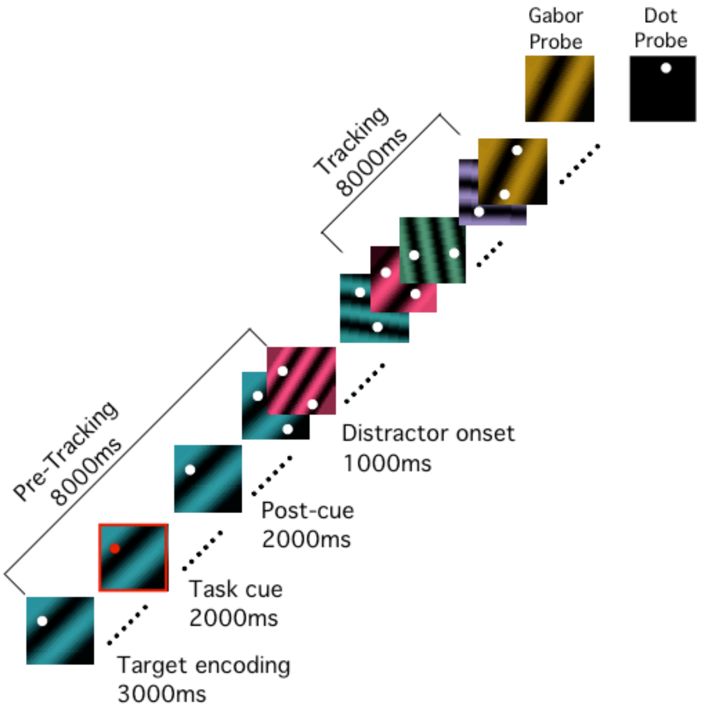

Figure 7: Timeline for a dual-task trial involving the gabor and dot tracking tasks. The probed task (gabor or dot task) was selected at random.

General Methods

Trial outline

The task procedure for these studies involved concurrent presentation of two tracking tasks. In Experiment 1, participants performed the gabor tracking task, the tone tracking task, or both tasks concurrently. In Experiment 2, participants performed the gabor tracking task, the dot tracking task, or both tasks concurrently

A trial began with target encoding (3000ms). Participants were presented with a target for each of the two tasks and were instructed to encode both targets. Targets remained onscreen for the entire duration of the target encoding period. Since both targets were always presented, single- and dual-task trials were equivalent until the task cue was presented. The task cue (2000ms) instructed participants which target(s) they should follow during the tracking interval. Following 2000ms of cue presentation, the cue was removed and both targets remained for 2000ms. This 2000ms post-cue phase gave participants more than adequate time to adjust their task set to attend to the task-relevant target(s). Following the post-cue period, a single distractor for each task was presented. The distractor differed from the target (see individual task methods) in the task-relevant feature(s), and was easily differentiated from the target even though both target and distractor were presented concurrently. The distractor onset phase (1000ms) gave participants time to adjust to *selectively* attending to the target(s) before the onset of tracking. A timeline for a trial is shown in Figure 7 within the context of a dual-task trial involving both the gabor and dot tasks

During the tracking interval (8000ms) the target and distractor for each task continually changed in the task-relevant features (see individual task methods). To differentiate target from distractor, participants needed to constantly attend to the target, since the feature values that allow the target to be distinguished from the distractor changed over time. Note that during the tracking phase the stimuli for both tasks were presented, even during single-task conditions. Thus, differences in performance between single- and dual-task conditions cannot be explained by differences in sensory stimulation during tracking.

To test whether participants successfully tracked the target in cued task(s), at the end of the tracking interval only one of the two stimuli remained on and participants were to indicate whether the remaining stimulus was a target or a distractor (each possibility equally likely). In dual-task trials, only one of the two tasks was probed, with the task selected at random (each task equally likely). For example, in a dual-task trial involving an auditory and visual tracking task, the probe may consist of either the presentation of the visual target, visual distractor, auditory target, or auditory distractor. In single-task trials, the probe was always for the cued task. The probe did not change in feature values during its presentation. Participants were instructed to indicate whether the probe item was a target or distractor by selecting one of two buttons with their right hand. Responses were unspeeded, and accuracy was stressed. Trials were self-paced and accuracy feedback was provided after each response.

Task Conditions

Each task had two levels of difficulty—hard and easy—with difficulty varying by the speed at which features changed during tracking. The difficulty of hard and easy task conditions was titrated by block and task based on performance in the single-task conditions in the prior block. Specifically, the rate of change in featural values for the easy difficulty level of a specific task decreased whenever participants made at least one an error on the previous block. The rate of change in featural values for the difficult condition decreased whenever participants made three or more errors (out of eight trials). However, if participants got no more than one incorrect response in difficult single-task trials in the previous block, the rate of change increased. The advantage of this difficulty titration is that it minimizes differences in task load across tasks, even accounting for individual differences in relative task competence. For each participant, performance in the easy single-task conditions should approach 100% and performance in the difficult single-task conditions should hover near 75%. Over the first few blocks, large fluctuations in task difficulty may be observed. However, by the end of the experiment changes in task difficulty over time should be minimal.

There were four single-task conditions formed by the cross of probed feature (auditory or visual) and task difficulty (easy or hard). There were eight dual-task conditions formed by the cross of probed feature (auditory or visual), task difficulty (easy or hard), and other task load (easy or hard). Note that to minimize confusion I use the term *difficulty* to refer to the difficulty level of the probed task and the term *load* to reference the difficulty level of the secondary (non-probed) task. This allows for an assessment of task performance across three levels of secondary task load: single-task, low load (easy), and high load (hard). Task conditions were randomly intermixed within blocks.

Each participant completed two single-task blocks followed by five intermixed blocks containing both single- and dual-task trials. The single-task blocks consisted of eight trials of each single-task condition for a total of 32 trials. These blocks served to provide an initial titration of task difficulty for each task and difficulty level. The intermixed blocks consisted of eight trials of each single-task condition and four trials of each of the eight dual-task conditions for a total of 64 trials. Thus, single- and dual- task trials happened equally often in intermixed blocks. Participants completed the seven blocks across two days with no more than two days separation. Participants completed four blocks the first day and three blocks the second day.

Bonus pay was provided, to motivate participants. Bonus pay ranged from 0-10$ per day and was based on both single- and dual-task performance. Specifically, bonus pay for single-task trials (maximum of 5$ per day) was based on the final setting of the difficulty titration, averaged across all single-task trials. Difficulty was adjusted in discrete steps for each study, allowing the final difficulty level to be represented as a percentage ranging from the minimum final difficulty value (assuming that difficulty decreased for both easy and hard conditions after each block) to the maximum final difficulty value (assuming that difficulty increased for the hard condition after each block) (see task-specific methods). To determine single-task bonus pay, this percentage was multiplied by five. To calculate bonus pay for dual-task trials, the percentage change in dual-task relative to single-task accuracy (averaged across all dual-task trials) was multiplied by ten, and this result was subtracted from 5$. Values above 5$ (less than 0% cost) or below 0% (greater than 50% cost) were not allowed. Participants were not told the details of the bonus pay calculations. They were simply told that bonus pay was linked to overall task performance and that they should try to perform well on all trials.

Task-specific methods

In the following section, the methods are described in detail for each of the three tracking tasks.

Gabor task

The gabor task consisted of the sequential presentation of two gabor patches in the center of the screen, one of which is designated as a target. When two gabors are presented alternately at a fast rate, participants can perceive and segregate both gabors even though they occupy the same spatial position (Blaser et al., 2000). Participants attended to the color, orientation, and spatial frequency values of the target to differentiate the two gabors. Attending to only one feature would be insufficient for the task, since, at any point the two gabors could share a value in one feature. However, the gabors were never the same value in two or more features at any given time. A previous study has shown that participants can selectively track one of two gabors at this rate of presentation by attending to one or more features (Blaser et al., 2000).

The gabors were presented in a 1.5° x 1.5° area in the center of the screen. Target and distractor gabors were presented on alternating screen refreshes at a rate of 10ms per refresh. Color values for the two gabors were drawn from a circle in the CIE

L*a*b* color space (centered at L=54, a=18, b=-8, with a radius of 59). The initial value of the two gabors differed by at least 60° of L*a*b* color space. Initial orientation values of the two gabors varied between 1° and 360°, and differed by at least 60°. The spatial frequency of the two gabors was randomly assigned within the range of .35°-.85° degrees of visual angle / period (angle per period, or APP). The initial spatial frequency value of the gabors differed by at least .1° APP. The task-cue to signal participants to attend to the gabor task was a red frame (.08° thick) surrounding the target gabor.

During the tracking phase, the color, orientation, and spatial frequency values of the two gabors changed at a rate specified by the difficulty of the gabor task (easy or hard) and by participants past performance. For the easy task condition, color and orientation initially changed at a rate of 1.25° (in color or orientation space) per 20ms, while spatial frequency changed at a rate of .01 APP. For the hard task condition, color and orientation initially changed at a rate of 1.75°, while spatial frequency changed at a rate of .014 APP. If the prior block performance was too low (1 or more incorrect response for the easy condition, 3 or more incorrect responses for the difficult condition), the rate of feature change was decreased for all features. Specifically, color and orientation change rate decreased by .25° and spatial frequency change rate decreased by .002 APP. If the prior block performance was too high (1 or less incorrect response for the difficult condition) the rate of change for color and orientation increased by .25° and spatial frequency change rate increased by .002 APP. The change rate could not be less than .25° for color and orientation, or less than .002 APP for spatial frequency. The direction of change was set randomly per feature per gabor, and had a 10% chance of changing direction every refresh (set independently for each feature). Note that, for a few participants the initial difficulty settings were different (1.0° for the easy condition; 1.25° for the hard condition).

Tone task

When a series of tones of two distinct pitch frequencies is presented at a high rate, participants perceive the tones as two distinct ‘auditory streams’ segregated by pitch (Bregman, 1971). This auditory phenomenon was leveraged to create a tone tracking task similar to the gabor tracking task. Initially, the target stream was presented in isolation as a series of tones at a constant pitch and stereo channel (left, center, or right) with a presentation rate determined by the task condition (easy or hard difficulty) and past task performance. However, beginning in the distractor onset phase, target and distractors were presented as alternating tones. This continued throughout the tracking interval as participants attended to the target’s pitch and position in stereo space.

At the start of each trial, the target and distractor were assigned an initial pitch value between 220 and 1540 Hz in steps of 120 Hz, with the restriction that the two starting pitch values differed by at least 360 Hz. Additionally, the distractor and target were assigned distinct positions in stereo space (left, right, or center). The left and right position of stereo space corresponded to the sound being played to the left or right ear, respectively, with no sound played to the other ear. The center position of stereo space involved sound presentation to both ears at 50% intensity.

Whenever an auditory stream changed in pitch or position in stereo space it did so over a period of six tones, termed *cycles*. Cycle rate and the number of cycles per trial varied according to the task difficulty condition (easy or hard) and performance in previous blocks. During the tracking phase, at the start of each cycle, target and distractor streams were assigned a new pitch and stereo position. Pitch was selected with the restriction that streams were assigned a different pitch value from the last cycle, and that target and distractor pitch values had to differ by at least 360 Hz. A stereo position (either left, center, or right) was assigned to the target and distractor each cycle, with replacement. However, to minimize confusion arising from the breakdown in perceptual segregation cues (Bergman, 1971), the target and distractor tones were not allowed to share a stereo position when the tones *crossed pitch* (e.g. the target was initially a higher pitch than the distractor, but then become lower). Instead, the target and distractor tones were assigned distinct positions in stereo space for the cycle prior to the pitch crossing, and stereo position did not change for the following cycle. Thus, when the tone streams crossed pitch, participants could use position in stereo space to differentiate the target from distractor. In order to prevent participants from relying on categorical labels, it was ensured that the tones crossed pitch at least once per trial. The average number of crosses averaged across each participant and each trial was 3.44.

Pitch and stereo position values changed across the two tones in a gradual fashion throughout a cycle. For example, if an auditory stream of 220 Hz in the left channel were to change into a 580 Hz stream in the right channel, for each tone of that cycle, the frequency and stereo position would change in 1/6 steps. The pitch values for those six tones would be 280, 340, 400, 460, 520, and 580 Hz. The intensity in the left stereo channel would change from 83%, 67%, 50%, 33%, 17%, 0%, and the intensity in the right stereo channel would change in the opposite direction.

The number of cycles per tracking phase (also the number of changes per trial) varied according to the task difficulty condition (easy or hard) and performance in previous blocks. Initially, easy trials had 9 cycles per tracking phase and hard trials had 12 cycles per tracking phase. Both cycle rate and the alternation rate (time between the onset of one tone to the onset of the next tone) were determined by the number of cycles. For example, if there were 10 cycles, the total duration of each cycle would be 800ms (8000ms / 10) with an alternation rate of 67ms (800ms / 12). Regardless of the alternation rate, there was always a 5ms gap between the onset of one tone and the onset of the next tone. The number of cycles per trial changed, independently for each difficulty condition, as a function of single-task performance in the prior block. Whenever performance was too low for the easy and hard conditions, the number of cycles decreased by one. In contrast, if performance was too high for the hard condition, the number of cycles increased by one. Note that, for some participants the initial difficulty settings differed (5 cycles for the easy condition; 9 cycles for the hard condition). To cue that the tone-task was task-relevant, the intermittent target tone stream was replaced by a long beep at the same pitch, intensity, and position in stereo space as the target. This beep lasted the duration of the task cue phase (2000ms).

Dot task

The Dot task is a variant of the multiple object tracking task (Pylyshyn & Storm, 1988) in which participants follow one target presented with a like distractor by following the target's spatiotemporal identity. The dots were solid black circles that subtended .19° of visual angle. Dots were presented within the 1.5° x 1.5° spatial extent of the gabor patches and appeared superimposed over the gabors. The target dot appeared red during the task-cue phase to indicate that the dot task was task-relevant. Prior to the tracking interval, the target and distractor differed in spatial position. During the tracking phase, target and distractor position changed over time. Participants were required to constantly attend to and follow the target's position to differentiate it from the distractor throughout the tracking interval.

Target and distractor dots were assigned unique random positions (no less than .31° apart) within a 1.2° x 1.2° area in the center of the gabor patches. At the start of the tracking interval target and distractor dots were independently assigned a motion direction vector selected from 45°, 135°, 225°, and 315°. The rate of change in position was determined by the difficulty condition (easy or hard) and prior single-task performance. For easy condition trials, the dots initially moved at a rate of .035° per screen refresh (10ms). For the hard condition, the dots initially moved at a rate of .047° per screen refresh. Speed of tracking was adjusted based on single-task performance in the prior block. If performance was lower than desired, tracking speed decreased by .0065° per screen refresh. In contrast, if performance was too high, tracking speed increased by .0065°. Note that for a few participants the initial difficulty settings were different (.029° for the easy condition; .035° for the hard condition).

On each refresh, each dot had a 10% chance of being assigned a new vector selected from 45°, 135°, 225°, and 315°. In addition, whenever a dot neared the edge (within .3°) its vector was flipped. If the target and distractor dots neared each other (within .25°) the dots were assigned vectors to move in opposite directions.

Analysis

Individual experiment analysis

For each experiment the data was analyzed to determine:

1) Whether performance in the single-tasks were comparable across tasks
2) Whether the tasks mutually interfered
3) Whether these dual-task costs were load-dependent

Each analysis described below was conducted on two data sets. To provide the least noisy estimate of the condition means (lower variability) analyses were conducted on blocks three through seven. However, this data set may not have allowed enough blocks to fine-tune the difficulty levels of single-task conditions. Therefore, a second set of analyses included only the blocks from the second session (5-7). It is important to note that most results were qualitatively the same across both data sets. To minimize duplication of the reported findings, analyses and figures are reported only using the data set that included blocks three through seven. However, the results for both data sets are reported in the rare instances in which the data sets produced qualitatively different results.

In order to compare the magnitude of dual-task costs across experiments it is important that the demands imposed by the various tasks were nearly equivalent. In an attempt to equate demands across tasks, the current study titrated performance for each task for easy and hard conditions. To test whether the titration method was successful in equating task performance, accuracy in single-task conditions was entered into a 2 (task) x 2 (difficulty) ANOVA. Although no difference in performance across tasks was expected, there should be a main effect of difficulty because the easy and hard conditions were titrated at 100% and 75% performance rates, respectively.

To determine whether the tasks mutually interfered a 3 x 2 ANOVA, with task accuracy as the dependent measure and factors of load (single-task, low, high) and difficulty (easy, hard), tested for a main effect of load. However, any dual-task costs revealed by an overall load effect could be due to the difficulty of performing two tasks concurrently, rather than competition for a common perceptual attention capacity. In a subsequent analysis, I asked whether dual-task costs increase with overall task load. Specifically, a 2 x 2 ANOVA, with factors of load (low, high) and difficulty (easy, hard), tested whether performance was worse in high load, relative to low load dual-task conditions.

The dual-task analyses were carried out on data collapsed across probe type. Due to the length of trials and the high number of conditions there are insufficient trials for reliable estimates of performance for each probe.

Across experiment analysis

In addition to the experiment specific analyses, the costs across studies were compared to determine:

1) Whether there was a difference in *concurrence costs* between uni- and multi-modal experiments.
2) Whether there was a difference in *load-dependent* dual-task costs between uni- and multi-modal experiments.

Concurrence costs (interference between two tasks that occur independent of task load) were measured as the difference in performance between single-task conditions and low load dual-task conditions (averaged across probed task). Note that load here refers to the difficulty of the secondary task. These costs were entered into a between-subjects 2 x 2 ANOVA with the factors of Experiment (uni-modal, multi-modal) and difficulty (easy, hard). To measure load-dependent costs, performance in dual-task high load conditions was subtracted from dual-task low load conditions. These costs were entered into a between-subjects 2 x 2 ANOVA with factors of Experiment and difficulty. The critical question in both analyses was whether the ANOVA test revealed a significant main effect of experiment (main effects of difficulty were not expected).

Experiment 1

Methods

The goal of this study was to determine the amount of dual-task interference observed in a multi-modal pairing of attentional tracking tasks. The gabor and tone tracking tasks were presented such that the task phases overlapped. Fourteen participants (seven male) were required to perform the gabor tracking task, the tone tracking task, or both tasks simultaneously. Single- and dual-task trials differed only during the task-cue phase. On dual-task trials both the gabor and tone task-cue were presented (see individual task methods).

Results and Discussion

Single-task performance

Percent correct for the single-task conditions by task and difficulty is shown in Figure 8A. Additionally, Figure 9A shows the change in difficulty across blocks, represented as the number of changes (tone task) and the rate of change in orientation, color, and spatial frequency values (in degrees or APP) per gabor alternation (gabor task). To compare single-task performance across tasks, percent correct for single-task trials was entered into a 2 x 2 ANOVA with factors of difficulty (easy, hard) and task (gabor, tone). This revealed a main effect of difficulty ($F(1, 13) = 50.83$, $p < .001$), no main effect of task ($F(1, 13) = .02$, $p = .90$), and an interaction between task and difficulty ($F(1, 13) = 11.22$, $p < .01$).

The results show that the difficulty manipulation was successful—hard trials had significantly lower performance, suggesting that performance for those tasks is more demanding and may require more capacity. In regards to whether the tasks were equated for difficulty, the lack of a task effect provides some support. However, the interaction between task and difficulty suggests that the difficulty titration did not work equivalently for each task. Indeed, the performance rates for the tone task were lower than expected for the easy difficulty condition and higher than expected for the hard difficulty condition. Why might this have happened? Note that increasing the number of cycles had the effect of decreasing the alternation rate between tones. Previous studies have shown that the ability to perceptually segregate tones improves as the alternation rate increases (Bregman & Campbell, 1971). While this effect occurs largely outside of the range of alternations in the current study (it is typically found at slower alternation rates) it is possible that two factors altered performance in an opposing manner as the number of cycles increased or decreased. In the easy condition, as difficulty levels decreased, the reduced number of changes may have decreased task difficulty while the decreased alternation rate may have added to the task's difficulty. A similar explanation may account for the higher than expected performance in the hard difficulty condition. Thus, it is possible that the auditory load manipulation was insufficiently strong and that the current study may actually underestimate load-dependent dual-task costs. This issue will be discussed further when dual-task costs for Experiment 1 and 2 are compared.

Comparison of dual-task versus single-task conditions

Figure 10A shows percent correct as a function of probed task difficulty and secondary task load. Note that this analysis collapses across the task that was probed (tone or gabor task). Percent correct was entered into a 2 x 3 ANOVA with factors of difficulty (easy, hard) and load (single-task, low, high). The results showed a main effect of difficulty, $F(1, 13) = 90.05$, $p < .001$, with worse performance for hard trials.

Additionally, there was a main effect of secondary load ($F(1, 13) = 20.97$, $p < .001$), but no interaction between difficulty and load ($F(1, 13) = .10$, $p = .90$).

These results show, in contrast to several previous studies (Alais, et al., 2006; Duncan, et al., 1997; Larsen, et al., 2003; Soto-Faraco & Spence, 2002; Treisman & Davies, 1973), that visual and auditory perceptual attention tasks can substantively interfere with each other. Furthermore, this interference was load-dependent. A subsequent ANOVA found that performance was worse in the high load relative to low load dual-task condition. Specifically, a 2 x 2 ANOVA with factors of difficulty (easy, hard) and load (low, high) found a main effect of difficulty ($F(1, 13) = 68.39$, $p < .001$), a main effect of secondary task load ($F(1, 13) = 8.11$, $p < .05$), but no interaction ($F(1, 13) = .08$, $p = .76$). This is strong evidence for competition between auditory and visual tracking tasks. Note that single- and dual-task conditions were perceptually equivalent except for the 2000ms task cue interval. Therefore, differences in low-level sensory stimulation cannot explain the results. Also, the fact that the observed interference increased with increased secondary task load refutes the possibility that costs may be due simply to coordinating two tasks.

Note that an interaction between the factors of difficulty and load was not observed. This interaction might be predicted from previous studies suggesting that tasks that tap into common processes will interfere in an over-additive fashion (Johnston et al., 1995; Logan, 1978, 1979; Sternberg, 1966). However, it is possible that over-additive costs may not be observed here since the size of an expected effect may scale with the mean value of accuracy performance (meaning that load-dependent costs may be numerically smaller for the difficult conditions as accuracy performance was lower), thus masking an over-additive interaction between the two factors. Note that this interaction was also not observed in the uni-modal condition. Therefore, the absence of a load x difficulty interaction does not provide evidence for a domain-specific source of perceptual attention.

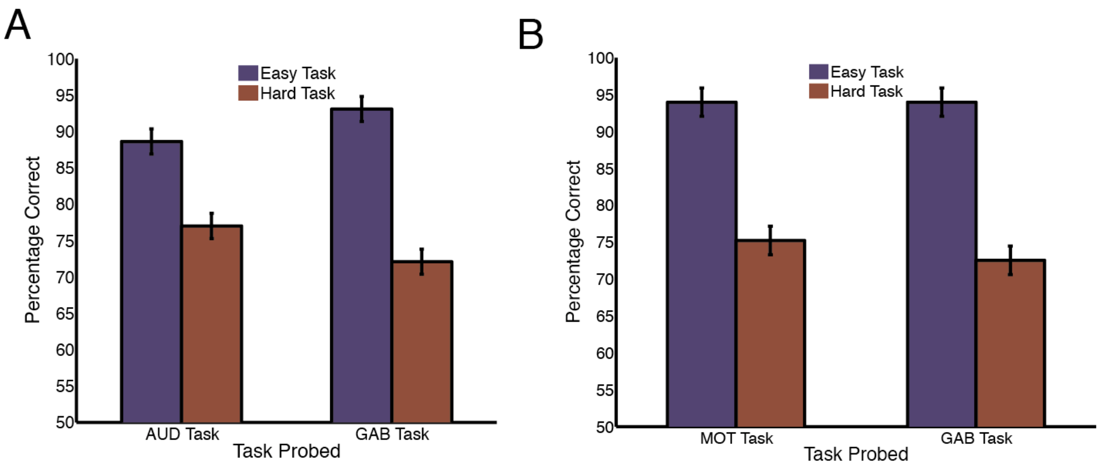

Figure 8: Single-task accuracy for the multi-modal (A) and uni-modal (B) experiments as a function of task difficulty and task. Error bars represent within-subject error of the main effect of task.

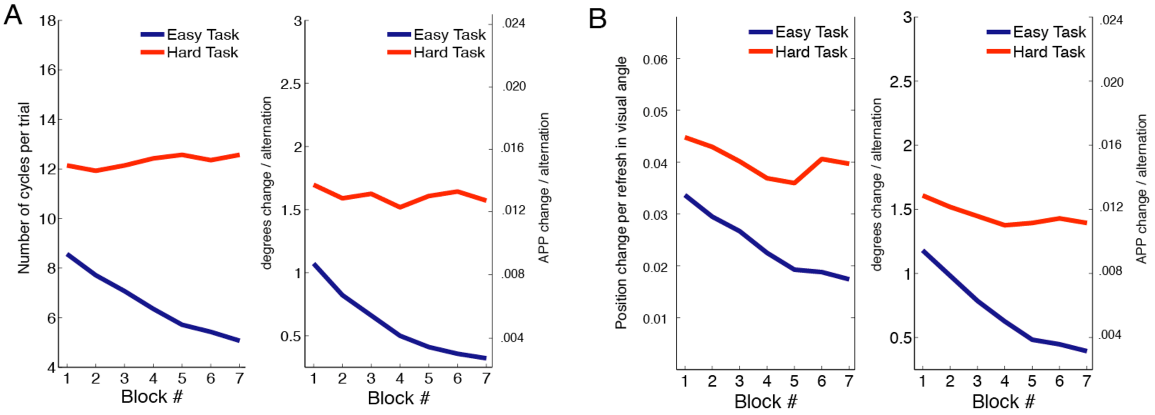

Figure 9: A: Difficulty levels across block in the multi-modal study for the tone tracking task (left) and gabor tracking task (right). B: Difficulty levels across block in the uni-modal study for the dot tracking task (left) and the gabor tracking task (right). For the gabor task, rate of orientation and color changes were measured in degrees / alternation while spatial frequency was measured in APP / alternation.

The current data show cross-modal interference in tasks that require tracking of perceptual information over time, in contrast to accounts that suggest that auditory and visual attention have independent perceptual attention capacity (Alais, et al., 2006; Duncan, et al., 1997). Dual-task costs in this multi-modal experiment can be compared to costs observed in Experiment 2, which pairs two visual tasks, to determine the relative combination of domain-general and domain-independent sources of perceptual tracking.

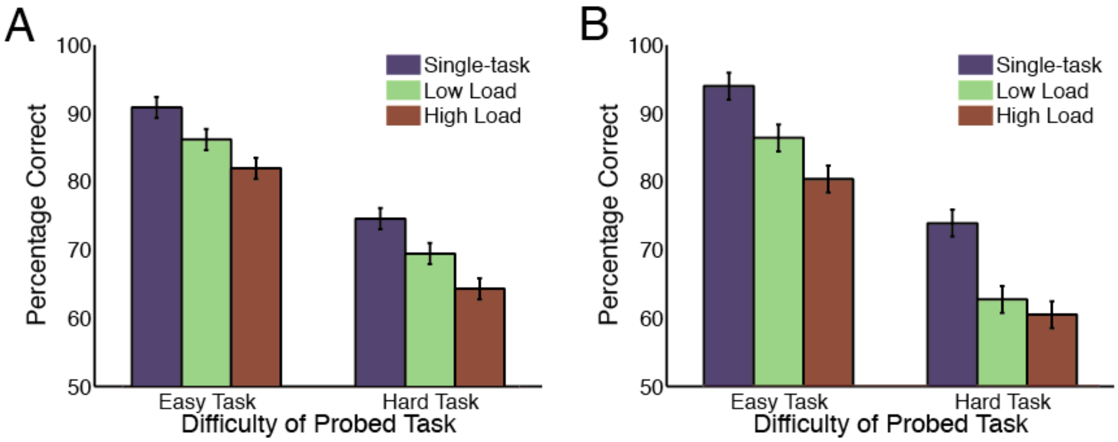

Figure 10: Task accuracy for the single-task conditions for the multi-modal (A) and uni-modal (B) experiments as a function of task difficulty and secondary task load. Error bars represent within-subject error of the main effect of load.

Experiment 2

Methods

A different set of fourteen participants (five male) were required to perform the gabor tracking task, the dot tracking task, or both tasks simultaneously. The trial phases of the gabor and dot tracking tasks overlapped (Figure 7), with single- and dual-task conditions differing only during the task-cue phase. The dot stimuli were always presented over the 1.5° x 1.5° gabor patch. On dual-task trials both the gabor and dot task-cue were presented (see individual task methods).

Results & Discussion

Single-task performance

Percent correct for the single-task conditions by task and difficulty, is shown in Figure 8B. Figure 9B shows the change in difficulty across blocks, represented as the rate of movement (in visual angle) per screen refresh (dot task) or the rate of change in orientation, color, and spatial frequency values (in degrees or APP) per gabor alternation (gabor task). To compare single-task performance across tasks, percent correct for single-task trials was entered into a 2 x 2 ANOVA with factors of difficulty (easy, hard) and task (gabor, tone). This revealed a main effect of difficulty ($F(1, 13) = 177.27$, $p < .001$), no main effect of task ($F(1, 13) = .52$, $p = .48$), and no interaction between task and difficulty ($F(1, 13) = .38$, $p = .55$). Thus, I can conclude that the two tasks were equivalently difficult for participants across both easy and hard difficulty conditions.

Comparison of dual-task versus single-task conditions

Figure 10B shows percent correct as a function of probed task difficulty and secondary task load. Percent correct was entered into a 2 x 3 ANOVA with factors of difficulty (easy, hard) and load (single-task, low, high). The results showed a main effect of difficulty, $F(1, 13) = 118.36$, $p < .001$, with worse performance for hard trials. Additionally, there was a main effect of secondary load ($F(1, 13) = 26.73$, $p < .001$), but no interaction between difficulty and load ($F(1, 13) = .72$, $p = .50$). A second analysis excluded single-task conditions to determine whether interference was load-dependent.

A 2 x 2 ANOVA with factors of difficulty (easy, hard) and load (low, high) found a main effect of difficulty ($F(1, 13) = 74.98$, $p < .001$), a main effect of secondary task load ($F(1, 13) = 5.20$, $p < .05$), but no interaction between the factors of difficulty and load ($F(1, 13) = .79$, $p = .39$). However, if the above ANOVA is restricted to only include data from the second session, no main effect of difficulty is observed ($p = .48$). This is likely due to a floor effect in performance during the hard difficulty trials as performance in the low load condition was less than 60% across the last three blocks. Predictably, this also shows an interaction between difficulty and secondary task load ($p=.02$) with a larger load effect in the easy difficulty condition. Thus, it is possible that the effect of secondary task load is underestimated in the hard difficulty condition.

The fact that the current study shows strong, load-dependent costs between two visual perceptual attention tasks is not surprising, and is consistent with previous studies (Lee, et al., 1999; Pastukhov, et al., 2008). However, this study goes beyond previous experiments by also manipulating attentional load. The results clearly show that dual-task costs increased with increased load. Thus, a novel contribution of this study is to rule out the possibility that previous evidence of interference between visual tasks is not simply due to executive demands required to coordinate two tasks or to load-independent representational interference.

Comparison of costs in Experiments 1 and 2

Dual-task costs were compared across Experiments 1 and 2 to test whether there were differences between the multi-modal and uni-modal task pairings. Task accuracy was entered into a between-subjects 2 x 2 x 3 ANOVA with factors of Experiment (multi-modal, uni-modal), difficulty (easy, hard) and load (single-task, low, high). There was a main effect of difficulty ($F(1, 26) = 208.31$, $p < .001$), a main effect of load ($F(2, 52) = 47.03$, $p < .001$), but no main effect of Experiment ($F(1, 26) = .63$, $p < .43$), and no significant interactions (all p's > .1). Thus, there is no evidence of a domain-specific contribution to dual-task costs between two tracking tasks when overall costs are examined. However, overall dual-task costs may emerge from both concurrence costs (measured by the difference between single-task and low-load dual-task performance) and load-dependent costs (measured by the difference between low load dual-task and high load dual-task performance). Concurrence costs may be influenced by a combination of competition for a shared, limited capacity, task coordination costs, and load-independent interference between tasks. Load-dependent costs measure the decrease in performance as load increases, and are thus relatively pure measures of competition between tasks because task coordination costs will be present in both the low load and high load dual-task conditions. These two forms of costs were independently compared across experiments.

To compare concurrence costs, participants' percent correct for the low load dual-task conditions were subtracted from performance in singe-task conditions (separately for each level of task difficulty). These costs were entered into a between-subjects 2 x 2 ANOVA with factors of Experiment (multi-modal, uni-modal) and difficulty (easy, hard). The ANOVA found a main effect of Experiment, $F(1, 26) = 4.21$, $p = .05$, with greater concurrence costs in the uni-modal condition, The results showed no main effect of difficulty ($F(1, 26) = 1.06$, $p = .31$), and no interaction between Experiment and difficulty ($F(1, 26) = .64$, $p = .43$).

To compare load-dependent costs, participants' percent correct for the high load dual-task conditions were subtracted from performance in low load dual-task conditions and entered into a between-subjects 2 x 2 ANOVA with factors of Experiment (multi-modal, uni-modal) and difficulty (easy, hard). The ANOVA found no main effect of

Experiment (*F*(1, 26) = .05, *p* = .82), no main effect of difficulty (*F*(1, 26) = .31, *p* = .56), and no interaction between Experiment and difficulty, (*F*(1, 26) = .80, *p* = .38). Results on both concurrence and load-dependent analyses were similar if the data set only included data from the second session. The only exception was that the ANOVA for concurrence costs found an interaction between the factors of difficulty and experiment (*F*(1, 26) = 4.52, *p* = .05) showing that domain-specific concurrence costs were greater in hard difficulty conditions.

Thus, while the overall ANOVA found no difference in costs between the uni-modal and multi-modal experiments, differences were revealed when tasks costs were separated into concurrence and load-dependent costs. Specifically, while load-dependent costs were found to be similar across both experiments, the concurrence costs were significantly higher in the uni-modal than multi-modal experiments.

Discussion of Chapter III

To explore the generality or specificity of perceptual attention, here I required participants to concurrently perform tracking tasks that shared or differed in modality. Significant dual-task costs were found between visual and auditory tracking tasks. Furthermore, these costs increased with dual-task load. Additionally, a comparison of concurrence and load-dependent costs between multi- and uni-modal task combinations found equivalent load-dependent costs. However, there was also evidence for a domain-specific contribution to perceptual tracking performance. While the overall concurrence costs certainly include a domain-general contribution (as costs were significant in both the uni-modal and multi-modal conditions), the fact that concurrence costs were greater in the uni-modal condition relative to the multi-modal condition suggests an additional *uni-modal specific concurrence cost*. These results suggest that perceptual attention performance is constrained by both domain-general and domain-specific sources. Furthermore, the results raise the interesting possibility that increasing attentional load affects performance in a domain-general way. This finding needs to be interpreted with some caution, however, as both uni-modal and multi-modal costs may have been underestimated in these studies, but for different reasons. One concern is that load-dependent costs may have been underestimated in the uni-modal condition due to a floor effect or a diminishing effect of withdrawing attention at low task performance rates. However, this would have had to significantly and disproportionably impact the uni-modal hard difficulty condition in order to explain the lack of a main effect of Experiment. It is also worth noting that while the lack of an effect of Experiment for load-dependent costs was a null result, costs were if anything slightly larger in the multi-modal condition. Indeed, Bayesian analysis (Rouder et al., 2009) on the comparison of costs across experiments (averaged across task difficulty; $t = -.23$, $p = .82$) suggests that the null hypothesis is 3.62 times more likely than the alternate hypothesis. While further research is necessary to refute the possibility that a floor effect caused the lack of an effect of Experiment for load-dependent costs, the current results argue that increased tracking speed draws (at least significantly) on an amodal source of capacity.

Load-dependent costs may also have been underestimated in the multi-modal condition since the single-task performance data suggested a weaker effect of secondary auditory task load (low task difficulty performance was worse than expected, and high task difficulty performance was better than expected). Note though that such effects go in the opposite direction necessary to account for the uni-modal specific concurrence cost and the domain-general load-dependent effect. Thus, this effect cannot explain the current data, but suggests caution in designing an auditory tracking task where alternation rate is manipulated.

Why might domain-specific costs not increase under more difficult tracking conditions? One possibility is that domain-specific costs arose from an inconsistency in the spatial window of visuospatial attention required by the dot and gabor tasks. Tracking a target dot requires focal attention (Alvarez & Cavanagh, 2005) whereas attending to the gabor might require a broader attentional window to perceive the orientation and spatial frequency. Previous experiments on the extent of spatial attention have suggesting that while the spatial extent of attention is adjustable, it cannot be shrunk to less than 2° x 2° of visual angle (Eriksen and St. James, 1986). The spatial extent of the gabor patches in the current study (1.5° x 1.5° of visual angle) were smaller than this minimum window of attention. Therefore, there is reason to suspect that similar attentional windows were adopted in single-task and dual-task conditions, although the possibility that costs arose from differences in the attentional focus across conditions (which would occur in dual-task conditions but not increase with secondary task load) cannot be completely ruled out.

Another possibility is that the domain specific costs arise from the need to maintain and differentiate modality-specific object representations. The number of representations increased between single- and dual-task conditions, but not with increased task load. This makes the speculative prediction that domain-specific costs will increase if tracking set size, rather than tracking difficulty, is manipulated. The source of these domain specific costs may be, in the case of interference between visual tasks, in posterior regions of parietal cortex (Shim, Alvarez, Vickery, & Jiang, 2009). While posterior parietal cortex has been shown to be critically involved in multiple object tracking (Culham, Cavanagh, & Kanwisher, 2001; Jovicich et al., 2001), it has recently been shown that activity in this region is not modulated by tracking speed, even though tracking speed substantially affected performance (Shim et al., 2009). In contrast, manipulations of tracking set size modulate activity in posterior parietal cortex (Culham et al., Jovicich et al., 2001; Shim et al., 2009). While strong evidence points to the critical role of the posterior parietal cortex in visuospatial tracking, the degree to which it contributes in a domain-specific fashion is not yet known.

Alternatively, it may be possible to explain the uni-modal specific concurrence cost as due to interference or representational overlap in early visual areas. Increasing the tracking rate of stimuli may not exacerbate the overlap in representational demands in early visual areas. Instead, increasing tracking speed may increase the speed at which information in early visual / auditory needs to be communicated to brain regions for further processing. If the demands imposed by increased tracking speed draw selectively on processing stages that occur after representation in domain-specific sensory cortex this could explain the lack of domain-specific effects found for increasing task load.

The current study was motivated by the inability of previous dual-monitoring studies to rule out participant strategies that allow time-sharing of attention as explanations for the lack of multi-modal costs. The findings of significant and load-dependent costs in the current study suggest that the lack of costs with these tasks should be reconsidered. However, the findings do not necessarily imply that previous findings were due to these concerns. Indeed, there are other factors that need to be considered. Dual-monitoring studies involve measuring psychophysical thresholds for either detecting or classifying target stimuli or detecting targets among a stream of distractors (e.g. Alais, et al., 2006; Duncan, et al., 1997). It may be that such tasks draw on different types of perceptual demands. Both tracking tasks and measurements of psychophysical thresholds are considered to draw on perceptual attention (Alvarez & Cavanagh, 2005; Lee, et al., 1999; Pastukhov, et al., 2008; Scholl, 2004). However, perceptual attention may not represent a monolithic capacity, and there may be distinct mechanisms of attention at different stages of perceptual processing (Kastner & Pinsk, 2004). Indeed, the tracking tasks require the integration of sensory information, often across multiple sources sensory information (e.g. color and orientation; stereo position and pitch), into a perceptual object that must be individuated from a distractor object. In contrast some studies showing distinct perceptual processing for auditory and visual tasks have measured discrimination thresholds for basic visual or auditory attributes (such as brightness or pitch). These same sensory attributes when presented in the context of a visual search task have been shown to 'pop out', meaning that the number of distractors does not matter (Treisman & Gelade, 1980; Wolfe, 1994). These differences across tasks suggest the possibility that psychophysical measures of discrimination are due to modulation of activity in neurons that preferentially code a specific sensory attribute (possibly as early as the lateral geniculate nucleus for visual stimuli; O'Conner, Fukui, Pinsk, & Kastner, 2002), whereas tracking is mediated by competition among objects for representation in higher areas of visual / auditory cortex (Kastner & Pinsk, 2004; Kastner & Ungerleider, 2000, 2001).

It is also possible that dual-monitoring studies draw only on perceptual attention while tracking tasks also draw on central attention, here defined as the limitations in selecting among competing post-perceptual representations (Eriksen & Eriksen, 1974; Stroop, 1935). Indeed, past studies have shown that central attention tasks can interfere with participants' tracking ability (Allen, et al., 2006; Kunar et al., 2008; Tombu & Seiffert, 2008). However, interference between central attention and perceptual attention tasks are not limited to tracking, but occur in a wide variety of tasks including visual search, navigation, target detection, and the detection of unexpected stimuli (Fougnie & Marois, 2007; Han & Kim, 2004; Strayer & Drews, 2007). Therefore, such findings suggest that distinctions between perceptual and central attention may not be as complete as suggested by past evidence and theoretical attention typologies (Giesbrecht, et al., 2001; Johnston et al., 1995; Lavie et al., 2004; Pashler 1989; 1991; Posner & Peterson, 1990; Vogel et al., 2005), but do not provide strong evidence that tracking draws to a greater extent on central attention than in dual-monitoring tasks. Regardless of the reason that the current study provides a different pattern of result as those of past studies, the finding of strong, load-dependent costs between an auditory and visual tracking task places limits on the conditions under which domain-specificity in perceptual processing can be observed.

CHAPTER IV

CONCLUSION

The degree to which attention and WM capacity limits depend on the nature of the stimuli being processed is a fundamental question about our cognitive architecture as it addresses the issue of the generality or the specificity of the processes that constrain our ability to represent our environment. Do such stark limits, demonstrated by our failure to notice drastic changes in our environment over time and space (Simons & Levin, 1997; Rensink, 2002; Scholl, 2004), occur because of competition for a single, capacity-limited process? Previous experimental findings offer no clear consensus and theoretical models provide conflicting answers (Baddeley, 1986; Cowan, 1995; Duncan, 1994). The goal of this research was to examine, within the context of both attention and working memory, the magnitude of dual-task costs between two sets of stimuli that differed in their modality and task-relevant feature(s). I addressed this issue with a manipulation of task load, as load-dependent increases in dual-task costs are difficult to explain by costs in dual-task coordination or perceptual interference and therefore provide strong evidence for competition between tasks.

In Chapter 2 it was found that auditory and spatial WM loads could be maintained independently, with no interference between modalities even when load was sufficiently high such that each task was beyond single-task capacity. Several alternate explanations for the costs were ruled out. Experiments 2-4 suggested that a lack of costs wasn't due to articulatory rehearsal of the auditory stimuli. Experiments 5 & 6 refuted the possibility that long lasting sensory traces of the stimuli were eliminating the need for storage in WM. Together, these results strongly argue that storage in WM is mediated largely by domain-specific systems.

The experiments in Chapter 3 examined the domain-generality versus domain-specificity of our limited perceptual attention. In contrast to the findings of the previous chapter, strong dual-task costs were observed when auditory and visual tracking tasks were performed concurrently. Furthermore, such costs were shown to be load-dependent—they increased with increased secondary task difficulty. These multi-modal costs were then compared to a uni-modal condition that paired two visual tasks: a gabor tracking task and a multiple object tracking task. The overall dual-task costs did not differ across multi- and uni-modal conditions. A subsequent analysis separately examined concurrence and load-dependent costs. Concurrence costs were significantly higher in the uni-modal task combination, suggesting the contribution of a domain-specific source of interference when tracking two visual stimuli (in addition to domain-general costs that were equivalent in both experiments). In contrast, load-dependent costs were equivalent in the multi- and uni-modal conditions. While it is possible that the design was not sensitive enough to detect differences in load-dependent costs across experiments, the lack of any trend in this effect suggests that the costs of increasing tracking difficulty originate largely from domain-general sources.

In trying to synthesize these two findings, the contrast in results for working memory and attention is striking. Indeed, it would be difficult to accommodate these findings within a framework that suggests that capacity limits in attention and WM have a single underlying source (Cowan, 1995, 2006; Duncan, 1994; Rensink, 2002). Instead, the dichotomous results are consistent with the idea that the capacities for attention and working memory originate from at least partially dissociable processes (Fougnie & Marois, 2006). Our ability to maintain items in WM seems to originate from domain- and perhaps stimulus-specific storage systems. In contrast, our ability to selectively attend to some aspects of our environment depends at least partially on domain-general capacities. These results provide support for frameworks with independent processing and storage mechanisms (Baddeley & Logie, 1999; Duff & Logie, 2001; Kane et al., 2004; McElree, 2001; Oberauer, 2002; Oberauer et al., 2001). In this framework, the term processing is linked to mechanisms of attention and the processes that update and manipulate working memory representations. Storage refers to the short-term maintenance of information in the absence of processing. Storage may be mediated by specialized rehearsal mechanisms (Baddeley & Logie, 1999), perhaps implemented through self-sustaining neural activity in cell assemblies that code for a particular stimulus attribute (Funahashi & Inoue, 2000; Hebb, 1949). The current findings are consistent both with neurocomputational models of WM that describe how representations can be sustained by recurrent excitation in a neural network (Amit, et al., 1994; Hopfield, 1982) and with experimental evidence showing that active rehearsal is not necessary for maintenance in VWM (Washburn & Astur, 1998),

Processing and storage may be mediated by distinct mechanisms and serve distinct functional goals (Woodman & Vogel, 2005). Indeed, consider if the only information we were able to store was that which we were currently processing. Under such conditions, every time an unexpected event or object were to grab our senses, whatever was previously attended would be lost or would have to be retrieved from long-term memory. Thus, it doesn't seem ideal that the contents of WM would be solely constrained by attention (Rensink, 2002). More sensibly, WM can be considered a temporary but highly accessible store where important information can reside while the contents of attention shift to some highly salient event, or to multiple steps of a complex problem. Indeed, while withdrawing attention can interfere with the storage of information in VWM (Matsukura & Vecera, 2009; Oh & Kim, 2004; Woodman & Luck, 2004), such costs can be explained by representational overlap. Indeed, interference does not occur when the attention and VWM task differ in task-relevant features (Matsukura & Vecera, 2009; Woodman, et al., 2001).

The lack of interference between auditory and spatial WM tasks reveals much about the mechanisms that allow for the temporary storage of perceptual information. These findings are consistent both with models that propose distinct storage structures for modalities (Baddeley, 1986; Baddeley & Logie, 1999; Kane et al., 2004; Scarborough, 1972) and those that argue that a major limitation in WM is due to representational interference among stored stimuli (Johnson, et al., 2009; Oberauer & Kliegl, 2001, 2006; Olson & Poom, 2005). However, costs between auditory and visuospatial WM loads can interfere under some conditions. Indeed, while dual-task costs were not observed in Experiments 1-6 & 9 of Chapter 2, significant costs were found in Experiments 7 and 8. The source of these costs was attributed to the requirement to bind features into coherent objects. Feature binding is an attentionally demanding process (Treisman & Gelade, 1980) and may draw on amodal sources of processing. WM tasks that require the updating or manipulation of the contents of WM may also lead to domain-general costs (Garavan, 1998; McElree, 2001; Postle et al., 2005; Oberauer, 2001, 2002; but see Mohr & Linden), as may tasks that require the selection of target stimuli among distractors (Vogel, McCollough, & Machizawa, 2005).

Perceptual tracking revealed a cognitive capacity that is at least partially distinct from the demands posed by the WM tasks. The theories that propose distinct storage and processing mechanisms are both underspecified and inadequate to fully explain perceptual tracking capacity. Processing is typically defined as an amodal, undifferentiated resource (Duff & Logie, 2001; Oberauer, 2002), but the term lacks the precision to define what does and what does not require processing. In addition, the source of both domain-general and domain-independent costs in perceptual tracking shows, in conjunction with evidence for multiple forms of attentional selection (Giesbrecht, et al., 2001; Johnston et al., 1995; Lavie et al., 2004; Pashler 1989; 1991; Vogel et al., 2005), that our attentional limits are constrained by a complex cognitive architecture not easily explained by such frameworks.

The strong costs between auditory and visual tracking tasks differ from the findings of previous studies that rely on dual-monitoring tasks. This difference in findings could have occurred because past paradigms were not ideal for revealing limits in perceptual capacity or because tracking and monitoring draw on distinct limits of selection. The present results are insufficient to provide a definitive answer to the source of difference in findings across paradigms. However, the fact that increasing tracking speed led to a domain-general cost in performance suggests that a manipulation of the perceptual difficulty of a tracking task can draw on amodal sources. Future work is necessary to build on this finding and rule out alternate explanations. However, evidence for both domain-specific and domain-general sources of tracking capacity is consistent both with single resource theories that allow for potential structural interference within a modality (Kahneman, 1973) and with theories that posit a single domain-general source of attentional control that acts to modulate perceptual processing in modality-specific sensory cortex (Johnson, et al., 2005; Laurienti, et al., 2002).

A goal of the present work was to help constrain theory by exploring the generality and specificity of both attention and working memory. The results provide further evidence against a single, common capacity that underlies attention and working memory (Cowan, 2006; Rensink 2002). Instead, the results suggest that our limited capacity in representing our environment is due to the interplay of several factors.

Whether dual-task results reveal domain-general or domain-specific costs may depend on whether the tasks draw on processing or storage and the stage(s) of processing where task demand exceeds capacity. While current theoretical frameworks predict and explain some of the above distinctions, no framework encompasses them all.

The dual-task paradigm provides a powerful tool to examine the overlap in cognitive processes between two tasks. However, care is needed in designing appropriate dual-task designs. One must consider whether there is a source of costs ancillary to the goal of the study that could impact the results. For example, if an auditory and visual task both use spatial coordinate frames, interference between these tasks may reflect representational costs rather than competition for a limited capacity process. Additionally, a lack of costs may not be sufficient to conclude no overlap between tasks unless sufficient care is taken to ensure that overall difficulty is sufficient and that performance cannot be assisted by some other process. Finally, rather than simply including single- and dual-task conditions, it is ideal to also increase the dual-task load. This load manipulation provides a way to measure dual-task costs that cannot be due to the executive demands of performing two tasks. In interpreting the dual-task costs it is important to have a good baseline for comparing a lack of costs (usually a single-task condition) and a putative ceiling where extensive overlap should occur (in the current studies this was the uni-modal condition in Chapter 3, and the set-size manipulation in Chapter 2). This allows cross-experiment comparisons to measure the extent of costs. While the findings of Chapter 2 and 3 are important within the context of attention and working memory, the overall framework of dual-tasking that was developed here should be valuable to many fields that examine the generality versus specificity of mental faculties in topics ranging from intelligence (Jensen, 1998; Spearman, 1927) to long-term memory (Toth & Hunt, 1999; Tulving, 1985).

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