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Citation for published version:
Morey, CC, Morey, RD, Van Der Reijden, M & Holweg, M 2013, 'Asymmetric cross-domain interference between two working memory tasks: Implications for models of working memory', *Journal of Memory and Language*, vol. 69, no. 3, pp. 324-348. https://doi.org/10.1016/j.jml.2013.04.004

Digital Object Identifier (DOI):
10.1016/j.jml.2013.04.004

Link:
Link to publication record in Edinburgh Research Explorer

Document Version:
Peer reviewed version

Published In:
Journal of Memory and Language

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Please cite this manuscript as:

Morey, C. C., Morey, R. D., Van Der Reijden, M., & Holweg, M. (2013). Asymmetric cross-domain interference between two working memory tasks: Implications for models of working memory. *Journal of Memory and Language, 69(3)*, 324-348. 10.1016/j.jml.2013.04.004

Word count: 13,408 (Abstract: 117; 65 references)

Date submitted: 23 April 2012

Date re-submitted: 24 December 2012

Date of final resubmission: 22 April 2013

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This research was completed in partial fulfillment of the requirements for the master's degrees of M. van der Reijden and M. Holweg. Thanks are due to Andrea Winkens for her preliminary data collection and analysis on this topic, and to Malte Bieler, Christian Hummeluhr, Irene Krap, and Yixia Zheng for assistance with data collection. We thank Nelson Cowan, Robert Logie and Pierre Barrouillet for helpful remarks on an earlier version of this manuscript. WoMMBAT software is freely available from http://cran.r-project.org/ as package WMCapacity.

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Abstract

Observations of higher dual-task costs for within-domain than cross-domain task combinations constitute classic evidence for multi-component models of working memory (e.g., Baddeley, 1986; Logie, 2011). However, we report an asymmetric pattern of interference between verbal and visual-spatial tasks, such that imposing a verbal memory load provokes graded decreases in visual memory performance, but imposing a visual memory load does not much affect verbal memory performance. Across multiple experiments, we verify that this pattern cannot adequately be explained as a mere byproduct of stimulus recoding or strategic preference. Current working memory models do not predict this persistent finding, thus a change in ongoing debate about relationships between attention and maintenance of verbal and visual mental representations is necessary.

Keywords: working memory, attention, verbal short-term memory, visuo-spatial short-term memory, visual short-term memory

Asymmetric cross-domain interference between two working memory tasks: Implications for models of working memory

Despite decades of effort, research aimed at understanding whether mental representations are held in separate storage buffers according to different codes remains inconclusive. Various models of working memory make conflicting claims about how information is mentally represented. Some models claim that information of different codes or modalities is represented separately in independently-functioning storage buffers, which are also distinct from domain-general attention (Baddeley, 1986, 2007, 2012; Baddeley, Allen, & Hitch, 2011; Baddeley & Logie, 1999; Logie, 2011; Repovš & Baddeley, 2006). Other models propose that a common attentional resource is shared across stimulus domains, making no explicit separation between acoustic and visual memory representations (Barrouillet, Bernardin, Portrat, Vergauwe, & Camos, 2007; Cowan, 2005; Oberauer, 2009). Although many empirical studies have addressed this issue, evidence remains inconclusive enough to allow drastically different interpretations, resulting in these conflicting theories.

The aim of this research is to re-examine the evidence supporting proposals of separate, domain-specific storage modules in working memory. Our analysis begins with identification and description of an asymmetric pattern of cross-domain interference, in which verbal stimuli interfere with visual memoranda to a greater extent than visual stimuli interfere with verbal memoranda. This pattern occurs persistently (e.g., Meiser & Klauer, 1999; Shah & Miyake, 1996; Vergauwe, Barrouillet, & Camos, 2010) but has typically been ignored or dismissed post-hoc, or at most provoked only brief discussion. The need to dismiss this pattern indicates its potential theoretical importance; the modular working memory models described by Baddeley and colleagues (e.g., Baddeley, 2012; Logie, 2011) do not propose any process that predicts that storage of some kinds of information would require more domain-general attention than others. We then present new findings, in which we replicate this asymmetric pattern and eliminate theoretically trivial explanations of it. Finally, we evaluate our evidence in light of various theoretical frameworks, considering whether these existing frameworks can adequately handle this pattern of interference without substantial changes. We conclude that while some models might accommodate our findings by making new explicit assumptions that do not contradict their current assumptions, the integrity of the multi-component model of Baddeley (2007) is threatened by this particular pattern of cross-domain interference.

1.1. Domain-specificity and domain-generality in working memory

A classic approach to understanding resource sharing in working memory is to compare memory performance under various dual-task conditions. If two tasks require the same resource, then performance should deteriorate when those two tasks are performed simultaneously compared to single-task performance or to a situation in which two tasks relying primarily on different modules are performed simultaneously. If two tasks depend on separate resources, then performance might not decrease much when they are performed together. Many studies have been designed to compare these two scenarios directly, manipulating the stimulus domain of the tasks so that in one case, two visual or two verbal tasks are performed, whereas in another, a visual and a verbal task are performed together. Sometimes significantly more dual-task interference is observed when two tasks from the same domain are performed together than when two tasks from separate domains are performed together (e.g., Cocchini, Logie, della Sala, MacPherson,

& Baddeley, 2002; Logie, Zucco, & Baddeley, 1990; Meiser & Klauer, 1999). Observing such an interaction is considered the gold standard for declaring that visual and verbal tasks make use of domain-specific working memory modules.

In some cases, surprisingly little interference is found under dual-task circumstances, an observation that inspired assumptions of modularity. Baddeley and Hitch (1974) paired verbal memory loads comprising varying numbers of digits with verbal reasoning or comprehension tasks varying in difficulty. They observed the largest decreases in reasoning or comprehension performance when a 6-digit memory load was recited aloud throughout a trial, with little or no decrease observed with smaller, unrecited memory loads. However, the dual-task impairments caused by reciting a near-capacity memory list were rather modest, not nearly as devastating as might be expected if only a single pool of resources were required to carry out both tasks. This evidence suggests that multiple resources support online cognition. Baddeley (1986) proposed that these multiple resources included a verbal short-term memory system capable of storing and rehearsing acoustically-represented information, a visual short-term memory system for visual and spatial representations, and the central executive, which was not capable of storing information, but whose processing limits applied to both the acoustic-phonological and visual-spatial sub-systems.

Consistently with Baddeley's (1986) expectations, deficits are typically observed when two tasks are performed simultaneously, regardless of the domain of the memoranda involved, but deficits observed with two tasks from the same domain are usually larger. However, only a handful of studies boast truly compelling double dissociations with larger domain-specific than cross-domain costs to accuracy (e.g., Cocchini, et al., 2002; Logie, et al., 1990; Meiser & Klauer, 1999; Salway & Logie, 1995; Shah & Miyake, 1996). Though these examples clearly suggest that any adequate model needs an explanation for why within-domain interference exceeds cross-domain interference, even some of these strong examples do not clearly show that cross-domain interference is trivial. For instance, Logie et al. (1990) observed cross-domain dual-task costs of 15-20% with respect to single-task performance. Thus, cross-domain interference clearly occurs, and must also be explained somehow by any comprehensive working memory model.

Observations of symmetric cross-domain interference are not necessarily problematic for multi-component models, which predict interference in at least two ways. Cross-domain interference could arise because stimuli presented as visual images might be recoded verbally for maintenance or vice versa. This process could induce some cost (Brandimonte, Hitch, & Bishop, 1992) and also cast doubt on whether visual-spatial and verbal maintenance were truly undertaken at the same time. However, cross-domain interference has been observed between tasks where precautions against stimulus recoding were taken (e.g., Jones, Farrand, Stuart, & Morris, 1995; Morey & Mall, 2012), so it is unlikely stimulus recoding can account for all such instances. More pertinently, it is difficult to assert that any task relies exclusively on a domain-specific module, so one might reasonably suppose that cross-domain interference arises because performing two tasks at once requires coordination from the domain-general central executive because a concurrence cost is induced (Navon & Gopher, 1979); such a cost would especially be expected whenever two juxtaposed tasks are difficult (Logie, 2011). Critically, Logie (2011) explains that this expected cost could occur to either the verbal or the visual-spatial task. Given that multi-component models suppose that a domain-general processor assists with both verbal and visual-spatial storage, one would expect to see symmetric decreases in dual-task compared to single-task performance in which a cost is experienced in both juxtaposed tasks, or at least to see inconsistent asymmetric patterns across many experiments because differing task demands or participant preferences are present and induce differing task prioritization.

However, a review of dual-task research in working memory reveals that asymmetric cross-domain interference in which visual memoranda are impaired more than verbal memoranda is typical, and has not been adequately explained. For example, Meiser and Klauer (1999) observed differential effects of concurrent rehearsal suppression tasks on verbal and spatial serial memory depending on whether the suppression tasks were performed during encoding or during retention. When performed during encoding, only same-domain rehearsal suppression impaired performance, contrary to Jones et al. (1995). However when performed during retention, a more complex pattern emerged: for spatial memory, both articulatory suppression and spatial tapping impaired recall, but for verbal memory significant interference occurred only with concurrent articulation. This outcome highlights two important findings: first, the maintenance period of a task is especially sensitive to disruption, and second, that verbal and spatial maintenance seem to be differentially affected by concurrent processing.

The observation that maintenance of visual images or spatial locations is more sensitive to interference than maintenance of phonological information could possibly explain why cross-domain costs are observed in some instances but not in others. But it is difficult to see how such an assumption could be clearly instantiated in the multi-component models of working memory so that this prediction is actually represented by the model. The assumption predicts greater interference to visual memory from verbal memory, yet it is not clear how this outcome could be represented by adding or removing a component, or by simply changing the relationships between components. Perhaps verbal materials have extra possibilities for rehearsal, unavailable for mentally representing visual images, which can and should be instantiated in models of working memory. This possibility has been explicitly proposed (Camos, Lagner, & Barrouillet, 2009; Hudjetz & Oberauer, 2007), but much past evidence of asymmetric interference that might support it and provide strong evidence for further theorizing has been overlooked. Next, we reconsider this neglected evidence and its implications.

1.2. Diverse evidence of an asymmetric relationship between verbal, visual-spatial, and domain-general resources

In two similarly designed studies measuring working memory using complex span tasks, Shah and Miyake (1996) and Vergauwe, Barrouillet, and Camos (2010) reported asymmetric patterns of interference between verbal and spatial storage and verbal and spatial processing. Vergauwe et al. created complex working memory span tasks including each combination of verbal and visual storage and verbal and visual processing, and manipulated the expected cognitive load incurred by the processing component of the task by varying the number of responses and the length of time interpolated between the presentations of the to-be-remembered stimuli. For both the verbal and the visual memoranda, memory capacity decreased as cognitive load increased, regardless of the domain of the processing task. However, an interaction between the domain of the memoranda, domain of the processing stimuli, and cognitive load suggested that processing visual-spatial items did not impair verbal memoranda as much as processing verbal items, while visual memoranda seemed to be equally impaired by both processing tasks. Vergauwe et al. emphasized that because interference was observed in each combination of tasks, it must have some domain-general, central source. Shah and Miyake observed a similar pattern when comparing combinations of verbal and spatial storage and processing tasks, but attributed it to probable verbal re-coding of visual stimuli, suggesting that if visual stimuli were maintained in visual code, the asymmetric interference would not occur.

Concurrent maintenance of verbal and spatial lists also results in substantial dual-task impairments (Depoorter & Vandierendonck, 2009), which also show an asymmetric tendency (Morey & Mall, 2012). Morey and Mall measured serial reconstruction for lists of words and lists of spatial locations randomly selected from an unstructured viewing area. They found that concurrently maintaining a verbal and a spatial list resulted in significant decreases in performance compared to conditions in which one of the two lists could be ignored. Interestingly, serial position functions revealed that though this dual-task cost was present for the early-list items for both types of memoranda, the final verbal item was not impaired by concurrent maintenance of a spatial list, whereas the final spatial item was impaired by maintenance of a verbal list. Adding a suffix after presentation of the final items seemed to induce symmetry in the dual-task costs, with the final verbal item also suffering. These intriguing results suggest that while dual-task costs occur, verbal information does not always suffer from them. Cost-free maintenance of the final item in a list plausibly reflects the operation of a domain-specific verbal module, but for spatial items, Morey and Mall never observed this preservation.

Evidence of an asymmetric relationship between verbal and visual working memory storage is also consistent with relationships observed using other empirical techniques. Latent variable analyses suggest that visual-spatial short-term memory might be more closely related to executive functions or attention than verbal short-term memory. Miyake and colleagues (Miyake, Friedman, Rettinger, Shah, & Hegarty, 2001) examined a battery of visual-spatial tests designed to measure short-term memory, working memory, executive functioning, and domain-specific spatial abilities. They tested a three-factor model supposing that three latent variables (visual-spatial short-term memory, visual-spatial working memory, and executive functioning) supported observed relationships between the tasks meant to represent them. In fact, the latent variables for visual-spatial short-term and working memory were highly correlated (r=.86) and each of these correlated equally well with the latent variable representing executive functioning. Miyake and colleagues considered this outcome in light of research in which verbal short-term memory measures are less strongly related to intelligence than visual short-term memory measures (Conway, Cowan, Bunting, Therriault, & Minkoff, 2002), and concluded that although simple verbal storage might be separable from executive functioning in working memory, simple spatial storage was closely related to executive functioning (though see also Bayliss, Jarrold, Gunn, & Baddeley, 2003, who support three distinct components). Subsequent research supported a model in which spatial storage could be separated from executive processes (Kane, Hambrick, Tuholski, Wilhelm, Payne, & Engle, 2004), but this analysis also revealed a stronger relationship between short-term and working memory measures in the spatial than in the verbal domain, as well as a stronger relationship between spatial short-term memory and fluid intelligence than verbal short-term memory and fluid intelligence. Altogether, this evidence favors the notion that spatial memory is more strongly related to general factors than verbal memory is.

To summarize, there are many empirical reasons to predict an asymmetric pattern of interference effects between verbal and visual maintenance, and to suppose that this asymmetry is a fundamental phenomenon that must be explained as models of working memory develop, rather than a nuisance that can be dismissed post-hoc. This asymmetric pattern would pose a serious challenge to the structure of modular models and also is not explicitly predicted by most other models of working memory. However, the evidence of asymmetric interference between acoustic-verbal and visual-spatial memory tasks that we described above has all involved visual-spatial sequential memory (e.g., Meiser & Klauer, 1999; Morey & Mall, 2012; Shah & Miyake, 1996; Vergauwe et al., 2010). This evidence is difficult to interpret because it is plausible that the sequential visual stimuli could have been verbally recoded. Alternatively, it would be reasonable to suppose that a sequential visual memory task would not be accomplished only with domain-specific visual-spatial resources, on the grounds that it has been shown that binding serial order to item identity poses a greater burden for visual than verbal lists (Gmeindl, Walsh, & Courtney, 2011). The strength and generalizability of this pattern would be bolstered if it were also observed using simultaneous visual memory tasks, which are now frequently used to measure visual memory capacity (e.g., Todd & Marois, 2004; Vogel, Woodman, & Luck, 2001; Xu & Chun, 2006) and have been previously shown not to rely on verbalization (Morey & Cowan, 2004).

1.3. Predicting and characterizing asymmetric interference

We carried out a series of studies specifically designed to test whether visual and verbal memory tasks interfere with each other in this asymmetric manner, and particularly whether this interference occurs during simultaneous maintenance of verbal and visual memoranda. In each study, we estimated memory capacity for verbal and visual information using recognition tasks that involved memory for items in their serial or spatial context. We chose to juxtapose color-position and digit-order recognition because previous work identifying an asymmetric pattern of cross-domain interference used serial verbal and spatial memory tasks, but interpretation of these results is made ambiguous by the possibility of verbal recoding. Objections can also be raised toward juxtaposing simultaneously-presented sounds and images. Simultaneously presented auditory stimuli coming from multiple spatial locations (such as those used by Fougnie and Marois (2011) and Saults and Cowan (2007) in Experiments 1-4) would include spatial information, as simultaneously-presented visual array stimuli do. Interference between two tasks that both require maintenance of spatial locations might occur because of limits to how much spatial information can be maintained at once. Inasmuch as our goal was to document interference between representations stored in different codes, we strived to minimize potential overlap between features across stimulus sets.

The tasks we juxtapose here are thus likely to encourage mental representation in code matching the presentation modality, with minimal overlapping feature dimensions, and are also representative of tasks frequently used to measure verbal and visual memory. Combining these tasks could plausibly produce three alternative outcomes: 1) little or no interference between concurrent verbal and visual maintenance, 2) equally large interference to verbal memory from visual memory as from visual memory to verbal memory, or 3) the asymmetric pattern described above, in which concurrent visual memory declines with a concurrent verbal task, but verbal memory is relatively preserved.

In order to isolate interference due to concurrent storage as much as possible, we used the retro-cue experimental design of Cowan and Morey (2007), which manipulates the order of two stimulus presentations and compares conditions in which the tested set is cued early in the retention interval with conditions in which the tested set is unknown until the probe appears. This design allows for clearer isolation of interference that occurs due to maintaining two stimulus sets from interference attributable to encoding one set while maintaining another. Considering the research described above (particularly Meiser & Klauer, 1999), we believed that separating interference due to stimulus encoding from interference due to concurrent maintenance would yield clearer evidence about whether observed interference is due to unequal resources for maintenance or unequal resources for some other cognitive process. Experiments 1a, 1b, 2a, and 2b used Cowan and Morey's retro-cue design for this purpose, and in each experiment observed that maintaining verbal memoranda interferes with maintaining visual-spatial memoranda, but never found convincing evidence that verbal memoranda were impaired specifically by the maintenance of visual-spatial information.

Finally, we manipulated the relative amount of reward assigned to each task to simulate voluntary attention allocation toward one stimulus set and away from another. We did this to explicitly test the hypothesis that verbal storage is preserved from cross-domain dual-task interference because participants tend to emphasize the verbal task, perhaps due to a learned preference for serial verbal rehearsal. Again, we found that maintaining a set of verbal stimuli impaired visual memory maintenance. The effects of the reward manipulation revealed that while attention can be allocated in a graded fashion to visual-spatial memoranda, for verbal memoranda it seemed to be applied in an all-or-none manner. In the General Discussion, we consider how various models of working memory could be made to account for this pattern of results.

2. Experiments 1a and 1b

2.1. Method

2.1.1. Participants. Participants from the student population at the University of Groningen chose to take part in Experiment 1a in partial fulfillment of a course requirement. After excluding one participant for near-chance performance in the easiest visual condition, this sample included 14 males and 12 females, leaving N=26. Age ranged from 19 to 32 (M=21.65, SD=2.94).

New participants from the same student population completed Experiment 1b. Three participants were excluded from analyses: two on suspicion of colorblindness (each made 2 errors out of 6 Ishihara test trials), and one for near-chance performance in the easiest visual condition. The remaining sample included 5 males and 19 females, for N=24. Ages ranged from 18-25 (M=20.96, SD=1.71). All participants in both sub-experiments reported normal vision and hearing.

2.1.2. Apparatus and Stimuli. The experiment was executed with E-Prime (Schneider, Eschmann, & Zuccolotto, 2002). Each participant was tested in a quiet room equipped with a personal computer and headphones. Each session began with a short version of the Ishihara (1966) color test, using 6 plates requiring single-digit responses.

2.1.2.1. Visual array probe recognition task. Visual stimuli were displayed on a 19-inch color monitor at a viewing distance of approximately 50 cm. Visual arrays included 2, 3, 4, or 6 squares (0.65˚ x 0.65˚) arranged randomly on a neutral gray background, each with a color selected randomly without replacement from one of nine easily discriminable colors (red, blue, violet, green, yellow, light green, cyan, magenta, or white). The items in the arrays were separated by at least 2˚ of visual angle, measured from the center of the squares. Arrays were presented for 500 ms, and then masked by the presentation of 4x4 square patterns in the same configuration as the study array including all the colors in the set. At test, a single colored square appeared onscreen, which was either the same color as that square was during study or was changed to a different color that also appeared at study. Gray squares outlined in black appeared in all the positions occupied by a studied square, in order to offer contextual support for the judgment and reduce the possibility that participants were unsure which square was being probed. Because a change to an unstudied color was not possible, it was necessary to remember where each studied color was located, not only which colors were presented.

Experiments 1a and 1b differed from each other in only one respect. In Experiment 1a visual arrays could include 0, 3, or 6 items. However, capacity for visual items is typically thought to be limited to 3-4 items (e.g., Cowan, 2001; Vogel, Woodman, & Luck, 2001). For many of our participants, both 3- and 6-item visual arrays might have met or exceeded working memory capacity, possibly restricting the effect of visual memory load on verbal task performance. In Experiment 1b, we therefore included smaller visual arrays of 2 or 4 items, in order to see whether clear effects of visual storage on verbal storage could be observed in that range.

2.1.2.2. Verbal sequence probe recognition task. Each aurally-presented list of 3 or 6 digits was drawn randomly without replacement from the digits 1-9 (spoken in a male voice). Each individual digit fit within a 500-ms window. A 500-ms suffix including all the digits sounded 500-ms after the offset of the final digit in the list. At test, a single digit appeared onscreen with underscores denoting positions in the sequence (see Figure 1). Digits appeared either in the position in which they had been presented or in another randomly-selected position.

2.1.3. Procedure. An experimenter personally guided each participant through instructions and an 8-trial practice session. Figure 1 depicts the events in a dual-task trial. Participants observed verbal and visual stimulus presentations (presentation order was randomized within-participants). After the offset of the mask or suffix, a cue appeared. If a question mark appeared only in the upper box, then the first stimulus set would be tested. If a question mark appeared only in the lower box, the second stimulus set would be tested. These informative cues always accurately predicted the upcoming test stimulus. In some trials, question marks appeared in both boxes, and in these cases, either stimulus set could be tested after a 3000-ms delay. Single-task trials for both the verbal-sequential and the visual array task were also mixed into these sessions. For these trials, we varied the interval between offset of the study stimulus and onset of the test stimulus so that there were single-task trials approximating retention intervals for tests of the first and second stimulus sets in the dual-task procedure, for both the verbal and visual recognition tasks. We calculated these intervals by summing the duration of each trial event that would have occurred between offset of the study array and onset of the test array in dual-task trials. This resulted in single-task delays of 4500, 7000, and 9000 ms in the visual task, and 4500 and 7000 ms in the verbal task.

All participants were allowed breaks as needed, and a mandatory break of at least one minute was imposed for all participants after the second of three experimental blocks of trials. Sessions lasted approximately 90 minutes, and participants completed a total of 252 experimental trials each.

2.1.4. Data analysis. The advantage of the Cowan and Morey (2007) cued probe paradigm is that it allows the separation of interference due to concurrent maintenance from interference due to concurrent encoding. This is critical for testing hypotheses about concurrent maintenance, because all of the models of working memory we are comparing acknowledge that performing two tasks together can result in some dual-task cost. However, in the modular models, this interference is not due to maintenance per se, but to other processes involved when performing two tasks at once. In this paradigm, we can observe the dual-task costs specific to maintenance by comparing performance in cued conditions, in which participants can focus on the to-be-tested information throughout the retention interval, to performance in the uncued conditions, in which information from both stimulus sets must be maintained during the retention interval. If resources needed for maintenance are not shared between two tasks, we expect little or no effect of the cue on performance. The order of stimulus presentation is also a theoretically interesting factor; when the tested stimulus set was presented first, the untested set was encoded during maintenance of the tested set. However, when the tested stimulus set was presented second, no interference from encoding another stimulus set was possible. Finally, the number of items in the concurrent task provides a means to compare the costs of concurrently encoding or maintaining various amounts of information. These three variables were the primary focus of our modeling in Experiments 1a, 1b, 2a, and 2b.

Using these recognition tasks, it is also possible to estimate working memory capacity in a similar manner for both stimulus types, which allows for principled comparison between tasks. Specifically, we used a generalization of Cowan's multinomial model (often called *k*; Cowan, 2001) to estimate working memory for both tasks. Highly efficient estimates of *k* were obtained through Morey and Morey's (2011) estimation software. The software provides Bayesian estimates of capacity that are analogous to those found by using the common formula for Cowan's *k*, but are more efficient and less biased. Interested readers can find estimation details in the Appendix.

Cowan's (2001) assumptions include the notion that memory for a probed item is all-or-none: either the item is maintained in memory, or there is no information available and the participant must guess. For visual change detection, this claim has substantial empirical support (e.g., Awh, Barton, & Vogel, 2007; Cowan, Naveh-Benjamin, Kilb, & Saults, 2006). Although we constructed the verbal recognition task to be comparable to the visual task, it is doubtful that this strong assumption holds for verbal memoranda. Because certain items from a list are more likely to be remembered than others, one might reasonably suppose that whenever the first or last item appears as the probe in a middle-list position, a participant may correctly reject the probe based on knowledge about another serial position. Examining our proportions correct for the verbal recognition task for trials with 6 digits, correct responding unsurprisingly seemed more likely when the first (*M*=.91) item was probed than when another item was probed (*M*=.74, collapsed across positions 2, 3, 4, 5, and 6). Accuracy in the final position (*M*=.81) was comparable to accuracy in the second position (*M*=.80). We therefore handled this problem by excluding trials probing the first item or position from our entire verbal task analysis.[1]

An argument for asymmetry rests on being able to argue for null results, namely that some factors that affect visual working memory capacity do not similarly affect verbal working memory capacity. Null hypothesis significance tests, such as traditional ANOVA using *p* values, have a well-known inability to state evidence for null effects: a null hypothesis may be rejected, but the acceptance of the null hypothesis on the basis of a nonsignificant *p* value is a basic statistical error. We thus used the deviance information criterion (DIC; Spiegelhalter, Best, Carlin, & van der Linde, 2002) to compare models. DIC is a measure of model fit with a penalty for model flexibility, conceptually similar to the more popular Akaike Information Criterion (AIC; Akaike 1974), with the main difference being that DIC is useful for hierarchical models such as the model underlying these Bayesian capacity estimates, while AIC is not. Hypotheses about which independent variables are related to the dependent variable may be tested by creating models including each possible combination of independent variables as predictors of a parameter and comparing the DICs generated for each model, where lower DIC values indicate improvements in fit. We also provide mean proportions correct to allow readers to better assess the typicality of our results. Potentially interesting sources of variation in *k* in this experiment might arise from cue condition

[1] We also analyzed data from the verbal task without excluding any trials, and although the capacity estimates were somewhat higher overall, the same model achieved the winning DIC as in the reported analysis.

(informative versus uninformative retro-cue), stimulus presentation order (first or second), and concurrent task load (0, 3, or 6 items to process or maintain in the verbal task, 0, 2, 3, 4, or 6 items to process or maintain in the visual task). Because the designs of Experiments 1a and 1b were nearly identical, data were combined for analysis.

2.2. Results

Descriptive statistics for single-task performance are given in Table 1; those for dual-task performance can be found in Tables 2 and 3. In Table 4, we report model-fit tests with DICs, which include the most plausible contending model identified in our analyses (shown in bold text), models closest to the winning model in terms of factors (i.e., to show that adding or removing a factor did not improve fit) and the highest- and lowest-parameter models, in this case, the model including the full interaction of each independent variable on *k* and the model including only a main effect of participant on *k*, respectively. These outcomes make clear that the visual and verbal tasks are impacted very differently by the variables we manipulated.

For the visual task, DICs indicated that the best-fitting model included all three independent variables on *k*: an interaction between presentation order and concurrent load, and a main effect of cue condition (see Table 4). Visual *k*s are plotted in the upper panel of Figure 2, and it is clear that visual *k*s suffered from maintaining a verbal memory load compared with a single-task condition (compare with the black square) or the retro-cued conditions (compare the red with the teal lines). Each plotted point represents the posterior mean capacity for the corresponding condition, derived from the model containing the full interaction of all conditions with participant as a random additive effect. The error bars are posterior standard deviations on the difference between each condition mean and single-task performance. As the size of the verbal memory load increased, visual *k*s decreased. When the colored squares were presented second (compare the dashed with the solid lines), the impact of the concurrent verbal load was reduced, and the cost associated with maintaining an increasing verbal memory load was neutralized. Taken together with the absence of any difference to visual array performance in the single task condition with different inter-stimulus intervals (see values in Table 1, which are as likely to increase with ISI as to decrease), this suggests that encoding and maintaining a verbal memory load even briefly provokes some interference to visual memories, and this interference increases as both stimulus sets are maintained.

To understand this pattern, we carried out multiple comparison analyses by calculating the posterior odds that the difference between any two factor levels was greater than 0 versus less than 0. Because the WoMMBAT software returns samples from the posterior distribution of the factor level effects, posterior differences between the factor level effects can easily be computed. The posterior odds, then, is the ratio between the proportion of the posterior samples that are greater than 0 and the proportion less than 0. Visual inspection of the upper panel Figure 2 suggests that the Order x Verbal Load interaction is due to a smaller effect of verbal load when the to-be-remembered visual array was presented after the auditory list. Compared to single-task performance, the odds that verbal loads of both 3 and 6 items provoked a cost ranged from 9800:1 to at least 50,000:1 (i.e., this outcome held for each of the 50,000 samples we ran) when the visual array was presented before the auditory list, but were much lower when the array was given second (about 7:1 with 3 verbal items and 30:1 with 6 verbal items). The difference between maintaining 3 versus 6 verbal items was likewise clear when the visual array was presented first (24,500:1), but less so when presented second (about 4:1). The odds that capacities on cued trials were higher than on uncued trials were ≥ 50,000:1. Interpreting these odds values differs from interpreting post-hoc tests in that there is no agreed upon criterion for which differences are sufficiently large to consider important. In this case though, we think there can hardly be disagreement that an odds ratio in the tens of thousands is larger than one in the tens or lower.

For the verbal task, we observed a substantially simpler best-fitting model, including only a main effect of the size of the concurrent visual load on *k*. Examining the lower panel of Figure 2, it is clear that the effects of visual load on verbal *k*s are not comparable to the effects of verbal load on visual *k*s. First, post-hoc comparison of odds ratios shows that the case for a dual-task cost is not compelling: verbal *k*s while maintaining 2 or 3 visual items are not reduced compared to single-task verbal *k*s (odds <1:1), while the odds of a reduction with 4 and 6 visual items are 15:1 and with 3:1, respectively. Unlike for visual *k*s, there was no systematic decrease with increasing concurrent task load. Second, there is no systematic effect of the cue manipulation that could indicate that the small effect of the size of a concurrent visual memory load is specific to concurrent maintenance. Note that, unlike with null effects in an ANOVA, we may interpret the absence of cue condition and presentation order on *k* as evidence that these factors did *not* impact verbal memory capacity.

Arguably Experiment 1a might not have been perfectly suited for discovering an impact of visual load on verbal capacity because visual arrays included at least 3 items, which approaches typical estimates of capacity. We modeled Experiment 1b separately but still found no compelling evidence that concurrently maintaining visual information impaired verbal capacity; using only this sub-experiment, neither visual load nor cue condition improved model fit. The best model included an effect of presentation order on *k*, but the impact of presentation order was opposite of that observed with visual *k*s (i.e., verbal *k*s were slightly better when the verbal stimuli were presented first). Readers can examine only the estimates for 0, 2, and 4 visual items in the lower panel of Figure 2 for comparison. Note that different samples of participants completed the trials with 2 and 4 versus the trials with 3 and 6 squares; the small effect of visual array size in the combined analysis could therefore have reflected across-experiment variance. That no effect of visual task load is observed in Experiment 1b when considered alone supports this interpretation.

2.3. Discussion

For visual stimuli, mental representations suffer when a verbal stimulus set is encoded during visual set maintenance and when a verbal stimulus set must be concurrently maintained for several seconds. The degree of interference observed during visual maintenance depends on how many verbal items must be concurrently encoded or maintained. For verbal stimuli however, maintaining visual stimuli over several seconds did not incur such consistent costs; though the number of visual items had a small effect on verbal capacity estimates, this effect was not limited to concurrent maintenance. This pattern suggests that a concurrent verbal memory load impairs visual memory more than a visual memory load impairs verbal memory, and that concurrent maintenance of a verbal memory load is at least as detrimental to visual maintenance as concurrent encoding of a verbal list.

This pattern is consistent with the asymmetries we described in the introduction, but here interpretation is clearer. Whereas previous research reporting this pattern has employed serial memory tasks (e.g., Shah & Miyake, 1996; Vergauwe et al., 2010), we employed a simultaneous visual array task, in which verbal re-coding is unlikely to be effective (Morey & Cowan, 2004). This lessens the likelihood that the asymmetry is due merely to tendencies to verbally recode the visual stimuli.

With Experiments 2a and 2b, we sought to replicate this pattern under a variety of experimental conditions. In each sub-experiment, we manipulated the imposition of concurrent articulation. If it is the case that verbal memoranda have access to a domain-specific rehearsal mechanism and visual-spatial memoranda do not (e.g., Barrouillet & Camos, 2010) and that a domain-specific verbal mechanism is impaired by articulation, than more symmetry in interference might be induced under articulation. Across sub-experiments, we manipulated whether the visual stimuli were presented simultaneously, as in Experiment 1, or sequentially. Given the lack of apparent effects of retention time alone on either verbal or visual recognition (refer to Table 1), we do not think it is likely that the asymmetric pattern we observed is due to the visual task being shorter than the verbal task. However, sequential presentation of the color squares renders the tasks even more comparable in that presentation time is equated and both tasks require the accumulation of information into some whole pattern.

3. Experiments 2a and 2b

3.1. Method

3.1.1. Participants.

3.1.1.1. Experiment 2a. Twenty-six participants were recruited from the undergraduate population of the psychology program and from the community at large to take part in Experiment 2a. Participants were compensated with their choice of credit toward a course requirement or €7 per hour. One participant was excluded from all analyses because of inadequate color vision. The remaining sample (*N*=25) included 18 women and 7 men, between 17 and 35 years old (*M*=22.96, *SD*=3.86). Each participant completed two sessions of approximately 90 minutes each.

3.1.1.2. Experiment 2b. Twenty-one participants were recruited from the undergraduate population of the psychology program and from the community at large to take part in Experiment 2b. Participants were compensated with €7 per hour. One participant, a native Chinese speaker, was excluded from all analyses a priori after reporting the use of a visualization strategy based on pictographs for remembering the digits. The remaining sample (*N*=20) included 9 women and 11 men, between 20 and 28 years old (*M*=24.05, *SD*=2.29). Each participant completed two sessions of approximately 90 minutes each.

3.1.2. Apparatus and Stimuli.

3.1.2.1. Experiment 2a. The tasks and stimuli used in Experiment 2a were the same as those described in Experiment 1 with the following exceptions. Digits were presented in an artificial female voice created using AT&T Natural Voices software (AT&T, 2011), and to avoid some loss of data that occurs when excluding trials involving certain serial positions from analysis, probe selection was weighted such that each of the middle positions in 6-item lists were four times more likely to be probed than the first or final position. In 3-item lists, probes from each position remained equally likely. No participant reported noticing these weightings.

Participants were instructed to say the word “the” aloud at a rate of twice per second for half of the experimental trials. Articulatory suppression blocks occurred during both experimental sessions for all participants, with order reversed across sessions and randomly counterbalanced across participants; thus some participants received an articulation block then a silent block in Session 1 and then a silent block and articulation block in Session 2, while for other participants this sub-block ordering was reversed. This procedure was adopted to avoid confounding articulation with practice or fatigue effects. An experimenter supervised all sessions and reminded participants about the articulation instructions if their speech ever slowed below two utterances per second.

3.1.2.2. Experiment 2b. The tasks in Experiment 2b were the same as those in 2a with the following exceptions. Visual stimuli were presented sequentially, one square at a time. A colored square remained visible for 300 ms, and then was replaced by a gray square with a black outline. Square onsets were 500 ms apart, the same as the timing between digit onsets. The object of the visual recognition task was to remember which colors appeared in which locations onscreen; serial order memory was not tested.

Because sequential presentation of the visual stimuli created an irrelevant stimulus dimension at test (i.e., serial order), we added an irrelevant stimulus dimension to the verbal recognition task also. After the identity of the digit was chosen randomly without replacement, the timbre of voice speaking the digit was chosen randomly from a set of six artificial voices, created using AT&T Natural Voices (2011). The object of the verbal recognition task was to remember which digits sounded in order, and the recognition test was administered as described in Experiment 1. Memory for voice timbre was never tested.

3.1.3. Procedure.

Other than the changes described above to the tasks themselves, the procedure in both Experiment 2a and 2b was similar to that described in Experiment 1. The first session began with a colorblindness screening, then an experimenter explained the instructions to participants. Participants completed 8 supervised practice trials, and were encouraged to ask the experimenter questions as needed. Participants were reminded of the instructions at the beginning of the second session and completed another set of supervised practice trials. Participants were debriefed after their second experimental session was complete. All participants completed 528 experimental trials.

3.2. Results

Estimation of working memory capacity proceeded as described in Experiments 1a and 1b. Mean proportions correct, for single- and dual-task conditions, are given in Tables 5, 6, 7, 8, and 9. We examined verbal recognition first by serial position to determine whether it was necessary to exclude probes involving the first and final items. As in Experiment 1, probes of the first position were always higher than those in the middle of a six-item list (differences of 0.20 between the first item and the middle-item average in both experiments). In 3-item lists, as in Experiment 1, accuracy was also a little higher for the first position than the other positions (differences of 0.02-0.05, but near ceiling). Despite the inclusion of auditory suffixes (as in Experiments 1a and 1b), recognition when the sixth item was probed appeared to be higher than recognition when a mid-list position was probed (Experiment 2a: M_{Item6}=0.91, $M_{Mid\text{-}list}$=0.74; Experiment 2b: M_{Item6}=0.85, $M_{Mid\text{-}list}$=0.72). In 3-item lists this was not the case (Experiment 2a: M_{Item2}=0.96, M_{Item3}=0.97; Experiment 2b: M_{Item2}=0.92, M_{Item3}=0.92). We therefore excluded probes involving the first item from all lists and sixth item of 6-item lists from the verbal task modeling. Results of model comparisons are reported in Tables 10 and 11. We tested every possible combination of cue condition, presentation order, concurrent task memory load, and articulation on *k*, always with participant also included as a random intercept term.

3.2.1. Experiment 2a, with simultaneous visual arrays. According to the DIC values, the best-fitting model for visual capacity estimates included a 4-way interaction between articulation condition, cue condition, presentation order, and verbal task load. These estimates are plotted in Figure 3. As in Experiment 1, *k*s usually decreased as the number of to-be-remembered verbal items increased. Dual-task costs were evaluated by comparing *k*s in dual-task conditions with single-task *k*s. During silent blocks, multiple comparisons indicated that dual-task costs occurred in all conditions except when visual items were presented second and then cued, with odds of a decrease ranging from 5:1 to at least 50,000:1. With articulation, odds that maintaining 3 verbal items provoked a cost were low, ranging from 1.2:1 to 17:1, but became convincing with 6 verbal items, ranging from 50:1 to 1255:1. Evidence favoring a positive difference between concurrently maintaining 3 versus 6 verbal items was also usually ample, with odds ranging from 4.5:1 to 421:1. Regardless of articulation condition, there was no evidence for a dual-task cost when visual items were presented second and then cued. In this condition, dual-task capacity estimates sometimes exceeded single-task capacity estimates.

3.2.2. Experiment 2a, verbal sequences. In the verbal task, we observed effects of the manipulated variables on *k*s, but these effects differed systematically from the effects observed on visual *k*. The best-fitting model of verbal *k*s included an interaction between articulation and cue condition. These verbal capacity estimates are plotted in Figure 4. Multiple comparisons indicated that without articulation, odds that cues increase verbal capacity estimates were 1:1, whereas with articulation the odds favoring a cuing benefit were 69:1. However, a simpler model, the one including only an effect of articulation condition, has a nearly identical DIC value; with values so close, Spiegelhalter et al. (2002) recommend accepting the simpler model. No model including an effect of visual task load or an interaction including visual task load achieved a fit comparable to the fit of the winning model, indicating that verbal *k*s were in any case not affected by the amount of visual information to be concurrently encoded or maintained. The error bars in Figure 4 are the posterior standard deviations on the difference between single-task performance and each other plotted estimate. For most values (regardless of cue condition or visual task load) these ranges included or exceeded the single task value. Thus while there was evidence that cue condition and articulation interacted, in context this does not actually indicate that *k*s in the dual-task conditions were generally lower than those in the single-task condition.

3.2.3. Experiment 2b, sequential visual arrays. Plots of visual *k*s can be found in Figure 5. The best model included an interaction between articulation condition and verbal load, reflecting a reduction in the effect of an increasing verbal task load during articulatory suppression. Even so, multiple comparisons indicated that a verbal task load impaired visual *k*s in both articulation conditions. During suppression, odds that differences between verbal loads of 3 or 6 and single-task estimates exceeded 0 were at least 1140:1 and odds for decreases from 3 to 6 verbal items were at least 12:1. Without suppression, odds that capacity estimates with no verbal load exceeded those with some verbal load were at least 390:1 and odds favoring a decrease from 3 to 6 verbal items were at least 50,000:1. Thus in both articulation conditions, there was clearly a dual-task cost. The winning model also included an interaction between cue condition and presentation order, reflecting the lack of a dual-task cost when the visual stimuli were presented after the verbal items and then immediately cued for test. Odds that *k*s in this condition exceeded those in each other combination of cue condition presentation order were at least 9800:1.

3.2.4. Experiment 2b, verbal sequences. The best model of verbal *k*s included an interaction between articulation condition and presentation order, an effect of cue condition, and an effect of visual task load. Estimates are plotted in Figure 6, where the interaction appears clearly. With suppression, an advantage emerges when the verbal stimuli are presented after the visual stimuli. Odds that capacity estimates were higher when verbal stimuli were presented second were 920:1 during suppression compared to <2:1 during the silent blocks. Effects of cuing and concurrent task load were small compared to those observed in visual *k*s: the odds that cued estimates exceeded uncued were approximately 9:1. The odds that verbal *k*s decreased with 2 visual items compared to none were <1, and 9:1 comparing 5 visual items with none. The odds of a decrease between 2 and 5 visual items were better at 57:1, but consider the context provided in

Figure 6. Most of the dual-task estimates either lie within a standard deviation of single-task estimates or exceed the single-task estimate. A cost to verbal capacity with 5 visual items seemed strongest without articulatory suppression, in the conditions in which the verbal stimuli were presented first. This apparent cost (which was not however consistent enough to be picked up in the modeling) could be explained by supposing that some participants tried to articulate the names of the serially-presented colors in the subsequent visual array.

3.3. Discussion

Again, we observed clear effects of a verbal memory load on visual memory performance. Experiments 2a and 2b show that these effects occur even during articulatory suppression and even when the visual items are presented sequentially. Consistently with the outcome of Experiment 1, little or no cost to visual capacity was observed when maintenance of the visual array was unimpeded by encoding verbal stimuli or concurrently holding verbal memoranda during the retention interval.

Suppressing articulation consistently affected capacity estimates in both tasks (although more so in the verbal task) but did not induce a symmetric pattern of interference. Although articulation seemed to produce orderliness in the verbal *k*s, bringing out impacts of cuing (Experiment 2a) or presentation order (Experiment 2b), in neither experiment did convincing dual-task costs appear in verbal *k*s. Effects of cue and visual task load in Experiment 2b are most obvious in the silent condition, when the effect of visual task load can be plausibly explained by supposing that some participants attempted to verbalize the sequentially-presented colors. Possibly some participants adopted this strategy regardless of suppression condition; this possibility highlights the challenges of measuring visual recognition memory when using verbalizable stimuli.

Overall, these results continue to show clear effects of a concurrent verbal memory load on visual working memory capacity, but less clear effects of a concurrent visual task load on verbal working memory capacity. Taken together with Experiments 1a and 1b and with previous findings (Meiser & Klauer, 1999; Morey & Mall, 2012; Shah & Miyake, 1996; Vergauwe et al., 2010), these results indicate that asymmetric interference in this direction is a robust phenomenon, which persists with many kinds of tasks and in spite of concurrent articulation.

Another plausible alternative explanation of this asymmetric interference is that participants spontaneously assign priority to verbal rehearsal at the expense of visual maintenance. This explanation was invoked by Logie, Cocchini, della Sala, and Baddeley (2004) to explain an asymmetry between verbal and visual dual-task costs in patients with Alzheimer's disease. We know of no bias in our instructions to support this proposition, and indeed, with our cue manipulation we explicitly indicated to participants on some trials that rehearsing the verbal stimuli was totally unnecessary. Even so, we cannot guarantee that participants were capable of abandoning verbal stimuli. These results would therefore be strengthened by evidence that the same asymmetric pattern occurs when participants can prioritize stimuli prior to encoding. We tested this hypothesis in Experiment 3 by manipulating the proportion of total reward allotted for correct responses in each task, so that in some trial blocks, participants were explicitly instructed to favor the visual task and were preferentially rewarded for correct responses in the visual task.

4. Experiment 3

4.1. Method

4.1.1. Participants. We recruited 63 volunteers from the student population of the University of Groningen to participate in this study. One of these demonstrated insufficient color vision and 3 others experienced a computer malfunction, leaving a final *N*=59 (12 men and 47 women) between 19 and 30 years old (*M*=21.66, *SD*=2.01). We recruited a large number of participants because we realized after beginning data collection that our software was not recording the 6-item single-task verbal trials. We have these data for 19 participants. Balanced designs are however not necessary for carrying out the hierarchical Bayesian modeling we planned, thus we need not restrict our modeling to any subset of our sample or conditions.

4.1.2. Apparatus and stimuli. All experimental materials and the verbal and visual probe recognition stimuli we employed were the same as described in Experiment 1b, except that set sizes of 2 and 5 squares were included and presentation of the 3-item verbal lists was slowed to one digit per second (compared with 2 per second in the 6-item lists) to equate the duration of verbal stimulus presentation across list lengths. In the dual-task conditions of Experiment 3, participants always made responses to both stimulus sets. Requiring two responses created potential imbalances in timing because probe latencies are variable. We equated the time allotted for presentation of the verbal stimuli so that the times between stimulus presentations and probe screens would not vary with verbal memory load. Note that in previous experiments using simultaneous visual array presentations, increasing ISIs in the ranges we are comparing (between 4500 and 9000 ms) had little or no effect on visual or verbal single-task performance (refer to Table 1).

4.1.3. Procedure. After giving written consent and completing a brief color vision assessment, participants practiced each task separately with an experimenter's supervision. After completing both 6-item practice blocks, participants completed a block of 24 randomly mixed single-task control trials, divided equally between 3- and 6-item verbal trials and 2- and 5-item visual trials. Another identical single-task control block occurred at the end of the session to control for possible effects of practice and fatigue, resulting in a total of 12 single-task trials for each task and difficulty level. The maintenance interval for single-task trials was 5000 ms for both tasks.

In the dual-task trial blocks, it was necessary to probe both stimulus sets because we introduced a new independent variable, reward, which was meant to encourage participants to allocate resources toward one stimulus set to whatever extent possible. We created a reward system similar to the one reported by Morey, Cowan, Morey, and Rouder (2011), in which the total reward given for correct responses on each trial was constant, but shifted by block to favor one task or the other. Participants earned 0, 30, 70, or 100 points for correct responses to each task. The total reward possible on any trial was 100 points. In randomly-ordered blocks, participants could receive either 1) 100 points for a correct response to the verbal probe and 0 points for a correct response to the visual probe, 2) 70 points for the verbal response and 30 for the visual response, 3) 30 points for the verbal response and 70 for the visual response, or 4) 0 points for verbal response and 100 for the visual response. Participants were informed that after the practice trials ended, each of the 4 blocks would end when 3200 points were accumulated, which required at least 32 trials. The block eventually ended after 64 trials, regardless of whether the participant accumulated enough points. No matter how many trials were performed, the participant would still receive the same agreed-upon inducement for participating in the study, which was credit toward a course requirement corresponding to 90 minutes of work. Participants could thus save a substantial amount of time by optimizing their performance according to the reward scheme. Reward for each block was announced by an introductory screen at the beginning of each block, and reinforced through feedback after every trial indicating how many points were earned for each response.

Each trial began with a 900-ms fixation. After a 100-ms delay, the first stimulus set occurred. Presentation of the visual stimulus set included a 500-ms study array, followed by a 500-ms blank grey screen, and then a 500-ms mask. Presentation of the verbal stimulus set included a 3000-ms sequence including 3 (1000 ms each) or 6 (500 ms each) digits, a 500-ms delay, and a 500-ms suffix. The probe screen for the first stimulus set appeared immediately after the offset of the mask or suffix of the second stimulus set, and the probe screen for the second stimulus set appeared immediately after the participant responded to the first probe. For both tasks, probes were constructed with the same format as in Experiments 1 and 2. After the response to the second probe, participants received feedback about performance on each task, in the order of the probes. Feedback screens indicated whether the response was correct or incorrect, and the number of points earned for the response. Each feedback screen lasted 1500 ms. Finally, the participant was informed of the total points earned so far in the block, and invited to begin the next trial when ready.

4.2. Results

Estimation of working memory capacity proceeded in the same manner described in previous experiments. Mean proportions correct, for single- and dual-task conditions are given in Tables 12, 13, and 14, and model fit statistics can be found in Table 15. We tested every possible combination of variables on *k*, always including participant as a main effect. Experiment 3 included three independent variables of potential interest for each task: stimulus presentation and test order (i.e., whether the verbal presentation occurred before the visual presentation, or vice versa), proportion of reward assigned to the task being modeled, and the size of the concurrent memory load, enabling models that included only single variables to a model including a three-way interaction. We then compared the effects of these variables on capacity estimates for each task.

Graphs of Bayesian capacity estimates as a function of reward and concurrent memory load are given in Figure 7. For the visual task (upper panel of Figure 7), we observed a graded effect of the reward manipulation, such that capacity estimates decreased as proportion of reward shifted from the visual to the verbal task. The best-fitting model also included a main effect of presentation order, such that capacity estimates in all conditions were higher (posterior mean effect on k= 0.13) when visual presentation and test occurred first. In this order, participants observed the study digit sequence in between the presentation of the study visual array and the visual probe rather than responding to the digit probe while maintaining the visual image. The error bars shown in Figure 7 reflect the posterior standard deviations on the difference between single-task *k*s and each dual-task *k*s. Multiple comparison analyses indicated that when 100% of the reward was allocated to the visual task, the odds that the difference between holding 3 or 6 verbal items was *less* than 0 were only 2:1, extremely poor evidence for this difference. However at other reward levels, the odds that holding 6 verbal items differed from holding 3 verbal items were always at least 430:1 favoring differences greater than 0. Regarding the effect of reward, this appeared to be finely graded, with visual capacity estimates always decreasing as reward was allocated toward the verbal task. Consideration of the posterior odds confirms this. We compared each descending data point within each level of concurrent verbal load (thus, reward=100 with reward=70, then reward=70 with reward=30, etc., separately for both verbal loads of 3 and 6). Each descending comparison produced posterior odds of at least 70:1 that the difference between the two conditions compared was greater than 0. A summary of the effects of reward and verbal task load on visual capacity estimates is thus that reward always affected capacity in a graded manner, such that the more reward allotted to the verbal task, the lower visual capacity estimates became, and an increasing concurrent verbal memory load also impaired visual performance, except when 100% of reward was assigned to visual task performance and the verbal stimuli could be ignored throughout the trial.

For capacities in the verbal task (depicted in the lower panel of Figure 7), the best-fitting model also included an interaction between reward and visual task load on *k*. However, it is clear that this interaction cannot reflect the same effects as the interaction between reward and verbal task load on visual *k*s. Unlike in the visual analysis, high visual task load did not much differ in most cases from low visual task load (indeed, the extent of the posterior standard deviation error bars of most dual-task capacities in the verbal task panel encompass the single-task value), and the effect of reward level, though clearly apparent, is not gradual. Considering potential differences between concurrently maintaining 2 or 5 squares at each reward level, the evidence of differences greater than 0 was decisive when reward for verbal performance was 0 points (49,000:1) and fairly convincing when reward for verbal performance was 30 points (30:1); for the same comparisons at other reward levels, the posterior odds never exceeded 0.15:1 (i.e., 7:1 *against* a difference in this direction). Differences between levels of reward were less clearly graded than in the visual task, but reward did affect verbal estimates in the direction expected. Regardless of visual task load, evidence for differences greater than 0 between 100 points and 70 points was weak (0.15:1 or less), while the same comparisons between 70 and 30 points were convincing only for the 5-item comparison (1,600:1 with 5 visual items, 4:1 with two visual items) and differences between 30 and 0 points were convincingly greater than 0 (at least 50,000:1). However, even though differences between intermediate reward levels were plausible, verbal capacities in the 30 point condition still did not decrease persuasively from single-task performance, with odds at most of 4:1. Thus a reasonable summary of the effects of reward on verbal capacity estimates suggests that decreasing reward decreases capacity, but not in the gradual manner observed in the visual task. Instead, verbal capacity in a dual-task setting is similar to single-task verbal capacity when any attention is allocated to the verbal task, and decreases only when the verbal task can be ignored. Increasing visual task load convincingly impairs verbal capacity only when there is no incentive to allocate attention to the verbal task.

4.3. Discussion

Experiment 3 was carried out to test the hypothesis that the asymmetric patterns of interference observed in Experiments 1 and 2 came about because of some preference for rehearsing verbal information, serving to protect it in the face of interference. We manipulated the proportion of reward assigned to correct performance on each task, in order to measure capacities when one task was explicitly emphasized over the other. Assuming that any supposed preference for verbal maintenance is at least partially under voluntary control, participants should have divided attention between the verbal and visual tasks during blocks of trials in which some reward was assigned to each, but should have allocated most attention for the task garnering the greatest reward. Our reward manipulation was clearly effective. In both tasks, higher reward was generally associated with higher capacity estimates. However, this was particularly true in the visual task, where reward induced a graded allocation of attention. In the visual task, increasing verbal task load also impaired visual capacity estimates in all conditions in which any reward was given for concurrent verbal task performance. In the verbal task, increasing visual task load only impaired verbal task performance in the conditions in which correct visual responses took most of the reward, but even in the low-intermediate reward condition, verbal capacities did not convincingly differ from those obtained in the single-task condition. This pattern of results suggests that paying any amount of attention to verbal stimuli may be sufficient to maintain lists of these lengths, but that decreasing attention to visual stimuli incrementally decreases performance.

5. General Discussion

Across several experiments, we consistently observed asymmetric dual-task interference between visual and verbal working memory tasks. In four experiments using the retro-cue design of Cowan and Morey (2007), we observed that sustained concurrent maintenance of verbal memoranda produced more interference with visual memory than only encoding and very briefly maintaining a verbal list. It was clear that effects of verbal task load on visual memory capacity were confined to conditions in which verbal memoranda were either encoded while visual stimuli were being maintained or when verbal and visual memoranda were concurrently maintained over a delay. This selective interference was not present for the verbal task, where the effects of a concurrent visual memory load were inconsistent. Although we sometimes observed effects of cueing or visual task load on verbal capacity estimates, we did not observe clear dual-task costs to verbal capacity estimates; usually, verbal capacity estimates in dual-task conditions were comparable to or even exceeded single-task estimates. With Experiment 3, we eliminated the possibility that this pattern was attributable to preferential rehearsal of verbal memoranda or to difficulty recovering to-be-tested information on the appearance of the retro-cue. These results confirm asymmetric patterns previously observed in published literature, and further establish that this pattern cannot be due merely to theoretically trivial explanations such as stimulus recoding or preferential rehearsal. Instead, this is a phenomenon that models of working memory must account for.

5.1. Implications of asymmetric interference for existing models of working memory

Our evidence is consistent with the idea that domain-general attention, a construct assumed by all the models we described in the introduction, plays a different role in verbal maintenance than in visual maintenance, and that attention's role in visual maintenance is fundamental, whereas its role in verbal maintenance is merely supporting. Because various models of working memory are broadly consistent with each other, we consider in turn models that suppose that maintenance of verbal and visual-spatial information occurs in separate buffers and those that make no such assumption. Although the models we consider also differ in other ways, we focus on the intersections between domain-specific and domain-general functions with the goal of pushing theorists toward revisions that we think would make both kinds of model more comprehensive.

The multi-component model of working memory (e.g., Baddeley, 1986, 2012; Baddeley & Logie, 1999; Logie, 2011; Repovš & Baddeley, 2006) postulates separate stores for visual-spatial and acoustic-phonological memoranda. Some variants of the multi-component model explicitly propose specialized visual rehearsal components (e.g., Logie, 2011), while others treat these vaguely. Other models of working memory suppose a hierarchical relationship between working and long-term memory (Cowan, 2005; Oberauer, 2009; Unsworth & Engle, 2007) and do not explicitly postulate separate modules for storing visual and phonological memoranda. Similarly, the time-based resource-sharing model (TBRS; Barrouillet et al., 2007) proposes one attentional resource that can be used for storage or other processes, whereas the central executive of the multi-component model can only process but not store information. None of these models has predicted the asymmetric pattern of interference we found. We separately consider the implications of asymmetric cross-domain interference for models that propose domain-specific components and for models that do not.

5.1.1. Variations of the multi-component model. Baddeley's (2012) multi-component model includes the domain-specific phonological and visual-spatial storage buffers, and two domain-general components: the central executive, for attentional control, and the episodic buffer, a limited-capacity domain-general buffer thought to interface with long-term memory. The domain-specific storage buffers are supposed by some to have parallel rehearsal capabilities and restrictions (Logie, 2011): both can hold a limited amount of information which is forgotten if not rehearsed, and both are believed to suffer from limitations arising from sensory-specific coding. For example, it is believed that similar visual materials are poorly remembered compared to more distinct ones (Awh, Barton, & Vogel, 2007; Logie, della Sala, Wynn, & Baddeley, 2000); this is comparable to the phonological similarity effect believed to arise from confusability of phonemes during the rehearsal of acoustically-coded sounds (Baddeley, Lewis, & Vallar, 1984). Both systems are supposed to have rehearsal capabilities (Logie, 2011; Repovš & Baddeley, 2006): inner speech for the phonological system, and spatial rehearsal, possibly based on eye movements (Pearson & Sahraie, 2003; Lawrence, Myerson, & Abrams, 2004; Tremblay, Saint-Aubin, & Jalbert, 2006), for the visual-spatial system. Some versions of the multi-component framework do not commit to a visual-spatial rehearsal function, but allow that it is a possibility (Baddeley, 2012).

That both verbal and visual-spatial maintenance can be supported by domain-general components has been long assumed. Logie (2011, p. 244) articulated this logic clearly, explaining that domain-general systems can be enlisted for the support of domain-specific information when the amount of information to be maintained exceeds the capacity of the domain-specific system. This explains cross-domain interference within the multi-component framework without threatening the domain-specificity of the phonological loop or the visuo-spatial sketchpad. However, this contribution of domain-general resources to domain-specific maintenance is explicitly assumed by Logie to flow both to the verbal and visual sub-systems. Assuming only that domain-general resources assist both domain-specific sub-systems, our results are truly puzzling. We must invoke this mechanism to explain why encoding or maintaining verbal items impairs visual memory (and thus assume that in our dual-task conditions, domain-general resources are allocated to assist verbal maintenance) yet scant evidence of this sharing appears in the verbal recognition measure.

Our results thus indicate the relationships between attention and storage must differ for visual-spatial and verbal memoranda, but it is unclear how this assumption could best be implemented. One proposal for modifying modular models to better reflect asymmetric relationships between visual-spatial and auditory-verbal short-term memory and attention would be to integrate the visual-spatial system with the attention system. Phillips and Christie (1977) promoted this modification to Baddeley's (1986) emerging multiple-component model. They measured recognition of visual patterns while performing various concurrent tasks, some passive (such as listening as a list of digits was read) and some that required active manipulation (such as performing arithmetic operations on the digits). Over several experiments, Phillips and Christie observed significant decreases in visual memory performance during performance of active secondary tasks, regardless of whether those active tasks were primarily visual or verbal. Their visual memory task itself required only storage, not manipulation. These results imply that merely storing visual images requires resources common to actively processing non-visual information.

Phillips and Christie (1977) reasoned that special-purpose resources for visual memory might be an unnecessary component of a working memory system. Although some explicit attempts to falsify this hypothesis have been made (e.g., Farmer, Berman & Fletcher, 1986; Duff & Logie, 2001) we think that Phillips and Christie's criticism of Baddeley's original tripartite structure remains valid, and that relationships between visual maintenance and general attention should be further scrutinized. However, adopting the Phillips and Christie proposal would not merely entail removing a module from Baddeley's (2012) current framework. If we decide that we can do without a domain-specific visual-spatial storage buffer, we must change the assumptions of some other module to allow for the maintenance of visual images. This combination of functions might once have fallen to the episodic buffer, which was originally proposed as a domain-general storage module linked to the domain-specific buffers via the central executive (Baddeley, 2000). However, recent iterations of the multi-component model have been modified to posit that the episodic buffer is also passive, like the domain-specific buffers (Baddeley et al., 2011). Supposing that visual information is maintained in the episodic buffer also would not fit well with our evidence, else why should the contents of the domain-general, passive episodic buffer and the verbal sub-system interfere?

Our results, consistently with those of Phillips and Christie (1977), suggest a domain-general component that can be more flexibly allocated toward storage, rehearsal, manipulation, and response processes. However, our results also indicate that verbal maintenance might be distinct from some of these functions, suggesting that some modularity might be necessary to explain patterns of interference between working memory tasks. Such a system has also been described as an extension of the TBRS model (Barrouillet & Camos, 2010). Under such a system, verbal rehearsal could still rely on moderate support from the general attentional resource that also serves processing and storage functions, depending on the length of the verbal list rehearsed. Initiating rehearsal of a short verbal list would require fewer general resources than rehearsal of a longer list.

If the general component also stores visual items, then rehearsing no verbal items would enable the assignment of the entire general module to the maintenance of visual memoranda, and rehearsing a few verbal items would produce a smaller decrease in resources available for visual memory than rehearsing a long list of verbal items. Because the verbal store itself is separate from the general resource, verbal storage should be primarily a function only of whether any executive resources are devoted to initiating rehearsal; under these assumptions, an item-for-item trade-off between verbal and visual-spatial storage would not be expected. This is important, inasmuch as correctly maintaining a list of verbal items does not interfere more with visual memory than incorrectly maintaining a list of verbal items (Morey & Cowan, 2004). Positing separate verbal rehearsal and domain-general attention components would explain why cross-domain conflicts occur during maintenance, but do not necessarily lead to equivalent storage trade-offs. Our results do not unequivocally support such a system; under these assumptions, one might have expected to observe larger effects of visual task load on verbal capacity with articulatory suppression. Clearly further investigation into exactly which processes could be independent is warranted.

We consider the possibility of reducing the number of distinct components or processes supposedly contributing to working memory advantageous for advancing understanding. Though the delineations proposed in various multi-component models of working memory have been arrived at through a process of logic and empiricism, it does not follow that this process has been without error. The main empirical basis for these delineations are the persistent occurrence of double dissociations based loosely on stimulus domain, but double dissociations alone are not necessarily sufficient for supposing two underlying mechanisms (see Dunn & Kirsner, 2003). Some results that support proposals of multiple components would also be predicted by more parsimonious assumptions about interference based on similarity (e.g., Sanders & Schroots, 1969). Moreover, even if we concede that a large body of empirical work suggests a functional distinction between verbal and visual-spatial memory, such a distinction might be handled by a 2-component system similar to that proposed by Barrouillet and Camos (2010). We therefore see no compelling reason to invoke four or more separate working memory components to explain our findings or to explain this literature broadly. Given that the literature on dual-task dissociations does not unambiguously support these dissociations to begin with, we think that more parsimonious explanations ought to be reconsidered. Baddeley's multiple component model accounts for several patterns in behavioral and neurological data, most importantly here, for a tendency towards larger within-domain than cross-domain interference. Any model of working memory must be able to account for this pattern, but assuming the intricate multi-component framework is not necessarily essential to accomplish this.

5.1.2. Attention-as-storage models. Several models of working memory assume that an attention-based mechanism is capable of storing information of any domain or code, or performing processing operations. Such models include the embedded process model of Cowan (2005), the TBRS model (Barrouillet et al., 2007), and the models of Oberauer (2009). These models predict interference whenever the capacity of attention is exceeded, regardless of the domain of the information occupying the focus of attention; information outside the focus of attention is subject to interference or decay, while information currently held in the focus of attention is preserved. While these models do not predict our findings of asymmetric interference outright, they may include processes that can plausibly account for them.

One such process is long-term memory activation, which could plausibly differ for verbal and visual stimuli. The embedded processes model of Cowan (2005) supposes that the focus of attention, which might be considered the source of measured working memory capacity, differs from activated long-term memories only in the level of activation exhibited. Cowan's model could accommodate the proposal that sensory information that is more likely to activate long-term semantic representations is more likely to be accessible outside of the focus of attention, perhaps because it can better survive time-based decay or representation-based interference while not in the focus itself. By simply supposing that our verbal memoranda were more likely to activate long-term memories than our visual-spatial memoranda, Cowan's model includes a process that could explain why verbal memoranda were more impervious to interference than visual-spatial memoranda. Barrouillet et al. (2007) might similarly describe this in terms of cognitive load; support from long-term memory might reduce the cognitive load incurred in refreshing the verbal memoranda, because they might not need to be re-activated as frequently as memoranda without this semantic support. However, it is not clear that differential activation of long-term memory is sufficient to explain all of our results, because asymmetric patterns of interference can also be observed with meaningless nonwords serving as verbal information (Morey & Mall, 2012).

Another related process within Cowan's model that could be considered is the relative ease of chunking various kinds of stimuli; perhaps verbal stimuli are more easily encoded into chunks than visual stimuli, and perhaps the process of grouping the verbal items becomes more intense with longer lists. If this were the case though, one might expect maintaining the grouped verbal stimuli to pose less of a burden than encoding them, an outcome which our data do not support. Our data in Experiment 1 show that concurrently maintaining verbal information impairs visual information most, and while the difference between concurrently encoding versus encoding and maintaining verbal stimuli was smaller in subsequent experiments, there was never any evidence to support the idea that verbal encoding was the main source of interference. This evidence is thus not consistent with the idea that creating verbal groupings is specifically what provokes interference with visual memory.

Thus although embedded models include plausible processes that might explain this asymmetric pattern post-hoc, it is not clear whether embedded models would have predicted this pattern a priori, and this is a serious problem for researchers interested in testing hypotheses about working memory. Embedded models might just as easily have predicted an item-for-item trade-off in both domains, at least when the focus of attention can be isolated, as argued by Saults and Cowan (2007). In other cases (e.g., Cowan, Saults, & Morey, 2006), Cowan and colleagues have suggested that concurrent tasks like articulatory suppression are necessary to restrict resources available for task performance to the focus of attention. That they do not suggest any necessity for also restricting non-verbal rehearsal possibilities is suggestive of an implicitly assumed asymmetry of resources available to assist in memory of verbal and visual-spatial information. In Experiment 2a however, we observed asymmetry of interference both with and without articulatory suppression, suggesting that even articulation at a rate of twice per second is not sufficient to render verbal memoranda as susceptible to cross-domain interference as visual memoranda are. It is crucial that models make specific enough predictions to enable falsification: in attention-based models, the conditions under which item-for-item storage trade-offs are to be expected should be clearly specified, as should the circumstances under which activated long-term memories might be expected to offer differing levels of support in a short-term memory task. When a model can accommodate such a large range of empirical outcomes, one ought to consider whether that model is actually too flexible.

5.1.3. Alternatives. Accounting for the asymmetric pattern of interference we found is essential for theorists formulating new predictions about cross-domain interference in working memory. Incorporating new assumptions implied by this pattern will strengthen existing models of working memory; ultimately, we believe that if new assumptions are adopted by some models while constructs are eliminated from others, disparate theories will move closer together, perhaps enroute to the formation of a new model that includes the best elements from classic models. One prediction that we endorse, consistently with others (Barrouillet & Camos, 2010; Camos et al., 2009; Hudjetz & Oberauer, 2007), is that it should be explicitly supposed that verbal information has access to more avenues of rehearsal or representation than visual-spatial information. This assumption introduces a targeted modularity that is consistent with much extant data (including much of the data presented here), while maintaining some of the appealing elegance of the embedded models. An even more parsimonious account may be made by supposing, consistently with the gestural-perceptual hypotheses of Jones and colleagues (e.g., Jones, Macken, & Nicholls, 2004), that verbal information derives its advantage in the face of interference from the availability of the unique motor processes that enable speech, rather than by a dissociable rehearsal component or store.

5.2. Conclusions

We demonstrated that maintaining verbal items provoked a graded decrease in visual memory, but maintaining visual items did not much impair verbal memory, and any effect of visual memory load on verbal memory capacity could never be restricted to a conflict between concurrent maintenance of visual and verbal materials. We demonstrated that this asymmetric pattern of interference, which is not itself new, is robust and cannot be explained by theoretically trivial accounts such as stimulus re-coding. These findings reflect fundamental differences in the role of attention in supporting verbal and visual memoranda. Our results suggest that the symmetry inherent in the assumption of dedicated specialized systems for both verbal memories and visual memories should be questioned. Modification of existing models to allow predictions of asymmetry may require substantial simplification (as in the case of the Baddeley's (2012) multi-component model) or stronger assumptions (as in the case of attention-based models such as Cowan's (2005) embedded process model). Our results would be consistent with a comprehensive working memory model that incorporates sharing of some domain-general resource between storage and attention processes, as assumed by embedded models (Cowan, 2005; Oberauer, 2009) and by the TBRS model (Barrouillet & Camos, 2010) but that also provides an explanation for superior verbal resistance to interference, such as a specialized verbal store or rehearsal mechanism (e.g., Baddeley, 2007; Barrouillet & Camos, 2010) or by explicitly proposing that processes that exist for supporting verbalization (such as speech-based motor planning, Jones et al., 2004) are either not present or are far weaker for visualization.

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Appendix

The working memory capacity estimates in this paper were obtained using the Working Memory Modeling using Bayesian Analysis Techniques (WoMMBAT; R.D. Morey, 2011; R.D. Morey & C.C. Morey, 2011), free software available as an R package that allows users to fit a formal process model of capacity suggested by Rouder, Morey, Cowan, Zwilling, Morey, and Pratte (2008) to the data using hierarchical Bayesian methods. The process model underlying the model itself is a generalization of the capacity model of Cowan (2001; Cowan, Elliott, Saults, Morey, Mattox, Hismjatullina, & Conway, 2005), but the hierarchical Bayesian methods offer several advantages over Cowan et al.'s simple formula for capacity, including more efficient, less biased estimation; avoidance of uninterpretable negative capacity estimates; and Bayesian model fit indicators that allow for arguing in favor of null hypotheses, which is not possible using traditional inferential methods. Finally, because *k* estimates calculated in the traditional manner are likely to violate the homoscedasticity and normality assumptions of ANOVA, the Bayesian model fits yield sounder statistical inferences than ANOVA would (Morey, 2011). The analysis process using WoMMBAT differs from a traditional ANOVA (see Morey & Morey, 2011, for a full example of an analysis), but the interpretation of the resulting capacity estimates is comparable, with the exceptions given above.

Each task (visual or verbal) was analyzed separately. The working memory capacity model on which WoMMBAT is based (Morey, 2011; Rouder, et al., 2008) assumes 3 parameters: capacity of working memory (*k*), guessing bias (*g*), and probability of paying attention on any particular trial (*z*). Analysts may model each of these parameters separately by specifying which independent variables should affect each parameter. Transformations of the parameters of interest (*k*, *g*, and *z*) are assumed to arise from linear combinations of effects of the independent variables in a manner similar to logistic regression.

We did not allow *z* to vary across models; instead, the lapsing rate was set at the default of 0.05, which is informed by previous experiments (Rouder et al., 2008). This is consistent with the advice given by Morey and Morey (2011): allowing *z* to vary requires the inclusion of trials with only 1 or perhaps 2 memory items, where perfect accuracy would be expected whenever participants are attending, and our designs did not always fulfill this criterion. Because we were not theoretically interested in testing hypotheses about *g*, we first tested several plausible variable combinations on *g* with a full interaction of all variables of interest on *k,* to determine the simplest possible combination of variables that best fit *g*. Based on these preliminary analyses we always allowed *g* to vary only across participants in both tasks, assuming that participants might differ in their guessing bias, but that no variable we manipulated systematically affected guessing bias.

We compared models including each possible combination of independent variables (plus the participants, as an additive random effect) on *k*. The WoMMBAT software uses Markov Chain Monte Carlo (MCMC) methods to fit each model individually (see Rouder and Lu (2005) for an introduction to these methods for psychologists). Because these analyses can be time-consuming, we began in an exploratory manner running fairly small analyses with 1,000-2,000 MCMC iterations to determine which models were close competitors. We eliminated poorly fitting models and then re-ran the remaining contenders for 50,000 iterations for a conclusive test of the sometimes-small differences between DICs. MCMC chain convergence was assessed visually, according the process described by Morey and Morey (2011).

Table 1.
Single task proportions correct (with standard deviations), Experiments 1a and 1b.

		ISI		
		4500 ms	7500 ms	9000 ms
Visual arrays				
Change				
	2 squares	0.90(.30)	0.89(.32)	0.93(.26)
	3 squares	0.77(.42)	0.87(.34)	0.82(.39)
	4 squares	0.82(.39)	0.85(.36)	0.79(.41)
	6 squares	0.62(.49)	0.64(.48)	0.71(.46)
Same				
	2 squares	0.92(.28)	0.90(.30)	0.90(.30)
	3 squares	0.90(.31)	0.81(.40)	0.78(.42)
	4 squares	0.58(.39)	0.68(.47)	0.74(.44)
	6 squares	0.53(.49)	0.55(.50)	0.54(.50)
Verbal sequences				
Change				
	3 digits	0.99(.12)	0.98(.14)	
	6 digits	0.88(.33)	0.84(.37)	
Same				
	3 digits	0.94(.24)	0.97(.18)	
	6 digits	0.69(.46)	0.70(.46)	

Note. Experiment 1a (*N*=26) included 3 or 6 squares in the visual task while Experiment 1b (*N*=24) included 2 or 4 squares. Verbal sequence proportions correct include data from all serial positions.

Table 2.
Dual-task conditions, proportions correct (with standard deviations), visual array task, Experiments 1a and 1b.

With 3 digits

		2 squares	3 squares	4 squares	6 squares
Cue/Order					
Change					
	Cue, 1st	0.85(.36)	0.71(.46)	0.71(.46)	0.68(.47)
	Cue, 2nd	0.90(.30)	0.69(.46)	0.85(.36)	0.68(.47)
	No cue, 1st	0.79(.41)	0.67(.47)	0.69(.46)	0.60(.49)
	No cue, 2nd	0.86(.35)	0.69(.46)	0.74(.44)	0.60(.49)
Same					
	Cue, 1st	0.76(.43)	0.77(.42)	0.71(.46)	0.62(.49)
	Cue, 2nd	0.85(.36)	0.86(.35)	0.75(.44)	0.59(.50)
	No cue, 1st	0.82(.39)	0.72(.45)	0.60(.49)	0.45(.50)
	No cue, 2nd	0.86(.35)	0.78(.42)	0.57(.50)	0.63(.49)
With 6 digits					
Cue/Order					
Change					
	Cue,1st	0.81(.40)	0.67(.47)	0.67(.47)	0.58(.50)
	Cue, 2nd	0.87(.33)	0.73(.45)	0.75(.44)	0.64(.48)
	No cue, 1st	0.74(.44)	0.58(.50)	0.69(.46)	0.53(.50)
	No cue, 2nd	0.72(.45)	0.68(.47)	0.69(.46)	0.51(.50)
Same					
	Cue, 1st	0.72(.45)	0.68(.47)	0.58(.50)	0.58(.50)
	Cue, 2nd	0.93(.26)	0.83(.38)	0.74(.44)	0.55(.50)
	No cue, 1st	0.67(.47)	0.64(.48)	0.51(.50)	0.55(.50)
	No cue, 2nd	0.88(.33)	0.77(.42)	0.64(.48)	0.58(.50)

Note. Experiment 1a (N=26) included 3 or 6 squares in the visual task while Experiment 1b (N=24) included 2 or 4 squares.

Table 3.
Dual-task conditions, proportions correct (with standard deviations), verbal sequence task, Experiments 1a and 1b.

3 digits	2 squares	3 squares	4 squares	6 squares
Cue/Order				
Change				
Cue, 1st	0.99(.12)	0.97(.16)	0.94(.23)	0.92(.27)
Cue, 2nd	0.99(.12)	0.96(.19)	0.93(.26)	0.96(.19)
No cue, 1st	0.96(.20)	1.00(.00)	0.96(.20)	0.94(.25)
No cue, 2nd	0.94(.23)	0.97(.16)	0.97(.17)	0.95(.22)
Same				
Cue, 1st	0.93(.26)	0.97(.16)	0.94(.23)	0.94(.25)
Cue, 2nd	0.97(.17)	0.99(.11)	0.92(.28)	0.90(.31)
No cue, 1st	1.00(.00)	0.95(.22)	0.94(.23)	0.90(.31)
No cue, 2nd	0.96(.20)	0.92(.27)	0.96(.20)	0.96(.19)
6 digits				
Cue/Order				
Change				
Cue, 1st	0.81(.40)	0.94(.25)	0.88(.33)	0.91(.29)
Cue, 2nd	0.82(.39)	0.88(.32)	0.75(.44)	0.87(.34)
No cue, 1st	0.89(.32)	0.79(.41)	0.85(.36)	0.85(.36)
No cue, 2nd	0.90(.30)	0.81(.40)	0.85(.36)	0.81(.40)
Same				
Cue, 1st	0.71(.46)	0.64(.48)	0.69(.46)	0.68(.47)
Cue, 2nd	0.64(.48)	0.77(.42)	0.72(.45)	0.71(.46)
No cue, 1st	0.71(.46)	0.73(.45)	0.60(.49)	0.69(.46)
No cue, 2nd	0.69(.46)	0.77(.42)	0.61(.49)	0.67(.47)

Note. Experiment 1a (*N*=26) included 3 or 6 squares in the visual task while Experiment 2b (*N*=24) included 2 or 4 squares. Proportions correct include trials testing all serial positions.

Table 4.
Model Comparison, Experiments 1a and 1b.

	n par. *k*	DIC
Visual memory task		
Cue x Order x Verbal Load	62	7051.2
Order x Verbal Load + Cue	**58**	**7047.7**
Order x Verbal Load	56	7069.5
Cue + Order + Verbal Load	57	7054.2
Verbal Load	53	7105.8
Only subject effects	50	7175.4
Verbal memory task		
Cue x Order x Visual Load	70	3089.8
Order x Visual Load + Cue	62	3087.7
Cue + Visual Load	57	3082.0
Order + Visual Load	57	3081.2
Visual Load	**55**	**3080.0**
Only subject effects	50	3084.8

Note. Smaller DIC values indicate superior model fit. For each task, the winning model is shown in bold text. MCMC chains 50,000 iterations long converged on the values given in this table. Preliminary hypothesis testing on all possible models was conducted with chains of 1,000 iterations. We report the most complex model (here, the 3-way interaction), the simplest model (only subject, i.e., no effects of independent variables or interactions), the winning model for each analysis, and when necessary, the models adjacent in complexity to the winning model. All models included main effects of subject on *k* and *g*.

Table 5

Single task proportions correct (with standard deviations), Experiments 2a and 2b.

Experiment 2a: Simultaneous visual arrays (*N*=25)			
		Silent	Articulation
Visual arrays			
Change			
	2 squares	0.95(.22)	0.90(.30)
	5 squares	0.80(.40)	0.76(.43)
Same			
	2 squares	0.89(.31)	0.87(.34)
	5 squares	0.73(.45)	0.66(.47)
Verbal sequences			
Change			
	3 digits	0.98(.13)	0.96(.20)
	6 digits	0.87(.34)	0.80(.40)
Same			
	3 digits	0.995(.07)	0.96(.20)
	6 digits	0.76(.43)	0.62(.49)
Experiment 2b: Sequential visual arrays (*N*=20)			
Visual arrays			
Change			
	2 squares	0.90(.30)	0.89(.32)
	5 squares	0.83(.38)	0.63(.48)
Same			
	2 squares	0.92(.27)	0.81(.40)
	5 squares	0.60(.49)	0.57(.50)
Verbal sequences			
Change			
	3 digits	1.00	0.94(.24)
	6 digits	0.89(.31)	0.71(.45)
Same			
	3 digits	0.96(.19)	0.87(.34)
	6 digits	0.76(.43)	0.62(.49)

Note. Verbal sequence proportions correct include data from all serial positions.

Table 6

Dual-task conditions, proportions correct (with standard deviations), visual array task, Experiment 2a.

		Silent		Articulation	
		2 squares	5 squares	2 squares	5 squares
With 3 digits					
Cue/Order					
Change					
	Cue, 1st	0.89(.31)	0.83(.38)	0.89(.31)	0.75(.44)
	Cue, 2nd	0.93(.25)	0.80(.40)	0.88(.33)	0.69(.46)
	No cue, 1st	0.85(.36)	0.68(.47)	0.77(.42)	0.69(.46)
	No cue, 2nd	0.91(.29)	0.73(.45)	0.89(.31)	0.68(.47)
Same					
	Cue, 1st	0.84(.37)	0.67(.47)	0.88(.33)	0.65(.48)
	Cue, 2nd	1.00	0.77(.42)	0.89(.31)	0.67(.47)
	No cue, 1st	0.91(.29)	0.68(.47)	0.85(.36)	0.73(.45)
	No cue, 2nd	0.93(.25)	0.73(.45)	0.91(.29)	0.67(.47)
With 6 digits					
Cue/Order					
Change					
	Cue,1st	0.87(.34)	0.72(.45)	0.87(.34)	0.63(.49)
	Cue, 2nd	0.88(.33)	0.72(.45)	0.91(.29)	0.80(.40)
	No cue, 1st	0.77(.42)	0.63(.49)	0.76(.43)	0.68(.47)
	No cue, 2nd	0.72(.45)	0.69(.46)	0.75(.44)	0.73(.45)
Same					
	Cue, 1st	0.87(.34)	0.71(.46)	0.80(.40)	0.53(.50)
	Cue, 2nd	0.93(.25)	0.76(.43)	0.95(.23)	0.71(.46)
	No cue, 1st	0.79(.41)	0.77(.42)	0.83(.38)	0.64(.48)
	No cue, 2nd	0.88(.33)	0.68(.47)	0.92(.27)	0.64(.48)

Note. *N*=25.

Table 7
Dual-task conditions, proportions correct (with standard deviations), verbal sequence task, Experiment 2a.

With 3 digits

		Silent		Articulation	
		2 squares	5 squares	2 squares	5 squares
Cue/Order					
Change					
	Cue, 1st	0.99(.12)	1.00	0.96(.20)	0.99(.12)
	Cue, 2nd	1.00	0.99(.12)	0.96(.20)	0.96(.20)
	No cue, 1st	0.96(.20)	0.97(.16)	0.96(.20)	0.96(.20)
	No cue, 2nd	0.99(.12)	0.99(.12)	0.95(.23)	0.93(.25)
Same					
	Cue, 1st	0.95(.23)	0.97(.16)	0.92(.27)	0.96(.20)
	Cue, 2nd	0.96(.20)	0.95(.23)	1.00	1.00
	No cue, 1st	1.00	0.97(.16)	0.95(.23)	0.91(.29)
	No cue, 2nd	0.99(.12)	0.97(.16)	0.95(.23)	0.92(.27)
6 digits					
Cue/Order					
Change					
	Cue, 1st	0.89(.31)	0.81(.39)	0.67(.47)	0.73(.46)
	Cue, 2nd	0.92(.27)	0.91(.29)	0.81(.39)	0.73(.45)
	No cue, 1st	0.92(.27)	0.83(.38)	0.77(.42)	0.79(.41)
	No cue, 2nd	0.83(.38)	0.91(.29)	0.80(.40)	0.79(.41)
Same					
	Cue, 1st	0.79(.41)	0.79(.41)	0.59(.50)	0.65(.48)
	Cue, 2nd	0.85(.36)	0.79(.41)	0.73(.45)	0.71(.46)
	No cue, 1st	0.71(.46)	0.81(.39)	0.63(.49)	0.61(.49)
	No cue, 2nd	0.72(.45)	0.76(.43)	0.56(.50)	0.64(.48)

Note. *N*=25. Proportions correct include trials testing all serial positions.

Table 8
Dual-task conditions, proportions correct (with standard deviations), visual array task, Experiment 2b.

With 3 digits

	Silent		Articulation	
	2 squares	5 squares	2 squares	5 squares
Cue/Order				
Change				
Cue, 1st	0.83(.38)	0.67(.48)	0.78(.42)	0.70(.46)
Cue, 2nd	0.93(.25)	0.67(.48)	0.90(.30)	0.70(.46)
No cue, 1st	0.85(.36)	0.70(.46)	0.78(.42)	0.65(.48)
No cue, 2nd	0.85(.36)	0.68(.47)	0.73(.44)	0.73(.45)
Same				
Cue, 1st	0.92(.28)	0.60(.49)	0.78(.42)	0.57(.50)
Cue, 2nd	0.90(.30)	0.70(.46)	0.82(.39)	0.60(.49)
No cue, 1st	0.85(.36)	0.65(.48)	0.75(.44)	0.48(.50)
No cue, 2nd	0.87(.34)	0.67(.48)	0.80(.40)	0.55(.50)
With 6 digits				
Cue/Order				
Change				
Cue,1st	0.72(.45)	0.68(.47)	0.80(.40)	0.72(.45)
Cue, 2nd	0.90(.30)	0.68(.47)	0.88(.32)	0.63(.49)
No cue, 1st	0.68(.47)	0.73(.45)	0.67(.48)	0.65(.48)
No cue, 2nd	0.73(.45)	0.65(.48)	0.77(.43)	0.50(.50)
Same				
Cue, 1st	0.68(.47)	0.55(.50)	0.67(.48)	0.52(.50)
Cue, 2nd	0.87(.34)	0.62(.49)	0.77(.43)	0.67(.48)
No cue, 1st	0.78(.42)	0.50(.50)	0.77(.43)	0.47(.50)
No cue, 2nd	0.90(.30)	0.62(.49)	0.80(.40)	0.55(.50)

Note. *N*=20.

Table 9

Dual-task conditions, proportions correct (with standard deviations), verbal list task, Experiment 2b.

With 3 digits		Silent		Articulation	
		2 squares	5 squares	2 squares	5 squares
Cue/Order					
Change					
	Cue, 1st	0.97(.18)	0.97(.18)	0.87(.34)	0.85(.36)
	Cue, 2nd	0.98(.13)	0.98(.13)	0.92(.28)	0.97(.18)
	No cue, 1st	1.00	0.93(.25)	0.87(.34)	0.90(.30)
	No cue, 2nd	0.98(.13)	0.98(.13)	0.92(.28)	0.97(.18)
Same					
	Cue, 1st	0.98(.13)	0.90(.30)	0.93(.25)	0.83(.38)
	Cue, 2nd	0.97(.18)	0.98(.13)	0.93(.25)	0.95(.22)
	No cue, 1st	0.95(.22)	0.98(.13)	0.85(.36)	0.78(.42)
	No cue, 2nd	0.97(.18)	0.93(.25)	0.88(.32)	0.92(.28)
With 6 digits					
Cue/Order					
Change					
	Cue,1st	0.88(.32)	0.77(.43)	0.68(.47)	0.77(.43)
	Cue, 2nd	0.83(.38)	0.75(.44)	0.73(.45)	0.78(.42)
	No cue, 1st	0.83(.38)	0.82(.39)	0.78(.42)	0.82(.39)
	No cue, 2nd	0.83(.38)	0.85(.36)	0.78(.42)	0.68(.47)
Same					
	Cue, 1st	0.83(.38)	0.67(.48)	0.62(.49)	0.60(.49)
	Cue, 2nd	0.77(.43)	0.82(.39)	0.70(.46)	0.58(.50)
	No cue, 1st	0.82(.39)	0.63(.49)	0.65(.48)	0.52(.50)
	No cue, 2nd	0.68(.47)	0.75(.44)	0.67(.48)	0.55(.50)

Note. *N*=20. Proportions correct include trials testing all serial positions.

Table 10

Model Comparison, Experiment 2a

	n par. *k*	DIC
Visual memory task		
AS x Cue x Order x Verbal Load	**49**	**6151.1**
AS x Verbal Load + Cue x Order	35	6155.6
AS x Cue	29	6187.8
Only subject effects	25	6214.5
Verbal memory task		
AS x Cue x Order x Visual Load	49	3512.1
AS x Cue + Visual Load	32	3503.1
AS x Cue + Order	31	3502.1
AS x Cue	**29**	**3500.0**
AS	27	3500.6
Only subject effects	25	3549.1

Note. Smaller DIC values indicate superior model fit. For each task, the winning model is shown in bold text. MCMC chains at least 50,000 iterations long converged on the values given in this table. Preliminary hypothesis testing on all possible models was conducted with chains from 1,000 iterations. We report the most complex model (here, the 4-way interaction), the simplest model (only subject, i.e., no effects of independent variables or interactions), the winning model for each analysis (bold), and a selection of models adjacent in complexity to the winning model (chosen because their fits were competitive). All models included main effects of subject on *k* and *g*.

Table 11
Model Comparison, Experiment 2b

	n par. *k*	DIC
Visual memory task		
AS x Cue x Order x Verbal Load	44	5511.6
AS x Verbal Load + Cue x Order	**30**	**5501.6**
AS x Verbal Load	26	5528.1
Cue x Order	24	5581.6
AS + Cue + Order + Verbal Load	29	5507.6
Only subject effects	20	5610.3
Verbal memory task		
AS x Cue x Order x Visual Load	44	3109.2
AS x Visual Load + Cue x Order	30	3105.0
AS x Order + Cue + Visual Load	**29**	**3099.0**
AS x Order + Cue	26	3102.6
AS x Order + Visual Load	27	3100.1
AS x Order	24	3102.8
AS + Cue + Order + Visual Load	29	3100.0
Cue + Visual Load	25	3184.0
Only subject effects	20	3189.1

Note. Smaller DIC values indicate superior model fit. For each task, the winning model is shown in bold text. MCMC chains at least 50,000 iterations long converged on the values given in this table. Preliminary hypothesis testing on all possible models was conducted with chains from 1,000 iterations. We report the most complex model (here, the 4-way interaction), the simplest model (only subject, i.e., no effects of independent variables or interactions), the winning model for each analysis (bold), and a selection of models adjacent in complexity to the winning model (chosen because their fits were competitive). All models included main effects of subject on *k* and *g*.

Table 12
Single task proportions correct (with standard deviations), Experiment 3.

	Change	Same
Visual arrays		
2 squares	0.95(.21)	0.95(.21)
5 squares	0.74(.44)	0.70(.46)
Verbal sequences		
3 digits	0.98(.13)	0.99(.12)
6 digits	0.91(.28)	0.77(.42)

Note. N=59, but due to a programming error, only 19 participants have single-task data for 6-digit lists. WoMMBAT can handle an unbalanced experimental design, so all of these data were pooled for analysis. Verbal sequence proportions correct include data from all serial positions.

Table 13

Dual-task conditions, proportions correct (with standard deviations), visual array task, Experiment 3.

Digits-Squares (*N*=31)	0% reward	30% reward	70% reward	100% reward
Change trials				
3 digits				
2 squares	0.66(.47)	0.80(.40)	0.81(.39)	0.86(.35)
5 squares	0.57(.50)	0.63(.49)	0.65(.48)	0.63(.49)
6 digits				
2 squares	0.55(.50)	0.66(.47)	0.73(.45)	0.92(.27)
5 squares	0.39(.49)	0.51(.50)	0.60(.49)	0.58(.50)
Same trials				
3 digits				
2 squares	0.78(.42)	0.94(.25)	0.93(.26)	0.94(.23)
5 squares	0.61(.49)	0.62(.49)	0.68(.47)	0.69(.46)
6 digits				
2 squares	0.75(.43)	0.91(.28)	0.91(.29)	0.96(.21)
5 squares	0.51(.50)	0.62(.49)	0.68(.47)	0.64(.48)
Squares-Digits (*N*=28)				
Change trials				
3 digits				
2 squares	0.77(.42)	0.85(.36)	0.91(.29)	0.94(.24)
5 squares	0.63(.49)	0.59(.49)	0.74(.44)	0.80(.40)
6 digits				
2 squares	0.63(.49)	0.79(.41)	0.81(.40)	0.95(.21)
5 squares	0.57(.50)	0.54(.50)	0.63(.49)	0.77(.42)
Same trials				
3 digits				
2 squares	0.80(.41)	0.90(.30)	0.95(.22)	0.98(.15)
5 squares	0.50(.50)	0.67(.47)	0.63(.48)	0.75(.43)
6 digits				
2 squares	0.66(.47)	0.87(.34)	0.92(.28)	0.95(.23)
5 squares	0.49(.50)	0.60(.49)	0.70(.46)	0.73(.45)

Note. N=59.

Table 14

Dual-task conditions, proportions correct (with standard deviations), verbal list task, Experiment 3.

Digits-Squares (N=31)		0% reward	30% reward	70% reward	100% reward
Change trials					
2 squares					
	3 digits	0.89(.32)	0.97(.18)	1.00	0.95(.22)
	6 digits	0.71(.45)	0.87(.34)	0.83(.38)	0.89(.31)
5 squares					
	3 digits	0.85(.36)	0.97(.18)	0.98(.14)	0.99(.12)
	6 digits	0.70(.46)	0.83(.38)	0.86(.35)	0.87(.34)
Same trials					
2 squares					
	3 digits	0.83(.38)	0.98(.14)	0.99(.08)	0.96(.19)
	6 digits	0.61(.49)	0.78(.41)	0.79(.41)	0.81(.39)
5 squares					
	3 digits	0.73(.44)	0.92(.27)	0.98(.14)	0.98(.15)
	6 digits	0.56(.50)	0.75(.43)	0.84(.37)	0.81(.40)
Squares-Digits (N=28)					
Change trials					
2 squares					
	3 digits	0.88(.32)	0.98(.15)	0.99(.12)	0.98(.12)
	6 digits	0.76(.43)	0.79(.41)	0.85(.36)	0.82(.39)
5 squares					
	3 digits	0.86(.35)	0.94(.24)	0.98(.12)	0.99(.09)
	6 digits	0.74(.44)	0.81(.40)	0.85(.36)	0.79(.41)
Same trials					
2 squares					
	3 digits	0.84(.37)	0.97(.17)	0.99(.09)	0.98(.13)
	6 digits	0.65(.48)	0.80(.40)	0.84(.37)	0.79(.41)
5 squares					
	3 digits	0.72(.45)	0.96(.20)	0.98(.15)	0.99(.09)
	6 digits	0.50(.50)	0.77(.42)	0.85(.36)	0.84(.37)

Note. N=59. Proportions correct include data from all serial positions.

Table 15
Model Comparison, Experiment 3

	n par. *k*	DIC
Visual memory task		
Reward x Order x Verbal Load	75	9155.4
Reward x Verbal Load + Order	**69**	**9151.7**
Reward x Verbal Load	67	9152.3
Only subject effects	59	9528.2
Verbal memory task		
Reward x Order x Visual Load	75	4794.8
Reward x Visual Load + Order	69	4785.2
Reward x Visual Load	**67**	**4785.1**
Reward	63	4791.5
Only subject effects	59	5084.7

Note. Smaller DIC values indicate superior model fit. For each task, the winning model is shown in bold text. MCMC chains of 20,000-50,000 iterations long converged on the values given in this table. Preliminary hypothesis testing on all possible models was conducted with chains from 1,000-2,000 iterations. We report the most complex model (here, the 3-way interaction), the simplest model (only subject, i.e., no effects of independent variables or interactions), the winning model for each analysis, and the most plausible model that was more parsimonious than the winning model. All models included main effects of subject on *k* and *g*.

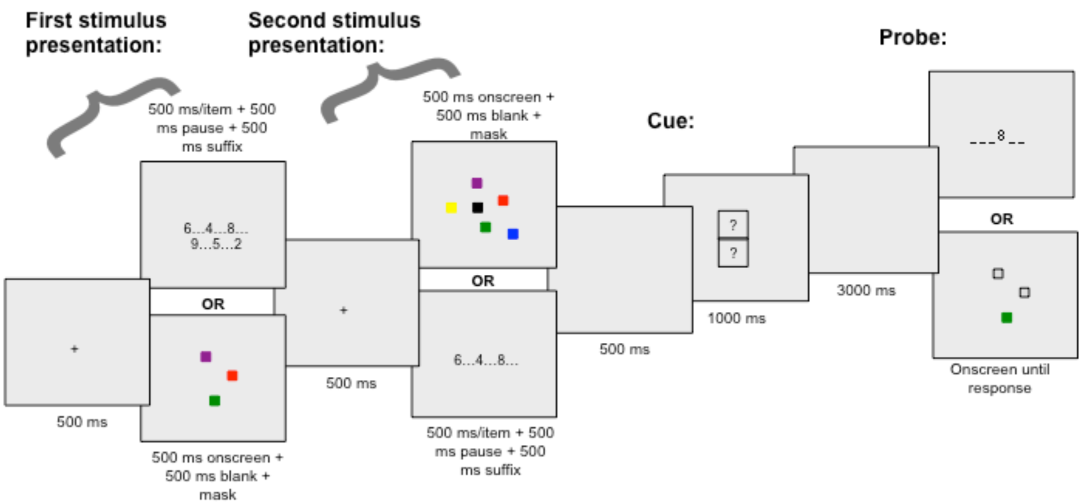

Figure 1. Trial procedure, Experiments 1a and 1b. Following the upper trajectory, the verbal probe is a change probe (8 was in the 3rd, not the 4th position) and following the lower trajectory, the visual probe is a same probe (the colored object was also green at study).

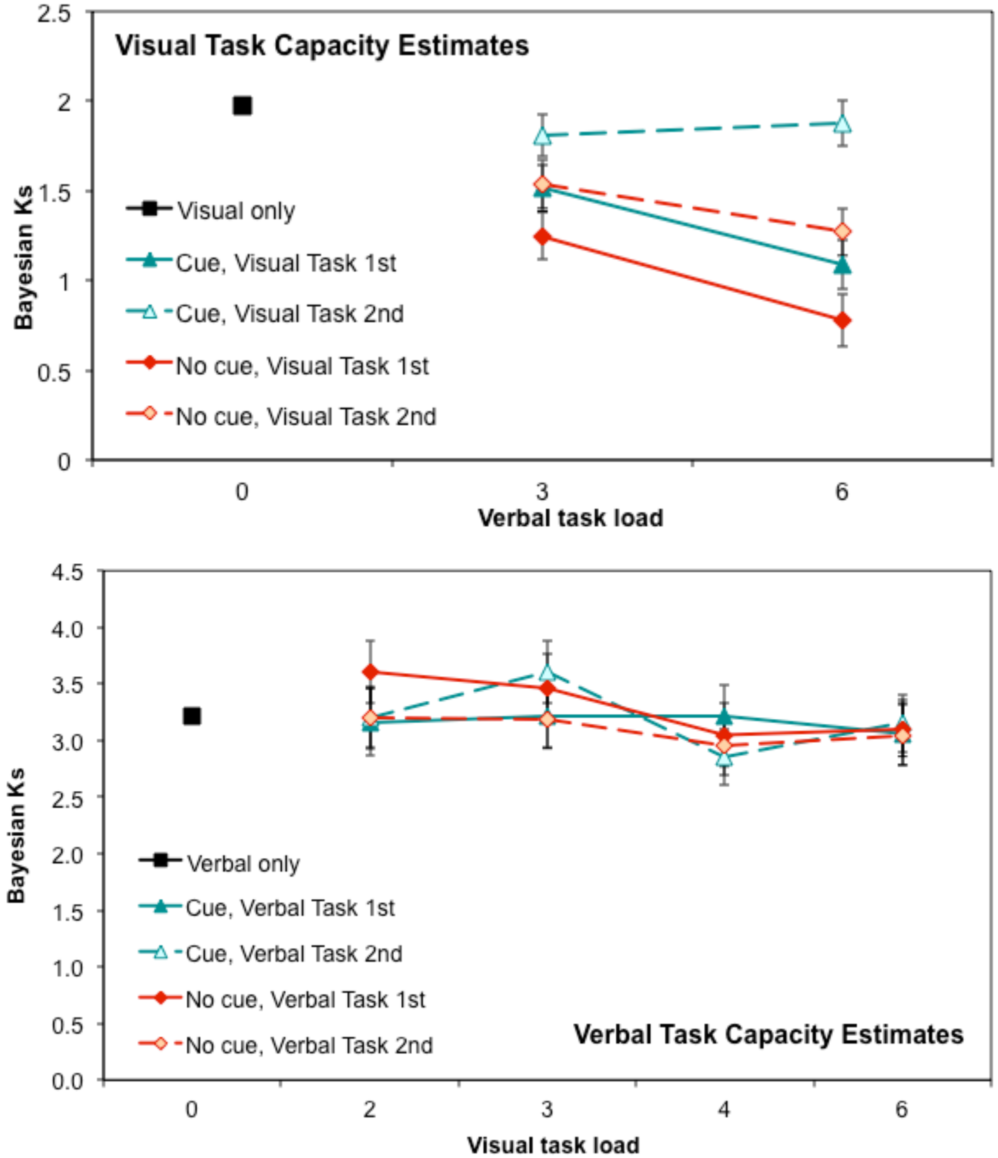

Figure 2. Experiments 1a and 1b, capacity estimates (with posterior standard deviations on the difference between each dual-task condition and single-task performance).

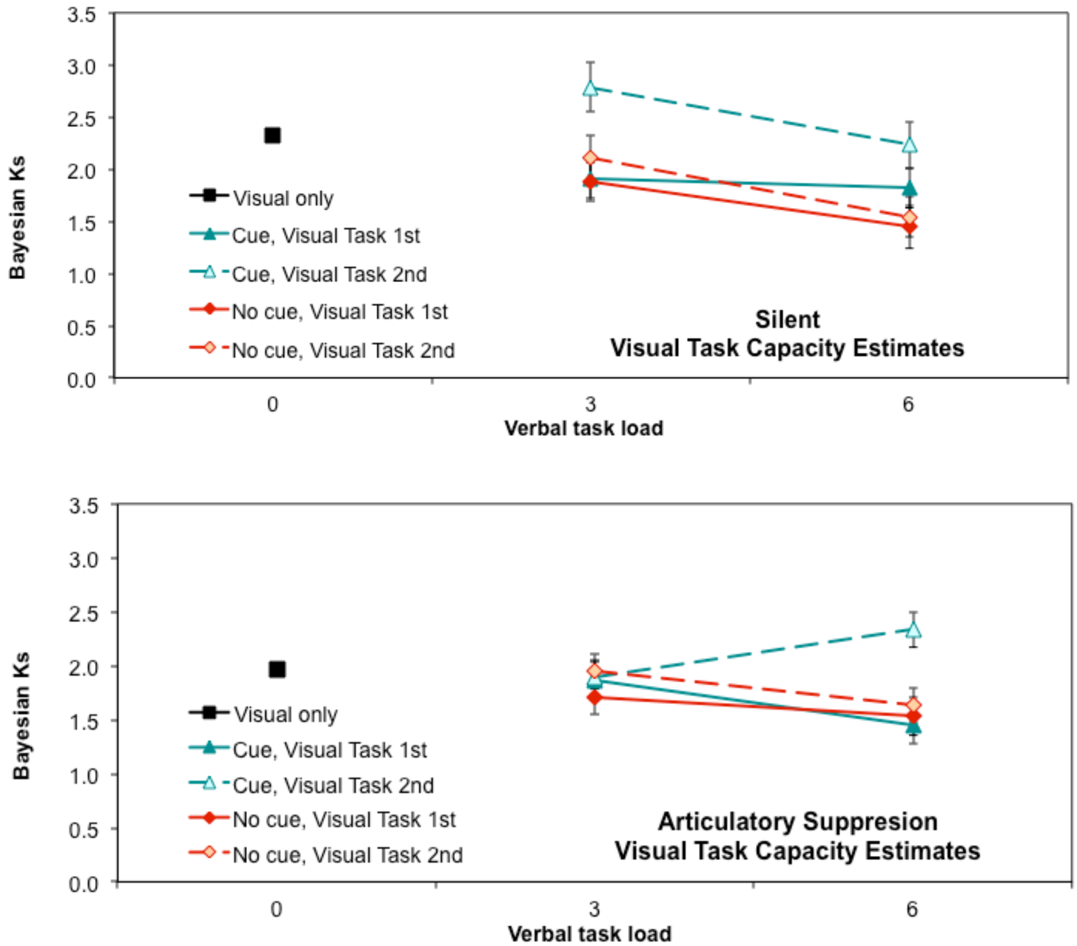

Figure 3. Visual task capacity estimates, Experiment 2a (with posterior standard deviations on the difference between each dual-task condition and single-task performance). The upper panel shows estimates during silent blocks and the lower panel shows estimates during articulation blocks.

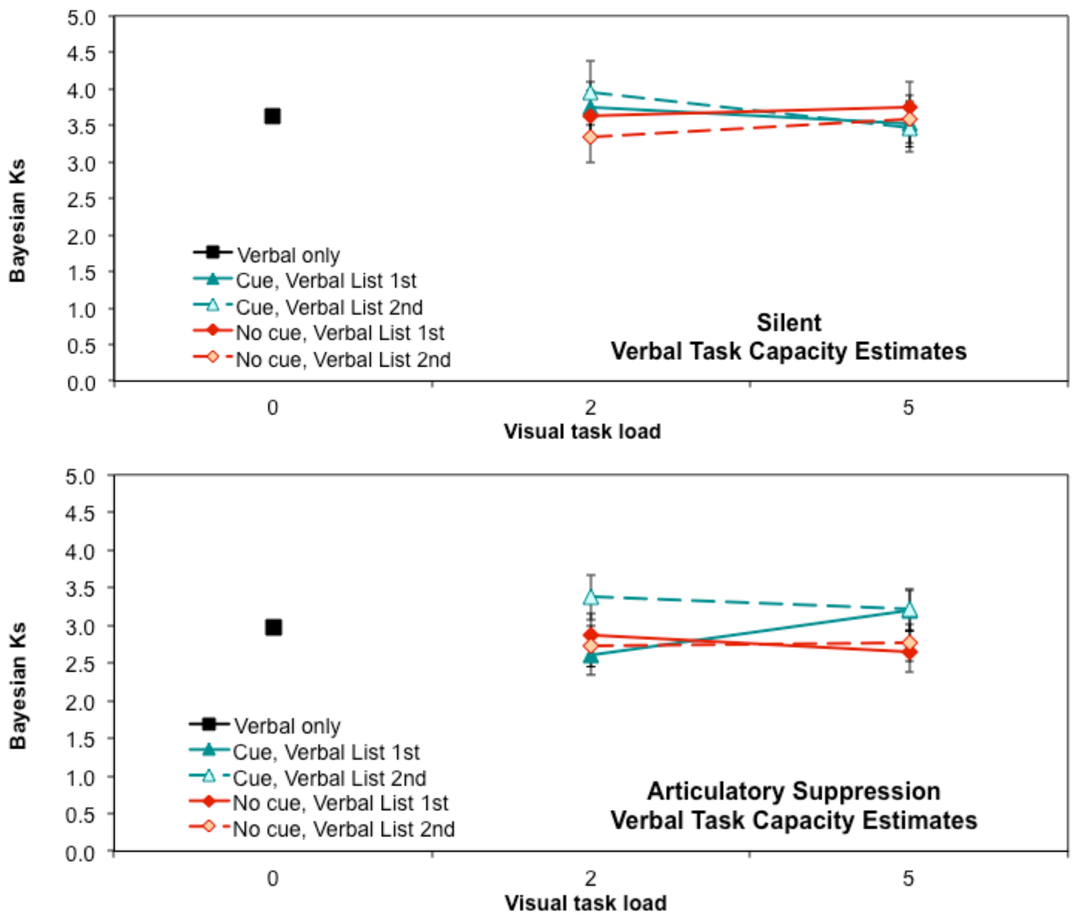

Figure 4. Verbal task capacity estimates, Experiment 2a (with posterior standard deviations on the difference between each dual-task condition and single-task performance). The upper panel shows estimates during silent blocks and the lower panel shows estimates during articulation blocks.

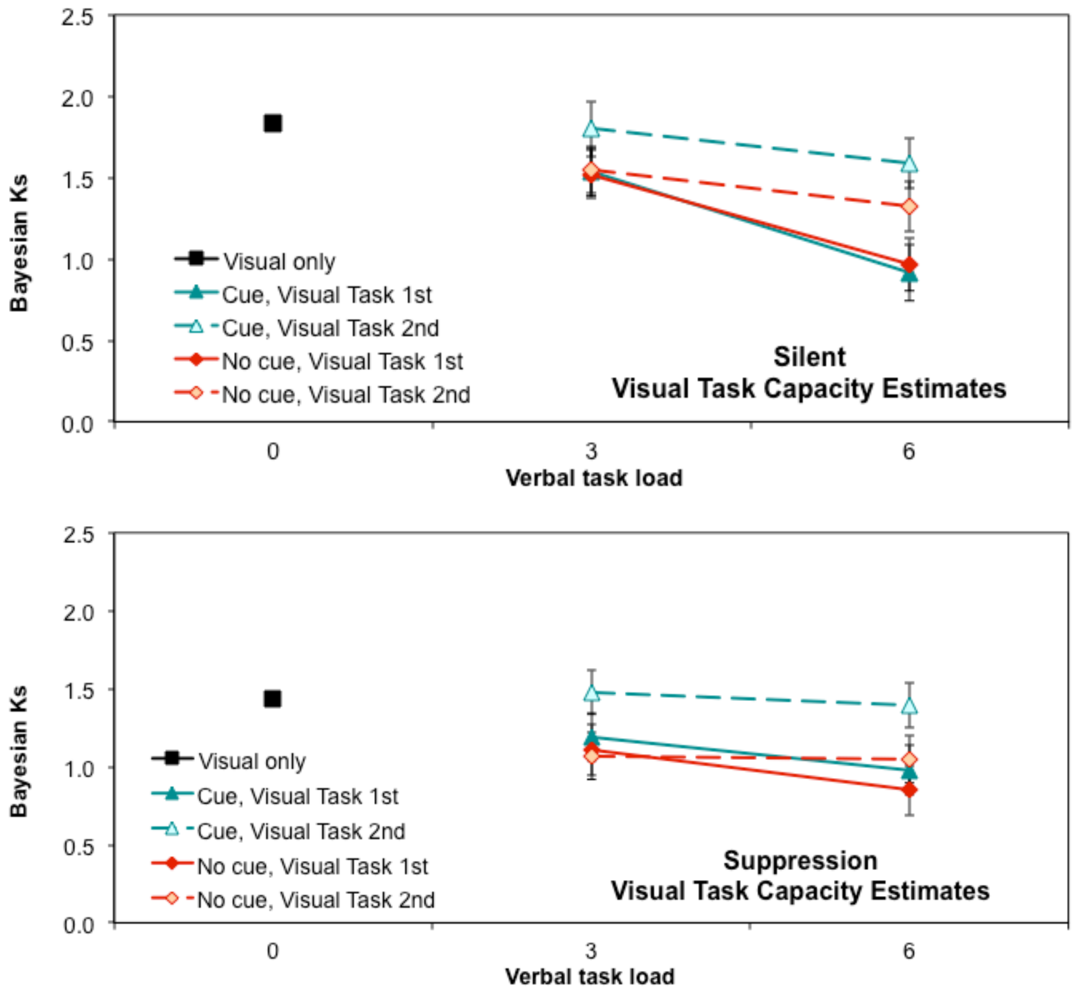

Figure 5. Visual task capacity estimates, Experiment 2b (with posterior standard deviations on the difference between each dual-task condition and single-task performance). Contents of visual arrays were presented serially in Experiment 2b. The upper panel shows estimates during silent blocks and the lower panel shows estimates during articulation blocks.

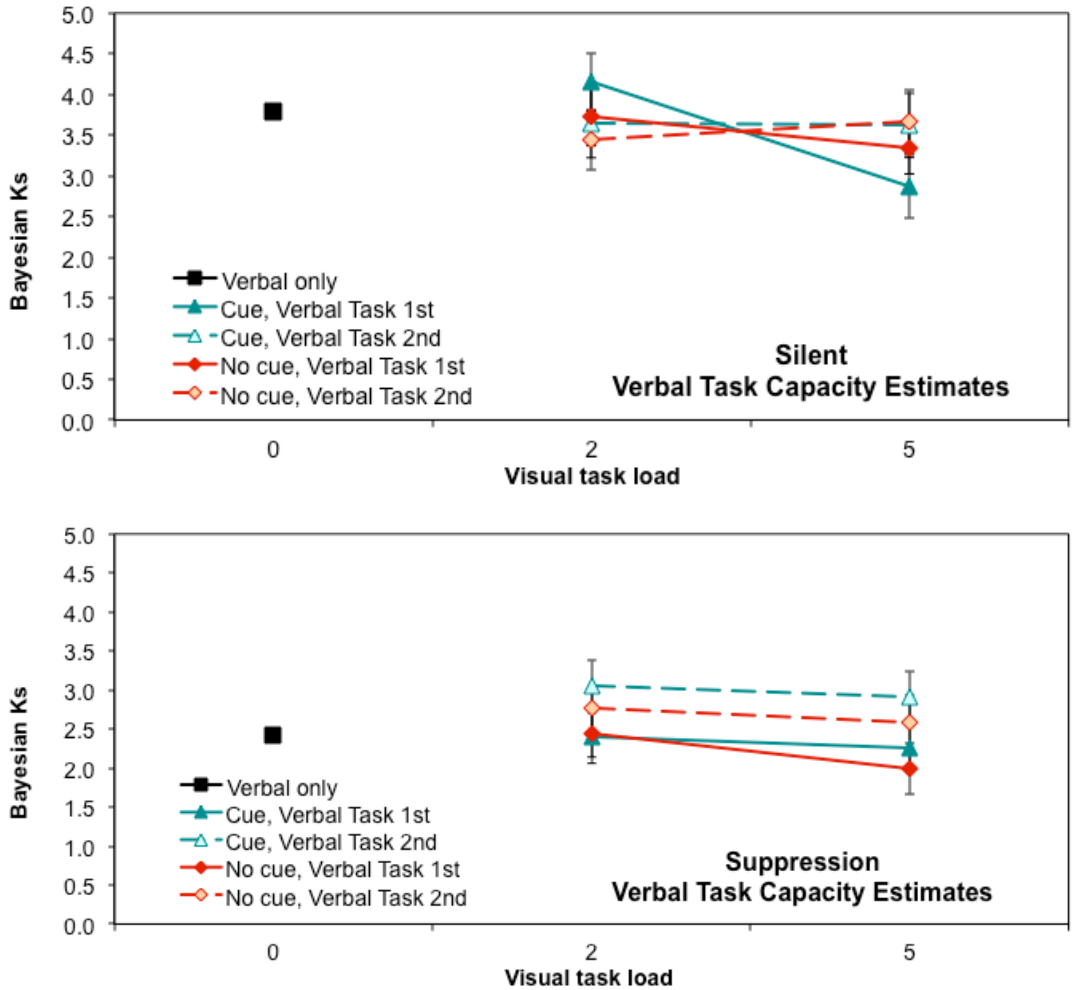

Figure 6. Verbal task capacity estimates, Experiment 2b (with posterior standard deviations on the difference between each dual-task condition and single-task performance). Contents of visual arrays were presented serially in Experiment 2b. The upper panel shows estimates during silent blocks and the lower panel shows estimates during articulation blocks.

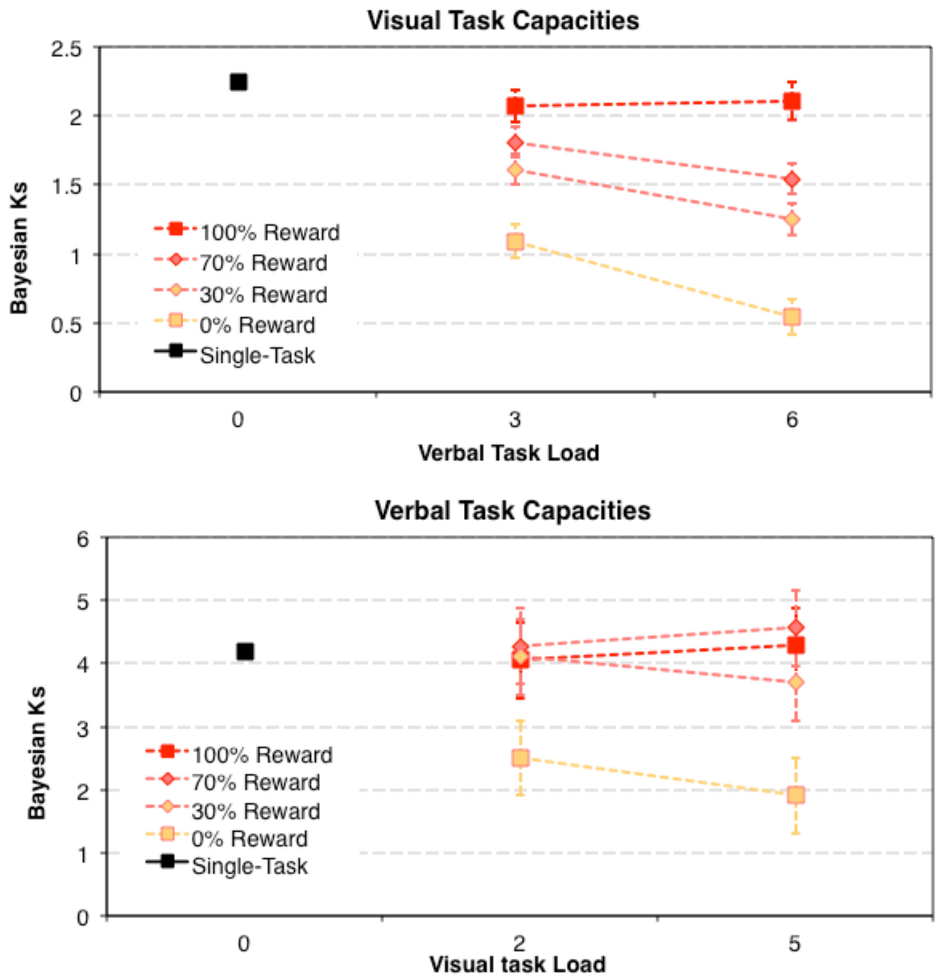

Figure 7. Experiment 3, capacity estimates (with posterior standard deviations on the difference between each dual-task condition and single-task performance).