Visual short-term memory always requires general attention

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Abstract

The role of attention in visual memory remains controversial; while some evidence suggests that memory for binding between features demands no more attention than memory for the same features, other evidence indicates cognitive costs or mnemonic benefits of explicitly attending to binding. We attempt to reconcile these findings by examining how memory for binding, features, and features during binding is affected by a concurrent attention-demanding task. We demonstrate that performing a concurrent task impairs memory for as few as two visual objects regardless of whether each object includes one or more features. We argue that this pattern of results reflects an essential role for domain-general attention in visual memory, regardless of the simplicity of the to-be-remembered stimuli. We discuss the implications of these findings for theories of visual working memory.

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When a claim seems too good to be true, it meets with persistent skepticism. One such claim in cognitive psychology, that maintaining multi-feature visual objects requires no more attention than memorizing the same number of uni-feature visual objects (Luck & Vogel, 1997), has endured a barrage of hypothesis tests yielding mixed evidence. If binding requires attention, performing non-visual but attention-demanding tasks while maintaining visual memoranda should impair memory for binding more than memory for features, yet much evidence suggests otherwise. While the predicted interaction that would disconfirm Luck and Vogel's claim has been reported (Brown & Brockmole, 2010), it has also eluded investigators despite earnest attempts to find it (e.g., Allen, Baddeley, & Hitch, 2006; Allen, Hitch, Mate, & Baddeley, in press).

Evaluation of this claim is essential for advancing theories of working memory. Understanding binding influences assumptions about the proper unit of measurement in working memory (Cowan, 2001; Cowan & Rouder, 2009), and also underlies debate on how attention and storage are related. This is most clearly evident when comparing two proposed versions of Baddeley's multi-component model (Baddeley, 2000; Baddeley, Allen, & Hitch, 2011). Addressing the limitations of his three-component model (Baddeley, 1986), Baddeley (2000) proposed a domain-general episodic buffer, capable of maintaining bindings between features initially encoded into the domain-specific buffers. Information was believed to move to the episodic buffer via the central executive. Consistently with the conclusions of Wheeler and Treisman (2002), who failed to replicate Luck and Vogel's (1997) claim, Baddeley proposed that the domain-specific visual-spatial buffer maintained features separately, and that maintenance of binding

occurred after applying attention. This assumption led to a series of hypothesis tests, in which Allen, Baddeley, and Hitch (2006) compared memory for bound objects and features during an attention-demanding backwards-counting task. Allen et al. found no evidence that the concurrent task impacted memory for multi-feature objects any more than uni-feature objects, an outcome at odds with Baddeley's (2000) proposal. The replicability of these findings (e.g., Allen, Hitch, & Baddeley, 2009; Allen, Hitch, Mate, & Baddeley, in press) and their consistency with complementary tests of the attentional costs of maintaining feature bindings (e.g., Delvenne & Bruyer, 2004; Delvenne, Cleeremans, & Laloyaux, 2010; Gajewski & Brockmole, 2006; Luck & Vogel, 1997; Vogel, Woodman, & Luck, 2001) led Baddeley and colleagues to propose that feature binding can be maintained within the domain-specific visual buffer without any additional involvement from the central executive (Baddeley et al., 2011).

However, this proposal neglects a robust and consistent finding: in each iteration of the basic experimental design of Allen et al. (2006), performing a non-visual task concurrently with a visual recognition memory task reduced accuracy, sometimes considerably. This has been shown to occur with four (Allen et al., 2006) or three (Allen et al., in press; Brown & Brockmole, 2010) memoranda, during backwards counting (Allen et al., 2006), when concurrent tasks carried out during encoding and maintenance periods (Allen et al., 2006) or during encoding, maintenance, and test (Allen et al., in press; Brown & Brockmole, 2010), and with encoding times varying from 250 ms to 1000 ms. Furthermore, other researchers also report interference between non-visual tasks and visual recognition, whether interference occurs during encoding (Dell'Acqua & Jolicœur, 2000) or retention (Phillips & Christie, 1977; Stevanovski & Jolicœur, 2007).

Any model of working memory should be able to explain why interference is so consistently observed between non-visual attention-demanding tasks and visual short-term memory storage.

We therefore aimed to replicate and extend the work of Allen et al. (2006; in press) and Brown and Brockmole (2010) by examining the effects of a non-visual secondary task on memory for visual objects with one or more features. Our design includes innovations that add to previous work. First, we included study displays of only 2 objects. Possibly, the elusiveness of the interaction between attention condition and visual materials is related to visual memory limits: if participants are given more visual memoranda than they can comprehend, perhaps any concurrent task provokes strong interference, obscuring this interaction. If this is in fact the case, then discrepancies between Brown and Brockmole and Allen et al. could have occurred because Brown and Brockmole happened to sample a group of participants with somewhat higher visual memory capacities. Second, we subjected our data to Bayesian analysis of variance (ANOVA; Rouder, Morey, Speckman, & Province, in press). Whereas a typical analysis does not allow for the interpretation of null effects, this Bayesian technique allows us to evaluate the strength of the evidence *against* including an interaction in the ANOVA model.

Including trials with only 2 visual items will also yield theoretically important evidence regarding interference with visual memory. In verbal memory, small amounts of information can be retained without dual-task cost, but when verbal lists reach or exceed capacity, dual-task costs appear (Baddeley & Hitch, 1974; Morey & Cowan, 2004). If we observe dual-task costs even with sub-capacity visual memory loads, this would cast

further doubt on whether visual recognition tasks are accessing a strictly visual, domain-

specific resource (Cowan & Morey, 2007; Saults & Cowan, 2007; Vergauwe, Dewaele,

Langerock, & Barrioullet, 2012).

Finally, we planned to compare correct rejections of feature lures across groups in

order to compare feature memory during binding with feature memory when binding was

not required. Despite different contexts in which each group studied the color-shape

stimuli, these feature lures could be rejected when participants remembered all features of

a dimension. Similar analyses have previously suggested that maintaining binding can

boost memory for the weaker of two features (Morey, 2009; 2011). Replicating this

finding with two visual feature dimensions would give further insight into whether

features are maintained differently when binding is required compared to when it is not.

Method

Participants

Participants were recruited from the psychology student population at the

University of Groningen and indicated consent in writing. One participant with

inadequate color vision was excluded from analyses along with thirteen participants who

responded below chance in the tone categorization task, leaving a sample with N=102 (37

males), 19-33 years old (M=22.09, SD=2.43).

Apparatus and stimuli

Participants worked in private booths. Stimuli and responses were controlled

using E-Prime (Schneider, Eschmann, & Zucolotto, 2002). To-be-remembered stimuli

were chosen from the shapes and colors depicted in Figure 1. Stimuli were always

presented in one of 8 fixed locations taken from the perimeter of an imaginary central circle.

In the tone categorization task, participants judged three tones (220, 460, and 620 Hz) as low, medium, or high by pressing 1, 2, or 3 respectively on the number keypad.

Procedure

Each session began with a color-blindness screening (adapted from Ishihara, 1966). An experimenter then explained the instructions to the participant and supervised a practice session, comprising 8 trials of the visual memory task, 10 tone classification trials, and 8 dual-task trials. After the practice session, the participant completed the remainder of the trials independently.

Each trial began with a 1000-ms fixation (+), followed by a 500-ms array of 2, 3, or 4 objects. Three within-subjects retention conditions were randomly mixed. In some trials, a probe appeared after only 900 ms, similar to retention intervals used previously (e.g., Allen et al., 2006; in press; Brown & Brockmole, 2010). Sometimes a probe appeared after an unfilled period of 4100 ms. In concurrent tone trials, two tones were presented during a 4100 ms retention interval, which participants were encouraged to judge as quickly as possible. The first tone always occurred 100 ms after the offset of the study array, and the second tone occurred 1200 ms after the first tone. Responses were recorded for 1800 ms after the onset of each tone. To encourage participants not to ignore the tones, a warning appeared in between trials whenever their cumulative accuracy dropped below 50%. The retention interval was followed by the presentation of a probe color, shape, or colored shape, shown at the center of the screen.

Participants in the binding-probe group (*N*=24) always experienced bound color-shape probes, and thus needed to try to remember color-shape binding throughout the session. These participants indicated at test whether that particular colored shape was part of the study array. Half of the probes were targets, in which the same colored shape was studied. The remaining trials were equally divided between three types of lure probes: 1) recombined lures included a studied shape and a studied color that came from different studied objects, 2) shape lures included an unstudied shape presented in a studied color, and 3) color lures included a studied shape presented in an unstudied color.

In the remaining three groups, participants were probed with single features. In the mixed feature-probe group (*N*=28), participants observed colored shapes at study and a gray shape or a colored arc at test and indicated whether that shape or color was present in the study array. Targets, lures, and color and shape probes occurred with equal probability. Participants in the mixed feature-probe group did not need to attend to color-shape binding explicitly, but they never knew in advance which feature would be tested in a trial, and so must have tried to remember both colors and shapes throughout the session. Participants in the blocked feature-probe group (*N*=33) experienced the same task as participants in the mixed feature-probe group, except that tests of color and shape recognition were presented in separate blocks, so that the participant knew at study which dimension would be tested. Participants in the feature-only group (*N*=17) were shown either uni-feature colored arcs or gray shapes in each study display, in separate blocks, with uni-feature probes. Even implicit color-shape binding would of course be impossible in the feature-only group; we therefore take performance in these conditions to be our simplest measure of visual short-term memory.

Results

For all analyses, we set a criterion of $p<.05$ for declaring statistical significance,

and reported generalized eta square values to convey effect size (see Bakeman, 2005).

We first report analyses of hit rate (correctly responding that a target was present at

study) minus false alarm rate (incorrectly responding that a lure was present at study).

Because we tested memory for several items with a single probe, hits – false alarms (H-

FAs) is an appropriate measure of discrimination (Rouder, R.D. Morey, C.C. Morey, &

Cowan, 2011). Figures 2 and 3 provide mean H-FAs by each manipulated variable in

each group to show the robustness of the effects and to help readers compare these results

to previous findings. However to simplify hypothesis testing, we report analyses of

variance (ANOVAs) collapsing across all set sizes and for set size 2 to support our claim

that the secondary task impairs visual memory even for material that should be within

normal visual short-term memory capacity. We further evaluated non-significant

interactions by comparing Bayes factors (Rouder, Morey, Speckman, & Province, in

press). Bayes factors enable comparison of models within an ANOVA design, which give

us the power to evaluate the evidence in the data for the absence of an interaction without

interpreting a non-significant p-value. Finally, we compared rates of correct rejections of

features lures as a means of comparing feature memory during binding with separate

feature memory (cf. Morey, 2011).

Discrimination: Hits – False Alarms

Consistently with previous research (e.g., Allen et al., 2006), preliminary analyses

showed that in the feature-probe groups, color discrimination was superior to shape

discrimination. We therefore compared binding discrimination to color and shape

discrimination in separate analyses, to avoid reporting interactions that might be due to differences between color and shape feature discrimination, rather than feature and binding discrimination.

Binding versus color features. We carried out a 2-way ANOVA on H-FAs with task group (binding-probes, mixed feature-probes, blocked feature-probes, features-only) as a between-participants factor and retention condition (900 ms, 4100 ms, 4100 ms with tones) as a within-participant factor. This analysis revealed significant main effects of retention condition ($F(2,196)=42.91$, $MSE=0.02$, $\eta^2_G=.12$) and group ($F(3,98)=15.87$, $MSE=0.07$, $\eta^2_G=.25$), but no interaction ($F(6,196)=1.80$, $\eta^2_G=.02$). Participants performed best with the 900-ms retention interval ($M=0.74$), poorer with the unfilled 4100-ms interval ($M=0.65$), and even worse with a filled 4100-ms retention interval ($M=0.43$, *Fisher's Least Significant Difference*=0.04). Bonferroni-corrected *t*-tests indicated that discrimination in the binding-probe group ($M=0.44$) was lower than in the mixed-feature group ($M=0.58$), which was lower than discrimination in the blocked-feature ($M=0.70$) and feature-only groups ($M=0.69$), which did not significantly differ ($p\approx1$). Means for all combinations of these conditions (plus set size) are given in Figure 2.

This analysis provides no evidence that a secondary task impairs memory for binding any more than memory for features. We computed Bayes factors (Rouder et al., in press) for each combination of main effects and interaction in our ANOVA model, which can then be compared. The model including main effects of group and retention condition yielded the highest Bayes factor ($BF=112.82$) followed by the model including both main effects and their interaction ($BF=106.19$). Using these values, we calculated

the Bayes factor between the two models, whose interpretation is straightforward: the
evidence provided in the data for the simpler model is greater by a factor of 761:1.

Examining the means in Figure 2 makes clear how unfailingly the concurrent task
impaired recognition, consistent with the observation that storing visual memoranda
draws upon domain-general attention resources. We repeated the ANOVA above
including only the displays of two stimuli to provide an even stronger test of this
hypothesis. Here, we did observe a significant retention condition by group interaction
($F(6,196)=3.11$, $MSE=0.03$, $\eta^2_G=.04$), and investigated by carrying out two follow-up
ANOVAs, one comparing the 900-ms and 4100-ms unfilled retention intervals, and one
comparing the 4100-ms unfilled and filled retention intervals. When comparing the short
and long conditions, the interaction remained significant ($F(3,98)=3.39$, $MSE=.03$, η^2_G
$=.03$); in the binding-probe and mixed-feature groups, larger differences were observed
between the 900-ms and 4100-ms retention intervals (compare the light and medium gray
circles in Figure 2, upper panels) than in the blocked-feature and feature-only groups
(Figure 2, lower panels). However, considering 4100-ms filled and unfilled retention
intervals, group and retention condition did not significantly interact ($F(3,98)=0.92$, η^2_G
$=.009$), even if limited to comparisons between only the binding-probe and blocked
feature groups ($F(1,55)=2.90$, $\eta^2_G=.01$) or the binding-probe and feature-only groups
($F(1,39)=0.12$, $\eta^2_G<.001$).

Binding versus shape features. We carried out the ANOVAs described above
including binding-probes and shape feature-probes, observing main effects of retention
condition ($F(2,196)=43.33$, $MSE=.02$, $\eta^2_G=.13$) and group ($F(3,98)=3.02$, $MSE=0.09$, η^2_G
$=.06$), but no interaction ($F(6,196)=0.41$, $\eta^2_G=.004$). As in the previous analysis,

discrimination was best with the 900-ms interval ($M=0.60$), significantly lower with the

unfilled 4100-ms interval ($M=0.49$), and lower still with the filled 4100-ms interval

($M=0.26$; $FLSD=0.04$). Levels of group were less distinct in this analysis than in the

binding versus color analysis. Bonferroni-corrected t-tests indicated significant

differences between the mixed-feature ($M=0.38$) and both the blocked-feature ($M=0.50$)

and feature-only ($M=0.49$) groups, but no other comparisons were statistically significant.

According to a Bayes factor analysis, models excluding the interaction were preferred.

The model with the highest Bayes factor included only the retention condition effect

($BF=98.35$), followed by the model including both main effects ($BF=96.62$). The best

model including the interaction yielded a Bayes factor of 88.54. Comparing these models,

the retention condition effect model was preferred to the two-effect model by a factor of

6:1, but both of these models were preferred to the best model including the interaction

by factors of at least 3218:1.

We repeated this ANOVA including only the 2-item trials. Only a main effect of

retention condition was observed ($F(2,196)=29.62$, $MSE=0.04$, $\eta^2_G=.11$), with levels

ordered in the same manner as prior analyses (refer to Figure 3). Neither the effect of

group ($F=1.53$, $\eta^2_G=.03$) nor the interaction ($F=1.45$, $\eta^2_G=.02$) were statistically

significant.

Correct lure rejections

Comparing correct rejections of feature lures across the binding and feature-only

groups gives some idea of whether feature memory itself differs when remembering

binding is an explicit task goal (Morey, 2011). Even though participants in the binding-

probe condition made a judgment about a colored shape, recalling which features had

been present could aid in the correct rejection of feature lures, where an unstudied feature

was part of the test probe. A plausible benefit that may come with intentional binding is

superior memory for the features involved, particularly the weaker features (Morey,

2009; 2011). These benefits should appear as improved retention of larger numbers of

features when maintaining binding is necessary.

We therefore ran an ANOVA on correct rejection rates with group as a between-

participants factor and set size as a within-participant factor separately for shape feature

lures, the weaker feature according to the discrimination analyses. We observed

significant main effects of group ($F(3,98)=4.37$, $MSE=0.05$, $\eta^2{}_G=.08$), set size

($F(2,196)=27.73$, $MSE=0.01$, $\eta^2{}_G=.09$), and an interaction of these factors

($F(6,196)=3.38$, $MSE=.01$, $\eta^2{}_G=.03$). We conducted follow-up ANOVAs in order to

diagnose the meaning of this interaction, including combinations of three task groups. An

analysis including only the feature-probe groups yielded no significant group by set size

interaction ($F=2.07$, $p>.08$). Analyses excluding each other group but including the

binding-probe group always yielded significant group by set size interactions (Fs from

3.02-4.60). This suggests that in the full analysis, the group by set size interaction reflects

differences between the binding-probe and feature-probe groups. Furthermore, in an

analysis including only the binding-probe and features-only groups, no interaction

emerged ($F=1.06$). This is consistent with the idea that remembering a colored shape is

more like remembering a uni-dimensional object than remembering the features of multi-

dimensional objects.

Figure 4 depicts correct rejection rate as a function of group, feature dimension,

and set size. In the binding-probes group, set size has a smaller effect on shape correct

rejections than that seen in the mixed and blocked feature-probes groups. This suggests

that the interaction reflects steeper cost for encoding more items in the conditions in

which bound features are studied but only one feature is tested than in the binding-probe

condition, where remembering binding is necessary for making correct responses.

Discussion

Consistently with the claim that remembering bindings between features requires

no more attention than remembering the features themselves, these analyses yielded no

evidence of differential effects of dividing attention during retention of binding or

features only. Regardless of the number of visual memoranda or the number of features

per object, performing simple non-visual judgments during maintenance provoked robust

interference. Comparison of Bayes factors, which allow us to weigh the evidence for an

interaction in these data, yielded no reason to believe that an interaction between group

and retention condition is present in these data. We therefore conclude that the persistent

secondary task cost we observed is similar for binding and feature judgments, replicating

Allen et al. (2006; in press).

Importantly, we also confirmed that performing a non-visual, attention-

demanding task provoked a concurrent cost even when only two objects were to be

remembered. This finding, along with our analysis of lure correct rejections showing that

weaker features benefit from maintenance during binding (see Figure 4), is also difficult

to reconcile with models proposing that visual features are separately maintained (e.g.,

Wheeler & Treisman, 2002). Our results also tended to suggest that recognition of bound

objects (as measured by H-FA) tended to be less accurate than recognition of separate

features, something not clearly consistent with the idea that maintaining bound objects

costs no more than maintaining separate features (e.g., Luck & Vogel, 1997). Together, it seems that maintaining multi-feature objects conveys both cognitive benefits and costs (Morey, 2009; 2011), but maintaining any sort of visual memoranda implicates general attentional processes.

Our results joins others showing interference between maintenance of visual information and performance of some non-visual task (e.g., Saults & Cowan, 2007; Stevanovski & Jolicœur, 2007; Vergauwe et al., 2012) in challenging theories proposing separate resources for domain-general attentional processes and visual-spatial storage. Research using other techniques already hints at close relationships between visual short-term memory and visual working memory (e.g., Miyake, Friedman, Rettinger, Shah, & Hegarty, 2001) and suggests that storage capacity in visual short-term memory is strongly related to attentional selection (Vogel, McCullough, & Machizawa, 2005). Other work explicitly suggests that verbal information has access to more resources than visual-spatial information (e.g., Camos, Lagner, & Barrouillet, 2009; Camos, Mora, & Oberauer, 2011; Hudjetz & Oberauer, 2007; Morey & Mall, in press). While evidence suggests that verbal memoranda have access to both domain-specific and domain-general working memory resources (Jarrold, Tam, Baddeley, & Harvey, 2011), isolating analogous domain-specific visual working memory resources has proved more difficult; here we show that even small amounts of uni-dimensional visual memoranda cannot be held without cost during a non-visual task. Going forward, greater consideration should be given to understanding relationships between general attention and visual memory regardless of whether binding is to-be-remembered. Specifically, further consideration should be given to the idea that visual maintenance is carried out not within a domain-

specific buffer, but instead within a domain-general system such as the focus of attention (Cowan, 2001).

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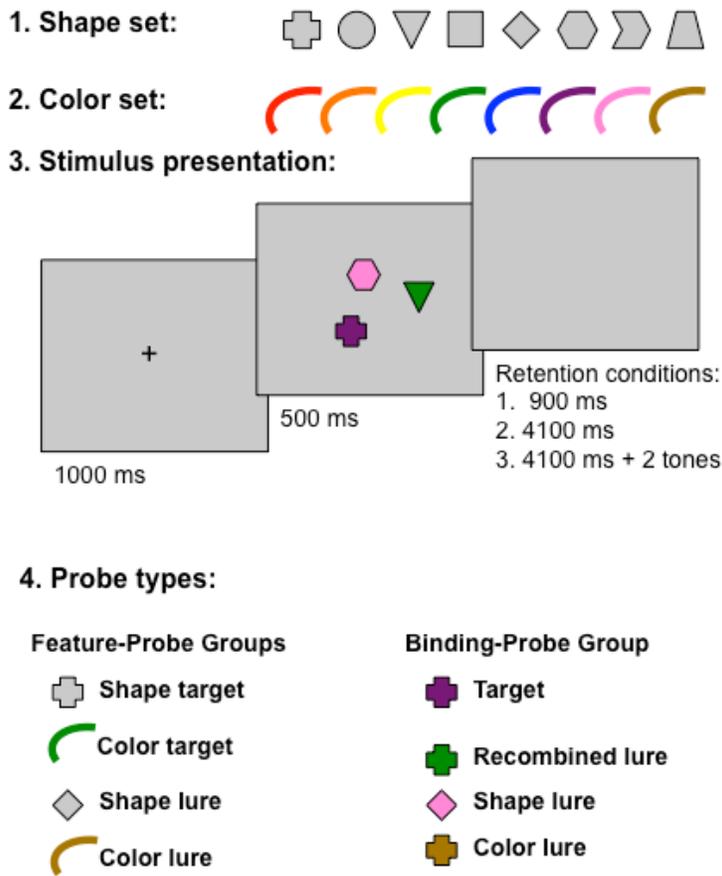
Figure 1. Stimulus sets, procedure, and test types. In all groups, 50% of tests were targets and 50% lures. The color set included red, orange, yellow, green, blue, purple, pink, and brown (shades taken from the standard colors in the MSOffice palette). Stimulus presentation for the feature-only group is not depicted. These participants observed either uni-dimensional gray shapes or colored arcs at study.
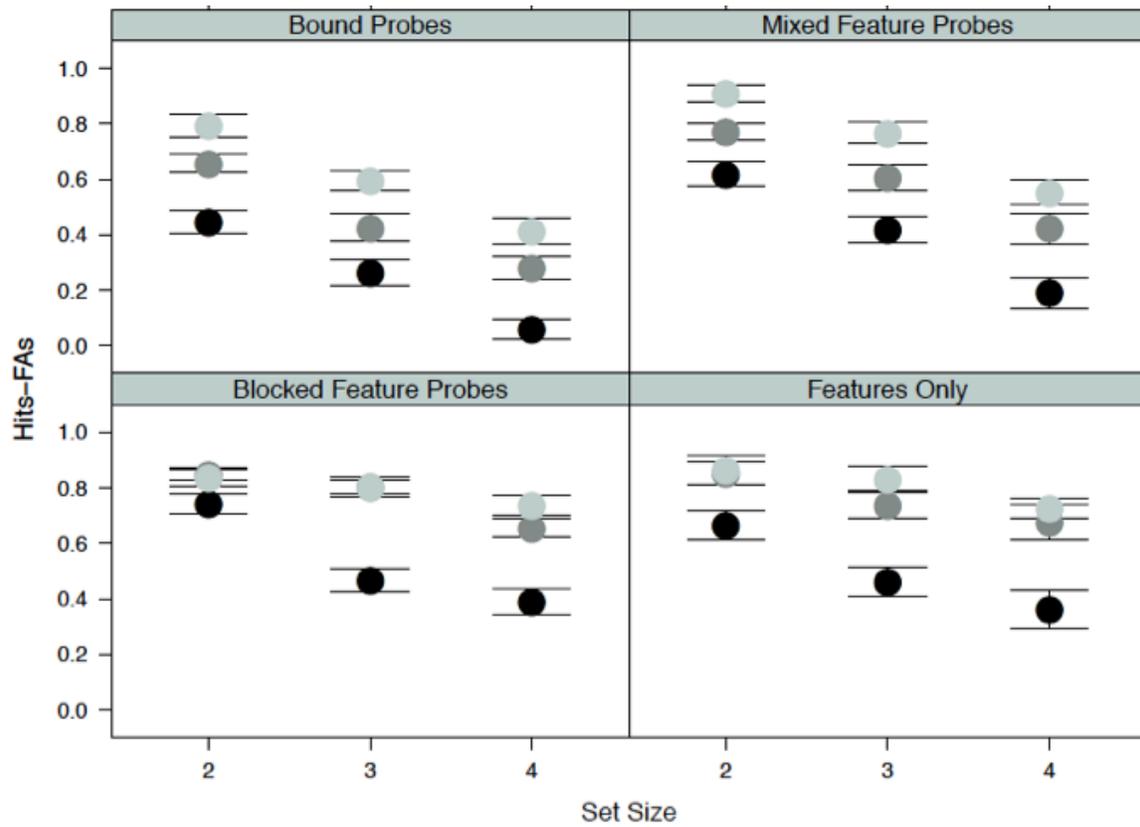
Figure 2. Discrimination accuracy for bound probes and color feature-probes under each of the encoding conditions. Error bars are standard errors of the mean, with the Cosineau-Morey correction applied (R.D. Morey, 2008).
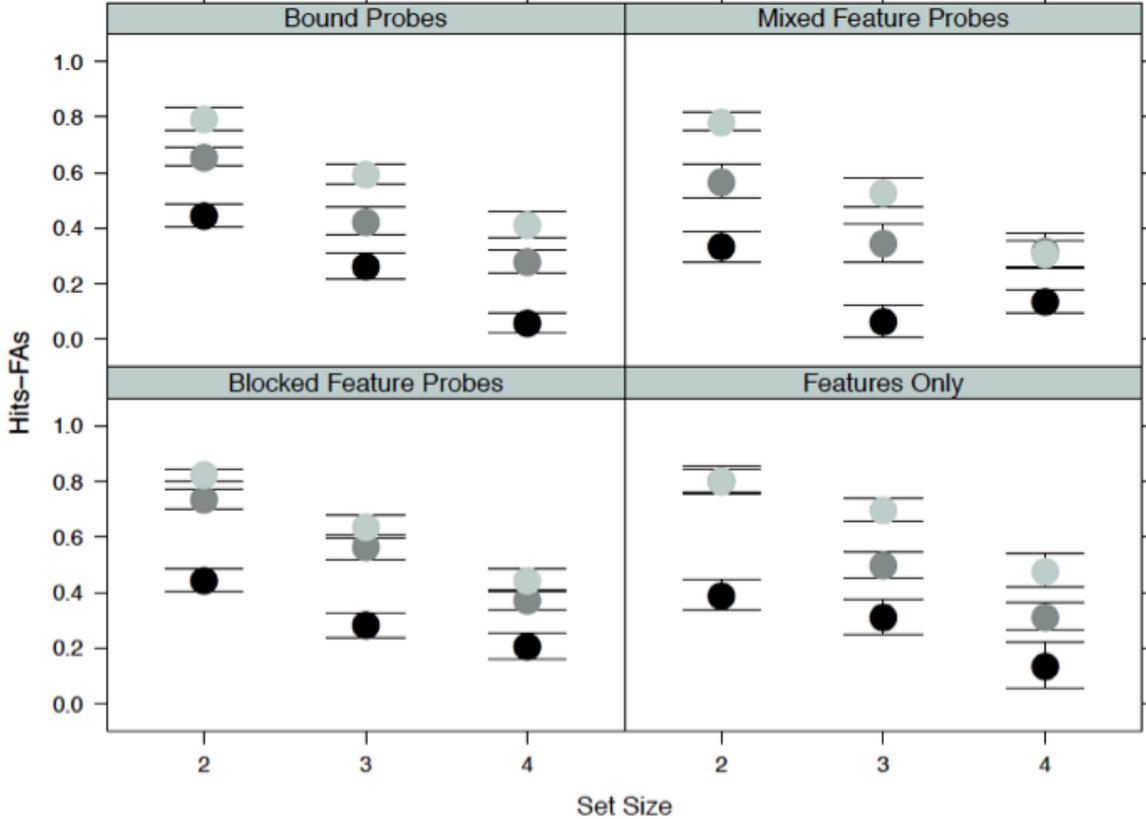
Figure 3. Discrimination accuracy for bound probes and shape feature-probes under each of the encoding conditions. Error bars are standard errors of the mean, with the Cosineau-Morey correction applied (R.D. Morey, 2008).
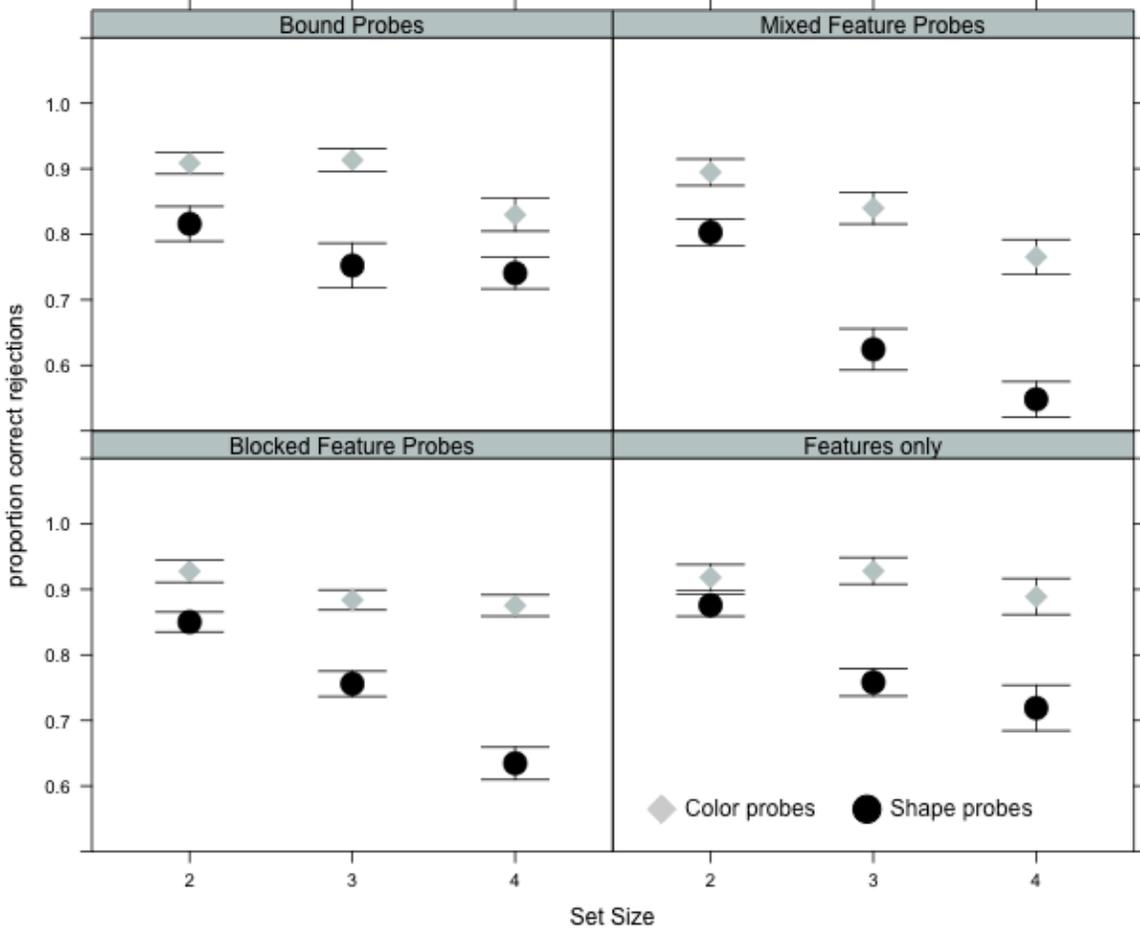
Figure 4. Mean correct rejection of color and shape lures, by task group and set size.

Error bars are standard errors of the mean, with the Cosineau-Morey correction applied

(R.D. Morey, 2008).