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*Published in:*  
Archives of Scientific Psychology

*DOI:*  
[10.1037/arc0000014](https://doi.org/10.1037/arc0000014)

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*Document Version*  
Publisher's PDF, also known as Version of record

*Publication date:*  
2015

[Link to publication in University of Groningen/UMCG research database](#)

*Citation for published version (APA):*

Morey, C. C., Cong, Y., Zheng, Y., Price, M., & Morey, R. D. (2015). The Color-Sharing Bonus: Roles of Perceptual Organization and Attentive Processes in Visual Working Memory. *Archives of Scientific Psychology*, 3(1), 18-29. <https://doi.org/10.1037/arc0000014>

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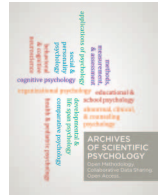
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# The Color-Sharing Bonus: Roles of Perceptual Organization and Attentive Processes in Visual Working Memory

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### ABSTRACT

Color repetitions in a visual scene boost memory for its elements, a phenomenon known as the color-sharing effect. This may occur because improved perceptual organization reduces information load or because the repetitions capture attention. The implications of these explanations differ drastically for both the theoretical meaning of this effect and its potential value for applications in design of visual materials. If repetitions capture attention to the exclusion of other details, then use of repetition in visual displays should be confined to emphasized details, but if repetitions reduce the load of the display, designers can assume that the nonrepeated information is also more likely to be attended and remembered. We manipulated the availability of general attention during a visual memory task by comparing groups of participants engaged in meaningless speech or attention-demanding continuous arithmetic. We also tracked eye movements as an implicit indicator of selective attention. Estimated memory capacity was always higher when color duplicates were tested, and under full attention conditions this bonus spilled over to the unique colors too. Analyses of gazes showed that with full attention, participants tended to glance earlier at duplicate colors during stimulus presentation but looked more at unique colors during the retention interval. This pattern of results suggests that the color-sharing bonus reflects efficient perceptual organization of the display based on the presence of repetitions, and possibly strategic attention allocation when attention is available.

### SCIENTIFIC ABSTRACT

Color repetitions in a visual scene boost working memory capacity for its elements, a phenomenon known as the color-sharing effect. This may occur because improved perceptual organization reduces information load or because the repetitions capture attention. The implications of these explanations differ drastically for both the theoretical meaning of this effect and its potential value for applications in design of visual materials. Previous research suggests that the color-sharing bonus is restricted to tests of the repeated colors themselves, which tends to support the idea that the repetitions capture attention, possibly to the exclusion of the remaining elements. We explicitly manipulated the availability

This article was published April 13, 2015. It was accepted under the editorial term of Harris Cooper and Gary R. VandenBos.

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Components of this project formed part of the bachelor's theses of Yongqi Cong and Yixia Zheng. Thanks are due to Meredith Minear for facilitating data collection at the College of Idaho. Data and analysis scripts are available at Open Science Framework, <https://osf.io/v72dt/>.

For further discussion on this topic, please visit the *Archives of Scientific Psychology* online public forum at <http://arcblog.apa.org>.

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of general attention during a visual change detection task by comparing groups of healthy young adults engaged in articulatory suppression or backward counting. We also tracked eye movements as an implicit indicator of selective attention. Estimated memory capacity was always higher when color duplicates were tested, and under full attention conditions this bonus spilled over to the unique colors too. Analyses of gazes showed that with full attention, participants tended to glance earlier at duplicate colors during stimulus presentation but prioritized looking at unique colors during the retention interval. This pattern of results suggests that the color-sharing bonus occurs due to efficient perceptual organization of the display, which might be enhanced by strategic attention allocation when attention is available.

*Keywords:* visual short-term memory (STM), visual working memory, working memory, divided attention, Gestalt principles

*Supplemental materials:* <http://dx.doi.org/10.1037/arc0000014.supp>

*Data Repository:* <http://doi.org/10.3886/ICPSR35640.v1>

Though vision seems to provide rich sensory input, short-term memory (STM) for visual detail has been shown to be extremely limited. Limits have been demonstrated in a variety of contexts, including estimates of visual STM capacity (e.g., Awh, Barton, & Vogel, 2007; Rouder et al., 2008) as well as in consciousness of changes to the visual environment (Simons & Levin, 1997), suggesting that only a small fraction of the visual detail available is retained moment to moment. Compared with verbal stimuli, where enhancement of memories through mnemonic techniques such as chunking or rehearsal seem to be the default strategy (so much so that uncovering true mnemonic limits requires restricting these strategies; Cowan, 2001), visual memories seem less amenable to comparable techniques. Verbal rehearsal of the details in visual images can impair accuracy (e.g., Brandimonte & Collina, 2008; Schooler & Engstler-Schooler, 1990), and evidence that nonvisual judgments severely impair visual memory suggest that there either is no domain-specific rehearsal process available for visual imagery, or at least that any such resource is of little use for boosting retention (Morey & Bieler, 2013; Stevanovski & Jolicœur, 2011; Vergauwe, Barrouillet, & Camos, 2010). Here, we examine one possible means by which visual memory limitations do seem to be ameliorated, namely perceptual organization, with the aim of better understanding how perceptual regularities boost memory capacity for visual information.

There is broad agreement that principles of visual perceptual organization, such as explicit connections between otherwise distinct items or gestalt principles of grouping, aid visual STM. Xu (2006) showed reduced performance costs for remembering two disparate features attached by a connecting line compared with scenarios in which the same features were shown in disconnected formats. Woodman, Vecera, and Luck (2003) similarly found enhanced performance on a visual change detection task when a color explicitly connected to a previously cued spatial location was probed. These cues were effective even when delivered after the stimulus presentation, indicating that object-based grouping influenced which features were encoded, not merely how the features were perceived. In both studies, the focus on explicit connections between the features emphasizes the idea that multiple, disparate features may be grouped into single-object representations, even if they are spatially separated. If associations between two features such as frequently paired colors are learned, visual memory load is likewise reduced (Brady, Konkle, & Alvarez, 2009). Assuming that a limited number of visual objects can be maintained at once (e.g., Awh et al., 2007; Matsukura & Hollingworth, 2011; Rouder et al., 2008), visual memory load reduction may be achieved through the organization of more features within fewer objects.

However, perceptual organization has also been shown to boost visual memory in contexts that lack obvious object-based grouping cues. Feature similarity, specifically in the form of repeated colors (but also in other feature dimensions, Mate & Baques, 2009), has been shown to improve recognition (Peterson & Berryhill, 2013; Quinlan &

Cohen, 2012). Quinlan and Cohen (2012) manipulated the presence of duplicate colors in displays of to-be-remembered colored shapes. At test, the probe object could contain a duplicate or a unique color. They found higher accuracy for tests of duplicate compared to unique colors, and a null effect for unique colors from a display including duplicates compared to a baseline condition including displays of only unique colors. Peterson and Berryhill (2013) likewise observed a color-sharing bonus on a color-location memory task, showing higher accuracy for tests of duplicate than unique colors. Furthermore, this advantage was clearest when duplicates were presented adjacently during study. These findings show that a relationship between two distinct items can be formed regardless of whether they are depicted with explicit connectors.

Though there is a consensus regarding the idea that perceptual organization eases visual memory, it is not clear how this occurs. Findings of an exclusive advantage for tests of duplicate colors (Peterson & Berryhill, 2013; Quinlan & Cohen, 2012) pose difficulties for both *continuous-resource* models of visual working memory, which suppose that some resource is distributed across the visual scene, and *discrete-slot* models of visual working memory, which alternatively suppose that a fixed number of objects can be maintained, while all information not belonging to the retained objects is forgotten. Under the continuous-resource assumptions (e.g., Bays, Catalao, & Husain, 2009; van den Berg, Shin, Chou, George, & Ma, 2012), a display with fewer feature exemplars contains less overall information, and therefore duplicate colors should have boosted performance for all tests of displays including duplicates. A continuous-resource theory must adapt to explain why performance for the duplicates themselves should be superior to performance for the singletons, making some new assumption about why the duplicates consume more of the limited resource. Similarly, assuming a discrete-slot scenario in which information about a limited number of objects is maintained (e.g., Donkin, Nosofsky, Gold, & Shiffrin, 2013; Rouder et al., 2008), and further that a repeated feature can evoke perceptual grouping processes that enable two objects to be encoded as a unit (Quinlan & Cohen, 2012), one would expect that more “slots” remain available for storing additional unique features when a duplicate is present. Discrete-slot hypotheses likewise fail to predict preference for the duplicates; even if we assume that the duplicates form a single-object group, it does not necessarily follow that this group should be preferentially encoded. Both classes of models thus predict that the color-sharing bonus should spill over to singletons from a display that includes duplicates, but this prediction has so far not been borne out.

Both Quinlan and Cohen (2012) and Peterson and Berryhill (2013) showed a pronounced bonus when one of the repeated-color objects was probed, but little (if any) advantage for the singleton items presented alongside repeated colors. Across both reports, the only evidence possibly pointing to such an advantage came from analyses

estimating the number of groups maintained in Peterson and Berryhill's Experiment 1. This evidence begs for modification of assumptions about visual working memory. One possibility raised by Quinlan and Cohen (based on a hypothesis of Kahneman & Henik, 1977) was that multiple perceptual groups are encoded sequentially, with priority going to the salient repeated items. They tested this by including arrays with two instances of color repetition, and found that performance on tests of repetitions was similarly high when one or two repetitions were present, but that the performance difference between repeated and unrepeated items was reduced in displays with two repetitions compared with only one. This outcome was inconsistent with the sequential encoding hypothesis, meaning that a satisfactory explanation of these phenomena remains elusive.

Nonetheless, if we consider this sequential encoding explanation fully, one would still have expected an advantage of singletons in the repeated-color conditions over singletons in a baseline condition including no repetitions. Though encoding the perceptual groups may be prioritized, more capacity should still remain for encoding the remaining singletons when repetitions are present than when they are absent. This could be reflected in the apparent reduction in the color-sharing bonus observed by Quinlan and Cohen when two color-repetition groups were present. However, this boost to singletons in a repetition display may be rather small because any excess cognitive resource made available due to repetition-based grouping would be distributed across all the remaining singletons in the display, potentially leading to very small increases in accuracy to any particular singleton probe. Greater power or more sensitive measurement techniques might be required to establish this or to accumulate persuasive evidence that singletons indeed do not benefit from the color-sharing bonus. One aim of the present experiments is to more precisely measure the extent of the color-sharing effects, testing whether they extend to other items in a color-repetition display. Further progress in understanding how to modify either discrete-slot or continuous-resource theories to encompass the color-sharing bonus depends on knowing whether singleton items benefit at all from color repetitions.

Another key line of evidence needed to advance understanding of perceptual organization effects on visual memory capacity concerns the degree to which these effects reflect attentional capture. It is argued that organizing features hierarchically into unified objects (e.g., Woodman et al., 2003) or encouraging grouping by similarity or proximity results in improved memory because the corresponding information load is reduced by these structures. However, in the case of gestalt-like perceptual grouping, it is also plausible that the grouping manipulation captures attention, directing it toward a subset of objects to the detriment of others. This explanation fits well with the findings of Peterson and Berryhill (2013) and Quinlan and Cohen (2012)'s Experiment 1 though results of Quinlan and Cohen's second experiment, in which two groups of redundant colors were remembered as well as a single color repetition, casts doubt on it. Although it seems that gestalt-like effects in discrimination appear irrespective of the availability of general attention (e.g., Lamy, Segal, & Ruderman, 2006; Moore & Egeth, 1997), this does not necessarily mean that organization principles are automatically applied to temporary storage of visual images, nor does it exclude the possibility that when attention is available, it is liable to be drawn toward items that form a group to the detriment of the remaining items. A second aim of our experiments was therefore to examine the color-sharing bonus under conditions of full and divided attention, aiming to see whether the bonus remained when attention was impoverished. Adding to previous evidence, we also recorded eye movements during stimulus presentation and retention to enable evaluation of implicit evidence that perceptual groups capture and consume attentional resources.

We conducted two experiments designed to compare the color-sharing bonus in visual memory under full and divided attention, and collected eye movement data as an implicit measure of attention allocation to be analyzed alongside estimated memory capacities. Participants completed a visual change detection task in which they attempted to remember the locations of colored squares. On some trials, the studied array contained two squares sharing the same color along with uniquely colored squares; on other trials, each square's color was unique. In Experiment 1 participants performed visual change detection with concurrent articulatory suppression, which should not draw upon attentional resources and should not substantially reduce visual memory capacity (Luria, Sessa, Gotler, Jolicœur, & Dell'Aqua, 2010; Morey & Cowan, 2004; Sense, C.C. Morey, Heathcote, Prince, & R.D. Morey, 2014). We estimated working memory capacity using a hierarchical Bayesian implementation (R.D. Morey, 2011; R.D. Morey & C.C. Morey, 2011) of the discrete-objects model of Rouder et al. (2008), testing especially for variance in capacities based on whether the study display contained color duplicates or not and whether a duplicate or unique color was probed. All of the inferences we report about capacities come from this hierarchical Bayesian modeling, which gives our analysis important advantages over previous work. Hierarchical modeling makes efficient use of data, providing the sensitivity to detect the anticipated small differences between tests of unique colors presented with or without duplicates. Unlike the analyses previously published on this topic, our Bayesian analysis allows for positive inference about null effects. If the best model excludes a particular factor, that is positive evidence against the existence an effect of that factor in our data, rather than a lack of power or sensitivity that cannot be interpreted (e.g., a null  $p$  value).

In conjunction with analyses of working memory capacity, we calculated the time spent fixating duplicate and unique colors and the speed with which each type of object was fixated during stimulus presentation and retention periods. If the color-sharing bonus arises simply because the repeated colors capture attention, then a tendency to fixate these items earlier during stimulus presentation or longer during either period should appear. During retention, looking toward the previous positions of stimuli is believed to reflect retrieval attempts (Ferreira, Apel, & Henderson, 2008), and has been shown to predict recognition accuracy in similar visual change detection tasks (Mall, Morey, Wolff, & Lehnert, 2014). Comparison of gaze durations and speeds during stimulus presentation and retention in these tasks has the potential to reveal novel information about how perceptual organization boosts encoding and how it influences rehearsal of visual information, and to help generate new hypotheses about both the color-sharing bonus and maintenance processes for visual memoranda. As with our analyses of memory capacity, we carried out Bayesian hypothesis testing so that we are capable of comparing the strength of evidence both for null and alternative hypotheses.

## Experiment 1

### Method

**Participants.** Thirty students from the University of Groningen participated in the study, including eye tracking, in exchange for partial course credit. In addition, 30 students from the College of Idaho in Caldwell, Idaho participated in the study in exchange for course credit; these participants' gazes were not tracked. All participants completed and passed a color-blindness screening (Ishihara, 1966) prior to participation. Three participants were excluded from all analyses because of incomplete data; for two of these, the problem

was a software malfunction, and one experienced a headache. Therefore, the final sample for behavioral analyses consisted of 57 participants (38 females, 19 males), ranging from 19 to 25 years of age ( $M = 20.86, SD = 1.29$ ). For eye movement analyses, one additional participant was excluded due to a malfunction that resulted in incomplete sample acquisition, leaving a final sample of 26 participants in the gaze analyses.

**Apparatus and stimuli.** The stimuli consisted of colored squares with black outlines. There were nine possible colors (see Figure 1). The size of each square was  $0.53 \times 0.53$  cm, subtending  $0.45^\circ \times 0.45^\circ$  degrees of visual angle. Stimuli were presented at random locations around the center of the screen. All the possible square locations fell within an area of  $6.10^\circ \times 4.55^\circ$  around the center with the constraint that no square could be located within  $1.24^\circ$  of another square or of the center.

Stimuli were presented on a 17-inch CRT monitor. Participants were seated approximately 67 cm in front of the computer. The experiment was controlled by a PC running E-Prime (Schneider, Eschmann, & Zuccolotto, 2002). The participants in Groningen used a Microsoft Sidewinder gamepad to enter responses. Two buttons on the gamepad were marked with *same* and *change* for the respective responses. Participants in Caldwell used the *S* and *D* keys on the standard keyboard to enter same or different responses.

For a subset of participants, eye-movements were recorded throughout the experiment at a rate of 1000 Hz using an EyeLink 1000 eye-tracking system with  $0.01^\circ$  resolution. These participants were situated with their chin placed on a chinrest tower, located 50 cm from the camera. Interest areas were defined as the 1.06 cm square region centered on each presented square and the 1.06 cm square region situated at the center of the screen. Fixations were only assigned to interest areas when they fell within these predefined boundaries. With

these distances between the participant and the screen and between the presented squares, participants could see all the squares at once if they looked near the center of the screen.

We used a change detection task similar to Quinlan and Cohen’s (2012). Participants briefly studied an array of stimuli and then after a short retention interval were shown a test screen with a single item encircled. Participants were to indicate whether the encircled item was the same or had changed from the study array. Study arrays included one, three, five, or seven items. Arrays with three or more items could include one repeated color or all unique colors. For trials including a color duplicate, the probed item at test was either one of the duplicate color items, or one of the uniquely colored items, with equal probability. Fifty percent of these tests showed a color square that was the same as in the study array, and 50% showed a change.

Two types of test array were included in separate, randomly ordered blocks of trials, whole-array and single-item tests, of which only the single-item tests were considered in these analyses. For these different kinds of tests, the assumptions used to calculate capacity ( $k$ ) would necessarily differ based on the different information available to inform decisions in each test scenario. For the hypotheses considered here, single-item tests are ideal; with single-item tests, no color information about the untested items from the study array is available to introduce further biases at test. With whole-array tests including a repeated color, a change to a duplicate color item could not occur without either changing the whole array such that it no longer included a color repetition, or changing it instead so that it included a new color repetition. The whole-array tests were included to enable a separate comparison of various types of  $k$  models, but these analyses (which will not detail the color-sharing bonus effects) will be reported elsewhere.

All conditions were manipulated within participants. For all factors except set size, experimental trials were always divided equally. For set size however, we included fewer trials for Set Size 1 than for Set Sizes 3, 5, and 7 (16 trials per participant for Set Size 1 and 64 trials for each of the other types) because repetition manipulations were naturally impossible with only one item. The main purpose of including one-item trials was to evaluate whether participants were performing with due attention.

**Procedure.** Participants from the University of Groningen, whose eye movements were monitored, were each tested individually. Participants from the College of Idaho were tested in groups of up to five, without eye tracking. All participants indicated written consent to take part. The experiment started with a 6-item Ishihara test (1966) in order to detect color vision deficiencies. After passing the color-blindness test, participants read the instructions for the experiment, which explained what the stimuli would be like and how to enter responses. Participants were required to explain these instructions in their own words to the experimenter, and then to complete a supervised 12-trial practice session. For the relevant sample, after making sure the participants understood the task, the experimenter adjusted and calibrated the eye tracker to monitor the participants’ right eyes. Before the experimental trials started, another instruction screen was presented. The participants were instructed to verbally repeat the words *two* and *ten* quietly at a pace of two words per second throughout all experimental trials. This was to ensure that they did not verbally rehearse the names of the colors.

Each experimental trial started with a black fixation cross in the center of the screen, which remained for 4,000 ms. The study array was then presented for 1,200 ms, followed by a 3,000-ms retention period, during which the colors of the squares disappeared, but the outlines remained on the screen. The presence of placeholders allows inference about rehearsal; it has been previously shown that partici-

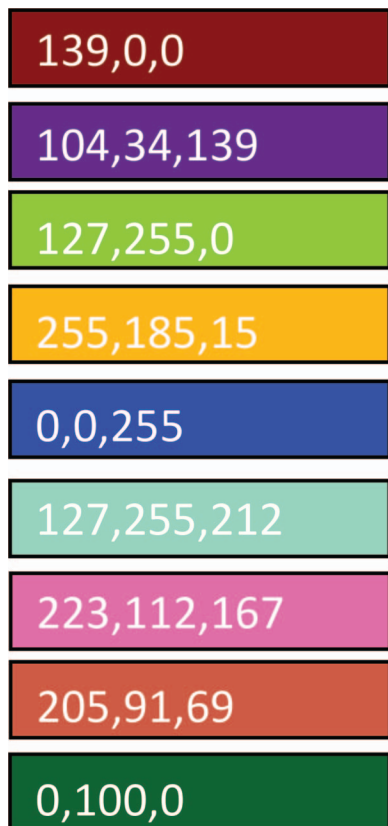


Figure 1. Potential stimulus colors.

participants direct gazes to the locations of previously studied stimuli if there are placeholders to look at (e.g., Tremblay, Saint-Aubin, & Jalbert, 2006). Including a long retention interval also prevents sensory memory (Sperling, 1960) from contributing to performance. The test screen then followed, where the single-item probe was circled for emphasis. The participants then indicated whether the probed item was of the same color or a different color compared to the study array. The test screen remained visible until a response was entered. See Figure 2 for an illustration of the trial procedure.

Every participant contributed 208 trials toward this analysis across two blocks (plus two blocks of whole-array trials that were excluded from these analyses). Each block contained trials from all combinations of conditions, the order of which was randomized. Participants were allowed to take breaks between blocks and initiate the next block when they were ready. The eye-tracker was freshly calibrated at the beginning of every block. The experimental session lasted approximately 90 minutes.

## Results

**Capacity estimates.** Mean proportions correct, separately for same and change trials, are provided in Table 1. We estimated memory capacity using WoMMBAT (R. Morey & C. Morey, 2011), which produces hierarchical Bayesian estimates of capacity (R. Morey, 2011) based on the model proposed by Rouder et al. (2008). These methods are ideally suited to our design and hypotheses because they provide optimal estimates of  $k$ , avoiding nuisances that arise with simpler models (such as uninterpretable negative capacity estimates) and because WoMMBAT compares user-specified models using Deviance Information Criterion (DIC; Spiegelhalter, Best, Carlin, & van der Linde, 2002), allowing principled inference not only about which factors influence parameters of interest, but furthermore, which factors do not.

Three parameters are modeled in WoMMBAT:  $k$ , which estimates the number of items maintained,  $z$ , which indexes the likelihood of avoiding attentional lapses, and  $g$ , which estimates guessing bias (Morey, 2011; Rouder et al., 2008). The  $z$  and  $g$  parameters are nuisance parameters, necessary to include but not of central interest (similar to the criterion in signal detection models where the sensitivity parameter  $d'$  is of interest). We were primarily interested in the  $k$  parameter, especially whether  $k$  would increase for displays including duplicate versus only unique colors, or specifically for the duplicate color probes, replicating Quinlan and Cohen (2012). In our modeling, we allowed  $z$  to vary between participants but not between factors; because the order of presentation of the levels of our independent variables was randomized and because the probe status variable was only known at the probe stage of the trial, assuming variance in attentional lapsing as a function of these experimental conditions

Table 1  
Means (and Standard Deviations), Experiment 1

	Set Size			
	1	3	5	7
Same trials				
All Singletons-singleton	0.97 (.08)	0.82 (.14)	0.63 (.20)	0.51 (.18)
Repetition-singleton	NA	0.84 (.15)	0.66 (.21)	0.46 (.22)
Repetition-duplicate	NA	0.91 (.11)	0.80 (.19)	0.72 (.17)
Change trials				
All Singletons-singleton	0.98 (.06)	0.94 (.11)	0.87 (.12)	0.81 (.14)
Repetition-singleton	NA	0.96 (.06)	0.93 (.09)	0.83 (.17)
Repetition-duplicate	NA	0.93 (.12)	0.85 (.14)	0.85 (.14)

Note.  $N = 57$ .

did not make sense. Although we were not theoretically interested in estimates of  $g$ , we conducted exploratory analyses to examine model fits with various combinations of the two independent variables on  $g$ , because both the presence of color repetitions and the status of the probed item (i.e., whether it was a duplicate or unique color) could plausibly affect guessing bias, and we would ideally want to examine  $k$  estimates that optimally controlled performance fluctuations due to guessing bias. These analyses indicated that main effects of color repetition at presentation and status of the probed item should be assumed for  $g$ . We therefore carried out modeling of  $k$  with these main effects as factors on  $g$ , and additionally between-participants variance as a factor on  $k$ ,  $z$ , and  $g$ .

We were interested in comparing  $k$  estimates for tests of duplicate colors, tests of unique colors (i.e., singletons) from a display including duplicates, and tests of unique colors from a display composed of only unique colors. We therefore compared models coding these three trial types separately (i.e., one 3-level color-repetition factor) with those coding for only the presence of repetitions at study (two levels, singletons-only or repetition present) or alternatively coding only for the status of the tested item (two levels, singleton or repetition tested). Model fit statistics are given in Table 2. For each model, 100,000 Markov Chain Monte Carlo samples converged onto these estimates, which are shown in Figure 3. The best-fitting model according to the DIC differentiates three types of trials. In this method, we can also obtain comparisons between specific effects by computing the posterior odds that a difference is positive versus negative. Posterior odds were  $>100,000:1$  that duplicate-test  $k$ s were higher, rather than lower, than  $k$ s in both other conditions, and posterior odds that the tests of singletons presented with a duplicate were greater than tests of singletons in the all-singleton condition were  $>760:1$ . This is very strong evidence favoring these differences, even the small advantage for singletons from a display containing a color repetition.

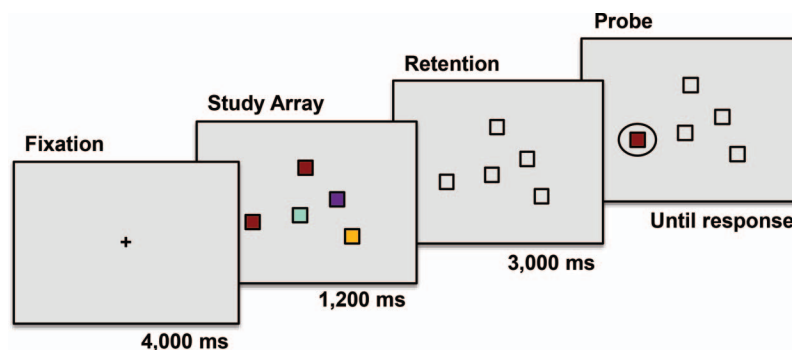


Figure 2. Trial procedure. In this example, a repeated color is tested. The correct response is *same*.

Table 2  
Hierarchical Bayesian Model Fit Statistics, Experiment 1

Model	Number parameters on $k$	DIC
<b>Three levels</b>	<b>60</b>	<b>9710.0</b>
Two levels: Repetition or not at study	59	9734.3
Two levels: Duplicate or singleton probed	59	9712.6
Participant variance only	57	9774.4

Note. The best-fitting model with the lowest DIC is indicated in bold text. Each model was estimated with 100,000 MCMC iterations, with between-participant variance on the  $k$  parameter in addition to the other factors being compared across models of  $k$ . All models included between-participant variance on the  $z$  parameter and between-participant variance plus main effects of color repetition condition and probe status on the  $g$  parameter.  $N = 57$ .

We also checked whether the distance between the repeated colors, which was allowed to vary randomly, had any impact on accuracy. For trials with repetitions, we subjected these distances to a Bayes Factor analysis of variance (ANOVA) calculated with R-package BayesFactor (version 0.9.7, Morey & Rouder, 2014; Rouder, Morey, Speckman, & Province, 2012) with response accuracy (correct, error) and probe status (duplicate, singleton) as potential factors along with between-participants variance. Rather than  $F$  and  $p$  values, output statistics are Bayes factors favoring each possible model (i.e., a model including both independent variables and their interaction, simpler models including only subsets of these factors) over a model where the only differences in accuracy are due to participants' differing levels of performance. Bayes factors can be directly interpreted as the evidence from the data favoring one model over another. Specifically, the evidence is defined as the change induced by the data in the odds favoring one model over another. The model with the highest Bayes factor is the model for which the most evidence has been obtained, and any two models can be compared to make inferences about effects (or absences of effects) of particular factors. All of the possible models in this design favored the null hypothesis. The model least favoring the null included only a main effect of response accuracy, with a Bayes Factor (BF) favoring the null of more than 9. This indicates that within the range of space our stimuli occupied (where duplicates were separated by at least 1.66 and at most 8.86 degrees), proximity of the color repetitions did not affect performance.

**Gaze analysis.** Our accuracy analysis replicated Quinlan and Cohen's (2012) finding of a color-sharing bonus. However, we ob-

served this both for tests of the duplicates themselves and also tests of unique colors within a display containing a repetition. We turned to gaze data to attempt to learn more about the underlying causes of the color-sharing bonus. First, we tested whether directly fixating a specific item resulted in more accurate performance if that item was subsequently tested. For fixations during the stimulus presentation, fixating the eventual probe led to higher accuracy ( $M = 0.84$ ,  $SD = 0.21$ ) compared with not directly fixating it ( $M = 0.77$ ,  $SD = 0.14$ ); the same was true for fixations during retention ( $M_{Fixed} = 0.83$ ,  $SD = 0.17$ ;  $M_{Unfixed} = 0.77$ ,  $SD = 0.14$ ). Bayesian  $t$  tests using BayesFactor (Morey & Rouder, 2014; Rouder, Speckman, Sun, Morey, & Iverson, 2009) favored the alternative hypothesis by a factor of 16 for the stimulus period, and 30 for the retention interval. These effects enable a meaningful evaluation of whether preferential looking toward duplicates can account for the clear color repetition advantage we observed in capacity estimates.

Descriptive statistics showing the mean trial dwell time and counts of fixations directed toward duplicates, singletons within a display including duplicates, singletons in all-singleton displays, and the center of the screen are given in Table 3; these are provided in order to show that we acquired adequate amounts of data across the critical conditions we intended to compare. Bayes factor ANOVAs testing for effects of presence of repetitions and set size on number of fixations recorded to items or to the center yielded strong evidence for an effect of presence of repetitions ( $BF$  of at least 200:1 during the stimulus period compared to models excluding this effect, and 8:1 during the retention interval), with repetitions decreasing the number of recorded fixations on average. The results of the same analyses performed on the duration of looking toward items or the center was equivocal. These results are consistent with the idea that repetitions allow for more efficient encoding of a display.

We next tested whether participants differentially looked at duplicates compared to singletons in arrays containing a repeated color. To test whether participants looked more at the duplicates, we calculated the relative proportion of time spent fixating a duplicate compared with total time spent fixating any item directly. We compared these proportions to those one would expect if gazes fell randomly on items in the display (i.e.,  $2/\text{set size}$ ). For the stimulus presentation period,

Table 3  
Fixation Counts and Gaze Durations Toward Center, Singletons, and Color Duplicates, Experiment 1

	Mean fixation count	Mean duration fixated
Stimulus Presentation		
All Singletons		
Center	0.69 (0.26)	196 (83)
Singleton	1.03 (0.52)	248 (121)
Repetition		
Center	0.69 (0.27)	202 (89)
Singleton	0.53 (0.35)	126 (78)
Duplicate	0.39 (0.23)	98 (60)
Retention interval		
All Singletons		
Center	0.30 (0.21)	115 (101)
Singleton	2.09 (0.90)	855 (434)
Repetition		
Center	0.33 (0.21)	132 (120)
Singleton	0.74 (0.47)	482 (304)
Duplicate	1.19 (0.69)	313 (244)

Note.  $N = 26$ . Fixations were trimmed by interest period (stimulus presentation and retention interval). The stimulus presentation interest period included 100 ms prior to the stimulus onset, in order to more frequently avoid trimming the initial central fixation. Standard deviations in parentheses.

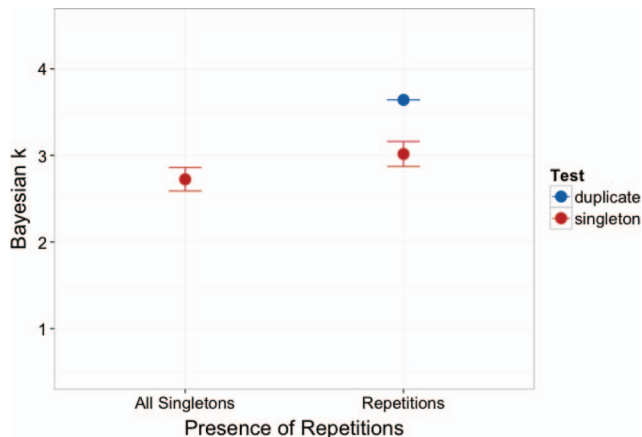


Figure 3. Hierarchical Bayesian  $k$  estimates. Error bars reflect the posterior standard deviation on the difference between tests of singletons in each repetition condition and tests of duplicate colors.  $N = 57$ .

Bayes factor  $t$  tests were carried out separately for each set size. At Set Size 5, the Bayes factor favored the null hypothesis that participants were as likely to look at duplicates as singletons ( $M_{ss5} = 0.40$ ,  $SD = 0.12$ ;  $BF = 4.76$ ). At Set Sizes 3 and 7, evidence suggested that participants fixated duplicates at a rate different from the rate expected ( $BF_{ss3} = 6.95$ , and  $BF_{ss7} = 3.45$ , respectively); however, the respective effects were in opposite directions. For Set Size 3, the mean rate of fixating duplicates was less than expected ( $M_{ss3} = 0.57$ ,  $SD = 0.16$ ), while at Set Size 7, participants gazed more often than expected at duplicates ( $M_{ss7} = 0.35$ ,  $SD = 0.13$ ). These mixed outcomes do not provide strong evidence of disproportionate looking toward color repetitions compared with singletons during presentation. However, during the retention period a clearer pattern of preferential looking emerged, but toward the singletons rather than the duplicates. At Set Sizes 3 and 5, Bayes factors favoring the alternative hypothesis were 194:1 and 18:1, respectively, with mean values indicating *less* looking toward duplicates than would be expected by chance ( $M_{ss3} = 0.57$ ,  $SD = 0.10$ ;  $M_{ss5} = 0.33$ ,  $SD = 0.10$ ). For Set Size 7, the null hypothesis of no difference between observed and expected values was favored approximately 5:1. All together, these analyses yielded no support for the proposition that duplicates were more frequently attended than singletons. Instead, they provided substantial evidence that singletons were more likely to be fixated during retention than duplicates.

It could also be the case that duplicates draw attention earlier than singletons. We tested this by analyzing the speeds with which participants first fixated the center of screen, a duplicate, or a singleton in presentations that included color repetitions. We ran a Bayes factor ANOVA on log-transformed times to first fixation, testing for effects of interest area type (singleton, duplicate, or center; we fitted models coding all three levels separately and models coding only for two levels, center or noncenter), and set size (3, 5, 7). The evidence in favor of the model including three unique levels of interest area was decisive against the model with two levels ( $BF_{\text{Difference}} > 1$  million), with both models decisively outperforming the null hypothesis ( $BFs > 10^{400}$ ). These results indicate that the differences between interest areas are not only due to the strong tendency to fixate the center first (or exclusively). While the earliest fixations were likely to land on the center of the screen ( $M = 671$ ,  $SD = 119$ ), initial fixations toward duplicates ( $M = 1,144$ ,  $SD = 85$ ) were typically earlier than those toward singletons ( $M = 1,233$ ,  $SD = 62$ ). These data, depicted in Figure 4, are consistent with the claim that duplicate colors are more likely to attract attention early than unique colors.

We conducted the same analysis during the retention period; here, the best model included the 3-level interest area factor and set size ( $BF > 10^{13}$ ), outperforming the model with the 2-level interest area factor and set size ( $BF > 140$ ) by well over 1 million. However, during retention a different pattern emerged from that observed during stimulus presentation: previous locations of singleton colors were more likely to be sought earliest during the retention interval ( $M_{\text{Singleton}} = 1453$ ,  $SD = 236$ ;  $M_{\text{Duplicate}} = 1677$ ,  $SD = 230$ ). Thus, though participants did not spend the most time looking at duplicates, duplicates caught their gazes earlier during stimulus presentation, and they subsequently seemed to prioritize looking toward singletons, perhaps covertly rehearsing them, during the retention period.

## Discussion

Our analyses logically replicated those of Quinlan and Cohen (2012), showing higher working memory capacities when duplicates were tested than when singletons are tested. Additionally, our hierarchical Bayesian analyses detected a numerically small advantage for singletons from arrays that included a repetition compared to single-

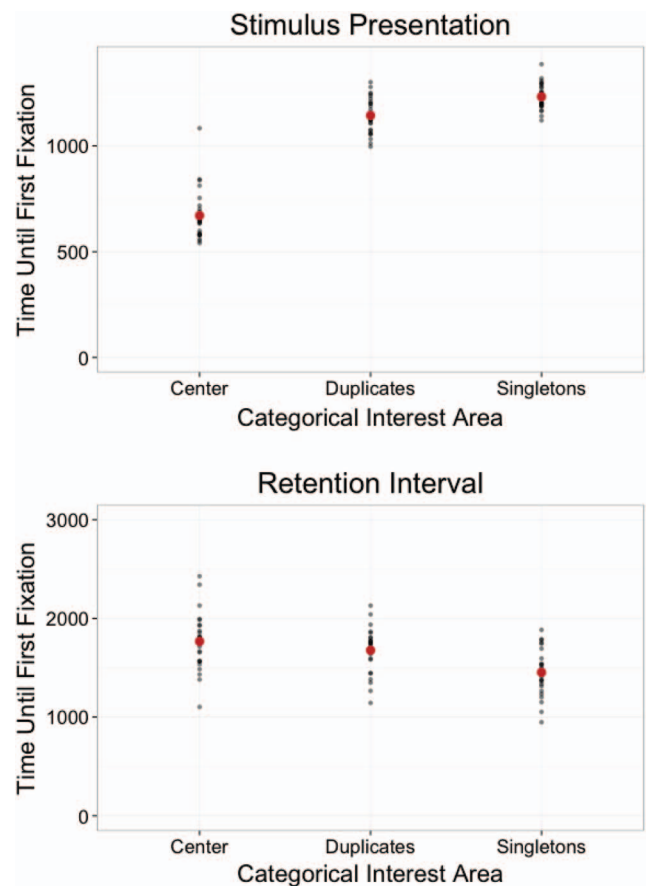


Figure 4. Distributions of average earliest fixation (by participant and set size) for the central, singleton, and duplicate interest areas in trials with repetitions, for the stimulus presentation (upper) and retention periods (lower). Mean values are shown in red; inferences were performed on log-transformed values. The stimulus presentation period was defined as 100 ms prior to the study array onset in order to avoid truncating central fixations that began prior to the appearance of the stimulus display.  $N = 26$ .

tons from an all-singleton array, suggesting that capacity saved by encoding a color repetition does indeed spill over to the remaining objects.

Eye movement data suggest explanations for these findings. Importantly, duplicates were not fixated longer or more frequently than singletons, ruling out the hypothesis that duplicates are better remembered because participants selectively encode them. Although the duplicates were not fixated longer than the singletons, the duplicates were fixated slightly earlier during stimulus presentation, and seemed to reduce the number of fixations to all items of interest. During retention however, prior locations of singletons tended to be fixated earliest. This pattern suggests that color repetitions may capture attention initially and that when a repeated color is present, participants focus their efforts during retention first on recalling the singletons. Though the Bayes factors presented with this earliest fixation analysis might seem decisive, caution is warranted because on a nontrivial subset of trials, participants did not fixate the center or any object. Nonetheless, this analysis included an average of 114–120 observations per participant during retention and stimulus presentation respectively, with 5–90 observations per participant for each category of interest area. This sort of analysis is naturally dependent on participant variability in behavior; we did not instruct participants to selectively look at any class of items.



Our results also suggest that repetition-based grouping of visual imagery likely differs from repetition-based grouping of verbal items in lists, where adjacent or near-adjacent repetitions facilitate accurate recall, but distant repetitions inhibit it (Crowder, 1968). While Peterson and Berryhill (2013) found a color-sharing bonus only when color repetitions occurred in adjacent objects, accuracy in their nonadjacent conditions was not lower than baseline accuracy, providing no evidence of distal inhibition. In our design, we observed no variation in the color-sharing effect based on distance, suggesting that the color-sharing bonus can occur regardless of proximity within the display parameters we used.

We next tested whether this behavior could be due to strategic attentive processes by examining performance on the same task in a sample of participants who carried out an attention-demanding backward counting task instead of articulatory suppression. Backward counting is a commonly used as a secondary task meant to occupy working memory generally during visual memory tasks (e.g., Allen, Baddeley, & Hitch, 2006; Allen, Hitch, Mate, & Baddeley, 2012) because it does not require the presentation of visual information that could interfere with memory for visual imagery due to visual similarity. Furthermore, backward counting provides an elegant contrast to articulatory suppression, because in both tasks we can require that participants speak similar verbal materials aloud, but in backward counting, determining each successive utterance requires the performance of some mental operation. This manipulation allows for inference about whether the color-sharing bonus depends on the availability of general attention, which is believed to be more occupied during backward counting than articulatory suppression (e.g., Allen et al., 2006).

## Experiment 2

### Method

**Participants.** Twenty-one individuals recruited from student population of the University of Groningen participated in this study in exchange for either a €14 honorarium or partial course credit. Two participants were excluded from all analyses because they performed worse than our a priori criterion (85% correct) in the 1-item conditions of the visual change detection task. One additional participant was excluded for failing to count backward by threes as instructed (average decrement per trial was less than 1). The sample analyzed thus consisted of 18 participants (11 female, 7 male). Participants ranged in age from 19 to 26 ( $M = 22.06$ ,  $SD = 1.92$ ).

**Stimuli and apparatus.** The only difference between Experiments 1 and 2 was in the secondary task. Instead of articulatory suppression, participants in Experiment 2 counted backward by intervals of 3. Participants counted aloud, so that as with the articulatory suppression task in Experiment 1, they completed the visual memory task while saying numbers aloud. The initial number was selected randomly from digits 133–199, and presented at the beginning of the trial positioned above a centrally located fixation “+”. The visual stimuli were chosen in an identical manner to those used in Experiment 1.

**Procedure.** Participants were each tested individually. After completing a short color-blindness test, participants read instructions for the task, including how to enter responses on the gamepad. They were required to explain these instructions in their own words to the experimenter, who corrected their understanding if necessary. Participants then completed 12 supervised practice trials to confirm that they understood the task.

The experimenter then calibrated the eye tracker to the participants’ right eyes. Then participants were instructed to verbally count back-

ward in intervals of three from the number presented at the beginning of the trial. For instance, if the number 133 appeared before the trial, the participant would say “133, 130, 127 . . .” until they were prompted to indicate a response. They were presented with an example prior to beginning to ensure they understood what to do.

Similarly to Experiment 1, each experimental trial started with a black fixation cross in the center of the screen, plus a random three-digit number, which remained onscreen for 4,000 ms. The stimulus, retention, and test screens followed as described in Experiment 1. After entering a *same* or *change* response on the gamepad, participants were prompted to type the last number they said aloud during the counting task. This screen remained visible until the participant entered a response. Participants completed 208 trials each, divided into two blocks, with breaks administered as described in Experiment 1. An experimental session lasted approximately 90 minutes (due to the administration of two additional blocks of whole-array visual change detection trials, which are not included in these analyses).

### Results

**Capacity estimates.** Working memory capacity estimates were modeled as described in Experiment 1, with between-participants variance entered as a factor on both the  $z$  and  $g$  parameters and both repetition condition and probe status entered as factors on the  $g$  parameter. Descriptive proportions correct are given in Table 4 and model fit statistics on  $k$  are given in Table 5. With backward counting replacing articulatory suppression, the best-fitting model included only a 2-level main effect of probe status. The posterior odds that tests of duplicate colors yielded higher  $k$  values than tests of singletons in either the repetition or the all-singleton condition were more than 14,000:1. Thus a simpler 2-level model differentiating between only between tests of duplicates and singletons outperformed the 3-level model differentiating between tests of singletons from all-singleton or study arrays with repetitions. This means that there is no evidence for a difference in memory for singletons in the all-singleton and repetition conditions in these data.

**Gaze analysis.** Table 6 shows the average counts and durations during all-singleton and repetition trials, to show that similar amounts of gaze data were acquired in both kinds of trials. Here, we observed no evidence at all of any effect of presence of repetitions on the number of fixations recorded toward areas of interest (all  $BFs$  favor the null hypothesis). Notably, the values obtained in Experiment 2 (see Table 6) are also comparable to those obtained in Experiment 1 (see Table 3), suggesting that differences between gazes across experiments were not due merely to a decreased tendency to look at the monitor while counting in Experiment 2.

Table 4  
Means (and Standard Deviations), Experiment 2

	Set Size			
	1	3	5	7
Same trials				
All Singletons-singleton	0.93 (.12)	0.65 (.19)	0.54 (.16)	0.48 (.17)
Repetition-singleton	NA	0.69 (.24)	0.49 (.13)	0.40 (.16)
Repetition-duplicate	NA	0.89 (.13)	0.65 (.18)	0.62 (.24)
Change trials				
All Singletons-singleton	0.95 (.08)	0.84 (.14)	0.80 (.15)	0.73 (.21)
Repetition-singleton	NA	0.88 (.17)	0.80 (.16)	0.78 (.23)
Repetition-duplicate	NA	0.80 (.17)	0.69 (.25)	0.69 (.24)

Note.  $N = 18$ .

Table 5  
Hierarchical Bayesian Model Fit Statistics, Experiment 2

Model	Number parameters on $k$	DIC
Three levels	21	3938.2
Two levels: Repetition or not at study	20	3935.0
<b>Two levels: Duplicate or singleton probed</b>	<b>20</b>	<b>3926.4</b>
Participant variance only	18	3939.7

Note. The best-fitting model with the lowest DIC is indicated in bold text. Each model was estimated with 100,000 MCMC iterations, with between-participant variance on the  $k$  parameter in addition to the other factors being compared across models of  $k$ . All models included between-participant variance on the  $z$  parameter and between-participant variance plus main effects of color repetition condition and probe status on the  $g$  parameter.  $N = 18$ .

We tested whether directly fixating a specific item resulted in more accurate performance if that item was subsequently tested. Here we did not observe any advantage for fixating the eventual probe during the stimulus presentation or retention period. Bayesian  $t$  tests favored the null hypothesis by a factor of about 4 during stimulus presentation ( $M_{\text{fixated}} = 0.71$ ,  $M_{\text{unfixated}} = 0.68$ ) and about 6 during the retention interval ( $M_{\text{fixated}} = 0.69$ ,  $M_{\text{unfixated}} = 0.68$ ), suggesting that looking behavior under divided attention may be less diagnostic than under full attention. In light of this finding, we present only the proportional fixation duration analyses that we planned a priori.

As in Experiment 1, there was no reason to believe that participants were more likely to look at the duplicates than expected by chance. Bayesian  $t$  tests carried out separately per set size comparing the average time per trial that the repeated items were fixated during stimulus presentation yielded Bayes factors in favor of the null hypothesis (no different from chance, i.e., 2/set size) of 3.15–3.57. During the retention interval, evidence also slightly favored the null hypothesis, with BFs ranging from 1.23–2.70. While none of these values provide strong evidence for either hypothesis, there is no evidence to suggest that repetitions were selectively attended.

## Combined Analyses, Experiments 1 and 2

**Comparison of  $k$  estimates.** To confirm that the selection of a simpler model in Experiment 2 was not simply due to the lower

Table 6  
Fixation Counts and Gaze Durations Toward Center, Singletons, and Color Duplicates, Experiment 2

	Mean fixation count	Mean duration fixated
Stimulus presentation		
All Singletons		
Center	0.81 (0.26)	340 (130)
Singleton	0.66 (0.33)	205 (98)
Repetition		
Center	0.80 (0.25)	337 (119)
Singleton	0.38 (0.22)	113 (66)
Duplicate	0.29 (0.17)	92 (58)
Retention interval		
All Singletons		
Center	0.31 (0.18)	183 (168)
Singleton	1.64 (0.73)	860 (374)
Repetition		
Center	0.30 (0.16)	174 (174)
Singleton	0.88 (0.48)	494 (279)
Duplicate	0.68 (0.42)	373 (231)

Note.  $N = 18$ . Fixations were trimmed by interest period (stimulus presentation and retention interval). The stimulus presentation interest period included 100 ms prior to the stimulus onset, in order to frequently avoid trimming the initial central fixation. Standard deviations in parentheses.

sample size, we assessed the strength of evidence for differences in outcomes during backward counting in Experiment 2 and articulatory suppression in Experiment 1 through an analysis including both experiments. We compared models including the 3-level trial type variable with models including only the 2-level probe status variable (i.e., the best model emerging in Experiment 2), now including possible interactions between these variables and experiment in order to evaluate the evidence that different models were needed to account for the data in Experiments 1 and 2. Models including interactions with experiment were superior to models without experiment as factor, confirming that the evidence for explaining the data from the separate experiments using different models was sufficiently strong. The model with an interaction between experiment group and the 3-level factor (DIC = 13,634.4) was superior to the comparable model including an interaction between experiment group and the 2-level factor coding only for duplicate or singleton probe status (DIC = 13,635.6). Comparison of the  $k$  estimates in Figures 3 and 5 indicates that backward counting substantially impaired visual memory capacity, although the advantage for tests of duplicates remained intact under divided attention.

**Comparison of proportional looking toward center versus items.** Comparison of Tables 3 and 6 suggests that while counting backward in Experiment 2, a larger proportion of gaze time was oriented toward the center. These displays were designed so that all objects can be viewed when fixating the center of the screen; therefore, an increased tendency to fixate the center could indicate reduced voluntary searching under divided attention. To test whether central fixating differed across experiments, we ran a Bayes factor ANOVA on the proportion of recorded fixations during stimulus presentation (when central fixations were mostly likely) with the interest area (center or an object) and presentation condition (repetition or all singletons) as within-participants factors and experiment as a between-participants factor. The winning model included main effects of experiment, interest area, and an interaction between experiment and interest area ( $\text{BF} > 10^{95}$ ). This model outperformed the model without the interaction by a factor of more than 1 million. With articulatory suppression in Experiment 1, somewhat more fixations were recorded toward objects ( $M = 0.22$ ,  $SD = 0.22$ ) than toward the center ( $M = 0.17$ ,  $SD = 0.18$ ). With backward counting however, more fixations were recorded toward the center ( $M = 0.28$ ,  $SD = 0.25$ ) than toward objects ( $M = 0.19$ ,  $SD = 0.22$ ). This outcome is consistent with the suggestion that strategic looking was reduced in

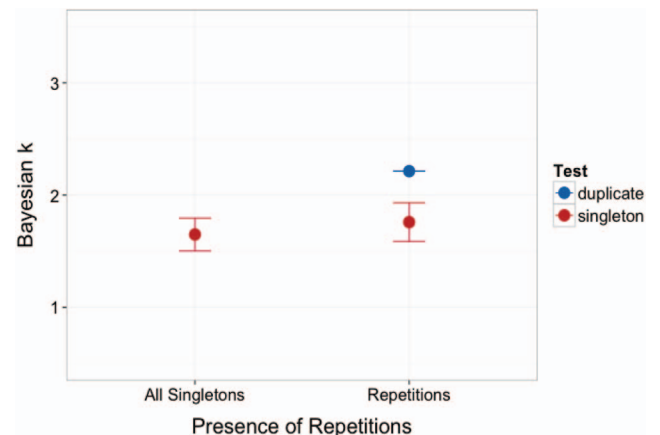


Figure 5. Hierarchical Bayesian  $k$  estimates. Error bars reflect the posterior standard deviation on the difference between tests of singletons in each repetition condition and tests of duplicate colors.  $N = 18$ .

Experiment 2 compared with Experiment 1, despite accumulating a similar amount of valid gaze data.

## Discussion

The behavioral results of Experiment 2 indicate that part of the color-sharing bonus persists when attention is divided during backward counting. Capacity estimates remained higher when a repetition was tested compared to when a singleton was tested. This result is consistent with findings that perceptual grouping effects remain present regardless of divided attention (e.g., Lamy et al., 2006; Moore & Egeth, 1997) and indicates that the color repetition bonus is not due solely to strategic factors, but is partially due to the reduced load afforded by perceptual grouping. We also observed that this bonus seemed restricted to tests of the duplicates themselves under divided attention. Cross-experiment analyses indicated that the spillover of the color-sharing bonus to encompass unique as well as repeated colors observed in Experiment 1 was likely not present during backward counting in Experiment 2. However, this analysis should be considered cautiously; possibly with the reduced capacities observed with backward counting in Experiment 2, proportional scaling of the pattern observed in Experiment 1 makes the small advantage for singletons within a repeated-item display even slighter, and thus more difficult to detect. (Note however that proportional scaling of differences could not explain increased central fixating in Experiment 2 relative to Experiment 1.) This pattern suggests the possibility that the color-sharing bonus has multiple sources: a perceptual source unaffected by general attention, and a strategic, presumably domain-general source that is applied when general attention is available.

## General Discussion

Consistently with previous reports (Peterson & Berryhill, 2013; Quinlan & Cohen, 2012) we observed a color-sharing bonus in visual working memory capacity. This boost to capacity was most pronounced when one of the duplicates was probed, but the hierarchical Bayesian modeling techniques we used allowed us to observe a small but apparently genuine advantage for singletons from arrays with repetitions compared to the arrays with only unique colors under full attention conditions in Experiment 1. When general attention was divided by requiring backward counting by threes in Experiment 2, overall capacity decreased considerably but the color-sharing bonus for the duplicates remained, indicating that this aspect of the color-sharing bonus is not likely dependent on general attention. However, under divided attention the color-sharing bonus was restricted only to tests of duplicates, and did not extend to the other items from the same displays. These results indicate that the color-sharing bonus may have at least two sources, and the source of the benefit derived from testing a duplicate does not depend entirely on the availability of general attention, but the generalization of this benefit to other items in a repetition display may depend on the availability general attention resources.

A few necessary limitations in our research designs lead us to consider our gaze analyses exploratory. This was the first research project applying eye movement analyses to the color-sharing bonus; we therefore kept stimulus and task parameters close to values known to produce the color-sharing effect and recorded participants gazes without issuing any particular instructions regarding looking. The distances between stimuli were such that fixating specific items was not necessary to encode them; we would expect this factor to limit the variability of movement observed. In these data the relationships we observed between fixating the eventual probed item and accuracy were weaker than in some similar previous work (e.g., Mall et al.,

2014). In Mall et al., while participants were not explicitly instructed to look at particular items, they were advised that certain categories of items were more likely to be tested than others. Under those instructions with similar interitem spacing, we observed substantially more variability in gazes, suggesting that explicitly manipulating some factor related to selective attention promotes selective looking. However, a similar manipulation or overt looking instructions would have been inappropriate here, where our aim was to use natural looking behavior to consider various selective attention explanations for the color-sharing bonus. These choices limited the potential relationships between looking and accuracy, but importantly, would not have biased looking behavior toward any particular pattern. It is therefore reasonable to use these data to generate new hypotheses about relationships between selective attention and the maintenance of visual materials.

The gaze data collected under the full attention conditions of Experiment 1 suggested that the duplicates were likely to capture attention early during stimulus presentation. This is consistent with two possible explanations of this aspect of the color-sharing bonus: a) that duplicates capture attention to the exclusion of other items, or b) that duplicates are used to organize the display into some more coherent pattern. Analyses of the proportion of time that duplicates were fixated tend to rule out the first explanation; for displays with repetitions, if anything participants tended to fixate the duplicates less than the singletons, rather than more. Furthermore, participants tended to fixate singleton items earliest during the retention interval, and fixating the probed item during retention was as beneficial to accuracy as fixating it during stimulus presentation. These circumstances give rise to the idea that repetitions afford a global pattern around which participants organize the scene, and subsequently the challenge is to remember the details ensconced within this global pattern. If we assume that looking during retention can be understood as attempts at covert retrieval (Ferreira et al., 2008), then these gazes could be seen as reflecting attempts to remember the items. Perhaps participants do not attempt to retrieve the repeated items as quickly because they are more likely than the singletons to be known already. Participants may therefore spend their time and attention attempting to retrieve the colors of the singletons. This speculation is broadly consistent with the capacity analyses, and not inconsistent with the explanation proposed by Quinlan and Cohen (2012) that groups of duplicates are preferentially encoded. Our data further suggest that any prioritization of the duplicates occurs early, enabling subsequent focus on other elements, meaning that focus on the duplicates is not merely a selective attention effect.

Under the divided attention conditions of Experiment 2, gazes were no longer clearly related to accuracy. Similar amounts of gaze data were acquired in Experiment 2 as in Experiment 1; the clearest difference between the two gaze data sets was an increase in the tendency to fixate the center of the screen at the expense of other items in Experiment 2. As in Experiment 1, there was no reason to think that duplicates were fixated more frequently than singletons in the repetition displays. That the color-sharing bonus was still observed in Experiment 2 is further evidence that no strategic effort is required to induce the organization of information afforded by the repeated color, though such effort might assist with remembering the other details in the display. This pattern of results is broadly consistent with findings reported by Postle, Idzikowski, Della Sala, Logie, and Baddeley (2006), who concluded that overlap between resources needed for eye movement control and spatial memory were unlikely to be specific to the visual domain. A consistent interpretation of our data could be that backward counting and control of eye movements interfered with each other, thereby preventing strategic rehearsal of

singletons and contributing to the overall reduction in performance on our color-location binding task under divided attention.

These results provide some direction for rethinking both fixed-slot and continuous-resource models of visual working memory as well as some practical guidance for increasing the memorability of visual displays. First, we should perhaps not be talking about “visual” working memory per se, as concurrent nonvisual tasks clearly have a large effect on storage capacity (see also Morey & Bieler, 2013). Second, it is the case that the color-sharing bonus spills over to the nonrepeated items, though this effect is small. While this knowledge does not allow us to discriminate between fixed-slot and continuous-resource models, it provides direction for both classes toward modifications that would enable either model to better account for data. One notion that our data tends to support is that a global representation of the gist of a scene must be maintained along with any individual items (e.g., Alvarez & Oliva, 2009; Vidal, Gauchou, Tallon-Baudry, & O’Regan, 2005; Walker & Davies, 2003). This supposition is at odds with the strictest of the fixed-slot assumptions, but the strongest versions of these models (i.e., models that assume only all-or-none, object-based representations) have already been ruled out; both object and resource limits in visual memory are now commonly supposed (e.g., Anderson, Vogel, & Awh, 2013; Awh et al., 2007; Zhang & Luck, 2008). Practically, our results also confirm that visual feature redundancy improves memory for both the redundant and unique features within the image, which has broad implications for design of visual interfaces and aesthetics.

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Received July 28, 2014

Revision received November 17, 2014

Accepted December 9, 2014 ■