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Category-based grouping in working memory and multiple object tracking

Ansgar D. Endress

Department of Psychology, City University, London, UK

Ilia Korjoukov

Okozlab Ltd, London, UK

Luca L. Bonatti

Universitat Pompeu Fabra, Barcelona, Spain, ICREA, Pg. Lluís Companys 23, 08010

Barcelona, Spain

Ansgar D. Endress

Department of Psychology, City University London

Northampton Square, London EC1V 0HB, UK

E-mail: ansgar.endress.1@city.ac.uk

Author Note

Abstract

Two prominent cognitive capacity limitations are the maximal number of objects we can place in working memory (WM) and the maximal number of objects we can track in a display. Both are believed to have a numeric value of 3 or 4, which has led to the proposal that we have a general cognitive capacity, and that this capacity is most likely linked to limitations of how many objects we can attend simultaneously. Based on previous results showing that we can memorize more objects if they come from different categories than if they come from the same category (e.g., Feigenson & Halberda, 2008; Wood, 2008; Wong, Peterson, & Thompson, 2008), we compare how category-based grouping affects performance for WM and multiple object tracking (MOT). We present participants with either "pure" displays of either cars or faces, or with "mixed" displays of cars *and* faces. Overall, the effects of category are weak. In some analyses but not others, we replicate the mixed advantage for WM, albeit with a small effect size. In contrast, we observe a weak pure advantage for MOT tasks, at least in a meta-analysis of five experiments, but not in all experiments. Accordingly, WM and MOT tasks differed significantly in their sensitivity to category membership. We also find that WM is slightly better for faces than for cars, but that no such difference exists for MOT. We tentatively suggest that cognitive capacity limitations in different domains are at least partially due to limitations of distinct mechanisms.

Keywords: Working memory; Interference; Temporary Memory; Memory Capacity

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Introduction

In one of his classic papers, Miller (1956) noted that people seem to have similar cognitive capacities across a variety of domains, which he called the magical number 7, plus or minus 2. This suggested that we might have a common cognitive capacity that is used by many cognitive processes. While Miller (1956) used the magical number more as a rhetorical device than an accurate estimate (Cowan, 2001), and while the magical number might be closer to 4 than to 7 (Cowan, 2001), relatively little research has directly addressed the relation between capacity limitations in different domains (Cowan, 2015).

Two of the most prominent cognitive capacity limitations are the maximal number of items we can place in working memory (WM), and the maximal number of items we can attend to. Specifically, in those theories of Working Memory (WM) that posit discrete memory capacities (e.g., Awh, Barton, & Vogel, 2007; Luck & Vogel, 1997; Miller, 1956; Cowan, 2005; Rouder et al., 2008; W. Zhang & Luck, 2008; but see Alvarez & Cavanagh, 2004; Bays & Husain, 2008; Bays, Catalao, & Husain, 2009; van den Berg, Shin, Chou, George, & Ma, 2012 for authors holding WM to be a continuous resource), WM is often considered to have a constant capacity of 3 or 4 items.

Likewise, the number of items we can attend to is usually measured in multiple object tracking (MOT) experiments. In such experiments, participants have to follow target objects among identical distractors while the objects move randomly on the screen. The classic result holds that we can track about 3 or 4 of such items (Pylyshyn & Storm, 1988; Scholl & Pylyshyn, 1999).

There is a third cognitive capacity that is limited to 3 or 4 but that we will not investigate in the current experiments: the number of items we can enumerate exactly without counting (Jevons, 1871; Trick & Pylyshyn, 1994), a capacity that is called subitizing. If we see 3 or 4 dots on a screen, we know immediately and exactly how many dots we see. In contrast, if we have to enumerate more items under time pressure, we can only make an approximate estimate of the number of items. Hence, the

enumeration error is constant and close to zero up to 3 or 4 objects, and then grows proportionally to the number of items to be enumerated, yielding a Fechner-Weber pattern (Izard & Dehaene, 2008; Revkin, Piazza, Izard, Cohen, & Dehaene, 2008).

The observation that all of these cognitive capacities are limited to 3 or 4 has led to the proposal that they might rely on a common mechanism, and that this mechanism might be attention. Specifically, our ability to enumerate 3 or 4 items exactly and immediately might rely on the same mechanisms that allow us to track 3 or 4 items attentionally (e.g., Chesney & Haladjian, 2011; Feigenson, Carey, & Hauser, 2002; Knops, Piazza, Sengupta, Eger, & Melcher, 2014; Piazza, Fumarola, Chinello, & Melcher, 2011; Trick & Pylyshyn, 1994), be it through a system of attentional pointers or through an object-file system.[1]

Likewise, according one of the most prominent models of WM (Cowan, 2005), WM capacity limitations are really limitations of attention. Specifically, in his and other models (e.g., Baddeley & Hitch, 1974; Engle, 2002; Oberauer, 2002), we need to attend to items in memory to prevent them from disappearing from memory through forgetting and interference. As we can attend only to 3 or 4 items, we have a WM capacity of 3 or 4 items. Hence, WM, subitizing, and attention are thought to have a common capacity, and this capacity is due to attention.

In the current experiments, we address the relationship between WM and attentional tracking by comparing their functional properties. Specifically, we ask whether these cognitive capacities differ in their sensitivity to the category structure of the items they have to remember and track, respectively.

Evidence for a general cognitive capacity

While there are few direct tests of the view that a general cognitive capacity underlies capacity limitations in different domains, it receives support from a number of results. For example, Piazza et al. (2011) observed that visual WM capacity correlated

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[1] Trick and Pylyshyn (1994) argued against a common mechanism between WM and subitizing, and argued that the spatial pointers called FINSTs that allow us to track objects are pre-attentive. However, as we will discuss below, other authors argued that small-number processing might be related to WM via attention, and used MOT as a measure of attention.

with subitizing capacity, and explained this finding with the hypothesis that both mechanisms relied on the parallel individuation system (though the relationship between WM and subitizing does not seem to hold up when WM is measured with complex span tasks; Tuholski, Engle, & Baylis, 2001). In line with this view, Chesney and Haladjian (2011) found that the more items participants had to track in a MOT task, the more errors they made in a simultaneous subitizing task (that is, tracking more items increased their *coefficient of variation*, and thus reduced their subitizing capacity). Further, a verbal WM task reduced the subitizing capacity to a lesser extent than MOT, at least when participants had to remember 3 letters, suggesting that the subitizing capacity might be specifically impaired by a visual task.

However, in Chesney and Haladjian's (2011) experiments, the subitizing display was presented for just 50 ms. As a result, it was presumably quite easy to miss while performing a demanding visual task such as MOT. Further, subitizing requires attentional resources (Burr, Turi, & Anobile, 2010). Consequently, to enumerate the items in the subitizing display, participants needed to disengage from the MOT stimuli and to select the enumeration stimuli. Both aspects of the task might plausibly increase subitizing errors even if subitizing and MOT did not rely on the same processes. In line with this view, the verbal WM task also reduced subitizing capacities, albeit only when participants had to remember 5 letters.

The relationship between tracking and WM is similarly unclear. Different authors have used an individual difference approach to investigate the interrelations among different types of attentional processing. For example, both Oksama and Hyönä (2004) and Huang, Mo, and Li (2012) found that MOT performance correlated with measures of visual WM, confirming that attentional tracking and WM might be related.

However, Oksama and Hyönä (2004) also showed that MOT correlates not only with visual WM but also with verbal WM when people have to track the identity of items on top of their positions. Similarly, Huang et al. (2012) found a correlation between MOT and counting speed *outside* of the subitizing range, as well as with other tasks that have no obvious relation to attentional pointers or object files (e.g., with the

reaction time to report the color of an item). As a result, it is unclear what these correlations mean in terms of the underlying psychological mechanisms. In fact, Huang et al. (2012) used no less than 18 different tasks, and showed that performance on most of them correlated with a general "attention" factor that might well reflect how vigilant or compliant participants are during the experiment.

Doubts about a general cognitive capacity

While the results discussed so far do not provide unambiguous evidence for a general cognitive capacity, there are also other observations that are problematic for the view that cognitive capacity limitations are due to the limitations of a parallel attention system. First, dual-task experiments revealed only limited interference between attentional tasks and WM tasks (e.g., Fougnie & Marois, 2006; Hollingworth & Hwang, 2013; H. Zhang, Xuan, Fu, & Pylyshyn, 2010), a result that would be unexpected if WM and attention relied on the same mechanism. That said, there clearly are interactions between MOT and WM. For example, WM might help binding features (e.g., colors) to objects that are tracked (Makovski & Jiang, 2009b). However, our tracking abilities and our WM abilities still seem to interfere remarkably little with each other.

Second, neither attention nor WM might necessarily have a capacity of 3 or 4. When objects move slowly, we can track up to 8 of them, and when they move very fast, only a single one (Alvarez & Franconeri, 2007; Franconeri, Jonathan, & Scimeca, 2010; Holcombe & Chen, 2012). Likewise, the capacity of WM might not be fixed either, but rather depend on other factors such as the complexity of the stimuli (e.g., Alvarez & Cavanagh, 2004; Eng, Chen, & Jiang, 2005), their familiarity (e.g., Feigenson, 2008; Jackson & Raymond, 2008) their confusability (e.g., Baddeley, 1966; Conrad, 1963; Conrad & Hull, 1964; Viswanathan, Perl, Visscher, Kahana, & Sekuler, 2010), the degree to which they can be perceived categorically (Olsson & Poom, 2005), the presence of proactive interference among items (Banta Lavenex, Boujon, Ndarugendamwo, & Lavenex, 2015; Endress & Potter, 2014; Sands & Wright, 1980), the spatial configuration of the items (e.g., Makovski, 2016), and on the specific stimuli. For example, Cowan, Johnson, and Saults (2005) observed memory capacities of about 6

words (under high proactive interference), while Wood (2008) observed memory capacities of only 2 actions.

Third, in a general sense, memory and some forms of attention (though not necessarily the tracking variety) are certainly related. For example, attention is critical to place items in memory to begin with (see, among many others, e.g., Chen & Cowan, 2009; Craik & Lockhart, 1972; Lepsien & Nobre, 2007; Majerus et al., 2014; Morey & Bieler, 2013; Vergauwe, Camos, & Barrouillet, 2014). Further, recent physiological data suggest that, at least in some situations, WM limitations might be in part encoding limitations (e.g., Buschman, Siegel, Roy, & Miller, 2011; Tsubomi, Fukuda, Watanabe, & Vogel, 2013). Conversely, WM might also support attention. For example, and as mentioned above, WM might help binding features to objects that are tracked (e.g., Makovski & Jiang, 2009b). Further, items that are maintained in WM attract attention in unrelated search tasks, suggesting that WM might even guide attention (e.g., Downing, 2000; see also Awh & Jonides, 2001 for a review).

However, the mechanistic basis for this two-way interaction between WM and attention is unclear. Directing attention away from a stimulus certainly impairs performance on tasks using this stimulus (see Awh & Jonides, 2001 for a review of the WM literature, but also Toro, Sinnett, & Soto-Faraco, 2005; Turk-Browne, Jungé, & Scholl, 2005, for similar results in rather different domains such as statistical learning). However, such results only show that items need suitable encoding conditions for appropriate processing, but it is unclear to what extent attentional tracking capacities are required. Likewise, while WM can certainly guide attention, so can LTM, even when participants never had any reason to memorize the stimulus features that guide attention (Fan & Turk-Browne, 2016).[2] Given that items in LTM are clearly not

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[2]In Fan and Turk-Browne's (2016) experiments, participants first encoded either the position or the color of a novel shape, a manipulation that affects LTM for the stimulus dimension that was encoded and probed (Fan & Turk-Browne, 2013). Following this encoding phase, participants completed a 1-item delayed match-to-sample task with *monochrome* versions of these shapes. Critically, in the retention interval, participants viewed two colored Gabor patches, and had to decide which of them was tilted. One of the patches had the color that the memory object had during the encoding phase, and the other patch a different color. Participants were faster to detect the target (i.e., the tilted shape) if it shared the color that the memory object had during the encoding phase, suggesting that LTM representations can guide attention.

actively maintained by attentional mechanisms, this suggests that attentional guidance from memory representations does not rely on attentional processing in memory.

The current experiments

The results reviewed so far are certainly problematic for the general capacity view. However, they do not rule it out. That is, the numeric value of the capacities might not be fixed, but the nature of these limitations might still be the same.

Here, we ask whether attention and WM rely on a common mechanism by comparing their functional properties. Specifically, participants had to track or remember items in a display. The items were either all from the same category (faces **or** cars; hereafter "pure" displays), or mixed both categories (faces **and** cars; hereafter "mixed" displays). We asked whether attention and memory would show a benefit or a cost for mixed compared to pure displays. (In the experiments below, items in "mixed" displays differ both in their category membership and their composition of visual features; when we refer to mixed or pure displays below, we always refer to this experiment situation.)

We selected faces and cars as contrasting categories, because evidence suggests that objects and humans are processed differentially early in development (e.g., Bonatti, Frot, Zangl, & Mehler, 2002) and have a different brain representation in adulthood (e.g., Gauthier, Tarr, Anderson, Skudlarski, & Gore, 1999; Kanwisher, McDermott, & Chun, 1997), suggesting that participants likely see them as members of distinct categories.

Our predictions are as follows. With respect to WM, there is a considerable literature showing that it is easier to memorize items from different categories than items from only one category (Feigenson & Halberda, 2008; Wood, 2008; Wong et al., 2008). While it has been suggested that the increase in memory capacity might be due to the availability of independent memory stores for items from different categories, it is also possible that using different categories reduces interference among memory items, and thus improves performance (Endress & Szabó, in press). Be that as it might, we

would expect mixed displays to be easier to remember than pure displays.

With respect to MOT, previous research is ambiguous as to whether pure sets should be harder or easier to track than mixed sets. On the one hand, when targets in MOT experiments can be grouped based on different features, tracking performance is somewhat improved (e.g., Erlikhman, Keane, Mettler, Horowitz, & Kellman, 2013; Störmer, Li, Heekeren, & Lindenberger, 2011; see Keane, Mettler, Tsoi, & Kellman, 2011, for non-feature-based grouping results). However, MOT performance is also impaired when targets are grouped with distractors (e.g., Erlikhman et al., 2013; Keane et al., 2011; Scholl, Pylyshyn, & Feldman, 2001; Störmer et al., 2011). Crucially, when each target has a "paired" distractor it shares a feature with (while the features are different across target-distractor pairs), performance is worse than when all items share this feature. For example, Makovski and Jiang (2009a) used colored digits as stimuli. In the "paired digit" condition, the digits 1, 2, 4 and 7 would appear both as targets and as distractors, such that each digit would appear exactly twice in a display. Results showed that performance in this condition was worse compared to a condition where all eight digits were identical (e.g., all consisted of the digit 1). However, while Makovski and Jiang (2009a) found a similar disadvantage when targets and distractors were paired in both digit identity and digit color, color-based pairing was insufficient to yield such an disadvantage (see also Horowitz et al., 2007, who did not find a significant disadvantage in the paired condition, using cartoon animals as stimuli).[3] Given these results, we would expect better MOT performance for pure displays compared to mixed displays (or equivalent performance in both kinds of displays). Crucially, we will ask whether the effect of display composition is different between WM and MOT tasks.

In sum, we would expect a memory advantage for mixed displays, and a tracking advantage for pure displays. This expectation also appears to make intuitive sense. While memory performance can be improved by grouping items (e.g., through

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[3]In principle, it is possible that the very presence of distinct objects has a tracking cost compared to a homogenous tracking condition, because identifying the objects might distract resources from tracking even if the object identity is irrelevant for the task. This is particularly plausible because, at least in the case of WM, participants seem to encode the locations of objects relatively automatically (Makovski & Jiang, 2008). Empirically, however, tracking is improved if items have unique features (Makovski & Jiang, 2009a).

chunking), object tracking requires keeping targets separate from distractors.

However, there is an important limitation of this design. Even if MOT and WM have different properties with respect to category-based grouping, it would only show that *some* mechanisms involved in these tasks are separable, but not that there are no shared mechanisms that are involved in both tasks. Here, we thus address only the most extreme version of a shared capacity view, namely that the mechanisms involved in WM and object-tracking are identical. Given the current state of the art, we believe that the assessment of the status of the simplest hypothesis is a first step towards a fuller understanding of the relationship between different cognitive capacities.

To foreshadow our results, we found that memory is slightly better in mixed displays, whereas tracking works slightly better with pure displays. WM and MOT might thus rely on distinct capacities. However, these effects were weak and not consistent across experiments. While this limits the generality of our results, we believe that the current data are problematic for the stronger version of the shared capacity view. For readability, we first present meta-analyses of our experiments (as well as analyses treating the different experiments as one big experiment), and then report analyses of individual experiments.

Materials and methods

Apparatus

Stimuli were presented on a Philips 109B 19″ CRT monitor at a resolution of 1024 × 768 pixels and a refresh rate of 60 Hz. The experiment was administered in a soundproofed booth and was run using EventIDE (http://www.okazolab.com).

Materials

In the MOT experiments, stimuli were a frontal view of the face of Lionel Messi and a frontal view of a compact car (Seat Cordoba). We selected these stimuli because they represent one of the best-known soccer player and best selling cars in Spain. Each image spanned 53 × 53 pixels (approximately 3.8 × 3.8^o at a typical view distance for 60 cm). In the WM experiments, stimuli were frontal views of faces of FC Barcelona

players and frontal views of various cars. Each image measured 75 × 75 pixels (approximately 5.4 × 5.4º at a typical view distance for 60 cm). We used a total of 12 images for each category.

Procedure

Multiple Object Tracking.   Participants were informed that they would see 12 moving objects in each trial. They were told that, at the beginning of each trial, some of them would blink, and that their task was to follow them. Each trial started with a fixation cross (30 × 30 pixels; 2.2 × 2.2º) for 1000 ms. Then all objects would appear. After a delay of 300 ms, the target objects would blink for 4.8 s at a frequency of 5 Hz. The objects started moving 300 ms after they had stopped blinking, for a tracking period of 10 s.

The stimuli were either 12 faces, 12 cars (pure conditions), or 6 cars *and* 6 faces (mixed condition). Each condition was equally represented among the trials (except in Experiment 3a, where mixed trials were twice as frequent as either pure condition). The number of targets (among 12 objects in total) are given in Table 1. In the mixed condition, half of the targets (and half of the distractors) came from either category.

The initial positions of the objects defined by random polar coordinates relatively the screen center. The polar angle (θ) was randomly chosen between 0 and 360 degrees, whereas the polar eccentricity (R) was random chosen between 0 and 9.5 dva (266 pixels).

To avoid overlap between the objects, we constructed imaginary circles inscribing the objects, and discarded the stimulus positions when the circles corresponding to two objects overlapped. As a result, distances between stimulus centers were greater than 106 pixels.

Motion trajectories for all 12 objects were precalculated at the start of every trial. They were represented as series of object positions separated by 32 ms frames. Each object started moving in a random direction and speed. In each of the following frames, the position of each object was updated (i) according to its current velocity in the x and

the y direction, (ii) by a repulsive component in the x direction that increased exponentially ($\sigma = .018$) as the object approached the left or the right screen boundary, (iii) by a repulsive component in the y direction that increased exponentially ($\sigma = .018$) as the object approached the top or the bottom screen boundary, and (iv) by repulsive components away from each of the other objects whose magnitude was a negative exponential function ($\sigma = .2$) of the objects' mutual distance. The repulsive components prevented the objects from colliding with each other and the screen boundaries. Details for this algorithm are given in Appendix A. Further, with a change probability of 0.1, a new speed was selected randomly between the maximum and the minimum velocity; these differed across the experiments, and are given in Table 1.

After the motion stopped, tracking performance was evaluated in one of two ways. In the "*yes/no*" method (after Pylyshyn & Storm, 1988), one object was highlighted by a green mask immediately after the objects stopped moving. Participants had to indicate whether the highlighted object was among those they had tracked. If it was, they had to press the up-arrow key, otherwise the down-arrow key. After their response, participants received feedback for 2000 ms: For correct answers, they were shown a green plus sign, and for incorrect answers a red minus sign.

In the "*click-all*" method, all objects turned into white circles after their movements stopped. As there were 6 targets among 6 distractors, participants had to select 6 by clicking on them. Circles turned orange after being clicked on, and participants could unselect them. Once they had clicked on 6 circles, participants were given feedback for 2000 ms: Their correct choices turned green, and the incorrect ones red. They were also given the percentage of correct choices.

Further characteristics of the different MOT experiments are given in Table 1.

Working memory. The WM experiments used the change detection method (e.g., Alvarez & Cavanagh, 2004; Luck & Vogel, 1997; Phillips, 1974; Vogel, Woodman, & Luck, 2001). Participants were informed that they would see a number of objects on the screen, and, after a delay, a second array of objects. They had to decide whether any of the objects had changed. Each trial started with a fixation cross (76×76 pixels;

5.5 × 5.5º) of 1000 ms, the sample array for durations between 2000 ms and 4000 ms (see Table 2 for the specific durations), a retention delay of 900 ms, and finally the test array. The presentation duration of the test array was always identical to that of the sample array. Participants had to indicate with the arrow keys whether one of the objects had changed between the sample and the test array (up: change; down: no change). Following this, they were given feedback for 2000 ms.

The arrays contained an even number of objects between 2 and 10 (see Table 2), either all faces, all cars (pure conditions), or half faces and half cars (mixed condition). All three conditions were equally frequent.

The initial positioning of the objects was identical to that in the multiple object experiments.

Familiarity judgment. In Experiments 1, 2b and 3b, we assessed the participants' familiarity with the stimuli. They were asked to rate, on a scale from 1 to 5, whether they would recognize the face and the car if they saw it in the street.

Sample size and participants

In the experiments below, we aimed for 24 participants per experiment. However, we also report some pilot experiments for which the whole sample has not been acquired. Further, in the analyses below, we excluded participants who scored at less than 50% correct for set-size 6. We considered that participants performing at less than 50% clearly did not pay attention to the task. For example, in the click-all method, even tracking a single item would yield a percentage of correct responses of about 55%, and, if participants can track about 3 items, their percentage of correct responses should be about 67% (Hulleman, 2005). The reason for excluding participants based on set-size 6 is because this set-size was present in all experiments. The number of excluded participants is given in Tables 1 and 2.

The final numbers of participants in each experiment are given in Tables 1 and 2. In total, 107 participants (76 females, mean age 21.7 y, range 18 – 34) took part in the MOT experiments, and 98 participants (68 females, mean age 21.1 y, range 18-30) took

part in the WM experiments. They were recruited from the Universitat Pompeu Fabra community, and tested in sessions of 1.5 h that comprised other unrelated experiments (generally on statistical learning) in addition to the current experiments.

Meta-analyses

For readability, we first present the results of our meta-anlyses, before presenting the individual experiments in more detail. In the meta-analyses, we focus on set-size 6, because the effects were most consistent at this set-size. On the one hand, it seems plausible that the effects might be maximal at an intermediate set-size, as it avoids both ceiling and floor effects. On the other hand, choosing a set-size based on the data inflates the type I error rate. Given the weakness of the results presented below, this is a serious caveat.

To compare the results across the MOT experiments and the WM experiments, we use difference scores, where a difference score between two conditions C_1 and C_2 is defined as the normalized difference $(C_2 - C_1)/(C_2 + C_1)$.

In all (meta-) analyses, we discarded the first block as training.

Multiple object tracking

Pure vs. mixed displays. In our first meta-analysis, we averaged the score from the pure conditions (i.e., either all cars or all faces), and compared it to the mixed condition in a difference score. Positive difference scores indicate an advantage for pure displays over mixed displays.

A mixed-effect meta-analysis (Borenstein, Hedges, Higgins, & Rothstein, 2010) combined the results for set-size 6 for Experiments 1-3 (100 participants in total). As shown in Figure 1, we observed a weak pure advantage, $d_\text{meta-analysis} = .023$, $SE = .0007$, $CI_{.95} = .0008, .037$, $Z = 3.04$, $p = .002$. We also entered all difference scores into a t test against 0, and observed a weak advantage for the pure conditions ($d = .026$, $SD = .086$), $t(99) = 3.0$, $p = .0034$, Cohen's $d = .3$, $CI_{.95} = .0087, .0429$.[4] The likelihood ratio against the null hypothesis was 8.96 after correction with the Bayesian

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[4]The significance level was somewhat smaller with a signed rank test, $V = 2958$, $p = .012$.

Information Criterion, and 31.63 after correction with the Akaike Information Criterion (AIC). Fifty-four participants showed a pure advantage, 41 a mixed advantage, and 5 had a difference score of exactly 0. There were no consistent results for other set-sizes.[5]

Faces vs. cars. We also asked whether, for set-size 6, there was a processing advantage for faces over cars. In the analyses below, positive difference scores indicate a face advantage.

As shown in Figure 2, a meta-analysis (100 participants in total) did not detect a deviation from chance, $d_{\text{meta-analysis}} = .002$, $SE = .003$, $CI_{.95} = -.005, .009$, $Z = .62$, $p = .534$, nor did a combined t-test, $(d = .004, SD = .078)$, $t(99) = .48$, $p = .635$, Cohen's $d = .048$, $CI_{.95} = -.012, .019$.[6] The likelihood ratio in favor of the null hypothesis was 8.93 after BIC correction, and 2.53 after AIC correction. There were 43 participants with a face advantage, 43 participants with a car advantage, and 14 participants with a difference score of exactly 0.

Working memory experiments

Pure vs. mixed displays. As shown in Figure 3, a mixed meta-analysis for set-size 6 (97 participants) revealed a significant mixed advantage, $d_{metanalysis} = -.021$, $SE = .009$, $CI_{.95} = -.039, -.003$, $Z = 2.26$, $p = .024$, as did a combined t-test of all experiments $(d = -.021, SD = .094)$, $t(96) = 2.17$, $p = .032$, Cohen's $d = .22$, $CI_{.95} = -.04, -.002$.[7] The likelihood ratio in favor of the alternative hypothesis was 1.07 after

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[5]Given that, in some experiments, we observed weak mixed advantages for set-sizes 2 and 4, we also performed a meta-analysis for these set-sizes in the experiments where these set sizes were administered (i.e., Experiments 1 and 3b, 44 participants in total). For set-size 2, the meta-analysis did not detect a deviation from chance, $d_{\text{meta-analysis}} = -.009$ $SE = .0097$, $CI_{.95} = -.0285, .0096$, $Z = -.968$, $p = .333$. Entering the combined difference scores into a t-test did not detect a deviation from chance either $(d = -.000, SD = .035)$, $t(43) = 1.64$, $p = .107$, Cohen's $d = .25$, $CI_{.95} = -.019, .002$; a signed rank test was not significant either, $V = 75$, $p = .268$. Correcting with the Bayesian Information Criterion, the likelihood ratio in favor of the null hypothesis was 1.72. However, after correction with the Akaike information criterion, we observed a likelihood ratio of 1.29 in favor of the alternative hypothesis (.78 in favor of the null hypothesis). We thus tentatively conclude that the significant mixed advantages we observed for set-size 2 were type I errors.

For set-size 4, the mixed-factor meta-analysis did not detect a deviation from chance, $d_{\text{meta-analysis}} = -.012$, $SE = .009$, $CI_{.95} = -.030, .007$, $Z = -1.25$, $p = .211$. However, a combined t-test reached significance, $(d = -.012, SD = .040)$, $t(43) = 2.05$, $p = .0465$, Cohen's $d = .31$, $CI_{.95} = -.025, -.0002$; a signed rank test was significant as well, $V = 121$, $p = .0219$. Further, likelihood analysis favored the alternative hypothesis ($\Lambda = 1.23$ after BIC correction and $\Lambda = 2.72$ after AIC correction). It thus is unclear whether there is a mixed advantage at set-size 4.

[6]A signed rank test did not differ from chance either, $V = 1735$, $p = .561$.

[7]A signed rank test differed from chance as well, $V = 1378$, $p = .0234$.

BIC correction, and 3.72 after AIC correction. 57 participants had a mixed advantage, 30 a pure advantage, and 10 had a difference score of exactly 0.

Cars vs. faces. Curby and Gauthier (2007) showed that, at least for long encoding durations, faces are better remembered than other objects (but see Wong et al., 2008). We thus compare whether our results reveal a similar effect, by conducting a meta-analysis of the difference score between face only trials and car only trials.

As shown in Figure 4, a meta-analysis for set-size 6 (97 participants) revealed a significant face advantage, $d = .023$, $SE = .005$, $CI_{.95} = .012, .033$, $Z = 4.21, p = .00003$, though a combined t-test was only marginal ($d = .024$, $SD = .121$), $t(96) = 1.93$, $p = .057$, Cohen's $d = .2$, $CI_{.95} = -.0007, .048$.[8] The likelihood ratio is 1.54 in favor of the null hypothesis after BIC correction, but in favor of the alternative hypothesis after AIC correction ($\Lambda = 2.25$). 43 participants had a face advantage, 31 a car advantage and 23 a difference score of exactly 0.

Discussion

In the current experiments, we asked to what extent visual attention and visual WM share common properties. Based on earlier results suggesting that it might be easier to memorize mixed sets (Feigenson & Halberda, 2008; Wood, 2008; Wong et al., 2008) but that it might be harder to track mixed sets (at least when the sets are defined by simple features; Erlikhman et al., 2013), we sought to directly contrast parallel attention and WM in their respective sensitivity to category information, using identical stimuli.

Meta-analyses of our experiments revealed a small pure advantage for MOT experiments, and a small mixed advantage for WM experiments, at least for set-size 6. As shown in Figure 5, the difference scores were significantly larger for MOT experiments than for WM experiments, $t(195) = 3.62$, $p = .0004$, Cohen's $d = .516$. This difference was confirmed by a signed rank test, $p = .0002$, as well as by comparing the counts of participants having pure, mixed and no advantages, using Fisher's test, $p = .0035$. Further, a mixed model analysis on the difference scores with set-size and

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[8]A signed rank test was significant as well; $V = 1011$, $p = .043$.

task (MOT vs. WM) as fixed factors, their interactions as well as participants and choice type (click-all vs. up/down) as random factors revealed that the difference score was higher for set-size 6 (with a reference level of 2), $\beta = .041$, $SE = .018$, $p = .031$, but that, for set-size 6, the difference score was reduced in the WM experiments $\beta = -.053$, $SE = .026$, $p = .038$.

In line with previous results, we thus tentatively conclude that visual attention and visual WM have different properties and hence are likely not to share all of their underlying mechanisms.[9]

However, while the MOT experiments differed from the WM experiments in the effects of the category structure of the stimuli with a medium effect size (Cohen's $d = .516$), the effects in the individual tasks were fairly weak, and, as we will now show, not observed in all experiments.

Results of individual experiments

The meta-analyses above revealed a small advantage for pure displays in MOT experiments, and a small advantage for mixed displays in WM experiments. While the effect sizes were rather small in both cases, we note that these results are in line with previous results in both MOT (Erlikhman et al., 2013; Horowitz et al., 2007; Makovski & Jiang, 2009a) and WM (e.g., Feigenson & Halberda, 2008; Wood, 2008; Wong et al., 2008). However, these effects did not emerge in all experiments. We thus report the individual experiments below, noting in each case to what extent they were consistent with the meta-analysis above.

Multiple object tracking

Experiment 1. In Experiment 1, participants had to track 2, 4 or 6 objects among 12 items in total. At the end of the tracking phase, a single object was highlighted, and participant had to indicate whether it was among the target objects.

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[9]While performance did not differ between car and face trials in MOT experiments, we observed a small face advantage for WM experiments. However, the face advantage did not differ significantly between MOT and WM experiments, either in a t-test, $t(195) = 1.38$, $p = .17$, Cohen's $d = .196$, or in a signed rank test, $p = .185$, or when comparing counts of face vs. car vs. no advantages, $p = .131$.

Participants completed three blocks with 36 trials each.

We analyze the different set-sizes individually, both because not all experiments below comprise all set-sizes, and because Levene's test indicated differences in the variance of the difference scores at different set sizes. While we report t-tests, the pattern of significance is unchanged with a signed rank test.

For set-size 2, the difference score ($M = .0003$, $SD = .033$) did not differ significantly from zero, $t(23) = .042$, $p = .967$, Cohen's $d = .0085$, $CI_{.95} = -.014, .014$. For set-size 4, the difference score ($M = -.021$, $SD = .039$) revealed a mixed advantage, $t(23) = 2.65$, $p = .014$, Cohen's $d = .54$, $CI_{.95} = -.037, -.0046$. In contrast, with set-size 6, the difference score ($M = .08$, $SD = .12$) revealed a pure advantage, $t(23) = 3.32$, $p = .003$, Cohen's $d = .68$, $CI_{.95} = .03, .13$. The distribution of the number of participants with mixed and pure advantages is shown in Table 3; performance in terms of the percentage of correct responses is shown in Figure 6.

Participants were highly familiar with the face (Lionel Messi; $M = 4.75$, $SD = .85$; on a scale of 1 to 5) and also reasonably familiar with the car ($M = 3.54$, $SD = 1.59$).

Experiments 2a and 2b.   In Experiments 2a and 2b, participants had to track 6 items among 12 items in total. At the end of the tracking phase, the items changed to white circles, and participants had to click on all targets. There were 4 blocks with 30 trials each. The difference between Experiments 2a and 2b was that in Experiment 2b (but not in Experiment 2a), participants had to rate their familiarity with the objects. As for Experiment 1, we report t-tests, but the pattern of significance is unchanged with a signed rank test.

Given that the difference scores did not differ between Experiments 2a and 2b, $F(1,45) = .08$, $p = .779$, $\eta_p^2 = .002$, we combined the experiments. The difference scores ($M = .012$, $SD = .031$) indicated a small pure advantage, $t(46) = 2.58$, $p = .013$, Cohen's $d = .38$, $CI_{.95} = .003, .021$. Counts of participants showed pure, mixed or no advantages are shown in Table 3.

Participants in Experiment 2b rated the face as highly familiar ($M = 4.39$, $SD = .89$) and also the car as reasonably familiar ($M = 3.78$, $SD = 1.35$).

Experiments 3a and 3b.   Experiment 3a was a replication of set-size 6 in Experiment 1, except that the frequency of mixed and pure trials was equated. There were 4 blocks of 32 trials. Experiment 3b was an exact replication of Experiment 1, except that it comprised four rather than three blocks. Again, we report t-tests, but the pattern of significance is unchanged with a signed rank test.

In Experiment 3a, the difference score ($M = .009$, $SD = .05$) failed to differ from chance, $t(8) = .51$, $p = .621$, Cohen's $d = .17$, $CI_{.95} = -.030, .047$, potentially due to the small sample size.

In Experiment 3b, we observed a small pure advantage for set-size 2 ($M = -.019$, $SD = .035$), $t(19) = -2.48$, $p = .0226$, Cohen's $d = .55$, $CI_{.95} = -.035, -.003$. For set-size 4, the difference scores did not differ from chance ($M = -.002$, $SD = .041$), $t(19) = .25$, $p = .804$, Cohen's $d = .056$, $CI_{.95} = -.021, .017$. For set-size 6, the difference score did not differ from chance either ($M = .003$, $SD = .12$), $t(19) = .1$, $p = .923$, Cohen's $d = .022$, $CI_{.95} = -.05228, .057$. The counts of participants showing pure and mixed advantages in shown in Table 3.

In sum, in Experiments 3a and 3b we do not find the same effects as in Experiments 1 and 2.

Working memory

Experiment 4.   In Experiment 4, participants had to detect changes in arrays of 2, 4, 6, 8 or 10 cars or faces. The presentation duration was 3 s for the sample array. There were 4 blocks of 60 trials each.

As in the MOT experiments above, we analyze the set-sizes individually. As above, a signed rank test yielded the same pattern of significance as a t-test.

The difference score did not differ from chance for any of the set-sizes: Set-size 2: $M = -.011$, $SD = .041$, $t(18) = 1.14$, $p = .27$, Cohen's $d = .26$, $CI_{.95} = -.030, .009$; set-size 4: $M = -.016$, $SD = .093$, $t(18) = .72$, $p = .478$, Cohen's $d = .17$, $CI_{.95} = -.061, .030$; set-size 6: $M = -.026$, $SD = .116$, $t(18) = .99$, $p = .333$, Cohen's $d = .23$, $CI_{.95} = -.082, .029$; set-size 8:$M = .015$, $SD = .162$, $t(18) = .39$, $p = .698$, Cohen's $d = .091$,

$CI_{.95}$ = -.063, .093; set-size 10: M = .017, SD = .152, $t(18)$ = .5, p = .625, Cohen's d = .11, $CI_{.95}$ = -.056, .091.

Counts of participants showing pure, mixed or no advantages are shown in Table 4; the performance in terms of the percentage of correct responses is shown in Figure 7.

Experiment 5. In Experiment 5, participants had to detect changes in arrays of 4, 6 or 8 cars or faces. The presentation duration was 3 s for the sample array. There were 6 blocks of 36 trials each.

For set-size 6, we observed a significant mixed advantage, where the difference score indicated better performance for pure displays (M = -.034, SD = .08), $t(23)$ = 2.07, p = .0498, Cohen's d = .42, $CI_{.95}$ = -.067, -.00004. In contrast, the difference score did not differ from chance for set-size 4 (M = -.012, SD = .054), $t(23)$ = 1.09, p = .289, Cohen's d = .22, $CI_{.95}$ = -.035, .011, nor for set-size 8 (M = -.017, SD = .118), $t(23)$ = .72, p = .481, Cohen's d = .15, $CI_{.95}$ = -.067, .033.

Experiment 6a and 6b. Experiment 6a was a replication of set-size 6 from Experiment 5, except that we used 6 blocks with 40 trials each. Experiment 6b was an exact replication of Experiment 5.

In Experiment 6a, the difference score failed to differ from zero (M = -.0105, SD = .045), $t(9)$ = .74, p = .477, Cohen's d = .23, $CI_{.95}$ = -.043, .022, perhaps due to the small sample size.

In Experiment 6b, the difference score did not deviate from zero either for set-size 4 (M = -.023, SD = .079), $t(23)$ = -1.39, p = .177, Cohen's d = .28, $CI_{.95}$ = -.056, .011, or for set-size 6 (M = .0008, SD = .0896), $t(23)$ = .04, p = .967, Cohen's d = .009, $CI_{.95}$ = -.037, .039, or for set-size 8 (M = -.005, SD = .094), $t(23)$ = -.28, p = .782, Cohen's d = .06, $CI_{.95}$ = -.045, .034.

Experiment 7. Experiment 7 was identical to Experiment 4, except that the sample array was presented for 2 s rather than 3 s.

The difference score did not deviate from chance for any set-size, set-size 2: (M = -.012, SD = .039), $t(9)$ = -.99, p = .349, Cohen's d = .31, $CI_{.95}$ = -.040, .016; set-size 4: (M = -.007, SD = .123), $t(9)$ = -.17, p = .87, Cohen's d = .053, $CI_{.95}$ = -.095, .081;

set-size 6: ($M = .017$, $SD = .093$), $t(9) = .59$, $p = .572$, Cohen's $d = .19$, $CI_{.95} =$ -.0495, .084; set-size 8: ($M = .145$, $SD = .214$), $t(9) = 2.14$, $p = .061$, Cohen's $d = .68$, $CI_{.95} = $ -.008, .298; set-size 10: ($M = $ -.025, $SD = .130$), $t(9) = $ -.61, $p = .558$, Cohen's $d = .19$, $CI_{.95} = $ -.118, .068.

Experiment 8.   Experiment 8 was identical to Experiment 4, except that the sample array was presented for 4 s rather than 3 s.

The difference score did not deviate from zero for any of the set-sizes, set-size 2: ($M = .003$, $SD = .050$), $t(9) = .2$, $p = .844$, Cohen's $d = .064$, $CI_{.95} = $ -.032, .039; set-size 4: ($M = $ -.025, $SD = .07$), $t(9) = $ -1.03, $p = .33$, Cohen's $d = .33$, $CI_{.95} = $ -.080, .030; set-size 6: ($M = $ -.0795, $SD = .116$), $t(9) = $ -2.17, $p = .058$, Cohen's $d = .69$, $CI_{.95} = $ -.162, .003; set-size 8: ($M = .051$, $SD = .122$), $t(9) = 1.3$, $p = .225$, Cohen's $d = .41$, $CI_{.95} = $ -.037, .138; set-size 10: ($M = .037$, $SD = .129$), $t(9) = .9$, $p = .39$, Cohen's $d = .29$, $CI_{.95} = $ -.055, .129.

General Discussion

What types of mechanisms underlie our cognitive capacity limitations? Following Miller's (1956) seminal paper, a prominent view has been that capacity limitations in different domains might be interrelated (see Cowan, 2001, 2015), perhaps because they are all based on a parallel individuation system. While some results supported this view (e.g., Chesney & Haladjian, 2011; Knops et al., 2014; Piazza et al., 2011; see also Huang et al., 2012; Oksama & Hyönä, 2004, for individual differences approaches), there is also a growing literature suggesting that VWM might not rely on a parallel individuation system. For example, there seems to be relatively little cross-talk between a MOT task and a VWM task (Fougnie & Marois, 2006; Hollingworth & Hwang, 2013; H. Zhang et al., 2010), and the effects that WM was thought to exert on attention can also stem from LTM (Fan & Turk-Browne, 2016).

Similarly, the specific value of the capacity limitations is not necessarily diagnostic for the underlying mechanisms. In the case of MOT experiments, the tracking capacity seems to depend on task parameters such as the speed of the objects (e.g., Alvarez &

Franconeri, 2007; Franconeri et al., 2010; Holcombe & Chen, 2012). In the case of WM experiments, a variety of factors influence the observed memory capacities (see introduction). As a result, the historic assumption that different cognitive capacities share a numeric value might be little more than a coincidence due to the way in which these capacities have been tested.

The current results are consistent with this view, suggesting that WM limitations cannot be explained just based on attentional limitations. Indeed, attention and WM seem to have different properties: MOT performance was slightly better in pure displays than in mixed displays, while WM performance was slightly better with mixed displays. These effects were rather weak, but are consistent with earlier results on both MOT (Erlikhman et al., 2013; Makovski & Jiang, 2009a) and WM (e.g., Feigenson & Halberda, 2008; Wood, 2008; Wong et al., 2008). In particular, the effect of category membership seems to differ between MOT and WM experiments, respectively. While this effect was not large, it was not expected to be so. Considering the primitive nature of the mechanisms involved in WM and MOT, the very fact of finding that categories differentially affect them is remarkable. We thus tentatively conclude that WM and MOT do not rely on the same mechanisms.[10]

Do our results rely on category-membership or visual similarity?

While MOT and WM seem to react differently to manipulations of group-membership, it is important to ask which aspects of this manipulation drove these differences. In fact, not only did our different-category items belong to different categories, but they also had different visual features. As a result, differences in category membership or on visual feature composition might affect MOT and WM performance, and might also affect them differently. While the current data do not allow us to tease apart these possibilities, we speculate that WM performance might be more affected by category membership, while MOT performance might be more sensitive to similarity in

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[10]We draw this conclusion for the change detection paradigm where all items are presented simultaneously, as this paradigm provided the most consistent evidence for fixed visual WM capacity limitations. However, it is possible that the link between MOT and WM might be stronger when the WM paradigm has a temporal component, for example when items are presented and tested sequentially (e.g., Endress & Potter, 2012; Intraub, 1980; Potter, Staub, Rado, & O'Connor, 2002).

perceptual features. This speculation is based on a number of observations. With respect to WM, it is well known that items have to be encoded categorically for adults to have non-trivial WM capacities (e.g., Olsson & Poom, 2005). Likewise, in infants and children, visually identical objects are memorized *as if* they belong to different categories if they are given distinct verbal labels (e.g., Feigenson & Halberda, 2008). Given the close two-way relationship between verbal labels and categories (e.g., Balaban & Waxman, 1997; Markman, 1994; Waxman & Markow, 1995), these results suggest that category membership might be critical to WM. This view is also consistent with recent findings suggesting that memory items are stored in those brain regions where they are processed to begin with (e.g., Lee, Kravitz, & Baker, 2013; Riggall & Postle, 2012; Sreenivasan, Vytlacil, & D'Esposito, 2014; Sreenivasan, Curtis, & D'Esposito, 2014). As items from different categories likely are stored in different regions, this might reduce interference among memory representations, and thus lead to better memory.

In the case of MOT, performance can be affected by simple perceptual features such as color (e.g., Erlikhman et al., 2013; Makovski & Jiang, 2009a; Störmer et al., 2011), and, more generally, by perceptual grouping (e.g., Keane et al., 2011; Scholl et al., 2001; Yantis, 1992). To the extent that colors are not perceived as categories, category structure might thus be more important for WM than for MOT. If this speculation is correct, it would suggest another way in which WM and MOT dissociate. Further research is needed to test this possibility.

Explaining earlier evidence for a general cognitive capacity

The arguments provided thus far do not imply that there is no relation between attention, small number processing and WM. In fact, data such as Piazza et al.'s (2011) correlation between the subitizing range and VWM capacity still require an explanation. We speculatively envision three such explanations. Our first account is maybe the most deflationary one. Given that MOT performance correlates with performance on tasks that, at least intuitively, have no obvious mechanistic relationship to parallel individuation (e.g., to report colors, Huang et al., 2012), it is possible that all

tasks that require processing of visual objects are linked through some common factors such as general vigilance or compliance with the task.

Our second account is based on the observation that, at least in the case of WM and MOT, there might be a common limitation that does not imply a common mechanism. Specifically, Franconeri, Alvarez, and Cavanagh (2013) suggested that, in MOT experiments, objects might interfere with each other when they pass close to each other, and that this interference limits how many objects we can track (see also Zhong, Ma, Wilson, Liu, & Flombaum, 2014, for a related model).

With respect to (visual) WM, Endress and Potter (2014) argued that proactive interference was a common feature of many WM experiments, due to the reuse the same set of limited items across many trials.[11] Accordingly, they showed that there are no clear bounds to how many items we can temporarily remember when proactive interference is minimized (though it is not clear to what extent memory is limited when participants have to encode spatial information; see e.g. Banta Lavenex et al., 2015; Makovski, 2016). Further, Endress and Szabó (in press) provided mathematical proofs that limited memory capacity estimates are essentially an inevitable consequence of the mere existence of inter-item interference. Both MOT and WM might thus be constrained by inter-item interference, though the mechanisms that are susceptible to interference likely differ across these tasks.

Our third speculative account is that WM might use a parallel attentional system to individuate items that need to be retained; however, once the memory items are individuated, WM might also be constrained by other limiting factors. That is, WM and MOT might rely on a common set of mechanisms, but also on other mechanisms that are different across the tasks. If so, it will be important to perform more targeted experiments to find out which mechanisms are shared, and which are not. Under either of these accounts, the current results suggest that WM and parallel attention may have at least partially dissociable properties, and might thus not rely on the same

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[11]For example, in Luck and Vogel's (1997) change detection experiment, participants had to remember color patches, such that 7 colors where reused across hundreds of trials. Hence, the participants faced the question to decide whether they had seen a given color in the current trial, or one of the preceding trials, leading to proactive interference.

mechanisms.

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Table 1

Experimental details of the MOT experiments.

Experiment	# Participants (of which excluded)[a]	# Blocks	# Trials/ block	# Targets	v_{min}[b]	v_{max}[b]	Task
1	25 (1)	3	36	2,4,6	1.25	7.5	yes/no
2a	24 (0)	4	30	6	4.25	10.4	click all
2b[c]	24 (1)	4	30	6	4.25	10.4	click all
3a[d]	10 (1)	4	32	6	1.25	7.5	yes/no
3b	24 (4)	4	36	2,4,6	1.25	7.5	yes/no

[a] The column gives the total number of participants and the number of excluded participants. For example, in Experiment 1, 25 participants were tested in total, and one was excluded from analysis. Participants were excluded if they performed below 50% for set-size 6.

[b] v_{min} and v_{max} are the maximum and the minimum velocity, respectively, measured in degrees of visual angle per second (dva/s).

[c] Experiments 2a and 2b differ in the familiarity of the stimuli was assessed only in Experiment 2b.

[d] In Experiment 3a, the frequency of pure and mixed trials was equated. That is, the frequency of each pure condition was half of the frequency of the mixed ones.

Table 2
Experimental details of the WM experiments.

Experiment	# Participants (of which excluded)[a]	# Blocks	# Trials block	Array sizes	Sample and test duration
4	20 (1)	4	60	2,4,6,8,10	3000
5	24 (0)	6	36	4,6,8	3000
6a[b]	10 (0)	6	40	6	3000
6b	24 (0)	6	36	4,6,8	3000
7	10 (0)	4	60	2,4,6,8,10	2000
8	10 (0)	4	60	2,4,6,8,10	4000

[a]The column gives the total number of participants and the number of excluded participants. For example, in Experiment 4, 20 participants were tested in total, and one was excluded from analysis. Participants were excluded if they performed below 50% for set-size 6.

[b]In Experiment 6a, the frequency of pure and mixed trials was equated. That is, the frequency of each pure condition was half of the frequency of the mixed ones.

Table 3

Counts of a participants with pure vs. mixed advantages in the MOT experiments

Set-size	Exp. 1			Exps. 2a & 2b			Exps. 3a & 3b		
	Pure	**None**	**Mixed**	**Pure**	**None**	**Mixed**	**Pure**	**None**	**Mixed**
2	5	14	5	NA	NA	NA	1	10	9
4	2	7	15	NA	NA	NA	5	7	8
6	15	3	6	27	1	19	12	1	16

Table 4

Counts of a participants with pure vs. mixed advantages in the WM experiments.

Set-size	Exp. 5			Exp. 6a & 6b			Exp. 7			Exp. 8		
	Pure	None	Mixed	Pure	None	Mixed	Pure	None	Mixed	Pure	None	Mixed
2	3	6	10	NA	NA	NA	3	3	4	5	4	1
4	8	1	10	6	2	16	5	1	4	5	0	5
6	6	3	10	13	2	19	4	2	4	2	1	7
8	10	0	9	10	3	11	8	0	2	5	2	3
10	10	0	9	NA	NA	NA	3	0	7	7	0	3

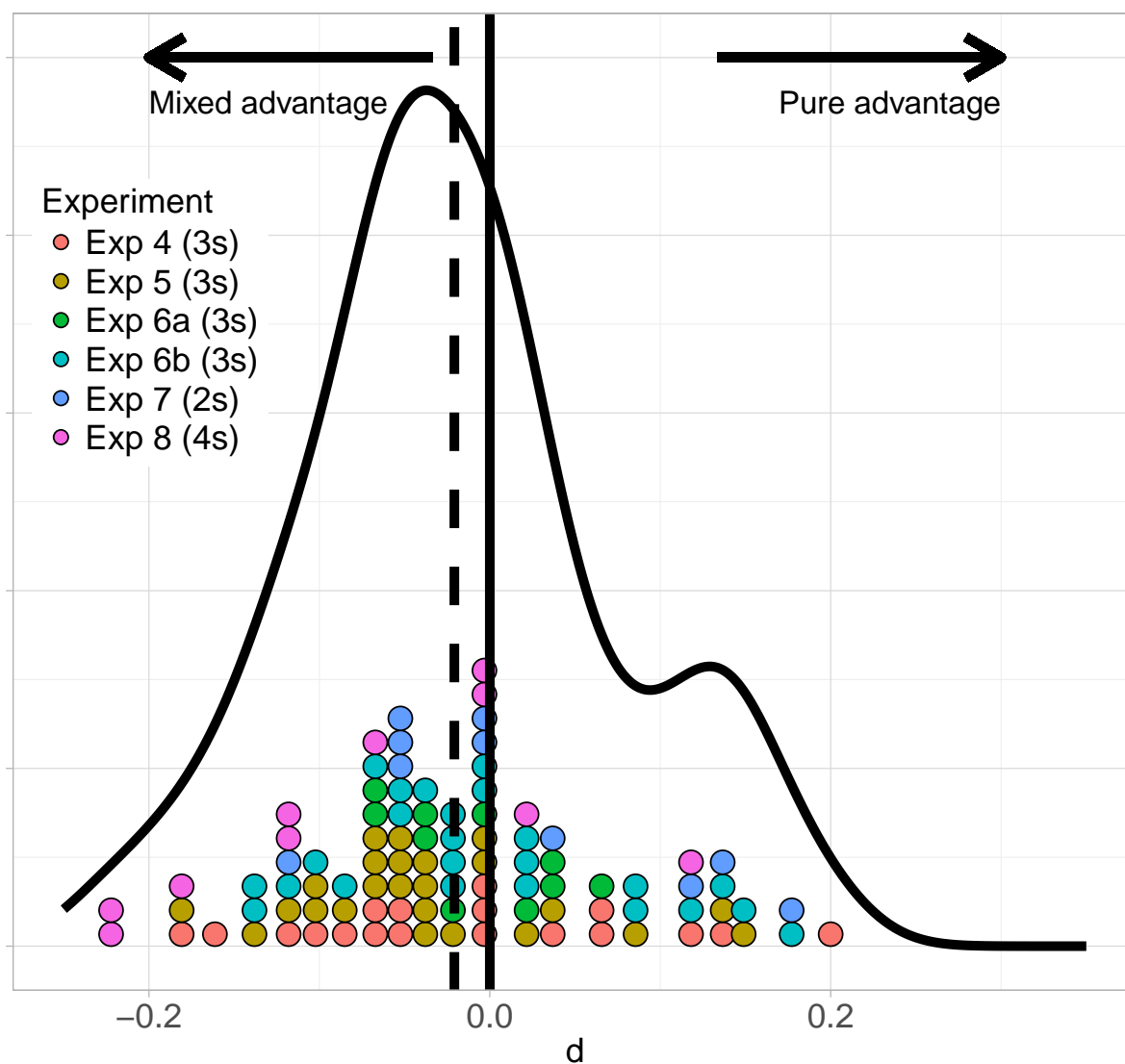*Figure 1*. Distribution of individual difference scores (pure-mixed)/(pure+mixed) across the different multiple object tracking experiments. Positive difference scores indicate a processing advantage for pure (i.e., car **or** face) displays, while negative difference scores indicate an advantage for mixed (car **and** face) displays. Circles represent individual participants; the solid line represents the density estimate with a Gaussian kernel and a bandwidth of .01615 (as determined by the Sheather-Jones method). Across experiments, participants had a small pure advantage.
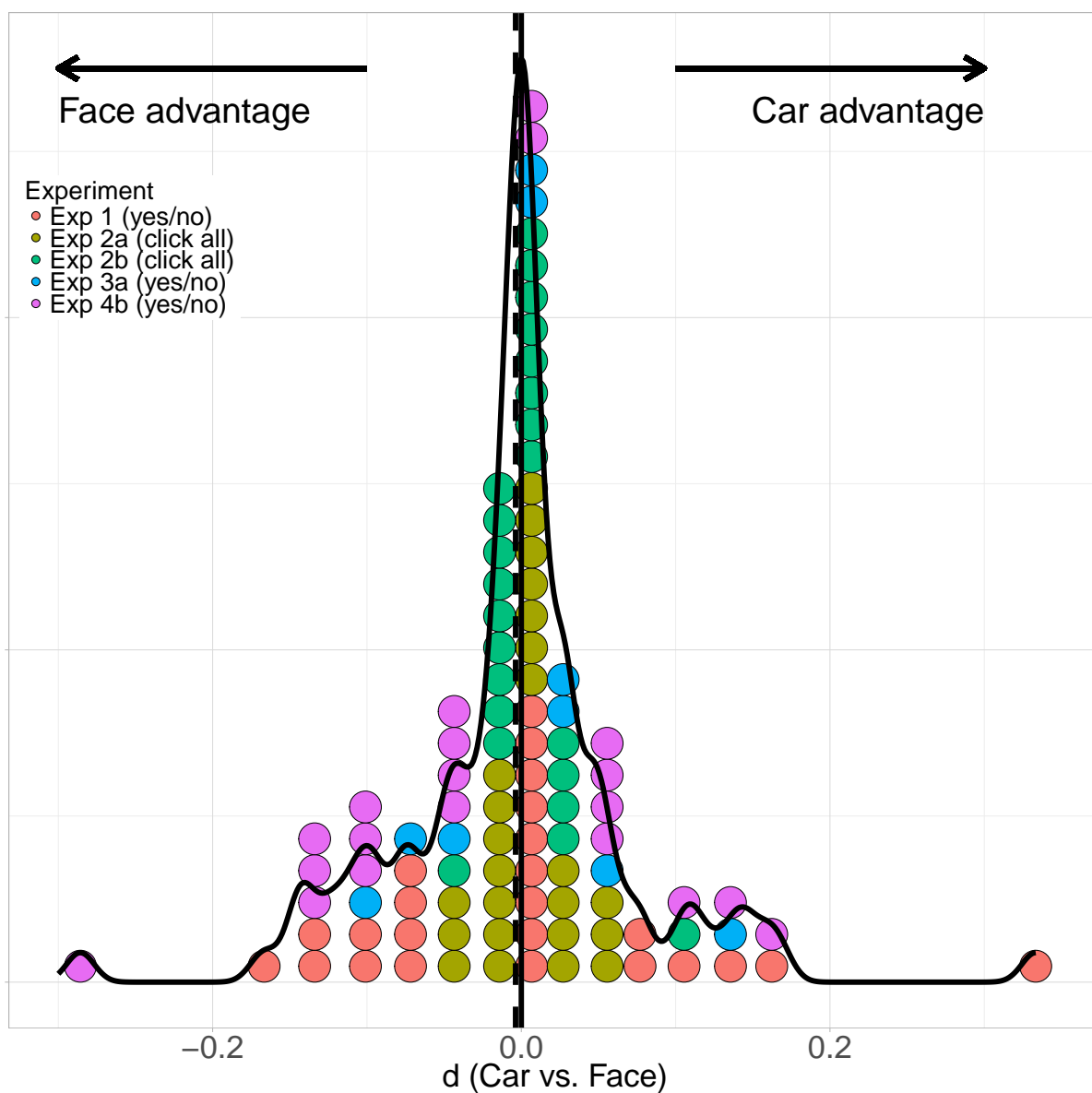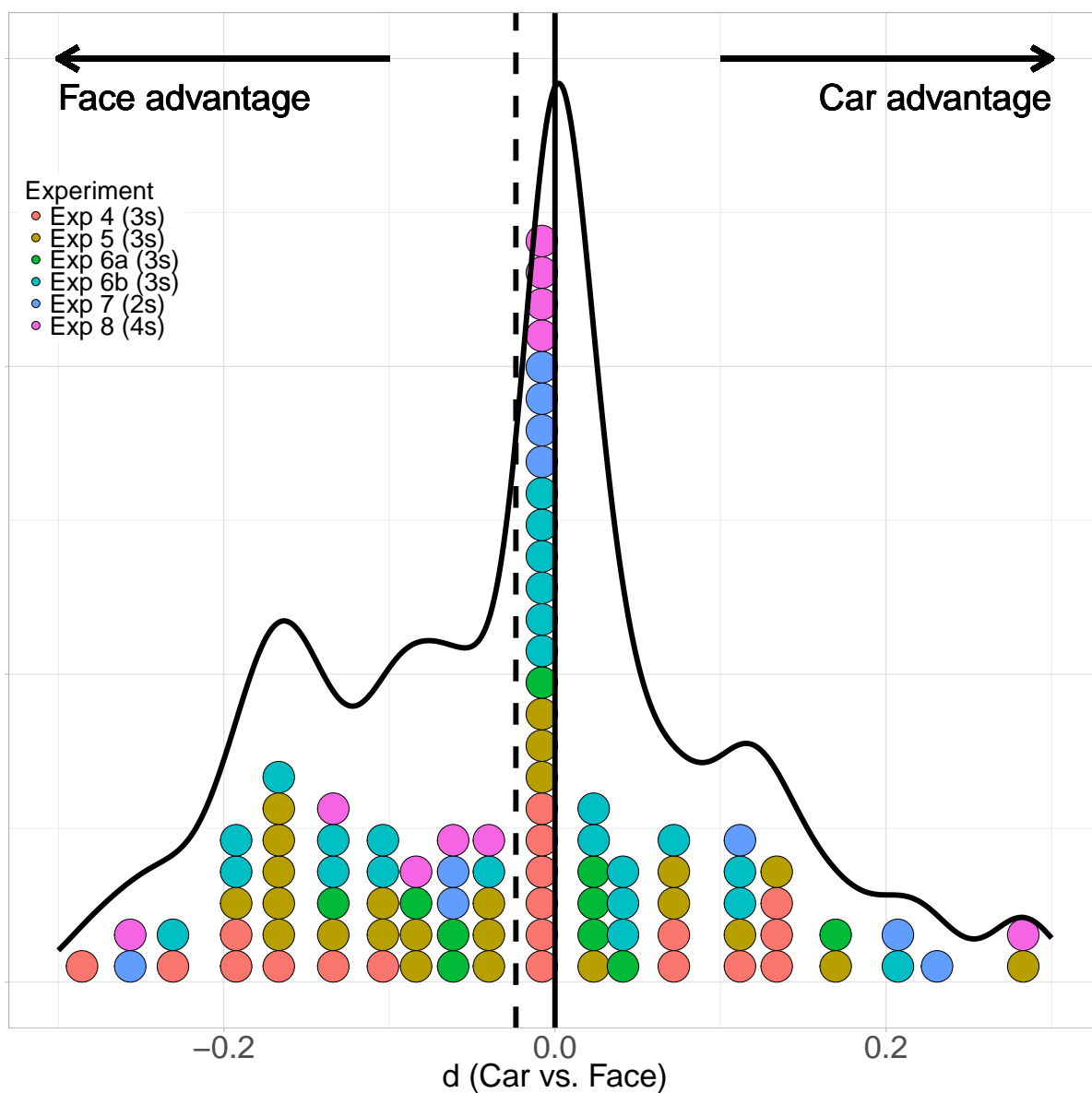
Figure 2. Distribution of individual difference scores (car-face)/(car+face) across the different multiple object tracking experiments. Positive difference scores indicate a processing advantage for car displays, while negative difference scores indicate an advantage for face displays. Circles represent individual participants; the solid line represents the density estimate with a Gaussian kernel and a bandwidth of .00895 (as determined by the Sheather-Jones method). Across experiments, participants showed no advantage in either direction.

Figure 3. Distribution of individual difference scores (pure-mixed)/(pure+mixed) across the different change detection experiments. Positive difference scores indicate a processing advantage for pure (i.e., car **or** face) displays, while negative difference scores indicate an advantage for mixed (car **and** face) displays. Circles represent individual participants; the solid line represents the density estimate with a Gaussian kernel and a bandwidth of .0293 (as determined by the Sheather-Jones method). Across experiments, participants had a small mixed advantage.
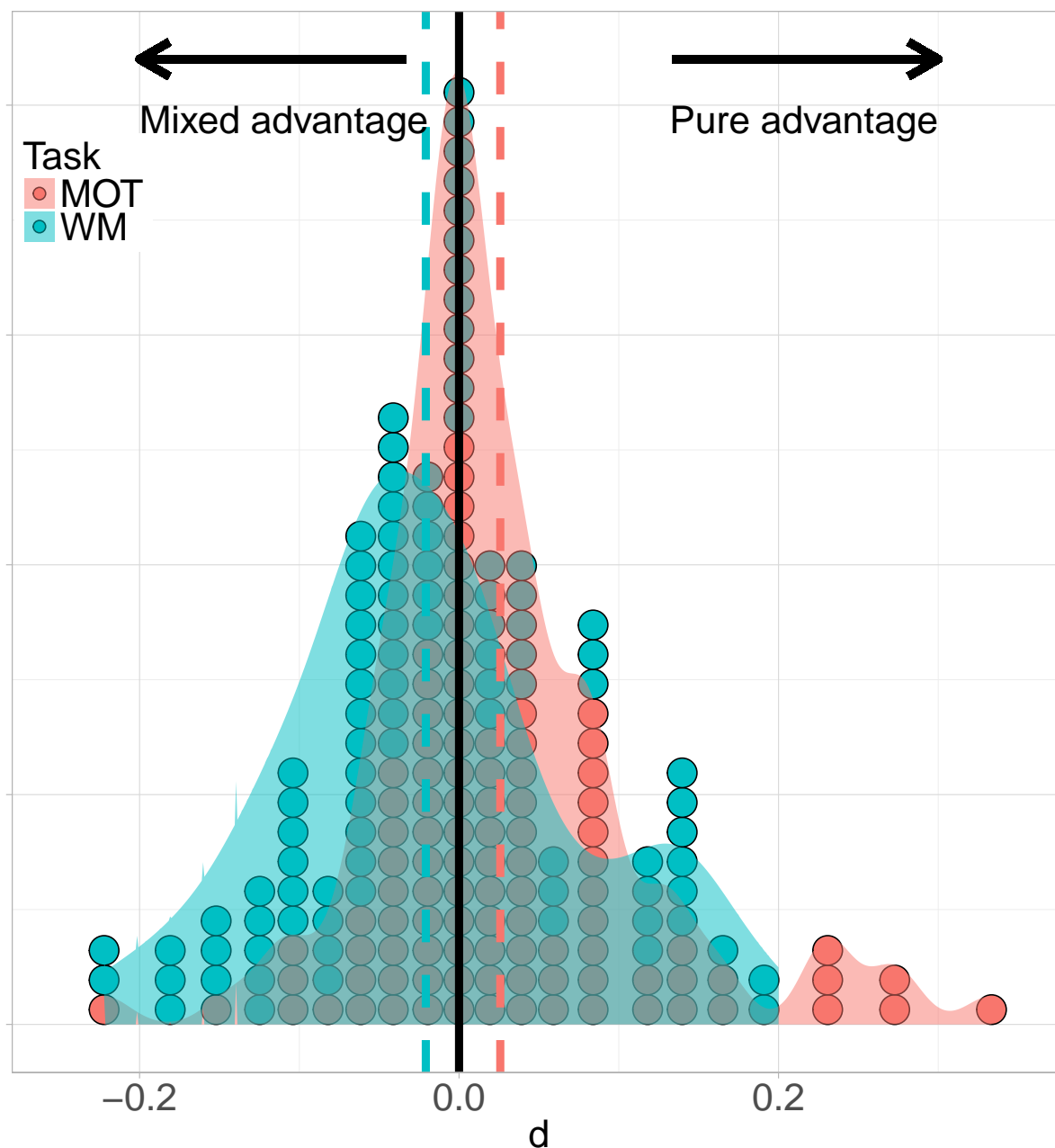
Figure 4. Distribution of individual difference scores (car-face)/(car+face) across the different change detection experiments. Positive difference scores indicate a processing advantage for car displays, while negative difference scores indicate an advantage for face displays. Circles represent individual participants; the solid line represents the density estimate with a Gaussian kernel and a bandwidth of .01966 (as determined by the Sheather-Jones method). Across experiments, participants had a small face advantage.

Figure 5. Distribution of individual difference scores (pure-mixed)/(pure+mixed) across multiple object tracking (red) and working memory (turquoise) experiments. Positive difference scores indicate a processing advantage for pure (i.e., car **or** face) displays, while negative difference scores indicate an advantage for mixed (car **and** face) displays. Circles represent individual participants; the shaded areas represent the corresponding density estimates. Across experiments, the pure advantage was larger for MOT experiments than for WM experiments, indicating the MOT experiments showed a small pure advantage while WM experiments showed a small mixed advantage.
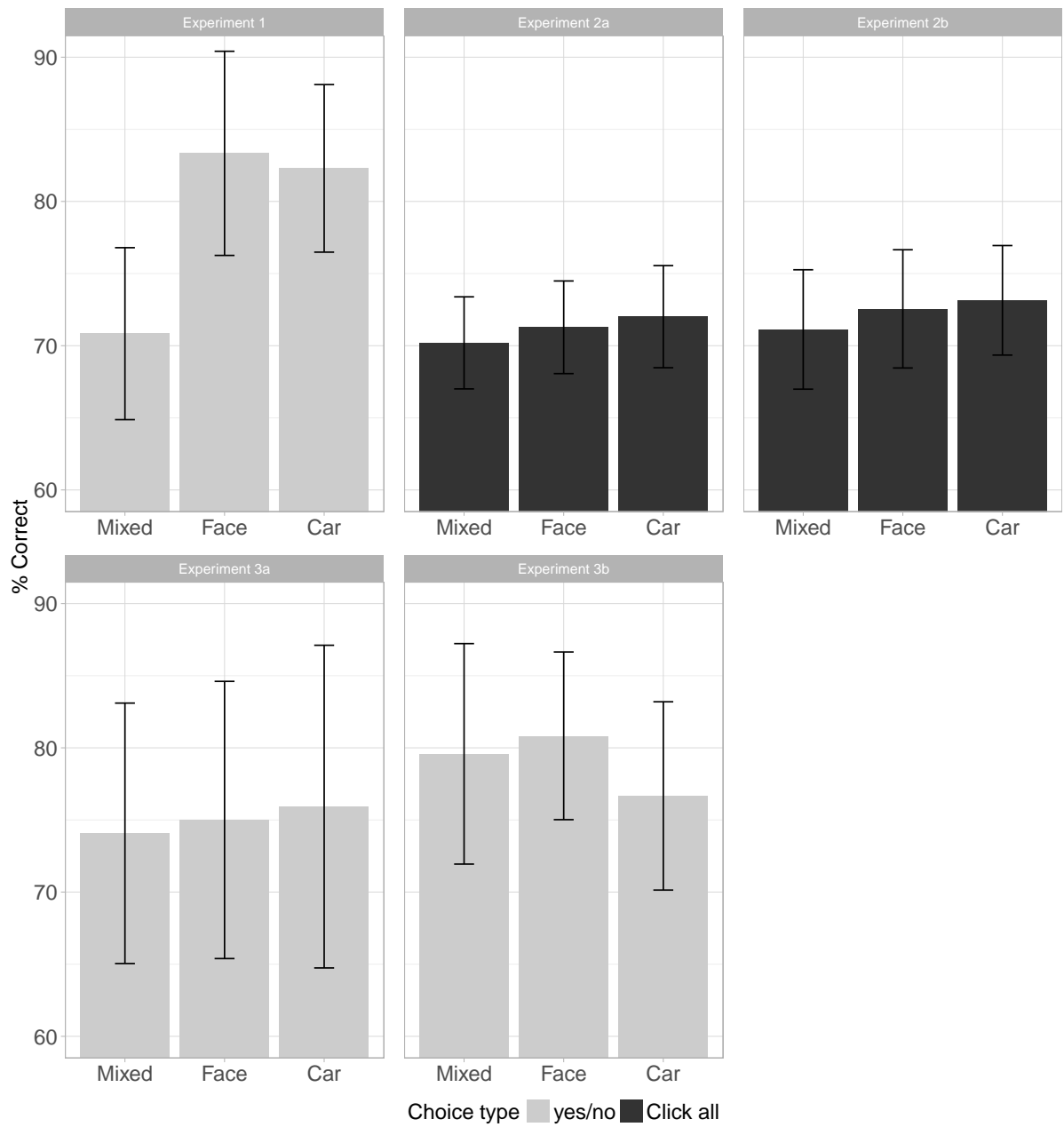
Figure 6. Performance in terms of the percentage of correct responses in the MOT experiments. Error bars show 95% confidence intervals.
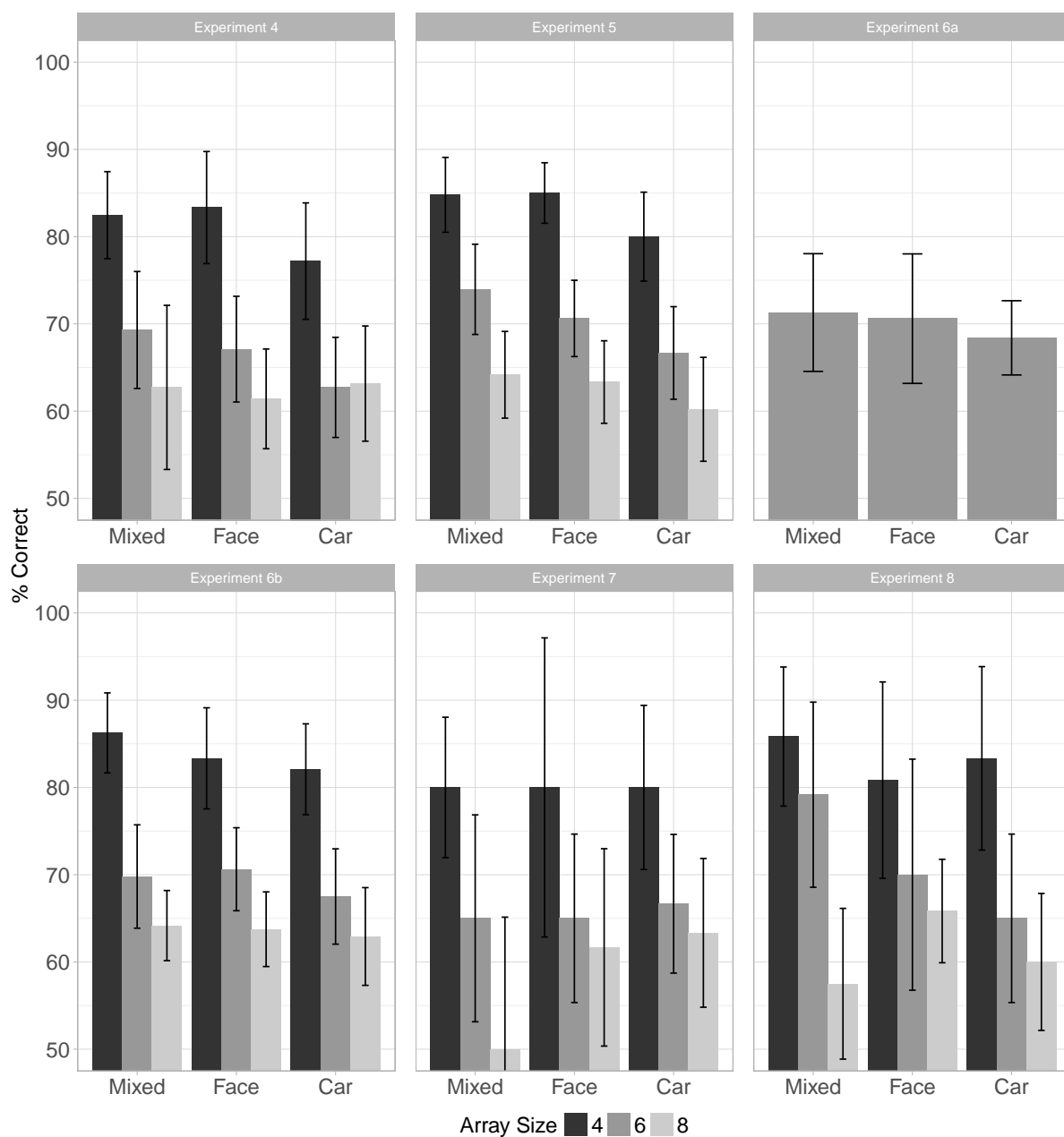
Figure 7. Performance in terms of the percentage of correct responses in the WM experiments. Error bars show 95% confidence intervals.

Appendix

Multiple object tracking algorithm

```
// The code below shows the algorithm for object j


// The objects current 2D velocities initially define the
    ↪ displacements in the x and y direction
// The vector CurrentObjectVelocities stores the norms of the
    ↪ velocities (i.e., their magnitudes)
// of all objects.
// Initially, all objects are assigned random velocities. At
    ↪ each time step, velocities change to another
// random value with probability .1.


double MaxVelocity=10.4;


DeltaX[j,i]=CurrentObjectVelocities[j]*Math.Cos(
    ↪ CurrentObjectDirections[j]*Math.PI/180.0);


DeltaY[j,i]=CurrentObjectVelocities[j]*Math.Sin(
    ↪ CurrentObjectDirections[j]*Math.PI/180.0);



// Next, the wall repulsion components are added


double LeftScreenEdgeRepulsionMagnitude=Math.Exp(−0.5*
    ↪ NormDistanceToLeftScreenEdge/WallRepulsionSigma);


double RightScreenEdgeRepulsionMagnitude=Math.Exp(−0.5*
    ↪ NormDistanceToRightScreenEdge/WallRepulsionSigma);
```

```
double TopScreenEdgeRepulsionMagnitude=Math.Exp(-0.5*
    ↪ NormDistanceToTopScreenEdge/WallRepulsionSigma);


double BottomScreenEdgeRepulsionMagnitude=Math.Exp(-0.5*
    ↪ NormDistanceToBottomScreenEdge/WallRepulsionSigma);



DeltaX[j,i]=DeltaX[j,i]+MaxVelocity*(Math.Cos(0)*
    ↪ LeftScreenEdgeRepulsionMagnitude+Math.Cos(Math.PI)*
    ↪ RightScreenEdgeRepulsionMagnitude);


DeltaY[j,i]=DeltaY[j,i]+MaxVelocity*(Math.Sin(Math.PI/2.0)*
    ↪ TopScreenEdgeRepulsionMagnitude+Math.Sin(3.0*Math.PI/2.0)*
    ↪ BottomScreenEdgeRepulsionMagnitude);




// Finally, the mutual repulsion components among objects are
    ↪ added


For (int jj=0;jj<ObjectNumber;jj++)


{


// Object j doesn't interact with itself
If (jj==j) continue;

...
```

```
            DeltaX [ j , i]=DeltaX [ j , i]−MaxVelocity ∗Math . Exp(−0.5∗
                ↪  NormTwoObjectDistance / ObjectRepulsionSigma ) ∗Math
                ↪  . Cos ( Theta ) ;


DeltaY [ j , i]=DeltaY [ j , i]−MaxVelocity ∗Math . Exp(−0.5∗
    ↪  NormTwoObjectDistance / ObjectRepulsionSigma ) ∗Math . Sin ( Theta
    ↪  ) ;


. . .


}
```