How we learn to ignore singleton distractors: Suppressing saliency signals or specific features?

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Abstract

Salient visual information can sometimes capture attention despite our goals, however, there are several ways we can minimize or eliminate such distraction. One such way is learned distractor rejection, in which we increasingly ignore salient, irrelevant distractors across repeated exposures. Here we probe the mechanism underlying this learned rejection. What must be learned about the distractor to promote effective ignoring? Specifically, is feature rejection, alone, sufficient to learn rejection of salient distractors, or do the items' saliency signals need to also be rejected? To test between these possibilities, we used a modified version of the learned distraction rejection paradigm (Vatterott & Vecera, 2012). Participants viewed training blocks containing either a salient singleton distractor, or a set of three non-salient "tripleton" distractors, followed by test blocks in which the distractor was always a salient color singleton. Critically, the distractors in test blocks always shared a feature (color) with the corresponding training blocks. By comparing attentional capture in the test blocks as a function of the preceding training block we were able to observe whether experience with saliency was necessary for learned distractor rejection. Results revealed unexpected difficulties in replicating learned distractor rejection, suggesting the true effect size may be smaller than initially reported. With respect to our main objective, we found no difference in the rejection of test block distractors based on whether participants had viewed salient or non-salient distractors during training. That is, we found similar attenuation of singleton presence costs in the test blocks regardless of whether they followed singleton or tripleton training blocks. These results show that experience in rejecting saliency signals is not a requirement of learned distractor rejection.

Keywords: visual attention, attentional capture, distractor rejection, learning

Introduction

Successfully navigating the visual environment requires that we contend with a seemingly limitless quantity of potentially distracting information. Sometimes, we involuntarily select this information, or experience *attentional capture,* which interferes with the processing of task-relevant information. Our behavioral goals demand that we minimize attentional capture as much as possible; fortunately, the human mind is equipped with mechanisms to avoid such distraction. While the exact nature of these mechanisms is the subject of healthy debate, there is growing consensus that people are highly adept at learning to ignore distractors that have some sort of consistent property, such as feature or spatial location (see Luck et al., 2021, for recent discussion). Here, we take a closer look at one such learning phenomenon, specifically a situation where the distractors have repeated feature information, in what has been referred to as *learned distractor rejection* (Vatterott & Vecera, 2012). Our present goal is to better understand how such rejection is implemented.

To begin with some background, attentional capture is often investigated using the *additional singleton paradigm* (Theeuwes, 1992). In this task participants perform a visual search for a uniquely shaped item as the target. To elicit distraction, a color singleton – an item unique in color – is presented on a subset of trials. Typically, the inclusion of this color singleton in the search arrays results in a singleton presence cost, that is, a slowing of response times to find the target when the distractor singleton was presented compared to when it was absent from the display (Theeuwes, 1991, 1992, 2004). These singleton presence costs are often interpreted as attentional capture by the color singleton distractor and have been argued via *stimulus-driven* accounts of attentional capture to occur because of the distractor's salience (Theeuwes, 1991, 1992, 2004, 2010; Wang & Theeuwes, 2020).

However, alternative accounts posit a more *goal-driven* explanation for the observed attentional capture. Bacon and Egeth (1994) argued that observers often experience distraction because they adopt a *singleton detection mode,* a strategy that prioritizes all salient search items (see also Pashler, 1988). In a seminal study, Bacon and Egeth (1994) manipulated search displays to render singleton detection mode a non-viable strategy; under these conditions, observers would presumably need to adopt a *feature search mode,* prioritizing the specific target-defining feature. They hypothesized that because the singleton distractor did not match sought after target features it would no longer garner any attentional priority when participants used a feature-based search strategy. Consistent with this prediction, singleton presence costs were eliminated due to Bacon and Egeth's manipulations (see also Burra & Kerzel, 2013; Lamy & Egeth, 2003; Leber & Egeth, 2006).

As initially conceived by Bacon and Egeth (1994), adopting a feature search strategy should be sufficient to allow observers to avoid distraction by salient, irrelevant distractors. However, more recent research has questioned this assumption, proposing that, as an additional requirement, observers must gain experience in ignoring the specific distractor (De Tommaso & Turatto, 2019; Gaspelin & Luck, 2018b; Stilwell et al., 2019; Vatterott et al., 2018; Vatterott & Vecera, 2012; Zehetleitner et al., 2012).

This point was illustrated by Vatterott and Vecera (2012), who introduced the learned distractor rejection paradigm. Much like the additional singleton paradigm, participants search for a consistent shape target, while the presence of a color singleton distractor, appearing in a non-target location, is manipulated. Importantly, heterogeneous distractor shapes are used to elicit a feature search mode for the target. The critical modification of this paradigm is that the singleton distractor color is updated at the start of every block. For example, across four consecutive blocks, a participant might be presented with red, magenta, yellow, and orange distractor singletons, respectively. This design allowed Vatterott and Vecera (2012) to observe the time course of learned distractor rejection over the duration of each block, by comparing singleton presence costs in the first half vs. the second half of each block. Results showed an attenuation of singleton presence costs in each block. Importantly, significant attentional capture was found at the beginning of blocks, indicating that learned rejection does not transfer to novel distractor colors. This result may seem to contradict the findings of Bacon and Egeth (1994), who should have observed significant distractor presence effects at the beginning of the experiment. However, averaging condition means across the entire experiment likely made it difficult to discern the relatively short-lived capture effect in the earlier study.

Following Vatterott and Vecera's (2012) findings, several other studies on learned rejection have provided ample evidence that individuals first experience distraction but efficiently learn to reject salient distractors while their color remains consistent (De Tommaso & Turatto, 2019; Gaspelin & Luck, 2018b; Stilwell et al., 2019; Vatterott et al., 2018). Notably, most of these studies used only salient color singletons. In these cases, while the rejection of the singleton distractors is clearly reliant on specific color values, it may also be simultaneously reliant on the learning to handle the salience of the distractors.

Here we ask if observers can learn to ignore salient distractors without experiencing salience. One related line of research might appear to shed some light on this question. Several studies have shown that learned distractor rejection can act on non-salient distractors, presumably rejecting them based only on experience with specific feature information (Lien et al., 2022; Stilwell & Vecera, 2019, 2022b, 2022a; Won & Geng, 2018). For example, Stilwell & Vecera (2019) observed that, following a sufficient learning period, participants were faster to locate a target in the presence of a group of learned-to-be-irrelevant distractors. That is, when a set of the items in the display (3 out of 8 in this case) were rendered in an irrelevant color, participants were likely able to search through fewer total items, therefore, locating the target more quickly. This benefit due to the presence of a specific color in the display was taken to indicate that participants had learned to reject those items based on the irrelevance of their color (i.e., the target was never this color).

We have, therefore, seen that learned distractor rejection can act on both salient and non-salient distractors, and that feature information likely plays an important role in both cases. That is, as shown by Vatterott and Vecera (2012), switching the specific feature value (i.e., color) of the distractor resulted in a rebound of attentional capture and required the subsequent re-learning of distractor rejection. On the other hand, work with non-salient distractors has provided evidence for rejection of said distractors when little else was consistent about them. Critically, however, the extant literature has yet to examine whether training with non-salient feature distractors is sufficient for individuals to subsequently reject salient feature distractors.

Whether individuals can learn to ignore salient distractors without experience salience carries significant theoretical implications. Classic goal-driven accounts, such as the contingent involuntary orienting hypothesis (Folk, Remington & Johnston, 1992; Folk, Leber & Egeth, 2002), have held that attentional capture is determined primarily by the match between a distracting stimulus and one's attentional control setting. It has been further shown that non-salient distractors indeed capture attention when they match one's attentional control setting, although increasing the salience of these distractors does modulate capture effects (Lamy, Leber & Egeth, 2004). Stimulus-driven accounts of attentional capture, as mentioned above, place much greater emphasis on salience, arguing that that color singletons automatically capture attention strictly because of their saliency (Theeuwes 1992, 2004; see also Theeuwes 2010). Sawaki and Luck (2010) introduced an account that is neither strictly goal-driven nor salience-driven, arguing that capture can be avoided, but doing so requires an act of suppression. By this account, singletons automatically generate an "attend-to-me signal," which can then be suppressed, as evidenced by examining electrophysiological measures of attentional orienting (N2pc) and distractor suppression (Pd). Building on this finding, the *signal suppression hypothesis* originally proposed that effective rejection of a salient distractor relies on the suppression of said distractor's saliency signal (Gaspelin et al., 2015; Sawaki & Luck, 2010). Despite recent updates to the account, which put greater emphasis on knowledge of distractor features (Gaspelin & Luck, 2018b, 2019), a theoretical process of suppressing saliency signals remains a potential way in which individuals handle distractors. In short, many accounts of attentional capture emphasize the importance of rejecting salient distractors (but see Lien et al., 2022). However, it is still unclear how salient distractors come to be ignored.

One possibility is that, when the to-be-ignored distractor is salient, it may be harder to ignore, potentially due to its purported attentional-capturing-capabilities, and it therefore requires additional experience with, and subsequent rejection of, the generated saliency signals. Alternatively, the rejection of salient distractors may be purely based on consistent distractor features, similar to what occurs when observers learn to reject a group of non-salient items sharing a color. To put it simply, in the case of a salient distractor, it remains unclear what role the salience itself plays in learned distractor rejection.

In the current study, we used the learned distractor rejection paradigm (Vatterott & Vecera, 2012) to investigate whether experience with a salient distractor's simple features is sufficient to learn to reject it. However, unlike the singleton distractors used in previous studies, we instead used displays in which the salience of the target and distractor colors were equated. We then tested how this rejection would hold up when the distractors suddenly became more salient. If learning to reject saliency signals is necessary when learning to ignore a salient distractor, then we should observe attentional capture when the ignored distractors suddenly become salient. However, if learned feature-based rejection alone is sufficient for rejection of salient distractors, then said rejection should survive the introduction of salience.

Experiment 1

In Experiment 1 we sought to test whether experience with consistent distractor features (color) will allow for effective distractor rejection when the critical distractor suddenly becomes salient. Our approach was to present pairs of training and test blocks to participants while manipulating the salience of non-target-colored distractors in the training blocks. Participants viewed either *singleton training blocks* in which a color singleton was presented in a constant distractor color throughout the block, or *tripleton training blocks* in which half of the distractor items were presented in the non-target-color, rendering them non-salient (see Lien et al., 2022). After training, both groups were exposed to the same exact test blocks, in which the distractor was always a salient color singleton, which was always the same color as in the training blocks.

If effective rejection of a salient distractor requires learned rejection of saliency signals, and therefore, experience with salience, then greater singleton presence costs will be observed in test blocks following tripleton (non-salient) training blocks. This will appear as notable slowing of RTs on singleton present, compared to singleton absent, trials in these blocks as opposed to the minimal difference expected in test blocks following singleton (salient) training (Vatterott and Vecera, 2012). If, however, experience with the distractor feature is sufficient to learn rejection of a salient distractor, singleton presence costs should be equal across training conditions. That is, RT differences between singleton present and singleton absent trials in test blocks will not differ as a function of the corresponding training block.

Method

Participants

50 participants were recruited via the Research Experience Program (REP) at The Ohio State University (19 women, 30 men, 1 non-binary; mean age = 18.8 years). Participants received course credit as compensation. All participants were required to have normal color vision and normal or corrected to normal visual acuity. All experimental procedures were approved by The Ohio State University's Institutional Review Board.

Apparatus

All stimuli were coded and presented using PsychToolbox 3 (Brainard, 1997; Kleiner et al., 2007; Pelli, 1997). Participants completed the experiment in a dimly lit, sound-attenuated room. Head position was not fixed, and we report stimulus sizes based on a typical viewing distance of 70 cm. All stimuli were displayed on a 24'' Dell G2422HS monitor (1920 x 1080 resolution).

Stimuli

All stimuli were presented on a black background. A white fixation cross (comprised of two intersecting perpendicular lines, each .4° by .1° visual angle) was presented in the center of the screen throughout the experiment. Search arrays consisted of six shapes evenly spaced on an imaginary circle centered at fixation (each shape was centered at an eccentricity of 3.7°). The target was always a green outline circle (RGB 0, 255, 0; .1° line thickness; 2.1° diameter), which appeared equally often at each of the six locations, with presentation order randomized within each block. The remaining five non-target shapes were made up of outline squares (1.8° by 1.8°), diamonds (1.8° by 1.8°), and triangles (2.5° on a side) which were randomly chosen with the constraint that a maximum of two distractors could appear as the same shape.

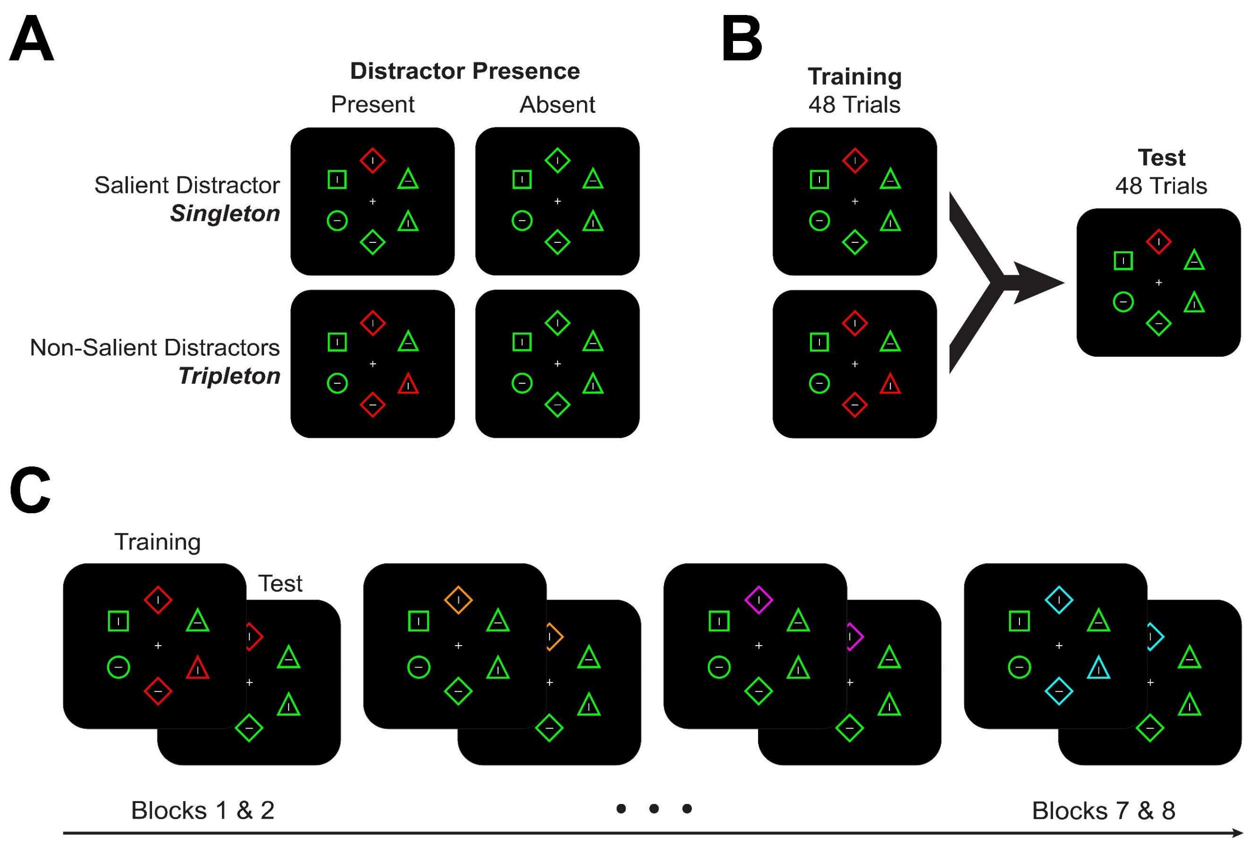

Figure 1. (A) Types of search displays used in task. Singleton present trials contained a uniquely colored distractor item. On tripleton present trials, three distractor items were rendered in the distractor color. (B) Training-test procedure. Participants viewed training blocks containing either singleton or tripleton distractors. Test blocks contained only singleton distractors. (C) Example block procedure. Participants completed four training-test block pairs. At the start of each training block the distractor color was switched. Ordering of training block types and distractor color were counterbalanced across participants.

On distractor absent trials, all search items were rendered in the target color (e.g., all green). On distractor present trials one or three non-target shapes were presented in one of four distractor colors, red (RGB 255, 0, 0), orange (RGB 255, 150, 0), magenta (RGB 255, 0, 255), or cyan (RGB 0, 255, 255). The locations of these color distractors were randomly chosen on each trial, with the constraint that they could never appear at the target location (Figure 1A).

Procedure

Participants completed an initial practice block, followed eight experimental blocks. The practice block consisted of 48 trials, in which all search stimuli were target colored. The purpose of this block was to familiarize participants with the visual search task in order to minimize errors during experimental blocks. The practice block was excluded from all analyses. The following eight experimental blocks were divided into four pairs of training and test blocks in which color distractors were presented on half of all trials. Training blocks were either singleton- or tripleton- distractor blocks and were always immediately followed by a singleton distractor test block. Each participant completed two tripleton-training block pairs and two singleton-training block pairs (Figure 1C); with ordering counterbalanced in an ABBA/BAAB pattern across participants. Color distractors were presented on 50% of all trials during experimental blocks, 24 out of the total 48 trials in each block.

During singleton training blocks, distractor present trials were comprised of one non-target shape rendered in the opposite color to the target, while all others appeared in the target color, creating a color singleton. On distractor present trials during tripleton training blocks, a tripleton color-distractor was presented. That is, three out of the five non-target search items were rendered in the distractor color, resulting in an equal split of target and distractor-colored items in the display. In other words, half of the search items appeared in the target color (green), while the other half of items appeared in the distractor color (e.g., red). Test blocks were always singleton-distractor blocks, identical to the training blocks of the same name, described above, and shared a distractor color with the corresponding training block (Figure 1B).

Distractor color was consistent throughout a training-test block pair but changed at the beginning of each training block. The ordering of distractor color was counterbalanced across participants using a balanced Latin square, such that each color occurred once at each position (1-4) and each ordering of two colors occurred once in the counterbalancing order.

Each trial began with a white fixation cross presented in the center of the screen for 500 ms. The fixation display was followed by the onset of the search array. Participants searched for the target shape and responded based on whether a white line segment (.7° by .1° visual angle) presented at the center of the shape was horizontal or vertical. After each search trial, auditory feedback was given in the form of low tone to indicate an incorrect response. Following trials on which participants failed to make a response the text “too slow” was presented in the center of the screen in addition to the sounding of the low tone. Manual response times (RTs) and accuracy were recorded for each trial. Additional feedback was on average response time and accuracy was given following each block, and participants were instructed to take a short break before continuing.

Prior to the start of the experiment participants were informed of the specific features of the target, a green circle, and were instructed to do try to ignore any shapes rendered in an alternate color. No instructions were given concerning the specific distractor colors, or number of color-distractor items, therefore, throughout the experiment participants were unaware of distractor color of an upcoming block, nor whether they would view a tripleton or singleton block.

Results and Discussion

2 out of the 50 participants collected were removed from all analyses due to accuracy or response times outside the range of 3 standard deviations from overall participant averages. One additional participant was removed for reporting non-normal or corrected to normal visual acuity or color vision. Final analyses were conducted on the 47 remaining participants.

Response Times

Trials on which an error was made, or no response was given were excluded from analyses. Additionally individual subject condition means and standard deviations were calculated based on singleton presence (present vs. absent) and block type (training vs. test). RTs outside the relevant three standard deviation range were removed from all analyses. In total, 5.7% of trials were excluded based on these criteria. Multiple comparisons were corrected for using the Holm-Bonferroni procedure (Holm, 1979), where appropriate.

***Training Blocks*.** We first carried out a manipulation check of the learned distractor rejection effect described by Vatterott and Vecera (2012). We focused on the singleton training blocks, as they contained singleton distractor blocks following singleton distractor blocks of a different color (i.e., the test block of the preceding block pair). To verify an attenuation of attentional capture in these blocks, we collapsed across all singleton training blocks, then performed a paired samples *t* test comparing singleton presence effects (present – absent) in the first half of the blocks (41 ms) to those in the second half (10 ms), $t(46) = 3.16$, $p = .003$, $d_z = .46$. The results indicated a successful replication, with higher initial singleton presence costs, followed by an attenuation of these costs later in the singleton training block, RTs shown in Figure 2A. We

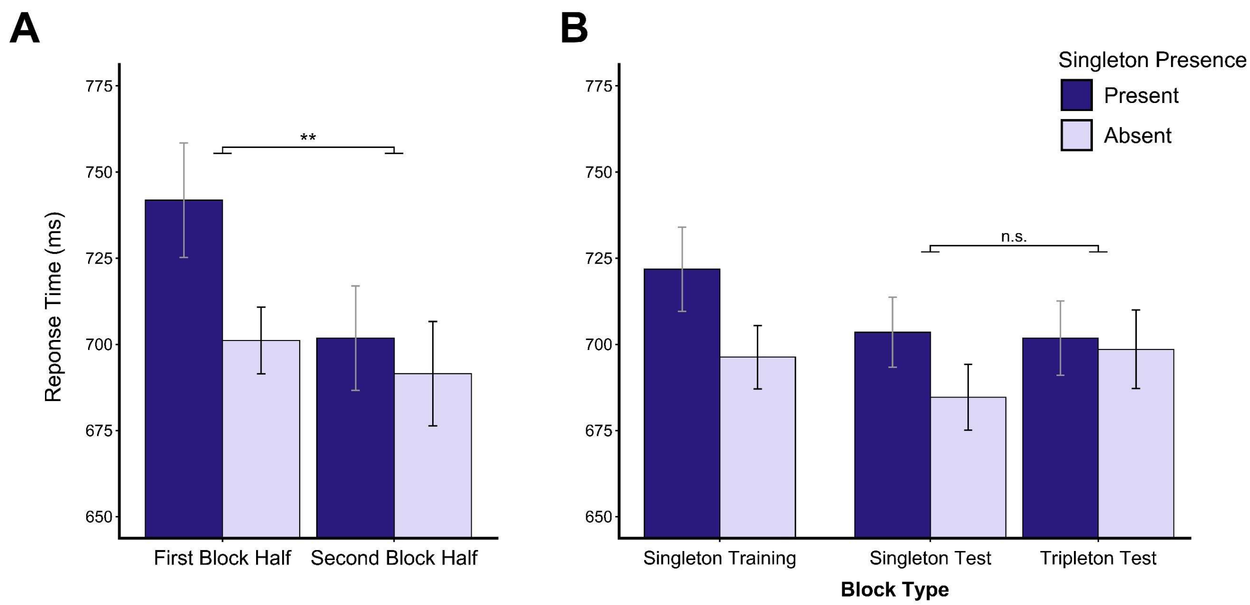

Figure 2. (A) Mean RTs in collapsed across all singleton training blocks, displayed as a function of singleton presence and block half. (B) Mean RTs by block type and singleton presence. **Note that all data presented are from testing blocks and are labeled based on the preceding training block.** Error bars represent within-subjects 95% confidence intervals (Cousineau, 2005; Morey, 2008).

additionally conducted two planned comparisons, consisting of paired *t* tests on each block half separately, comparing RTs on singleton present and absent trials. A significant difference was observed in the first, $t(46) = 4.62$, $p < .001$, $d_z = .67$, but not the second block half, $t(46) = 1.31$, $p = .197$, $d_z = .19$, indicating that that attentional capture observed in the first half of the collapsed singleton training blocks was statistically negligible in the second half of the block. Singleton presence effects in this analysis were collapsed across all blocks, but an exploratory analysis of block-by-block effects is discussed later.

Test Blocks. We then proceeded to assess attentional capture in our critical blocks by comparing across all block types containing singleton distractors. In addition to our singleton and tripleton test block conditions, we included singleton training blocks as a control condition, serving as a baseline level of attentional capture expected without learned rejection from the previous block. Note that, unlike the analysis described above, RT means in the following analyses were computed across entire blocks, not block halves. We conducted a 3 x 2 ANOVA with factors of block type (singleton training vs. singleton test vs. tripleton test) and singleton presence (present vs. absent). A significant main effect of singleton presence was observed, with slowed RTs on distractor present (709 ms) compared to distractor absent (694 ms) trials, $F(1, 46) = 12.76$, $p < .001$, $\eta_p^2 = .22$. The main effect of block type approached, but did not reach, significance, $F(2, 92) = 2.56$, $p = .083$, $\eta_p^2 = .05$. Critically, the interaction between singleton presence and block type was significant, $F(2, 92) = 3.18$, $p = .046$, $\eta_p^2 = .07$, Figure 2B.

To further investigate the significant block type by singleton presence interaction we conducted follow up simple 2 (block type) x 2 (singleton presence) ANOVAs examining the main effects of singleton presence and the interaction within each block type pairing. When data from singleton and tripleton test block conditions were entered into the 2 x 2 ANOVA, we found a significant main effect of singleton presence, $F(1, 46) = 5.17$, $p_{hb} = .028$, $\eta_p^2 = .10$., but no significant interaction, $F(1, 46) = 2.87$, $p_{hb} = .194$, $\eta_p^2 = .06$. A second ANOVA was conducted on singleton training and test blocks. We again found a significant singleton presence main effect, $F(1, 46) = 21.18$, $p_{hb} < .001$, $\eta_p^2 = .32$, but, surprisingly, no significant interaction, $F(1, 46) = 0.46$, $p_{hb} = .503$, $\eta_p^2 = .01$. Finally, we conducted a 2 x 2 ANOVA using data from singleton training and tripleton test blocks; both the main effect of singleton presence, $F(1, 46) = 6.60$, $p_{hb} = .027$, $\eta_p^2 = .13$, and the interaction effect, $F(1, 46) = 7.23$, $p_{hb} = .030$, $\eta_p^2 = .14$, were significant.

The results of our simple interactions show that we did not observe a significant difference in singleton presence costs between test blocks based on whether experience with salience was provided or not. Further, singleton presence costs were reduced when participants had previous experience with said distractor's color. Taken together, these results provide evidence for rejection of a salient distractor based on first-order features, without the need for rejection of salience.

However, one critical limitation of this experiment was the lack of a reduction in singleton presence effects between singleton training and singleton test blocks, revealed by the non-significant simple interaction test. Thus, our replication of Vatterott and Vecera (2012) may not have been as robust as we had originally assumed. This prompted us to more closely inspect the learned distractor rejection effect separately in each of the singleton training blocks. On such inspection, it appears that the attenuation of singleton presence effects only occurs in the first block and not for blocks 2-4. To evaluate this statistically, we performed an exploratory 2 x 2 x 2 ANOVA with Singleton Training Order (first vs. second singleton training block) and Block Half (first vs. second half of block) as within subjects' factors, and a between subjects' factor of Block Ordering (ABBA vs. BAAB), see Figure 3. This comparison was designed to mirror the 2 x 4 ANOVA used by Vatterott and Vecera (2012) to test the same question, albeit with some changes due to design differences. The main effects of block half, $F(1, 45) = 11.25$, $p = .002$, $\eta_p^2 = .2$, and singleton training order, $F(1, 45) = 5.68$, $p = .021$, $\eta_p^2 = .11$, were both significant. Marginal effects were found in the interaction between block half and block ordering, $F(1, 45) = 4.05$, $p = .0501$, $\eta_p^2 = .08$, as well as the interaction between all three variables, $F(1, 45) = 3.21$, $p = .080$, $\eta_p^2 = .07$. All other comparisons

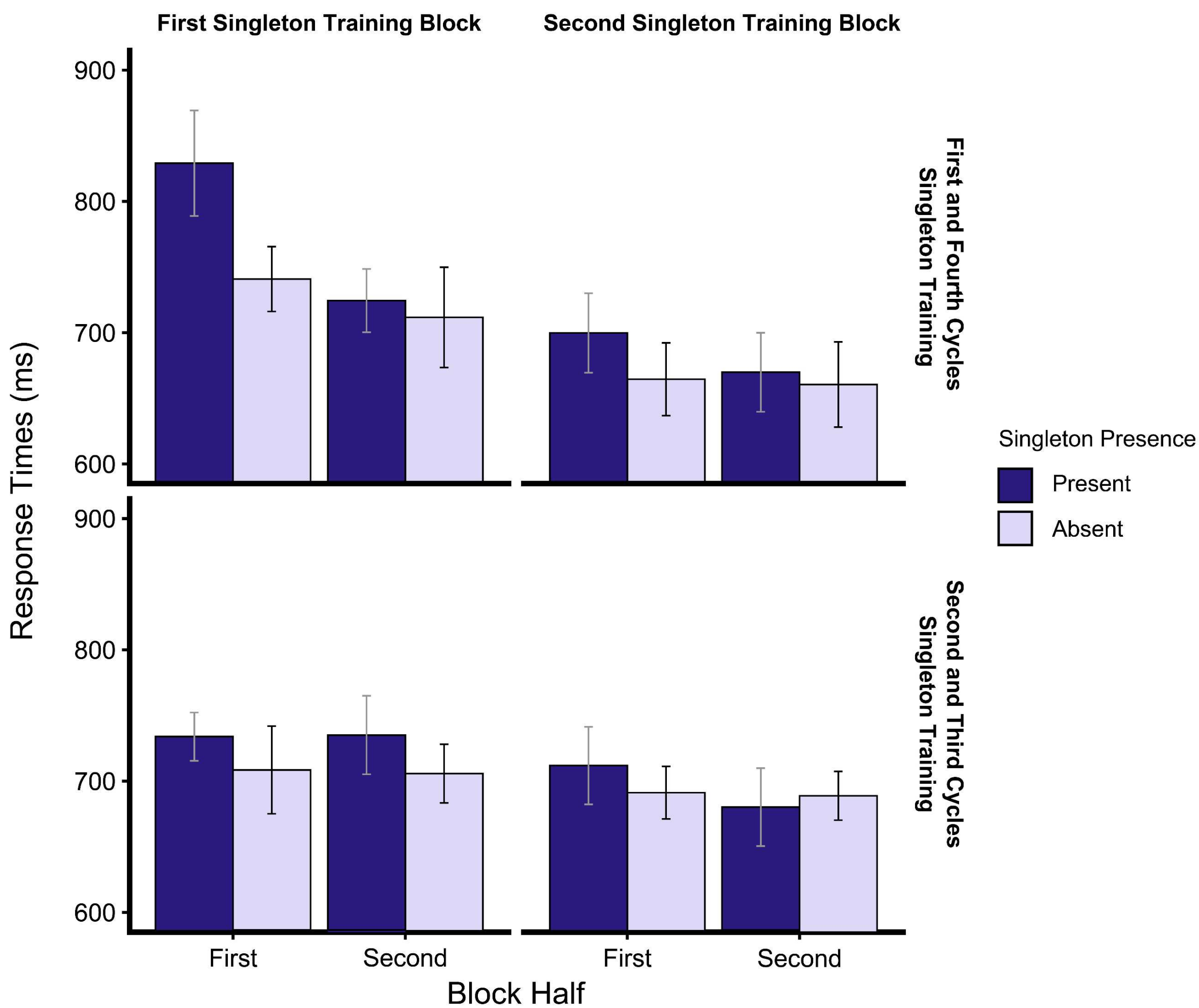

Figure 3*.* Mean RTs by block half and singleton presence in singleton training blocks. Columns represent the order of blocks, each participant completed two singleton training blocks. Rows distinguish between counterbalancing groups. For half of participants the first experimental block was a singleton training block, while the second was the 7th experimental block (the 4th training-test pair; upper row). The other half of participants viewed singleton training blocks during the 2nd and 3rd training-test cycles (lower row). Error bars represent within-subject 95% confidence intervals.

were non-significant, *F*s < 2.12, *p*s > .153. Although these statistics do not conclusively support a successful replication – or conversely a failure to replicate – the learned distractor rejection effect across all blocks, it is important to note that our experiment was not designed to test this comparison. Nevertheless, the numerical pattern of attentional capture across blocks, paired with the marginal significance of the three-way interaction seem inconsistent with the original results of Vatterott and Vecera (2012).

Our main result from this experiment was the lack of any difference in singleton presence effects for blocks following singleton training vs. tripleton training. However, these results are difficult to interpret in light of the failure to robustly reproduce the learned distractor rejection results of Vatterott and Vecera (2012). There are many plausible reasons for our replication difficulties, including our choice of stimuli and more complex task conditions (including tripleton training blocks). While it is essential to investigate these and other possible reasons, we wish to keep the present work focused on our initial question: is experience with salience necessary for the learned rejection of salient distractors? Given that the first block of Experiment 1 showed clear attenuation of the distractor effect, we set out to focus on only one training-test block pair in Experiment 2, in order to provide a satisfactory test of our critical hypotheses.

Error Rates

The same 3 x 2 ANOVA, as described above was conducted on error rates. Only a significant effect of block type was observed, $F(2, 92) = 3.80$, $p = .026$, $\eta_p^2 = .08$, all other Fs < 1.1, ps > .337. pairwise comparisons revealed that the significant main effect of block type was driven by higher error rates in tripleton test blocks (4.37%) than singleton test blocks (3.29%), $t(46) = 3.03$, $p = 0.009$, $d_z = 0.46$.

Experiment 2

As mentioned above, in Experiment 1 we did replicate the initial capture, then rejection of a salient color singleton in the first block of the experiment. In order to continue our investigation of the role that salience plays in rejecting a salient distractor, Experiment 2 includes just one training-test block pair. This change also required a switch to a between-subjects manipulation of training block type. Further, in addition to the singleton and tripleton training conditions from Experiment 1, we added a third group in which the training block consisted of only distractor-absent trials. This no-distractor group should therefore have no experience with the distracting feature at the start of the test block, providing a baseline level of expected attentional capture during the test block that would be expected without any form of prior distractor rejection experience. Therefore, in addition to our previous hypothesis, if experiencing a color distractor in training blocks leads to a reduction of capture, regardless of salience, then attentional capture will be the greatest in test blocks following a training block without any color distractors.

Note one potential concern with the no-distractor control group involves previous work showing that distractor interference is increased when distractors have not been previously presented; and that this may occur independently of the learned feature ignoring effect that we are investigating (Müller et al., 2009; Zehetleitner et al., 2012). Thus, our interpretation will focus primarily on the similarity of capture effects in the singleton and tripleton test blocks, with the test block of the no-distractor group simply serving to show that there was an attenuation of capture in both other cases.

Method

Experimental procedures and statistical analyses were preregistered prior to data collection. The preregistration document is posted at https://doi.org/10.17605/OSF.IO/65RXH. Any deviations from our preregistration are declared as such.

Participants

161 participants, in total, were recruited via the Research Experience Program (REP) at The Ohio State University (51 women, 16 men, 1 non-binary, 1 unlabeled; mean age = 19.9 years) and through an online paid subject recruitment platform, Prolific.co (34 women, 46 men, 2 non-binary; mean age = 30.2 years)[1]. In the absence of an informative power analysis, we determined that a large sample of 50 participants per group should provide sufficient power. REP participants received course credit as compensation. Prolific.co participants were paid $5 for completion of the experiment which took 7.4 min on average. All participants completed the experiment online. Prolific.co participants were required to be aged 18-40, be located in the United States, have an approval rating of at least 96% and have completed at least 50 prior approved submissions. All participants were required to have normal color vision and normal or corrected to normal visual acuity, though this was difficult to ensure using the online format. All experimental procedures were approved by the Institutional Review Board at The Ohio State University.

Apparatus

Stimuli for this experiment were drawn using HTML Canvas Graphics and were presented using the jsPsych plugin via JavaScript (Leeuw et al., 2023). Experiment 2 was conducted online, so we did not have control over features of the visual environment such as lighting, viewing distance and computer/monitor specifications. All visual angles are reported assuming a 24’’ monitor with a 1920 x 1080 resolution (i.e.,

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[1] Gender distributions and mean age are reported for participants retained in final analyses. See results section for more information on participant removal.

35.8 px/cm) at a viewing distance of approximately 70 cm (see Experiment 1 for visual angles).

Stimuli

Stimuli were coded to be identical to those described in experiment one with a few exceptions. Target color was consistent throughout the experiment as either green (RGB 0, 255, 0) or red (RGB 255, 0, 0) and was counterbalanced across participants within each training group assignment. On distractor absent trials all search items were rendered in the target color (e.g., all green). On distractor present trials one or three non-target shapes were presented in the opposing color – i.e., red if the target color was green, or vice versa.

Procedure

The procedure of Experiment 2 was similar to that of Experiment 1, with a few key differences. A minor change was made to the practice block, increasing the number of distractor-absent practice trials at the start of the experiment to 60, the same number used in Vatterott and Vecera (2012). More importantly, we reduced the number of experimental blocks from the eight presented in Experiment 1, to just two, a single training-test block pair, in light of our incomplete replication of Vatterott and Vecera (2012), particularly in the 2nd through 4th blocks. As Experiment 2 employed a between-subjects training manipulation, participants were evenly divided between three distinct training groups, creating singleton-, tripleton-, or no-distractor training groups, as follows.

Singleton and tripleton training blocks were identical to those of Experiment 1. The third type of training block, a no-distractor block consisted of only distractor absent trials. As in the other two groups, the no-distractor group's test block was a singleton distractor block, presenting a salient color singleton. Each experimental block consisted of 48 trials.

Feedback in Experiment 2 was visual, given in the form of onscreen messages declaring the response "CORRECT", "INCORRECT", or "TOO SLOW." Prior to the start of the experiment participants informed of the specific features of the target (e.g., green circle) and were instructed to do try to ignore the distractor color. Experimental instructions were identical across training group assignments; therefore, participants were unaware of whether they would see singleton, tripleton, or no distractors during the experimental blocks.

Results and Discussion

We reported in our preregistration that participants whose overall mean RT or accuracy fell outside of a three standard deviation range from participants within the same group would be removed and replaced. However, we opted to add an additional step prior to the removal of these participants, which cut all participants with mean accuracy below 60%. We made this change because more participants than expected showed near-chance level accuracy. We determined that 60% was a sensible threshold to assume that participants did not reasonably complete the task. The threshold was decided upon independently of the critical inferential statistics. In total, 11 participants were removed from final analyses because they failed to meet the above criteria. A final sample size of 150 participants (50 participants per group) was used in all analyses.

Response Times

Trials on which an error was made, or no response was given, were excluded from analyses. Individual subject condition means and standard deviations were calculated based on singleton presence (present vs. absent) and block type (training vs. test). RTs outside the relevant three standard deviation range were removed from all analyses. In total, 6.7% of trials were excluded based on these criteria. Multiple comparisons were corrected for using the Holm-Bonferroni procedure, where appropriate.

Following the null effect in the critical comparison from Experiment 1 we planned to perform Bayesian analyses alongside all frequentist tests. In the absence of a justification for the use of an informed prior, we used a uniform prior, assuming all models were equally likely (i.e., prior probability equals 1 divided by the number of models), for all Bayesian analyses. For Bayesian ANOVAs, main effect and interaction Bayes factors (BF_{incl}) are reported (van den Bergh et al., 2020). Interaction Bayes factors were calculated by comparing a null model, containing only main effects, to an alternative one which contained both main effects and the interaction term.

As in Experiment 1, we first assessed the attenuation of attentional capture in the singleton training blocks, using a paired samples t test comparing singleton presence costs (present-absent) by block half. Singleton presence costs in the first half of the block (53 ms) did not significantly differ from those in the second half (24 ms), $t(49) = 1.47$, $p = .148$, $d_z = .21$, $BF_{10} = 0.42$, Figure 4A. This result seemed to indicate a failure to replicate the attenuation of attentional capture between the first and second halves of a singleton distractor block, originally reported by Vatterott & Vecera (2012).

We next conducted at 3 x 2 mixed measures ANOVA on the test phase data, with training condition (singleton vs. tripleton vs. no-distractor) and singleton presence (present vs. absent) as factors. A significant main effect of singleton presence was found with slower mean RTs on singleton present trials (735 ms) than singleton absent trials (708 ms), $F(1, 147) = 22.94$, $p < .001$, $\eta_p^2 = .14$, $BF_{incl} = 2.10*10^3$. The main effect of training condition was not significant, $F(2, 147) = 0.16$, $p = .856$, $\eta_p^2 = .002$, $BF_{incl} = 0.30$. The interaction between training condition and singleton presence was marginally significant, $F(2, 147) = 2.94$, $p = .056$, $\eta_p^2 = .04$, $BF_{incl} = 0.65$. Mean RTs are depicted in Figure 5A.

The non-significance in our replication of the learned distractor rejection effect (Vatterott and Vecera, 2012), along with the marginal significance of our 3x2 interaction indicated to us that we may have been underpowered for the strength of the effect under investigation. This likely occurred due to an overestimation of the size of these effects based on previously reported figures, as well as the results we obtained in Experiment 1. We therefore decided to perform a post-hoc sample size augmentation according to the procedure from Sagarin et al. (2014). All further analyses with statistically significant results will report an additional $p_{augmented}$ statistic, which provides the inflation of Type I error for the comparison due to the addition of participants into our sample. To avoid further increases to the false positive rate, we recruited an additional set of 150 participants before re-analyzing the data, which was declared as an addendum to our preregistration prior to the augmentation. All the following analyses were conducted after the sample size augmentation.

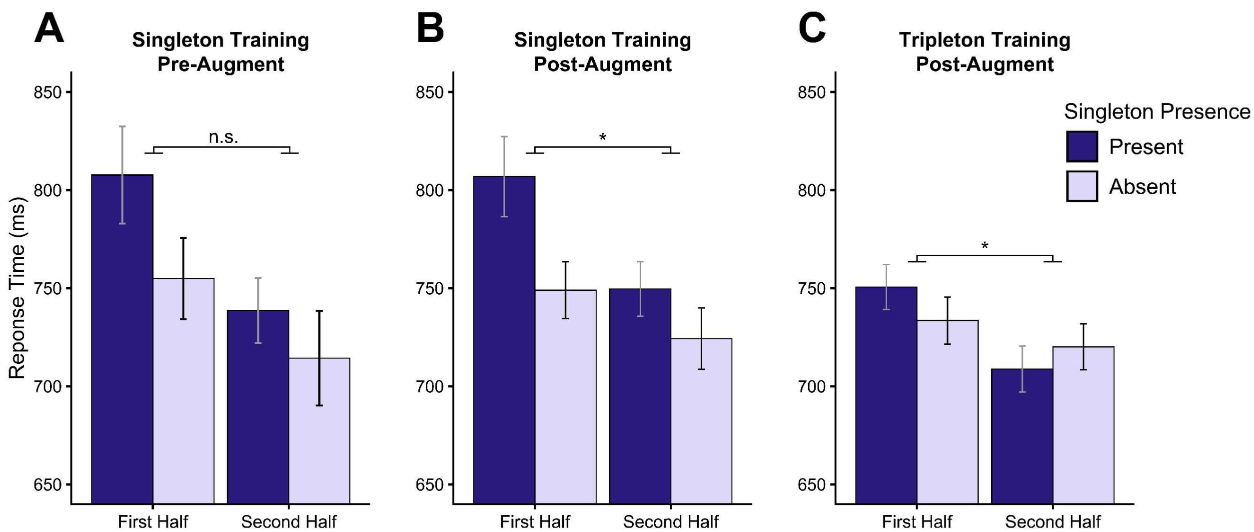

Figure 4. Mean RTs from singleton-group training blocks as a function of singleton presence and block half (A) before (n = 50) and (B) after (n = 100) sample size augmentation. (B) Mean RTs graphed in the same way from tripleton training blocks. Error bars represent within-subject 95% confidence intervals.

A total of 156 additional participants (60 women, 86 men, 3 non-binary, 1 prefer not to answer; mean age = 31.7) were recruited on Prolific.co. Six participants were removed from all analyses due to response times or accuracy outside of a 3 standard deviation range from the mean of participants within the same group. The removal of participants in the augmented sample was independent of the original sample of 150 participants. Therefore, no additional participants from the original sample were removed in this way. The same within-participant trimming procedure, as discussed above, was performed on the combined sample, a total of 6.4% of all trials were discarded due to this procedure.

Training Blocks. With the addition of 150 participants, we achieved a significant result when comparing singleton presence costs in the first half of the singleton training block (58 ms) to those in the second half of the same block (25 ms), $t(99) = 2.34$, $p = .021$, $d_z = .23$, $p_{augmented} = [.056, .062]$, $BF_{10} = 1.49$, shown in Figure 4B. Following Vatterott and Vecera (2012) and Experiment 1 we conducted two additional exploratory paired-samples t tests on present and absent trials in each block half individually. These comparisons revealed that response times were significantly slower on distractor present trials in both the first, $t(99) = 4.67$, $p < .001$, $d_z = .47$, $p_{augmented} = [.05, .050]$, $BF_{10} = 1.65*10^3$, and second, $t(99) = 2.80$, $p = .006$, $d_z = .28$, $p_{augmented} = [.051, .052]$, $BF_{10} = 4.34$ halves of singleton training blocks.

These results indicate that an attenuation of capture occurred from the first to second halves of the singleton training block. The reason for the replication failure is still unclear; however, the present results, along with some more recent related experiments (Ramgir & Lamy, 2023; Ruthruff et al., 2022), suggest that the true effect size for learned distractor rejection is smaller than initially estimated by Vatterott and Vecera (2012). We will further discuss differences in the designs and results in the General Discussion.

At the suggestion of reviewers, we performed additional analyses on tripleton training blocks. These analyses were not preregistered. Note that we do not include $p_{augmented}$ values for the following analyses as we conducted them after our sample augmentation.

Our goal was to assess how the presence of tripletons impacted behavior, much in the same fashion that we examined how singletons impacted behavior in the singleton training blocks. Thus, we carried out the same analyses as described for the singleton training blocks. We started with a paired samples t-test comparing tripleton presence vs absence across block halves. Note that we refer to the tripletons as “distractors” here, although we acknowledge they may not be considered distractors in the same way as singleton items. We found significantly greater distractor presence costs in the first half of the block (17 ms) compared to the second (-11 ms), $t(99) = 2.58$, $p = .011$, $d_z = .26$, $BF_{10} = 2.55$. We performed two additional paired samples t-tests, separately comparing RTs on singleton present and absent trials in the first and second halves of tripleton training blocks. We observed a significant RT difference in first half of the block, $t(99) = 2.32$, $p = .022$, $d_z = .23$, $BF_{10} = 1.43$, with slower response times when the distractors were present compared to absent. No significant difference was found in the second half of tripleton training blocks, $t(99) = 1.54$, $p = .127$, $d_z = .15$, $BF_{10} = 0.35$, though a numerical benefit was observed on distractor present trials. It is important to note that the lack of a significant benefit could simply reflect the fact that we have few trials in which this benefit is likely to be observed. Perhaps with a longer training block we would find this benefit to be robust.

Test Blocks. A 3 x 2 ANOVA, as described above, was conducted on the full sample. We again observed a significant main effect of singleton presence with slower RTs on singleton present (740 ms) compared to singleton absent (718 ms) trials throughout testing blocks, $F(1, 297) = 35.81$, $p < .001$, $\eta_p^2 = .11$, $p_{augmented} = [.05, 0.050]$, $BF_{incl} = 9.40 * 10^5$. The main effect of training group was still non-significant, $F(2, 297) = 0.55$, $p = .577$, $\eta_p^2 = .004$, $BF_{incl} = 0.26$. Critically, the interaction between training group and singleton presence was now significant, $F(2, 297) = 4.65$, $p = .010$, $\eta_p^2 = .03$, $p_{augmented} = [.050, .055]$, $BF_{incl} = 2.72$, see Figure 5B.

With the now-significant training condition by singleton presence interaction in the test phase data, we conducted three follow-up simple 2 (training group) x 2 (singleton presence) ANOVAs. We began with our critical comparison (singleton and tripleton training groups); similar to Experiment 1, we found a significant main effect of

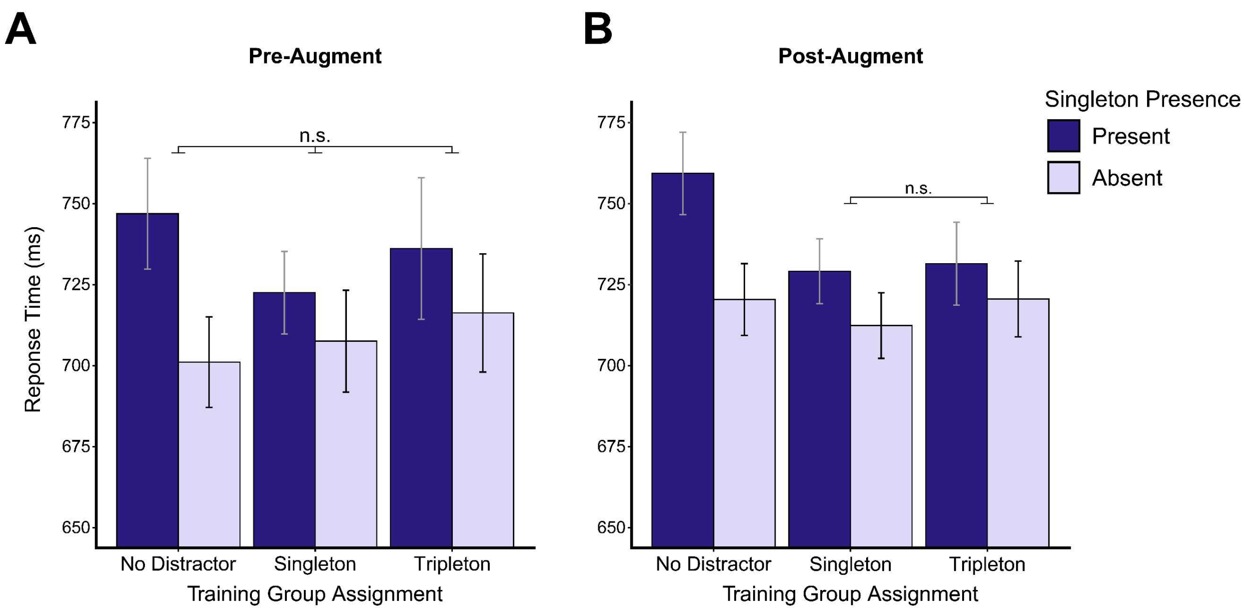

Figure 5. Mean RTs from Experiment 2 graphed as a function of singleton presence training group assignment. (A) RTs taken from original sample of 150 participants and (B) the full sample of 300 participants. Error bars are within-subject 95% confidence intervals calculated within each group, chosen for ease of comparing the effect of singleton presence within training group assignment.

singleton presence when comparing between the two test groups, $F(1, 198) = 9.86$, $p_{hb} = .002$, , $\eta_p^2 = .05$, $p_{augmented} = [.050, .051]$, $BF_{incl} = 11.36$, but no significant interaction effect, $F(1, 198) = 0.72$, $p_{hb} = .399$, $\eta_p^2 = .004$. This lack of an interaction shows that there was no meaningful difference in singleton presence costs between these groups. To assess support for the null hypothesis, we conducted a Bayesian ANOVA, which indeed supported the null, $BF_{incl} = 0.24$.

For the second 2 x 2 ANOVA, we included singleton and no-distractor training groups. We found a significant main effect of singleton presence, $F(1, 198) = 41.35$, $p_{hb} < .001$, $\eta_p^2 = .17$, $p_{augmented} = [.05, .050]$, $BF_{incl} = 6.57*10^6$, and a marginally significant interaction, $F(1, 198) = 4.82$, $p_{hb} = .058$, $\eta_p^2 = .02$, $p_{augmented} = [.05, .067]$, $BF_{incl} = 1.57$.

Finally, for the last 2x2 ANOVA, we included tripleton and no-distractor training groups. We found both a significant main effect of singleton presence, $F(1, 198) =$

26.89, p_{hb} < .001, , η_p^2 = .12, $p_{augmented}$ = [.05, .050], BF_{incl} = 1.45*10^4, and a significant interaction effect, F(1, 198) = 8.41, p_{hb} = .012, , η_p^2 = .04, $p_{augmented}$ = [.050, .051], BF_{incl} = 8.28.

These results mirror those of our first experiment, while overcoming some of its limitations. In our critical comparison between singleton and tripleton test blocks, we found that there were no differences in singleton presence costs as a function of whether participants had experience with the distractor's salience or not. Surprisingly, but consistent with Experiment 1, the numerical pattern of greater attentional capture in no-distractor test blocks relative to singleton test blocks was not statistically reliable after correcting for multiple comparisons (uncorrected p-value = .029). Unfortunately, it is no more clear here, than in Experiment 1, why we do not observe a more robust attenuation of singleton presence costs in this specific comparison, especially considering the learned distractor rejection observed in the singleton training block. However, as stated above, we were primarily focused on comparing capture between the singleton and tripleton test blocks. Therefore, despite this puzzling result, the robust learned distractor rejection we found in the tripleton training group provides convincing evidence for the relative unimportance of salience when learning to reject distractors.

Error Rates

The same 3 x 2 ANOVA as described above was performed on error rates. We found a significant main effect of singleton presence, F(1, 297) = 11.49, p < .001, η_p^2 = .04, $p_{augmented}$ = [.050, .050], BF_{incl} = 22.07, however, this difference was not the result of a speed-accuracy tradeoff, instead participants had higher error rates in the singleton present condition, where response times slower. The main effect of condition, F(2, 297)

= 1.48, p = .229, η_p^2 = .01, BF_{incl} = 0.12, and the interaction between training group and singleton presence, F(2, 297) = 3.01, p = .051, η_p^2 = .02, BF_{incl} = 0.55, were not significant.

General Discussion

The goal of the current experiments was to investigate the role that experience with salience plays in rejecting a distractor that is, itself, salient. To do this we used a training-test procedure contained within a modification of the learned distractor rejection paradigm (Vatterott and Vecera, 2012). More specifically, we presented participants with pairs of training and test blocks while a) manipulating the salience of the color distractors in the training block and b) presenting a salient color singleton in the test block.

In Experiment 1 we observed no difference in attentional capture between test blocks as a function of whether these blocks followed singleton or tripleton training blocks. However, we lacked confidence in our replication of the learned distractor rejection effect, which limited our ability to interpret the test block data. Experiment 2 focused on the parts of Experiment 1 in which we successfully observed learned distractor rejection (specifically, the first block of trials). Here, after taking steps to obtain substantial power – following a post-hoc sample size augmentation – we were able to observe a reasonably robust learned distractor rejection effect. Upon successfully producing this effect, we obtained novel evidence that experience with distractor features in the absence of saliency signals provides the learning needed for effective rejection of a salient distractor.

Implications for mechanisms of suppression

Our results seem to fall in line with more recent versions of the signal suppression hypothesis (Gaspelin & Luck, 2019, 2018c, 2018b). As mentioned earlier, the initial formulation placed an emphasis on distractor salience, holding that effective rejection of a distractor relies on suppressing the distractor's saliency signal (Gaspelin et al., 2015; Sawaki & Luck, 2010). Accordingly, salient search items have been theorized to generate an attention capturing signal, but this very signal is proposed to allow suppression of the item (Sawaki & Luck, 2010). Recent work, however, has highlighted the importance of experience with the basic properties of the distractor (i.e., first order features), arguing that saliency signals alone are not sufficient for effective distractor suppression; rather, knowledge of first order distractor features is integral to this suppressive mechanism (Gaspelin & Luck, 2018b; see also Gaspelin & Luck 2019; Luck et al., 2021). Our current findings support this shift towards a feature-based, rather than a saliency-based, model of distractor rejection. However, unlike much of the support for the signal suppression hypothesis (Chang & Egeth, 2019; Gaspelin et al., 2015, 2017; Gaspelin & Luck, 2018a; Stilwell & Gaspelin, 2021), our experiment was designed only to test distractor ignoring, or rejection, without a specific measure of distractor *suppression*, per se. Additionally, we do not find a complete elimination of attentional capture in test blocks, indicating that the distractor rejection we observe may reflect an incomplete suppression of distractors. This leaves open some possibility of a contribution of salience-rejection in the case of suppression below baseline-distractor levels.

While we did not include a direct measurement of suppression, a recent investigation of the suppression of non-salient distractors (Lien et al., 2022) lends support to the possibility that experiencing non-salient, irrelevant features may successfully translate to robust suppression of singletons sharing the same feature. When presenting tripletons in a capture-probe paradigm (see Gaspelin et al. 2015), Lien et al. (2022) found probe accuracy for non-salient, distractor colored, items to be lower than that of target-colored distractors. Further, this suppression was nearly identical to that of salient singleton distractors and occurred regardless of whether salient distractor trials were included in the experiment. Based on these results, Lien et al. (2022) concluded that participants engaged in feature-based suppression (referred to as “distractor-based suppression” by the authors) in order to de-prioritize distractor-colored search items below baseline levels of non-critical target-colored items.

In addition, other research has provided further evidence that more salient distractors do not necessarily receive greater suppression (Hauck et al., 2023; but see Stilwell et al., 2023). Despite not including a suppression metric in this study, it is clear that if participants were, in fact, using salience information in the singleton training condition, it provided minimal, if any, benefit to the eventual rejection of salient distractors in the test blocks compared to solely-feature-based rejection. Therefore, our results add to previous findings, providing evidence that the feature-based suppression of non-salient distractors may employ the same suppressive mechanism as is used when suppressing a salient distractor. Still, where salient distractors are concerned, it remains possible that an additional rejection of saliency signals is necessary to observe suppression of salient distractors, despite providing little benefit beyond that.

One might argue that we did not completely eliminate salience in our tripleton blocks. That is, local variations in salience are inevitable, such as when one red object is placed in between two green objects (which would contain more color contrast than a red object between, say, another red object and one green object). However, we must emphasize that, due to the constraints of matching 3 green and 3 red items in each display, the local contrast of each tripleton item is necessarily paired with the local contrast of one of the target-colored items. Therefore, while variation in salience does inevitably occur between items, this variation is matched between tripleton and target-colored items. As a result, it is improbable that a rejection strategy targeting – or even incorporating – the salience of an item would have formed during the tripleton training blocks.

We would also like to note that, without a direct measure of suppression it is possible that what we refer to here as “distractor rejection” could be attributed to a mechanism that acts though enhancement of the target items features, with no regard of distractor items whatsoever (Oxner et al., 2023), although previous work has argued that rejection can include both components of target enhancement and distractor (Chang & Egeth, 2019). Importantly, our conclusions about the role of salience in learned distractor rejection are independent of the mechanism by which the aforementioned reduction is achieved.

What is the theoretical mechanism underlying our learned rejection results? One possibility, the *habituation* account of distractor rejection, holds that as experience with a salient distractor is gained, the *orienting response* (OR) to the search item habituates (Turatto & Pascucci, 2016; see also Turatto, 2023). This habituation could result in an attenuation of capture that is not due to any active or strategic process, but rather a passive accumulation of evidence enforcing the distractor’s irrelevance (De Tommaso &

Turatto, 2019; Pascucci & Turatto, 2015; Turatto, Bonetti, & Pascucci, 2018; Turatto, Bonetti, Pascucci, et al., 2018; Turatto & Pascucci, 2016). Though much of the evidence for habituation comes from onset distractors (Pascucci & Turatto, 2015; Turatto, Bonetti, & Pascucci, 2018; Turatto, Bonetti, Pascucci, et al., 2018; Turatto & Pascucci, 2016), Won and Geng (2020) found colored distractors that had been presented passively (i.e., with no task) were less distracting in the context of a visual search compared to novel distractors. For our purposes, their results indicate that attentional capture by color singletons may also be subject to a habituation mechanism (see also De Tommaso & Turatto, 2019).

For the present results, in the singleton training blocks, habituation of the orienting response to the salient color singleton is plausible. However, how habituation might have acted during tripleton training blocks is somewhat less clear. Results from our tripleton training blocks may provide critical insight here in that response times were initially slower when tripletons were presented. There are multiple potential explanations for this slowing of RTs that are unrelated to a shift of attention to the tripleton items, such as filtering costs (Folk & Remington, 1998). However, In the case that there was an initial orienting response to the tripletons – despite their non-salience – then this response could have habituated over time; eventually leading to a reduction in the capture by singletons sharing the habituated color. Importantly, if it was simply the orienting response that habituated, one might expect some rebound of this response when suddenly increasing the salience of color distractor (see characteristic 8 from Turatto, 2023). However, we did not observe a rebound. One alternative possibility is that habituation during tripleton training may have acted on the feature representation of the tripleton color. In other words, the relevance of the distractor color became habituated though experience with tripleton distractors. It is relevant here to consider that the amount of distractor experience was greater in tripleton compared to singleton training blocks. That is, whereas there was only one distractor color item in singleton displays, tripleton displays presented three, such that participants viewing tripleton displays had three times as much experience with the distractor color. Future research could attempt to manipulate physical distractor exposure between salience levels to determine whether an increased number of distractor items per display bolsters the habituation process. The current design does not extensively explore how habituation mechanisms may behave differently across salience levels, and therefore cannot distinguish whether different mechanisms were at work in out experimental groupings. Despite this, the current findings leave open the possibility that the diminishment of attentional capture observed over time resulted from habituation of distractor-related processing.

Implications for theories of attentional capture

Our findings seem inconsistent with stimulus-driven accounts of attentional capture that place great emphasis on the importance of distractor salience (Theeuwes, 1992, 2004, 2010; Wang & Theeuwes, 2020). Continuing along this line of reasoning, one potential interpretation of our results is that we find no attentional capture in the test block following tripleton training simply because salient color singletons are not especially distracting (Ruthruff et al., 2021).

Ruthruff et al. claimed that color singletons lack the power to capture attention, even when they are salient. In some respects, this account is consistent with our findings. For example, if color singletons are not especially distracting, then experience with rejecting salience should not be necessary. We do of course find robust distraction in the singleton training and no-distractor test blocks, but based on the arguments of Ruthruff et al. (2022), one might consider these results as being due to a *surprise capture effect* (Ernst et al., 2020; Horstmann, 2002, 2015). That is, novelty of the color distractor item rather than its salience drives the distraction. Nevertheless, we do find some evidence for attentional capture by color singletons in cases where a distractor novelty effect is unlikely; when we examined only singleton and tripleton test blocks, (i.e., test blocks following training blocks which contained distractors), we still observed an overall attentional capture effect. It could be assumed that singletons following the tripleton training blocks were “novel,” driving an overall attentional capture effect, but this claim is inconsistent with the lack of a difference between singleton and tripleton testing blocks. Therefore, it is unlikely that the evidence we provide here for feature rejection without the need for salience is due to a baseline ineffectiveness of color singleton distractors. Instead, we propose that, as mentioned above, the salience of the distractor means little if observers have already implemented robust rejection of its first-order features, and that this vulnerability to learned feature rejection is what causes the apparent inability to capture attention in many cases.

Learned Distractor Rejection

When it came to obtaining the learned distractor rejection effect in our current design (De Tommaso & Turatto, 2019; Ramgir & Lamy, 2023; Vatterott & Vecera, 2012), we unfortunately encountered difficulty. In Experiment 1 our replication appeared successful when collapsing across blocks, and we did observe a robust attenuation of attentional capture when a singleton training block was the first experimental block (i.e., the first block containing color distractors). However, we observed a minimal rebound of attentional capture in singleton training blocks following the first one. Further, participants who viewed a tripleton training block during the first training-test cycle showed little evidence of needing to re-learn distractor rejection in the subsequent singleton training blocks. In Experiment 2, we failed to observe robust learned distractor rejection in training blocks prior to our sample size augmentation.

It is important to note that, despite closely modeling our study on Vatterott and Vecera's (2012) design parameters, our design was not identical to theirs. It therefore remains possible that our replication difficulties in Experiment 1 stemmed from these differences, principle among them the inclusion of tripleton distractors. Because of the ordering of tripleton and singleton training-test pairs across participants, our lessened learned distractor rejection effect is perfectly confounded with participants having previously completed a tripleton distractor block. However, it is not clear what, if any, theoretical accounts would posit that experience with tripletons should enable an observer to bypass the need for learned rejection. Further, it is important to note that the confound of having viewed a tripleton block aligns perfectly with completing a block containing singleton distractors (i.e., in the singleton test block that followed every tripleton training block).

One plausible explanation for the lack of attentional capture rebound at the beginning of singleton training blocks in Experiment 1 is the rejection of second-order distractor salience. That is, participants may have employed a rejection mechanism focused on the distractor's saliency signal itself, regardless of basic first-order distractor features. Recent research has found that purely salience-based distractor rejection is possible in certain situations (Ma & Abrams, 2022; Vatterott et al., 2018; Won et al., 2019), though this finding is somewhat contested (Gaspelin & Luck, 2018b). To test a second-order suppression account of the present results, we inspected – in exploratory fashion – the time-course of distractor rejection throughout singleton training blocks, using a more fine-grained approach. We sorted singleton training blocks from Experiment 1 into 6 bins containing 8 trials each. What we saw was that – for the singleton training blocks following the first one – the singleton presence cost in the first bin was numerically greater than in subsequent bins, suggesting that capture did initially occur, and rejection was learned extremely rapidly (see Ruthruff et al., 2022 for a similar analysis). These results argue against a strong version of a second-order suppression account of our data; that is, if second-order rejection of saliency signals occurred, it did not completely eliminate the requirement of re-learning rejection. That said, it may have contributed to the increased learning speed in the second through fourth singleton training blocks compared to the first.

A second possibility comes in the form of selective dimension-weighting (Found & Müller, 1996; Müller et al., 1995). The dimension-weighting account (DWA) proposes that stimulus dimensions can be selectively up- and down-weighted as their relevance to the current task is learned. For example, once distractor-colored items started to appear in our displays, participants may have started to down-weight the color dimension. The lack of an observable attentional capture rebound may have result from an inability of salient color singletons to attract attention with color in a down-weighted priority state.

Taken together, the current findings indicate that learning to reject salient color singletons does not require an additional component of learning to reject the distractor's saliency signal. However, the current experiment was designed only to test the requirement of rejecting saliency, therefore, further investigation is required to determine if experience with salience provides any additional contribution or benefit when learning to ignore salient distractors, despite not being required for eventual rejection.

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Declaration of Interests

The authors report no potential conflicts of interest.

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