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Taming the White Bear: Initial Costs and Eventual Benefits of Distractor Inhibition

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Abstract

Previous research indicates that prior information about a target feature, such as its color, can speed search. Can search also be speeded by knowing what a target will *not* look like? In the two experiments reported here, participants searched for target letters. Prior to viewing search displays, participants were prompted either with the color in which one or more nontarget letters would appear (ignore trials) or with no information about the search display (neutral trials). Critically, when participants were given one consistent color to ignore for the duration of the experiment, compared with when they were given no information, there was a cost in reaction time (RT) early in the experiment. However, after extended practice, RTs on ignore trials were significantly faster than RTs on neutral trials, which provides a novel demonstration that knowledge about nontargets can improve search performance for targets. When the to-be-ignored color changed from trial to trial, no RT benefit was observed.

Keywords

inhibition, attention, visual search, perceptual learning

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When people visually search for a target item (e.g., a set of car keys), they are often faced with a large amount of clutter (i.e., nontarget items). In the case of car keys, the search is fairly easy because people have a representation of what the target should look like; therefore, they will search until they find an object that matches the target. However, during some visual searches, they might instead have information about what the target will *not* look like. Intuitively, knowing what not to look for—what to ignore—should reduce the number of items that need to be considered, thus reducing search times. Does knowing what feature to ignore prove, in fact, to be a benefit in visual search?

Prior work has suggested the surprising possibility that even when subjects are given valid feature information about to-be-ignored items (e.g., their color), it actually hurts rather than helps their performance (e.g., Moher & Egeth, 2012; Tsal & Makovski, 2006). There is a family resemblance between these results and the *white-bear effect* (Wegner, Schneider, Carter, & White, 1987). In an experiment by Moher and Egeth (2012), when observers were given nontarget feature information (in the form of a pretrial cue) on a trial-by-trial basis, reaction times (RTs) were slower than when they were given no information at all. Moher and Egeth suggested that this was because, in their search process, observers first selected the to-be-ignored item and then inhibited it. In a follow-up experiment, Moher and Egeth (2012) included a probe-dot detection task on a subset of trials. They found that when observers were given to-be-ignored information, they were faster at detecting a probe dot at the location of the to-be-ignored distractor when it was presented early in the trial (117 ms) compared with later in the trial (167 ms). It seems that observers in Moher and Egeth's experiment selected the to-be-ignored item and subsequently rejected it. Thus, observers were unable to preattentively reject to-be-ignored information; that is, participants failed to create a "template for rejection" (e.g., Arita, Carlisle, & Woodman, 2012; Woodman & Luck, 2007).

In contrast to the foregoing, research by Moher, Lakshmanan, Egeth, and Ewen (2014) demonstrated that when target and distractor information were held constant

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for the duration of the experiment, observers appeared to attenuate processing of distractor features rather than boost processing of target features. This work suggests that a template for rejection can be created when dealing with to-be-ignored information. However, it is not yet clear what drives the creation of such a template. A possible explanation for these conflicting results may be that successful ignoring of task-irrelevant distractor information is something that needs to be learned.

We consider here the possibility that creation of a template for rejection is facilitated by extended experience with a consistently ignored feature. This possibility is supported by the difference in methods of two previous studies (i.e., Moher & Egeth, 2012; Moher et al., 2014) and the striking difference in their outcomes. In the first study (Moher & Egeth, 2012), the to-be-ignored feature changed on a trial-by-trial basis, whereas in the second study (Moher et al., 2014), the to-be-ignored feature remained fixed for an entire experimental session. There were, of course, other differences between these studies, but the difference in consistency of mapping of stimuli onto responses seems an especially promising avenue for investigation in light of previous work on the topic (e.g., Shiffrin & Schneider, 1977; Vatterott & Vecera, 2012; Zehetleitner, Goschy, & Müller, 2012).

To examine this possibility, we conducted two experiments. In Experiment 1, participants completed a visual search task in which they could learn about one particular distractor feature. At the start of each trial, participants were provided with either a cue that provided no information about the upcoming stimulus display (neutral cue) or a cue that instructed them which color in the following stimulus display to ignore (ignore cue). The ignore cue was always valid, that is, it always referred to a presented distractor. If participants can learn to effectively ignore consistent to-be-ignored features, then over the course of the experiment, we expected them to produce faster RTs on ignore trials than on neutral trials. In Experiment 2, we used the same paradigm as in Experiment 1, but with a larger stimulus array, to minimize the possibility that participants might learn to attend to the other colors rather than try to ignore the to-be-ignored color. Additionally, we were interested in whether participants could learn to ignore the to-be-ignored feature across many items (6 out of 12 items). The paradigms used in both of these studies allowed us to investigate whether consistency in the mapping of to-be-ignored information is critical for developing a template for rejection.

Experiment 1

Method

Participants. A group of 26 Johns Hopkins University undergraduate students and community members (mean age = 20.5 years; 10 male, 16 female) with normal or corrected-to-normal visual acuity and normal color vision participated in the experiment. We conducted a power analysis using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007), which showed that given an f of 0.25, 26 participants would be required to have 85% power to detect the effect in our design (Cohen, 1988, suggests that an effect size of 0.25 indicates a medium-sized effect). Data collection therefore stopped once we reached 26 participants. One subject was eliminated because she had an extremely high error rate (32%; the mean error rate was 4% for the 25 remaining subjects). The participants received extra credit in undergraduate courses or monetary payment as compensation, and all gave informed consent. The Johns Hopkins Homewood Institutional Review Board approved the protocol.

Apparatus. Experimental sessions were carried out on a Dell Precision T3400 2.33-GHz computer. Stimuli were presented on a Dell 1708FP monitor (refresh rate = 60 Hz, resolution = 1,280 × 1,024 pixels). Stimulus presentation and data analysis were performed using programs written in MATLAB (The MathWorks, Natick, MA) and Psychophysics Toolbox software (Brainard, 1997).

Stimuli. Stimuli appeared surrounding a central fixation cross that subtended 0.55° of visual angle at a viewing distance of approximately 60 cm. Stimuli consisted of four letters from the English alphabet, which were randomly assigned to appear in one of four locations surrounding fixation at 0°, 90°, 180°, and 270° from vertical. Each letter subtended a visual angle of 0.86°, and the distance between fixation and the closest edge of each letter subtended 4.96° of visual angle.

On each trial, either a capital "B" or "F" was selected randomly to appear as the target letter. Additionally, a lowercase "b" or "f" was selected randomly to appear as one of three distractor letters. The distractor letter on each trial was either compatible with the target letter, meaning that it shared the target's identity (e.g., "B" was the target and "b" the distractor), or incompatible, meaning that it did not share the target's identity (e.g., "F" was the target and "b" the distractor). The compatibility manipulation was included to make our paradigm as similar as possible to those used in previous studies. Specifically, Moher and Egeth (2012) included it in their design because Munneke, Van der Stigchel, and Theeuwes (2008), who examined the ability to ignore cued spatial locations, found an interesting effect of compatibility that was dependent on whether they cued a distractor location. However, Chao (2010) used a similar paradigm and found no significant interaction with cue type and compatibility, and Moher and Egeth (2012) also found that interaction to be not significant. Those failures to replicate Munneke et al.'s results notwithstanding, we included this manipulation to investigate whether learning to ignore a consistently to-be-ignored color would be affected by the relationship of the target and distractor information. The remaining two distractor letters on every trial were a "k" and an "x," one of which was chosen randomly to be uppercase.

There were two trial types. On neutral trials, the color of each of the four letters was selected randomly without replacement from a set of four colors (red, blue, green, and yellow). However, on ignore trials, the to-be-ignored item always appeared in the to-be-ignored color, while the other three letters were randomly assigned the remaining three colors.

Design and procedure. At the beginning of each trial, a cue was presented for 1,000 ms in white letters above the fixation cross (see Fig. 1). This cue indicated the trial type. On ignore trials, the name of the color to be ignored was stated in the cue (e.g., "Ignore Red"); on neutral trials, the cue just said "Neutral." These cues subtended 0.57° of visual angle vertically and between 2.86° and 5.92° of visual angle horizontally, and they were always valid. Unlike in previous studies, the color to be ignored remained the same for the entire duration of the experiment. The specific color to be ignored was randomly assigned for each participant. On ignore trials, the letter appearing in the to-be-ignored color was always a distractor. Neutral cues gave the participants no information about the color of the target or nontarget items on the upcoming trial. Because only four colors were used in the experiment, the to-be-ignored color could appear on neutral trials.

After the cue was presented, the fixation cross remained in the center of the screen for 1,000 ms. Then the four letters appeared on the screen and remained until participants responded. The participants were told to indicate whether a capital "B" or "F" was present by pressing the "z" key or "/" key, respectively. They were instructed to respond as quickly and accurately as possible. Following their response, there was an intertrial interval consisting of a 500-ms blank black screen. No feedback was provided. Participants completed 720 trials total; 50% of the trials were ignore trials, and 50% were neutral trials, randomly intermixed. Experimental sessions lasted 60 to 75 min.

Results

Following the procedure of Moher and Egeth (2012), we removed RTs that were faster than 100 ms and more than 3.5 standard deviations above or below the mean. The latter criterion was based on a modified recursive trimming procedure developed by Van Selst and Jolicoeur (1994). This resulted in the elimination of 1% of all trials. Additionally, we removed trials with errors from the analysis, which accounted for about 4% of all trials. Mean RTs for all included trials are given in Table 1. To analyze the effect of practice (or experience), we grouped the trials into four 180-trial blocks.

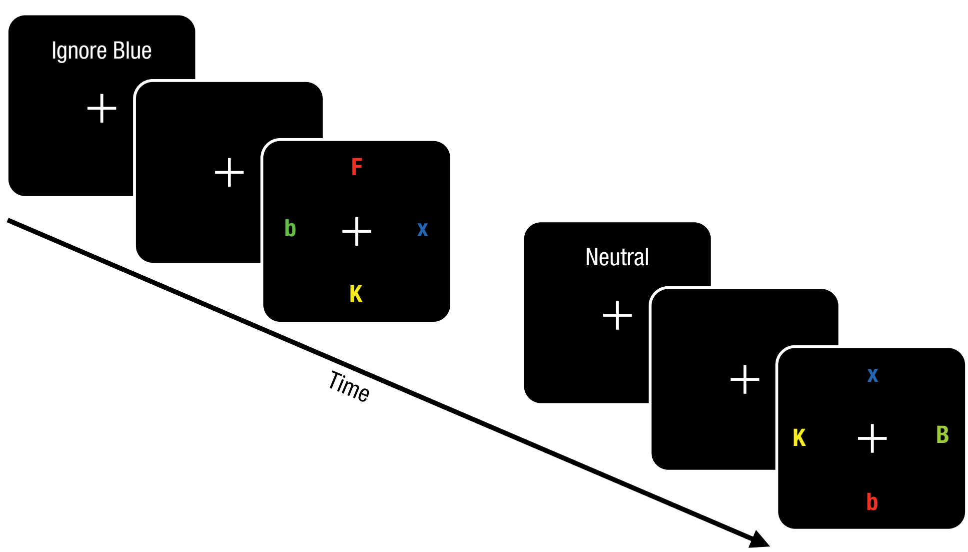

Fig. 1. Examples of the two trial sequences used in Experiment 1. On ignore trials (left), a cue at the start of the trial told participants which color in the following stimulus array to ignore. On neutral trials (right), the cue gave no stimulus information. On both types of trial, one of the colored letters in the stimulus array was the target ("B" or "F"), which participants had to indicate by pressing a key. Each participant was given a randomly selected color to ignore for the duration of the experiment (the color was counterbalanced across observers). There were 720 trials, half of which were ignore trials and half of which were neutral trials, randomly intermixed.

Table 1. Mean Reaction Times (in Milliseconds) for Experiment 1

Trial type	Block 1	Block 2	Block 3	Block 4
Ignore	1,131 (274)	981 (194)	963 (204)	928 (224)
Neutral	1,091 (235)	993 (188)	979 (204)	970 (252)

Note: Standard deviations are given in parentheses.

We included compatible and incompatible trials in a 2 (trial type: ignore vs. neutral) × 2 (compatibility: compatible vs. incompatible) repeated measures analysis of variance (ANOVA). There was a significant main effect of compatibility, $F(1, 24) = 4.59$, $p < .05$, $\eta_p^2 = .16$, with RTs on incompatible trials (M = 1,011 ms) being slower than RTs on compatible trials (M = 997 ms); however, there was no significant effect of trial type, $F(1, 24) = 0.604$, $p = .445$. Additionally, there was no significant interaction between trial type and compatibility, $F(1, 24) = 0.106$, $p = .75$. These results were similar to those found by Moher and Egeth (2012) and Chao (2010). Therefore, in our further analyses, we collapsed across compatible trials and incompatible trials.

Figure 2 shows the mean difference in RT on ignore trials and neutral trials for all participants across all four blocks. We performed a 2 (trial type) × 4 (block) repeated-measures ANOVA on RTs. We found no main effect of trial type, $F(1, 24) = 0.746$, $p = .396$. However, we found a significant main effect of block, $F(3, 72) = 9.871$, $p < .01$, $\eta_p^2 = .3$; RTs decreased over the course of the experiment. Critically, we also found a significant interaction between trial type and block, $F(3, 72) = 5.668$, $p < .01$, $\eta_p^2 = .2$. Additional contrasts revealed that this interaction was largely driven by the differences between trial type in Block 1 and trial type in Block 4, $p < .001$, which accounted for approximately 95% of the effect. Finally, analyses of simple main effects revealed that RTs for the ignore cue were significantly slower than RTs for the neutral cue in Block 1, $p < .05$, and RTs for the ignore cue were significantly faster than RTs for the neutral cue in Block 4, $p < .05$.

The results suggest that when participants ignored the nontarget information, there was a cost in the beginning (Block 1), which is similar to what Moher and Egeth (2012) found. However, as participants learned about the (consistently mapped) to-be-ignored information, they efficiently ignored the nontarget information, which resulted in a benefit in RT (Block 4).

We conducted a further analysis restricted to neutral trials. Given our experimental design, two types of neutral trials were presented. Specifically, because neutral trials used the same four colors (i.e., red, green, blue, yellow) that were used in ignore trials, each neutral trial contained a letter in the color that a participant was learning to ignore. Thus, there were two different types of neutral trial: The target was either (a) the color the participant was learning to ignore or (b) some other color. If participants were learning something about the to-be-ignored color and that this color should not contain the target, then this should have resulted in an RT profile that roughly paralleled the profile observed between neutral and ignore trials.

To investigate this possibility, we performed a 2 (neutral-trial type) × 4 (block) repeated measures ANOVA

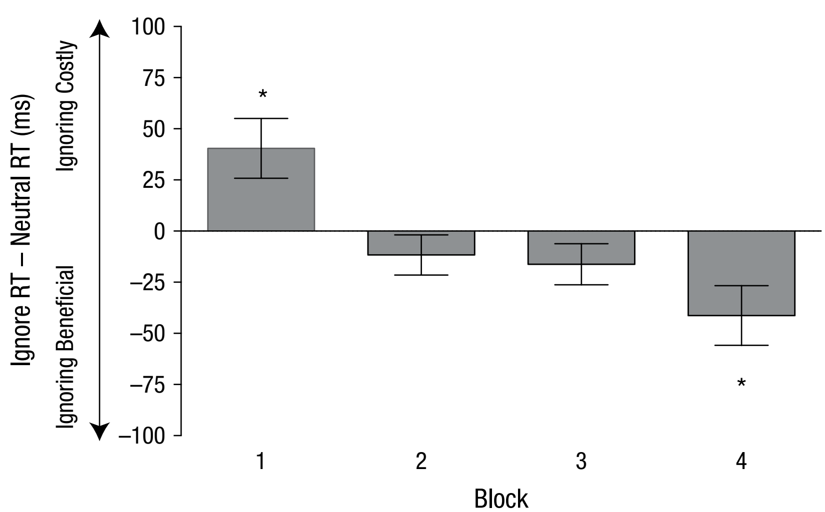

Fig. 2. Mean difference in reaction time (RT) between ignore and neutral trials in Experiment 1, separately for each block of 180 trials. Asterisks indicate blocks in which RTs for the two trial types were significantly different (*$p < .05$). Error bars show ±1 *SEM* calculated within participants using the method of O'Brien and Cousineau (2014).

on RTs (Fig. 3). We found no main effect of neutral-trial type, $F(1, 24) = 0.06$, $p = .809$. However, we did find a significant main effect of block, $F(3, 72) = 3.91$, $p < .05$, $\eta_p^2 = .14$; RTs decreased over the course of the experiment. Critically, there was a significant interaction between neutral-trial type and block, $F(3, 72) = 6.16$, $p < .002$, $\eta_p^2 = .2$. In Block 1, participants were faster when the target on a neutral trial was in the to-be-ignored color. This suggests that at this point, participants were selecting the feature they were starting to learn to ignore on ignore trials, as suggested by the probe-dot-detection results in Moher and Egeth (2012). However, over time (Blocks 2 and 3), this pattern reversed: Participants were slower to select the target on neutral trials when it was the to-be-ignored color on ignore trials. Additionally, in Blocks 2 and 3, RTs on neutral trials were faster when the target was in another color; this suggests that participants were efficiently inhibiting the to-be-ignored color. However, in Block 4, there was no RT difference between the neutral-trial types. It is difficult to know whether this effect was a real reduction in the interference or a reflection of noise in the data; further experiments will be needed to untangle these possibilities. Finally, dropping neutral trials on which the target was the color the participants were learning to ignore still yielded a significant interaction ($p < .05$) when we compared ignore trials with the remaining neutral trials (similar to the pattern shown in Fig. 2).

Finally, the RT benefit shown in Figure 2 could possibly have been driven by the larger number of target-color repetitions (e.g., the target "B" was green on a given trial, and the target "F" was green on the following trial) on ignore trials (average of 30% of trials across all participants) than on neutral trials (average of 25% of trials across all participants).[1] The reason for this difference is that targets on ignore trials could be one of three colors, while targets on neutral trials could be one of four colors. Thus, the number of possible target repetitions was higher for ignore trials. To explore the effect of this imbalance of probabilities, we conducted an additional analysis in which we removed all target-repetition trials. A 2 (trial type) × 4 (block) repeated measures ANOVA revealed that even when we discounted the possibility that target repetitions facilitated the benefits shown on ignore trials, the results were the same. Specifically, we found no main effect of trial type, $F(1, 24) = 0.512$, $p = .481$. We found a significant main effect of block, $F(3, 72) = 9.311$, $p < .001$, $\eta_p^2 = .28$; RTs decreased over the course of the experiment. Finally, there was still a significant interaction between trial type and block, $F(3, 72) = 5.974$, $p < .01$, $\eta_p^2 = .2$. Therefore, it seems the benefit of learning to ignore a particular color was not facilitated by more target-repetition trials in the ignore condition than in the neutral condition.

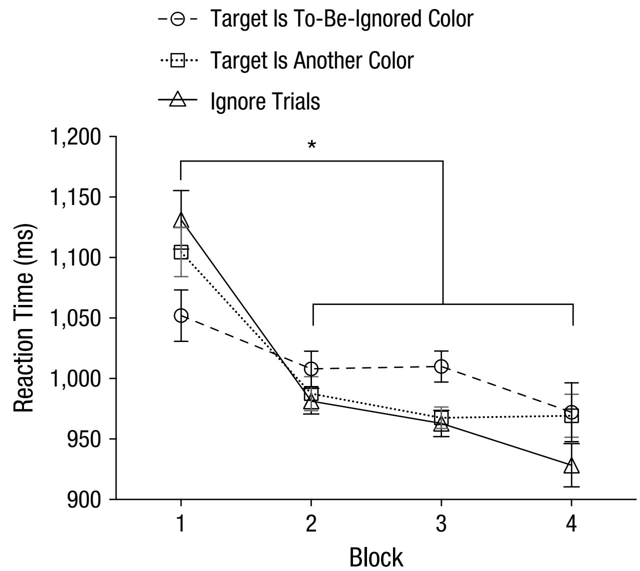

Fig. 3. Mean reaction time (RT) on the two types of neutral trial in Experiment 1, separately for each block of 180 trials. Neutral trials were separated according to whether the target was in the color that participants had been instructed to ignore on ignore trials or in a different color. RTs for ignore trials are included for comparison purposes. Asterisks indicate significant differences between RTs for each block (*$p < .05$). Error bars show ±1 *SEM* calculated within participants using the method of O'Brien and Cousineau (2014).

Discussion

Experiment 1 reveals a surprising result: Inhibiting nontarget-feature information can be costly or beneficial depending on how much the participant has learned about the to-be-ignored feature. Furthermore, the cost in RT for ignore trials compared with neutral trials suggests that participants were unable to use a template for rejection early in practice to facilitate efficient feature inhibition. However, after a few hundred trials of learning to ignore the to-be-ignored feature, participants showed a benefit (i.e., faster RTs). This demonstrates that while participants cannot create a template for rejection immediately, they can after learning to ignore a particular to-be-ignored feature.

Experiment 2

In Experiment 1, we demonstrated that when participants learned to inhibit to-be-ignored feature information, they searched more efficiently, compared with when they were provided with no information. However, some critical questions remain. First, how general are the results from Experiment 1? Specifically, will similar results be obtained under different experimental conditions? Second, do participants learn to ignore the to-be-ignored feature or are they merely learning to attend to the other possible target colors? Finally, do the same effects demonstrated in Experiment 1 remain when interference on neutral trials is removed (i.e., if the to-be-ignored color never shows up on neutral trials)?

To address these questions, we conducted two follow-up experiments. In Experiment 2a, participants were provided with a to-be-ignored color cue on ignore trials, but, unlike in Experiment 1, the to-be-ignored information changed from trial to trial. Additionally, participants viewed a stimulus display somewhat similar to one used by Arita et al. (2012); instead of ignoring 1 item on ignore trials, as in Experiment 1, participants could ignore 6 out of 12 items (i.e., these 6 to-be-ignored items were all in the same color, while the other 6 items were all different from one another and selected from a set of seven colors). Experiment 2b was essentially the same as Experiment 2a, except that the to-be-ignored color remained consistent throughout the experiment.

Method

Participants. A group of 52 Johns Hopkins University undergraduate students and community members (mean age = 22.9 years; 10 male, 42 female) with normal or corrected-to-normal visual acuity and normal color vision participated in the experiments (26 participants were assigned to each experiment). A power calculation similar to the one used in Experiment 1 led us to stop data collection once we reached 26 participants. The participants received extra credit in undergraduate courses or monetary payment as compensation and gave informed consent. The Johns Hopkins Homewood Institutional Review Board approved the protocol.

Stimuli, design, and procedure. Except as noted, the stimuli, design, and procedure were the same as in Experiment 1. The key difference was the number of stimuli and their arrangement in the search displays (see Fig. 4). Stimuli consisted of 12 capital letters from the English alphabet displayed around a central fixation cross. Each letter was randomly assigned to appear in 1 of 12 locations, and the 12 letters were equally spaced on the circumference of an imaginary circle. The distance between fixation and the closest edge of each letter subtended 5.5° of visual angle. As in Experiment 1, either a "B" or an "F" was selected randomly as the target letter on every trial. Additionally, 11 other uppercase letters were selected as the nontarget items. Nontarget letters were chosen from the other 24 letters from the English alphabet. Eight colors were used: red, blue, green, yellow, pink, purple, orange, and aqua.

On neutral trials, six letters were selected randomly to appear in the same color (we refer to this as the *majority color*). The other six letters each appeared in a different color, which was selected randomly without replacement. The spatial location of all colored letters was randomly selected so that stimuli would not be grouped by

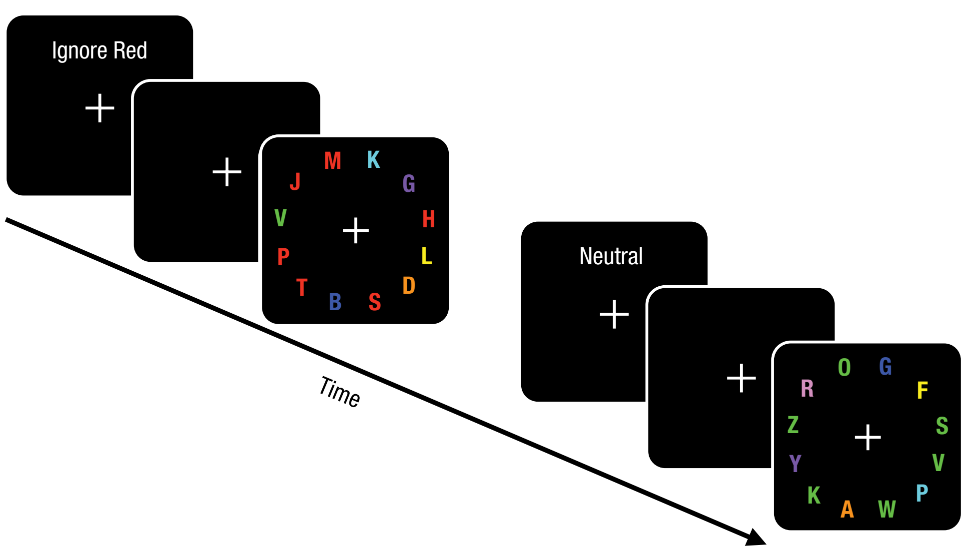

Fig. 4. Examples of the two trial types, ignore (left) and neutral (right), in Experiments 2 and 2b. Cues gave the same information as in Experiment 1, and participants again searched for a target letter ("B" or "F") in the stimulus display. However, the display in Experiments 2a and 2b consisted of a circular pattern of 12 letters, 6 of which were the same color, whereas the other 6 each were a different color. There were 720 trials, half of which were ignore trials and half of which were neutral trials, randomly intermixed. Crucially, in Experiment 2a, the to-be-ignored color varied from trial to trial, whereas in Experiment 2b, the to-be-ignored color was the same from trial to trial, as in Experiment 1.

color. On neutral trials for both Experiments 2a and 2b, the target letter could appear randomly either in the majority color or as any of the other colors. Thus, a color cue was not informative on neutral trials.

On ignore trials, the to-be-ignored color was always the majority color. Additionally, the target was never in the to-be-ignored color; rather, it was always one of the heterogeneously colored letters (see Fig. 4). Therefore, participants could benefit from the to-be-ignored information by knowing that the target was not the to-be-ignored color, thus they could attempt to ignore 6 out of 12 items. Critically, in Experiment 2a, the to-be-ignored color varied from trial to trial; thus, the participant could not learn anything consistent about the to-be-ignored information. However, in Experiment 2b, the to-be-ignored information was held constant for the entire experiment (e.g., a participant could ignore red on all ignore trials). The specific to-be-ignored color was randomly assigned for each participant. As on neutral trials, the spatial location of all colored letters was randomly selected so that stimuli would not be grouped by color. Participants completed 720 trials total, 50% of the trials were ignore trials and 50% neutral trials, randomly intermixed. Experimental sessions lasted 60 to 75 min.

Results

For Experiments 2a and 2b, we removed responses that were faster than 100 ms and more than 3.5 standard deviations above or below the mean. This resulted in the elimination of 1% of all trials for each experiment. Additionally, we removed all trials with errors from the analysis, which accounted for approximately 3% of all trials for each experiment. Finally, to analyze the effect of condition across the duration of the experiments, we divided the trials into ten 72-trial blocks. Mean RTs for all included trials are given in Table 2.

For both experiments, we calculated a mean difference score by subtracting the RT on neutral trials from the RT on ignore trials (see Fig. 5). This was done for each participant for each block. Although we present a difference score in Figure 5 for clarity, all analyses were conducted on raw RTs.

We first ran a 2 (trial type) × 10 (block) repeated-measures ANOVA for Experiment 2a. We found no significant effect of trial type, $F(1, 25) = 0.006$, $p = .939$, and no significant interaction of trial type and block, $F(9, 225) = 1.555$, $p = .13$. Therefore, it does not appear that participants acquired a template for rejection, contrary to the findings of Arita et al. (2012). However, as Beck and Hollingworth (2015) point out, the inhibitory benefit found by Arita et al. (2012) was very likely supported by the fact that participants could rely on a simple spatial template. This was because all of the to-be-ignored items in their study were grouped in one hemifield, while the other items were located in the opposite hemifield. Therefore, it seems that when to-be-ignored information is variable (and stimuli are not grouped by color), participants have a difficult time developing a beneficial template for rejection.

In contrast, in Experiment 2b, we found a significant benefit of learning about the to-be-ignored color, similar to the findings in Experiment 1. While there was a significantly greater RT cost in Block 1 on ignore trials than on neutral trials, participants were able to learn to successfully ignore the to-be-ignored color, which resulted in a greater RT benefit on ignore trials than on neutral trials in Blocks 2 through 10 (see Fig. 5). A 2 × 10 repeated-measures ANOVA revealed a significant effect of trial type, $F(1, 25) = 36.821$, $p < .001$, $\eta_p^2 = .6$, and a significant interaction of trial type and block, $F(9, 225) = 3.235$, $p < .01$, $\eta_p^2 = .12$. Furthermore, we conducted a 2 (experiment) × 2 (trial type) × 10 (block) three-way ANOVA, in which we found that the interaction in Experiment 2b

Table 2. Mean Reaction Times (in Milliseconds) for Experiments 2a and 2b

	Experiment 2a		Experiment 2b	
Block	Ignore trials	Neutral trials	Ignore trials	Neutral trials
Block 1	1,971 (457)	1,965 (529)	1,989 (451)	1,927 (434)
Block 2	1,813 (491)	1,869 (541)	1,723 (346)	1,909 (437)
Block 3	1,761 (532)	1,819 (507)	1,632 (419)	1,733 (486)
Block 4	1,748 (577)	1,793 (595)	1,623 (555)	1,727 (476)
Block 5	1,736 (427)	1,708 (457)	1,552 (358)	1,688 (432)
Block 6	1,737 (438)	1,724 (450)	1,577 (356)	1,716 (512)
Block 7	1,723 (443)	1,627 (361)	1,596 (403)	1,683 (349)
Block 8	1,678 (475)	1,705 (435)	1,670 (694)	1,801 (681)
Block 9	1,649 (528)	1,651 (490)	1,595 (464)	1,741 (484)
Block 10	1,659 (563)	1,626 (538)	1,573 (426)	1,748 (518)

Note: Standard deviations are given in parentheses.

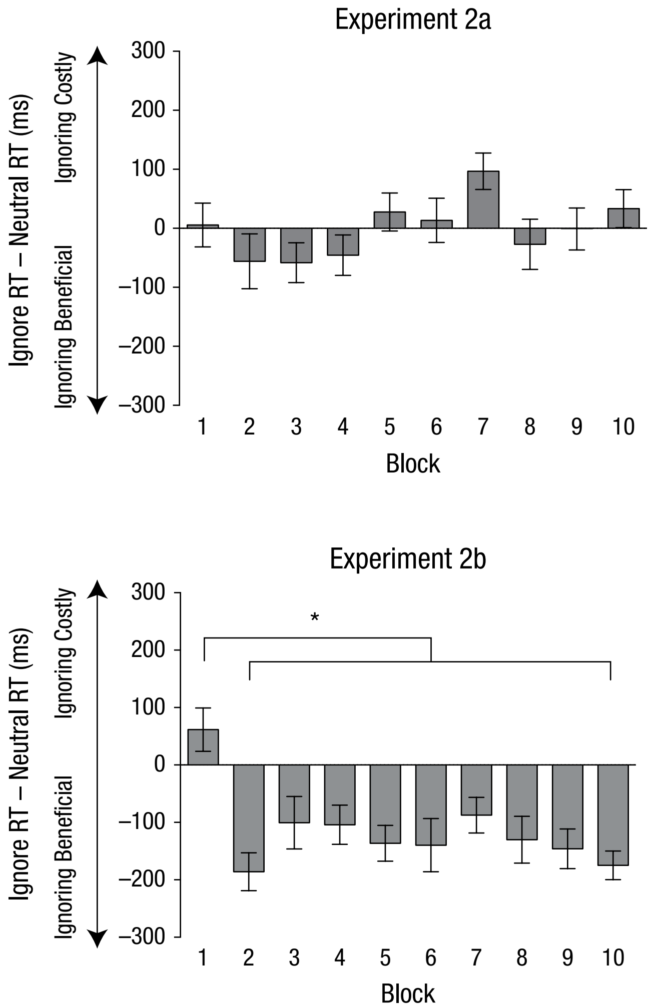

Fig. 5. Mean difference in reaction time (RT) between ignore and neutral trials in Experiments 2a and 2b, separately for each block of 72 trials. Asterisks indicate significant differences between RTs for each block (*$p < .05$). Error bars show ±1 *SEM* calculated within participants using the method of O'Brien and Cousineau (2014).

was significantly different from the interaction in Experiment 2a, $F(9, 450) = 2.106$, $p < .05$, $\eta_p^2 = .04$.

Additionally, we examined whether target-color repetitions (e.g., the target "B" is green on a given trial, and the target "F" is green on the following trial) played a role in the benefit of learning to ignore. Recall that in Experiment 1, we found that the learning-to-ignore benefit was not facilitated by more target-repetition trials in the ignore condition than in the neutral condition. In Experiment 2b (in which participants learned a to-be-ignored color), there were a similar number of target-repetition trials in ignore trials and neutral trials (average of 14% of trials across all participants) because of the large number of colors available in both tasks. However, we still examined whether the benefit of learning to ignore was facilitated by target repetitions. Specifically, we conducted a 2 (trial type) × 10 (block) repeated measures ANOVA in which we removed all target-repetition trials for Experiment 2b (in which participants received consistent to-be-ignored cues). This analysis revealed that, again, even when we removed the possibility that target repetitions were facilitating the benefit shown on ignore trials, the same results were seen. Specifically, we found a main effect of trial type, $F(1, 25) = 44.645$, $p < .01$, $\eta_p^2 = .641$. We also found a significant main effect of block, $F(9, 225) = 5.819$, $p < .01$, $\eta_p^2 = .189$; RTs decreased over the course of the experiment. Finally, we found that there was still a significant interaction between trial type and block, $F(9, 225) = 2.621$, $p < .05$, $\eta_p^2 = .1$. Therefore, it seems that the learning-to-ignore benefit was not facilitated by target repetitions, which mirrors the results of Experiment 1.

We also found an interesting interaction between target repetitions (target repetition vs. no target repetition) and trial type (ignore vs. neutral). Target repetitions speeded search for neutral trials—RT was longer on trials without target repetition (*M* = 1,770 ms) than on trials with target repetition (*M* = 1,738 ms). However, the opposite was true for ignore trials—RT was shorter on trials without target repetition (*M* = 1,642 ms) than on trials with target repetition (*M* = 1,696 ms). We ran a 2 (target repetition) × 2 (trial type) repeated measures ANOVA and found a significant main effect of trial type, $F(1, 25) = 13.2$, $p < .05$, $\eta_p^2 = .35$, but no significant effect of target repetition, $F(1, 25) = 0.617$, $p = .440$. We found a significant interaction between trial type and target repetition, $F(1, 25) = 6.753$, $p < .05$, $\eta_p^2 = .213$. These results suggest that while target repetitions benefitted participants on neutral trials (as is normally the case), they actually hindered participants on ignore trials. Perhaps because participants were learning to ignore a specific feature, anything conflicting with that information in visual working memory resulted in interference. Therefore, not only did target repetitions not facilitate the learning-to-ignore RT benefit that we demonstrated, but also they seemed to interfere with search on those trials.

Discussion

In Experiments 2a and 2b, we replicated and extended the results of Experiment 1 by demonstrating with rather different displays that participants can learn to ignore specified feature information. Additionally, in Experiment 2b, the RT benefit on ignore trials appeared more quickly than in Experiment 1. Because the displays in Experiments 1 and 2b were not identical, we cannot make strong statements about the learning rates in the two experiments. Nevertheless, it does seem notable that evidence of a significant ignoring benefit was evident after just 72 trials in Experiment 2b, while in Experiment 1 such a benefit was not evident until near the end of the session, even though many more colors were used in Experiment 2b (eight) than in Experiment 1 (four). Additionally, Arita et al. (2012) found that the benefit of top-down cues was much greater for larger visual-set sizes than for smaller visual-set sizes. For the current experiments, participants in Experiment 1 could ignore 1 out of 4 items initially, while participants in Experiment 2 could ignore 6 out of 12 items initially. Therefore, the rapid emergence of the ignoring benefit in Experiment 2b is likely a function of the number of items that participants could ignore.

If participants were learning to attend to the other colors rather than learning to inhibit the to-be-ignored color, then learning should have been more difficult in Experiment 2b. Chao (2010), using similar displays to those we used in Experiment 1, explicitly compared a condition in which one location was cued to be ignored with a condition in which the other three locations were cued to be attended. When three possible locations were precued, participants could not use this information to improve target detection. Allen and Humphreys (2007) found similar results using a preview search task, in which a number of distractors is presented before the onset of the rest of the search display. Finally, in our Experiment 2b, eight colors were used, which is well beyond the capacity of visual working memory (e.g., Cowan, 2010).

General Discussion

The current study provides a novel demonstration that knowledge about nontargets can improve search performance. Across two experiments, we presented evidence that learning to ignore to-be-ignored information can result in a benefit that is modulated by participants' time spent learning about the to-be-ignored feature. Additionally, the present study goes beyond previous research by demonstrating that the benefits of learning to ignore consistent distractor information are not only influenced by time spent learning, but also they are affected by the utility of that information. In Experiment 1 after a few hundred trials, participants clearly showed an RT benefit. We compared the time course of learning in Experiment 1 to Experiment 2, where participants could ignore multiple items in the display rather than just one item, and found that the benefit of learning to ignore emerged after only 72 trials. Results from Experiment 2 demonstrated that, within the same task, participants benefited only from to-be-ignored cues when those cues were consistent, which allowed participants to learn to ignore them. Overall, the current study presents new evidence detailing the circumstances in which negative cuing of distractors can facilitate the speed of visual search.

The current results are supported by a number of previous studies demonstrating the benefit of attentional sets that develop with sufficient learning (Cosman & Vecera, 2014; Gal et al., 2009; Geng, 2014; Moher et al., 2014; Zehetleitner et al., 2012). On the face of it, it might look like the present design—with its explicit verbal instructions—resulted in explicit learning of an attentional set. However, it is difficult to argue that no form of implicit learning occurred. Previous research has shown that observers are very sensitive to statistical regularities in visual search displays (e.g., Turk-Browne, Jungé, & Scholl, 2005). It would be interesting in further research to compare the efficacy of explicit and implicit learning in the acquisition of an attentional set to ignore a specific feature.

The present experiments demonstrate circumstances in which the benefit of distractor inhibition arises from consistent learning; however, the specific attentional mechanisms that support such behavior have yet to be determined. Belopolsky, Schreij, and Theeuwes (2010) demonstrated in a series of attentional-capture experiments that top-down goals (e.g., "find the square") do not result in the filtering out of irrelevant salient objects. Rather, they demonstrated that participants will still select salient irrelevant stimuli, but they will subsequently rapidly disengage from those stimuli that do not match the top-down goal (e.g., a bright red triangle). It seems possible that participants in the present study utilized a similar mechanism.

Although the rapid-disengagement hypothesis is not universally accepted (e.g., Chen & Mordkoff, 2007; Eimer & Kiss, 2008; Folk & Remington, 2006; McDonald, Green, Jannati, & Di Lollo, 2013), it should be noted that the context for all of these critiques is attentional capture by salient singletons. In the present study (as in Moher & Egeth, 2012), there were no salient singletons; that is, all items were equally salient. It is possible that some form of rapid disengagement is operative in such circumstances. If rapid disengagement is not responsible, what other kind of mechanism might account for the observed change in performance? Some investigators have argued that active suppression can prevent the allocation of attention to known distractors (e.g., Sawaki, Geng, & Luck, 2012). Distinguishing the role of active suppression and rapid disengagement in the circumstances of the present experiments will require additional empirical efforts.

It seems that learning plays a major role in whether feature inhibition results in a cost or a benefit. The ability to hone selection mechanisms to efficiently choose information to attend and information to ignore is critical for understanding how observers strategically search within visual environments.

Author Contributions

Both authors contributed to the study design. Testing and data collection were performed by C. A. Cunningham. C. A. Cunningham analyzed and interpreted the data under the supervision of H. E. Egeth. C. A. Cunningham drafted the manuscript, and H. E. Egeth provided critical revisions. Both authors approved the final version of the manuscript for submission.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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