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The ignoring paradox: Cueing distractor features leads first to selection, then to inhibition of to-be-ignored items

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Abstract Observers find a target item more quickly when they have foreknowledge of target-defining attributes, such as identity, color, or location. However, it is less clear whether foreknowledge of nontarget attributes can also speed search. Munneke, Van der Stigchel, and Theeuwes *Acta Psychologica* 129:101–107, (2008) found that observers found the target more quickly when they were cued to ignore a region of space where a target would not appear. Using a similar paradigm, we explored the effects of cueing nontarget features on search. We found that when we cued observers to ignore nontarget features, search was slowed. The results from a probe-dot detection task revealed that this slowing occurred because, paradoxically, observers initially selected an item appearing in the to-be-ignored color. Finally, we found that cueing nontarget features sped search when placeholders matching the location of the to-be-ignored color preceded presentation of the search display by at least 800 ms; thus, it appears that some limited inhibition of to-be-ignored items occurs following selection. Taken together, these results suggest that observers are unable to explicitly avoid selection of items matching known nontarget features. Instead, when nontarget features are cued, observers select the to-be-ignored feature or the locations of objects matching that feature early in search, and only inhibit them after this selection process.

Keywords Inhibition · Attention · Visual search

Observers are often faced with overwhelmingly complex visual environments. Some basic visual features can be acquired "preattentively," meaning that they can be encoded in parallel across the visual field (e.g., Neisser, 1967). However, selection of an individual item for further processing is a capacity-limited process. This means that searching in a scene for a complex target, such as a set of keys on a cluttered desk, can be a difficult task; selection must move through the scene until the target is found.

This selection process need not be random, however. Selection can be biased towards target-matching attributes that may be available preattentively, such as target location or color (e.g., Duncan, 1981; Egeth, Virzi, & Garbart, 1984; Green & Anderson, 1956; Posner, 1980; Zhang & Luck, 2009). For example, an observer might quickly find a friend in a crowd if he or she knows that the friend is wearing a red shirt. Inhibition is also known to play a critical role in general visual processing, both behaviorally (Volkmann, Schick, & Riggs, 1968) and at a neuronal level (Morrone, Burr, & Maffei, 1982). Therefore, it seems plausible that deprioritization of nontarget attributes might also play a role in guiding selection.

Numerous studies have shown that deprioritization of nontarget attributes can guide the selection process *implicitly* (meaning in the absence of the observer's intention). For example, Posner and Cohen (1984) demonstrated that when a location is exogenously cued, selection of a target at that location is slowed if there is a long enough lag between the cue and the onset of the target, an effect known as *inhibition of return*, or IOR. Features, too, can be implicitly inhibited. In *negative-priming* (NP) studies, participants are slower to respond to targets that were recently rejected as distractors (Fox, 1995; Tipper & Cranston, 1985). The *distractor-previewing effect* (DPE) is an example of implicit inhibition of specific colors (Goolsby, Grabowecky, & Suzuki, 2005; Goolsby & Suzuki, 2001; Lleras, Kawahara, Wan, & Ariga, 2008), in which selection of target items appearing in a recently deprioritized color is delayed (Lleras et al., 2008; Shin, Wan,

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Fabiani, Gratton, & Lleras, 2008). And in *visual-marking* paradigms, observers inhibit, and thus avoid selection of, a previewed set of nontarget items that spatially overlap with subsequently presented potential target items (Humphreys, Watson, & Jolicœur, 2002; Watson & Humphreys, 1997; Watson, Humphreys, & Olivers, 2003; but see Donk & Theeuwes, 2001, for a bottom-up account of this effect). Furthermore, observers find the target more slowly when it matches the dominant color of the previewed set, suggesting a carryover feature-based inhibitory effect (Braithwaite & Humphreys, 2003).

These phenomena demonstrate that inhibitory mechanisms exist that can guide selection. However, explicitly accessing these inhibitory mechanisms in a goal-directed manner poses unique challenges. One problem is that guidance of selection is heavily influenced by the contents of working memory (Downing, 2000; Pashler & Shiu, 1999; Soto, Heinke, Humphreys, & Blanco, 2005; Soto, Hodsoll, Rotshtein, & Humphreys, 2008; but see Downing & Dodds, 2004; Han & Kim, 2009; Olivers, 2009; Woodman & Luck, 2007, for accounts of a more flexible relationship). Working memory mechanisms are typically recruited to process explicit task cues, including those instructing participants to ignore a specific feature or location. For example, if a participant were instructed to "ignore the red thing," then the contents of working memory would include a representation of red, which might draw attention to red items. Therefore, for explicit inhibition to be effective, an observer must overcome the tendency of the contents of working memory to automatically guide the selection process. Furthermore, explicit inhibition in general can lead to paradoxical effects in which observers fixate on the item that they are trying to inhibit (e.g., "white bear" effects; Tsal & Makovski, 2006; Wegner, 1994).

Despite these obstacles, a study by Munneke, Van der Stigchel, and Theeuwes (2008) found evidence that participants could identify a target more quickly when they were cued to ignore a nontarget location prior to the display onset. Munneke et al. had participants search for a capital "B" or "F" target that could appear in one of four fixed locations on the screen. Prior to some trials, one of the four locations was cued to indicate that the target would *not* appear in that location on the upcoming trial. Munneke et al. found that responses were faster following these trials, as compared to following neutral trials in which no locations were cued. Additionally, a lowercase "b" or "f" was present on all trials, creating a compatibility effect similar to that observed in the Eriksen flanker task (Eriksen & Eriksen, 1974). On cued trials, the lowercase "b" or "f" appeared in the cued (to-be-ignored) location. Munneke et al. found less interference from these lowercase letters on cued than on neutral trials, indicating that participants were actively inhibiting processing of the letter at the to-be-ignored location. Chao (2010) found converging evidence for speeded response times when participants were cued to ignore a specific nontarget location (although Chao did not replicate Munneke et al.'s finding of reduced interference from distractor letters at those locations), and several other studies have provided evidence that cueing participants to ignore or inhibit nontarget locations can speed search (Ruff & Driver, 2006; Serences, Yantis, Culberson, & Awh, 2004; Van der Stigchel, Heslenfeld, & Theeuwes, 2006).

Thus, it appears that inhibitory mechanisms can be explicitly accessed in order to ignore known nontarget locations. However, it is less clear whether inhibitory mechanisms can also be explicitly accessed when nontarget features (other than location) are cued. Friedman-Hill and Wolfe (1995) found observers could not efficiently locate a target when they were cued to look among a subset of items defined as not matching a specific color (e.g., find the oddly oriented nonred item). In contrast, Woodman and Luck (2007) found that observers located a target more quickly if distractor items in a to-be-ignored color were present than if no distractor items matched the to-be-ignored color. They concluded that observers used a "template for rejection" whereby selection of items matching the to-be-ignored feature could be avoided during search, and thus set size was effectively reduced when items appeared in the to-be-ignored feature, because those items could be immediately rejected (Woodman & Luck, 2007).

These studies provide conflicting evidence for the efficacy of inhibition when nontarget features are cued. However, these studies have not directly examined whether or not performance on a search task improves when observers know what feature to avoid selecting, relative to performance with similar displays in which observers do not have access to this information. In Woodman and Luck (2007), for example, a nontarget color was cued on every trial. Therefore, it is unclear whether initiating a "template for rejection" actually improves performance relative to baseline neutral trials in which no information about upcoming distractor features is available. If efficient inhibition is possible when a nontarget feature is cued, we would predict an increase in search efficiency on cued trials relative to neutral trials.

In the present experiments, we directly compared neutral trials to trials in which a to-be-ignored color was cued. In Experiment 1, we used a modified version of the paradigm from Munneke et al. (2008). Our participants were instructed to identify a target letter ("B" or "F") among four differently colored letters. On a randomly selected 50 % of all trials, observers were cued to ignore a specific color prior to the appearance of these letters. These "ignore" cues validly indicated that the target would not appear in the cued color on the upcoming trial. On the remaining trials, participants received "neutral" cues that provided no information about target or distractor colors on the upcoming trial.

If efficient inhibition is possible when nontarget features are cued (e.g., Woodman & Luck, 2007), the set size of the display should be effectively reduced to three on ignore-cued trials, as participants would avoid selecting the item appearing in the to-be-ignored color. Thus, participants should find the target more quickly on ignore-cued trials than on neutral-cued trials. If this type of inhibition cannot be explicitly used to find the target (e.g., Friedman-Hill & Wolfe, 1995), there should be no effect of cue type. A third alternative is that ignore cues could trigger an inefficient search strategy; for example, observers might select item(s) appearing in the to-be-ignored color[1] while trying to inhibit them (cf. the "attentional white bear" effect; Tsal & Makovski, 2006). This would result in slower response times on ignore-cued trials than on neutral-cued trials.

Experiment 1

Method

Participants A group of 22 Johns Hopkins University undergraduate students and community members (mean age = 19.6 years; 12 male, 10 female) with normal or corrected-to-normal visual acuity and normal color vision participated in sessions lasting 30–60 min. The participants received extra credit in undergraduate courses or monetary payment as compensation and gave informed consent. The protocol was approved by the Johns Hopkins Homewood Institutional Review Board.

Apparatus Images were displayed on a Dell Precision T-3400 2.33-GHz computer with a Dell 1708 FP monitor. Stimulus presentation and data analysis were performed using programs written in MATLAB (The Mathworks) and using the PsychToolbox software (Brainard, 1997). The screen had a refresh rate of 60 Hz, and the resolution of the screen was 1,280 × 1,024 pixels.

Stimuli On each trial, four English letters appeared surrounding a fixation cross that appeared at the center of the screen, subtending 0.55 deg of visual angle at a viewing distance of approximately 60 cm. These letters were randomly assigned to appear in one of four locations (surrounding fixation) at 0 °, 90 °, 180 °, and 270 ° (polar angle) from vertical. Each letter subtended a visual angle of 0.86 deg, and the distance between fixation and the closest edge of each letter subtended 4.96 deg of visual angle.

On every trial, either a capital "B" or "F" was selected randomly to appear as the target letter. Additionally, a lowercase "b" or "f" distractor letter was selected randomly to appear. This lowercase "b" or "f" was either *compatible* with the target letter, meaning that it shared the identity of the uppercase "B" or "F," or *incompatible*, meaning that it did not share the identity of the uppercase letter. The remaining two letters were a "k" and an "x" on every trial, one of which was chosen randomly to be uppercase. Each of the four letters was selected randomly without replacement to appear in one of four colors (with CIE chromaticity coordinates as follows: red, $x = .6383$, $y = .3291$; blue, $x = .1886$, $y = .1564$; green, $x = .2906$, $y = .5663$; and yellow, $x = .4193$, $y = .5052$). These colors were linearly separable in CIE color space (cf. Bauer, Jolicœur, & Cowan, 1998).

Prior to each trial, a cue appeared at the center of the screen (Fig. 1). Each cue initially appeared as a gray border

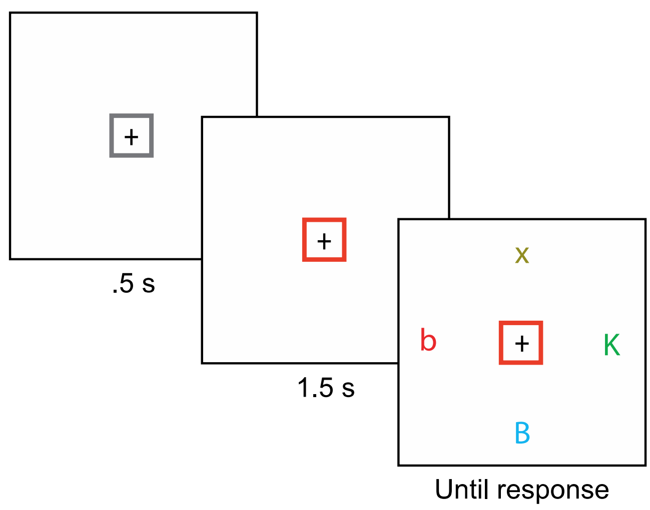

Fig. 1 Sample trial for Experiment 1. Observers were asked to indicate whether a capital "B" or "F" was present as quickly and accurately as possible. Prior to a randomly selected half of all trials, a color frame indicated that the target would not appear in that color on the upcoming trial (as pictured). On the remaining trials, a neutral cue appeared (a frame in which each pixel was randomly chosen to appear in one of the four possible target colors), giving the participant no information about the color of the distractor on the upcoming trial. The trial shown is "compatible," meaning that the item appearing in the to-be-ignored color is compatible with the target response. This and all other figures are not to scale; colors are adjusted for publication clarity

[1] We note here that in the present paradigm, the cue indicates a to-be-ignored feature only (i.e., the location of a to-be-ignored item is unknown). Nevertheless, selection and/or inhibition may occur through feature-based mechanisms or through location-based mechanisms, whereby observers localize an item or items matching the to-be-ignored feature and select and/or inhibit those locations. Except where explicitly stated otherwise, we intend the phrase "select item(s) appearing in the to-be-ignored color," and similar phrasing throughout this article referring to selection or inhibition of an "item" or "items," to be agnostic as to whether the selection or inhibition in the present study occurred on the basis of features or locations. See the General Discussion for more details.

of an empty square box, 0.76 deg of visual angle on a side with a stroke width of 0.1 deg. On each trial, the cue was randomly assigned to be either a neutral cue or an ignore cue. For a neutral-cued trial, each pixel in the border was randomly assigned to one of the four colors that the letters would appear in, making the border of the box appear multicolored. For an ignore-cued trial, the entire border was filled with one of the four possible colors (whichever color was assigned to the lowercase "b" or "f" distractor on that trial), such that the border appeared as a solid color.

Design and procedure Each trial began with the presentation of the gray box at the center of the screen for 500 ms. Following this, the box changed color to either an ignore or a neutral cue for 1,500 ms. Participants were informed that ignore cues indicated that the target would not appear in the to-be-ignored color (the color of the cue) on the upcoming trials. These cues were 100 % valid. On ignore trials, the letter appearing in the to-be-ignored color was always the distractor "b" or "f." Neutral cues gave the participants no additional information about the color of the target or nontarget items on the upcoming trial. Following cue presentation, the four letters appeared on the screen and remained until participants responded. The participants were told to determine whether a capital "B" or "F" was present. They indicated their response by pressing the "z" key if a "B" was present and the "/" key if an "F" was present. They were instructed to respond as quickly and accurately as possible. Following their response, a blank black screen was presented for a 500-ms intertrial interval.

Participants began the experiment with 36 uncued training trials. Following each of these training trials, participants were informed whether they had answered correctly with the presentation of the word "Correct" or "Incorrect" at the center of the screen, subtending 0.57 deg of vertical visual angle and approximately 6.68 deg of horizontal visual angle. This feedback appeared only for the training portion of the experiment.

Following the initial training phase, a cue was presented before each trial for the remainder of the experiment. There were ten blocks of 50 trials, the first of which was considered a practice block. After each block, participants were given an opportunity for a brief rest.

Results and discussion

We removed all responses faster than 100 ms from the analyses. We subsequently used a modified recursive trimming procedure (Van Selst & Jolicœur, 1994) to remove response times 3.5 standard deviations above or below the mean within each experimental condition for each participant. This resulted in the elimination of 2.5 % of all trials. Following this procedure, all error trials were removed from the response time analyses, resulting in the removal of 4.7 % of all of the remaining trials. Two participants were replaced due to poor overall performance (<70 %).

We performed a 2 × 2 repeated measures analysis of variance (ANOVA) on cue type (ignore vs. neutral) and compatibility (compatible vs. incompatible; i.e., whether the lowercase letter shared the identity of the uppercase letter) for both response times and accuracy. As expected, we found a main effect of compatibility on response times, with faster responses on compatible trials (753 ms) than on incompatible trials (809 ms), $F(1, 15) = 58.66$, $p < .001$. There were also more errors on incompatible trials (5.4 %) than on compatible trials (4 %), an effect that approached significance, $F(1, 15) = 3.61$, $p < .08$.

Surprisingly, responses on ignore trials (798 ms) were *slower* than responses on neutral trials (764 ms), $F(1, 15) = 6.02$, $p < .05$. Furthermore, there were *more* errors on ignore trials (5.5 %) than on neutral trials (3.9 %), $F(1, 15) = 16.32$, $p < .01$ (Fig. 2). There was no interaction between cue type and compatibility for either response times or accuracy, *p*s > .1.

Despite having additional, valid information about the upcoming display, participants responded more slowly and less accurately on ignore than on neutral trials. Furthermore, there was no reduction in the compatibility effect from the to-be-ignored distractor on ignore trials, suggesting that observers were unable to inhibit identity information from the to-be-ignored distractor. These results do not support Woodman and Luck's (2007) notion of a "template for rejection" in which observers can avoid selection of items matching a nontarget feature in order to speed search. Instead, they suggest that search becomes *less* efficient when nontarget features are cued.

It is possible that the color cues used in this experiment produced sensory activation of the to-be-ignored color at a low-level processing stage, resulting in implicit perceptual priming effects (cf. Maljkovic & Nakayama, 1994). Indeed, in this experiment, we found that when the color of the target item was repeated across successive trials, responses were faster (753 ms) than when the color was not repeated (769 ms), $F(1, 15) = 8.95$, $p < .01$. The elapsed time between two trials (at least 2.5 s) was longer than the elapsed time between the cue and the target within a trial (1.5 s). Considering that we found evidence for low-level perceptual priming between successive trials, it is reasonable to conclude that some low-level priming may have occurred in the shorter interval between the cue and the trial, resulting in priming of the to-be-ignored item. Therefore, interpreting the results of Experiment 1 is problematic; we do not know whether observers were unable to inhibit items appearing in the to-be-ignored color in search because of a fundamental limitation in the explicit use of

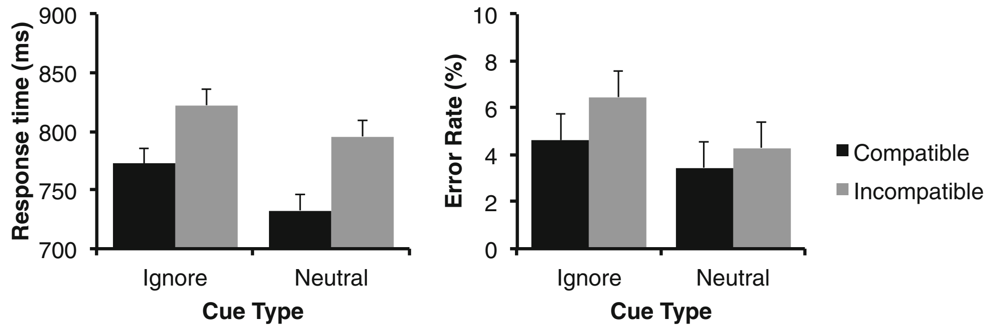

Fig. 2 Results from Experiment 1. Participants responded more slowly and made more errors following ignore cues than following neutral cues. Error bars were calculated from the within-subjects interaction error term (Loftus & Masson, 1994)

nontarget feature information, or whether they were prevented from inhibiting those items because they were drawn to the items by low-level priming effects.

Furthermore, the target display itself was imbalanced with respect to color, as the ignore cue remained on the screen during search (as in Munneke et al., 2008). It may have been harder to ignore an item appearing in the to-be-ignored color when there were two items on the screen appearing in that color, either because that color took up more area on the screen than any other color or because the cue and the to-be-ignored distractor were automatically grouped together due to their shared color. To address these issues in Experiment 2, we replaced color cues with printed verbal instruction cues that disappeared before the onset of the search display.

Experiment 2

Method

Participants A group of 18 Johns Hopkins University undergraduate students and community members (mean age = 19.2 years; seven male, 11 female) with normal or corrected-to-normal visual acuity and normal color vision participated in sessions lasting 30–60 min. The participants received extra credit in undergraduate courses or monetary payment as compensation and gave informed consent. The protocol was approved by the Johns Hopkins Homewood Institutional Review Board. The apparatus was the same as in Experiment 1.

Stimuli The colors and letters used were identical to those used in Experiment 1. Each cue was presented as a printed verbal instruction cue rather than as a color cue. Neutral cues appeared as the word “Neutral” printed in white, and ignore cues appeared as the phrase “Ignore [color]” printed in white. These cues subtended 0.57 deg of visual angle vertically and between 2.86 and 5.92 deg of visual angle horizontally.

Design and procedure The word cue appeared on the screen for 1 s, followed by a white fixation cross at the center of the screen for 1 s. The target display appeared with no cue present at the center of the screen (Fig. 3). In all other respects, Experiment 2 was identical to Experiment 1.

Results and discussion

We used the same trimming procedure as in Experiment 1 (Van Selst & Jolicœur, 1994). This resulted in the elimination of 2.6 % of all trials. Following this procedure, all error trials were removed from the response time analyses, resulting in the removal of 3.7 % of all of the remaining trials.

We conducted the same 2 × 2 repeated measures ANOVA as in Experiment 1 (with the factors Compatibility and Cue Type). As in Experiment 1, we found a main effect of compatibility: Responses were faster on compatible trials (814 ms) than on incompatible trials (849 ms), $F(1, 17) = 8.25$, $p < .05$. There was no effect of compatibility on accuracy, $F(1, 17) < 1$.

Response times were significantly slower on ignore trials (854 ms) than on neutral trials (808 ms), $F(1, 17) = 5.69$, $p < .05$. The error rate was also higher on ignore trials

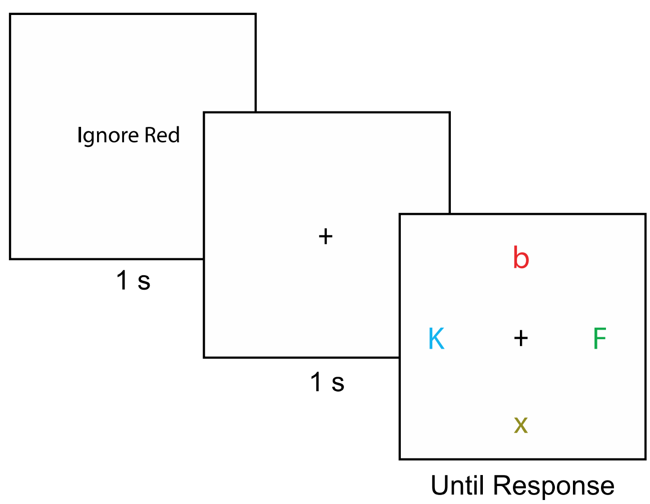

Fig. 3 Sample trial for Experiment 2. Experiment 2 was similar to Experiment 1, except that the cues were printed verbal instructions rather than color cues and the timing was different, as the cue was presented for 1 s, followed by 1 s of fixation, followed by the target display

(4.3 %) than on neutral trials (3 %), $F(1, 17) = 9.07$, $p < .01$ (Fig. 4). Furthermore, the compatibility effect was larger in the ignore condition (57 ms) than in the neutral condition (13 ms), an effect that approached significance, $F(1, 17) = 3.1$, $p < .1$. There was no interaction between compatibility and cue type for accuracy, $F(1, 17) < 1$.

Again, we found that ignore cues did not speed search; instead, search was slower and less accurate following ignore cues in both Experiments 1 and 2. In both experiments, we also failed to find evidence for reduced processing of the identity of the to-be-ignored letter in the form of reduced compatibility effects on ignore trials, suggesting that observers were unable to inhibit the identity of the to-be-ignored item. We even observed a trend toward greater interference from the to-be-ignored item on ignore trials in Experiment 2. Together, these results suggest that cues instructing observes to ignore nontarget features interfere with search.

Why does validly cueing a nontarget feature lead to less efficient search? One possibility is that participants are able to avoid selecting items appearing in the to-be-ignored color during search (e.g., Woodman & Luck, 2007), but that this process of avoiding selection taxes a central limited-capacity resource. This would be consistent with the main effect of cue type observed in Experiments 1 and 2, because although the set size on ignore trials would be effectively reduced by one, the search process itself would be slower overall due to of the limited availability of attentional resources. Thus, selection of the target item on ignore trials could be delayed relative to neutral trials.

Another possibility is that the act of processing the ignore cue itself required more capacity-limited central resources than did the act of processing the neutral cue. This explanation seems unlikely, as 1.5–2.0 s elapsed between the presentation of the cue and the presentation of the search display, but it is possible that the concept of an ignore cue is so unfamiliar to observers that they take a long time to process it. Furthermore, search might have been slowed because participants hold the ignore cue in working memory (but do not need to hold the neutral cue in memory) during the search process itself. According to either of these accounts, the cue should have no impact on whether or not observers successfully avoid selection of an item matching the to-be-ignored feature, but responses on ignore trials would be slower because of overall processing differences between the two cue conditions.

A third possibility is that, following ignore cues, observers select the item that they are supposed to ignore. This would be similar to the "attentional white bear" effect (Tsal & Makovski, 2006), whereby observers use a "process-all" mechanism to select all items or locations where relevant information appears, even if that information is relevant only in the sense that it is *not* the target. In the present design, unlike the attentional white bear effect, observers would be biasing more resources toward known distractors than toward possible targets. This means that the first item selected on ignore trials would be guaranteed not to be the target, which would produce results consistent with data from the first two experiments. The trend toward more interference from the identity of the to-be-ignored item in Experiment 2 also hints at this selection hypothesis.

These three accounts make different predictions about the selection of the to-be-ignored item early in the search process. In order to differentiate among these accounts in Experiment 3, we adapted a probe-dot detection paradigm (cf. Kim & Cave, 1995, 1999) to explore observers' selection process during search when they were given a printed verbal cue to ignore a specific color, as in Experiment 2. We presented the search display for a very brief duration, followed quickly by the presentation of a probe dot at one of the four locations where the letters had previously appeared. We reasoned that if observers were successfully inhibiting the to-be-ignored item, they would respond most slowly to probe dots appearing at the location of the to-be-ignored item. Alternatively, if observers were initially selecting the to-be-ignored item, they would respond faster to probe dots appearing at the location of the to-be-ignored item when the probe dots were presented at a short SOA. We expected this distractor selectivity to diminish over time.[2]

Experiment 3

Method

Participants A group of 16 Johns Hopkins University undergraduate students (mean age = 19.6 years; eight male, eight female) with normal or corrected-to-normal visual acuity and normal color vision participated in sessions lasting 30–60 min. The participants received extra credit in undergraduate courses as compensation and gave informed consent. The protocol was approved by the Johns Hopkins

[2] Although we presented the probe dots at two different stimulus onset asynchronies (SOAs), both SOAs were chosen for the purpose of examining very rapid shifts of attention based solely on the ignore cues. We reasoned that we would not observe target selectivity at such early SOAs because we presumed that letter identification in this task would be a slower process than identification of basic, low-level features such as color. Thus, while these SOAs might be well suited to detect shifts of attention based on color identification, observers would be unlikely to have reliably distinguished the target letter at these SOAs. As expected, there was no main effect of probe-dot location when comparing the target location to other, nontarget locations (not including the to-be-ignored distractor), and there was no interaction with cue type (*p*s > .1). Therefore, to increase power, we conducted all analyses for Experiment 3 with responses to probe dots at the target location collapsed together with responses to probe dots at the other two nondistractor locations (this grouping is hereafter referred to as the "nondistractor probe-dot location").

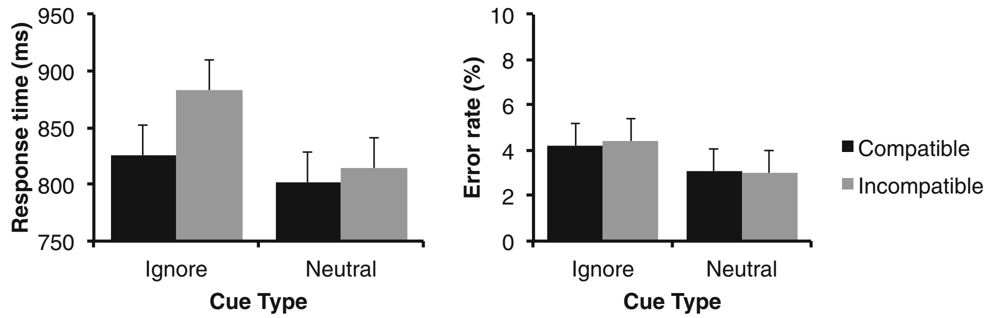

Fig. 4 Results from Experiment 2. Participants responded more slowly and made more errors following ignore cues than following neutral cues. Error bars were calculated from the within-subjects interaction error term (Loftus & Masson, 1994)

Homewood Institutional Review Board. The apparatus was the same as in Experiment 1.

Stimuli The colors and letters used were identical to those in Experiment 2. In addition, on some trials a white dot subtending 0.38 deg of visual angle appeared at one of the four letter locations.

Design and procedure The letters and cue types were identical to those in Experiment 2. Each trial started out with the presentation of a cue for 1 s. Following the cue, a fixation cross was placed at the center of the screen. After another second, the four letters appeared on the screen for a duration of 50 ms. A delay followed this presentation, after which the observers were prompted to indicate whether a "B" or an "F" had been present. They right-clicked the mouse to indicate a "B," and left-clicked the mouse to indicate an "F."

On each trial, there was a 50 % chance that a probe dot would appear. On probe-dot trials, a white dot appeared during the delay interval in one of the four locations where a letter had previously appeared. This dot appeared either 67 or 117 ms after the offset of the letters, and thus 117 or 167 ms after the onset of the letters. The dot remained on the screen for 30 ms. On trials in which no probe dot was present, nothing was presented during this 30-ms interval. Participants were required to press the space bar as quickly as possible to indicate that they had seen the dot. After they responded or after 1,400 ms had elapsed (whichever was shorter), they were prompted to indicate with a mouse click whether they had seen a "B" or an "F" (Fig. 5).

Participants began the experiment with 15 training trials in which a white dot was presented at one of the four possible target locations at random intervals. Participants had to detect the presence of this white dot and to press the space bar in response. Following this, they received 15 training trials of only detecting the "B" or "F," meaning that no probe dot would appear during the delay period. Following each of these training trials, participants were informed whether they had answered correctly with the presentation of the word "Correct" or "Incorrect" at the center of the screen. Next, they received an additional 15 training trials combining these two tasks, meaning that a probe dot could appear during the delay period and that they had to respond to both the probe dot and the target letter. In addition to feedback on the search task, they were given feedback on the probe dot task indicating whether they had correctly detected a probe, missed a probe, or made a false alarm response when no probe had appeared. No cues appeared during any of these training trials, as the trials were meant to allow participants to become familiar with the basic task.

Following the initial, uncued training phase, a cue was presented before each trial for the remainder of the experiment. There were ten blocks of 50 trials, the first of which was considered a practice block. After each block, participants were given an opportunity for a brief rest.

Results and discussion

Search task Although this was not a speeded response task, and participants were instructed to emphasize only accuracy, we nevertheless measured and analyzed response times, and report these data here for the sake of completeness. All error trials were removed from the response time analyses, resulting in the removal of 6.3 % of all trials. Two participants were replaced due to high false alarm rates on the probe-dot task (>25 % false alarm rate).

We performed a 2 × 2 repeated measures ANOVA on cue type and compatibility. There was no main effect of compatibility on response times or accuracy, *p*s > .1.[3] We found a main effect of cue type on accuracy, with more errors on

[3] Though the lack of a compatibility effect is not consistent with the results from the first two experiments, there were key differences between the present experiment and Experiments 1 and 2 (e.g., the brief stimulus presentation and the emphasis on accuracy over response time). Therefore, it may not be surprising that we did not observe any compatibility effects. For example, it may be that with such a brief stimulus presentation, the task became more difficult, and excess cognitive resources were not available to maintain a representation of recently discarded letters. If this was the case, we would not necessarily expect the identity of nontarget letters to interfere with target responses.

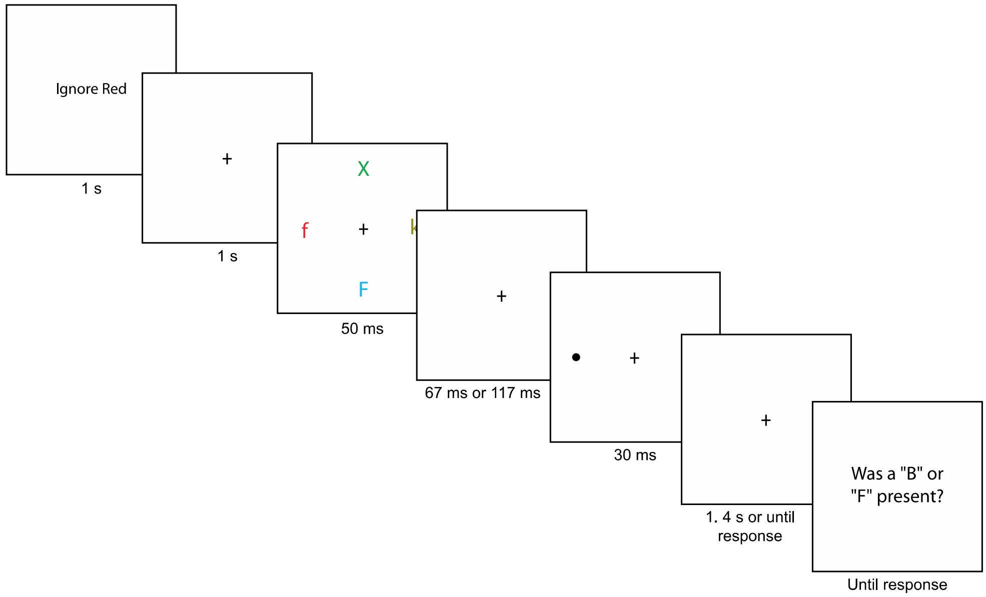

Fig. 5 Sample trial for Experiment 3. Displays were presented briefly, and participants indicated the presence of a “B” or “F” following a delay. A probe dot appeared during the delay on a randomly selected half of all trials at a randomly selected location (from among the four locations where letters had been presented), and participants had to indicate the presence of the probe dot with a buttonpress

ignore trials (6.9 %) than on neutral trials (5.6 %), $F(1, 14) = 11.6$, $p < .01$. Although response time was specifically not emphasized for this task, there was a main effect of cue type on response times, with faster responses on neutral trials (483 ms) than on ignore trials (516 ms), $F(1, 15) = 7.59$, $p < .05$. There was no significant interaction between cue type and compatibility for either response time or accuracy, *F*s < 1. These results confirm that feature-based ignore cues negatively impacted search performance even with a briefly presented display.

Probe-dot task We used the same trimming procedure as in Experiment 1 (Van Selst & Jolicœur, 1994). This resulted in the elimination of 0.7 % of all probe-dot trials. Following this procedure, we also removed trials in which the participants did not correctly identify the target; this resulted in the elimination of 6.3 % of all of the remaining trials.

We conducted a 2 × 2 × 2 repeated measures ANOVA with the factors Cue Type (ignore vs. neutral), Probe-Dot Location (distractor vs. nondistractor), and SOA (short [117 ms] vs. long [167 ms]) for both response times and miss rates.

Miss rates overall were very low (1.8 %). Miss rates were slightly lower at the distractor location (1 %) than at the nondistractor locations (1.9 %), an effect that approached significance, $F(1, 15) = 4.25$, $p < .06$. No other main effects or interactions were significant with respect to miss rates (*p*s > .1); therefore, the remaining discussion will focus on response times only.

Probe response times were slower following ignore cues (503 ms) than following neutral cues (489 ms), $F(1, 15) = 5.67$, $p < .05$. No other main effects or two-way interactions were significant for response times, *p*s > .1.

Our main question was whether early in the search process (i.e., at the short SOA), the distractor item would be selected more frequently than other items when a to-be-ignored feature was cued. If this were the case, we expected that the interaction between SOA and probe-dot location would differ according to cue type, resulting in a three-way interaction between cue type, probe-dot location, and SOA; we did find this significant three-way interaction, $F(1, 15) = 5.72$, $p < .05$.

To better understand the three-way interaction, we conducted 2 × 2 ANOVAs with the factors SOA and Probe-Dot Location for ignore trials and neutral trials separately. On ignore trials at the shorter SOA, response times were faster at the distractor location (483 ms) than at nondistractor locations (512 ms), but at the longer SOA, response times were slower at the distractor location (518 ms) than at nondistractor locations (499 ms); this interaction was significant, $F(1, 15) = 5.16$, $p < .05$ (Fig. 6). We conducted a simple main effects analysis to compare the difference at each SOA. The difference in response times between distractor and nondistractor locations at the short SOA was significant, $F(1, 15) = 4.66$, $p < .05$, but this difference at the long SOA did not reach significance, $F(1, 15) = 1.38$, $p > .1$. For neutral trials, there was no interaction between probe-dot location and SOA, $F(1, 15) < 1$ (Fig. 6).

These data do not support the notion of a “template for rejection” in which observers avoid selection of items matching a to-be-ignored feature, nor do they support the idea that cue effects in the first two experiments occurred only because of different resource demands between the two cue types. Instead, these results demonstrate that when observers were instructed to ignore a nontarget color, they instead selected an item appearing in that color early in the search process, resulting in faster response times to probe

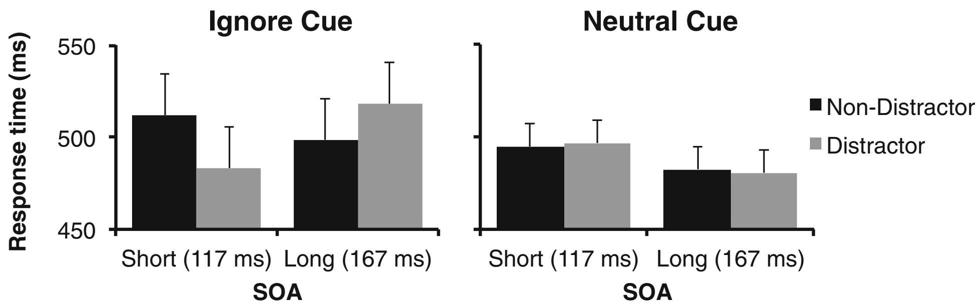

Fig. 6 Probe-dot results from Experiment 3. Following ignore cues, probe-dot responses were significantly faster at the distractor location than at nondistractor locations at the early SOA, but responses were numerically slower at the distractor location at the longer SOA. No interaction emerged between probe-dot location and SOA for neutral trials. Error bars were calculated separately for each SOA and each cue type from the within-subjects error term (Loftus & Masson, 1994)

dots appearing at the location of the to-be-ignored distractor at early probe-dot SOAs. This cannot be due to a bias toward selecting the lowercase "b" or "f," because no such pattern was observed on neutral trials. Therefore, we can conclude that when participants were cued to ignore a non-target feature, they initially selected an item matching that feature.

If inhibition occurs following selection, ignore cues might help the observer by preventing reselection of a nontarget item following that initial selection, and thus biasing selection toward only potential target items. Alternatively, if observers persistently fixate on the to-be-ignored item, and thus constantly devote limited resources to that feature or location during the search process, ignore cues would continue to hinder search even following the initial selection of the to-be-ignored item. At the longer probe-dot SOA, responses to probe dots appearing at the location of the to-be-ignored distractor were slower than responses to probe dots appearing at the locations of other, potential target items (though this difference did not reach significance), tentatively suggesting that the to-be-ignored item might have been inhibited following selection.

In Experiment 4, we further examined the possibility that observers are able to disengage from and inhibit the to-be-ignored item following selection. On each trial, we presented color placeholders prior to the appearance of the search display, which indicated the color and location of the upcoming letters. After a delay, these placeholders were removed to reveal the search letters. Because the presentation of the letters was delayed, but the placeholders indicated where the to-be-ignored item(s) would appear, the observers had enough time to select and reject a particular feature (or the locations of items matching that feature; see the General Discussion) before the letters appeared. Thus, if inhibition is possible following selection in this task, we expected to see ignore-cue benefits when the duration of the placeholder display was sufficiently long.

We varied the duration of the placeholder display in order to explore the time course of selection and inhibition following ignore cues. At short placeholder durations, we predicted a pattern of results similar to our first three experiments—that is, performance should be worse on ignore trials than on neutral trials, because participants would still be in the process of selecting the to-be-ignored items when the letters appeared. At longer placeholder durations, we predicted the opposite effect (an ignore-cue benefit), assuming that inhibition follows selection of the to-be-ignored item. Furthermore, we varied the number of items appearing in each display (4 vs. 12). At set size 12 (SS12), three to-be-ignored items appeared in the display, as compared to only one to-be-ignored item at set size 4 (SS4). If observers were able to inhibit all items appearing in a to-be-ignored color simultaneously, any benefit from the ignore cue observed at the longer SOA should be larger for SS12 than for SS4, because there would be three items (rather than just one item) that could be eliminated from the search space. This allowed us to explore the capacity of any inhibition occurring in this task.

Experiment 4

Method

Participants A group of 32 Johns Hopkins University undergraduate students (mean age = 19.7 years; ten male, 22 female) with normal or corrected-to-normal visual acuity and normal color vision participated in sessions lasting approximately 60 min. The participants received extra credit in undergraduate courses as compensation and gave informed consent. The protocol was approved by the Johns Hopkins Homewood Institutional Review Board. The apparatus was the same as in Experiment 1.

Stimuli On every trial, either four or 12 letters appeared on the screen, each subtending approximately 0.57 deg of visual angle in width and height. These letters appeared in the same four colors as in the first three experiments. In each display were one target "B" or "F," one lowercase "b" or "f," and two or ten distractor letters randomly chosen from capital and lowercase "k," "p," "x," and "o." The letters could appear in one of 100 randomly chosen locations; these locations formed a 10 × 10 matrix surrounding fixation, with a maximum distance from the center to the far edge of the farthest letter of approximately 9.53 deg of visual angle. A minimum distance of 1.43 deg of visual angle separated the edge of one letter from the edge of its nearest neighbor. The same number of letters always appeared in each color (one or three per color, depending on whether the set size was 4 or 12). Prior to the appearance of the letters, solid color squares were placed in the location of the upcoming letters, subtending 0.96 deg of visual angle. These placeholders indicated the color of the letter about to appear in each location (Fig. 7).

The cueing procedure was the same as in Experiment 2. At SS12, one of the three letters appearing in the to-be-ignored color was a lowercase "b" or "f," and the remaining two were chosen from the possible set of distractor letters. At SS4, the one letter appearing in the to-be-ignored color was a lowercase "b" or "f."

Design and procedure Each trial started with the presentation of a cue for 1 s. Following the cue, a fixation cross was presented for 1 s. Next, either four or 12 color placeholders appeared on the screen for either 100, 800, or 1,500 ms. These placeholders were then removed to reveal the letters, which remained on the screen until the observer responded. Participants had to indicate whether a "B" or "F" target was present with a buttonpress.

The display size and duration of the placeholders (or placeholder duration) was varied between blocks. Participants received six blocks of 80 trials each, with the opportunity for a brief rest provided between blocks. There was one block for each of the six possible combinations of display size and placeholder duration. The order of these blocks was randomly assigned to each participant.

At the beginning of the experiment, participants received 36 uncued training trials with feedback, as in Experiments 1 and 2. The display size and placeholder duration were randomly varied during this training period.

Results and discussion

We used the same trimming procedure as in Experiment 1 (Van Selst & Jolicœur, 1994). This resulted in the elimination of 1.7 % of all trials. Following this procedure, we also removed trials in which the participants did not correctly identify the target; this resulted in the elimination of 4.1 % of all of the remaining trials.

We conducted a 2 × 3 × 2 × 2 repeated measures ANOVA with the factors Set Size (4 or 12), Placeholder Duration (100, 800, or 1,500 ms), Cue Type, and Compatibility for both error rates and response times. More errors occurred on trials at SS4 (4.5 %) than at SS12 (3.5 %), $F(1, 31) = 7.27$, $p < .05$. Error rates were higher on ignore trials (4.8 %) than on neutral trials (3.2 %), $F(1, 31) = 14.43$, $p < .01$. No other main effects or interactions emerged with respect to error rates, *p*s > .1, so the rest of our discussion will focus on the response time data.

Response times were faster at SS4 (903 ms) than at SS12 (1,428 ms), $F(1, 31) = 343.02$, $p < .001$. Furthermore, there was a main effect of placeholder duration, $F(2, 62) = 18.98$, $p < .001$. Post-hoc contrasts revealed that this was mostly accounted for by the difference between response times at the 100-ms placeholder duration (1,254 ms) relative to the remaining two durations (800-ms duration, 1,117 ms; 1,500-ms duration, 1,126 ms), $F(2, 62) = 37.81$, $p < .001$. No other main effects were significant, *p*s > .1. As in Experiment 3, we did not observe any compatibility effects.[4]

According to our predictions, ignore cues should slow responses at the short placeholder duration (as in Exps. 1–3), because 100 ms would not be enough time for observers to select the to-be-ignored item(s) and subsequently inhibit them. Instead, observers would be in the process of selecting the to-be-ignored item(s) when the letters appeared, and thus would select one or more items guaranteed not to be the target when the placeholders were removed. At longer placeholder durations, the effect of ignore cues should be reversed; responses should be faster following ignore cues because the long placeholder duration would give observers sufficient time to select and subsequently inhibit the to-be-ignored item(s). We did find a significant interaction between cue type and placeholder duration, $F(2, 62) = 20.24$, $p < .001$, as the effect of cue type flipped between short and long placeholder durations (Fig. 8). Post-hoc contrasts revealed that the effect of cue type at the shortest placeholder duration differed from the effect of cue type at the remaining durations, $F(2, 62) = 39.92$, $p < .001$. Using simple main effects analysis, we found a main effect of cue type at all three placeholder durations. In line with our predictions, response times were slower for ignore trials (1,304 ms) than for neutral trials (1,204 ms) at the 100-ms placeholder duration, $F(1, 31) = 23.3$, $p < .001$, but faster for ignore trials than for neutral trials at both the 800-ms duration [1,099 vs. 1,136 ms; $F(1, 31) = 7.57$, $p < .05$] and the 1,500-ms duration [1,105 vs. 1,145 ms; $F(1, 31) = 5.99$, $p < .05$]. Thus, feature-based ignore cues aided search

[4] Again, the task was more difficult than in those in Experiments 1 and 2 (smaller letters, nonpredictable locations). Thus, compatibility effects might not have occurred because excess cognitive resources may not have been available to maintain representations of recently discarded distractor letters, as in Experiments 1 and 2.

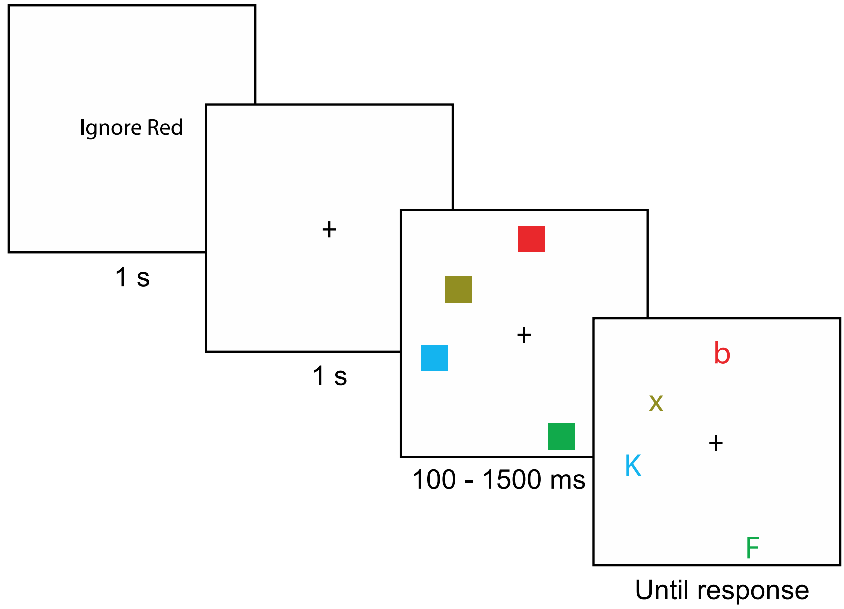

Fig. 7 Sample trial for Experiment 4. Color placeholders appeared at the locations of upcoming letters for a variable duration (100, 800, or 1,500 ms). On each trial, either four (as shown) or 12 letters were present. Again, observers had to indicate the presence of a "B" or an "F" target, and a pretrial ignore cue was presented prior to a randomly selected 50 % of all trials

at longer placeholder durations, suggesting that observers were able to disengage from and inhibit the to-be-ignored item(s) following selection.

Overall, participants responded more quickly following ignore cues (898 ms) than following neutral cues (908 ms) at SS4, but participants' responses were slower following ignore cues (1,441 ms) than following neutral cues (1,415 ms) at SS12, $F(1, 31) = 5.2$, $p < .05$ (no other interactions were significant, $ps > .1$). Therefore, the efficiency of this inhibitory mechanism may depend critically on display size and/or the number of to-be-ignored items.

Selection of multiple to-be-ignored items In the SS12 condition, three items were presented in the to-be-ignored color on each trial. It is possible that participants selected only one item matching the to-be-ignored color, and that the remaining two items matching that color were subsequently inhibited automatically, in parallel (cf. Braithwaite & Humphreys, 2003; McLaughlin, Masterson, & Herrmann, 1972). Another possibility is that observers may have selected only one individual item to inhibit, rather than trying to inhibit all to-be-ignored items, regardless of the number of to-be-ignored items present in the display. In either of these cases, observers in both set size conditions (SS4 and SS12) would typically start their search in a similar fashion following the presentation of an ignore cue. That is, observers would (1) select one item in the to-be-ignored color, which was guaranteed not to be the target; (2) inhibit that item and any additional items appearing in the to-be-ignored color; and (3) subsequently select only potential target items (i.e., items that did not match the to-be-ignored color). This pattern would result in a response time cost (i.e., slower response times on ignore than on neutral trials), accrued by selecting that first item that was guaranteed not to be the

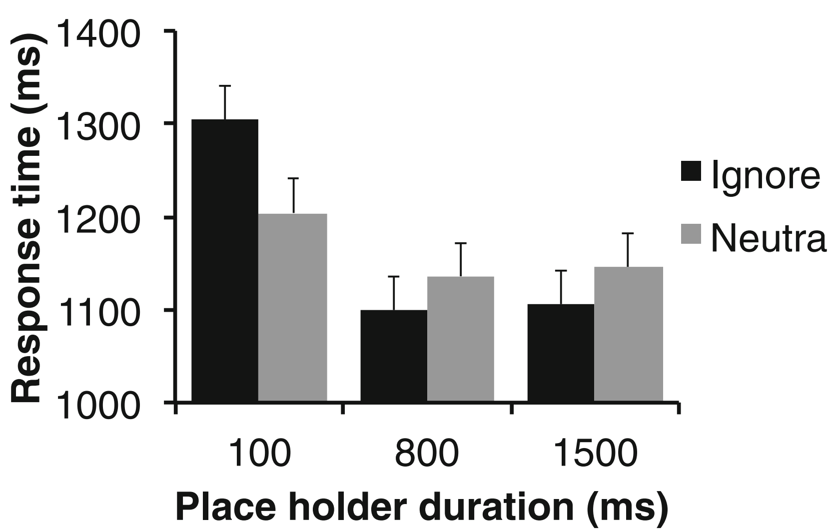

Fig. 8 Results from Experiment 4. At the shortest placeholder duration (100 ms), responses were slower on ignore trials than on neutral trials. At longer placeholder durations (800 and 1,500 ms), this result flipped; observers showed a benefit from ignore cues, with faster responses on ignore trials than on neutral trials. Error bars were calculated separately for each placeholder duration from the within-subjects error term (Loftus & Masson, 1994)

target, which would be roughly equivalent in the SS4 and SS12 conditions. Thus, at the short placeholder duration at which these ignore-cue costs are evident (as opposed to longer placeholder durations), we might expect the cost of the ignore cue to be similar for both set size conditions.

Alternatively, if there was no automatic spread of inhibition following the initial selection of one to-be-ignored item, observers in the SS12 condition might have selected each item individually in order to inhibit them. Thus, rather than an efficient, parallel selection and subsequent inhibition of all to-be-ignored items, observers would engage in an inefficient (perhaps even serial) selection of each individual to-be-ignored item. Since each to-be-ignored item was guaranteed not to be the target, each additional selection of a to-be-ignored item would likely increase the response time cost incurred by ignore cues. The data are more consistent with the inefficient selection of multiple to-be-ignored items; there was a much higher cost of the ignore cue at the 100-ms placeholder duration for SS12 (142 ms) than for SS4 (60 ms), $F(1, 31) = 5.64$, $p < .05$.

Inhibition of multiple to-be-ignored items Can observers efficiently maintain inhibition of multiple to-be-ignored items following the initial selection of those items? In the SS12 condition, observers might have been able to maintain inhibition of all three to-be-ignored items simultaneously for the duration of the search after initially selecting those items. In this case, successful inhibition at SS4 would effectively reduce the number of items to be searched by one (from four to three), whereas successful inhibition at SS12 would reduce the number of items to be searched by three (from 12 to nine). Thus, we would expect the benefit of the ignore cue (observed at the two longer placeholder durations) to be greater at SS12 than at SS4. Alternatively, the ability to inhibit items appearing in the to-be-ignored color following selection may be a capacity-limited process, and observers may only have been able to maintain inhibition of one or two items at a time. Therefore, we might expect to see roughly equivalent ignore-cue benefits at the longer placeholder durations.

In the SS4 condition, responses were 38 ms faster at the 800-ms placeholder duration, following ignore cues as compared to neutral cues, and 53 ms faster at the 1,500-ms placeholder duration. However, the benefit of ignore cues was numerically *smaller* at SS12 (36 ms at the 800-ms duration, 30 ms at the 1,500-ms duration) than at SS4. This suggests that observers were unable to effectively inhibit three items simultaneously, even at the longest placeholder duration. Thus, the ignore-cue benefits observed at longer placeholder durations in this experiment likely reflect a capacity-limited inhibitory mechanism.

The mechanism of inhibition The mechanism behind the inhibition in the present experiments is not yet fully understood. It may be that this inhibition is a purely top-down process, triggered by explicit ignore cues. On the other hand, search may be speeded at long placeholder durations because IOR (Posner & Cohen, 1984) occurs following selection and disengagement from the to-be-ignored item, meaning that the location of the to-be-ignored item(s) is automatically inhibited following selection.

One possible alternative account of our data suggests that there was no inhibition of the to-be-ignored item at all; instead, observers initially selected the to-be-ignored item, and after a period of time, disengaged from that item and shifted attention toward a different item. This would reflect a purely passive "shifting" process, rather than the active inhibition process that we have proposed.[5] Nevertheless, this shifting process could plausibly result in ignore-cue benefits with certain placeholder durations. This is because observers might have been shifting attention away from the to-be-ignored item as the actual search process began (i.e., after the placeholders were removed to reveal the letters). If this were the case, observers would then have had a one in three chance of selecting the target at the beginning of the search (rather than a one in four chance, as would be the case in the neutral condition).

To further explore the "shifting" account, here we will examine the cue benefits when there was one to-be-ignored item (SS4) at longer placeholder durations (800 and 1,500 ms). These trial types represent a subset of the data in which we can make simple predictions about the search process. We observed an ignore-cue benefit of 38 ms with the 800-ms placeholder duration at SS4. This result could be consistent with the shifting account, assuming that 800 ms is sufficiently long for the following events to occur: (1) selection of the to-be-ignored item and (2) disengagement from the to-be-ignored item and (at least the beginning of) a shift of attention toward a different item. Therefore, if 800 ms is sufficiently long for that initial process to occur, 1,500 ms should be approximately long enough for that process to reoccur, resulting in disengagement from that second item and selection of a third item. Because observers did not inhibit the first item that they selected (the to-be-ignored item), this third selection might be directed back toward that to-be-ignored item on some trials. This means that when the placeholders were removed after 1,500 ms to reveal the letters, the observers might have been in the process of shifting (back) toward the to-be-ignored item. Thus, by 1,500 ms the to-be-ignored item was among the set of possible items to be searched, and there should be no ignore-cue benefit when the search began. However, we did

[5] We thank Jaap Munneke for this suggestion.

observe an ignore-cue benefit at the 1,500-ms placeholder duration for SS4, one that was numerically even larger (53 ms) than the benefit observed at the 800-ms placeholder duration. Thus, the inhibition account provides a better fit than does the shifting account for the data from the present experiment. Still, further research will be necessary to fully understand the mechanism that allows for more efficient search with the combination of ignore cues and color placeholders; this will be discussed in greater detail in the General Discussion.

Summary Taken together, these results are consistent with the hypothesis that when observers are cued to ignore a specific color, inhibition of items appearing in that color may occur following selection. That is, after observers seek out items matching the to-be-ignored color, they inhibit those items for the remainder of their search. When color placeholders appear for a long duration (at least 800 ms) prior to the onset of the search display, ignore cues lead to faster search through the inhibition of nontarget items, although we should note that these benefits were not observed in error rates. Furthermore, there seems to be a limitation on this inhibition, as inhibitory benefits did not increase in magnitude when the number of to-be-ignored items increased; therefore, observers may be unable to maintain efficient inhibition of multiple to-be-ignored items appearing in noncontiguous locations. This is in contrast to the parallel inhibition observed in visual-marking studies (e.g., Watson & Humphreys, 1997), suggesting that the inhibitory mechanisms recruited in the present experiment may differ from the ones recruited in visual marking. There are several possible explanations for this discrepancy; for example, in visual marking a temporal asynchrony occurs between the marked and nonmarked items that did not occur in the present experiments. This asynchrony could provide a strong bottom-up grouping cue that allows for simultaneous inhibition of multiple items.

General discussion

In four experiments, we presented pretrial ignore cues that provided observers with explicit foreknowledge that the target letter would not appear in a particular color. Despite these cues being 100 % valid, observers were unable to use the information provided by these cues to speed search in the first three experiments. Instead, performance declined following ignore cues, as compared to after neutral cues, in all three of those experiments. We observed this decrease in performance whether the cue was a colored border (Exp. 1) or a printed verbal instruction (Exps. 2 and 3). Furthermore, probe-dot detection results indicated that participants were selecting a nontarget item appearing in the to-be-ignored color early in their search process (Exp. 3). Thus, when observers were instructed to ignore items appearing in a particular color, they instead first selected items appearing in that color before continuing their search. However, inhibition did occur following selection of the to-be-ignored items (Exp. 4); when observers were presented with color placeholders at least 800 ms before the search task that indicated where the to-be-ignored items would appear, they were able to use the ignore cues to speed search.

Friedman-Hill and Wolfe (1995) found that when observers searched a display to find a pop-out orientation target, they were unable to efficiently select a subset of items when those items were defined negatively (i.e., defined by what color they were not). Our results suggest that foreknowledge of nontarget features also fails to speed search when selection of individual items (rather than groups of items) is required to identify the target. Furthermore, our results suggest that observers may paradoxically prioritize selection of items appearing in a to-be-ignored feature (cf. Tsal & Makovski, 2006) and that inhibition of these items can occur following this selection.

At first glance, our data appear contrary to the findings of Woodman and Luck (2007). In their task, participants held a color in working memory while performing a search task. Their participants were explicitly told that that if an item in the search display matched the color held in working memory, this item would not be the target. They found that participants were faster to respond when one of the search items matched the color held in working memory (and thus could be inhibited) than when none of the items in the search display matched the color held in working memory. These data provided evidence against the conclusion that attention is automatically directed to items matching the contents of working memory (cf. Downing, 2000). Furthermore, Woodman and Luck proposed that participants were using a "template for rejection" to "strategically avoid searching items" (p. 369). Therefore, if any items did appear in that color, the search could be accomplished more quickly, since that was one less possible target item that participants had to search among (i.e., reducing the set size by one).

However, Woodman and Luck's (2007) data may actually be consistent with the pattern of behavior that we observed in the present experiments. One possibility is that participants in Woodman and Luck's task may have been seeking out the to-be-ignored item in all of their trial types. Perhaps on trials in which no item appeared in the to-be-ignored color, responses were slowed because observers could not find an item to ignore—making these analogous to target-absent trials. In this scenario, only once the observer determined that no item matching the to-be-ignored color was present would he or she commence search for the target. Therefore, responses would be slower when no item appeared in the to-be-ignored color than when an item did appear in that color, even though observers were selecting that to-be-ignored item. This explanation would not be consistent with a "template for rejection," but it would be consistent with a strategy in which observers actively seek out the to-be-ignored item in order to inhibit it (an explanation that is also consistent with the data from the present experiments).

Inhibition of features and locations

When observers were given foreknowledge of where item(s) in the to-be-ignored color would appear, in the form of color placeholders, and when they were given this information far enough in advance, responses were faster following ignore cues than following neutral cues (Exp. 4). It might be expected that if this inhibition were purely feature-based, inhibition would spread efficiently throughout the display following the initial selection of an item matching the to-be-ignored feature, much like the spread of inhibition to multiple items in parallel on the basis of their shared features that has been observed in visual-marking experiments (e.g., Braithwaite & Humphreys, 2003, 2007; Braithwaite, Humphreys, & Hodsoll, 2003, 2004). Thus, although participants would initially select a specific item at a fixed location, purely feature-based inhibition (independent of locations) might follow. However, we found no evidence for this type of parallel spread of feature-based inhibition; instead, our evidence suggests that the inhibition that occurred in Experiment 4 reflected a capacity-limited inhibitory mechanism that was not well-suited for maintaining inhibition of multiple to-be-ignored items appearing at noncontiguous locations.

This indicates that the inhibition that occurred in our experiments may not have been feature-based at all; instead, observers may have been seeking out items appearing in the to-be-ignored feature one by one and marking those *locations* as irrelevant, in order to speed response times on ignore trials. This may ultimately reflect a purely location-based inhibition strategy, making the results of Experiment 4 similar to other demonstrations of explicit location-based inhibition (Chao, 2010; Munneke et al., 2008), while extending these results by demonstrating a limitation in the number of locations that can be simultaneously suppressed. The fact that observers would adopt such an inefficient strategy strongly suggests that they were unable to explicitly tap into a purely feature-based inhibitory mechanism, even though such a mechanism does exist and is used implicitly in certain situations (e.g., the distractor-previewing effect). Thus, it appears likely that the only mechanism for inhibition directly (i.e., explicitly) accessible to observers is one that is location-based. When observers try to deliberately ignore nontarget features, they instead select to-be-ignored items at a location in space and subsequently inhibit them, a strategy that we will term "search and destroy."

The "search and destroy" concept may be broadly applicable. For example, there is evidence that previewed nontarget items in visual-marking tasks are selected prior to inhibition (Humphreys, Jung Stalmann, & Olivers, 2004). It may be that selection is always necessary prior to any form of explicit inhibition. In Chao's (2010) study, cues instructing participants to ignore a specific location did not speed response times when a short cue–target SOA was used (507 ms). Cues to ignore a nontarget location only sped response times when the cue–target SOA was at least 1,500 ms in both Chao's and Munneke et al.'s (2008) studies. In Experiment 4 of our study, 800 ms was the minimum duration of the color placeholders at which participants could select and subsequently inhibit items appearing in to-be-ignored colors. Thus, it is possible that even purely location-based inhibition requires selection of the to-be-ignored location, and that observers in Chao's and Munneke et al.'s studies were using a "search and destroy" strategy to inhibit nontarget locations. Additional research will be needed to test this prediction and to clarify the mechanisms involved in explicit inhibition during search.

The present research has some commonalties with the research of Tsal and Makovski (2006) on the attentional white bear phenomenon. Their findings suggest that participants allocate attention to any location where an item is expected, regardless of its task relevance. Furthermore, this allocation is somewhat flexible, in that more resources are deployed to expected target locations than to expected distractor locations. However, our task involved visual search, as opposed to target discrimination at a central location. Thus, while both tasks involve the use of limited attentional resources, our task extends the findings to suggest that explicit foreknowledge of distractor features hinders visual search. This finding has implications for models of visual search (e.g., Treisman & Gelade, 1980; Wolfe, 1994).

The present results go beyond the attentional white bear phenomenon in several other important ways. For example, in our Experiments 1–3, the locations of the four items were fixed; thus, observers expected items to appear in those locations on every trial. Nonetheless, observers devoted more attentional resources to the to-be-ignored distractor item than to the other three items, one of which was guaranteed to be the target. This suggests that the flexibility of Tsal and Makovski's (2006) "process-all mechanism" is not limited to target selectivity, but also includes a counterintuitive form of distractor selectivity: That is, attentional resources are preferentially deployed to locations where a known nontarget is set to appear, rather than to locations where an item that may or may not be the target is set to appear. In the present study, the location and color of the target were not predictable. Thus, this distractor selectivity might only occur when early target selectivity is not possible and the only precise information available to observers prior to the onset of the search display is related to the distractor properties. Additionally, in the present study, features (rather than locations) were known before stimulus onset, providing evidence that foreknowledge of distractor features (in addition to locations) may bias attention toward to-be-ignored items. Finally, in the present study, overall performance decreased when distractor features were known ahead of time, as compared to when observers had no explicit foreknowledge of distractor features. Thus, a measurable cost was associated with attention devoted to nontarget items. In Tsal and Makovski's (2006) study, there were no significant changes in overall response times or accuracy when attentional resources were devoted to expected distractor locations. Therefore, the present data show that access to explicit information about nontarget properties is surprisingly detrimental, because "search and destroy" can be a slow, counterproductive process (though foreknowledge of salient distractor probability may be an exception; see, e.g., Moher, Abrams, Egeth, Yantis, & Stuphorn, 2011; Müller, Geyer, Zehetleitner, & Krummenacher, 2009).

Conclusion

When observers were instructed to explicitly ignore a nontarget feature, they instead selected items matching that feature prior to inhibiting them. This "search and destroy" strategy may be useful under certain conditions, such as when observers are required to inhibit an item at a later point in time, and that item is presently visible. However, without such a preview period, "search and destroy" is an inefficient, disruptive strategy. Therefore, observers given perfectly valid information about upcoming nontarget features would be best served to completely disregard that information when conducting their search. This suggests a counterintuitive principle: Acquiring information about the visual characteristics of the upcoming search task does not necessarily improve performance. In fact, additional valid information about nontarget properties can hinder search.

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