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Inhibition in time-based visual selection: Strategic or by default?

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Abstract

The Visual Marking mechanism (Watson & Humphreys, 1997) allows new objects to be prioritized by applying top-down inhibition to a set of previewed distractors, increasing the efficiency of future visual search. However, if this inhibition results in little or no search facilitation, do people continue to apply it or do they strategically withhold it? Here we present six experiments in which we examined how participants control this inhibitory mechanism. Experiments 1 to 3 showed that in difficult search contexts, participants did not modulate the extent to which they applied inhibition based on the proportion of trials in which inhibition would have been useful. This was the case, even when explicitly cued before each trial as to the utility of applying inhibition (Experiment 4). In contrast, when search was conducted in predominantly easy search contexts, there was some evidence that inhibition was applied strategically (Experiments 5 and 6); however, the extent of this control was relatively modest. The findings are discussed in terms of the mechanisms of top-down attentional control and implications for failures of attention in real world contexts.

Introduction

Humans possess numerous top-down attentional mechanisms that provide a volitional ability to select among competing items. As such, attention can to some extent be directed freely, without being controlled exclusively by the changing nature of our surroundings. This top-down attentional system serves an active role in guiding our behavior in goal-relevant ways. It is often claimed that top-down control allows processing to be strategically applied in effortful tasks. For example, strategic, volitional behavior has been previously observed in visual search tasks that investigate the deployment of selective attention (Smilek, Enns, Eastwood, & Merikle, 2006; Bacon & Egeth, 1994). In the current study, we examine whether and to what extent people exercise strategic control over a top-down inhibitory mechanism in selective attention.

Top-down attentional inhibition

In order to process aspects of our environment which are most relevant to our current needs efficiently, our cognitive system needs to deprioritize and filter out irrelevant information. A critical way in which this can be achieved is through inhibiting the representations associated with redundant information (Treisman & Sato, 1980; Moher et al., 2014).

Evidence for the use of inhibition in enabling attentional selection over time can be found in the *visual marking* literature. In a preview search task (Watson & Humphreys, 1997), distractors in a visual search procedure are separated in time, with one set of distractors (e.g., green Hs) shown (i.e., previewed) before another set of distractors (e.g., blue As). The target (e.g., a blue letter H) is only ever present in the second set of items, making the first set of items irrelevant to the goal of finding the target. Search efficiency in this *preview* task is typically found to be more efficient than if all the display items had been presented simultaneously (a *full-element baseline*; FEB). Perhaps more surprisingly, preview

search is often as efficient as if only the second set of items had been presented (a *half-element baseline*; HEB). This suggests that, in some situations, people can ignore fully the set of old, task-irrelevant stimuli, to restrict their processing to newly-arriving information. Thus, the visual system appears to be capable of selecting stimuli on the basis of their time of appearance. Specifically, new items can be selected at the expense of task-irrelevant items which were presented at an earlier point in time. This finding captures the remarkable ability of the brain to increase the efficiency of our behavior; irrelevant information is filtered out, enabling us to achieve our overall goal faster.

Further work has shown that, when searching for a single target, the capacity to prioritize new items is remarkably large. For example, efficient preview search has been found with displays of up to 15 new items (Theeuwes, Kramer, & Atchley, 1998) and up to 30 old items (Jiang, Chun, & Marks, 2002). However, in some situations, capacity limits appear to emerge. For example, Emrich, Ruppel, Al-Aidroos, Pratt, and Ferber (2008) showed that the probability of fixating an old item in preview search (see also Watson & Inglis, 2007) increased as a function of the number of eye movements made. By approximately the fifth saccade, people were more or less equally likely to fixate new or old items. Nonetheless, in terms of RT search slope measures, a robust preview benefit still emerged, even with this apparent decay in eye movement selectivity for new/old items. Subsequent work has shown that such capacity limitations are most likely to occur when multiple responses, such as mouse clicks or touch responses, have to be made to *all* new items (Watson & Kunar, 2012).

That said, the ability to ignore old items does not mean that those items are lost to our cognitive processing. If old preview items change in potentially ecologically-relevant ways (i.e. a change in their shape or identity), they then re-capture our attention. In contrast, less ecologically-relevant changes (such as changes in color or luminance) appear to have little

effect, and the altered items remain ignored (Watson & Humphreys, 1997, 2002; Watson, Braithwaite, & Humphreys, 2008).

Other findings show that moving, as well as stationary items can be effectively ignored (Watson & Humphreys, 1997, 1998, 2000), and that establishing prior representations of the old items can result in them being more easily ignored (Kunar, Humphreys & Smith, 2003; Hodson & Humphreys, 2005). Lastly, more behaviorally-salient stimuli, such as faces, might be more difficult to ignore than ones that are more abstract (Blagrove & Watson, 2010, 2014).

Three main proposals have been put forward to account for how old stimuli might be excluded from subsequent search. First, newly-appearing items might capture attention automatically, based on the luminance transients that they produce within the visual system (e.g., Donk, 2005, 2006; Donk & Theeuwes, 2001, 2003; Donk & Verburg, 2004; Kiss & Eimer, 2011). Second, the set of old and new items might be segregated for processing, based on some kind of temporal asynchrony signal. Grouping, based on common temporal signals, would then work to define two sets of stimuli (i.e., old and new) to which processing could be directed differentially (e.g., Jiang et al., 2002). Third, old items might be actively inhibited, which would reduce their competition for attention when new items arrive; a process that Watson and Humphreys (1997) called *Visual Marking*.

The most likely position is that all three processes contribute to generating a preview benefit to some degree, depending on the exact situation. Indeed, studies continue to explore the relative contributions of inhibition and automatic luminance onset capture in the prioritization of new stimuli. For example, the onset account predicts that a preview benefit should not be obtained if the stimuli are isoluminant with their background (Donk & Theeuwes, 2001, 2003; Pratt, Theeuwes, & Donk, 2007). However, a preview benefit can be obtained with stimuli isoluminant with their background, provided that a sufficient amount of

time is allowed for the locations of the previewed items to be perceived and encoded (Braithwaite, Hulleman, Watson, & Humphreys, 2006; see also Al-Aidroos, Emrich, Ferber, & Pratt, 2012).

A luminance onset only account also has difficulty in explaining why past representations of the old items influence the ability to find new items (Kunar et al., 2003; Hodsoll & Humphreys, 2005), and the finding that detecting probe stimuli at old item locations is more difficult than at new item or neutral, no-item locations (Watson & Humphreys, 2000; but see also Agter & Donk, 2005). Lastly, this account cannot explain the effect of semantic consistency when visual changes to old items occur (Osugi, Kumada, & Kawahara, 2010), the finding that a preview effect occurs when new items are presented during a blink (von Mühlenen, Watson, & Gunnell 2013), or why the detection of new items, carrying the features of old items, is impaired (i.e., the color carry-over effect; Olivers & Humphreys, 2003; Braithwaite, Humphreys, & Hodsoll, 2003, 2004), particularly with moving stimuli (Andrews, Watson, Humphreys, & Braithwaite, 2011).

The findings provided evidence for a robust, but partial preview benefit, suggesting a role for both the inhibition of old items and capture by the onset signals associated with the new elements (see Watson & Humphreys, 1997). To sum up, it is likely that a number of mechanisms contribute to the selection of new elements, depending on the specific task conditions (see also Al-Aidroos, et al., 2012; Herrero, Crawley, van Leeuwen, & Raffone, 2007).

Strategic attentional control

Previous work has shown that asking participants to adopt particular cognitive strategies can influence general visual search efficiency. Illustrating this, Smilek, Enns, Eastwood, and Merikle (2006) found that instructing participants to search *actively* or to remain in a *passive* state (i.e., waiting for a target to become visible), influenced search

efficiency depending on whether the task was easy or difficult. Passive search instructions led to more efficient search when the task was difficult, most likely because participants relied more on fast automatic processes than on slower executive control processes. Similarly, Bacon and Egeth (1994) have shown that participants can adopt a singleton search mode, in which any featurally unique target (e.g. a unique shape or color), captures attention. Alternatively, they can adopt a feature-based mode, in which only targets possessing a particular feature (e.g., a specific shape), capture attention (but see Theeuwes, 2004).

Importantly, the type of search mode or strategy that participants adopt can be manipulated by the contextual factors of the task. For example, if the target is always the unique item in a display, then a singleton mode is likely to be adopted, while if the target is not reliably defined by being the only singleton in the display, then a feature-based mode will be used (Bacon & Egeth, 1994; see also Folk & Anderson, 2010; Folk, Remington, & Johnston, 1992; Folk, Remington, & Wright, 1994, Horstmann & Becker, 2008, for related work on contingent involuntary attentional orienting).

In terms of time-based selection, some previous studies have indicated that the inhibition of old items might also be strategic and intentional. For example, in Watson and Humphreys' (2000) work, participants performed a preview search task on the majority (76%) of trials within a single block. On the remaining 24% of trials, a tone indicated that participants should look for a small probe dot which was presented at either the location of an old or a new item, rather than completing the search task. In this situation, successful probe dot detection was much poorer for old-location probes than for new-location probes (see also Osugi, Kumada, Kawahara, 2009). In contrast, when participants were instructed to detect a probe dot on *every* trial, performance did not depend on the location of the probe dot. This indicates that the inhibition of the old items had been withheld (Watson & Humphreys, 2000). This finding provides some support to the notion that inhibition might only be applied

when there is an advantage for people to do so, and that it is flexibly controlled depending on observer instructions (Watson & Humphreys, 1997, 2000). However, it is not known whether observers will spontaneously choose to adopt the process of ignoring old items, and what factors affect their strategy to do so.

Previous work has also argued that intentionally ignoring old stimuli is an effortful process, requiring both visual and attentional resources (e.g., Watson & Humphreys, 1997; Humphreys, Watson, & Jolicoeur, 2002; Olivers & Humphreys, 2002). Given the resource limited nature of visual processing, we might expect that a cognitively consuming process would not be implemented in situations in which it is of little use. Furthermore, if this inhibitory process is susceptible to strategic control, one might ask whether it is modulated in an all-or-none fashion, or continuously. For example, if there is little benefit from applying inhibition to increase task performance, participants might choose to abandon the use of inhibition altogether, or they might apply inhibition selectively to certain trials based on the perceived value it brings.

Another possibility is that participants follow a default state of always ignoring the old items, in order to enhance the selection of new stimuli (which could arguably be an overriding ‘objective’ for the visual system). For example, ‘knowing’ that the target is not present in the current set of (previewed) items might trigger participants to apply inhibitory processes by default. Thus, observers might invest resources in inhibiting old items in all time-based selection situations, irrespective of whether it helps them or not, or potentially impairs task performance overall.

Overview of the Experiments

We report six experiments in which we examined spontaneously-generated strategic inhibition in preview search. In Experiments 1-4, the general approach was to present observers with two different time-based search conditions. In one condition, inhibiting the old

items was beneficial for task performance on the majority of trials, and so would produce an overall improvement in target search and task efficiency. In another condition, on the majority of trials, the old items changed in ways which would disrupt any inhibition that might have been applied to them. As a result, old items would compete strongly for attention with the new items. In this situation, applying inhibition to the old items would provide an advantage on only a minority of trials. Thus, in the first four experiments, we tested whether disruption of inhibition would encourage participants to modify their attentional strategy. In Experiments 5 and 6 we presented a highly salient target on the majority of the trials, making the search sufficiently easy for the target to be detected without having to ignore the old items. This tested whether inhibition is more likely to be strategically modulated when on the majority of occasions search can be performed without the need to suppress previously presented items, and can be performed using an alternative and more efficient strategy.

Experiment 1: Disrupting Location-based Inhibition

The aim of Experiment 1 was to establish whether disruptions of location-based inhibition would encourage strategic behavior. There were two main trial types, Standard Preview and Jump. On a *Standard Preview* trial, one set of distractors (the preview items) was added to the display, followed 1 second later by a second set. The target was only ever present in the second set (as in Watson & Humphreys, 1997). On a *Jump* trial, the first set of items ‘jumped’ (i.e., moved abruptly to new locations) when the second set of stimuli was added to the display. Based on previous work, we anticipated that such a jump would disrupt any inhibition applied to the old items (Watson & Humphreys, 1997, but see also Kunar, Humphreys, Smith, & Hulleman, 2003). These two types of trials were presented in differing ratios in two separate blocks of trials. In the *Standard Preview* block, the majority of trials were standard preview trials and the minority were jump trials. In the *Jump* block, this was reversed.

If participants apply inhibition to the old items strategically, they should be more likely to apply it in the *Standard Preview block*, and less likely to apply it in the *Jump* block. This would result in efficient search for standard preview trials in the *Standard Preview* block (where it would be advantageous overall to apply inhibition) and less efficient search for standard preview trials in the *Jump* block (where applying inhibition would only be useful on a relatively small number of trials). However, if 1) inhibition in preview search is not applied strategically, or 2) disruptions in location-based inhibition are not sufficient to drive a change in inhibitory strategy, or 3) changing old item locations is not sufficient to disrupt the inhibition of old items (cf. Kunar et al., 2003), then we would expect no difference in performance in the standard preview trials across the two blocks. Note that in this design, there is no requirement for the typical FEB and HEB conditions to be included. This is because the most important comparison is between search performance in the standard preview trials across the two main conditions (i.e., performance on standard preview trials in the Standard Preview block vs. performance on standard preview trials in the Jump block).

Method

Participants. Participants were 12 undergraduates (all female) from the University of Warwick, who received course credit for participating. Their ages ranged from 18-44 ($M = 21.3$, $SD = 7.34$ years). Participants reported normal or corrected to normal visual acuity in this and all remaining experiments.

Stimuli and Apparatus. A Samsung 550P5c-S03 laptop was used to present the displays and record the participants' responses. Stimuli were displayed on the 15-inch laptop monitor, at the panel's native resolution of 1366×768 pixels and 60 Hz update rate. Displays were generated and responses recorded by a custom written computer program. The target was a light blue [RGB values = 68, 164, 176; CIE_{xy} = .234, .225; lum = 34 cd/m²] square and distractor stimuli were light blue circles and pink [RGB values = 211, 103, 126; CIE_{xy} =

.333, 236; lum = 30 cd/m²] squares, presented against the black monitor background. The sides of the squares measured 8 mm and the circles had a diameter of 10 mm. There was an equal number of blue and pink items present in each search display, with the target taking the place of one of the blue distractors. Search displays were generated by placing items at random into the cells of an invisible, centrally-placed 6 × 6 grid, with the constraint that there was an equal number of each type of distractor on the left and right side of the display. Grid spacing was 110 pixels (28 mm) center-to-center and stimulus locations were further jittered by +/- 20 pixels (5 mm) in the x- and y-axes, in order to reduce stimulus regularity. The number of items in the final search display in all conditions (the display size) was 4, 8, or 12. The target item was constrained to fall into the two most leftward or rightward columns (columns 1, 2, 5, or 6). This ensured that the target was always unambiguously to the left or right of display center. Responses were recorded via an 8-button gamepad interface device, connected via a USB interface.

Design and Procedure. There were two types of trials, *Standard Preview* and *Jump*. A trial in the *Standard Preview* condition consisted of a blank screen (500 ms), followed by a central white [RGB = 180,180,180] fixation dot (2 mm × 2 mm), and then 2, 4, or 6 pink squares. After a further 1000 ms, 1, 3, or 5 blue circles, respectively, were added to the display, along with the blue square target, to form a final display size of 4, 8, or 12 items. Search displays remained visible until participants responded, which started the next trial. Participants indicated the location of the target by pressing the left-shoulder button of the game pad, if the target was on the left side of the display, or the right-shoulder button if it was on the right side of the display. A trial in the *Jump* condition was similar, except that the set of previewed items ‘jumped’ to new locations when the second set of stimuli (which contained the target) was added (see Figure 1). Participants were not explicitly informed that

the stimuli would jump, nor of the 80:20 split in the block, in order to encourage spontaneous and ecologically valid behavior.

Importantly, a preview item could not jump to a location previously occupied by a different preview item. In addition, the items in the second set of stimuli could not be placed in the location of a previously-occupied preview item. This ensured that any residual location-based inhibition across displays could not impact on search efficiency.

The two types of trials were combined in different proportions to form two types of search blocks. In the *Standard Preview* block, 80% (144 trials) of trials were standard preview trials and 20% (36 trials) were jump preview trials. In the *Jump* block, 80% (144 trials) of trials were jump preview trials and 20% (36 trials) were standard preview trials. Each block contained 180 search trials, with each combination of target location and display size represented equally. In addition to the search trials, each block also contained 18 (10%) *catch* trials, in which no target was present. On catch trials, participants responded by pressing a third button on the gamepad. These target-absent catch trials prevented participants from being able to respond by searching only half of the display (e.g., concluding that the target is on the right, if it is not found on the left; see e.g., Al-Aidroos et al., 2012; Blagrove & Watson, 2010, for previous uses of this method). Trial order was individually randomized for each block, and block order was counterbalanced across participants. Participants completed an 18 trial practice block before each of the full blocks of trials. Within each block, there was a break after 60 trials. The break was self-paced by the participant, and lasted until the participant pressed any key on the keyboard.

Results

Reaction Times: Trials with RTs less than 200 ms or greater than 10,000 ms were removed as outliers (0.86% of the data). Overall mean correct RTs are shown in Figure 2 and search slope statistics in Table 1. Mean correct RTs were analyzed using a 2 (Block type:

80% Jump or 80% Standard Preview) \times 2 (Trial Type: Jump or Standard Preview) \times 3 (Display size) repeated measures ANOVA. There was no main effect of Block, $F < 1$, but there were significant main effects of Trial Type $F(1,11) = 13.07$, $MSE = 13588.25$, $p < .005$, and Display Size $F(2,22) = 49.18$, $MSE = 4431.19$, $p < .001$. RTs were longer for Jump trials, and increased as the Display Size increased. There was a significant Trial Type \times Display Size interaction $F(2,22) = 18.97$, $MSE = 3539.66$, $p < .001$. Jump trials were more influenced by Display Size than Standard Preview trials. Neither the Block \times Trial Type nor the Block \times Display Size interactions reached significance, both $F_s < 1$. The three-way Block \times Trial Type \times Display Size interaction was also non-significant $F(2,22) = 1.07$, $MSE = 8873.43$, $p = .36$. These analyses suggest that the strength or likelihood of applying inhibition to the old items did not depend on the proportion of Jump versus Standard Preview trials within a single block. However, as further confirmation of this, we conducted two planned comparisons comparing the Jump trials from each type of block and the Standard Preview trials from each type of block separately.

Jump trials only: A 2 (Block: 80% Jump or 80% Standard Preview) \times 3 (Display size) repeated-measures ANOVA showed that RTs increased with display size, $F(2,22) = 104.57$, $MSE = 2456.55$, $p < .001$. There was a numerical trend for the search slope of the 80% Jump block to be shallower than the 80% Standard Preview block, however, neither the main effect of Block nor the Block \times Display Size interaction reached significance, both $F_s < 1$.

Standard Preview trials only: RTs increased with display size, $F(2,22) = 5.11$, $MSE = 5514.31$, $p < .05$. There was also a numerical trend for search in the 80% Standard block to be more efficient than in the 80% Jump block. Nevertheless, neither the main effect of Block nor the Block \times Display Size interaction approached significance, both $F_s < 1$.

Errors: Mean percentage errors were low overall (1.46%) and are shown in Table 2. Errors were analyzed using a 2 (Block: 80% Jump or 80% Standard Preview) \times 2 (Trial type:

Jump or Standard Preview) \times 3 (Display size) repeated measures ANOVA. The main effects of Block, Trial Type, and Display Size were not significant, nor were the Block \times Trial Type, Block \times Display Size interactions, all F s < 1 . The Trial Type \times Display Size interaction was also not significant $F(2,22) = 1.48$, $MSE = 17.09$, $p = .12$, nor the three-way Block \times Trial Type \times Display Size interaction, $F < 1$. The overall error rate on catch trials was 3.70%, which confirms that participants were searching over the whole display. Due to the small number of trials, catch trial errors were not analyzed further.

Discussion

The first consideration, given that we did not include the typical HEB and FEB conditions, was whether we obtained any evidence that the old items were being suppressed and the new items prioritized. Clearly this was the case, with a significant difference in search efficiency between the Standard Preview trials and the Jump trials, and Standard Preview search slopes being less than a third of the Jump slopes. This confirms that having old items jump to new locations when the new items were added was sufficient to disrupt the preview benefit substantially (see also Kunar, et al., 2003). The implication is that the new items were being prioritized for search in the Standard Preview conditions.

However, the main goal of Experiment 1 was to determine whether participants would spontaneously adopt different inhibitory strategies if applying inhibition helped improve their search (most of the time), compared to if it would have no benefit (most of the time). The results showed that there was a robust preview benefit, even in conditions in which, 80% of the time, inhibiting the old items would not have been useful – presumably here because the jumping of the old items served to abolish or reset the suppression of previewed items. Moreover, search efficiency (in terms of search slopes) on Standard Preview trials did not differ between conditions in which inhibition was predominantly useful (80% Standard

Preview trials) or not useful (80% Jump trials). This suggests that participants were not applying inhibition strategically.

Several possibilities might account for these findings. First, it might be that participants have no choice but to inhibit old items when they are looking out for new items. This might seem at odds with the findings from Watson and Humphreys (2000), in which detecting a probe dot was poor for probes presented at old item locations, when participants were engaged in a search task on the majority of trials. When all the trials were probe dot trials, there was no difference between detection of probes at old item locations, compared with probes at new item locations. However, note that in this case, when all trials were probe trials, there was *never* any need to inhibit the old items and new items never had to be prioritized over the old. It might be that whenever a task involves search for new items, the default is to inhibit old items, irrespective of whether the inhibition is advantageous or not. According to this account, inhibitory processing of old items is the default state and is mandatory whenever new items must be prioritized.

Second, numerous studies have suggested that there is a location-based inhibitory component involved in generating the preview benefit with stationary stimuli (e.g., Olivers, Watson, & Humphreys, 1999; Osugi, et al., 2009; Watson & Humphreys, 2000). However, there is evidence that a single change in location (i.e. a jump) might not always be sufficient to disrupt the inhibition created during the preview of the old items (Kunar, et al., 2003). Even though our ‘jumps’ were essentially random relocations, and the configuration of the old items was disrupted (cf. Kunar, et al., 2003; Watson, 2001), it is possible that a single jump was not able to fully abolish the preview benefit, leading participants to continue to apply inhibition to the old elements.

Third, a single jump might not have been noticed by our participants. Change blindness studies (O’Regan, Rensink, & Clark, 1999; Rensink, 2000; Simons & Levin, 1997)

demonstrate that people are very poor at noticing changes that occur when the transients that would normally be associated with such changes are rendered less visible. This is typically achieved by interleaving a blank screen (i.e., an artificial eye blink) between the changed images (e.g. Cole, Kentridge, & Heywood, 2004), presenting the changes during a real eye blink (O'Regan, Deubel, Clark, & Rensink, 2000), or by presenting competing transients ('mudsplashes', see O'Regan, et al., 1999) at the time when the change occurs. In terms of time-based selection, Watson and Kunar (2010) showed that shape changes in old items, that would normally disrupt the preview benefit, are rendered less effective if the changes are masked by moving occluders. When the change was not directly visible, it had less of a disruptive influence. In Experiment 1, it is possible that participants did not notice that the old items had changed their locations because the jumps were effectively masked by the onset of the new set of (relevant) search items. That is, the transients associated with the onset of the new items might have acted as 'mud splashes' (O'Regan, et al., 1999), which masked the motion of the old items. If participants were not aware of the jumps in the old items, then they might not have had a sufficiently strong explicit signal for changing their inhibitory strategy across the two different blocks of trials.

In order to test these possibilities, in Experiment 2 we made it much more obvious that the old items changed their locations before the new items arrived. Furthermore, although not approaching significance, we note that, numerically, the search rate on Standard Preview trials in the 80% Jump condition was over twice as slow as Standard Preview trials in the 80% Standard Preview condition (11.40 ms/item vs. 4.49 ms/item), which would be consistent with the strategic application of inhibition. Hence, Experiment 2 also provided a useful confirmation of the robustness of the current findings.

Experiment 2: Salient Multi-Location Jumps

Experiment 2 was similar to Experiment 1, except that the old items jumped locations four times during the preview period, and a fifth time when the new items appeared. If the lack of strategically controlled inhibition in Experiment 1 was due to participants not noticing the changes to the old items, or due to a single jump not being sufficiently disruptive, then we should now obtain evidence for the strategic application of inhibition.

Participants. Participants were 12 students at the University of Warwick (11 female, 1 male), aged between 19 and 23 years ($M = 18.8$, $SD = 1.47$) and participated in exchange for course credit or payment.

Stimuli, Apparatus, and Procedure. The apparatus and procedure were identical to those of Experiment 1. The stimuli were similar, except that on Jump trials, the previewed items jumped five times before the new items appeared, and the largest display size was 16 items. On a Jump trial, the previewed items appeared for 800 ms, they then jumped to new locations and remained visible for 300 ms, before jumping to another set of new locations and so on. On the fifth jump, the new items were added and the display remained unchanged until participants responded. On Standard Preview trials, the previewed items appeared for 800 ms, after which the new items were added to the display.

Results

Reaction Times: Trials with RTs less than 200 ms or greater than 10,000 ms were removed as outliers (0.49% of the data). Mean correct RTs for the Jump and Standard Preview trials are shown in Figure 3 and search slope statistics in Table 3. As in Experiment 1, search data were calculated using a $2 \times 2 \times 3$ repeated-measures ANOVA, which revealed a significant main effect of Trial Type $F(1,11) = 17.83$, $MSE = 70,650.08$, $p < .005$, and Display Size $F(2,22) = 64.53$, $MSE = 7528.64$, $p < .001$. The main effect of Block was not significant, $F < 1$. RTs on Standard Preview trials were shorter than on Jump trials, and RTs increased as the Display Size increased. There was also a significant Block \times Trial Type

interaction $F(1,11) = 9.79$, $MSE = 4603.21$, $p < .05$. Jump trial RTs were shorter in the 80% Jump block and Standard Preview trial RTs were shorter in the 80% Standard Preview block. The Trial Type \times Display size interaction was borderline¹ significant $F(1,11) = 3.27$, $MSE = 4775.80$, $p = .057$. However, the Block \times Display size interaction did not approach significance $F(2,22) = 2.26$, $MSE = 2999.48$, $p = .13$, nor did the Block \times Trial Type \times Display size interaction, $F < 1$. As in Experiment 1, planned follow-up comparisons compared the two Standard Preview and two Jump conditions individually. *Jump trials only:* RTs increased with Display Size, $F(2,22) = 39.76$, $MSE = 5934.16$, $p < .001$, however, neither the main effect of Block, $F(1,11) = 1.01$, $MSE = 14153.72$, $p = .34$, nor the Block \times Display Size interaction were significant, $F < 1$.

Standard Preview trials only: As with the Jump trials, RTs increased with display size, however, neither the main effect of Block, $F(1,11) = 1.03$, $MSE = 31615.17$, $p = .33$, nor the Block \times Display Size interaction proved significant, $F(2,22) = 1.75$, $MSE = 4436.64$, $p = .19$. Moreover, the numerical trend was for more efficient search in the 80% Jump block than in the 80% Standard Preview block (7.4 ms/item vs. 13.1ms/item respectively), which is the opposite of what would be expected if inhibition was being applied strategically.

Error rates. Error rates were low overall (1.53%) and are shown in Table 4. A 2 (Block) \times 2 (Trial type) \times 3 (Display size) repeated-measures ANOVA showed that the main effect of Display Size was marginally significant $F(2,22) = 2.89$, $MSE = 3.41$, $p = .08$. However, all other main effects and their interactions were non-significant, Block \times Display Size, $F(2,22) = 2.49$, $MSE = 5.17$, $p = .11$, Block \times Trial Type \times Display Size $F(2,22) = 2.54$,

¹ This is based on a non-directional test. Given that we would expect a preview benefit to occur in the standard preview condition but not in the jump condition, there is some justification for treating this as a directional test which would have been significant at the .05 level.

MSE = 2.64, $p = .10$, remaining F s < 1. The overall error rate on catch trials was low (4.39%), and these errors were not analyzed further.

Discussion

The main aim of Experiment 2 was to determine whether a strategic use of inhibition would emerge when the changes to the old items were made more salient, by having the old items jump several times. Despite these changes, there was no evidence that old items were being strategically inhibited. As in Experiment 1, search slopes of the Standard Preview trials did not differ between the two blocks of 80% Jump and 80% Standard Preview trials. Again, search on Jump trials was less efficient than on Standard Preview trials, suggesting that the stimulus jumps had been effective in disrupting the preview benefit. Taken together, Experiments 1 and 2 suggest that inhibition is not spontaneously withheld even when, 1) items move location, rendering any old item inhibition ineffective, and 2) when location changes are more salient and should be noticed easily.

Note that the old items in Experiments 1 and 2 moved location before the new items arrived and this would have had the effect of disrupting any location-based inhibition of those items. However, previous work has shown that old items can also be excluded by inhibition applied at the level of feature maps, that is, the item's color (Watson & Humphreys, 1998; Andrews, et al., 2011; Braithwaite, et al., 2003, 2004). In this way, old items remain deprioritized (even if they move), without the need for the involvement of complex tracking procedures (Watson & Humphreys, 1998), which are likely to be of low capacity (Pylyshyn & Storm, 1988). Accordingly, it is possible that participants might have continued to apply inhibition, because the old items maintained their color throughout the jump period, allowing a potential role for color-based inhibition to remain effective. Experiment 3 assessed this possibility by changing both the color and the locations of the previewed items before the new items arrived.

Experiment 3: Disrupting both feature and location-based inhibition

Even if participants realized that location-based inhibition was ineffective on 80% of the trials, they might still have tried to inhibit the previewed items because the color of the old items remained constant throughout the preview period. Furthermore, this maintenance of color could have encouraged them to continue to apply both location- and feature-based based inhibition, even when the old items jumped to new locations on the majority of trials. This possibility was tested in Experiment 3 by having the old items change both their locations and their color during the preview period. This aim of using this procedure was to disrupt both location- and feature-based inhibition.

Importantly, the color change of each old item was independent of the color changes of other items (i.e., the intermediate preview displays were of mixed colors). This procedure should make it more difficult to group the old items (Jiang, et al., 2002) into a single set, based on a common color (see also Duncan & Humphreys, 1989). In turn, this should prevent participants from being able to apply inhibition to a single color-feature map, and increase the likelihood that feature-based inhibition would be disrupted, due to the associated changes of activity within multiple color maps (Watson & Humphreys, 1998). Taken together, these aspects of the design should produce maximal disruption to both location- and feature-based inhibition, as well as providing highly-noticeable changes in terms of participants' subjective experience. As in the previous experiments, the proportion of Jump trials and Standard Preview trials was manipulated in an 80:20 ratio, across two different blocks of trials.

Method

Participants. Twelve students from the University of Warwick (6 male, 6 female) aged between 19 and 45 years ($M = 24.4$, $SD = 7.74$) and participated in exchange for course credit or payment.

Stimuli, Apparatus and Procedure. The stimuli and procedure were similar to those of Experiment 2, except that each preview item also changed color each time it jumped to a new location. Thus, a preview jump trial consisted of a set of pink squares, within which each square then jumped to a new location (after 800 ms), at the same time changing to a new color, independently within the set. The possible colors consisted of: light green [RGB = 50, 205, 50; CIE_{xy} = .323, 442; lum = 39 cd/m²], orange [RGB = 255, 265, 0; CIE_{xy} = .451, 454; lum = 79 cd/m²], yellow [RGB = 238, 238, 0; CIE_{xy} = .441, 448; lum = 66 cd/m²], olive green [RGB = 142, 142, 56; CIE_{xy} = .356, 352; lum = 26 cd/m²] and bright red [RGB = 238, 0, 0; CIE_{xy} = .564, 318; lum = 19 cd/m²]. The color change was randomized individually for each item, with every item having the same probability of changing into one of the five possible colors. After a further 300 ms, the items jumped and changed their color again. On the fifth jump, all items returned to the initial pink color, and at the same time the new (blue) items were added, together with the target, if presented. Having the items change back to their start color ensured that the final display on jump trials matched that of the Standard Preview condition and hence, allowed performance in the two trial types to be compared directly. The Standard Preview condition was identical to that of Experiment 2.

Results

Reaction times. Trials with RTs less than 200 ms or greater than 10,000 ms were removed as outliers (0.01 % of the data). Mean correct RTs for trials in the Jump and Standard Preview blocks are shown in Figure 4, and search slope statistics are shown in Table 5. Mean correct RTs were analyzed using a 2 (Block type: 80% Jump or 80% Standard Preview) \times 2 (Trial Type: Jump or Standard Preview) \times 3 (Display size) repeated-measures ANOVA. Standard Preview trials had shorter RTs than the Jump trials, $F(1,11) = 12.03$, MSE = 8715.81, $p < .01$, and RTs increased as the Display Size increased $F(2, 22) = 53.72$, MSE = 9923.85, $p < .001$. In addition, there was a significant interaction of Block \times Trial Type

$F(1, 11) = 32.62$, $MSE = 2150.44$, $p < .001$, indicating that the RTs for the Standard trials were longer overall in the 80% Jump block. A Trial Type \times Display Size interaction revealed that the Jump trials were affected more by the Display Size than the Standard Preview trials $F(2, 22) = 15.99$, $MSE = 3019.19$, $p < .001$. However, there was no main effect of Block $F(1, 11) = 2.92$, $MSE = 23228.80$, $p = .12$, and neither the Block \times Display Size, nor the three-way Block \times Trial Type \times Display Size interaction reached significance, both $F_s < 1$. Two planned comparisons compared the two Standard Preview and two Jump conditions individually, similarly to the previous experiments.

Jump trials only: RTs increased with display size, $F(2, 22) = 57.26$, $MSE = 7820.29$, $p < .001$, however, neither the main effect of block, $F < 1$ nor the Block \times Display Size interaction approached significance, $F(2, 22) = 1.48$, $MSE = 4042.89$, $p = .25$.

Standard Preview trials only: RTs increased with Display Size, $F(2, 22) = 26.08$, $MSE = 5122.59$, $p < .001$, and RTs were shorter overall in the 80% Standard Preview block than in the 80% Jump block, $F(1,11) = 9.39$, $MSE = 14688.32$, $p < .05$. However, of most relevance, search efficiency, as measured by the Block \times Display Size interaction, did not differ between the two types of block ($F < 1$), with slopes of approximately 12 ms/item for both blocks.

Error rates. Error rates were low overall (1.44%) and are shown in Table 6. A 2 (Block: 80% Jump or 80% Standard) \times 2 (Trial type: Jump or Standard) \times 3 (Display size) repeated-measures ANOVA revealed that there were no significant main effects or their interactions; Block, $F(1, 11) = 3.02$, $MSE = 8.98$, $p = .11$, and Display Size, $F(2, 22) = 2.42$, $MSE = 5.52$, $p = .11$, all remaining $F_s < 1$. The overall error rate on catch trials was 4.17% and these were not analyzed further.

Discussion

Experiment 3 aimed to determine if the strategic use of inhibition would emerge when the old items changed both their locations and their colors throughout the preview period. As in the previous experiments, search efficiency was reduced in the Jump trials compared with the Standard Preview trials, demonstrating that the color/location manipulation abolished the preview benefit. However, of most relevance, there was again no reliable difference between search efficiency on the Standard Preview trials in the 80% Standard Preview and 80% Jump blocks². Thus, participants continued to apply inhibition to the old items, even when: 1) it would only have been effective on 20% of the trials and, 2) it was clear that the old items changed their locations and colors before the new items arrived. It follows that the constant color of the old items throughout the preview period in Experiments 1 and 2 was unlikely to be responsible for the continued application of inhibition, even when the inhibition would clearly be ineffective on the majority of trials.

Comparison of Experiments 2 and 3

In order to increase statistical power and to compare any potential differences between Experiments 2 and 3 directly, we combined the data from Experiments 2 and 3, adding ‘Experiment’ as a between-subjects factor. Mean combined correct RTs for trials in

² Although there was no evidence for a difference in search slopes, there was evidence that overall RTs were shorter in the 80% Standard Preview block than in the 80% Jump block. It is also the case that a preview benefit is sometimes exhibited in overall RTs, rather than search slopes (e.g., Al-Aidroos et al., 2012; Blagrove & Watson, 2010). However, specifically in the present work, we would treat this difference with some caution. This is because an overall difference in RT can also reflect differences in arousal, alerting, and warning signal effects (see Watson & Humphreys, 1997). It is possible that the onset of the preview items act as a warning or alerting signal for the onset of the search display. Here, the presentation of the preview display in a Standard Preview trial might act as a more reliable warning signal for the onset of the search display, than the more complex multi-jump preview displays in the Jump trials. When a block contains mostly Standard Preview trials, participants might be more sensitive to the preview-based warning signal than when a block contains fewer Standard Preview trials. This would lead to a reduction in overall RTs in the 80% Standard Preview block. Hence, in the present work, we prefer to place most interpretational emphasis on the search slope measure of the preview benefit rather than on overall RT differences.

the Jump and Standard Preview blocks are shown in Figure 5 with the combined search slope statistics and error rates shown in Tables 7 and 8.

Response times. The overall pattern of results was similar to that of the individual analyses for Experiments 2 and 3. There was a significant main effect of Trial Type, $F(1, 22) = 27.42$, $MSE = 6339.48$, $p < .001$ and of Display Size, $F(2, 44) = 122.51$, $MSE = 7412.06$, $p < .001$, but no effect of Block, $F(1, 22) = 1.44$, $MSE = 8.98$, $p = .24$. In addition, the main effect of Experiment was also non-significant, $F < 1$. The Block \times Trial Type interaction did prove significant, $F(2, 44) = 122.51$, $MSE = 7412.06$, $p < .001$, as did the Trial Type \times Display Size interaction, $F(2, 44) = 15.09$, $MSE = 3897.47$, $p < .001$. No other main effects or their interaction were significant, all $F_s < 2.08$, $p_s > 0.13$. Importantly, the absence of a Block \times Display Size interaction suggests that there is no strategic control when location-based or both location-based and feature-based inhibition is disrupted. Planned comparisons assessed the two Standard Preview and the two Jump conditions individually.

Jump trials only: There was no significant main effect of Block, $F < 1$, while the main effect of Display Size was significant, $F(2, 44) = 96.97$, $MSE = 6877.21$, $p < .001$. The Block \times Display Size interaction was not reliable, $F(2, 44) = 1.21$, $MSE = 3419.87$, $p = .31$, and neither was the Display Size \times Experiment interaction, $F(2, 44) = 2.45$, $MSE = 6877.32$, $p = .09$. In addition, the between-subjects main effect of Experiment, the Block \times Experiment, and the Block \times Display Size \times Experiment interaction did not reach significance, $F_s < 1$.

Standard Preview trials only: There was a significant effect of Block, indicating longer overall RTs in the 20% Standard Block, $F(1, 22) = 6.59$, $MSE = 23151.56$, $p < .05$. The main effect of Display Size was also significant, $F(2, 44) = 50.97$, $MSE = 4432.21$, $p < .001$. The between-subjects effect of Experiment did not prove to be significant, nor did the Block \times Display Size, Block \times Experiment, or Display Size \times Experiment interactions, all F_s

< 1 . The Block \times Display Size \times Experiment interaction was also not significant $F(2, 44) = 1.73$, $MSE = 4366.05$, $p = .19$.

Error rates. Errors were analyzed in the same way as the RT data. There was a significant main effect of Display Size, $F(2, 44) = 5.17$, $MSE = 4.47$, $p < .05$, indicating that error rate increased with display size. There was no significant main effect of Block, $F(1, 22) = 1.62$, $MSE = 23151.56$, $p = .05$, nor Block \times Trial Type, $F(1, 22) = 1.05$, $MSE = 5.16$, $p = .32$, Block \times Display Size \times Experiment, $F(2, 44) = 1.97$, $MSE = 7.65$, $p = .15$ and Block \times Display Size \times Trial Type \times Experiment, $F(2, 44) = 2.65$, $MSE = 3.60$, $p = .08$ interactions. The Block \times Display Size, Block \times Experiment, Trial Type \times Experiment, Block \times Trial Type \times Experiment, Trial Type \times Display Size interactions, Display Size \times Experiment, Trial Type \times Display Size \times Experiment, and Block \times Trial Type \times Display Size interactions were all non-significant, $F_s < 1$.

The findings suggest that: 1) having the items change both location and color was no more disruptive to the preview benefit than having them simply change location, and 2) location and color changes in the previewed items were no more effective at prompting the strategic use of inhibition than location changes alone. Taken together, the results from Experiments 1 to 3 show that, even when inhibition is disrupted on the majority of trials, people continue to apply it.

So far, the effectiveness of applying inhibition has been manipulated by disrupting the inhibition applied to the old items (i.e., via changes in their location/color). Participants appeared insensitive to these disruptions and continued to apply inhibition. However, given that the blocks consisted of an 80:20 mix of different trials, participants might have chosen to adopt a single inhibitory strategy, because it would still have been effective in improving search efficiency, even if only on a minority of trials. Furthermore, there was no way for participants to be able to predict whether a Jump or Standard Preview trial was to appear next

and so, they would not have been able to modulate their inhibition on a trial-by-trial basis. In Experiment 4, we examine whether participants will modulate their application of inhibition if given advance information regarding the type of trial that will appear.

Experiment 4: Cued location-based disruptions of inhibition

In Experiments 1-3, we showed that participants continued to apply inhibition throughout both Standard Preview and Jump blocks. However, even though cognitive resources are consumed by applying inhibition, alternative strategies might have created even greater cognitive costs. For example, participants did not know whether a Jump or a Standard Preview trial was going to occur until sometime into the actual trial. By that stage, it might have been difficult or costly to reconfigure their attentional set (i.e., apply or withhold inhibition). Thus, a simple strategy of always applying inhibition might have appeared the easiest and most efficient approach. Furthermore, participants might have placed more decisional weight on the trials in which inhibition would have been helpful, and might have been unaware of the full extent of the 80:20/20:80 ratio of trials (i.e., on which inhibition would have been useful). If this is the case, then we might expect participants to apply inhibition strategically if they know in advance of each trial, whether the application of inhibition would be effective or not. In Experiment 4 we tested this by providing participants with advance information directly before each trial as to whether the trial would be 1) a Standard Preview trial (in which inhibition would be useful), or 2) a Jump trial (in which any inhibition would be disrupted, and therefore not effective). Specifically, directly before each trial we presented participants with an 80% valid visual cue, which informed them whether the next trial would be a Jump trial or a Standard Preview trial (see Figure 6).

If participants applied inhibition strategically, we would expect search to be more efficient on validly cued Standard Preview trials (participants would expect a Standard Preview trial and apply inhibition), than on invalidly cued Standard Preview trials (participants would expect a

Jump trial and not apply inhibition). In contrast, on Jump trials, cue validity should have little effect. This is because on validly cued Jump trials, participants would not apply inhibition (i.e., because they were expecting a jump). On invalidly cued Jump trials, participants would expect a Standard Preview trial and apply inhibition, but the inhibition would be rendered ineffective because of the Jump (as in Experiment 1).

Method

Participants. Twelve students from the University of Warwick (3 male, 9 female) aged between 21 and 37 years ($M = 25.25$, $SD = 4.71$) participated in exchange for payment.

Stimuli, Apparatus and Procedure. The apparatus and procedure were similar to those of Experiments 1-3. However, in Experiment 4, each trial was preceded with the words (the cue) ‘Jump’ or ‘No Jump’ presented for 1500 ms displayed at the screen center. Following the cue there was a blank screen (500 ms), followed by a central fixation dot (750 ms), after which the preview/search displays were presented. Consistent with previous experiments, there were two blocks with 180 search trials and 18 catch trials. Whereas blocks in the preceding experiments contained an unequal proportion (80:20/20:80) of Jump and Standard Preview trials, in the current experiment both blocks were identical. Each block consisted of 50% Jump trials and 50% Standard Preview trials. For both types of trials, the pre-trial cue was 80% valid. That is, 80% of *Jump* trials were cued validly as *Jump* trials, and 20% were cued invalidly as *No Jump* trials. Similarly, 80% of the Standard trials were cued validly as *No Jump* trials and 20% were cued invalidly as *Jump* trials.

The stimuli were the same as in Experiment 1, because the multiple jumps or color changes as used in Experiments 2 and 3 would have provided additional cues of trial type, prior to the final search display. This could have discouraged participants from following the written cues, and would have confounded trials that were invalidly cued.

Results

Reaction times. There were no outliers in the data (RTs less than 200 ms or greater than 10,000 ms). Mean correct RTs for Jump and Standard trials are shown in Figure 7 and Table 9 shows the search slope statistics. Mean correct RTs were analyzed using a 2 (Cue Validity: Valid or Invalid) \times 2 (Trial Type: Jump or Standard Preview) \times 3 (Display Size) within subjects ANOVA. Standard Preview trials were faster than Jump trials, $F(1,11) = 106.03$, $MSE = 3326.79$, $p < .001$, and RTs increased with Display Size, $F(2,22) = 63.39$, $MSE = 5511.80$, $p < .001$. RTs also increased more as display size increased on Jump trials than on Standard Preview trials, $F(2,22) = 29.45$, $MSE = 2654.61$, $p < .001$. There was no significant effect of Validity, nor was there significant interactions of Validity \times Trial Type, Validity \times Display Size, and Validity \times Trial Type \times Display Size, all $F_s < 1$. As above, to confirm these results for the two trial types individually, two separate planned comparisons were carried out.

Jump trials. RTs increased as Display Size increased, $F(2,22) = 67.01$, $MSE = 5654.65$, $p < .001$. However, neither the main effect of Cue Validity, nor the Cue Validity \times Display Size interaction reached significance, $F_s < 1$.

Standard Preview trials. RTs increased with Display Size, $F(2,22) = 19.38$, $MSE = 2511.77$, $p < .001$. However there was no significant effect of Cue Validity and Cue Validity did not interact with Display Size, both $F_s < 1$.

Error rates. Overall error rates were low (0.99%) and are shown in Table 10. A three-way repeated-measures ANOVA showed that there was a borderline significant effect of Trial Type, $F(1, 11) = 4.42$, $MSE = 11.47$, $p = .06$. However, the main effects of Validity, Display Size, and all interactions were non-significant, all $F_s < 1$. On catch trials, the overall error rate was 3.01% and these data were not analyzed further.

Discussion

One possibility for why participants did not apply inhibition strategically throughout Experiments 1 to 3 is that there was no information available to them directly before each trial indicating whether or not inhibition would be useful. Given that inhibitory processing would still have been useful overall (even if only on a minority of trials), participants might have chosen to apply it on every trial. Furthermore, even though the presence of changes in the previewed items would have been very salient on a trial-by-trial basis (especially in Experiments 2 and 3), the extent of the 20:80/80:20 trial distribution might not have been as salient. This is especially the case if participants had weighted their decisional focus or overall strategy on trials in which inhibition would have helped task performance. To test this, in Experiment 4 participants were explicitly cued in advance of each trial as to whether it would be a Jump or a Standard Preview trial.

If inhibition was applied in a strategic manner then we would expect to find a difference in search performance, when comparing validly cued Standard Preview trials (in which participants should have applied inhibition to the preview items) with invalidly cued Standard Preview trials (in which participants should have withheld inhibition). However, there was no hint that this was the case, with search slopes on validly cued Standard Preview trials being 7.13 ms/item, compared with 7.61 ms/item for invalidly cued Standard Preview trials. As in previous experiments, search on Jump trials was much less efficient than on Standard Preview trials (~20 ms/item vs. ~7 ms/item), confirming that changing the locations of previewed items when new elements were added was sufficient to abolish a preview benefit. The finding of a relatively large difference in search slopes between the Standard Preview and Jump trials also confirms that there was enough 'room' for Standard Preview search to become less efficient, if participants had chosen not to apply inhibition on the validly cued Standard Preview trials.

Overall, the findings confirm the conclusions obtained from Experiments 1 to 3 that participants appear to apply inhibition in time-based visual selection without strategic modulation. Moreover, the results of Experiment 4 show, 1) that this is not simply because participants were unaware of changes, 2) nor was it because there were unsure of the overall distribution of trials in which inhibition would have been helpful, and 3) the lack of strategic inhibitory control was not due to insufficient time for participants to readjust their attentional sets within a trial.

Of note in Experiments 1 to 4, participants had to search through the display in order to find the target item. Search was relatively inefficient, with search slopes on Jump trials of approximately 20 ms/item, which equates to a search rate of around 40 ms/item³. It is possible that inhibition might be applied by default in any time-based selection tasks in which a relatively inefficient search has to be made. This type of behavior might well be adaptive, if the cost of applying inhibition is relatively low compared to the potential cost of missing a predator in complex (i.e., inefficient) search conditions. To examine this possibility, in Experiment 5 we changed the global ‘search environment’ by introducing trials in which the target was easily detected without the need for search processes.

Experiment 5: Inhibition in Salient Preview Search Contexts

In Experiment 5, we assessed whether inhibition is applied strategically when target detection can be performed without having to search the display on a minority or majority of occasions. It is possible that inhibition is applied by default (a safe strategy) only when target detection requires effortful search. If a majority of trials do not require the engagement of search processes to find the target then this might trigger participants to stop applying inhibition.

³ Participants could respond as soon as they had found the target item. For a classic serial search (Treisman & Gelade, 1980), this would lead to participants searching through approximately half of the display items on each trial. Hence, the actual search rate through stimuli is approximately half (i.e., the search slope is double) of that obtained on target present trials.

In outline, the Standard Preview trials were the same as those presented in Experiments 1 to 4, consisting of a preview display of pink squares, followed by the addition of a search display containing blue circles and a blue square target item. However, the *Jump trials* were replaced with *Salient trials*. On a Salient trial, pink squares appeared for 1000 ms, followed by the addition of a *single* blue square target. Thus, on Salient trials, the target would be easily detected, because it would be a singleton blue item which was accompanied by a unique luminance onset within the display. Accordingly, on Salient trials, there would be no need to inhibit the old previewed items, because the target would pop-out (Treisman & Gelade, 1980) from the display.

If top-down inhibition operates by default, then it should be applied whenever the previewed items are presented – here, we should expect no difference between blocks of trials with many Salient trials, compared with few Salient trials. However, if inhibition is subject to the observer's control, then it might not be applied if it is expected that the subsequent search will be easy (i.e. the target will be obvious and minimal, if any, search will be needed to locate it).

Method

Participants. Twelve students (6 male, 6 female) from the University of Warwick aged 20 to 26 years ($M = 23.41$, $SD = 1.92$) participated for course credit or payment. .

Stimuli, Apparatus, and Procedure. The stimuli, apparatus, and procedure were similar to those of Experiment 1-4, except that, Jump trials were replaced by Salient trials. Thus one block of trials consisted predominantly of 80% Standard Preview trials and 20% Salient trials and the other block consisted of 20% Standard Preview trials and 80% Salient trials. A Salient trial consisted of the presentation of pink squares displayed for 1000 ms, after which a single blue square target was added to the display. There were no additional distractors in the second set of items. Thus, the display sizes for Salient trials consisted of 3,

5, and 9 items in total (see Figure 8). The Standard Preview trials were identical to those in Experiment 1.

Results

Response times. Trials with RTs less than 200 ms or greater 10,000 ms were removed as outliers (0.26 % of the data). Mean correct RTs for Salient and Standard Preview trials are shown in Figure 9, and the mean search slope statistics are shown in Table 11. As the display sizes for the Salient and Standard preview trials differed (3, 5, and 9 items vs. 4, 8, and 16 items), two separate 2 (Block: 80% Salient or 80% Standard Preview) \times 3 (Display Size) ANOVAs were performed for each trial type in order to compare search efficiency in each block.

Standard Preview Trials: For the Standard Preview trials, there was a marginally significant effect of Block $F(1,11) = 4.29$, $MSE = 8178.49$, $p = .062$, and a significant effect of Display Size $F(2,22) = 67.64$, $MSE = 1761.18$, $p < .001$. However, of most relevance was a significant Block \times Display Size interaction $F(2,22) = 4.18$, $MSE = 912.58$, $p < .05$, indicating that the Display Size affected the RTs of Standard Preview trials more in the 80% Salient block than in the 80% Standard Preview block.

Salient Trials: There were no significant main effects or their interaction; Block, $F(1,11) = 2.19$, $MSE = 2048.30$, $p = .17$, Display size, Block \times Display size, both $Fs < 1$.

Error rates. Error rates were low overall (1.41%) and are shown in Table 12. Again, due to the difference in display sizes, separate 2 (Block: 80% Salient or 80% Standard Preview) \times 3 (Display Size) ANOVAs were performed for each trial type.

Standard Preview trials: There were more errors overall in the 80% Salient block than the 80% Standard Preview Block, $F(1,11) = 5.17$, $MSE = 21.90$, $p < .05$. However, neither the main effect of Display Size, $F(2,22) = 1.07$, $MSE = 24.69$, $p = .36$, nor the Block

× Display Size interaction approached significance, $F(2,22) = 1.94$, $MSE = 22.89$, $p = .17$.

The overall error rate on catch trials was 4.16 %, and these data were not analyzed further.

Salient trials: Errors increased as the Display Size increased, $F(2,22) = 4.34$, $MSE = 5.22$, $p < .05$. However, there was no significant main effect of Block, $F < 1$, or a significant Block × Display Size interaction, $F(2,22) = 1.24$, $MSE = 2.24$, $p = .31$.

Discussion

The main purpose of Experiment 5 was to determine whether inhibition would be applied strategically, if the target was salient on a minority/majority of trials. Search slopes on salient trials were essentially flat, confirming our prediction that salient trials would produce efficient target detection. However, of most interest, and in contrast to Experiments 1 to 4, we now found a difference in performance between the Standard Preview trials across the two blocks of trials in which the ratio of Standard to Salient trials was manipulated. Specifically, when the block contained a minority of Salient trials, RTs on Standard Preview trials were marginally faster overall, and search slopes were flatter than when the block of trials contained a majority of Salient trials. This suggests that when the majority of trials within a search task do not require attentional search, participants do not apply (or are less likely to apply) inhibition to old, previewed distractors.

Experiment 6: Testing alternative accounts

Although the results of Experiment 5 are compatible with an account that suggests the strategic application of inhibition, there remain alternative explanations. First, it is possible that increasing the number of easy pop-out searches within a block of trials encouraged participants to change their style of search. Specifically, a greater ratio of pop-out trials might encourage participants to adopt a more passive style of visual search compared with a more active search style. For example, Smilek et al. (2006) found that actively instructing participants to adopt a passive style of search produced more efficient search (shallower

search slopes) than instructing participants to adopt an active style of search. However, this account seems unlikely, because if more passive search results in increased search efficiency, then we would expect that search would have been more efficient in the 80% Salient block of trials than in the 80% Standard Preview block of trials. However, we found the opposite, with search in the 80% Standard Preview block being *more* efficient than search in the 80% Salient block.

An alternative version of this account might, however, still hold. If passive search is less influenced by top-down preview inhibition, or if adopting a passive search strategy interferes with the deployment of top-down inhibition, then search would become *less* efficient in the 80% Salient block of trials. By this account, adopting a passive attentional set might prevent the adoption of a more active and top-down inhibitory set against the previewed items. This account has some links to the finding that maintaining an attentional set for secondary load tasks can reduce the preview benefit (Humphreys, Watson, & Jolicoeur, 2002).

A further alternative account can be developed on the basis of intertrial priming. Previous work has shown that when the target and distractor features do not change over consecutive trials, RTs can decrease (e.g., Becker, 2008ab; Lamy, Antebu, Aviani & Carmel, 2008; Maljkovic & Nakayama, 1994). Recall that in the Standard Preview trials, participants searched for a blue square among blue circles and pink squares. On the Salient trials, participants only needed to search for a blue luminance-onset square. Now, in the 80% Standard Preview block, there would have been many trials on which the Standard Preview trial target and distractor identities would have been repeated over consecutive trials. Such repetition of target and distractor features might have led to a reduction in RTs/improved search efficiency as the number of repeats increased. In contrast, when only 20% of the trials were Standard Preview trials, there would have been fewer repeats of the target-distractor

identities and so the improvement by priming would have been much less. According to this account, the improved search efficiency in the 80% Standard Preview block (i.e., compared with the 80% Salient block) represents a difference in the amount of target-distractor intertrial priming, rather than reflecting a difference in the application of inhibition to the previewed distractors. Going somewhat against this account, previous findings show that priming effects due to repetition of target and distractor features over consecutive trials usually produce a benefit in overall reaction times, and have little effect on search slopes (e.g., Geyer, Müller, & Krummenacher, 2006). Instead, in Experiment 5 we found any such repetition priming produced a marginal effect on overall RTs, but more importantly, there was also a significant reduction in search slope.

Nonetheless, to address these potential alternatives in Experiment 6, we repeated the conditions of Experiment 5, except that rather than presenting preview displays, we presented all the stimuli simultaneously. That is, Standard Preview trials were replaced with Standard FEB trials, which had no preview gap. FEB trials are used in preview search studies as a search efficiency baseline that does not involve the use of inhibition (Watson & Humphreys, 1997, 1998). If changes in search strategy or differences in intertrial priming can account for the results of Experiment 5, then we should obtain a similar pattern of results when all the stimuli are presented at the same time, given that the stimuli are identical to those of Experiment 5.

Method

Participants. Twelve students from the University of Warwick (all female) aged 18 to 19 ($M = 18.17$, $SD = 0.39$) participated for course credit.

Stimuli, Apparatus and Procedure. The stimuli, apparatus and procedure were the same as in Experiment 5, with the exception that all the search stimuli were presented simultaneously.

Results

Response times. There were no outliers (trials with RTs less than 200 ms or greater 10,000 ms) in the data. Mean correct RTs for the salient and standard FEB trials are presented in Figure 10, and mean search slope statistics in Table 13. As in Experiment 5, two separate 2 (Block: 80% Salient trials or 80% Standard FEB trials) \times 3 (Display Size) ANOVAs were conducted for each trial type, due to differences in display sizes (3, 5, and 9 items on Salient trials vs. 4, 8, and 16 items on Standard FEB trials).

Standard FEB Trials: There was a significant effect of Display Size, $F(2,22) = 86.62$, $MSE = 7093.17$, $p < .001$. However, neither the main effect of Block $F(1,11) = 1.97$, $MSE = 7555$, $p = .19$, nor the Block \times Display Size interaction, $F < 1$, were significant. Thus, there was no evidence that search was faster overall or more efficient in the 80% Standard FEB block of trials compared with the 80% Salient trials block.

Salient Trials: RTs increased at a rate of approximately 3 ms/item as Display Size increased, $F(2,22) = 4.08$, $MSE = 685.99$, $p < .05$. However, neither the main effect of Block, $F(1,11) = 1.69$, $MSE = 3296.43$, $p = .22$, nor the Block \times Display Size interaction, $F(2,22) = 1.54$, $MSE = 444.87$, $p = .24$ proved significant.

Error rates. Error rates were low overall (1.18%), and are presented in Table 14. Errors were analyzed using two separate 2 (Block: 80% Salient or 80% Standard FEB trials) \times 3 (Display Size) ANOVAs for each trial type.

Standard FEB trials: There were no significant main effects of Block, $F(1,11) = 2.61$, $MSE = 13.28$, $p = .13$, or Display Size, $F(2,22) = 1.33$, $MSE = 11.03$, $p = .29$. The Block \times Display Size interaction was also non-significant, $F < 1$. The overall error rate on catch trials was 4.63%, and catch errors were not analyzed further.

Salient trials: There were more errors in the 80% Standard FEB Block than in the 80% Salient Block, $F(1,11) = 5.50$, $MSE = 4.38$, $p < .05$. However, there was no significant effect of Display Size or a significant Block \times Display Size interaction, both $F_s < 1$.

Discussion

The main purpose of Experiment 6 was to test whether a shift to a passive search strategy, or intertrial priming effects could account for the results of Experiment 5, rather than being due to the strategic application of inhibition. This was achieved by repeating the conditions of Experiment 5, except that all stimuli appeared simultaneously rather than via the two-stage preview procedure. Both the passive search strategy and the intertrial priming accounts predict that we should observe a difference in search efficiency for the 80% Standard Preview block compared with the 80% Salient block, for Standard Preview search trials, which we did not.

Taken together, the results of Experiment 6 suggest that neither the passive account nor the intertrial priming account can explain the results of Experiment 5. Instead, we suggest that the difference in search efficiency across the 80% Salient block and 80% Standard Preview in Experiment 5 reflects the strategic deployment of inhibition applied to the previewed distractors. One might ask why we did not find any evidence for a priming effect in Experiment 6. The most likely explanation is that target-distractor priming might be weaker when there is a mix of both salient and non-salient search tasks within a single block of trials (cf., Maljkovic & Nakayama, 1994; Geyer, Müller, & Krummenacher, 2006).

The current results are also suggestive regarding the possible level of modulation of inhibition indicated in Experiment 5. Given that all the stimuli in Experiment 6 appeared simultaneously, there was no opportunity for participants to inhibit a subset of the stimuli. Hence, search performance in Experiment 6 is equivalent to what would be obtained if participants had not inhibited *any* of the previewed items in Experiment 5. In other words, the

conditions in Experiment 6 were equivalent to the full-element-baseline (FEB) conditions, often presented in previous studies of time-based selection.

With this in mind, if participants in Experiment 5 had not inhibited any of the preview stimuli, then search should be approximately the same as that obtained in Experiment 6. However, considering the 80% Standard Preview trial condition of Experiment 5 (in which inhibition was reduced), search was still substantially more efficient than in the equivalent condition of Experiment 6, in which inhibition could not have been applied. This suggests that, even when there were a large number of trials on which search was easy and inhibition was not necessary (Salient trials), people reduced their level of inhibition, but they did not choose to abandon it altogether. This reduction might reflect a weaker application of inhibition to all items in the field, or that inhibition might be applied in an all-or-none fashion, but the number of trials on which inhibition was applied was modulated. In the latter case, participants might be matching probability of applying inhibition to the proportion of trials in which it would be useful⁴.

General Discussion

Overview and summary of findings

In six experiments, we investigated whether top-down inhibition in time-based visual selection conditions is applied to old irrelevant items strategically, or by default, regardless of the subsequent level of benefit. This was accomplished by comparing preview performance in conditions, in which it was advantageous to apply inhibition, with conditions in which inhibiting old items would have no benefit. In Experiments 1-4, we disrupted the effects of inhibition and varied how obvious the disruption was. In Experiment 5, we made the target salient enough that it could be detected efficiently, without the aid of inhibitory processing or

⁴ Differentiating between these possibilities would require an analysis of the RT distributions, for which there is insufficient data in the present study.

effortful search. Experiment 6 served as a test of alternative accounts of the results from Experiment 5 and provided an indication of what search would be like if no elements were being inhibited.

Given that visual marking is claimed to be a top-down and resource-demanding process (Watson & Humphreys, 1997), we might expect that it would be applied maximally in conditions in which inhibition would be helpful, and withheld in conditions when inhibiting old items would be of little use. The alternative account is that, in conditions of time-based selection, inhibition of old items is the default behavior, and is not sensitive to the relative cognitive costs associated with applying it.

In conditions in which effortful search is required, our findings suggest that top-down inhibition operates mostly by ‘default’, and seems to be prone to little, if any, strategic modification (Experiments 1 to 4). In contrast, in situations in which the majority of targets can be detected without effortful search, there was evidence that participants applied inhibition strategically (Experiment 5). However, even in those situations, inhibition was not abandoned altogether, but was still applied to some degree (either weakly, or on a reduced number of trials).

Strategic application of top-down processes

Although top-down processes have been traditionally considered to be easily withheld and modified, our results show that people may not necessarily recruit them in a strategic way. Consistent with this, past research suggests that there may be a strong bias or preference towards certain search strategies, even if those strategies might not be optimal for performance (Bacon & Egeth, 1994; Leber & Egeth, 2006). For example, Bacon and Egeth (1994) found that participants switched to a feature-based search strategy, if a singleton-detection strategy was not efficient. However, when either strategy could be used to complete the goal, participants showed a preference for the singleton-detection strategy. This was the

case, even when the singleton detection strategy was susceptible to greater distraction and hindered performance. Similarly, Leber and Egeth (2006) demonstrated that if participants were trained in either one of these two strategies, they continued to use the same strategy, even if it led to worse performance. Both studies suggest that participants do not evaluate the effectiveness of the search strategies they are deploying, based on task demands, as long as their goal is reached.

It is possible that these effects are due to an implicit use of high-level cognitive processes operating without conscious intention, but in a goal-directed fashion, as recent research has shown for working memory (Hassin, Bargh, Engell, & McCulloch, 2009; Maljkovic & Nakayama, 1994), inhibitory control in executive functions (Van Gaal, Ridderinkhof, Johannes, Scholte, & Lamme, 2008; Van Gaal, Ridderinkhof, Johannes, Scholte, & Lamme, 2010), space-based attention (Zhou & Davis, 2012) and object-based attention (Norman, Heywood, & Kentridge, 2013).

There was, however, some evidence for the strategic application of inhibition in Experiment 5. This shows that inhibition is modified in some situations, as the inhibitory account of visual marking proposes. As noted earlier, participants might have modulated inhibitory processing by applying inhibition more weakly, or they might have applied inhibition on a reduced proportion of trials. However, an alternative explanation is that inhibition was withdrawn completely, and the resulting benefit was the result of a residual anticipatory set for the target stimuli. Consistent with this, previous research has found evidence for the involvement of dual attentional sets in preview search – an inhibitory set directed towards irrelevant items, and an anticipatory based on expectations of target features (Braithwaite & Humphreys, 2003; Watson & Humphreys, 2005). A greater proportion of ‘pop-out’ salient trials might have amplified, or served to maintain a feature-based anticipatory set, even if the inhibitory attentional set was withdrawn. Indeed, the relative

contribution of an anticipatory set, in addition to the inhibitory set in the preview benefit, remains an area of investigation. Note that the current data cannot distinguish between these different possibilities, but also that the accounts outlined above need not be mutually exclusive.

The current results demonstrate the nature of cognitive control and voluntary action in time-based visual selection. In summary, we hypothesize two reasons for applying effortful cognitive operations even when they produce little benefit. First, goal-directedness seems to be the key principle underlying the implementation of attentional strategies. Therefore, an inhibitory template against currently visible items is likely to be activated, when the goal is to find a target item which is anticipated, but has not yet appeared. In contrast, when the goal itself is changed, such as in Watson and Humphreys' (2000) study, when a probe dot always fell on the locations of old items and prioritizing new information was never needed, inhibition was abolished. Therefore, accomplishing the goal is likely to be crucial to strategic modification, rather than the actual efficiency of the strategy. Second, as this process seems to be carried out fairly implicitly, participants might not be fully aware of the costs of applying inhibition. Accordingly, despite visual marking being resource demanding, participants might have continued to apply it even in conditions in which there was little overall advantage to be gained. In this sense, the strategy would seem adaptive because the relatively small, possibly imperceptible cost of applying inhibition might be trivial, compared with the cost of missing potentially important new information.

Visual marking as a top-down inhibitory process and the effect of location changes

Given that there seems to be a consistent preview effect in all of the experiments, do the results demonstrate top-down control at all, or could they be due to bottom-up, automatic onset capture? With regards to the different accounts of the preview benefit, the present data provide additional support for a role of top-down inhibition (Watson & Humphreys, 1997). If

onset capture was entirely responsible for driving the preview benefit, we would have expected no difference between the Standard Preview trials in any of the experiments. Since there was a difference between the efficiency of Standard Preview trials in Experiment 5, depending on their proportion amongst the easier Salient trials, we can conclude that this result is due to some form of top-down regulation. Experiment 6 provides confirmation that such an effect is not observed when all items appeared simultaneously.

With regard to effect of old item location changes, in Experiments 1 and 2, Standard Preview search differed significantly from the jump preview conditions (approximately 9 ms/item in the Standard preview conditions compared with approximately 20 ms/item in the Jump conditions). Thus, a change of location in the preview items disrupted the preview benefit. Moreover, if we assume that only the new items are selected in Standard Preview trials, then an overall doubling of search slopes on Jump preview trials suggests that the old item location changes totally abolished the preview benefit. This finding confirms Watson and Humphreys' (1997) proposal that the preview benefit is based (at least partially) on the inhibition of the locations of old distractors (also shown in probe-dot procedures, e.g. Olivers & Humphreys, 2002; Osugi et al., 2009; Watson & Humphreys, 2000). It follows that old item location changes should disrupt the inhibition applied to those items, unless perhaps the configuration of the old items remains constant and the change in location is relatively modest (e.g., 1 degree of visual angle; Kunar et al., 2003).

Implications

The default recruitment of high-level cognitive processes, without the evaluation of their necessity, may have corollaries in terms of adding extra load and compromising the efficiency of working memory. It may also predict and account for potential failures of attention. As noted earlier, recent work has shown that, in addition to location-based inhibition, the features of old items (e.g., their color), can also be inhibited via visual marking

processes, and even with stationary old items (Andrews, et al., 2011; Braithwaite et al., 2003, 2004; Olivers & Humphreys, 2003). One of the consequences of such feature-based inhibition is that any new items that share the inhibited feature become much harder to detect (or are more easily missed) than items that do not possess the feature (Braithwaite & Humphreys, 2003, 2007). This feature-based inhibition has been linked to inattentional blindness phenomena (Most et al., 2001; Most, Scholl, Clifford, & Simons, 2005, see also Andrews et al., 2011), in which otherwise salient items can be missed by the visual system in certain circumstances. In terms of the present work, if individuals are prone to applying inhibition, then this could lead to amplified inattentional blindness, with potentially, the associated serious consequences for failing to notice new information that has common features with the old (e.g., the appearance of hazards whilst driving). These issues would be worth exploring further in relation to understanding and engineering safe, efficient behavior in the real world.

In sum, the findings from the present experiments show that observers generally do not evaluate the effectiveness of applying an inhibitory template in time-based visual selection, as long as it is a function of current behavioral goals. Search tasks and contexts in which the target is often salient can produce a shift to a more strategic application of prioritization processes; however, even then the shift is relatively modest. These results carry theoretical and methodological implications of how attentional mechanisms function and highlight the nature of top-down control, as well as potential challenges to our attentional system in complex, dynamic visual environments.

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Table 1. Search slope statistics for Experiment 1.

	80% Jump Block		80% Standard Preview Block	
	Jump Trials	Standard Trials	Jump Trials	Standard Trials
Slope (ms/item)	23.99	11.40	27.48	4.49
Intercept	498.38	515.78	473.74	575.14
R ²	0.97	0.77	0.99	0.99

Table 2. Mean percentage error rates for Experiment 1.

	Display Size		
	4	8	16
80% Jump Block			
Jump trials	1.22	0.87	3.29
Standard trials	1.39	2.78	1.39
80% Standard Block			
Jump trials	1.39	1.39	2.08
Standard trials	1.39	1.22	1.04

Table 3. Search slope statistics for Experiment 2.

	80% Jump Block		80% Standard Preview Block	
	Jump Trials	Standard Trials	Jump Trials	Standard Trials
Slope (ms/item)	15.48	7.41	16.79	13.07
Intercept	548.21	614.62	564.11	519.16
R ²	0.97	0.91	0.99	0.99

Table 4. Mean percentage error rates for Experiment 2.

	Display Size		
	4	8	16
80% Jump Block			
Jump trials	0.35	1.39	2.78
Standard trials	0.69	1.39	1.39
80% Standard Block			
Jump trials	2.08	0.69	1.39
Standard trials	1.74	1.04	2.25

Table 5. Search slope statistics for Experiment 3

	80% Jump Block		80% Standard Preview Block	
	Jump Trials	Standard Trials	Jump Trials	Standard Trials
Slope (ms/item)	19.88	12.33	24.65	12.06
Intercept	530.13	590.77	486.31	505.72
R ²	0.99	0.96	0.99	0.99

Table 6. Mean percentage error rates for Experiment 3.

	Display Size		
	4	8	16
80% Jump Block			
Jump trials	1.22	0.52	1.91
Standard trials	0.69	0	1.39
80% Standard Block			
Jump trials	0.69	2.08	2.78
Standard trials	1.91	1.56	1.91

Table 7. Search slope statistics for Experiments 2 and 3 combined.

	80% Jump Block		80% Standard Preview Block	
	Jump Trials	Standard Trials	Jump Trials	Standard Trials
Slope (ms/item)	17.68	9.87	17.61	12.57
Intercept	539.17	602.7	542.66	512.44
R ²	0.98	0.99	0.99	0.99

Table 8. Mean percentage error rates for Experiments 2 and 3 combined.

	Display Size		
	4	8	16
80% Jump Block			
Jump trials	0.78	0.95	2.34
Standard trials	0.69	0.69	1.39
80% Standard Block			
Jump trials	1.39	1.39	2.08
Standard trials	1.82	1.30	2.08

Table 9. Search slope statistics for Experiment 4

	Valid trials (80%)		Invalid trials (20%)	
	Jump Trials	Standard Trials	Jump Trials	Standard Trials
Slope (ms/item)	18.99	7.13	22.11	7.61
Intercept	504.67	529.68	487.56	510.24
R ²	0.99	0.98	0.99	0.97

Table 10. Mean percentage error rates for Experiment 4.

	Display Size		
	4	8	16
Valid trials			
Jump trials	0.69	0.87	1.91
Standard trials	0.52	0.17	0.52
Invalid trials			
Jump trials	1.39	1.39	3.47
Standard trials	0.69	0.00	0.69

Table 11. Search slope statistics for Experiment 5.

	80% Salient Block		80% Standard Preview Block	
	Salient Trials	Standard Trials	Salient Trials	Standard Trials
Slope (ms/item)	-1.17	13.57	0.32	9.49
Intercept	437.34	445.29	444.67	439.18
R ²	0.48	0.99	0.01	0.99

Table 12. Mean percentage error rates for Experiment 5

Condition	Salient trials		
	Display Size		
	3	5	9
80% Salient block	0.69	0.69	1.74
80% Standard block	0.00	0.00	2.08
Condition	Standard Preview trials		
	Display Size		
	4	8	16
80% Salient block	1.39	5.56	3.47
80% Standard block	1.56	0.87	1.91

Table 13. Search slope statistics for Experiment 6.

	80% Salient FEB Block		80% Standard FEB Block	
	Salient Trials	Standard Trials	Salient Trials	Standard Trials
Slope (ms/item)	4.30	27.86	1.99	24.52
Intercept	465.58	446.59	496.22	448.93
R ²	0.99	0.99	0.31	1

Table 14. Mean percentage error rates for Experiment 6

Salient FEB trials			
Condition	Display Size		
	3	5	9
80% Salient FEB block	0.35	0.52	0.52
80% Standard FEB block	1.39	0.69	2.78

Standard FEB trials			
Condition	Display Size		
	4	8	16
80% Salient FEB block	1.39	2.78	4.17
80% Standard FEB block	1.22	1.39	1.56

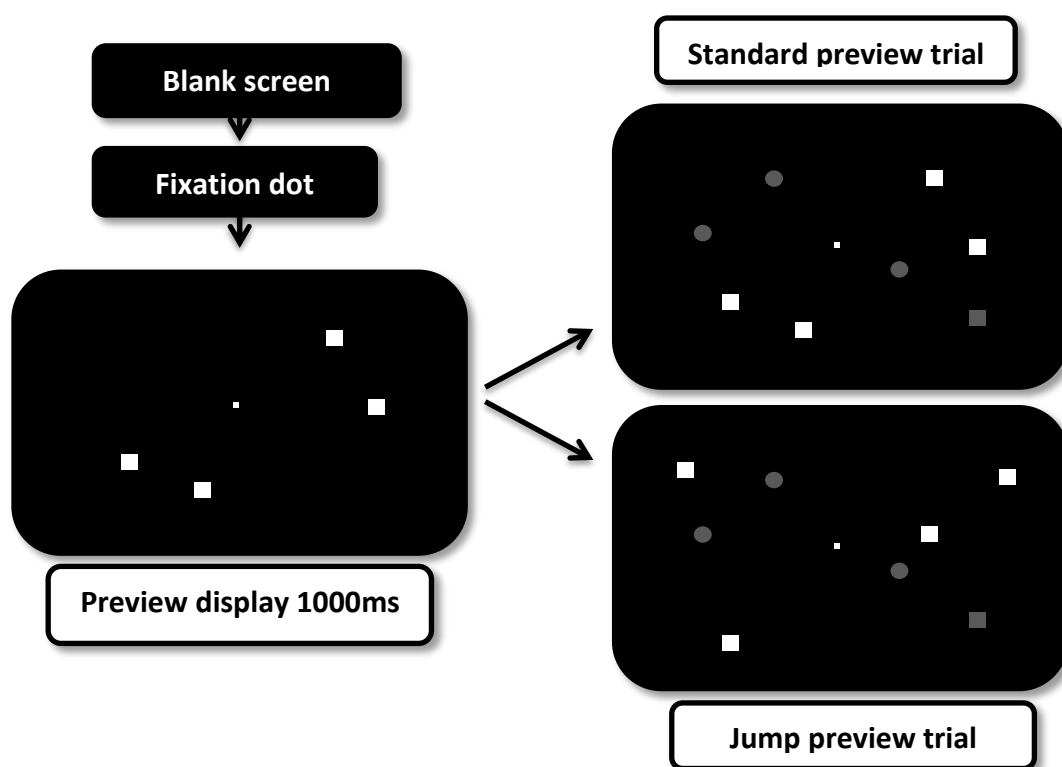


Figure 1. Example trial sequence from Experiment 1, where white represents pink and grey represents blue (not drawn to scale). The task was to indicate the location (left/right of center) of the blue square which appeared amongst the second set of items. On a standard preview trial, the new items were added to the preview items. On a Jump trial, the old items jumped to new locations when the new items were added. The mostly standard block consisted of 80% Standard Preview trials and 20% Jump trials. This ratio was reversed for the 80% Jump block.

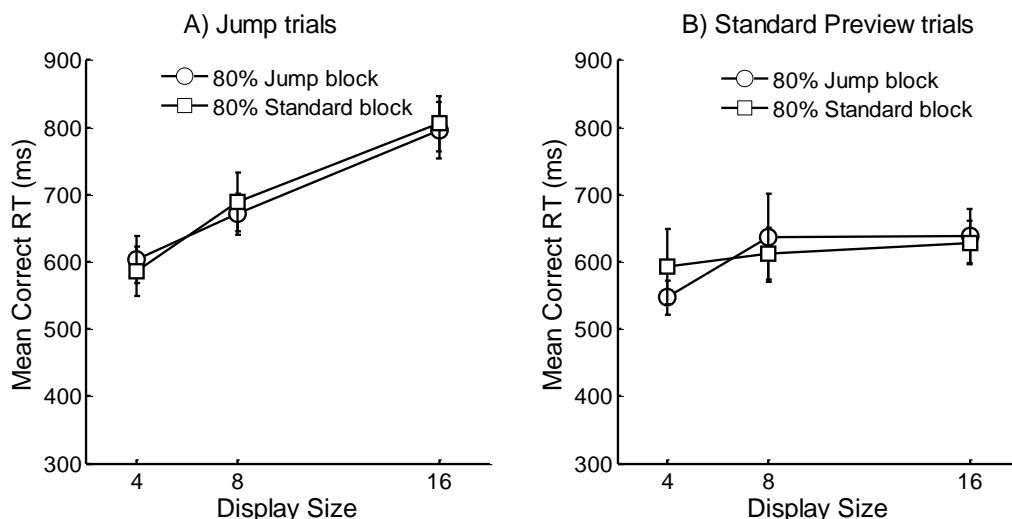


Figure 2. Mean correct RTs for Jump trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 1. Error bars represent standard errors and function in an arelational role (Rouder & Morey, 2005). If inhibition was being applied strategically, we would expect more efficient search for Standard Preview trials in the 80% Standard Preview block than in the 80% Jump block.

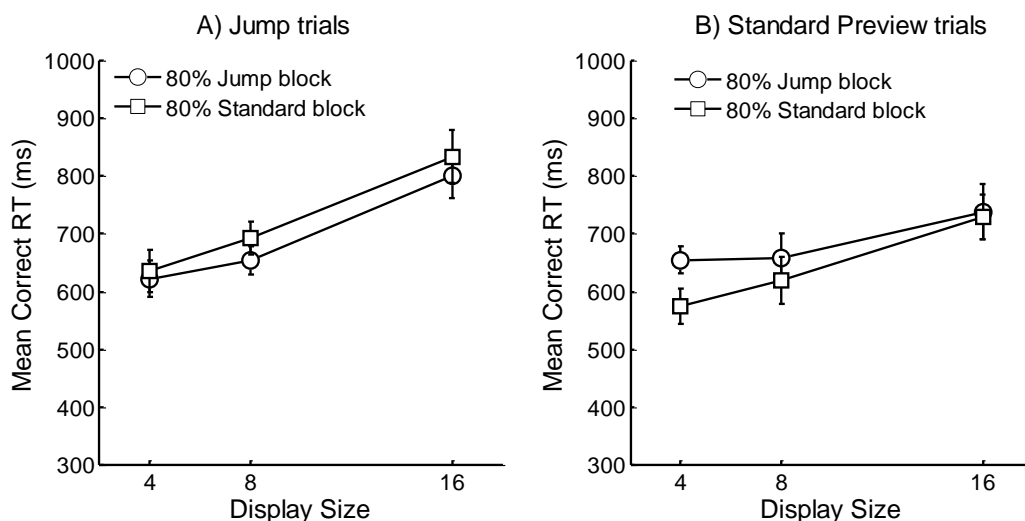


Figure 3. Mean correct RTs for Jump trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 2. Error bars represent standard errors. If inhibition was being applied strategically, we would expect more efficient search for Standard Preview trials in the 80% Standard block than in the 80% Jump block.

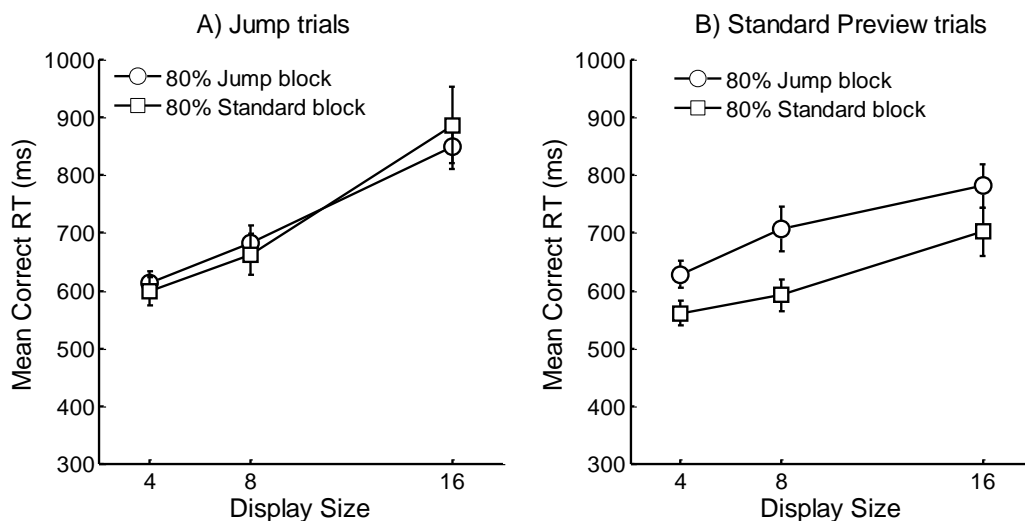


Figure 4. Mean correct RTs for Jump trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 3. Error bars represent standard errors. If inhibition was being applied strategically, we would expect more efficient search for Standard Preview trials in the 80% Standard block than in the 80% Jump block.

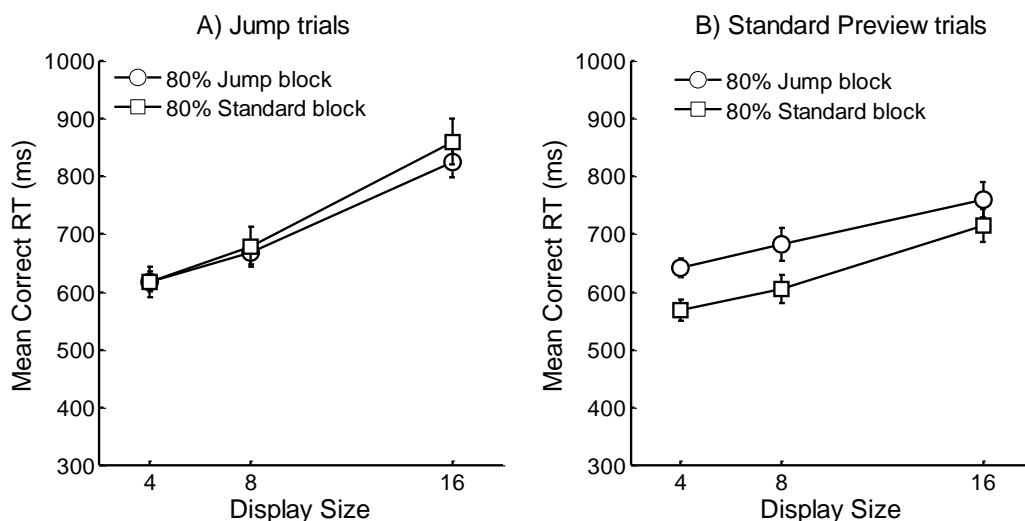


Figure 5. Mean correct RTs for Jump trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 2 & 3 combined. Error bars represent standard errors. If inhibition was being applied strategically, we would expect more efficient search for Standard Preview trials in the 80% Standard Preview block than in the 80% Jump block.

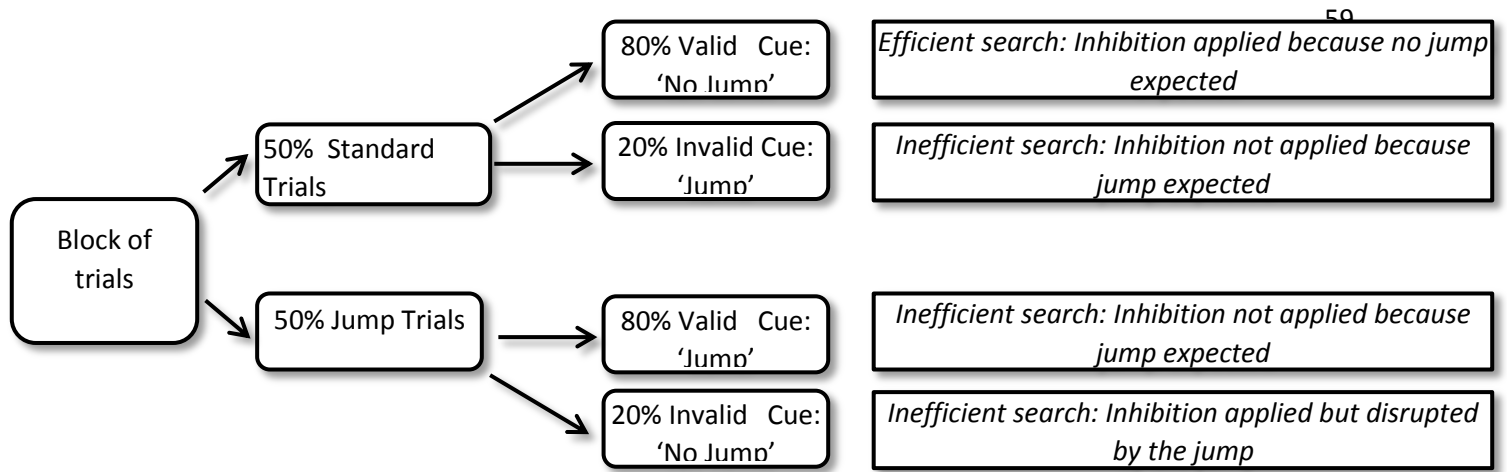


Figure 6. Summary of the design of Experiment 4 (rounded boxes) and search efficiency predictions if participants apply inhibition strategically (square boxes).

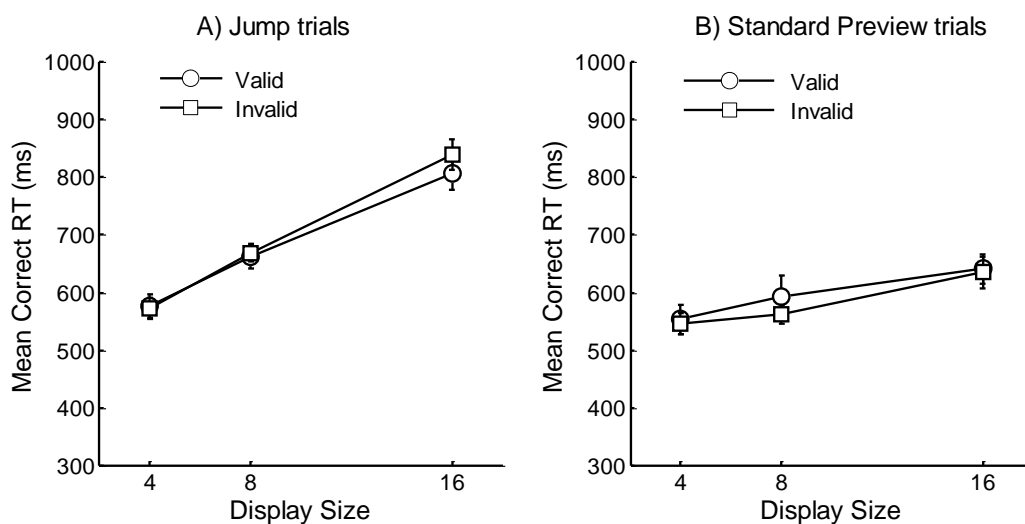


Figure 7. Mean correct RTs for Jump trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 4. If inhibition was being applied strategically, we would expect that for Standard Preview trials, search would be more efficient on validly cued (cued 'No Jump') trials than on invalidly cued trials (cued 'Jump').

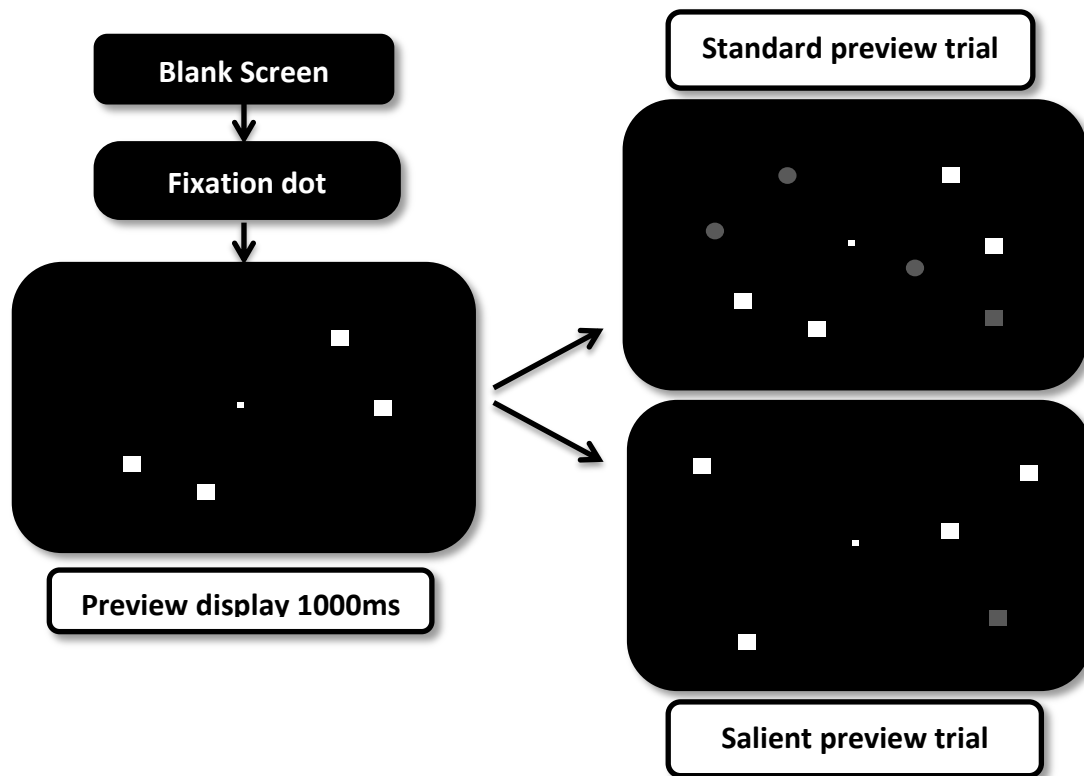


Figure 8. Example trial sequence from Experiment 4 where white represents pink and grey represents blue (not drawn to scale). The task was to indicate the location (left/right of center) of the blue square which appeared amongst the second set of items. On a *Standard Preview* trial, the new items were added to the preview items. On a *Salient Preview* trial, only the single blue square was added. The mostly standard block consisted of 80% Standard Preview trials and 20% Salient trials. This ratio was reversed for the mostly salient block.

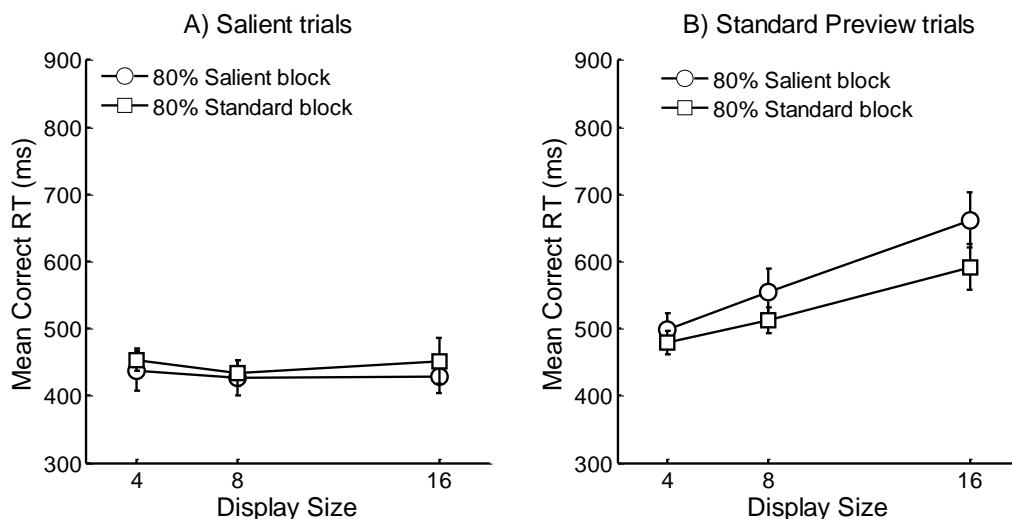


Figure 9. Mean correct RTs for Salient trials (Panel A) and Standard Preview trials (Panel B) as a function of Block and Display Size for Experiment 5. Error bars represent standard errors. If inhibition was being applied strategically we would expect more efficient search for Standard Preview trials in the 80% Standard Preview block than in the 80% Salient block.

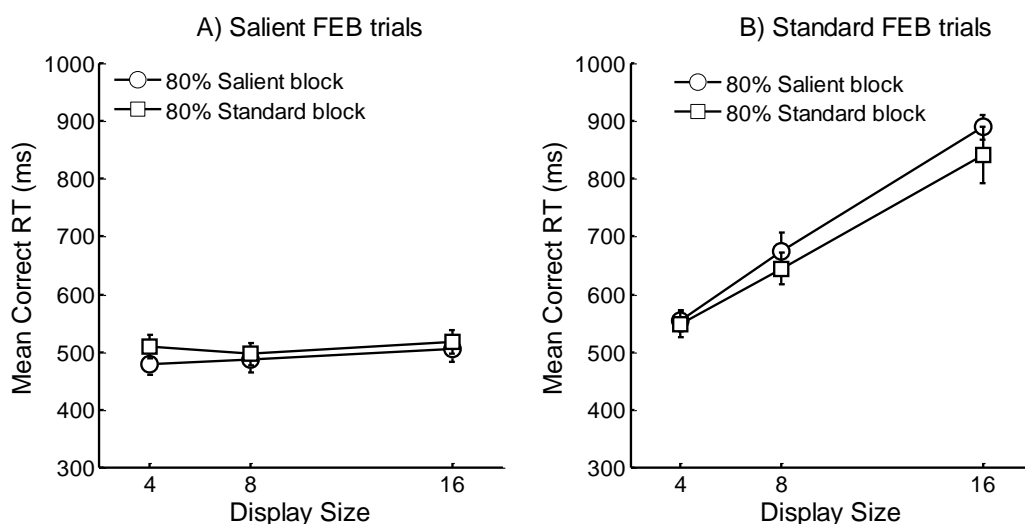


Figure 10. Mean correct RTs for Salient FEB trials (Panel A) and Standard FEB trials (Panel B) as a function of Block and Display Size for Experiment 6. Error bars represent standard errors.