



The cost of divided attention for detection of simple visual features primarily reflects limits in post-perceptual processing

Amelia H. Harrison^{1,2} · Sam Ling^{2,3} · Joshua J. Foster^{2,3}

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Abstract

Covert spatial attention allows us to prioritize processing at relevant locations. Perception is generally poorer when attention is distributed across multiple locations than when attention is focused on a single location. However, while divided attention typically impairs performance, recent work suggests that divided attention does not seem to impair detection of simple visual features. Here, we re-examined this possibility. In two experiments, observers detected a simple target (a vertical Gabor), and we manipulated whether attention was focused at one location (focal-cue condition) or distributed across two locations (distributed-cue condition). In Experiment 1, targets could appear independently at each location, such that observers needed to judge target presence for each location separately in the distributed-cue condition. Under these conditions, we found a robust cost of dividing attention. Next, we further probed what stage of processing gave rise to this cost. In Experiment 1, the cost of dividing attention could reflect a limit in the ability to make concurrent judgments about target presence. In Experiment 2, we simplified the task to test whether this was the case: just one target could appear on each trial, such that observers made a single judgment (“was a target present?”) in both the focal-cue and distributed-cue conditions. Here, we found a marginal cost of dividing attention that was weaker than the cost in Experiment 1. Together, our results suggest that divided attention does impair detection of simple visual features, but that this cost is primarily due to a limit in post-perceptual processes.

Keywords Attention · Divided attention · Detection · Capacity limits

Introduction

Our capacity to process sensory information is limited. Thus, we must prioritize the processing of relevant information to the detriment of other information. Covert spatial attention plays a central role in this effort, allowing us to prioritize processing at relevant locations without moving our eyes. Decades of research has shown that covert attention improves the quality and speed of perceptual processing (for review, see Carrasco, 2011). However, the benefit of covert attention is diminished when attention is divided between multiple stimuli. Studies of divided attention have generally found that

performance is worse when covert attention is spread over multiple locations in the visual field, compared to when attention is focused at a single, cued location (Itthipuripat et al., 2014; McMains & Somers, 2005; Pestilli et al., 2011; Popovkina et al., 2021; White et al., 2018, 2019).

Although many studies have reported robust costs of dividing attention on perceptual performance, recent evidence suggests that there may be an exception to this rule: dividing attention may not impair *detection of simple visual features* (Moreland et al., 2020; Scharff et al., 2011; White et al., 2017, 2018). White et al. (2017) noted that studies that have reported robust costs of dividing attention have used tasks that involve higher-order demands, either requiring observers to identify conjunctions of features such as letters, words, or complex objects (e.g., McMains & Somers, 2005; Popovkina et al., 2021; White et al., 2018; White et al., 2019), which are represented in higher visual areas, where attention is known to have a substantial impact on neural responses (for review, see Maunsell, 2015), or placing demands on working memory because successive stimuli must be compared across short delays (e.g., Itthipuripat et al., 2014; Pestilli et al., 2011). White et al. (2017) reasoned that divided attention may not

✉ Joshua J. Foster
jjfoster@bu.edu

¹ Department of Psychological and Brain Sciences, University of California, Santa Barbara, CA, USA

² Department of Psychological and Brain Sciences, Boston University, 677 Beacon Street, Boston, MA 02215, USA

³ Center for Systems Neuroscience, Boston University, Boston, MA, USA

impair detection of simple features that are encoded in primary visual cortex (V1), such as orientation, because attention effects are small or absent in V1 (Chen & Seidemann, 2012; Moran & Desimone, 1985; Yoshor et al., 2007). Indeed, White et al. (2017) found that detection of orientation targets was not impaired when attention was divided compared to when attention is focused on a single stimulus, a finding that has been replicated by several other studies (Moreland et al., 2020; Scharff et al., 2011; White et al., 2018; but see Chen & Seidemann, 2012). Thus, while divided attention impairs perception in tasks that require processing of feature conjunctions or place demands on working memory, emerging evidence suggests that divided attention does not impair detection for simple visual features.

There are two reasons that it is surprising that divided attention appears not to impair detection of simple visual features. First, although there is evidence that attentional modulation of neural responses in V1 is small or absent (Chen & Seidemann, 2012; White et al., 2017; Yoshor et al., 2007), other work has contested the view that attention effects in V1 are negligible (e.g., Briggs et al., 2013; Hembrook-Short et al., 2017, 2019). Second, even if divided attention does not attenuate sensory representations in V1, divided attention might nevertheless impair detection of simple features via modulation of representations in higher visual areas, or due to limits in post-perceptual stages of processes (e.g., decision making). Thus, even if dividing attention does not degrade sensory representations in early visual cortex, it might still lead to poorer detection of simple visual features due to limits in later stages of processing.

The finding that divided attention does not impair detection of simple features (Moreland et al., 2020; Scharff et al., 2011; White et al., 2017, 2018) warrants further investigation because it challenges the widely embraced notion that dividing attention impairs perceptual performance in general. In this study, we re-examined whether detection of simple features

is impaired when attention is divided across multiple locations. In two experiments, we asked observers to detect vertical target gratings, overlaid over a horizontal pedestal (Fig. 1). We modeled this detection task after two recent studies of divided attention (Chen & Seidemann, 2012; White et al., 2017). Across blocks of the task, we manipulated whether observers focused on one of two locations (*focal-cue* condition) or distributed across both locations (*distributed-cue* condition).

In Experiment 1, targets could appear independently at each location (with 50% probability), as was the case in recent studies that have found no effect of dividing attention on detection of simple features (Moreland et al., 2020; White et al., 2017, 2018). Therefore, in the distributed-cue condition, observers needed to judge whether a target was present at each location separately, and were then promoted to report whether a target was present at the post-cued location. In the focal-cue condition, the post-cued location matched pre-cued location with 89% validity, and mismatched on a small fraction (11%) of invalidly cued trials. Invalidly cued trials allowed us to verify that observers used the pre-cue to direct attention (manifesting as better performance for validly cued trials than for invalidly cued trials) in case we found no difference in performance between the focal-cue and distributed-cue conditions. To preview our results, we found a robust cost of dividing attention.

In Experiment 2, we further probed what stage of processing gave rise to the cost of divided attention seen in Experiment 1. Because targets occurred independently at each location in Experiment 1, the cost of divided attention in that experiment could reflect a limit in subjects' ability to make concurrent judgments about target presence. In Experiment 2, we simplified the task to test this hypothesis. In this experiment, just one target could appear on each trial (also with 50% probability), such that observers made a single judgment ("was a target present?") in both the focal-cue and

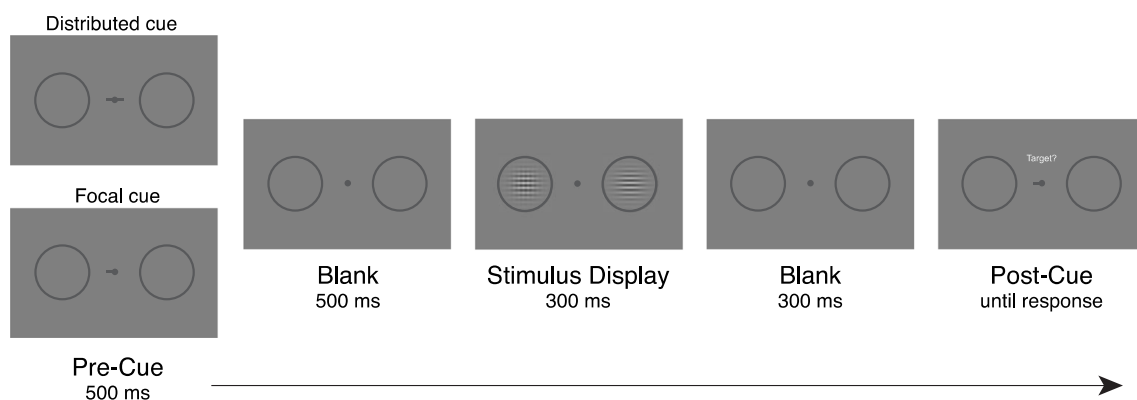


Fig. 1 Example trial sequence. Observers were asked to report whether a vertical target (present in the left aperture in this example) appeared on the side indicated by a post-cue. In the *distributed-cue* condition, the pre-cue was spatially uninformative. In the *focal-cue* condition, one side was cued in advance. In Experiment 1, targets could appear in each aperture

independently. In Experiment 2, only one target could appear on any given trial. Cue validity in the focal cue-condition was ~90% in Experiment 1 and 100% in Experiment 2. The sizes of the cue and fixation point are exaggerated here for clarity

distributed-cue conditions. The target (when present) always appeared at the cued location in the focal-cue condition or could appear at either location in the distributed-cue condition. Thus, observers made the exact same judgment in both conditions, with the only difference being whether the target location was known in advance of the stimulus display. If the cost of dividing attention seen in Experiment 1 reflects a limit in the subjects' ability to make concurrent judgments about target presence, we would expect no cost of dividing attention in Experiment 2. Indeed, we found only a marginal cost of dividing attention that was significantly smaller than the cost seen in Experiment 1, indicating that the cost seen in our first experiment primarily reflects a limit in post-perceptual decision making.

Methods

Participants

Participants were recruited from the Boston University community. All participants were at least 18 years old, reported normal or corrected-to-normal visual acuity, and provided informed consent according to procedures approved by the Boston University Institutional Review Board. Because the experiment was run during the COVID-19 pandemic, participants were screened for COVID-19 risk factors prior to enrollment in the study, and we excluded individuals with health conditions that increased their risk of developing a severe case of COVID-19 in accordance with Boston University guidelines at the time of data collection. Participants were screened for COVID-19 symptoms the day before testing and upon arrival at the lab. Participants were compensated \$12/h for participating in the study.

Experiment 1

For Experiment 1, our target sample size was ten participants, following White et al. (2018). Fourteen volunteers participated in the study (six males and eight females, mean age = 20.1 years, SD = 1.2). Three participants were excluded from the final sample for the following reasons: one participant withdrew from the study; we did not bring one participant back for the second session of the study because we were unable to obtain adequate eye-tracking data for that participant; and we did not bring another participant back for the second session of the study because they broke fixation on more than 25% of trials in the first session. The final sample was 11 participants (three males and eight females, mean age = 20.0 years, SD = 1.3). We exceeded our target sample size of ten participants by one because the final participant was scheduled before reaching our target sample size.

Experiment 2

For Experiment 2, our target sample size was 12 participants. This sample size was determined based on the effect size (Cohen's d_z , see *Statistical analysis*) found for the cost of divided attention in Experiment 1 of approximately 1.5. Anticipating that the effect may be smaller in Experiment 2, we carried out a power analysis, performed using G*Power 3.1 (Faul et al., 2009), which revealed that approximately 11 participants would be needed to detect an effect size of 1.0 with 90% power. Thus, we set a target sample of 12 participants so that we could counterbalance block order across participants (see pre-registration: osf.io/6np4v/). Sixteen volunteers participated in the study (two males and 14 females, mean age = 21.4 years, SD = 1.8). Four participants were excluded from the sample for the following reasons: one participant withdrew from the study; we did not bring one participant back for the second session of the study because we were unable to obtain adequate eye-tracking data for that participant; we did not bring another participant back for the second session of the study because they broke fixation on more than 25% of trials in the first session; and one participant was excluded because task performance after staircasing was at ceiling (accuracy of 98.7%). The final sample was 12 participants (two males and ten females, mean age = 21.3 years, SD = 1.8).

Apparatus and stimuli

Stimuli were generated using MATLAB (The MathWorks, Natick, MA, USA) and the Psychophysics Toolbox (Brainard, 1997; Pelli, 1997) on a PC running Ubuntu Version 16.04 LTS. Participants viewed stimuli on a gamma-corrected LCD monitor (Display++, Cambridge Research Systems Limited; 100 Hz refresh rate, 1,440 × 1,080 resolution). The full range of luminance was resolved to 14 bits (i.e., 16,384 luminance levels), which enabled precise control of the contrast of the low-contrast target Gabor patches. Participants were seated comfortably with their chin on a padded chin rest at a viewing distance of 135 cm. Stimuli were presented against a mid-gray background (108 cd/m²). There were no additional light sources in the room. Participants made their responses on a standard keyboard. Participants wore a face mask during the study in accordance with Boston University's COVID-19 research guidelines.

Task procedures

Experiment 1

Figure 1 shows a schematic of the detection task that participants performed. Participants fixated a dark gray dot (0.16° in diameter, 88 cd/m²) at the center of the screen and began a trial by pressing the spacebar. At the start of each trial, circular apertures appeared to the left and right of fixation (6.0° in

diameter, 0.2° thick, centered 5.66° horizontally from fixation, same luminance as the fixation dot), which marked the locations at which each stimulus would appear. These apertures remained on-screen for the duration of the trial. After a jittered interval between 300 and 500 ms, a pre-cue was presented for 500 ms. For the distributed-cue condition, this was composed of two small horizontal bars (0.18° long, 0.12° thick) that extended from fixation, pointing to either side of the screen. Thus, the pre-cue provided no information about which side the participants would be tested on in the distributed-cue condition. In the focal-cue condition, just one bar appeared, either pointing to the left or right side of the screen; this bar indicated the likely side that the participant would be tested on. The pre-cue was valid on 89% of trials. For the 500 ms after the pre-cue, only the fixation dot and apertures remained on-screen. Next, the stimulus display was presented for 300 ms. In this display, a horizontally oriented Gabor patch (two cycles per degree, Gaussian envelope with standard deviation of 1° , 20% Michelson contrast) was presented in each of the apertures. These horizontal Gabor patches served as pedestals for potential targets, which were vertically oriented Gabor patches. Note that because targets were always vertically oriented, feature-based attention did not change between the focal-cue and distributed-cue conditions. Thus, any difference in performance between conditions can only be due to differences in the spatial spread of covert attention. On each trial, there was a 50% chance that a target was present on each side, determined independently. Only the fixation dot and apertures remained on-screen for 300 ms after the stimulus display before a response screen was presented. Finally, the participant reported whether or not a target was present on the side indicated by a post-cue. On invalidly cued trials, the post-cue was black rather than gray to make it obvious to the participant that they were being tested on the uncued side because these trials were uncommon. Participants responded using the number pad of the keyboard (1 = target present, 2 = target absent). The participant's response was displayed above the fixation dot ("yes" or "no" for target present or target absent, respectively), and the participant could correct their response if they pressed the wrong key. There was no time limit for inputting a response. The participant confirmed their response by pressing the spacebar, and feedback was then presented for 300 ms: If the response was correct, the fixation dot flashed white; if the response was incorrect, the fixation dot flashed black and the word "Incorrect" was displayed above the fixation dot in black text. We instructed participants to maintain fixation and to avoid blinking for the duration of each trial (from the time the pre-cue was triggered to appear until the time the response screen was displayed). Participants were free to blink or move their eyes in between trials. Trials during which blinks or breaks from fixation were detected with an eye tracker were aborted, and responses were not collected on these trials (see *Eye tracking*).

Following White and colleagues (White et al., 2017, 2018), we manipulated the pre-cue condition (focal cue vs. distributed cue) across blocks. The conditions were alternated across blocks, with the order counterbalanced across participants. Each block of the distributed-cue condition included 64 trials, with the left and right stimuli probed equally often, such that participants needed to divide attention between the two stimuli. Each block of the focal-cue condition included 72 trials (64 validly cued trials and eight invalidly cued trials, such that cue validity was approximately 89%). Participants completed two sessions, each comprising 12 blocks, six focal-cue blocks and six distributed-cue blocks. The left side was cued in half of the focal-cued blocks, and the right side was cued in the other half (cf. Moreland et al., 2020; White et al., 2017, 2018). In the first session, which took approximately 2 h, participants were familiarized with the task, and we adjusted task difficulty by staircasing the contrast of the vertical Gabor targets (see *Staircase procedure*). In the second session, which took approximately 1.5 h, the contrast of the targets on each side of the display were set based on performance in the first session.

Experiment 2

Experiment 2 was identical to Experiment 1 except for three changes. First, and most substantially, just one target could appear on each trial (with 50% probability), and participants judged whether a target was present or absent, rather than making that same judgment for each location separately. Second, for efficiency, we did not include invalidly cued trials in focal-cued blocks, which comprised 64 validly cued trials. Thus, the target always appeared on the cued side in the focal-cued condition (i.e., the pre-cue was 100% valid). Third, we increased the sample size based on a power analysis performed on the basis of Experiment 1 (see *Participants*, Experiment 2).

Staircase procedure

In the first session of each experiment, we used a staircasing procedure to adjust the contrast of the vertical targets. For both experiments, the staircase session was identical to the main session of the task, with the only difference being that target contrast was adjusted trial-to-trial based on performance. We adjusted target contrast independently for the left and right apertures, for both the focal-cue and distributed-cue conditions. Thus, we ran four independent staircases. In Experiment 1, we included invalidly cued trials in the focal-cue condition, but these trials did not contribute to the staircase. We used a weighted up/down procedure to adjust target contrast: after a correct response, target contrast decreased by 5%; after an incorrect response, target contrast increased by 17.6%. This procedure held accuracy fixed at approximately 76% (see *Results, Contrast thresholds*). We set the contrast of the targets in the second session (for both conditions) on the

basis of the performance in the focal-cued conditions in the staircase session. We included the distributed-cue conditions in the staircase session solely to ensure that participants had equal practice on both conditions prior to the second session of the experiment. In Experiment 1, we set the contrast of the targets for each side of the display to mean target contrast across the last 80 trials of the staircase. For some participants in Experiment 1, we averaged across more trials to determine the contrast values if the staircase had clearly reached an asymptote sooner. In Experiment 2, to remove any subjectivity when determining contrast thresholds, we set the contrast of the targets for each side of the display to the target contrast after 15 reversals in the staircase. The number of trials averaged using this approach ($M = 91$, $SD = 10$) was comparable to Experiment 1. The target contrasts set on the basis of staircasing performance were 1.21% ($SD = 0.28$) for the left aperture and 1.16% ($SD = 0.22$) for the right aperture in Experiment 1, and 1.09% ($SD = 0.30$) for the left aperture and 1.14% ($SD = 0.26$) for the right aperture in Experiment 2. In both experiments, accuracy was higher than 76% in the second session, indicating that subjects continued to improve with experience. Importantly, performance was well off ceiling, confirming that the task was still challenging.

Eye tracking

We monitored gaze position at 1,000 Hz with a desk-mounted infrared eye tracker (Eyelink 1000 Plus, SR Research), which provides spatial resolution of 0.01° of visual angle, and average accuracy of $0.25\text{--}0.50^\circ$ of visual angle. In both experiments, trials were aborted if participants broke fixation or blinked to ensure that participants maintained steady fixation. We calibrated the eye tracker throughout the session as needed. We operationalized a break in fixation as the participant's gaze exceeding a specified distance from the fixation dot. This threshold was initially set to 1° from fixation. A threshold of 1° was too stringent for some participants, for whom noise in the eye-tracking data resulted in too many false alarms. We increased the threshold for these participants. The threshold was never larger than 1.5° . If the participant broke fixation during a trial, the text "Eye movement" appeared above the fixation point, and a black line extended from the fixation point to the recorded gaze position to indicate where the participant had moved their eyes. If the participant blinked during a trial, the word 'Blink' was immediately displayed above the fixation point. The participant was free to initiate the next trial after this feedback. We excluded participants for whom more than 25% of trials were aborted due to blinks or eye movements (see *Participants*).

Statistical analysis

For each observer, we calculated sensitivity to vertical targets, $d' = z(\text{hit rate}) - z(\text{false alarm rate})$ for each condition separately on Day 2 (when target contrast was fixed). We used an alpha level of .05 for statistical tests. Statistical results for directional hypotheses are noted as one tailed and non-directional hypotheses are noted as two tailed. We calculated effect sizes for paired-samples t-tests as Cohen's d_z :

$$d_z = \frac{t}{\sqrt{n}}$$

where t is the t-statistic and n is the sample size, and for independent-samples t-tests as Cohen's d_s :

$$d_s = t \sqrt{\frac{1}{n_1} + \frac{1}{n_2}}$$

where t is the t-statistic, and n_1 and n_2 are the number of observations in each sample (see Lakens, 2013). We used the BayesFactor Package (version 0.9.12; Morey & Rouder, 2018) for R to calculate Jeffreys-Zellner-Siow (JZS) Bayes factor from t-statistics ($r\text{-scale} = 0.7071$).

Results

Detection performance

Experiment 1

In Experiment 1, we tested whether divided attention impairs detection of vertical targets (Fig. 1). Figure 2a shows sensitivity (d') separately for the valid focal-cue, distributed-cue, and invalid focal-cue conditions. Sensitivity was significantly higher in the focal-cue condition when the cue was valid ($M = 2.12$, $SD = 0.67$) than when it was invalid ($M = 0.73$, $SD = 0.61$), $t(10) = 6.75$, $p < .001$, Cohen's $d_z = 2.03$, $BF_{10} = 1033.58$ (one-tailed test), confirming that observers took advantage of the pre-cue to orient attention to the cued location. Next, we tested whether dividing attention impairs target detection by comparing sensitivity for the valid focal-cue and distributed-cue conditions. We found that sensitivity was higher in the focal-cue condition ($M = 2.12$, $SD = 0.67$) than in the distributed-cue condition ($M = 1.59$, $SD = 0.53$), $t(10) = 4.88$, $p < .001$, Cohen's $d_z = 1.47$, $BF_{10} = 117.90$ (one-tailed test). The Bayes factor indicated very strong evidence for a cost of dividing attention. Thus, observers were poorer at detecting vertical targets when covert attention was distributed across two locations than when attention was focused at one location.

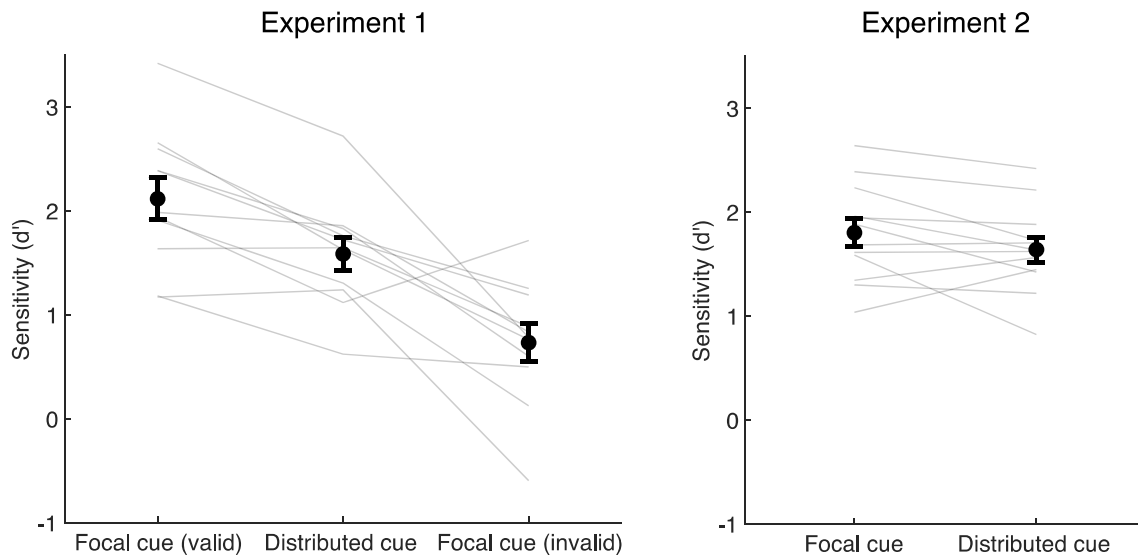


Fig. 2 Target detection sensitivity (d') for each experiment. Black circles and error bars show the mean \pm 1 SEM across participants. Gray lines show data for each participant.

Experiment 2

In Experiment 2, we further probed what stage of processing gave rise to the cost of dividing attention seen in Experiment 1. Observers performed the same detection task as in Experiment 1 with one major difference: only one target could appear on any given trial. Therefore, in the distributed-cue condition, observers judged whether a target was present or absent regardless of location, rather than making that same judgment for each location separately. If the cost of dividing attention seen in Experiment 1 reflects a limit in the ability to make concurrent judgments about target presence, then no cost of dividing attention should be seen in Experiment 2. For efficiency, we did not include invalidly cued trials in this experiment, having already established that observers used the pre-cue in Experiment 1. Figure 2b shows sensitivity in the focal-cue and distributed-cue conditions. As in Experiment 1, sensitivity was higher in the focal-cue condition ($M = 1.80$, $SD = 0.47$) than in the distributed-cue condition ($M = 1.64$, $SD = 0.42$); however, this modest difference was not significant, $t(11) = 1.73$, $p = 0.055$, Cohen's $d_z = 0.50$, $BF_{10} = 1.71$ (one-tailed test). The Bayes factor indicates weak evidence for a cost of dividing attention in this experiment. Figure 3 shows the cost of dividing attention on sensitivity (valid focal-cue – distributed cue) in each experiment. An independent-samples t-test confirmed that the cost of dividing attention was significantly larger in Experiment 1 ($M = 0.53$, $SD = 0.36$) than in Experiment 2 ($M = 0.16$, $SD = 0.32$), $t(21) = 2.58$, $p = .0175$, Cohen's $d_s = 1.08$, $BF_{10} = 3.44$. Thus, although the evidence for a cost of dividing attention was ambiguous in Experiment 2, this cost was smaller in Experiment 2 than in Experiment 1.

Because we did not include invalidly cued trials in Experiment 2, we did not have the opportunity to compare

performance for valid versus invalid trials in the focal-cue condition. As a result, we do not have positive evidence that observers used the spatial pre-cue to selectively attend the cued stimulus in the focal-cue condition like we did in Experiment 1. Therefore, one possibility is that observers did not make use of focal pre-cues in Experiment 2, which could explain why we observed no difference in performance between the focal-cue and distributed-cue conditions. However, this possibility is unlikely. Cue validity was higher in Experiment 2 (100%) than in Experiment 1 (~90%), and past work has established that spatial cueing effects increase with cue validity for central cues (e.g., Giordano et al., 2009; Kinchla, 1980; Riggio & Kirsner, 1997). Thus, increasing cue

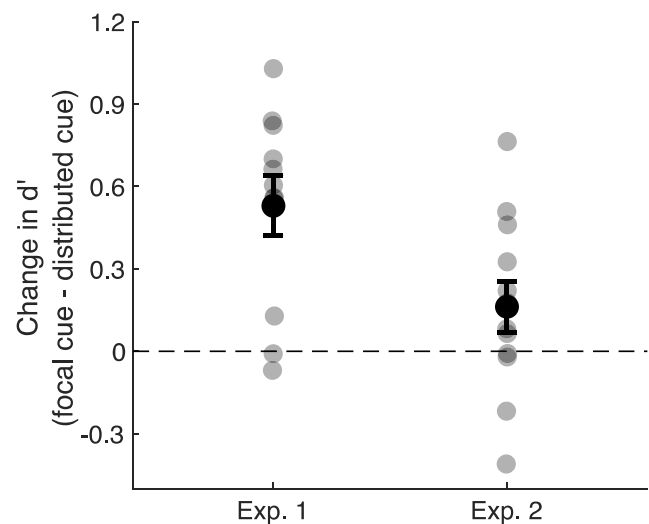


Fig. 3 Cost of dividing attention on sensitivity (d') for each experiment. Black circles and error bars shown the mean \pm 1 SEM across participants. Gray circles show data for each participant

validity in Experiment 2 should have, if anything, increased the size of the cueing effect.

Contrast thresholds

Although our planned analysis focused on sensitivity (d') in the second session of each experiment (when target contrast was fixed), for completeness we report accuracy and contrast thresholds for the staircasing session (calculated for each condition as mean accuracy and target contrast after 15 reversals in the staircase, then averaged across the locations). As expected, accuracy was $\sim 76\%$ for both conditions in Experiment 1 (focal-cue: $M = 76.2\%$, $SD = 2.25$; distributed cue: 77.2% , $SD = 2.32$) and in Experiment 2 (focal-cue: $M = 76.4\%$, $SD = 1.17$; distributed-cue: $M = 76.4\%$, $SD = 1.16$), confirming that the staircase procedure was effective. We found that differences in contrast thresholds mirrored our primary analysis of d' in the second session of the experiments (see *Detection performance*). In Experiment 1, contrast thresholds were lower in the focal-cue condition ($M = 1.23\%$, $SD = 0.25$) than in the distributed-cue condition ($M = 1.58\%$, $SD = 0.68$), $t(10) = 1.89$, $p = 0.044$, Cohen's $d_z = 0.57$, $BF_{10} = 2.12$ (one-tailed test). In Experiment 2, contrast thresholds were again lower in the focal-cue condition ($M = 1.12\%$, $SD = 0.24$) than in the distributed-cue condition ($M = 1.48\%$, $SD = 1.01$), but this difference was not significant, $t(11) = 1.29$, $p = 0.11$, Cohen's $d_z = 0.37$, $BF_{10} = 0.99$ (one-tailed test).

Eye-movement controls

In both experiments, we used a gaze-contingent procedure to abort trials with blinks or eye movements to ensure that observers maintained steady fixation throughout each trial

(see *Methods, Eye tracking*). In Experiment 1, an average of 8.7% ($SD = 3.6$) trials were aborted for participants in the final sample. In Experiment 2, an average of 6.5% ($SD = 3.2$) trials were aborted for participants in the final sample. Despite this stringent procedure, small residual biases in eye position toward the cued location might nevertheless remain if observers consistently made sub-threshold eye movements that were not detected. Figure 4 shows the mean gaze position across time as a function of cued location. The first two participants in Experiment 1 are not included in this analysis because the eye-tracking data were not saved due to a technical error, although the gaze-contingent procedure did operate for these participants. In both experiments, mean horizontal gaze position during the stimulus display varied by less than 0.1° across pre-cued locations. Thus, bias in eye position toward the pre-cued location cannot account for differences in detection performance between conditions focal-cue and distributed-cue conditions.

Discussion

In two experiments, we tested whether dividing covert attention across multiple locations impairs the detection of simple visual features. In Experiment 1, we observed a robust cost of dividing attention on detection performance. In this experiment targets appeared independently at each of two locations. Therefore, in the focal-cue condition, observers could make a single judgment about the presence of a target at the cued location, whereas in the distributed-cue condition, observers needed to judge whether a target was present at each location separately.

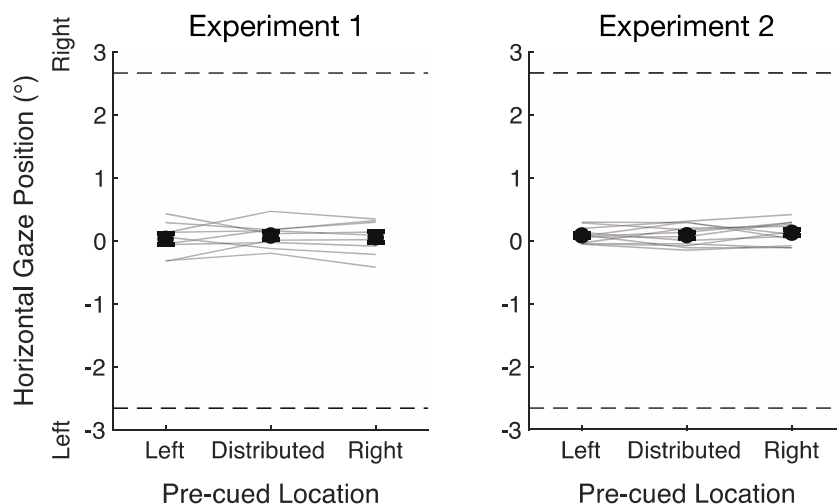


Fig. 4 Eye-movement controls. Each plot shows the average horizontal gaze position relative to the fixation dot during the 300-ms stimulus display as a function of pre-cued location. Black circles and error bars show the mean ± 1 SEM across subjects (the error bars are smaller than the

circle in some cases). Gray lines show data for each participant. Dashed lines at the top and bottom of each plot mark the inner edge of the stimulus apertures at 2.66° of visual angle

As a result, poorer performance in the distributed-cue condition could reflect a limit of subjects' ability to make two concurrent judgments about target presence. In Experiment 2, we simplified the task to test this hypothesis. Here, just one target was present on any given trial, so that observers made a single judgment about target presence in both the focal-cue and distributed-cue condition. If the cost of dividing attention seen in Experiment 1 was due to a limit in decision making, we would expect no cost of dividing attention in Experiment 2. In Experiment 2, the evidence for a cost of dividing attention was equivocal: the Bayes factor suggested that this experiment did not provide compelling evidence for or against a cost of dividing attention. However, the cost of dividing attention in this experiment was significantly smaller than the cost in Experiment 1. Thus, our results suggest that dividing covert attention does impair detection of simple visual features, but that this cost is largely due to a limit in post-perceptual processes because this cost was only robust when dividing attention required subjects to independently judge target presence at each location.

Our results are at odds with recent studies that have found that dividing attention does not impair detection of simple visual features (e.g., Moreland et al., 2020; White et al., 2017, 2018). In these studies, as was the case in our first experiment, targets could appear independently at each of the stimulus locations. For example, White et al. (2017) presented observers with two streams of dynamic noise and asked observers to detect brief target Gabors presented within this noise (White et al.'s Experiment 2). In the focal-cue condition, observers were pre-cued to the relevant stimulus, allowing them to focus exclusively on the cued location. In the distributed-cue condition, observers needed to judge target-present at each location, and report whether a target was present on the side indicated by a post-cue. White et al. (2017) found no cost in detection performance due to divided attention (also see Moreland et al., 2020; White et al., 2018). It is somewhat surprising that these studies found no cost of divided attention. Even if divided attention does not degrade sensory coding of the stimuli, detection performance may be worse in the distributed-attention due to limits in post-perceptual processing because observers must make two independent decisions in the distributed-cue condition, rather than a single decision in the focal-cue condition. Thus, no cost in detection performance suggests that observers can process two stimuli in parallel, at both perceptual and post-perceptual stages of processing. Our results tell a different story. Our first experiment was closely modeled after these recent studies, but unlike these studies, we found a robust cost of dividing attention on detection performance. In our second experiment, we did not find a reliable cost of dividing attention when we

simplified the task demands so that observers did not need to make two independent decisions in the distributed-cue condition. This finding suggests that the robust cost of divided attention in our first experiment reflects a limit in post-perceptual stages of processing related to the number of judgments an observer must make.

It is unclear why we observed a clear cost of divided attention in our first experiment while other similar studies (e.g., White et al., 2017) have not. We think the discrepancy is unlikely to be due to stimulus parameters because we modeled our stimuli closely after those used by White et al.'s (2017) Experiment 1. One factor that may be important to consider is the participants themselves. We recruited volunteers who had little previous experience performing psychophysical task, and it is not entirely clear how our participants compared in this respect with those in previous studies (e.g., White et al., 2017).

Although our results are incompatible with those of White et al. (2017), our results are consistent with other studies. For example, Chen and Seidemann (2012) had monkeys detect a vertical target over a horizontal pedestal, much like the detection task used in our experiments and by White et al. (2017) in their Experiment 1. Chen and Seidemann (2012) found that detection performance was poorer in the distributed-cue condition than in the focal-cue condition. Interestingly, however, Chen and Seidemann also found that divided attention did not attenuate stimulus-evoked responses in V1, which led them to conclude, like us, that the cost in behavioral performance reflected limits in later stages of processing. It must be noted that there are some important differences to consider between Chen and Seidemann's experiment and our own. In their experiment, as in our Experiment 2, only one target could be present on each trial in the distributed-cue condition. Thus, at first glance, Chen and Seidemann found a cost of divided attention under conditions where we did not. However, importantly, in their experiment, the animals reported target presence by making a saccade to the target's location. Therefore, their task involved a location-discrimination component that our Experiment 2 did not. This additional task requirement might explain why Chen and Seidemann found a cost of dividing attention while we did not in Experiment 2. Further work is needed to delineate the precise boundary condition between our experiments and Chen and Seidemann's. Nevertheless, Chen and Seidemann's results are broadly consistent with our conclusion that the cost of divided attention for detection of simple visual features primarily reflects limits in later stages of processing.

Conclusions

Dividing attention impairs perception in a wide range of perceptual tasks. However, recent studies have suggested that dividing covert attention may not impair detection of simple

visual features that are coded in early sensory cortex (Moreland et al., 2020; White et al., 2017, 2018). In this study, we re-visited this possibility. In contrast to these recent studies, we found that divided attention produced a robust cost in detection of simple orientation targets. Interestingly, the cost of divided attention was only significant in our first experiment, where subjects needed to independently judge target presence at each location when attention was divided, but not in our second experiment, where the task was simplified so that subjects always made a single judgment (“was a target present?”), regardless of whether attention was focused or divided. Together, our results show that dividing attention impairs the detection of simple features, but that this cost primarily reflects a limit in post-perceptual decision making, rather than early sensory processing. Although our results provide clear evidence that the cost of dividing attention reflects a limit in post-perceptual processing, further work is needed to pinpoint which stage(s) of post-perceptual processing limits performance when attention is divided.

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Open practices statement All data and analysis code are available on Open Science Framework (see osf.io/p96wn/). We pre-registered the methods and analysis plan for Experiment 2 on the Open Science Framework before we began data collection (see osf.io/6np4v/).

Author contributions All authors designed the study. AHH and JJF performed the experiments, analyzed the data, and drafted the manuscript. All authors revised the manuscript, and approved the final version for submission.

Declarations

Conflicts of interest None

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