

Top-Down Control Over Biased Competition During Covert Spatial Orienting

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Larger benefits of spatial attention are observed when distractor interference is prevalent, supporting the view that spatial selection facilitates visual processing by suppressing distractor interference. The present work shows that cuing effects with identical visual displays can grow substantially as the probability of distractor interference increases. The probability of interference had no impact on spatial cuing effects in the absence of distractors, suggesting that the enlarged cuing effects were not caused by changes in signal enhancement or in the spatial distribution of attention. These findings suggest that attentional control settings determine more than where spatial attention is directed; top-down settings also influence how attention affects visual processing, with increased levels of distractor exclusion when distractor interference is likely.

Spatial attention allows an observer to select specific locations within the visual environment, enabling better processing of attended than unattended stimuli. Current models acknowledge two distinct paths by which locations are selected. In the case of top-down selection, the goals and intentions of the observer determine the selected locations. In the case of stimulus-driven selection, the attended locations are determined by some aspect of the visual display. For example, the abrupt onset of new objects in the visual field can cause a stimulus-driven orienting of attention toward the location of the onset (Enns, Austen, Di Lollo, Rauschenberger, & Yantis, 2001; Jonides, 1981; Yantis & Jonides, 1984), even when this attentional shift is counter to the attentive goals of the observer (Remington, Johnston, & Yantis, 1992). However, although the distinction between stimulus-driven and top-down orienting is well established, there are interesting interactions between these processes.

Yantis and Jonides (1990) showed that when attention is highly focused at a cued location, abrupt onsets do not necessarily disrupt this attentional focus. Folk, Remington, and Johnston (1992) demonstrated that the observer's attentional control settings can determine which visual features (e.g., abrupt onsets or color singletons) will elicit stimulus-driven orienting in a given context. When observers anticipate a target that is defined by color, color singletons cause stimulus-driven capture, whereas abrupt onsets do not. However, when observers expect a target that is defined by its status as an abrupt-onset object, then abrupt onsets capture attention and color singletons have little effect. Thus, changes in

top-down settings can lead to dramatic differences in the profile of stimulus-driven effects.

Biased Competition During Spatial Selection

The studies just described focused on how specific locations are selected, but an equally important issue concerns the consequences of visual selection. That is, by what means is visual processing facilitated at attended relative to unattended locations? One hypothesis explains these effects as an emergent property of the competitive interactions that are integral to the visual system. Moran and Desimone (1985) illustrated this point by recording the activity of neurons in extrastriate cortex (V4) as a function of whether a stimulus was attended or not and whether that stimulus was accompanied by a *competing* stimulus within the receptive field of the recorded neuron. The key finding was that attention had an effect on visual responses only in the condition that included a competing stimulus. When two stimuli occupied the cell's receptive field (one stimulus was effective at driving the responses of the cell, whereas the other was not), the cell showed a good response when the effective stimulus was attended but a poor response when an ineffective stimulus was attended. However, when only a single stimulus occupied the cell's receptive field, the activity in the cell was unaffected by spatial attention. Moran and Desimone suggested that the effect of attention was to suppress the influence of unattended stimuli within a cell's receptive field. Thus, in cases in which there was no competition between attended and unattended stimuli, attention had no effect.

Reynolds, Chelazzi, and Desimone (1999) provided direct evidence for the idea that attention serves to mute the effects of competing distractors. They found that neurons in V2 and V4 showed suppressed visual responses to an effective stimulus when a second irrelevant stimulus was presented. However, when attention was directed toward the effective stimulus, the influence of the second stimulus was eliminated. Kastner, De Weerd, Desimone, and Ungerleider (1998) used functional magnetic resonance imaging to demonstrate the role of distractor interference in spatial selection. They measured attention effects in extrastriate cortex

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and found significantly larger modulations in visual activity when the targets shared the visual field with distractors than when the targets were presented in isolation. Finally, a number of behavioral studies have demonstrated the importance of distractor interference in spatial selection. Greater levels of distractor noise (from irrelevant distractor stimuli or from masking stimuli) lead to larger spatial cuing effects (e.g., Awh & Pashler, 2000; Cheal & Gregory, 1997; Palmer, Ames, & Lindsey, 1993; Shiu & Pashler, 1994). Evidence of this kind led Desimone and Duncan (1995) to suggest that spatial attention is best conceived of as an emergent property of competitive interactions within the visual system. By this view, the stimuli in multielement displays compete for limited processing resources by exerting an inhibitory effect on the processing of other stimuli. The effect of attention is to bias these competitive interactions in favor of the attended stimuli. Thus, attended stimuli are processed more effectively because they suffer less from the inhibitory effects of the stimuli that surround them.

Top-Down Control of Biased Competition

Biased competition models suggest that the characteristics of the display (i.e., the level of distractor interference) will determine the extent to which spatial selection affects visual processing. In addition, there is clear evidence that observers have top-down control over which locations will benefit from biased competition. That is, observers can make a voluntary decision about which locations will be selected (e.g., Jonides, 1980; Müller & Rabbitt, 1989; Yantis & Jonides, 1990) or about the features that will capture attention in a stimulus-driven manner (e.g., Folk et al., 1992). The primary point of the present work is to show that observers have top-down control over more than the spatial distribution of attention. We show that observers also have top-down control over the degree to which target processing is protected from distractor interference. With attended locations and display type held constant, we observed large increases in the size of spatial cuing effects when there was a high probability of interference from distractors.

Moreover, this effect was observed only when the target objects were in competition with distractor stimuli. The data suggest that the competitive advantage for the attended targets was amplified when there was a high probability of distractor interference. This top-down modulation of biased competition led to a substantial increase in the size of spatial cuing effects when the targets were in competition with distractor interference. In the absence of competition, however, cuing effects showed no change as a function of the probability of distractors. Previous research has made it clear that spatial cuing effects are larger when the targets must compete with distractors, but this has typically been interpreted as a stimulus-driven influence over the impact of spatial selection. By manipulating the probability of distractor interference, we show that both top-down and stimulus-driven factors determine the outcome of biased competition.

Experiment 1

Observers saw displays that contained many distractor stimuli (*noise* trials) or displays that contained only target stimuli (*clean* trials). During the *high-noise* blocks, observers saw 80% noise displays and 20% clean displays. During *low-noise* blocks, observers saw 20% noise displays and 80% clean displays. This design

allowed us to assess performance during noise and clean trials while independently varying observers' expectations regarding level of visual noise. As Table 1 illustrates, our design allowed an assessment of spatial cuing effects under four separate conditions: (a) noise display and observer expectation of a noise trial, (b) clean display but observer expectation of a noise trial, (c) noise display but no observer expectation of noise, and (d) clean display and observer expectation of a clean trial. We reasoned that the influence of endogenous or top-down factors should be revealed by a contrast of the right and left columns of Table 1 (i.e., as observers' expectations vary with the probability of different trial types), whereas the influence of stimulus-driven factors should be apparent in a contrast of the top and bottom rows of the table (i.e., as the displays vary between noise and clean).

Method

Observers. Eight students from the University of Oregon with normal or corrected-to-normal vision were paid to participate in a single 2-hr session.

Apparatus and stimulus displays. Stimuli were presented on a 15-in. (38-cm) color computer monitor driven by a Pentium III processor. Observers were seated 50 cm from the display. All stimuli appeared within a centrally placed 5×5 array of evenly spaced positions subtending 3.9° on each side. The center-to-center distance between adjacent positions was 0.8° . During noise trials, the target display contained 23 uppercase letters and 2 target digits. The distractor letters were randomly selected from all possible letters except for I, which was excluded because of its similarity to the number 1. The average size of the letters was about 0.7° on each side.

The target digits appeared in the diagonally opposed corners of the central 3×3 section of the 5×5 grid (see Figure 1 for an illustration of a noise trial). Thus, there were two possible configurations for the target digits: (a) upper right and bottom left corners of the central 3×3 section or (b) upper left and lower right corners of the central 3×3 section. This diagonal target arrangement was intended to minimize the likelihood of eye movements; a saccade toward one of the targets would necessarily disrupt perception of the other target location.¹ The letter distractors appeared in the remaining 23 positions of the grid. The arrangement of distractors was intended to equate lateral masking at each of the potential target positions. A new letter set (all letters were possible) and 2 target digits (from the digits 1–9) were randomly selected (without replacement) for each trial. All stimuli appeared as white objects on a black background. During clean trials, the target stimuli were positioned as in the noise trials, but no letter distractors were presented.

Design and procedure. The sequence of events in a single trial (depicted in Figure 1) was as follows. At the beginning of each trial, a fixation dot appeared in the central position of the array, surrounded by four additional dots that marked the potential target locations. Before the experiment began, each observer was instructed to pay attention to one pair of target locations (hereafter referred to as the cued locations) throughout the experiment. The cued locations were counterbalanced across observers. The dots at the unattended locations were included to equate any potential for forward masking at the valid and invalid locations. Then, 588 ms after the onset of the fixation point, the target display was presented for a period of time determined on a within-subject basis (see timing procedure described subsequently).

¹ This makes a clear prediction that eye movements should lead to a negative correlation between the probability of correctly reporting the two target locations during a specific trial. We conducted this analysis for all of the experiments reported here, and found significant positive correlations between accuracy at the two target locations for every experiment, in both the high- and low-noise conditions. Mean correlations were .269 and .240 for the high-noise and low-noise conditions, respectively.

Immediately after the offset of the target array, a masking array composed of # symbols was presented for 118 ms. During noise trials this masking array occluded the entire 5 × 5 grid, but during clean trials the masking symbols appeared only at the two locations that contained target stimuli. Finally, the masking array was replaced by a 5 × 5 array of 23 dots and 2 question marks (*postcues*) that indicated where the target digits had actually appeared. The postcues ensured that observers were accurately informed of target placement even during invalidly cued trials. The use of postcues and nondigit distractors was intended to minimize the likelihood of observers reporting information mistakenly gleaned from nontarget locations. Observers made an unspeeded report of the identity of both target digits by typing their responses into the computer. Targets were identified in a predetermined order (i.e., from left to right), but observers were free to correct their responses if they accidentally pressed the wrong key. Observers indicated that they had completed their responses by pressing the return key. Immediately after observers had entered their responses, the correct target identities and the number of points awarded for that trial were displayed as feedback. The next trial was initiated when the observers pressed the return key again.

The probability of noise and clean trials was manipulated across blocks. High-noise blocks consisted of 80% noise trials and 20% clean trials. Low-noise blocks consisted of 80% clean trials and 20% noise trials. Although the targets were equally likely to appear in either of the two configurations (i.e., cue validity was 50%), observers were encouraged to pay attention to the cued locations by means of a point system and monetary rewards based on these points. Observers were awarded 5 points for each correctly identified target during trials in which the targets appeared in the cued locations (for a maximum of 10 points awarded during these valid trials) but only 1 point for a correctly identified target in an invalid location (for a maximum of 2 points awarded during these invalid trials). During valid trials, 5 points were deducted for each incorrectly identified target (for a maximum deduction of 10 points during valid trials), but no points were deducted for mistakes during invalid trials. Observers' base pay was \$7 per hour, and they could earn up to an additional \$2 per hour on the basis of their point totals. As shown later, this reward system was successful in eliciting strong cuing effects.

Observers performed 10 blocks of 30 trials in the high-noise condition and the same number of trials in the low-noise condition. All blocks of one type (i.e., high or low noise) were completed before the other block type was presented. The order of these conditions was counterbalanced across observers. Cue validity was randomized across trials. Observers were instructed to pay attention to the cued locations, to maintain fixation whenever a trial was in progress, and to identify the digits as accurately as possible. Observers were informed about the probability of distractor interference in each block.

Timing. Observers vary significantly in the time needed to encode these target digits. As a means of ensuring an appropriate degree of difficulty for each observer, exposure duration was tailored to the abilities of each observer through a staircase timing procedure. Separate timing procedures were used for the noise and clean trial types to equate difficulty in these two conditions. Only validly cued trials were presented during this procedure. Observers began with the exposure duration set at 294 ms (an easy setting for all of the observers we tested). Exposure duration was adjusted as follows: If both digits were reported correctly, exposure duration was lowered by 11.8 ms (one monitor vertical refresh cycle, at 85 Hz); if one digit was reported incorrectly, exposure duration was raised by 11.8 ms. If both digits were reported incorrectly, exposure duration was raised

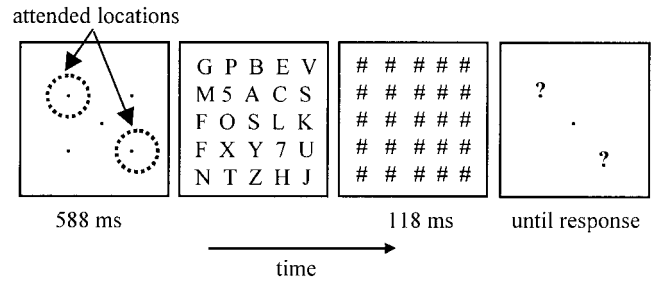


Figure 1. Sequence of events in a single trial of the task. A noise display is depicted here. In the case of clean displays, there were no letter distractors, and masks appeared only over the target locations.

by 23.5 ms. Each observer completed five blocks of 30 trials of this procedure for each display type (i.e., noise and clean), and the average exposure duration over the final block determined the exposure duration used during the experimental trials.

Results and Discussion

The mean exposure duration for the noise trials was 104 ms (*SD* = 16 ms). The mean exposure duration for the clean trials was 62 ms (*SD* = 5 ms). The data were analyzed by means of a three-way analysis of variance (ANOVA) with context (high noise vs. low noise), trial type (noise vs. clean), and validity (valid vs. invalid) as factors.² Figure 2 illustrates accuracy as a function of these three variables. Overall, accuracy was higher for clean trials than for noise trials, $F(1, 7) = 70.5, p < .01$. There was a strong spatial cuing effect; accuracy was higher for validly cued targets (71%) than for invalidly cued targets (47%), $F(1, 7) = 50.7, p < .01$. These data also replicate previous observations that spatial cuing effects are larger in the presence of distractor stimuli (i.e., during noise trials; 39%) than when no distractors are presented (i.e., during clean trials; 10%). This observation was confirmed by a significant interaction between validity and trial type, $F(1, 7) = 62.4, p < .01$.

We also observed a significant effect of context on the size of the spatial cuing effects. Cuing effects were 29% during the high-noise blocks but only 19% during the low-noise blocks, leading to a significant interaction between context and validity, $F(1, 7) = 8.1, p < .03$. As Figure 2 illustrates, this effect of context was restricted to the noise trials. Cuing effects were larger during the noise trials that were presented during the high-noise blocks (49%) than during the noise trials that were presented during the

² Half of the observers participated in the high-noise condition first, and half participated in the low-noise condition first. This raises the possibility that the order of the conditions had an effect. A separate ANOVA with order (high noise first or second), context, trial type, and validity as factors showed no main effect of order and no interaction of order with any other factor. In the subsequent experiments, there was likewise no effect of the order of the high- and low-noise conditions, with the exception of Experiment 2. In this case, we observed significant interactions between order and context and between order and validity, along with a significant four-way interaction among order, context, trial type, and validity, $F(1, 10) = 14.997, p < .01$. Context had a smaller effect on the size of cuing effects for the observers who had participated in the low-noise condition first. Because there was no trace of this effect in any of the other four experiments, we refrain from speculating about its origin.

Table 1
Design of Experiment 1

Trial type	High-noise blocks	Low-noise blocks
Noise	80%	20%
Clean	20%	80%

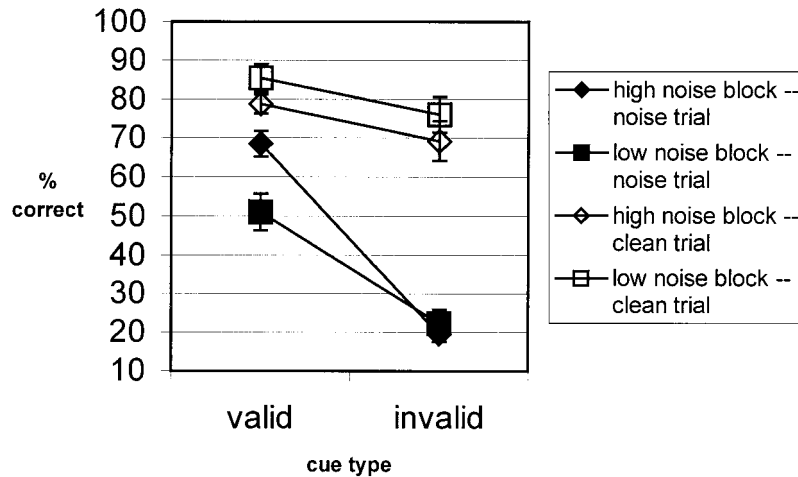


Figure 2. Accuracy in Experiment 1 as a function of cue validity, trial type, and context. Accuracy rates for noise trials are represented by the solid symbols. Accuracy rates for clean trials are represented by the open symbols. High-noise blocks are represented by diamonds, and low-noise blocks are represented by squares. Error bars represent the standard error of the mean.

low-noise blocks (29%), $t(7) = 4.8, p < .01$. There was no effect of context during the clean trials, however; validity effects in the high- and low-noise blocks were 9.6% and 9.4%, respectively, $t(7) = 0.0, p = .97$. Finally, Figure 2 also shows that the interaction between context and validity in the noise trials (shown by the solid lines in the graph) was solely a result of differences in accuracy during valid trials, leading to a significant interaction among context, trial type, and validity, $F(1, 7) = 17.2, p < .01$. Paired t tests confirmed that accuracy during validly cued noise trials was higher in the high-noise blocks (68%) than in the low-noise blocks (51%), $t(7) = 4.2, p < .01$; however, accuracy during the invalidly cued noise trials was equal in the high-noise blocks (20%) and the low-noise blocks (22%), $t(7) = 1.1, p = .31$.

To summarize, even when the type of display and the attended locations were held constant, spatial cuing effects were enhanced when distractor interference was highly probable. We hypothesize that attentional control settings change as a function of the overall context in which a trial appears. These settings determine the degree to which visual processing at the attended locations is protected from distractor interference; more distractor exclusion occurs when distractors are likely to interfere with target processing.

Although there was no main effect of context alone, observers were more accurate in general when the trial type matched the overall context of the block. Thus, accuracy was higher in the noise trials of the high-noise blocks (44%) than in the noise trials of the low-noise blocks (37%). Likewise, accuracy was higher in the clean trials of the low-noise blocks (81%) than in the clean trials of the high-noise blocks (74%). This pattern was confirmed by a significant interaction between context and trial type, $F(1, 7) = 13.6, p < .01$. Although these data are consistent with a general impairment of accuracy for unexpected trial types, this does not fully explain the results. Recall that accuracy during noise trials was higher in the high-noise context, but only for validly cued trials. There was no effect of context when the noise trials were invalidly cued. If the context effect is caused by a general disadvantage for unexpected trial types, then this effect should be

observed for both valid and invalid trials. Instead, the restriction of the context effect to the validly cued noise trials led to a substantial enlargement of the spatial cuing effects for the noise displays.

By contrast, context had no impact on the size of cuing effects in the clean trials, because the size of the context effect (i.e., better accuracy for clean trials in the low-noise than in the high-noise blocks) was identical in the attended (6.7%) and unattended (6.8%) locations. Thus, the effect of context on the clean trials may have been due to a general advantage for high-probability events, but this does not account for the pattern of results with the noise trials. This explanation assumes sensitivity to context effects on the invalidly cued noise trials; however, accuracy during these trials was only 21% (chance performance: 12%), raising the possibility of a floor effect. Recall that the increased size of the cuing effects in the high-noise condition was a direct result of larger context effects during valid trials than during invalid trials (in the noise condition). If this asymmetrical effect of context is due to a floor effect, this would bring into question whether spatial cuing effects are truly larger in the high-noise context. Experiment 2 was designed to test the hypothesis that a floor effect was responsible for the interaction between context and validity in the noise trials. We increased the exposure duration for the invalid trials by 50% to equate accuracy in the valid and invalid trials.

Experiment 2

Method

Observers. Twelve students from the University of Oregon with normal or corrected-to-normal vision were paid to participate in two 1.5-hr sessions held on separate days.

Stimulus displays. All aspects of the stimulus displays were identical to those of Experiment 1, except for the exposure duration of the invalidly cued target displays. The exposure duration during invalid trials was 50% longer than the time determined by the timing procedure. For example, if the timing procedure determined an exposure duration of 118 ms (10 monitor refresh cycles), the targets were displayed for 177 ms (15 monitor refresh cycles) during invalid trials.

Design and procedure. Each observer participated in two experimental sessions held on separate days. At the beginning of the first session, observers performed five blocks of 30 trials of the timing procedure with each display type. Observers then performed five blocks of 30 trials in the high-noise condition and five blocks of 30 trials in the low-noise condition. The order of these conditions was counterbalanced across observers. For the second session, observers began with two blocks of 30 trials of the timing procedure, with the starting exposure duration set to the exposure duration that had been determined for the first session. They then completed eight blocks of 30 trials in the high-noise condition and eight blocks of 30 trials in the low-noise condition. Again, the order of these conditions was counterbalanced across observers. As in Experiment 1, observers were instructed to pay attention to a specific configuration of locations throughout the entire procedure. The same point system and monetary rewards were in place. The observers were not informed that the invalid trials had a longer exposure duration.

Results and Discussion

Mean exposure durations for the noise trials were 97 ms ($SD = 22$ ms) for valid stimuli and 140 ms ($SD = 35$ ms) for invalid stimuli. Mean exposure durations for the clean trials were 53 ms ($SD = 10$ ms) for valid stimuli and 74 ms ($SD = 15$ ms) for invalid stimuli. The data were analyzed by means of a three-way ANOVA with context (high noise vs. low noise), trial type (noise vs. clean), and validity (valid vs. invalid) as factors. Figure 3 illustrates accuracy as a function of these three variables. Overall accuracy was higher for clean trials than for noise trials, $F(1, 11) = 35.4$, $p < .01$. As expected, the difference between accuracy in the valid and invalid trials was reduced by the increased exposure duration for the invalid trials. We therefore found no main effect of validity, $F(1, 11) = 0.69$, $p = .42$. Paired t tests showed that accuracy for noise trials was equal in the valid and invalid conditions (58% in both), $t(11) = 0.08$, $p = .93$. Nevertheless, given that the exposure duration was 50% higher for invalid stimuli, equivalent accuracy in the valid and invalid locations shows a clear effect of spatial selection. At the same time, this procedure allowed us to measure the effects of context at the valid and invalid locations without any concern about scaling artifacts.

With accuracy matched between valid and invalid positions in the noise trials, we found the same effects of context observed in

Experiment 1. Whereas the longer exposure duration for invalid trials led to a reversal of typical validity effects in the low-noise blocks (accuracy during valid trials was 6% lower than that during invalid trials), accuracy was equal between valid and invalid trials in the high-noise blocks. This led to a significant interaction between context and validity, $F(1, 11) = 7.6$, $p < .02$. As in Experiment 1, the effect of context was restricted to the noise trials. Cuing effects were larger during the noise trials that were presented during the high-noise blocks (7%) than during the noise trials that were presented during the low-noise blocks (in which a 6% advantage for invalid trials was observed), $t(11) = 3.0$, $p < .01$. However, no such effect of context was observed during the clean trials, in which spatial cuing effects were equivalent in the high-noise (-6.5%) and low-noise (-6.0%) blocks, $t(11) = 0.26$, $p = .80$. Thus, the interaction of context and trial type was also replicated in this experiment, $F(1, 11) = 15.8$, $p < .01$. Finally, the interaction between context and validity in the noise trials was a result of differences in accuracy only for validly cued targets, leading to a significant interaction among context, trial type, and validity, $F(1, 11) = 6.7$, $p < .03$. Paired t tests confirmed that accuracy during validly cued noise trials was higher in the high-noise blocks (68%) than in the low-noise blocks (49%), $t(11) = 6.9$, $p < .01$; however, there was not a reliable difference between accuracy in the invalid noise trials of the high-noise (61%) and low-noise (55%) blocks, $t(11) = 1.0$, $p = .32$.

With accuracy for invalid trials well above the floor, Experiment 2 replicated the key results of Experiment 1. When distractor interference was highly likely, spatial cuing effects were significantly larger. This enhanced spatial cuing effect was observed only in noise trials, and it was a direct result of higher accuracy in the validly cued trials of the high-noise blocks. Experiment 2 demonstrated that the context effect is restricted to processing at attended locations, even when accuracy at attended and unattended locations is perfectly matched. A general deficit for processing unexpected trial types cannot explain this effect. Instead, we hypothesize that context modulates the impact of spatial selection. When distractor interference is likely, processing at the attended locations benefits from a higher degree of distractor exclusion, leading to an increase in the size of spatial cuing effects.

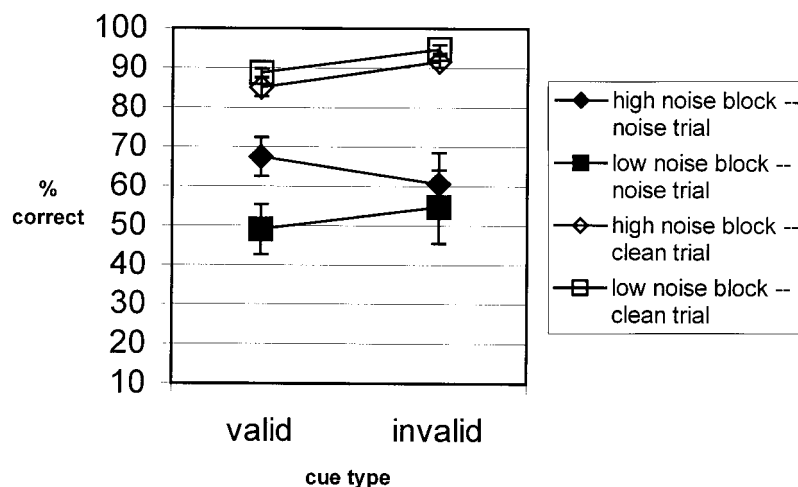


Figure 3. Accuracy in Experiment 2 as a function of cue validity, trial type, and context. Error bars represent the standard error of the mean.

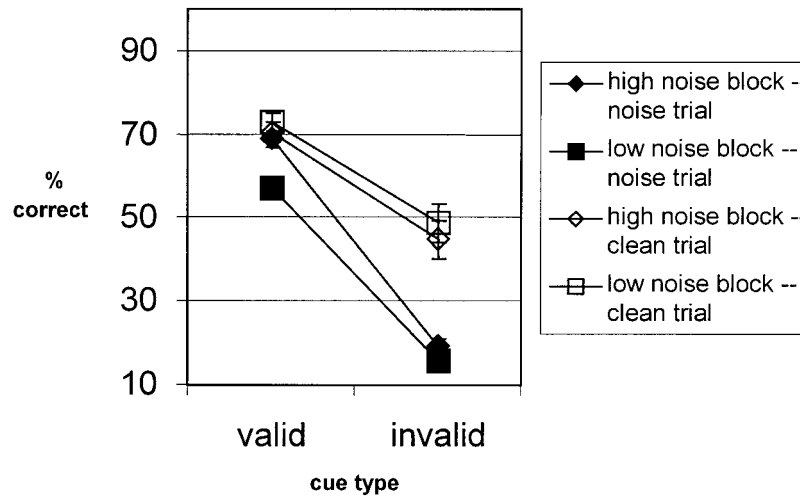


Figure 4. Accuracy in Experiment 3 as a function of cue validity, trial type, and context. Error bars represent the standard error of the mean. (Note: Some error bars are obscured by the markers.)

Experiment 3

The restriction of the context effect to noise trials suggests that likelihood of distractor interference has an impact on distractor exclusion per se instead of some other component of visual selection. A distractor exclusion account is consistent with our finding that context has no effect on performance with clean displays, because there are no distractors to exclude in these displays. In both Experiments 1 and 2, however, accuracy with the clean displays was significantly higher than that with the noise displays. The main effect of trial type may call into question the claim that context has a specific effect on performance with noise displays, if the effects of context are harder to detect at higher levels of accuracy. Because the same timing procedure was used to calibrate exposure duration for the noise and clean displays, it was not immediately obvious why accuracy was different for validly cued noise and clean trials. It is possible, nevertheless, that the relatively low validity of cues during the experimental blocks had an unforeseen effect. Observers may have occasionally selected unattended locations during the experimental blocks because of the relatively high probability of targets in the invalid locations, despite explicit instructions and monetary incentives to attend to the cued locations.

Moreover, if visual selection were important for suppressing distractor interference, then selecting uncued locations would have had a greater detrimental effect on accuracy for validly cued noise trials.³ This scenario might explain why accuracy was higher for clean than for noise displays in Experiments 1 and 2. We tested this hypothesis in Experiment 3 by increasing cue validity to 75% while maintaining the same system of points and monetary rewards as in Experiment 1. We reasoned that higher cue validity would increase observers' tendency to select only the cued locations, leading to larger cuing effects and a better match between accuracy in the noise and clean trials. Such a result would help to establish that the restriction of the context effect to noise trials was not a consequence of a scaling artifact (i.e., a result of relatively higher accuracy for validly cued clean displays). Experiment 3 also offered an opportunity to replicate the context effect using prob-

abilistic cues, the modal technique for manipulating the locus of spatial attention.

Method

Observers. Twelve students from the University of Oregon with normal or corrected-to-normal vision were paid to participate in two 2-hr sessions conducted on separate days.

Stimulus displays. All aspects of the stimulus displays were identical to those of Experiment 1.

Design and procedure. The procedure was similar to that of Experiment 1, but cue validity was raised to 75%. That is, targets appeared in the cued locations with a probability of .75 and in the invalid locations with a probability of .25. We used the same monetary rewards and point system as in Experiment 1.

Each observer participated in two experimental sessions conducted on separate days. At the beginning of the first session, observers completed 5 blocks of 30 trials of the timing procedure with each display type. Observers then performed 10 blocks of 40 trials in the high-noise condition and 10 blocks of 40 trials in the low-noise condition. The order of these conditions was counterbalanced across observers. For the second session, observers began with 2 blocks of 30 trials of the timing procedure, with the starting exposure duration set to the exposure duration that had been determined for the first session. They then completed 10 blocks of 40 trials in the high-noise condition and 10 blocks of 40 trials in the low-noise condition. The order of these conditions was counterbalanced across observers.

Results and Discussion

The mean exposure duration for the noise trials was 85 ms ($SD = 12$ ms). The mean exposure duration for the clean trials was 50 ms ($SD = 14$ ms). The data were analyzed by means of a three-way ANOVA with context (high noise vs. low noise), trial type (noise vs. clean), and validity (valid vs. invalid) as factors. Figure 4 displays accuracy as a function of these three variables.

³ The exposure duration determined by the timing procedure would not have compensated for this potential strategy, because all targets appeared at the attended locations during the timing procedure.

As this figure illustrates, the increase in cue validity was effective in equating accuracy between key conditions in the experiment. Accuracy was equal between the validly cued noise trials in the high-noise block (69%) and the validly cued clean trials in the high-noise block (71%; $p = .52$).⁴ However, the same interaction was observed between trial type and context; accuracy for validly cued noise trials dropped to 57% in the low-noise block, $t(11) = 6.0$, $p < .001$, whereas clean trial accuracy in the low-noise blocks (71%) showed a nonsignificant rise to 73% ($p = .21$). To summarize, all main effects and interactions observed in Experiment 1 were replicated in Experiment 3 ($ps < .01$ in all cases except for the triple interaction among block, trial type, and cue, $p < .05$). When distractor interference was very likely, spatial cuing effects were larger. This effect was manifest only for validly cued targets presented in noise displays.

Distractor Exclusion Versus Signal Enhancement

Two broad classes of models have been proposed to account for the relative improvement in visual processing at attended versus unattended locations. Signal enhancement models suggest that attention has the effect of directly enhancing the quality of visual representations at the selected locations. One such account is the sensory gain hypothesis suggested by Hillyard and colleagues (e.g., Hillyard, Vogel, & Luck, 1999). They have presented electrophysiological evidence that the sensory signal associated with a visual target is amplified when that target is attended. By this view, visual processing is better at the attended locations because the sensory gain is higher at those locations than at the unattended locations.

By contrast, distractor exclusion models (such as the biased competition account of Desimone & Duncan, 1995) suggest that visual processing is better at attended locations because attention serves to suppress interference from competing objects at unattended locations. One key difference between these processes is that distractor exclusion can have an effect only in the presence of interference from distractors (i.e., when there are distractors to exclude), whereas signal enhancement can benefit visual processing even in the absence of distractors. Our interpretations of the present results focus on this difference between the predictions of the signal enhancement and distractor exclusion accounts. Our primary goal is to characterize the process that produced larger spatial cuing effects in high-noise blocks than in low-noise blocks. Our conclusion is best illustrated by considering how each aspect of the present results might be explained by signal enhancement or distractor exclusion.

First, consider the main effect of cuing in these experiments. In the case of both noise and clean trials, accuracy at attended locations was significantly better than accuracy at unattended locations. In the case of the clean trials, in which no distractors were presented and masks appeared only at the target locations, signal enhancement may provide the most natural explanation of cuing effects. A similar conclusion has been drawn in previous studies revealing spatial attention effects in the absence of distractor interference (e.g., Cheal & Gregory, 1997; C. W. Eriksen & Hoffman, 1974; Henderson, 1996; Luck, Hillyard, Mouloua, & Hawkins, 1996). However, we observed significantly larger cuing effects in the noise trials. This suggests that distractor exclusion also played an important role in the selection process. One might challenge this conclusion on the grounds that the clean displays

were easier to process and were therefore less likely to show the beneficial effects of signal enhancement. But the clean trials were set at much faster exposure durations than the noise trials (by means of the staircase timing procedure included for each observer). This should have kept the difficulty of processing the clean displays within an acceptable range. In particular, consider the results of Experiment 3, in which accuracy for attended targets was perfectly matched for the attended targets in the noise and clean trials; here the cuing effects were still substantially larger for the noise displays. We conclude that whereas cuing effects in the clean trials may have resulted from signal enhancement, cuing effects in the noise trials included a clear contribution from distractor exclusion.

Now consider the effect of the context in which each trial was presented. Significantly larger cuing effects were seen in the noise trials in the high-noise blocks, when distractor interference was likely, than in the low-noise blocks, when distractor interference was unlikely. But the cuing effects in the clean trials were unaffected by the probability of distractor interferences. Is this result better explained by signal enhancement or distractor exclusion? Signal enhancement models could assert increased levels of signal enhancement at the attended locations during the high-noise blocks. For example, observers may have anticipated the increased difficulty of trials in the high-noise blocks and responded by increasing the degree of signal enhancement at the attended locations. If this were the case, however, then the attended targets in the clean trials should also have benefited from higher levels of signal enhancement in the high-noise blocks, leading to larger cuing effects for both display types. Instead, the cuing effects in the clean trials showed no change as a function of context. Thus, the restriction of the context effect to the noise trials may be incompatible with a signal enhancement account.

By contrast, an explanation of the context effect in terms of distractor exclusion is completely consistent with the interaction between context and display type. Better protection from distractor interference could explain why performance was better for noise trials during the high-noise blocks. Furthermore, changes in level of distractor exclusion should not have an impact on cuing effects with clean displays (when there are no distractors to exclude). Thus, distractor exclusion provides the best explanation of the larger spatial cuing effects observed in the high-noise blocks.

We have suggested that a general deficit in processing unexpected trials does not provide a satisfying explanation of increased cuing effects during high-noise blocks. This is based on the expectation that general impairments due to unexpected display types should be evident during both valid and invalid trials. Across the first three experiments, the effect of context on performance with the noise displays was seven times larger at the attended locations than at the unattended locations. This asymmetry was observed

⁴One might note that there was still a main effect of trial type in Experiment 3, with overall accuracy higher in the clean trials than in the noise trials. However, this difference was driven by performance in the invalid trials (in which unattended targets were significantly harder to see in the noisy displays). We reasoned that it was best to match noise and clean accuracy for attended targets when those targets were consistent with the current context. In line with this, accuracy was equivalent for validly cued noise trials in the high-noise block and validly cued clean trials in the low-noise block.

even when accuracy was equated between the valid and invalid conditions (Experiment 2).

It is not obvious why a general expectancy effect would show such a strong asymmetry between valid and invalid trials. Nevertheless, some kind of expectancy effect might account for the small but consistent effect of context on performance with the clean displays. Across the first three experiments, the respective accuracy rates on attended and unattended clean trials were 4.2% and 4.8% higher in the low-noise blocks (statistically equivalent effects in all three experiments). These matched effects at attended and unattended locations could reveal a general disadvantage in processing unexpected display types. For the noise trials, however, the effect of context was far larger at the attended locations (15.9%) than at the unattended locations (2.3%). This explains why context affected the size of cuing effects (as measured by the difference between valid and invalid trials) on the noise trials but not on the clean trials. The fact that context had very different effects on performance on noise and clean trials is inconsistent with the idea of a general disadvantage for unexpected display types. Thus, even though there may have been a small impairment associated with unexpected trials, the increased cuing effects for only the noise trials may be best explained by changes in degree of distractor exclusion.

Experiment 4

Experiments 1–3 provide evidence of changes in top-down setting as a function of the probability of distractor interference. We have argued that this top-down process should be distinguished from those that control where attention is directed. That is, instead of determining which locations will receive the benefits of spatial selection, this top-down mechanism determines how processing will be changed at the attended locations. One motivation for this distinction is the fact that observers were instructed to attend to precisely the same locations in the high- and low-noise blocks. This conclusion is also supported by the clear interaction we observed between context and display type. If the spatial distribution of attention changed from high- to low-noise blocks, then cuing effects should have changed on both noise and clean trials. However, context never had any impact on the cuing effects in clean trials. Thus, if signal enhancement accounts for the cuing effects in clean trials, then it can be concluded that the spatial distribution of this process was identical in the high- and low-noise blocks. Although the present results suggest that both distractor exclusion and signal enhancement may contribute to spatial cuing effects, it is tempting to conclude that a single mechanism determines which locations are affected by these processes. If so, then the conclusion that context did not affect the spatial distribution of signal enhancement would also extend to distractor exclusion. But the obvious alternative hypothesis is that the spatial distribution of distractor exclusion changed as a function of context, whereas signal enhancement was unaffected. This would challenge our claim that the top-down response to context in these experiments affects how attention operates rather than where attention is directed.

What could motivate changes in the spatial distribution of a selection process as a function of context? In both the high- and low-noise blocks, each observer was instructed to pay attention to the same locations. However, the target stimuli in Experiments 1–3 also appeared regularly at invalid locations. As we noted in the

introduction to Experiment 3, the inclusion of invalid trials may have occasionally motivated observers to select uncued locations. If the frequency of this strategy differed between high- and low-noise blocks, then context might have affected the spatial distribution of attention, even though the same locations were cued in each context. In most cases, it is impossible to specify exactly how observers will choose to distribute attention between cued and uncued locations, especially when observers know that the cues are imperfect predictors of target locations. In Experiment 4, we attempted to overcome this ambiguity with a task that would allow the most confident assessment of which locations should be selected. The targets in Experiment 4 were presented in the context of either high-noise or low-noise blocks, but observers were given 100% valid cues for the locations of the targets. Although one can never be absolutely certain that observers will adhere to cuing instructions, we reasoned that perfectly informative cues would provide the strongest possible motivation to attend to only the cued locations. Thus, Experiment 4 provided a strong test of whether increased levels of distractor exclusion would be observed in high-noise blocks, when changes in spatial distribution of attention are least likely to occur.

Our interpretations of Experiments 1–3 focused on the fact that larger cuing effects were observed during the high-noise blocks. The restriction of this effect to the noise trials motivated our conclusion that there were top-down changes in level of distractor exclusion. In Experiment 4, however, the size of cuing effects per se could not be evaluated, because no invalid trials were presented. Nevertheless, we were still able to assess the top-down effect of interest, because in each of the previous experiments it was expressed entirely by changes in performance at the attended locations. That is, the increase in the size of cuing effects for the noise trials was due to a large advantage in processing the attended targets in high-noise blocks relative to low-noise blocks. Context had no significant effect on the processing of unattended targets. Having established the locus of this top-down effect in the first three experiments, we were confident that the influence of context could be measured by a direct comparison of accuracy at the attended locations in the high- and low-noise conditions. Changes in distractor exclusion predicted an advantage in processing the noise displays when distractor interference was likely and no difference in processing the clean displays as a function of context.

Method

Observers. Sixteen students from the University of Oregon with normal or corrected-to-normal vision were paid to participate in a single 2-hr session.

Apparatus and stimulus displays. The stimulus displays were identical to those of the previous experiments.

Design and procedure. Each trial followed the same sequence of events as in Experiment 1. However, all trials were validly cued in this experiment. At the beginning of the session, observers performed five blocks of 30 trials of the timing procedure with each display type (noise and clean). Observers then performed six blocks of 40 trials in the high-noise condition and six blocks of 40 trials in the low-noise condition. The order of these conditions was counterbalanced across observers. Because the cues were perfectly informative, we did not employ monetary rewards or points.

Results and Discussion

The mean exposure duration for the noise trials was 80 ms ($SD = 27$ ms). The mean exposure duration for the clean trials was 51 ms ($SD = 13$ ms). The data were analyzed by means of a two-way ANOVA with context (high noise or low noise) and trial type (noise or clean) as factors. Figure 5 illustrates accuracy as a function of these variables. With 100% valid cues, Experiment 4 replicated the context effect revealed in the previous experiments. We observed a reliable interaction of context and trial type, $F(1, 15) = 38.9, p < .001$. Accuracy for the noise trials was higher in the high-noise blocks (67%) than in the low-noise blocks (43%), $t(15) = 10.4, p < .001$. Moreover, the magnitude of this effect was larger than in each of the previous experiments. Thus, our attempt to discourage any changes in the spatial distribution of attention between the high- and low-noise blocks did not reduce the size of the context effect. Also replicating the previous experiments, no significant effect of context was observed for the clean trials; accuracy rates were 75% and 77% in the high- and low-noise conditions, respectively, $t(15) = 0.7, p = .49$. These data converge with those of Experiments 1–3 to suggest that the context effect is not a result of changes in the spatial distribution of attention. Instead, we suggest that context influences the consequences of visual selection by modulating the degree of distractor exclusion.

Experiment 5

Probability of distractor interference has a clear impact on top-down settings. Identical noise displays are processed more effectively when the probability of distractor interference is high than when it is low. Because the displays are identical, this effect must be a result of changes in the state of the observers. However, the first four experiments did not address the time course of these changes. The possibility remains that repeated exposures to distractor interference induce gradual changes in the degree to which distractors will interfere with processing. Furthermore, such changes might even occur in a passive, stimulus-driven fashion. For example, consider the “tilt aftereffect” phenomenon. When an observer spends an extended period of time viewing a grating tilted to the left, subsequently viewed vertical gratings will appear to be tilted in the opposite direction. This clearly reflects a change in the observer rather than in the properties of the display. But this effect may be explained on the basis of local interactions within the

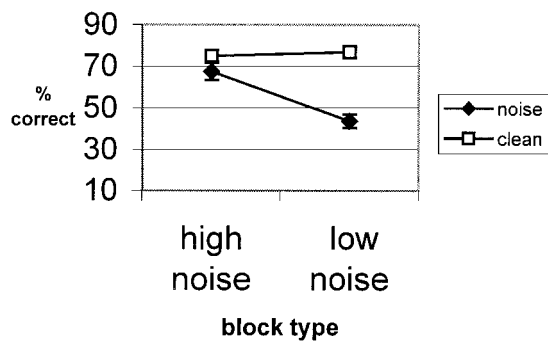


Figure 5. Accuracy in Experiment 4 as a function of trial type and context. Error bars represent the standard error of the mean. (Note: Some error bars are obscured by the markers.)

visual cortex and is not typically classified as an example of attentional control.

Another possibility is that top-down processes exert active control over the level of distractor exclusion. In this case, these settings should be capable of changing on a trial-to-trial basis as a function of current expectations regarding level of distractor interference. This possibility was not tested in Experiments 1–4, because context was manipulated across blocks. In Experiment 5, we introduced a trial-by-trial cue for the level of distractor interference in the display. By eliminating the confound between probability of distractor interference in a given trial and total amount of visual stimulation in a block, this design allowed us to assess the time course of the observed changes in distractor exclusion.

Method

Observers. Ten students from the University of Oregon with normal or corrected-to-normal vision were paid to participate in a single 2-hr session.

Apparatus and stimulus displays. The stimulus displays were identical to those of the previous experiments, with the following exceptions. The target array was a 6×6 grid subtending 5° on each side. The center-to-center distance between adjacent positions was 0.9° . During noise trials, the target displays contained 34 uppercase letters and 2 target digits. The distractor letters were randomly selected from all possible letters except for I, which was excluded because of its similarity to the number 1. After the first 25 distractor positions had been filled with a random order of these 25 letters, 9 of the letters were chosen randomly to fill the remaining distractor positions. The potential target positions were in the diagonally opposed corners of the central 4×4 section of the 6×6 grid. As in the previous experiments, there were two possible configurations for the targets: (a) upper right and lower left corners of the central 4×4 section or (b) upper left and lower right corners of the central 4×4 section.

Design and procedure. The sequence of events in a single trial was as follows. First, at the beginning of each trial, a fixation dot appeared in the central position of the array, surrounded by two additional markers. One pair of potential target locations was cued by two solid circles. The cued locations varied randomly from trial to trial. In addition, the probability of distractor interference was yoked to the locations where the circles appeared. Within a single observer, one set of locations was associated with a .8 probability of distractor interference. Trials in which these locations were cued were considered to be high-noise condition trials. The other pair of locations was associated with a .2 probability of distractor interference. Trials in which these locations were cued were considered to be low-noise-condition trials. Second, 1,528 ms after the onset of the fixation point and surrounding markers, the target array was presented. As in the previous experiments, the exposure duration was determined on a within-subject basis through the use of a staircase timing procedure.

Third, immediately after the offset of the target array, a masking array composed of # symbols was presented for 118 ms. During noise trials, this masking array occluded the entire 6×6 grid; during clean trials, however, the masking symbols appeared only at the two locations that contained target stimuli. Fourth, the masking array was replaced by a 6×6 array of 34 dots and 2 question marks that indicated the target locations. Observers indicated their responses in the same manner as in the previous experiments. Immediately after observers had entered their responses for a given trial, visual feedback indicated the number of correct responses in that trial. The next trial was initiated when the observers pressed the return key again.

Results and Discussion

The mean exposure duration for the noise trials was 134 ms ($SD = 61$ ms). The mean exposure duration for the clean trials was 35 ms ($SD = 6$ ms). The data were analyzed by means of a

two-way ANOVA with context (high noise or low noise) and trial type (noise or clean) as factors. Figure 6 illustrates accuracy as a function of these variables. There was a strong interaction between context and trial type, $F(1, 9) = 132, p < .001$. Accuracy for the noise trials was higher in the high-noise context (74%) than in the low-noise context (27%), $t(10) = 8.1, p < .001$. However, there was no significant effect of context on accuracy with the clean displays (72% and 76% in the high- and low-noise conditions, respectively), $t(10) = 1.0, p = .33$. These data show that the top-down settings of the observers changed on a trial-to-trial basis, as a function of the probability of distractor interference in the current trial. In this case, the increased degree of distractor exclusion in the high-noise condition cannot be explained by a passive response to the overall level of visual stimulation in a block of trials. Instead, we suggest that observers exert top-down control over degree of distractor exclusion according to the probability of interference in the upcoming display.

General Discussion

In the present experiments, observers reported the identities of target digits presented in displays that either were saturated with distractors (noise trials) or contained no distractors at all (clean trials). We observed significantly larger differences between accuracy of digit report at attended and unattended locations with the noise displays than with the clean displays. This result supports biased competition models suggesting that the effect of spatial selection is to protect the processing of attended targets from distractor interference. However, even when stimulus displays and attended locations were held constant, we observed large changes in the size of spatial cuing effects as a function of the context in which a trial was presented. When there was a high probability of distractor interference (as a result of a high proportion of noise trials within a block), spatial cuing effects were substantially larger than when distractor interference was unlikely. This context effect was observed only for noise trials. In the absence of distractors, context had no impact on spatial cuing effects. Our hypothesis is that the high-noise context elicited a top-down increase in level of distractor exclusion. Thus, although identical noise displays were presented during the high-noise and low-noise blocks, the consequences of selection were different in these two contexts. When distractor interference was likely, increased levels of distractor exclusion led to substantially better accuracy in terms of reports of attended targets. These experiments suggest that a complete model

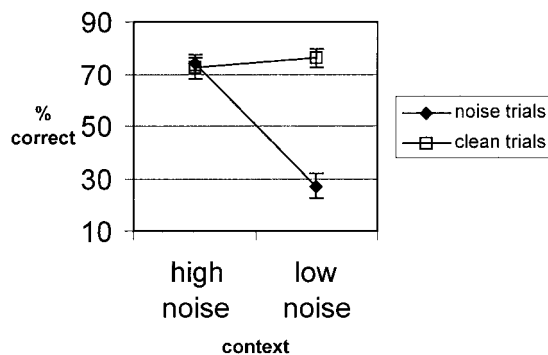


Figure 6. Accuracy in Experiment 5 as a function of trial type and context. Error bars represent the standard error of the mean.

of biased competition should acknowledge strong interactions between top-down and stimulus-driven factors.

We favor distractor exclusion as an explanation of the context effect primarily because it was restricted to noise trials. If the probability of distractor interference had caused changes in the level of signal enhancement at the attended locations, then the cuing effects in the clean trials should also have been enlarged. The absence of a context effect in clean trials is therefore inconsistent with a signal enhancement explanation. By contrast, the distractor exclusion model provides a ready explanation of why the context effect was observed only in noise trials: In the absence of interference from distractors, changes in level of distractor exclusion should not have any effect on accuracy. We are not suggesting, however, that distractor exclusion can explain all of the cuing effects observed in these experiments. We observed reliable cuing effects with the clean displays in Experiments 1 and 3. In the absence of any distractors to exclude, distractor exclusion does not provide a compelling explanation of these cuing effects. Thus, although distractor exclusion provides the best explanation for the context effect, a full account of cuing effects must invoke another component of selection.

How is distractor exclusion implemented? The competition between targets and distractors could be biased in at least two different ways. According to a direct suppression perspective, interference from distractors is reduced by active inhibition of visual processing at unattended locations. In this case, suppressed visual responses to items at unattended locations could reduce their ability to interfere with processing at the attended locations. An alternative possibility is that distractor exclusion involves the blocking of inputs from the unattended stimuli to the attended ones. By this view, processing at the unattended locations is not directly suppressed. Instead, there is a blocking of inhibitory inputs from unattended to attended competitors. Either model could account for improvements in target processing in the presence of distractor interference. If the larger cuing effects observed in the high-noise block were due to direct suppression at the unattended locations, one might predict that accuracy at unattended locations would have been worse during the high-noise blocks. However, we saw no changes in accuracy at the unattended locations as the probability of distractor interference was manipulated. This may appear to contradict the direct suppression account, but we tested only a subset of the unattended locations in Experiments 1–3, and these locations were not contiguous with the attended locations. This leaves open the possibility that there was direct suppression of visual processing at the locations adjacent to the attended locations but not at the unattended locations that were actually tested.

Cave and Zimmerman (1997) demonstrated the plausibility of this hypothesis when they observed flanking inhibition of distractors that were closest to the attended target locations. They found that reaction times to visual probes at locations directly adjacent to the attended targets were longer than those for probes presented farther away from the target locations. Their interpretation was that the target received more interference from nearby distractors, increasing the need for inhibition at those locations. Thus, although we are confident that degree of distractor exclusion changes as a function of probability of distractor interference, more work is required to identify the precise mechanism by which exclusion occurs.

The observed changes in degree of distractor exclusion appear to be mediated by changes in how attention affects processing at the selected locations rather than by changes in the spatial distribution of attention. Three features of our studies support this conclusion. First, in Experiments 1–4 observers were cued to pay attention to precisely the same two locations in the high- and low-noise contexts. Second, in Experiments 4 and 5 the targets appeared in the cued locations on 100% of the trials. This should have reduced the tendency to attend to uncued locations to a minimum, but we still observed large effects of context. Finally, changes in spatial distribution of attention might be expected to have some impact on cuing effects in clean trials, but no hint of this effect was observed. Thus, the top-down process documented in our studies may be qualitatively different from those that have been found in other studies of spatial attention.

For example, the studies of Jonides (1981) and Yantis and Jonides (1984) explored the distinction between top-down and stimulus-driven control of spatial attention. In both cases, however, the parameter of spatial selection that was controlled was one that determined which locations were eventually selected. Likewise, Folk et al. (1992) showed how observers' attentional control settings can determine which stimulus events will succeed in capturing attention. These studies show that top-down preparation to detect a target based on a specific defining feature (e.g., color) can induce attentional capture by irrelevant occurrences of the same feature. Also, whereas these studies show that top-down settings can be tuned according to a nonspatial feature such as color, this tuning has its effect in determining where attention will be focused.

Ours is not the first study to show that probability of distractor interference can affect attentional control settings. Lupianez and Milliken (1999) manipulated probability of distractor interference in a study of inhibition of return, a phenomenon in which responses to exogenously cued stimuli are slowed relative to those for stimuli in uncued locations. They showed that the onset of inhibition of return was hastened when the target display was likely to contain an irrelevant distractor. They suggested that top-down settings influenced the time course of capture by the exogenous cue, such that the initial facilitation produced by the cue endured longer when the distractor was expected. Although our studies and those of Lupianez and Milliken both demonstrate a clear effect of distractor probability, a few important differences should be noted. First, the distractor in the target displays used by Lupianez and Milliken was located about 14° away from the target, whereas the distractors in the present displays were only about 0.3° from the targets. Thus, the distractors in our studies induced powerful lateral masking effects, and those of Lupianez and Milliken did not. Furthermore, in the Lupianez and Milliken studies, the effects of distractor probability were identical for displays with and without distractors, whereas our effects were restricted to the trials that contained distractors. Therefore, whereas the effects of distractor probability in the Lupianez and Milliken study might be explained by changes in the spatial distribution of attention, the effects documented in the present studies are better explained by changes specific to the processing of distractor stimuli.

What is the nature of the distractor interference in the noise displays? We assume that the distractor letters induced a strong lateral masking effect, because every observer tested required more time to discriminate targets from the noise displays than from the clean displays. However, lateral masking can influence target

processing in multiple ways, including disruptions of early sensory processing and interference during postperceptual stages. For example, Wolford (1975) proposed a feature interaction model of lateral masking in which the features of the target and nearby distractors become confused over time. In this case, the distractor impairs performance by disrupting the early stages of building a perceptual representation of the target. By contrast, it is also known that nearby distractors can impair target processing by introducing response conflict (e.g., B. A. Eriksen & Eriksen, 1974). The present experiments do not provide conclusive evidence regarding the stage of processing that was affected by top-down changes in distractor exclusion. However, there are several reasons to consider an effect during perceptual stages of processing. Wolford and Chambers (1983) assessed several different sources of lateral masking effects and concluded that feature interaction effects were dominant when the targets and distractors were very closely spaced. This was precisely the situation in the current experiments, in which the distance between the targets and distractors was minimal.

In addition, the procedure we used was specifically designed to reduce distractor interference after the displays had been encoded. For example, the distractor letters were selected from a different alphanumeric category than the target digits to minimize confusion about which information should be reported. Likewise, the post-cues (which were perfectly informative), were intended to prevent observers from reporting information that had been gleaned from nontarget locations. Finally, previous research has clearly demonstrated that biased competition has an impact on processing in extrastriate regions of visual cortex (e.g., Reynolds et al., 1999). Thus, although further research is needed to pinpoint the locus of the effects observed in these studies, an attractive hypothesis is that top-down settings induce changes in biased competition during perceptual stages of processing. We hypothesize that the distractor letters and target digits have a lateral inhibitory influence on each other that is amplified as targets and distractors appear closer together. Spatial attention can bias these inhibitory interactions to give the advantage to the attended target digits. Furthermore, our results suggest that changes in top-down settings can influence the degree to which competition is biased in favor of the attended stimuli. Greater levels of distractor exclusion are observed when there is a high probability of interference from distractors.⁵

A number of investigators have suggested that cuing effects in the absence of distractors are best explained by the allocation of a beneficial resource toward the attended locations (e.g., Cheal & Gregory, 1997; C. W. Eriksen & Hoffman, 1974; Henderson, 1996; Hillyard & Mangun, 1987; Luck et al., 1996). Indeed, demonstrations of spatial cuing effects under noise-free conditions have been a primary source of support for signal enhancement models of attention. This hypothesis provides a pleasing explanation of cuing effects we saw with the clean displays. There are alternative possibilities, however. For example, it is possible that

⁵ The noise and clean trials differed in terms of type of mask as well as whether distractor letters were presented. Thus, the possibility should be considered that distractor exclusion had the effect of suppressing interference from the masks as well as the letters. Our view is that both the letters and the full-field mask can be viewed as strong sources of interference at the unattended locations. Thus, exclusion of interference during either stage of the trial sequence is consistent with our general conclusions.

even when no distractors are purposely introduced into a stimulus display, there is a baseline level of interference (whether internally generated or resulting from imperfect stimulus displays) that could impair processing at attended locations. If this were the case, then the exclusion of this interference could generate cuing effects with clean displays. Alternatively, if distractor exclusion involved the direct suppression of visual processing at unattended locations, then cuing effects could be generated even in the absence of interference. Direct suppression could benefit processing at the attended locations by suppressing the potentially interfering representations of objects in unattended locations. Even with distractor-free displays, targets that appear in unattended locations would be (unnecessarily) suppressed, leading to relatively better processing at attended locations.

Any of the possibilities just described could account for the cuing effects that were observed with clean trials, and additional research is needed to clarify the nature of this selection process. In any case, we have initial evidence that cuing effects during clean trials arise from a process that is functionally dissociated from distractor exclusion. The latter process is modulated by the probability of distractor interference, whereas cuing effects with clean trials show no change as a function of context. These results support previous suggestions that multiple selection processes contribute to spatial cuing effects (e.g., Cheal & Gregory, 1997; Lu & Doshier, 2000; Luck et al., 1996). Furthermore, the possibility of multiple mechanisms for spatial selection underscores the value of an approach that can isolate the contributions of a single selection process. The paradigm introduced here provides a means of selectively manipulating degree of distractor exclusion in the absence of changes in signal enhancement. Future research can harness this approach to provide a pure assessment of this key component of spatial selection.

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