Sensitivity and criterion effects in the spatial cuing of visual attention

HERMANN J. MÜLLER

University of Manchester, Manchester, England

and

JOHN M. FINDLAY

University of Durham, Durham, England

This study is concerned with three questions about the role of attention in peripheral detection: (1) Is the increased detection rate for spatial locations with high probabilities of target occurrence assigned to them due to sensitivity or criterion effects? (2) Does the effect of spatial cuing (probabilistic priming) require different explanations for letter detection and detection of luminance increments (Shaw, 1984)? (3) Can attention be shared between two separate locations cued to be most likely (Posner, Snyder, & Davidson, 1980)? These questions were investigated in two experiments, both using a signal detection plus localization task (rating method). In Experiment 1 (symbol detection), single or double cues indicating one or two most likely locations (three or two least likely locations) were presented. Introducing the second cued location resulted in a marked sensitivity gain for this position, relative to uncued locations in the single-cue condition. Decision criteria were more liberal for cued and more conservative for uncued locations. In Experiment 2, a luminance increment (single target probe) and two symbol detection (target plus distractors) tasks were compared. For symbol detection, there was a marked priming effect; but for luminance detection, cued locations showed no advantage in sensitivity. However, all tasks showed differential criterion setting for cued and for uncued locations. These results suggest that letter detection is capacity limited, whereas luminance increment detection is not, and furthermore, that decision criteria are largely preset according to a priori target probabilities assigned to particular locations.

How does the focusing and dividing of attention influence perception? Recent research on this question has been concerned with two effects: that of "display N," that is, the number of nontargets in the display, and that of "probabilistic priming," that is, the relative frequencies of target occurrence assigned to individual locations. Experiments with accuracy as the dependent measure and with high error rates have established that an increase in the number of nontargets, without an increase in the number of targets, reduces target detectability (e.g., Estes & Taylor, 1964), and that variations in the probabilities with which the target occurs at particular locations enhance target detectability for the more likely positions relative to the less likely locations (e.g., M. L. Shaw & P. Shaw, 1977). Equivalent effects have been demonstrated in reaction time (RT) experiments (e.g., Estes & Wessel, 1966; M. L. Shaw, 1978).

This research was supported by Medical Research Council Grants G80/0821, G83/6138N, and G84/03193N. The authors would like to thank D. D. Dorfman for providing a hard copy of the SDT RSCORE II program and to M. Shepherd for a copy of his EMDISP software system. Many thanks are also extended to J. Duncan for his invaluable advice concerning the joint analysis of detection-plus-localization responses and to P. M. A. Rabbitt and two anonymous reviewers for their helpful comments on an earlier draft of this paper. Requests for reprints should be directed to H. J. Müller, Age and Cognitive Performance Research Centre, University of Manchester, Manchester M13 9PL, England.

For the explanation of these effects, it seems useful to assume that there are at least two functional stages between stimulus and response: In the first, "coding," stage, which may consist of a number of substages, each stimulus is converted into an internal representation. In the second, "decision," stage, the internal representation is used to determine a response, for example, a "target present-absent" decision. According to signal detection theory (SDT; Green & Swets, 1966), the internal stimulus representation is characterized by a one-dimensional strength variable and the observer makes decisions about the presence of a target by comparing the strength of a representation against a criterion. Thus, the decisive question is whether the variations in the number of nontargets in a display and in frequencies of target occurrence at particular locations influence the quality of the stimulus representations generated by the coding process or whether the resulting effects are due to errors and criterion shifts in the decision process.

M. L. Shaw (1980, 1984) developed a model for the prediction of the maximum performance decrement that can be produced by an increase in display N on the assumption that the losses are entirely due to decision errors. This model assumes that there is a strength value associated with the stimulus (target or nontarget) at each location which represents the degree of similarity between

the internal representation and the target and, furthermore, that the strength distributions for targets and nontargets overlap. It follows that an increase in display N increases the probability that the strength of at least one nontarget will exceed the strength associated with the target. Thus, if the observer chooses the location with the greatest strength value, error rate will increase with display N even if the strength distributions remain unchanged.

M. L. Shaw (1984) found that in tasks requiring the detection and localization of letters (consistent and varied mapping), the actual performance decrement exceeded the maximum amount expected on the basis of the decision process explanation. Shaw concluded that division of attention among an increased number of locations resulted in greater overlap of the strength distributions for target and nontarget, that is, in reduced quality of the internal stimulus representations. Although Shaw thus found capacity limitation in the coding process for the detection of letter stimuli, her results for the detection of luminance increments were "radically" (sic) different: Here the entire display N effect could be accounted for in terms of the decision process. Shaw claimed that the same qualitative difference also held for the effect of probabilistic priming.

Most studies investigating this effect have used simple detection of change, that is, simple RTs to suprathreshold luminance increments (e.g., Posner, Nissen, & Ogden, 1978; Posner, Snyder, & Davidson, 1980). However, Duncan (1980a), Sperling (1984), and Shaw (1984) have recently argued that it does not necessarily follow from the mere presence of the priming effect that stimulus coding at the more likely positions is enhanced by allocation of limited attention resources. RT advantages for the more likely, relative to the less likely, locations can equally well be explained by a simple response bias, that is, a tendency of the processing system to reduce the amount of evidence it requires to decide whether a change has occurred at the more frequent positions (and, possibly, to raise the criterion for the less frequent locations). As is apparent, the response-bias explanation presupposes that the observer can adopt independent criteria (i.e., response thresholds) for positions with high and low probabilities of target occurrence.

However, Shaw's (1984) conclusion about the priming effect for the detection of luminance increments contrasts with the results of Bashinski and Bacharach (1980), who combined a "cost-benefit" analysis (Posner & Snyder, 1975) with a SDT rating procedure. This task required detection plus localization of a single target probe briefly flashed at either of two locations on signal trials (on noise trials, both locations remained blank). On cued trials, an arrow indicator primed the more ($p = .8$) and the less ($p = .2$) likely location. There were also trials with a neutral prime (i.e., $p = .50$ for each location). Bashinski and Bacharach found benefits in the sensitivity parameter, $P(A)$, for the more likely position (relative to the neutral condition). When the localization response was taken into account, there were also costs for the less

likely location. The likelihood ratios (betas) at the yes-no cutoff point were near-optimal and independent of the frequency with which the target occurred at a particular location. (Note, however, that there is a major problem with the joint detection-plus-localization analysis used by Bashinski and Bacharach which renders their conclusions questionable; see Appendix A for details.) Thus, although, according to M. L. Shaw (1984), focusing attention produces criterion shifts without sensitivity changes, according to Bashinski and Bacharach (1980), there are sensitivity effects without criterion changes.

A further question on which there is conflicting evidence concerns the limit to the divisibility of attention among spatial locations according to their assigned probabilities. M. L. Shaw and P. Shaw (1977) argued in favor of a flexible attention-sharing mechanism. They compared one condition in which a single letter appeared equally often at one of eight locations with another in which the frequencies of target occurrence assigned to individual locations were varied ($p = .25$ and $p = .05$ for each two locations and $p = .10$ for four locations). Shaw and Shaw found that after providing extensive practice with these frequency distributions, recognition accuracy, in the first condition, did not differ between locations, whereas in the second condition, it varied as a function of the probability with which the target occurred at any particular location.

Posner et al. (1980) questioned whether attention can be allocated freely over the visual field within a given trial, and argued that M. L. Shaw and P. Shaw's (1977) finding may be due to averaging across trials, that is, "subjects [may] sometimes attend to one position . . . and sometimes to another, and these probabilities match those assigned to target presentation" (p. 169; see also Eriksen & Yeh, 1985). Posner et al. investigated this possibility, employing a simple RT task (Experiment 5). They used four signal locations arranged horizontally to the left and right of a fixation mark. One location, indicated to be the most likely ($p = .65$) target position, varied across trials, while another location, the second most likely ($p = .25$) position, remained fixed throughout a block of trials. Posner et al. found that RTs for the second most likely position were faster than those for the uncued locations when it was adjacent to the most likely position but not when it was separated from it. Posner et al. (1980) concluded that, for luminance detection, there were "severe limits in the ability of subjects to assign attention to a secondary focus in addition to a primary focus"; however, the focus of attention "may vary in size according to the requirements of the experiment" (p. 171).

Thus, two questions seem open: Is luminance detection "radically" different from letter detection, and what are the "limits" to the ability to divide attention among spatial locations according to probabilistically defined priorities? These questions were investigated in two experiments. Both employed a task that was amenable to SDT analysis and required the detection plus localization of a target stimulus.

Experiment 1 investigated the limits to the divisibility of attention among spatial locations. The target stimulus was the symbol "T," which could appear in any of the four orthogonal orientations; nontargets were crosses of the same size and luminance. On every trial, a "single arrow" or a "double arrow" was presented to indicate the likely target position(s) in a display of four locations; that is, the cued positions numbered either one (single arrow) or two (double arrow) and the uncued locations, either three or two. The probability of target occurrence for the "cued 1" position was the same in single- and double-cue conditions. The probability of target occurrence for the "cued 2" location in double-cue conditions was either the same as that for "cued 1" (i.e., both arrow arms were equally long) or it was reduced (i.e., the second arm was shorter). Estimates of the sensitivity parameter, $P(A)$, and the bias parameter, β , were obtained independently for indicated and nonindicated locations and compared within and between cuing conditions.

Experiment 2 investigated differences between luminance increment and symbol detection tasks. There were three display conditions: The first was similar to that used by Bashinski and Bacharach (1980), that is, although the target was a T, the task required only detection of the luminance change produced by its arrival in the display; the second display condition was the same as in Experiment 1 (consistent mapping); and the third was a varied mapping task in which Ts in different orientations changed roles as targets and nontargets from trial to trial.

EXPERIMENT 1

Method

Observers. Three observers (I.P., N.A., and H.M.; males; ages 20–30 years; each with corrected-to-normal vision) participated in Experiment 1, which lasted 30 h inclusive of two practice sessions. They were all experienced psychophysical observers who had taken part in a previous study involving a similar task (Müller & Findlay, 1987). Payment was £1.50 per hour plus a bonus of £10.0 for completion of the experiment. H.M. also participated in a repetition of Experiment 1 involving the monitoring of eye movements (Experiment 1a).

Apparatus and materials. Stimuli were presented on a Hewlett Packard 1321A X-Y monitor with P31 phosphor, driven by a CED Alpha (LSI 2/20G) computer via a CED 502 Interface. Refreshing of the display and sampling of digital and analogue (Experiment 1a) inputs were controlled by a software system developed by Shepherd (1984). Observers viewed the screen from a distance of 115 cm with their heads resting on a chinrest. The laboratory was dimly illuminated by an incandescent lamp placed behind the screen and shielded from direct view (to eliminate reflections). The luminance of the stimuli was 0.1 fL, and the luminance of the background of the screen was 0.01 fL.

Display and timing. Figure 1 illustrates the sequence of frames presented on a given trial. Frame I displayed a central fixation dot and four boxes in the periphery in one of which the target stimulus, a T in any of the four orthogonal orientations, could appear later (Frame V). After 500 msec, the central fixation dot was replaced by a central box containing one of the four Ts (Frame II); this T, displayed for 1,500 msec, indicated the target stimulus for a given trial. Then, 1,000 msec after the reappearance of the fixa-

Figure 1. Sequence of frames (I–VI) presented on a given trial.

tion dot (Frame III), an arrow cue appeared in the display center for 350 msec indicating the most likely target position(s) (Frame IV). There were also neutral ("baseline") trials on which a cross (instead of the arrow) was displayed in the center; this cross indicated that the target was equally likely to appear at any of the four locations. Seven hundred and fifty milliseconds after the onset of the cue, the target display appeared for a limited exposure duration (Frame V). On signal trials, the target stimulus occurred in one of the four boxes and distractor crosses of the same size and luminance appeared in the remaining three boxes. On noise trials, distractor crosses appeared in all four boxes. The eccentricity of the target stimulus was 4.4° , and the size of its constituent lines was 0.25° . In Frame VI the exposure of the target and distractors was terminated by contour masks (squares of 0.25° line size).

Figure 2 illustrates the three cuing conditions used in Experiment 1. In the first condition (left), the cue in Frame IV was a single arrow that might or might not correctly prime the cued position (single cue). However, on this trial, the target (in Frame V) did not appear at the most likely position indicated by the arrow (valid 1), but rather at one of the uncued locations (invalid). In the second condition (middle), the cue in Frame IV was a double arrow with two arms of unequal length (double cue 1). The long arm indicated the most likely location (valid 1), and the short arm indicated the second most likely location (valid 2). Note that the central fixation dot remained in the display while the cues were presented in order to allow the long and short arrow arms to be discriminated when they formed a straight line. In the third condition (right), the cue was a double arrow consisting of two arms of equal length (double cue 2). This two-arm arrow indicated that the target might appear with equal probability at either of the two cued locations. Over the whole experiment, all four locations were indicated with equal frequencies, and under the double-arrow conditions, all configurations of the two arms occurred equally often.

Task. After the target display terminated (Frame VI), the observers made three responses, in succession, on a hand-held keypad: (1) a yes-no response indicating whether or not a target had been detected; (2) a rating of the certainty of the yes-no response—in the case of a positive response according to the categories "very certain," "certain," and "uncertain" and in the case of a negative response according to the categories "uncertain" and "certain"; and (3) a position response indicating in which of the four boxes a target had been detected; this response was also required in the case of a "no-target" response.

Design. Table 1 shows the frequencies of signal and noise trials and the frequencies with which, on signal trials, the target appeared at the cued and uncued locations. Table 1 shows that the frequency of the most likely location (valid 1) was the same for all three cuing conditions. The introduction of the second cued location (valid 2) in the double-arrow conditions reduced the frequencies of target

Figure 2. Cuing conditions: Single cue (left), double cue 1 (middle), and double cue 2 (right).

Table 1
Summary of the Frequencies of Signal Trials (Valid 1, Valid 2, and Invalid, Neutral) and of Noise Trials for Each Cuing Condition

Condition	Single	Double 1	Double 2
Valid 1	576	576	576
Valid 2		384	576
Invalid	256*	192†	96†
Noise	672	672	672
Neutral	256	256	256
Noise	128	128	128
Total	2400	2400	2400

*Number of invalid locations = 3. †Number of invalid locations = 2.

occurrence at the two remaining uncued locations (invalid). Note that signal and noise trials were presented in the ratio of 2:1. Adding the cell entries over the cuing and cue-validity conditions, Experiment 1 consisted of 7,200 trials in all. Each observer was presented with these 7,200 trials, in six experimental sessions of 1,200 trials. Each session was subdivided into three blocks of 400 trials, in which one of the three cuing conditions was presented. With the design thus blocked with regard to the cuing conditions, valid, neutral, and invalid trials were presented in randomized order. (The positions indicated by the arrow cues were also randomized across trials.) The order of presentation of single-cue, double-cue 1, and double-cue 2 blocks within a given session was counterbalanced across observers and sessions.

Determination of the threshold exposure durations. At the beginning of each session, the threshold exposure times for target detection were determined individually for each observer according to a modified PEST adaptive staircase procedure (Findlay, 1978) that aimed at a detection sensitivity $P(A) = 0.75$ on neutral trials. Ratings of the certainty of the yes-no response and position responses were also required on threshold trials to give the observers practice at the task, but only detection hits and false alarms were used to home in on the intended threshold level. Preexperimental estimations had shown that the 0.75 thresholds corresponded to a ratio between the average detection hit and false alarm rates of about 2:1. Table 2 presents, individually for each observer and session, the 0.75-threshold exposure durations estimated in this way. As can be seen from Table 2, the times varied between 20 and 40 msec and showed a reduction as the number of sessions increased.

Instruction. In two practice sessions, the observers were familiarized with the task. After an initial outline of SDT and the instruction to maximize the number of hits and minimize the number of false alarms, repeated threshold determinations were carried out until the estimated times stabilized. After each threshold determination, the observers were shown their hit and false-alarm rates for each rating category; they were then given specific instructions on how to improve performance. In the next stage, the observers were introduced to the three cuing conditions. They were told the frequencies of target occurrence at individual locations and instructed to divide their attention accordingly. Again, they practiced the task over several blocks of trials. The observers experienced no difficulty

in making the position response, even in the case of a "no-target" response. They were instructed to avoid responding to a given location if they were certain that it did not contain the target, since this would improve their overall performance. The observers were told to fixate on the central dot throughout the display cycle.

SDT analysis. The data were subjected to a two-stage SDT analysis: In the first stage, localization responses were ignored; that is, detection responses were accepted as hits if the yes-no responses were correct. In this stage, false-alarm rates were the same (i.e., pooled) for valid and invalid trials, but different for the neutral condition (since they were estimated from trials with neutral warning signals).

In the second stage, detection responses were accepted as hits only if both the detection and the localization responses were correct. In this stage, it was possible to assign false alarms to individual display locations, since the localization responses specified whether a target stimulus had been—erroneously—detected at a cued or at an uncued position. Restated, this analysis stage took into account the cue-validity condition (valid 1, valid 2, invalid) under which a particular noise trial would have fallen if it had been a signal trial (as indicated by the positive detection response). Accordingly, false-alarm rates differ between valid and invalid trials. For neutral trials, however, this conditional classification of false alarms was not possible. Thus, false-alarm rates are the same for the two analysis stages; note, however, that hit rates can vary, since correct detections may not always be associated with correct localizations.

This hierarchical STD analysis is similar to Bashinski and Bacharach's (1980). Note, however, that false-alarm rates in the joint detection-plus-localization analysis were calculated according to a solution that avoids the problem in the procedure used by Bashinski and Bacharach (see Appendix A for further details).

The sensitivity parameter $P(A)$, that is, the area under the receiver-operating characteristic (ROC) curve, was the principal performance measure (since it has the advantage of being relatively independent of the shapes and variances of the underlying distributions of noise and signal plus noise; see Green & Swets, 1966; Simpson & Fitter, 1973). In both stages of analysis, computer-determined estimates were obtained for the $P(A)$ parameter and its variance and for the likelihood ratio beta at the yes-no cutoff point. These estimates were based on the maximum-likelihood (ML) approach developed by Dorfman and Alf (1969) and calculated for each observer under each experimental condition. The $P(A)$ values obtained for the cue-validity conditions under the three cuing conditions were tested for statistical significance (one-tailed within-subject G tests; Gourevitch & Galanter, 1967).

Eye-movement monitoring. Since eye movements were not monitored in Experiment 1, and since effects of attention on sensitivity might be confounded with effects of retinal eccentricity (i.e., saccades to cued locations could be executed because of the 700-msec delay between cue and target onset), Experiment 1 was repeated for one observer, H.M., with his ocular fixation monitored (Experiment 1a). The design and procedure of Experiment 1a were the same as in Experiment 1, except that the numbers of trials under each cuing and cue-validity condition were only half the values given in Table 1 (three 4-h sessions of each three blocks of 400 trials). Target exposure time was 45 msec (average of the three sessions). Room lighting was normal (fluorescent overhead light). Head movements were eliminated by providing a dental bite bar. Eye movements, that is, the horizontal movement components, were monitored by a photoelectric method (ACS eye-movement system EM 130; see Abadi, 1980). Output from the eye-movement recorder was sampled every 10 msec, starting at cue onset and ending 150 msec after target onset. The eye-movement record for each trial was analyzed at the end of the trial. If a pre- or poststimulus saccade was detected, the trial was rejected and rerun later in the block. Saccade detection was based on comparisons of the differences between successive samples with a velocity criterion (two successive

Table 2
0.75-Target Exposure Times (in Milliseconds) for Each Observer and Session

Observer	Session						Mean
	1	2	3	4	5	6	
I.P.	38	34	35	31	29	27	32.3
N.A.	41	34	30	32	30	27	32.3
H.M.	34	30	27	25	26	24	27.7
Mean	37.7	32.7	30.7	29.3	28.3	26.0	30.8

differences, both in the same direction, exceeding approximately 30°/sec). To detect slow fixation drifts on accepted trials, mean (and variance) differences between the first 25 samples after cue onset and the last 25 samples before target offset of each eye-movement record were analyzed at the end of the experiment (see Table 10 in Appendix B).

Results

Table 3 presents the $P(A)$ values for the detection data (D) and the joint detection-plus-localization data (D+L), individually for each observer and separately for the three cuing conditions and for the valid (V), neutral (N), and invalid (I) trials. Note that the values for H.M. in Experiment 1a (eye-movement monitoring) are indicated by an asterisk; H.M.'s values for Experiments 1 and 1a were averaged before calculating the group means. Data were pooled over the neutral trials under the three cuing conditions, since an initial analysis did not reveal any significant differences depending upon which cue (single or double) was presented during a given block of trials.

Note that the $P(A)$ values for observer H.M. show no substantial differences between conditions with (H.M.*) and without (H.M.) eye-movement monitoring. This indicates that eye movements cannot account for the pattern of sensitivity effects found in Experiment 1. Further evidence for this is provided by the absence of $P(A)$ differences between "adjacent" and "opposite" cues (see Appendix B, in particular Tables 12 and 13, for details). That eye movement would produce such differences is shown in a further experiment (Experiment 1b), which is reported in Appendix B. In this experiment, varying the retinal eccentricity of cued and uncued positions (single cue) resulted in systematic sensitivity differences between

uncued locations adjacent and opposite to the cued position.

$P(A)$ values are generally higher and differences in $P(A)$ between cue validity conditions are generally smaller for the joint D+L analysis than for the analysis of the detection responses on their own. Note that all comparisons reported below are based on G tests between D+L data pooled over observers.

$P(A)$ is higher for the most likely location (V 1) in the single-cue condition than it is for the uncued locations in the neutral condition ($p < .001$ for all comparisons). In the double-cue 1 condition, $P(A)$ tends to be higher for the more likely one of the two cued positions; but note the exception of N.A. (D+L data). And $P(A)$ shows a reliable difference not only between the most likely position and the neutral and invalid trials (V 1-N: $p < .005$; V 1-I: $p < .001$), but also between the second most likely position and the latter conditions (V 2-N: $.10 > p > .05$; V 2-I: $p < .025$). In the double-cue 2 condition, there are no differences in $P(A)$ between the two indicated positions (V 1 and V 2), which are equally likely. Of course, from the point of view of the observer, the two locations (as defined in the computer program) are not distinguishable. Furthermore, $P(A)$ is significantly higher for these positions than for the neutral condition and the two uncued locations (V 1-N: $.10 > p > .05$; V 1-I: $p < .05$; V 1-N: $p < .05$; V 1-I: $p < .025$). There are no reliable differences between the neutral and invalid trials under any of the cuing conditions. Note, however, that for all observers, $P(A)$ appears lower for invalid than for neutral trials, except for H.M.* in the single- and for I.P. in the double-cue 1 condition (D+L data).

$P(A)$ is higher for the most likely location (V 1) in the single-cue condition than it is for the most and second most likely locations in the double-cue 1 and 2 conditions ($p < .01$ and beyond for all comparisons between the single-cue and the double-cue conditions in D+L data). $P(A)$ does not differ among the invalid trials in the single-cue, the double-cue 1, and the double-cue 2 conditions.

In the detection data, the differences in $P(A)$ between valid and invalid trials occur because hit rates are higher for cued than for uncued locations (see Table 4 for the mean hit and false-alarm rates). Since noise trials were pooled across cue-validity conditions (valid and invalid) within a given cuing condition (single or double), there are no differences between false-alarm rates. Such differences could occur between cuing conditions. However, total false-alarm rates do not vary much (the average false-alarm rates are 0.361, 0.354, 0.351, and 0.379 for the single-cue, the double-cue 1, the double-cue 2, and the neutral conditions, respectively).

Hit rates are lower for the joint D+L responses than for the detection responses, since, on some trials, correct detections were associated with localization failures. Reductions in hit rates are more than twice as marked for uncued than for cued locations. False-alarm rates are higher for cued than for uncued locations. That is, on noise trials, cued locations were about 1.5 times more likely

Table 3
Detection and Joint Detection-Plus-Localization Sensitivity:
Summary of $P(A)$ Values for Each Observer Under
Each Experimental Condition

Observer	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Single Cue								
I.P.	.909		.784	.779	.948		.900	.897
N.A.	.923		.788	.783	.946		.913	.912
H.M.	.951		.780	.784	.970		.909	.891
H.M.*	.892		.780	.770	.936		.897	.902
Mean	.918		.784	.780	.949		.905	.902
Double Cue 1								
I.P.	.894	.865	.784	.777	.926	.918	.900	.902
N.A.	.908	.894	.788	.783	.932	.935	.913	.891
H.M.	.940	.911	.780	.770	.953	.927	.909	.901
H.M.*	.882	.864	.780	.762	.917	.900	.897	.882
Mean	.904	.882	.784	.775	.931	.922	.905	.895
Double Cue 2								
I.P.	.868	.871	.784	.775	.920	.920	.900	.893
N.A.	.904	.910	.788	.768	.924	.935	.913	.875
H.M.	.922	.922	.780	.766	.923	.934	.909	.905
H.M.*	.880	.870	.780	.755	.914	.902	.897	.871
Mean	.891	.892	.784	.768	.921	.924	.905	.885

Note—V = valid, N = neutral, I = invalid. *Eye movements monitored.

Table 4

Detection and Joint Detection-Plus-Localization Hit (HR) and False-Alarm (FAR) Rates: Summary of Mean HR and Mean FAR Under Each Experimental Condition

	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Single Cue								
HR	.918		.769	.768	.877		.699	.663
FAR	.361		.379	.361	.108		.095	.084
Double Cue 1								
HR	.893	.872	.769	.753	.847	.802	.699	.640
FAR	.354	.354	.379	.354	.107	.093	.095	.078
Double Cue 2								
HR	.887	.886	.769	.732	.833	.848	.699	.604
FAR	.351	.351	.379	.351	.120	.128	.095	.052

Note—V = valid, N = neutral, I = invalid.

to be accepted as the target position than was any particular one of the uncued locations.

Thus, the general increase in $P(A)$ from the detection to the joint D+L analysis is due to the partitioning of the total (i.e., detection) false-alarm rates into cue-validity conditions (see Appendix A). But this increase is less marked for valid than for invalid trials, because more false alarms are assigned to cued than to any particular one of the uncued locations. The result is a decrease in costs plus benefits in $P(A)$ from the detection to the joint D+L analysis.

Figure 3 presents the normal-normal detection ROC curves, based on the certainty ratings, for each experimental condition; the values plotted are the means of the 3 subjects (excluding H.M.*). D+L ROC curves are not presented because $P(A)$ differences between cue-validity conditions are rather small (i.e., ROC curves are not clearly discernible).

As can be seen from Figure 3, the ROC curves show a linear relationship between the z-transformed rates of hits and of false alarms; this is also true for the D+L ROC curves (i.e., none of the chi-squared values based on the final ML slope estimates is significant). The linear ROC curves suggest that the underlying distributions of noise and signal plus noise are Gaussian. The slopes of the ROC curves vary systematically: For valid trials, the average

slopes are 0.57 and 0.60 for the detection and the joint D+L ROC curves, respectively; they are 0.87 and 1.09 for neutral trials and 0.98 and 1.01 for invalid trials. That is, although the variances of the noise and signal-plus-noise distributions are approximately equal, for the invalid and neutral trials, the variance of the signal-plus-noise distribution is greater than that of the noise distribution for the valid trials. The slope differences between the valid trials, on the one hand, and the neutral and invalid trials, on the other, are generally significant ($p < .001$, except for $p < .025$ for the comparisons between V 1-I and V 2-I in the double-cue 2 condition; G tests based on the ML estimates for the slopes and their variances for D+L pooled data).

To test whether the instruction to maximize hits and minimize false alarms was successful, the likelihood ratios (betas) at the yes-no cutoff were estimated (ML estimates) for each observer under each experimental condition. The individual beta values are presented in Table 5, separately for the detection and the joint D+L data. The values for H.M. in Experiment 1a (eye-movement monitoring) are indicated by an asterisk; H.M.'s values for Experiments 1 and 1a were averaged before calculating the group means. These beta estimates take into account the fact of the unequal variances of the noise and signal-plus-noise distributions.

The beta values are higher for the joint D+L responses than for the detection responses on their own. Furthermore, the beta values vary as a function of the probability with which the target occurs at a particular location: They are lower for valid than for neutral and invalid trials, for all observers. In other words, the lower the probability with which a target occurs at a particular location, the more evidence is required for a positive decision, and vice versa. This also applies to the comparison between the most likely and the second most likely location in the double-cue 1 condition.

Discussion

In general, the results of Experiment 1 agree with those of Bashinski and Bacharach (1980). There are sensitivity gains for cued position(s); there are only slight sensitivity losses for uncued locations relative to the neutral base-

Figure 3. Detection ROC curves for the single-cue (left), double-cue 1 (middle), and double-cue 2 (right) conditions and for the valid 1 (open triangles), valid 2 (filled triangles), neutral (open circles), and invalid trials (open squares).

Table 5
Detection and Joint Detection-Plus-Location Bias: Summary of the
Likelihood Ratios (Beta) at the Yes-No Cutoff for Each
Observer Under Each Experimental Condition

Observer	Detection				Detection + Localization			
	V1	V2	N	I	V1	V2	N	I
Single Cue								
I.P.	.233		.614	.722	.629		1.722	2.205
N.A.	.211		.735	.795	.581		2.467	2.777
H.M.	.145		.746	.860	.515		2.690	2.538
H.M.*	.384		.770	.770	1.201		2.460	2.662
Mean	.236		.702	.777	.689		2.255	2.527
Double Cue 1								
I.P.	.252	.391	.614	.667	.579	1.114	1.722	2.329
N.A.	.223	.298	.735	.749	.666	.998	2.467	2.147
H.M.	.156	.234	.746	.976	.425	.681	2.690	3.037
H.M.*	.455	.575	.770	.974	1.101	1.447	2.460	3.283
Mean	.260	.365	.702	.797	.669	1.059	2.255	2.545
Double Cue 2								
I.P.	.355	.339	.614	.725	.910	.812	1.722	2.729
N.A.	.304	.288	.735	.850	.737	.771	2.467	2.542
H.M.	.236	.221	.746	.962	.559	.547	2.690	3.661
H.M.*	.485	.470	.770	.946	.938	.941	2.460	3.421
Mean	.340	.324	.702	.843	.799	.776	2.255	2.937

Note—V = valid, N = neutral, I = invalid. *Eye movements monitored.

line (even if the position response is taken into account); however, slopes of ROC curves for a particular location decrease to follow the assigned probability of target occurrence.

The asymmetric relationship between the sensitivity gains for cued position(s) and the losses for uncued locations, relative to the neutral condition, may occur because the neutral trials underestimate baseline performance. Since neutral trials were far less frequent than cued trials (the ratio is 1:5.25), they were comparatively unexpected. Jonides (1981) found reduced efficiency of unexpected symbolic cues; this might apply as well to the neutral warning signal presented in Experiment 1 (see also Müller & Rabbitt, 1987). Comparisons between cued (valid and invalid) and neutral trials should therefore be interpreted with caution (see Jonides & Mack, 1984).

The finding that the slopes of the ROC curves are close to 1.0 for uncued locations and in the neutral condition but significantly lower for cued positions is expected if one assumes that the uncued, neutral, and cued conditions represent increasing levels of signal strength. That is, there is a positive relationship between the frequency of target occurrence at a particular location and the confidence associated with positive decisions (signal trials) for this location. Note, however, that this relationship involves a tradeoff in terms of slightly higher false-alarm rates for the increased certainty associated with the more likely locations.

The most important result of Experiment 1 (for which the "actual" neutral baseline appears irrelevant) is that there are substantial sensitivity gains for a second cued position, in comparison with the uncued locations in the

single-cue condition. This finding suggests that, for letter detection and for trial-by-trial cuing of the second most likely position, attention can be more effectively divided between two indicated locations than has been proposed by Posner et al. (1980). However, although division of attention appears possible, the available processing capacity cannot be increased. This can be concluded from the finding that $P(A)$ for the position cued to be most likely and, less reliably, for the uncued locations decreases from the single- to the double-cue conditions. That is, the total processing capacity is fixed.

On the basis of the estimates of the time it takes to shift attention from one location to another (e.g., Sperling & Reeves, 1980; Tsai, 1983a; Tsai estimates the velocity of attention shifts in response to peripheral cues to be about 1° per 8 msec), it seems unlikely that the division of attention between the cued locations in Experiment 1 is based on "attention switching." An "attention sharing" model that assumes that a fraction of the total processing capacity is allocated simultaneously to each of the input sources appears more appropriate (e.g., the "sample size" model of M. L. Shaw, 1980, 1984, and M. L. Shaw, Mulligan, & Stone, 1983; see also Green & Luce, 1973, and Luce, 1977). (Note that this argument depends on effective control of target exposure times, i.e., effective display termination by the masking contours. Experiment 1c, reported in Appendix C, shows that sensitivity gains for a second cued position cannot be explained in terms of inadequate masking.)

Suppose, for instance, that a fixed number of samples is available to be divided among the input locations, and that more samples are allocated to the more likely and fewer to the less likely locations. The greater number of samples allocated to cued positions reduces the variability associated with the sample mean and thus improves discriminability between target and distractor, and vice versa for uncued locations.

One should expect that the higher variability for uncued locations increases the probability that nontargets presented at them will exceed the criterion, whereas this is less likely for nontargets presented at cued locations. That is, on noise trials, a target should be more likely to be—erroneously—detected and localized at one of the uncued locations. However, the observed false-alarm rates are slightly higher, rather than lower, for cued locations.

This suggests that observers give preferential weight to evidence from cued locations, in accordance with the higher a priori signal probabilities assigned to them. Suppose that, on a noise trial, both the stimulus at a cued location and the stimulus at an uncued location produce a target impression, but that the evidence is stronger for the uncued location. Thus, if the observer decided on the basis of the strength of "sensory" impressions, he would allocate a false alarm to the uncued position. However, if he assigned preferential weight to the evidence from the likely target location, he would—erroneously—decide in favor of the cued position.

Thus, the slightly higher false-alarm rates for cued than for uncued locations can be taken as indicating some bias toward accepting information available from cued positions as target information. Thus, the summing of false-alarm rates over cue-validity conditions in the analysis of the detection responses on their own exaggerates the advantage in sensitivity for valid over invalid trials because it "reduces" the false-alarm rates for cued locations and "increases" them for uncued locations. Note that any bias toward accepting information from cued locations as target evidence would inflate not only false-alarm rates but also hit rates for cued locations, and vice versa for uncued locations. However, in the joint D+L analysis, inflated hit rates for cued locations are counterbalanced by higher false-alarm rates, and vice versa for uncued locations. That is, joint D+L $P(A)$ values give a less "biased" estimate of costs plus benefits in sensitivity than do detection $P(A)$ values.

The suggestion that the observer assigns weights to display locations according to their probabilistic priorities is consistent with a class of models that can be referred to as "weighted integration" (Kinchla, 1977) or "first-order integration" (M. L. Shaw, 1982) models. The "order 1 code" X for a given location is a "sensory" representation of the stimulus that is influenced by the type of stimulus presented (target or nontarget), its energy, and so forth, and, possibly, by the amount of attention allocated to this source. (Note that, in Shaw's sample size model, X is the strength of a continuous-state random variable and that the strength values are derived independently for each input source.)

For the localization decision, the observer might then weight the X s according to the a priori probabilities assigned to their locations and choose the position for which the weighted X is the maximum (M. L. Shaw et al., 1983). For the detection response, however, the (weighted) X s might be summed and compared with a criterion (M. L. Shaw, 1982). Thus, in the integration process, the strength value associated with the actual target position may play a decisive role in determining the detection response. Target localization, however, may fail if the strength values associated with one or more of the nontarget locations exceed the value of the target position because of sample strength variability and/or differential weighting.

The finding of a more positive response bias for cued positions and a more negative bias for uncued locations and the neutral condition does not agree with Bashinski and Bacharach's (1980) failure to find any bias effects (but see Appendix A). This result shows, in accordance with M. L. Shaw (1984; M. L. Shaw et al., 1983), that observers can and do adopt independent criteria at different locations. Furthermore, this finding is consistent with the weighted integration model; that is, assigning more weight to strength values for cued locations and less weight to strength values for uncued locations is equivalent to lowering and raising, respectively, the decision criteria for cued and uncued locations.

It seems reasonable to suggest that the decision criteria (i.e., the "weights") are largely set a priori, before display onset, on the basis of the probabilistic information provided by the spatial cue—that is, more liberal for more likely and more conservative for less likely locations. Note, for instance, that as the a priori probability assigned to uncued locations decreases from the single-cue through the double-cue 1 to the double-cue 2 condition, beta tends to increase (see Table 5). However, there might also be some a posteriori adjustment of decision criteria on the basis of the perceived competition among independent input sources for the D+L response. Suppose, for example, that the strength values of two or more equally likely locations favored a positive response. To decide optimally among these locations, the decision criterion might be raised. Note, for instance, that beta for cued locations tends to be higher in the double-cue 2 than in the single-cue condition, although the a priori probabilities are equal (see Table 5).

EXPERIMENT 2

Experiment 2 was designed to test these suggestions by presenting three display conditions with varying degrees of target-distractor similarity (i.e., target-distractor discriminability). It seems a reasonable assumption that the degree of response competition among locations that are in a "detected state" depends on the similarity between target and distractor. Thus, increasing the degree of target-distractor similarity should lead to a raise of criteria for optimal decision making.

On this hypothesis, the fact that Bashinski and Bacharach (1980) did not find any bias effects (not even in the detection data on their own) may be due to their display condition. In their study, the target was the only stimulus presented and the nontarget was a blank location. That is, target-nontarget discriminability was high (see Duncan, 1981) and the task required detection of a single luminance increment. Accordingly, the first display condition in Experiment 2, termed *single display*, was the same as that presented by Bashinski and Bacharach.

The second display condition, termed *multiple display 1*, was the same as in Experiment 1. For detection, the target T had to be discriminated from distractor crosses of the same size and luminance appearing simultaneously with it. The third condition, termed *multiple display 2*, required the discrimination of the target T from distractor Ts in different, that is, nontarget, orientations. The order of presentation of the three display conditions was randomized within blocks of trials so that the a priori decision criteria would be the same.

Method

Display. The only cuing condition presented in Experiment 2 was double cue 1 (double arrow with unequal arm lengths). On a signal trial in the single-display condition, the target T appeared in

one of the peripheral boxes and the other locations remained blank; on noise trials, all four boxes remained empty. On signal trials in the multiple-display 1 condition, the target T appeared in one of the boxes and distractor crosses of the same size and luminance appeared at the remaining locations; on noise trials, all four boxes contained distractor crosses. On signal trials in the multiple-display 2 condition, the target T appeared in one of the four peripheral boxes and three Ts in nontarget orientation appeared at the other locations; on noise trials, four nontarget Ts appeared in the periphery (one of the three possible nontarget Ts was randomly chosen and presented twice, i.e., at two locations). The tasks in the single-display and the multiple-display 1 conditions are consistent mapping tasks in terms of Schneider and Shiffrin (1977; Shiffrin & Schneider, 1977). Multiple display 2, however, is a varied mapping task, since target and distractor Ts changed roles from trial to trial. Unlike Experiment 1, the arrow cue in the center was displayed for 500 msec and the onset of the target display immediately followed the offset of the cue.

Design and procedure. The probabilities of signal and noise trials and the probabilities with which, on signal trials, the target occurred at the cued (V 1 and V 2) and uncued (I) locations were the same as in the double-cue 1 condition of Experiment 1. The three display conditions were presented in randomized order (i.e., randomized within the same blocks of trials). The threshold exposure times were determined on threshold trials presenting the multiple-display 1 neutral condition. The threshold exposure times introduced in the main experiment were the same for the three display conditions.

Observers. N.A. and H.M. participated in this experiment (I.P. was not available). Payment was £25 for completion of the experiment. N.A. was presented with 1,200 trials under each display condition (in three sessions); that is, the frequencies listed under the double-cue 1 condition in Table 1 were halved for a given display condition. H.M. completed six sessions, that is, 2,400 trials under each display condition. The average threshold exposure duration was 26.33 msec for N.A. and 21.67 msec for H.M.

Results

The data from Experiment 2 were analyzed in the same hierarchical fashion as in Experiment 1. Table 6 presents the $P(A)$ values for the detection data (D) and the joint detection-plus-localization data (D+L), individually for

each subject and separately for the three display conditions and for the valid, neutral, and invalid trials.

As in the previous experiment, $P(A)$ values are generally higher and differences in $P(A)$ between cue-validity conditions are generally smaller for the joint D+L data than for the detection data on their own. Note that all comparisons reported below are based on the joint D+L data (G tests between D+L data pooled over observers).

In the single-display condition, there are no differences in $P(A)$ between cue-validity conditions (D+L data). In the multiple-display 1 condition, $P(A)$ is slightly higher for the more likely one of the cued locations (i.e., for V 1 than for V 2) and higher for cued positions than for uncued locations and the neutral condition. However, only the difference between the most likely position and the uncued locations is reliable (V 1-I: $p < .05$). In the multiple-display 2 condition, the pattern is very similar. However, the difference between the most likely position and the uncued locations and that between the second most likely position and the uncued locations are more pronounced (V 1-I: $p < .01$; V 2-I: $p < .05$).

$P(A)$ (for all cue validity conditions) is significantly higher in the single-display condition than for uncued locations in the multiple-display 1 condition ($p < .05$ and beyond) and for the second most likely position, the uncued locations, and the neutral condition in the multiple-display 2 condition ($p < .05$ and beyond). Furthermore, $P(A)$ for uncued locations is higher in the multiple-display 1 condition than in the multiple-display 2 condition ($p < .05$). That is, in the multiple-display 2 condition, there is a marked loss in sensitivity for uncued locations relative to both the single- and the multiple-display 1 condition. And the magnitude of $P(A)$ differences between cued and uncued locations tends to increase with decreasing target-distractor discriminability (i.e., from single display through multiple display 1 to multiple display 2).

In the detection data, the observed $P(A)$ differences between valid and invalid trials within a given display condition occur because hit rates are higher for cued than for uncued locations; there are no false-alarm differences between valid and invalid trials since noise trials were pooled across cue validity conditions (see Table 7 for the mean hit and false-alarm rates). Among display conditions, however, false-alarm rates are lower for single than for multiple displays. For the most likely location, hit rates hardly differ among display conditions. For the remaining cue-validity conditions, however, hit rates decline with frequency of target occurrence and with target-distractor similarity.

In the joint D+L data, hit rates are reduced relative to the detection data. The losses, due to correct detections associated with localization failures, appear dependent on both cue validity and display condition: They increase from the most likely through the second most likely to the uncued locations and from the single display through the multiple-display 1 to the multiple-display 2 condition.

Table 6
Detection and Joint Detection-Plus-Localization Sensitivity:
Summary of $P(A)$ Values for Each Observer
Under Each Experimental Condition

Observer	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Single Display								
N.A.	.883	.876	.845	.844	.930	.918	.928	.928
H.M.	.892	.892	.847	.848	.926	.939	.922	.924
Mean	.887	.884	.846	.846	.928	.929	.925	.926
Multiple Display 1								
N.A.	.846	.836	.775	.776	.931	.895	.890	.881
H.M.	.861	.840	.782	.766	.921	.912	.900	.891
Mean	.854	.838	.779	.771	.926	.904	.895	.886
Multiple Display 2								
N.A.	.856	.810	.746	.728	.904	.896	.851	.825
H.M.	.844	.816	.759	.733	.903	.875	.865	.828
Mean	.850	.813	.753	.730	.904	.889	.858	.827

Note—V = valid, N = neutral, I = invalid.

Table 7

Detection and Joint Detection-Plus-Localization Hit (HR) and False-Alarm (FAR) Rates: Summary of Mean HR and Mean FAR Under Each Experimental Condition

	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Single Display								
HR	.832	.827	.770	.764	.805	.787	.758	.733
FAR	.231	.231	.258	.231	.072	.064	.065	.048
Multiple Display 1								
HR	.816	.802	.742	.671	.759	.721	.656	.590
FAR	.296	.296	.328	.296	.077	.089	.082	.066
Multiple Display 2								
HR	.810	.760	.707	.603	.736	.655	.594	.474
FAR	.292	.292	.375	.292	.071	.085	.094	.069

Note—V = valid, N = neutral, I = invalid.

False-alarm rates are slightly higher for valid than for invalid trials; that is, on noise trials, cued positions are about 1.25 times more likely to receive erroneous responses than is any particular one of the uncued locations.

Thus, partitioning of the total (i.e., detection) false-alarm rates into cue-validity conditions (see Appendix A) produces a general increase in $P(A)$ from the detection to the joint D+L analysis. This increase is more marked for invalid than for valid trials because fewer false alarms are assigned to uncued locations. As a result, costs plus benefits in sensitivity decrease from the detection to the joint D+L analysis. Note that, for the single-display condition, sensitivity differs between cue-validity conditions in the joint D+L analysis.

Figure 4 presents the normal-normal detection ROC curves, based on the certainty ratings, for each experimental condition; the values plotted are the means of the two observers. Again, D+L ROC curves are not presented because $P(A)$ differences between cue-validity conditions are too small for ROC curves to be clearly discernible.

The ROC curves show a linear relationship between the z -transformed hit and false-alarm rates (i.e., none of the chi-squared values based on the final ML slope estimates is significant). Slopes of the ROC curves vary systematically: For valid trials, the average slopes are about 0.67

and 0.70 for the detection and joint D+L ROC curves, respectively; for neutral trials, they are 0.82 and 1.02 and for invalid trials 0.84 and 0.85. Furthermore, slopes appear to be dependent on display condition: They are lower for single than for multiple displays (0.61 as compared to 0.81 in the D+L data). In the single-display condition, there are no significant slope differences between cue-validity conditions. In the multiple-display conditions, slope differences between valid 1 and invalid trials are reliable (D+L: $p < .05$).

The ML estimates of the likelihood ratios (betas) for the yes-no cutoff are shown in Table 8, individually for each observer and separately for the detection and the joint D+L data. The beta values are higher for the joint D+L responses than for the detection responses on their own. Furthermore, the beta values (in particular the D+L values) are lower for valid than for neutral and invalid trials, in both the multiple- and single-display conditions. The beta values are consistently (i.e., across observers and cue-validity conditions) higher for multiple than for single displays, but do not differ much between the multiple-display 1 and 2 conditions (D+L data).

Discussion

With regard to the sensitivity parameter $P(A)$, the results of Experiment 2 for the multiple-display 1 and 2 conditions replicate the findings for the double-cue 1 condition in Experiment 1. Both multiple-display conditions show gains and losses associated with the preferential allocation of attention to the cued locations. For the cued positions, there are no reliable differences in $P(A)$ between the multiple-display conditions; for the uncued locations, $P(A)$ is reduced in the multiple-display 2 condition in comparison with the multiple-display 1 condition.

$P(A)$ is generally higher in the single-display condition than in the multiple-display conditions. However, there are no differences between cue-validity conditions (D+L data); that is, there are no costs plus benefits associated with the preferential allocation of attention to the cued locations.

The fact that there are gains and losses in sensitivity for cued and uncued locations in the multiple-display con-

Figure 4. Detection ROC curves for the single-display (left), multiple-display 1 (middle), and multiple-display 2 (right) conditions and for the valid 1 (open triangles), valid 2 (filled triangles), neutral (open circles), and invalid trials (open squares).

Table 8

Detection and Joint Detection-Plus-Localization Bias: Summary of the Likelihood Ratios (Beta) at the Yes-No Cutoff for Each Observer Under Each Experimental Condition

Observer	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Single Display								
N.A.	.414	.416	.657	.555	1.185	1.219	1.949	2.132
H.M.	.459	.453	.695	.662	1.073	1.661	1.744	2.373
Mean	.436	.434	.676	.608	1.129	1.440	1.847	2.253
Multiple Display 1								
N.A.	.541	.589	.817	1.005	1.716	1.514	2.793	2.875
H.M.	.547	.596	.736	.934	1.643	1.855	2.143	3.087
Mean	.544	.593	.777	.969	1.680	1.685	2.468	2.981
Multiple Display 2								
N.A.	.537	.741	.868	1.096	1.332	2.207	2.175	2.591
H.M.	.571	.699	.932	1.082	1.571	1.986	2.293	2.760
Mean	.554	.720	.900	1.089	1.452	2.097	2.234	2.680

Note—V = valid, N = neutral, I = invalid.

ditions indicates that letter detection is a limited-capacity process. In this respect, consistent (i.e., multiple-display 1) and varied (i.e., multiple-display 2) mapping tasks do not differ essentially. However, the magnitude of costs plus benefits differs between multiple-display conditions. In other words, there are differences between consistent and varied mapping tasks in the efficiency with which the limited processing capacity is used (see also M. L. Shaw, 1984).

However, the failure to find costs plus benefits in sensitivity for cued and uncued locations in the single-display condition indicates that detection of a luminance increment in an otherwise empty field is not capacity limited. That is, luminance detection appears to be "radically" different from letter detection. This agrees with M. L. Shaw's (1984) findings for the display size effect (see also Cohn & Lasley, 1974).

Note that there appears to be a discrepancy between the present study and that of Shaw (1984), on the one hand, and Bashinski and Bacharach's (1980) experiment, on the other. Using only two locations, Bashinski and Bacharach reported $P(A)$ differences between valid and invalid trials of 0.168 for detection and 0.263 for joint D+L. In the present experiment, the corresponding values are 0.039 and 0.002 (average differences between V 1 and I in the single-display condition). In Bashinski and Bacharach's study, the increase in $P(A)$ between the detection and the joint D+L analysis is an artifact of their method of calculating D+L false-alarm rates (see Appendix A for details). It is possible that one might fail to find costs plus benefits in Bashinski and Bacharach's luminance-detection data if they were analyzed according to the joint D+L ROC procedure suggested in Appendix A.

For the most likely location (V 1), the slight loss in $P(A)$ for multiple relative to single displays is due to localization failures (i.e., misses). Similarly, the more marked losses for the less likely positions (V 2 and I) are largely accounted for by localization failures, but some-

what higher false-alarm rates associated with these positions in multiple than in single displays contribute to the reduction in sensitivity. (Note that in the D+L data, the proportion of localization failures among misses, i.e., of targets erroneously "detected" at nontarget locations, is 12.9% for the single-display and 23.8% and 27.1% for the multiple-display 1 and 2 conditions, respectively; these differences between display conditions are the more marked, the less likely a particular location.)

This pattern indicates greater sample strength variability for less likely locations in multiple displays. That is, the strength value associated with one of the less likely locations (V 2 and I) is more likely to exceed the value associated with the most likely position (V 1), resulting in a failure to choose that position for the localization response. The same argument provides an explanation for the finding that false-alarm rates for the most likely position are not higher for multiple than for single displays. Differences between the multiple-display 1 and 2 conditions can be explained along similar lines.

With regard to the bias effects, the likelihood ratios increase with decreasing cue validity in the single-display condition as well as in the multiple-display 1 and 2 conditions. These increased positive and negative biases for cued and uncued locations, respectively, in the single-display condition (especially in the D+L data) are at variance with the findings of Bashinski and Bacharach (1980), who reported costs plus benefits in sensitivity without bias changes. However, Bashinski and Bacharach's beta estimates were based on a flawed calculation of false-alarm rates (D+L analysis; see Appendix A), and, furthermore, they did not take into account variance differences between noise and signal-plus-noise distributions (i.e., the fact that the slopes of the normal-normal ROC curves for cued locations were different from 1.0).

Response biases are generally (i.e., across cue-validity conditions) more conservative in the multiple-display conditions than in the single-display condition. Since the three display conditions were presented in randomized order (within blocks of trials), decision criteria could not be differentially preset. Thus, decision criteria set a priori according to the probabilistic information provided by the arrow cue must have been readjusted on the basis of the information obtained during the presentation of the target display.

The more conservative biases in the multiple-display conditions are consistent with the assumption that target-distractor discriminability for a particular location is a function both of the similarity between target and nontarget and of the amount of processing capacity allocated to this position. The single-luminance increment on which the discrimination between target and nontarget is based in the single-display condition can be picked up more efficiently, that is, presumably without capacity limitation, than can the information required for target-distractor discrimination in the multiple-display conditions. Thus, in the multiple-display conditions, there is greater variability in sample strength for the less likely locations, that

is, an increase in the probability that the strength values associated with nontarget locations equal or exceed the value of the actual target position (signal trials) or exceed some criterion value (noise trials). The resulting response competition requires decision criteria to be raised for optimal decision making.

The same argument predicts more conservative decision criteria for the multiple-display 2 (varied mapping) condition than for the multiple-display 1 (consistent mapping) condition. But note that the difference does not appear reliable (at least in the D+L data). This indicates that the increase in target-nontarget similarity from consistent- to varied-mapping multiple displays is less fundamental than that from single to multiple displays.

GENERAL DISCUSSION

With regard to the question raised by Posner et al. (1980), Experiment 1 demonstrates that attention can be effectively divided between two likely target locations. Recently, Eriksen and Yeh (1985) argued against such a suggestion on the basis of a letter-detection experiment in which the most likely position was directly cued by a bar marker and the second most likely location was always diagonally opposite to the cued position. Eriksen and Yeh found benefits in response speed only for the cued position; for the second most likely location RTs were slower than in neutral control conditions but faster than for the uncued locations. When the probability of target occurrence at the cued location was increased, at the expense of the second most likely and uncued locations, there was a further divergence of RT functions. This pattern of results led Eriksen and Yeh to suggest that search strategies alternated, across trials, between a focused (serial) and a diffuse (parallel) mode of processing, with their mixture being determined by cue validity.

However, the type of spatial cue presented in this experiment may have interfered with effective attention sharing. Jonides (1981; see also Yantis & Jonides, 1984) and Müller and Rabbitt (1987) have shown that direct (peripheral) cues produce rapid and involuntary deployment of attention to the cued position, and that this process cannot be suppressed substantially even when cue validity is neutral. Thus, the failure of Eriksen and Yeh to find benefits in RT for the location diagonally opposite to the bar marker may be attributable to the dominance of "automatic" processes triggered by external properties of the cue over "controlled" processes, that is, attention sharing, based on its implicit spatial information.

With regard to the question raised by M. L. Shaw (1984), Experiment 2 does support the conclusion that the explanation of the priming effect differs "radically" between tasks requiring the detection of letters and those requiring the detection of luminance increments; the differences appear more qualitative than just quantitative in nature. This conclusion is at variance with Bashinski and Bacharach (1980).

Thus, for spatial cuing experiments measuring simple RT to luminance increments (e.g., Posner et al., 1980), the present data (as well as those of M. L. Shaw, 1984) would suggest that the benefits and costs in response speed typically found for cued and uncued locations are produced by differential setting of response thresholds, that is, low for cued and high for uncued locations. But note that the present experiments do not rule out that costs and benefits in simple RT at very short intervals (about 100 msec) following the onset of peripheral cues ("automatic" orienting) are caused by "sensitivity" rather than (mere) "bias" changes (see Müller, 1986). However, it seems at least questionable whether the simple luminance detection task is an "ideal model task" (Posner et al., 1980) for investigating the effects of spatial attention.

Given that costs plus benefits in simple RT to luminance increments reflect the differential setting of response thresholds for cued and uncued locations, the question arises why Posner et al. (1980; Experiment 5) failed to find consistent RT advantages for the second most likely location. This may be due to the fact that this position was not cued on every trial, but was fixed throughout a block.

To test this, Müller, Astell, and Findlay (1985) repeated Experiment 1 with the simple RT task used by Posner et al. (1980). That is, the task required a simple speeded response to a suprathreshold luminance increment, and this was the only stimulus presented, that is, nontarget locations remained blank ($\frac{2}{3}$ target trials and $\frac{1}{3}$ catch trials). Table 9 presents the mean RTs and their standard deviations (6 observers). Müller et al. (1985) found reliable benefits, relative to the neutral baseline, for cued positions and significant costs for the uncued locations, both under single- and double-cuing conditions [main effect of cue validity: $F(2,10) = 10.64, p < .005$]. Thus, it appears that consistent benefits for a second cued location can be obtained in a simple RT task when this position is cued on every trial (rather than blocked). This is consistent with Posner et al.'s (1980; Experiment 1) own finding that, with blocked designs, subjects apparently "[did] not continue to set themselves for the position in space at which the target was most expected" (p. 163).

Table 9
Simple RTs in Milliseconds: Summary of Mean Values and Their Standard Deviations (SDs) Under Each Experimental Condition

	V 1	V 2	N	I
Single Cue				
Mean	246		257	263
SD	33		31	39
Double Cue 1				
Mean	248	255	260	269
SD	22	21	32	28
Double Cue 2				
Mean	251		264	274
SD	24		27	30

Note—V = valid, N = neutral, I = invalid.

The results of the present experiments have some implications for the notion of "automatic detection" (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). According to Shiffrin and Schneider, one should expect that extensive practice with consistent-mapping multiple displays (multiple-display 1 condition) reduces the size of the priming effect as the demands on the limited processing capacity are reduced and finally bypassed. Experiments 1 and 2, however, demonstrate capacity limitation for consistent-mapping multiple displays despite the fact that the observers had all undergone extensive training. As can be seen from Table 2 (0.75-threshold exposure times for Experiment 1), display duration decreased from about 38 msec in the first session to 26 msec in the sixth session; that is, exposure durations had to be continuously readjusted to compensate for practice (automatization) effects (see also Kleiss & Lane, 1986, and Prinzmetal & Banks, 1983). However, the decrease in exposure durations was not associated with reductions of benefits and costs associated with the preferential allocation of attention to the cued locations. Thus, training with a consistent mapping task appears to produce an increase in the efficiency with which the available processing capacity is used rather than a bypassing of limited-capacity processes.

M. L. Shaw's (1980, 1984; M. L. Shaw et al., 1983) "sample size" model provides a good account for the priming effects found in the present experiments. This model is based on the assumption that a finite resource (attention, processing capacity, search effort) is divided among the input locations. The processing capacity allocated to a particular source can influence the internal stimulus representation generated in the coding stage. Since, in this model, the substages that compose the coding stage are considered together, it may not be incompatible with "late selection" theories of attention (e.g., Duncan, 1980b, 1981, 1985; Hoffman, 1978, 1979).

These theories generally assume that there is a preselective, parallel, and relatively capacity-free stage of processing deriving simple stimulus features and also aspects of meaning that must be passed on to a second stage in order to be preserved and form a reportable perception. The transfer from the first to the second stage is accomplished by a limited-capacity system that involves an "attention response" to the attributes defining the stimulus to be selected; this transfer is generally presumed to be serial in nature. Duncan (1981) argues that the finding of enhanced sensitivity for spatial locations known in advance cannot be taken as evidence for the idea that attention has a facilitatory effect on the early stages of perceptual analysis. Rather, selection of first-stage representations on the basis of spatial location is but one of several possible selection schedules which may, however, differ in efficiency.

For the double-cue conditions in the present experiments, the selection schedule implemented might have been one of transferring first-stage representations in a spatial order determined by the arrow cue until they were

terminated by masking. However, such a selection schedule would not predict the decrease in sensitivity for lower priority locations (i.e., the second cued and the uncued locations) from the multiple-display 1 condition to the multiple-display 2 condition, as observed in Experiment 2 (see also Tsal, 1983b). This decrease is expected on the basis of the assumption that reduced target-distractor discriminability increases the likelihood that nontarget stimuli compete for and gain access to a limited-capacity system. This finding appears incompatible with the assumption that display elements are fully identified in parallel and without capacity limitation at a first stage of encoding, and it may point to at least some role of attention for influencing the "quality" of information extracted in simultaneous processing of multiple input sources.

REFERENCES

- ABADI, R. (1980). *ACS eye movement system EM 130*. (Available from A. Abadi, 6 Whitworth Street, Manchester M1 3QW, England)
- BASHINSKI, H. S., & BACHARACH, V. R. (1980). Enhancement of perceptual sensitivity as the result of selectively attending to spatial locations. *Perception & Psychophysics*, **28**, 241-248.
- COHN, T. E., & LASLEY, D. J. (1974). Detectability of a luminance increment: Effect of spatial uncertainty. *Journal of the Optical Society of America*, **64**, 1715-1719.
- DORFMAN, D. D., & ALF, E. (1969). Maximum-likelihood estimation of parameters of signal-detection theory and determination of confidence intervals: Rating method data. *Journal of Mathematical Psychology*, **6**, 487-496.
- DUNCAN, J. (1980a). The demonstration of capacity limitation. *Cognitive Psychology*, **12**, 75-96.
- DUNCAN, J. (1980b). The locus of interference in the perception of simultaneous stimuli. *Psychological Review*, **87**, 272-300.
- DUNCAN, J. (1981). Directing attention in the visual field. *Perception & Psychophysics*, **30**, 90-93.
- DUNCAN, J. (1985). Visual search and visual attention. In M. I. Posner & O. S. M. Marin (Eds.), *Attention and performance XI*. Hillsdale, NJ: Erlbaum.
- ERIKSEN, C. W., & YEH, Y.-Y. (1985). Allocation of attention in the visual field. *Journal of Experimental Psychology: Human Perception & Performance*, **11**, 583-597.
- ESTES, W. K., & TAYLOR, H. A. (1964). A detection method and probabilistic models for assessing information processing from brief visual displays. *Proceedings of the National Academy of Sciences*, **52**, 446-454.
- ESTES, W. K., & WESSEL, D. L. (1966). Reaction time in relation to display size and correctness of response in forced-choice visual signal detection. *Perception & Psychophysics*, **1**, 369-373.
- FINDLAY, J. M. (1978). Estimates on probability functions: A more virulent PEST. *Perception & Psychophysics*, **23**, 181-185.
- GOUREVITCH, V., & GALANTER, E. (1967). A significance test for one parameter isosensitivity functions. *Psychometrika*, **32**, 25-33.
- GREEN, D. M., & LUCE, R. D. (1973). Speed-accuracy trade-off in auditory detection. In S. Kornblum (Ed.), *Attention and performance IV*. New York: Academic Press.
- GREEN, D. M., & SWETS, J. A. (1966). *Signal detection theory and psychophysics*. New York: Wiley.
- HOFFMAN, J. E. (1978). Search through a sequentially presented display. *Perception & Psychophysics*, **23**, 1-11.
- HOFFMAN, J. E. (1979). A two-stage model of visual search. *Perception & Psychophysics*, **25**, 319-327.
- JONIDES, J. (1981). Voluntary versus automatic control over the mind's eye. In J. Long & A. Baddeley (Eds.), *Attention and performance IX*. Hillsdale, NJ: Erlbaum.

- JONIDES, J., & MACK, R. (1984). On the cost and benefit of cost and benefit. *Psychological Bulletin*, **96**, 29-44.
- KINCHLA, R. A. (1977). The role of structural redundancy in the perception of visual targets. *Perception & Psychophysics*, **22**, 19-30.
- KLEISS, J. A., & LANE, D. M. (1986). Locus and persistence of capacity limitations in visual information processing. *Journal of Experimental Psychology: Human Perception & Performance*, **12**, 200-210.
- KOWLER, E., & STEINMAN, R. M. (1979a). The effect of expectations on slow oculomotor control: I. Periodic target steps. *Vision Research*, **19**, 619-632.
- KOWLER, E., & STEINMAN, R. M. (1979b). The effect of expectations on slow oculomotor control: II. Single target displacements. *Vision Research*, **19**, 633-646.
- LUCE, R. D. (1977). Thurstone's discriminational processes fifty years later. *Psychometrika*, **42**, 461-489.
- MÜLLER, H. J. (1986). *The effect of selective spatial attention on peripheral discrimination thresholds*. Unpublished doctoral dissertation, University of Durham, England.
- MÜLLER, H. J., & FINDLAY, J. M. (1987). *The effect of visual attention on peripheral discrimination thresholds in single and multiple element displays*. Manuscript submitted for publication.
- MÜLLER, H. J., ASTELL, N. G., & FINDLAY, J. M. (1985). *Division of attention among spatial locations: Effects on simple RT*. Unpublished manuscript, University of Durham, England.
- MÜLLER, H. J., & RABBITT, P. M. A. (1987). *Internally and externally guided orienting of visual attention: Time course of activation and resistance to interruption*. Manuscript submitted for publication.
- POSNER, M. I., NISSEN, M. J., & OGDEN, W. C. (1978). Attended and unattended processing modes: The role of set for spatial locations. In H. L. Pick & E. J. Saltzman (Eds.), *Modes of perceiving and processing information*. Hillsdale, NJ: Erlbaum.
- POSNER, M. I., & SNYDER, C. R. R. (1975). Attention and cognitive control. In R. L. Solso (Ed.), *Information processing and cognition: The Loyola Symposium*. Hillsdale, NJ: Erlbaum.
- POSNER, M. I., SNYDER, C. R. R., & DAVIDSON, B. J. (1980). Attention and the detection of signals. *Journal of Experimental Psychology: General*, **109**, 160-174.
- PRINZMETAL, W., & BANKS, W. P. (1983). Perceptual acuity limits in visual detection and search. *Bulletin of the Psychonomic Society*, **21**, 263-266.
- SCHNEIDER, W., & SHIFFRIN, R. M. (1977). Controlled and automatic human information processing: I. Detection, search, and attention. *Psychological Review*, **84**, 1-66.
- SHAW, M. L. (1978). A capacity allocation model for reaction time. *Journal of Experimental Psychology: Human Perception & Performance*, **4**, 586-598.
- SHAW, M. L. (1980). Identifying attentional and decision-making components in information processing. In R. S. Nickerson (Ed.), *Attention and performance VIII*. Hillsdale, NJ: Erlbaum.
- SHAW, M. L. (1982). Attending to multiple sources of information: 1. The integration of information in decision making. *Cognitive Psychology*, **14**, 353-409.
- SHAW, M. L. (1984). Division of attention among spatial locations: A fundamental difference between detection of letters and detection of luminance increments. In H. Bouma & D. G. Bouwhuis (Eds.), *Attention and performance X*. Hillsdale, NJ: Erlbaum.
- SHAW, M. L., MULLIGAN, R. M., & STONE, L. D. (1983). Two-state versus continuous-state stimulus representations: A test based on attentional constraints. *Perception & Psychophysics*, **33**, 338-354.
- SHAW, M. L., & SHAW, P. (1977). Optimal allocation of cognitive resources to spatial locations. *Journal of Experimental Psychology: Human Perception & Performance*, **3**, 201-211.
- SHEPHERD, M. (1984). EMDISP: A visual display system with digital and analogue sampling. *Behavior Research Methods, Instruments, & Computers*, **16**, 297-302.
- SHIFFRIN, R. M., & SCHNEIDER, W. (1977). Controlled and automatic human information processing: II. Perceptual learning, automatic attending, and a general theory. *Psychological Review*, **84**, 127-190.
- SIMPSON, A. J., & FITTER, M. J. (1973). What is the best index of detectability? *Psychological Review*, **80**, 481-488.
- SPELTING, G. (1984). A unified theory of attention and signal detection. In R. Parasuraman & D. R. Davies (Eds.), *Varieties of attention*. New York: Academic Press.
- SPELTING, G., & REEVES, A. (1980). Measuring the reaction time of an unobservable response: A shift of visual attention. In R. S. Nickerson (Ed.), *Attention and performance VIII*. Hillsdale, NJ: Erlbaum.
- TSAL, Y. (1983a). Movements of attention across the visual field. *Journal of Experimental Psychology: Human Perception & Performance*, **9**, 523-530.
- TSAL, Y. (1983b). On interpreting the effects of location preknowledge: A critique of Duncan. *Perception & Psychophysics*, **34**, 297-298.
- YANTIS, S., & JONIDES, J. (1984). Abrupt visual onsets and selective attention: Evidence from visual search. *Journal of Experimental Psychology: Human Perception & Performance*, **10**, 601-621.

APPENDIX A

It is not immediately clear from Bashinski and Bacharach (1980) how they calculated the false-alarm rates for the joint analysis of D+L responses. Bashinski and Bacharach agree (J. Duncan, personal communication, April 1985) that their procedure was as follows:

1. Let the total number of "noise," that is, nontarget, trials be N .
2. Across these N trials, let the number of false alarms assigned to, that is, erroneously localized at, the cued position be a and the number assigned to the uncued location be b .
3. For "signal," that is, target, trials with an arrow cue, let the probability that the target occurred at the cued position be p and the probability that the target occurred at the uncued location be $q = 1 - p$.
4. Bashinski and Bacharach then took the false-alarm rate for the cued position to be $a/(pN)$ and the false-alarm rate for the uncued location to be $b/(qN)$.

Restated, Bashinski and Bacharach performed an imaginary division of the noise trials into a proportion p for the cued position and a proportion $1 - p$ for the uncued location. That Bashinski and Bacharach (1980) performed such a division is also evident from their article: The false-alarm rates reported for the joint D+L analysis—0.17 for the cued and 0.37 for the uncued position (p. 245)—add up to a higher total false-alarm rate than that reported for the detection analysis—0.22 collapsed across cued and uncued locations (p. 244).

This procedure has one problem: It produces an advantage in the sensitivity parameter $P(A)$ for cued over uncued locations. Suppose, for example, that the a priori probabilities of a signal occurring at the cued and uncued locations are 0.8 and 0.2, respectively, as in Bashinski and Bacharach's experiment. Then, on the basis of Bashinski and Bacharach's procedure, the expected ratio between hit and false-alarm rates is 0.4:0.5 for the cued and 0.1:0.5 for the uncued position. That is, the ROC curves for the cued and the uncued locations, based on the expected hit and false-alarm rates, lie beneath, rather than on, the main diagonal (chance performance) and $P(A)$ is smaller for uncued than for cued locations.

There is a simple solution to this problem: The numbers a and b , that is, the false alarms assigned to the cued and uncued locations, should both be divided by N since they are based on the total N trials (otherwise, false-alarm rates could exceed 1.0!). Furthermore, if there is more than one uncued location, b must be divided by the number of uncued positions to give the false-alarm rate per location. Thus, for example, in the single-cue condition of Experiment 1, the false-alarm rate for the cued position is given by a/N and that for the three uncued locations by $(b/3)/N$.

Note that there is another solution: to count as a false alarm any response on signal trials on which the observer erroneously detects and localizes a target in a nontarget position. This is problematic, however, since any advantage in D+L hit rates for cued positions would result in reduced false-alarm rates for uncued locations.

It is important to note that recalculation of the false-alarm rates in Bashinski and Bacharach's experiment according to the solution set out above radically changes their conclusion concerning the relative accuracy for cued and uncued locations. Working backward from the joint D+L false-alarm rates reported by Bashinski and Bacharach, the false-alarm rates recalculated for cued and uncued locations are 0.14 and 0.07, respectively. That is, the false-alarm rate is twice, rather than half, as high for cued as for uncued locations. And, as a result, costs plus benefits in joint D+L sensitivity are smaller, rather than greater, than detection sensitivity on its own. It is not possible to recalculate $P(A)$, since Bashinski and Bacharach do not report joint D+L hit rates for cued and uncued locations. It cannot be ruled out, however, that the recalculated $P(A)$ values do not differ between valid and invalid trials.

Note that, in the present experiments, joint D+L false-alarm rates were calculated according to the solution proposed above. Note further that, for the joint D+L ROC curves, frequencies of misses and of correct rejections were pooled across "no, uncertain" and "no, certain" response categories.

APPENDIX B

There is evidence that eye movements cannot account for the pattern of sensitivity effects in Experiment 1.

Experiment 1a showed that when trials on which saccadic eye movements occurred are excluded, the pattern of results is very similar to that of Experiment 1, in which eye movements were not monitored. Of the 3,600 trials in Experiment 1a, the number of saccades detected was 16, all occurring before display onset (i.e., there was no evidence of postdisplay saccades within 150 msec after display onset). Thus, saccade probability was less than 0.005. However, there was evidence of slow fixation drifts prior to display onset.

Table 10 presents the size and direction of these drifts, separately, for neutral trials, for double-cuing trials with arrows pointing to different sides, and for single-cuing and double-cuing trials with arrows pointing to the same side. The table shows that for "neutral" and "different" trials, the distributions are approximately symmetric. However, for "same" trials, the distributions are skewed to the side (left or right) of the location(s) to which the single arrow (single-cuing condition) or both arrows (double-cuing conditions) were pointing; that is, approximately 60% of the shifts were in the cued, that is, "attended," direction. The mean and variance of the combined "same/left"

and "same/right" distribution are about equal to those of the "neutral" and "different" distributions. This suggests that fixation drifts in some attended direction also occurred on neutral trials and on trials with double arrows pointing to locations on different sides. Note, however, that the size of these drifts (horizontal components) was small: Approximately 75% were smaller than 0.375° and approximately 95% were smaller than 0.625° . These findings appear to be consistent with Kowler and Steinman's (1979a, 1979b) work on slow fixation drifts in the attended direction in tasks that required eye tracking of repetitive target steps: drifts occurred as early as 350 msec before expected target steps and with velocities up to $0.5^\circ/\text{sec}$.

Thus, even though it seems certain that such drifts occurred in Experiment 1, because of their small size they should have had little influence on sensitivity parameters. Shifts of greater size toward the most likely position(s) should show up in contrasts between equally likely display locations adjacent and opposite to the eye position in the same way as outright saccadic eye movements. Suppose, for instance, that in the single-cuing condition an observer moved his eyes toward the cued location before target onset. This would have decreased the retinal eccentricity of the cued position and increased it for the uncued locations. However, the increase would have been relatively greater for the diagonally opposite position than for the adjacent locations. For example, if the distance of the eye from the cued position decreased from 4.4° to 2.0° , that for the opposite position increased from 4.4° to 6.8° , while that for the two adjacent locations increased from 4.4° to only 5.0° . To assess the effect of such changes in retinal eccentricity, we conducted an experiment (Experiment 1b) that varied the distances of cued and (adjacent and opposite) uncued locations systematically.

In Experiment 1b, the observer viewed the fixation dot in the center of the display, expecting the target event to occur at some location on the positive or negative diagonal. Unlike Experiment 1, however, display locations were not marked by boxes and their distances from the display center were not fixed, but varied from trial to trial. Three seconds after the beginning of a trial, a single box (i.e., single peripheral cue) appeared for 50 msec, marking the likely target position (uncued locations were not marked). On neutral trials, 50-msec boxes occurred at all four display locations. The distance of the cued location from the display center could take the following value: 4.4° , 4.0° , 3.0° , or 2.0° . Thus, the distance of the diagonally opposite position was 4.4° , 4.8° , 5.8° , or 6.8° , and that of the adjacent locations, 4.4° , 4.4° , 4.6° , or 5.0° . On neutral trials, display locations were equidistant from the center (4.4°). The interval between cue and target onset (stimulus onset asynchrony) was 75 msec, that is, too short for eye movements to occur. Target and distractor exposure was terminated by contour masks.

One observer, H.M., was presented with 2,008 cued (i.e., valid, invalid, and noise) trials under each of the four eccen-

Table 10
Size (in Degrees of Visual Angle) and Direction (Left-Right) of Slow Fixation Drifts: Frequencies, Separately, for Neutral Trials (Central Cross), for Double-Cue Trials with Arrows Pointing to Different Sides, and for Single-Cue and Double-Cue Trials with Arrows Pointing to the Same Side

Trials	Left				Center			Right			
	1.25°	1.00°	0.75°	0.50°	0.25°	0.00°	0.25°	0.50°	0.75°	1.00°	1.25°
Neutral		4	14	66	122	186	119	52	9	4	
Different	5	8	62	133	285	390	282	115	51	10	3
Same/Left		11	20	108	218	274	156	46	7		
Same/Right			11	54	143	263	221	107	29	10	2

tricity conditions plus 768 neutral (signal and noise) trials, that is, 8,800 trials in all (eight 4-h sessions). Eccentricity conditions were presented in randomized order (within the same block of trials). The average target exposure time, determined at the beginning of each session for neutral threshold trials, was 35.5 msec. The experiment was conducted under normal room lighting (fluorescent overhead light). All other methodological details were the same as in Experiment 1.

Table 11 presents the $P(A)$ values for cued and for adjacent and opposite uncued locations and for each retinal eccentricity condition. $P(A)$ for the neutral condition (4.4°) was 0.765 for the detection data and 0.888 for the joint D+L data. Costs plus benefits in sensitivity explicable in terms of "attention," rather than retinal eccentricity, appear small in Experiment 1b (see $P(A)$ for the 4.4° eccentricity in the D+L data). Presumably, this is due to forward masking produced by the peripheral cue. Forward masking reduces gains for the cued position and for the neutral condition, relative to the uncued locations.

Note that the size of the attentional effect in Experiment 1b is irrelevant with respect to the comparison between adjacent and opposite uncued locations as a function of retinal eccentricity. Table 11 shows that when the eye is equidistant from all four display locations, there are no $P(A)$ differences between adjacent and opposite uncued locations. However, such differences occur when the eye moves closer to the cued location. At an eye-cued location distance of 2.0° , $P(A)$ for opposite locations (6.8° distance) is 0.051 (D+L data) lower than $P(A)$ for adjacent locations (5.0° distance). Linear regression analyses for cued and opposite locations showed the retinal eccentricity effect to be approximately 0.03 $P(A)$ units per degree of visual angle.

Thus, if observers in Experiment 1 had made eye movements in the direction of the cued position larger than about 1° , this should show up in differences, in the single-cue condition, between adjacent and opposite uncued locations. However, as can be seen from Tables 12 and 13, which present the corresponding "adjacent-opposite" contrasts individually for each observer in Experiment 1, there is no evidence for such differences. (Note that the values for H.M. in Experiment 1a—eye-movement monitoring—are indicated by an asterisk; H.M.'s values for Experiments 1 and 1a were averaged before calculating the group means.)

Similar arguments apply to the double-cuing conditions. Suppose that an observer made an eye movement to the (potentially) most informative region of the display. Thus, with double arrows pointing to adjacent locations (90° angle between arrows), the optimal strategy would be to move to the point in the middle between the cued positions. However, with double-arrow

Table 11
Detection and Detection-Plus-Localization Sensitivity: Summary of $P(A)$ Values for Cued and for Adjacent and Opposite Uncued Locations Under Each Retinal Eccentricity Condition

Degrees	Detection			Detection + Localization		
	Cued	Adjacent	Opposite	Cued	Adjacent	Opposite
4.4	.825	.702	.705	.903	.888	.886
4.0	.842	.708	.702	.911	.884	.878
3.0	.865	.695	.667	.936	.878	.849
2.0	.896	.688	.637	.959	.864	.813

Note—Distance of the central fixation dot from the cued location is in degrees of visual angle.

Table 12
Detection Sensitivity: Summary of $P(A)$ Values for Each Observer Under Each Experimental Condition

Observer	Adjacent			Opposite		
	V 1	V 2	I	V 1	V 2	I
Single Cue						
I.P.			.780			.776
N.A.			.788			.774
H.M.			.773			.790
H.M.*			.769			.774
Mean			.780			.777
Double Cue 1						
I.P.	.886	.867	.786	.911	.861	.758
N.A.	.914	.896	.777	.898	.890	.795
H.M.	.934	.906	.764	.951	.922	.784
H.M.*	.880	.867	.762	.885	.857	.762
Mean	.902	.883	.775	.909	.880	.775
Double Cue 2						
I.P.	.866	.874	.790	.871	.865	.744
N.A.	.902	.915	.767	.910	.902	.771
H.M.	.913	.922	.749	.938	.923	.802
H.M.*	.882	.868	.758	.877	.874	.750
Mean	.889	.895	.770	.896	.889	.764

Note—V = valid I, = invalid. *Eye movements monitored.

Table 13
Joint Detection-Plus-Localization Sensitivity: Summary of $P(A)$ Values for Each Observer Under Each Experimental Condition

Observer	Adjacent			Opposite		
	V 1	V 2	I	V 1	V 2	I
Single Cue						
I.P.			.898			.896
N.A.			.915			.901
H.M.			.888			.890
H.M.*			.901			.902
Mean			.903			.898
Double Cue 1						
I.P.	.923	.914	.898	.939	.921	.911
N.A.	.935	.930	.894	.925	.940	.886
H.M.	.947	.922	.904	.963	.935	.894
H.M.*	.920	.905	.890	.910	.891	.864
Mean	.931	.919	.896	.934	.925	.892
Double Cue 2						
I.P.	.917	.925	.901	.925	.911	.868
N.A.	.924	.938	.863	.924	.930	.891
H.M.	.920	.937	.899	.932	.927	.910
H.M.*	.896	.903	.870	.899	.901	.869
Mean	.916	.928	.883	.922	.918	.883

Note—V = valid, I = invalid. *Eye movements monitored.

indicators pointing to opposite locations (180° angle between arrows), the optimal strategy would be to maintain fixation at the display center. Thus, the distance from eye fixation of the uncued locations would have been 6.9° for adjacent cues but 4.4° for opposite cues. That is, sensitivity for uncued locations should be lower for adjacent than for opposite cues, and sensitivity for cued locations should be lower for opposite than for adjacent cues. However, in neither the double-cue 1 condition nor the double-cue 2 condition of Experiment 1 are there any such differences in the $P(A)$ data (see Tables 12 and 13).

Alternatively, an observer could have made an eye movement to the most likely location (i.e., to one of the two equally likely signal locations in the double-cue 2 condition). One should expect, then, that in the double-cue 1 condition (with different signal probabilities assigned to cued locations), sensitivity for the second most likely position would be lower for opposite than for adjacent cues (an 8.8° as opposed to 6.2° distance from eye fixation of most likely location). And in the double-cue 2 condition (with equal signal probabilities assigned to cued locations), sensitivity for both cued positions would be lower for opposite than for adjacent cues. However, again there is no evidence for such differences in the $P(A)$ data of Experiment 1 (see Tables 12 and 13).

Rather, sensitivity parameters are quite independent of the angle between arrow indicators (double-cuing conditions) and of the relationship of uncued locations with respect to the cued position (single-cuing condition). In other words, there is no evidence for large and strategic eye movements in the data of Experiment 1.

APPENDIX C

The effectiveness of the contour masks to terminate the display was tested in a further experiment (Experiment 1c). Contour masks might leave some useful information accessible even after display offset, since the visible persistence of the P31 phosphor, with which the monitor was equipped, is rather long when the laboratory is dimly illuminated. A closer look at some display characteristics provides a hypothesis about the stimulus attribute which might persist after mask onset. Contour masks were squares of the same size as that of target Ts and distractor crosses (0.25°). As can be seen from Figure 5, target Ts and distractor

Figure 5. Contour (left) and pattern (right) masks at target and nontarget locations.

Table 14

Detection and Joint Detection-Plus-Localization Sensitivity: Summary of $P(A)$ Values Under Each Experimental Condition

Mask	Detection				Detection + Localization			
	V 1	V 2	N	I	V 1	V 2	N	I
Contour	.895	.874	.794	.776	.931	.900	.898	.878
Pattern	.891	.869	.783	.758	.924	.901	.896	.879

Note—V = valid, N = neutral, I = invalid.

crosses were composed of two bars, each of which consisted of seven illuminated spots (i.e., they shared one spot at their intersection). Similarly, the bars of which contour masks were composed consisted of seven spots (i.e., they shared one spot at each corner). Thus, at display offset and mask onset, the outer spots of which target Ts and distractor crosses were composed were superseded by the masking contours while the spots inside the masks decayed. However, the number of spots decaying inside the masking contours differed between target (five spots) and nontarget (nine spots) locations. Thus, target detection and localization could have been successful, after display "termination" by the masking contours, to the degree to which any existing luminance differences between target and nontarget locations could be detected.

Experiment 1c was designed to eliminate, under one masking condition, any postmask luminance differences between display locations. The critical masking condition presented "pattern" masks which were composed of an outer square (like contour masks) plus an inner cross (see Figure 5). Note that the additional number of spots illuminated at pattern-mask onset was the same for target and nontarget locations, and also that the pattern masks appeared like continuously filled squares (i.e., the gaps could not be resolved) with fixation at the display center. The second masking condition presented contour masks as in Experiment 1. The cuing condition was double cue 1 (double arrows indicating different signal probabilities for V 1 and V 2). Target exposure durations, estimated at the beginning of each session for contour-mask threshold (neutral) trials, were the same for contour- and pattern-mask trials, which were presented in randomized order (i.e., randomized within the same block). Room lighting was the same as in Experiment 1. One observer, H.M., completed 2,400 trials under each masking condition (four 4-h sessions). His target exposure duration was 27.75 msec (mean of the four sessions).

Table 14 presents the $P(A)$ values for each cue-validity condition (V 1, V 2, N, and I) and stage of analysis (D and D+L), separately for contour- and pattern-mask trials. $P(A)$ values show the same pattern as in Experiment 1 (double-cue 1 condition) and the same pattern for contour- and pattern-mask trials. That is, the sensitivity gains for the second most likely position relative to the uncued location cannot be a postdisplay effect.

(Manuscript received January 30, 1986;
revision accepted for publication April 15, 1987.)