How Does Feature-Based Attention Affect Visual Processing?

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Five experiments are reported from which it is concluded that attending on the basis of a stimulus feature (e.g., red) does not directly affect the sensory quality of stimuli that possess that feature. Feature-based attention was manipulated in a visual search task by providing information about the probability that the target would possess a given feature (e.g., "The target has a 1.0 probability of being red when present."). Feature-based attention failed to aid performance under "data-limited" conditions (i.e., those under which performance was primarily affected by the quality of the stimulus) but did affect performance under conditions that were not data limited (Experiments 1–3). If attending to a feature had affected the sensory quality of stimuli, performance should have been aided under all conditions. Experiments 4 and 5 provided converging support for this conclusion.

How does attending to a particular feature, such as red, affect visual processing? Suppose that you are to meet a friend at a crowded baseball game and that you know that he will be wearing a red sweatshirt. One possibility is that looking for the red sweatshirt makes the red things in the crowd more salient (perhaps even brighter) than they would otherwise be. This would suggest that attending to a feature directly affects visual processing by directly enhancing the sensory quality of items that possess that feature. Alternatively, the sensory quality of the scene may not change as a function of feature-based attention. Looking for your friend's red sweatshirt may simply cause you to look at red items before looking at other items in the crowd. This would suggest that, rather than directly changing the sensory quality of a scene, feature-based attention results in the prioritization of items that possess the attended feature. If one assumes, for example, that there is a process that must be engaged by each item in turn (i.e., a serial process), then items that possess the attended feature might be processed before items without the attended feature. One undeniably serial process, for example, is the focusing of one's eyes on different items in a scene. The effect of attending to a feature, then, might be that items with the relevant feature are fixated before items without that feature. Similarly, a less overt process, such as focused spatial attention, may have to be allocated to one item at a time, in which case items that possess the relevant feature may be attended to before items that do not. Throughout this article, we use a serial process as the example for prioritization because it allows for clear discussion. Prioritization, however, can also apply to a parallel limited-capacity process, in which case prioritized items would receive a greater capacity allocation than they would if not prioritized.

Does Feature-Based Attention Affect Visual Processing at All?

Attending to particular features has been found to affect performance in visual search tasks (Bacon & Egeth, 1997; Egeth, Virzi, & Garbart, 1984; Kaptein, Theeuwes, & Van der Heijden, 1995; Zohary & Hochstein, 1989). In each of these studies, participants searched within spatial arrays of stimuli for a target item that was defined by a conjunction of stimulus features (e.g., a red X among red Os and black Xs). Typically, search time for conjunctively defined targets increases with the number of items in the display (e.g., Treisman & Gelade, 1980). Search performance was found to improve, however, when participants were enticed to limit their searches to items that possessed one of the two defining stimulus features (e.g., red). Specifically, when participants were encouraged—either explicitly (Egeth et al., 1984; Kaptein et al., 1995) or implicitly (Bacon & Egeth, 1997; Zohary & Hochstein, 1989)—to limit their searches on the basis of a particular stimulus feature, search times increased as a function of the number of items that shared that feature, but not as a function of the number of other stimuli in the display. On the basis of these results, it seems that search can be aided by attending to a particular stimulus feature. The question is, How is search aided?

In contrast to the studies just described, the results from recent studies suggest that attending to a specific stimulus feature does not aid visual search for a target that possesses that feature (Farell & Polli, 1993, Experiment 5; see also Francolini & Egeth, 1979, Experiment 3; Posner, Snyder, & Davidson, 1980, Experiment 2; Shih & Sperling, 1996; see also LaBerge & Brownston, 1974, for intermediate results). Participants in Farell and Polli's study located and identified stimuli for a target item that was defined by a conjunction of stimulus features (e.g., a red X among red Os and black Xs). Typically, search time for conjunctively defined targets increases with the number of items in the display (e.g., Treisman & Gelade, 1980). Search performance was found to improve, however, when participants were enticed to limit their searches to items that possessed one of the two defining stimulus features (e.g., red). Specifically, when participants were encouraged—either explicitly (Egeth et al., 1984; Kaptein et al., 1995) or implicitly (Bacon & Egeth, 1997; Zohary & Hochstein, 1989)—to limit their searches on the basis of a particular stimulus feature, search times increased as a function of the number of items that shared that feature, but not as a function of the number of other stimuli in the display. On the basis of these results, it seems that search can be aided by attending to a particular stimulus feature. The question is, How is search aided?

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a single digit that was present among distractor letters. Each of the characters in the arrays could be large or small, and participants were told at the beginning of a block of trials what the probability was of the target being either large or small (e.g., the target had a .75 chance of being large and a .25 chance of being small). Performance failed to improve as a function of this information. For example, even when the target had a 1.0 chance of being small—and participants knew this—targets were neither identified nor located more accurately than when the targets had a .50 chance of being small. These results, in contrast to those described earlier, suggest that search is not aided by attending to a particular stimulus feature.

This apparent conflict in results between the first set of studies (in particular, the Egeth et al., 1984, study) and the second set of studies (in particular, the Farell & Pelli, 1993, study) may aid in discriminating between the two possibilities described earlier (i.e., that feature-based attention directly enhances the sensory quality of items that possess the attended feature, or that feature-based attention results simply in the prioritization of items that possess the attended feature).

Overview of the Present Study

There are several differences between the Egeth et al. (1984) study and Farell and Pelli’s (1993) study. First, the dimension of the attended feature differed; Egeth et al. used color (and in one case, form), whereas Farell and Pelli used size. Second, the tasks were different; Egeth et al. used a conjunctive search task, whereas Farell and Pelli used a digit-among-letters search task. Finally, the displays were different. Egeth et al. used salient displays that were present until a response was made, and reaction time (RT) was the primary dependent measure. In contrast, Farell and Pelli used rapid-serial-visual-presentation (RSVP) streams of multiple-item arrays (see Sperling, Budiansky, Spivak, & Johnson, 1971), in which any given array was masked by the subsequent one; in this case, task accuracy was the primary dependent measure.

Experiments 1–3 of the present study showed that attending to a feature aided performance when displays were present until a response was made and RT was the dependent measure, whereas attending to a feature did not aid performance when displays were presented only briefly before being obscured by a mask and accuracy was the dependent measure. The critical difference between the Egeth et al. (1984) study and Farell and Pelli’s (1993) study, therefore, seems to have been the type of display and dependent measures that were used, rather than which stimulus dimension or which task was used.

These results allowed us to rule out the possibility that attending to a feature directly affects the sensory quality of stimuli that possess that feature. On what basis could we rule out this possibility? The conditions under which feature-based attention failed to aid performance were those that are referred to as data limited (Norman & Bobrow, 1975). These are conditions in which stimuli are presented for only a short period of time before being replaced by masks. Participants respond to the display under no speed stress. Thus, differences in accuracy across conditions are thought to reflect—nearly exclusively—differences in how well information (data) could be extracted from the display before it was replaced by the mask. Enhancing the quality of the stimulus (e.g., making it brighter) improves performance under data-limited conditions because it improves the quality of the available data. Therefore, if the effect of feature-based attention was a direct enhancement of stimulus quality, then performance should have improved under data-limited conditions (see also Santée & Egeth, 1982). These were precisely the conditions, however, under which no effects attributable to feature-based attention were observed in Experiments 1–3.

Finally, we sought converging evidence in Experiments 4 and 5 using different tasks. The results of all five experiments support the conclusion that feature-based attention fails to directly enhance the sensory quality of stimuli that possess that feature. The results are all consistent, however, with the alternative hypothesis that feature-based attention acts to prioritize stimuli in terms of when or to what extent they are processed. This prioritization is discussed in terms of the guidance of attention from one location to another—or from one item to another—within the visual array (e.g., Wolfe, 1994).

Experiment 1

As mentioned earlier, one salient difference between the Egeth et al. (1984) study and Farell and Pelli’s (1993) study is that Egeth et al. used a conjunction search task, whereas Farell and Pelli used a task in which participants searched for a digit among letter distractors. It is possible that conjunction search is particularly well suited for using feature information to aid search because the attended feature is a defining characteristic of the target. In contrast, for the digit-among-letters task, the attended feature (size in the case of Farell & Pelli’s, 1993, experiment) is simply additional, albeit potentially helpful, information.

Experiment 1 was similar to Farell and Pelli’s (1993) experiment in that participants searched for a digit target among letter distractors. The distractors within a display were randomly blue or green, and the probability of the target being either blue or green was manipulated from 0 to 1 across blocks of trials. Unlike Farell and Pelli’s study, however, displays were presented until a response was made, and RT served as the primary dependent measure. Thus, the task and manipulation of feature-based attention was like that of Farell and Pelli’s study, in which no benefit of attending to a feature was observed. The type of display and dependent measure, however, were like those of Egeth et al. (1984), in which a benefit was observed. If the lack of benefit in Farell and Pelli’s experiment was caused by the task that they used, then no benefits should occur in the present experiment—that is, there should be no effect of probability. If, however, the difference between their results and those of Egeth et al. was attributable to something else, and such effects are observable using the probability manipu-
Method

Participants. Sixteen students (aged 17–30 years) from the Johns Hopkins University undergraduate participant pool were tested in Experiment 1. All reported normal or corrected-to-normal visual acuity and color vision, and all were naive about the purpose of the experiment before being tested. Participants participated in partial fulfillment of a requirement for an introductory psychology course.

Apparatus and stimuli. Stimuli were 4 × 4 matrices of blue and green (enhanced graphics adapter/video graphics array [EGA/VGA] default 4-bit palette codes 9 and 2, respectively) letters and digits that were centered on the dark background of a super video graphics array (SVG) color monitor that was driven by a Diamond SVGA Color Graphics card (see Figure 1). The letters were chosen from all 26 letters of the alphabet except B, T, O, R, and S. The digits were chosen from the set {2, 4, 6, 7, 9}. The matrix subtended approximately 9.31° × 9.76° from a typical viewing distance of 50 cm. Each character subtended approximately 1.04° × 1.15° and was separated from its nearest neighbor by 1.72° (edge to edge) both vertically and horizontally. A 0.53° × 0.53° gray (EGA/VGA default 4-bit palette code 7) plus sign was presented at the center of the screen at the beginning of each trial as a fixation marker but was removed when the array of characters was presented.

Task. The participant's task was to indicate, for each trial, whether a digit was present or absent by pressing a button with the dominant or nondominant hand, respectively. Participants were asked to make their responses as quickly as possible while maintaining an accuracy level of 95% or better.

Design. A 2 (target: present or absent) × 4 (probability of target being of a given color, blue or green: .25, .50, .75, or 1.0) within-subjects design was used. Half the trials in each block were target-present trials, and the other half were target-absent trials. Probability was manipulated across blocks of trials. This determined the proportion of trials on which the target had a .50 chance of being blue or green. For the .50 condition, for example, half the targets were blue and the other half were green. Note that although the probability that the target would be of a given color varied across blocks, each of the nontarget characters always had a .50 chance of being either green or blue. Thus, on each trial, approximately half the stimuli were blue, and the other half were green. At the beginning of each block, a message was displayed on the screen indicating what the probabilities were of the targets being of a given color (e.g., "in the next block the digit will be green 75% of the time and blue 25% of the time.").

Each participant was tested in two 56-trial blocks for each of the five following probability ratios of blue targets to green targets: 0:1.0, 25:75, 50:50, 75:25, and 1:0:0.0, which resulted in 10 blocks of trials per participant. These five probability ratios gave the four probability conditions for a target being of a given color. For example, the blocks in which the ratio of blue targets to green targets was 25:75 yielded data for both the .25 blue condition and the .75 green condition. Similarly, the blocks in which the ratio of blue targets to green targets was 75:25 yielded data for both the .75 blue condition and the .25 green condition. The blocks were always presented in pairs of the same ratio relationship (e.g., two 25:75 blocks in a row), and the two 50:50 blocks were always the fifth and sixth blocks. Four different block orders were counterbalanced across participants. Collapsing across color, the design resulted in 28, 56, 84, and 112 target-present observations per participant for the four probabilities (.25, .50, .75, and 1.0), respectively.

Procedure. Each participant participated in a single 1-hr session that began with a set of written instructions that described the task. Participants then completed one 56-trial practice block in which the target had a .50 chance of being blue or green. Postpractice instructions emphasized that responses should be made as quickly as possible while maintaining a 95% or better level of accuracy. Participants were also asked to pay attention to the messages concerning the target-color probabilities at the beginning of each block and try to use that information to find the targets more quickly.

Figure 1 illustrates the events of a typical trial in Experiment 1. Each trial began with the presentation of a plus sign at the center of the screen. After 500 ms, the plus sign was replaced with the 4 × 4 array of colored characters. The display remained on the screen until a response was detected, at which point it disappeared. The plus sign for the next trial was presented 1,200 ms later.

Results

The mean correct RTs for target-present trials from Experiment 1 are shown in Figure 2. The data were collapsed across green and blue target colors because preliminary analyses revealed no significant effects involving target color.

A one-way repeated measures analysis of variance (ANOVA) run on the correct RTs (collapsed across target color) revealed a significant main effect of probability, $F(3, 45) = 22.45, MSE = 1,566.24, p < .01$. Planned comparisons confirmed that responses were significantly faster when the target was a given color with probability 1.0 (hereinafter called 1.0 targets) than when the target had a .50 probability of being either color (hereinafter called .50 targets) than to .50 targets, $t(15) = 5.03, MSE = 26.42, p < .01$. Although responses were slower to targets that were of a given color with .25 probability (hereinafter called .25 targets) than to .50 targets, $t(15) = 3.67, MSE = 50.33, p < .01$, responses to targets that were of a given color with .75 probability (hereinafter called .75 targets) were not reliably faster than
A probability of target being of a given color.

Figure 2. Mean correct reaction times from the target-present trials in Experiment 1. These are collapsed across target color (blue and green).

responses to .50 targets, $t(15) = 0.71, \text{MSE} = 30.29, \text{ns}$.

This suggests that there was a cost associated with the .25 targets but no benefit associated with the .75 targets. The most important comparison for our purposes, however, was between the 1.0 targets (i.e., complete certainty concerning the target color) and the .50 targets (i.e., maximum uncertainty concerning the target color). The reliable advantage of the former over the latter demonstrated that search was aided by attending to a particular color.

The same analyses were run on the arcsine transformations of the error rates (ERs) for the target-present trials collapsed across target color (ERs are given in Table 1). No significant effects that were different from those in the RTs were revealed.

Table 2 shows the RTs and ERs for the target-absent trials collapsed across target color. Notice that there are not separate target-absent trials for the .75 condition and the .25 condition because these conditions were part of a single block type (e.g., a .75 blue to .25 green block). When there was no target present, there was no distinction between the .25 and .75 conditions. These were simply target-absent trials that occurred within a block in which 75% of the target-present trials included a blue target and 25% included a green target. Target-absent trials from these blocks are therefore referred to as the .75/.25 condition.

Submitting the RTs to a one-way repeated measures ANOVA with probability (1.0, .75/.25, and .50) as the variable revealed a significant effect of probability, $F(2, 30) = 27.17, \text{MSE} = 8,951.3715, p < .01$. Planned comparisons confirmed that responses were faster in the 1.0 condition than in both the .50 condition, $t(15) = 6.13, \text{MSE} = 103.30, p < .01$, and the .75/.25 condition, $t(15) = 8.86, \text{MSE} = 64.22, p < .01$. There was no reliable difference in RT, however, between the .75/.25 condition and the .50 condition, $t(15) = 0.59, \text{MSE} = 109.77, \text{ns}$. The same analyses were run on the arcsine transformations of the ERs, but no reliable effects that were different from those that occurred in the RTs were observed.

Discussion

The results of Experiment 1 indicate that the apparent conflict in results between the Egeth et al. (1984) study and Farell and Pelli’s (1993) study was not caused by the different tasks that were used in the two studies. In particular, the fact that the attended dimension was a defining characteristic of targets in the Egeth et al. study, but was only additional—although potentially helpful—information in Farell and Pelli’s study did not determine the different outcomes of those two studies. As in Farell and Pelli’s study, participants in our Experiment 1 sought a digit target among letter distractors. Nonetheless, probability effects were observed, indicating that attending to a stimulus feature did aid the search for targets that possessed that feature. Most important, responses to targets of a known color (i.e., 1.0 targets) were reliably faster than responses to targets of a maximally unknown color (i.e., .50 targets).

Experiment 2

Another salient difference between the Egeth et al. (1984) study and that of Farell and Pelli’s (1993) study was that different stimulus dimensions were used for directing attention in the two studies. In the Egeth et al. study, participants attended to a particular color (or in some conditions, form), whereas in Farell and Pelli’s study, participants attended to stimuli of a given size. For some reason, it may be possible to attend to a given color, but not to attend to specific features within other dimensions, such as size. To test this possibility, we conducted a second experiment that was

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Note. Probability is the probability that the target was presented in the specified color.

1 The purpose of the ER ANOVAs for experiments in which RT was the primary dependent measure was to provide assurance against drawing conclusions from patterns in the RT data that could have been due to a speed-accuracy trade-off. As such, we present $F$ and $p$ values for the ER analyses only when there is a significant effect in the direction opposite that in the RTs.
similar to Experiment 1, except that stimuli were large and small instead of blue and green.

Method

Except for the differences noted below, the methods used in Experiment 2 were the same as those used in Experiment 1.

Participants. Sixteen different students from the Johns Hopkins University undergraduate subject pool were tested in Experiment 2.

Apparatus and stimuli. Stimuli were 4 × 4 matrices of small (0.60" × 0.85") and large (1.46" × 1.71") black letters and digits presented on gray (EGA/VGA default 4-bit palette code 7) backgrounds. Each stimulus was centered in a 2.75" × 3.0" cell of an 11.00" × 12.00" matrix. The edge-to-edge distance, both vertically and horizontally, between two large characters was 1.72", that between two small characters was 2.86", and that between a small character and a large character was 2.29". The sets of letters and digits were the same as those used in Experiment 1. Finally, the fixation marker was black instead of gray.

Task. The task was the same as that used in Experiment 1: to report whether a digit was present or absent.

Design and procedure. A 2 (target: present or absent) × 4 (probability of a target being of a given size, large or small: .25, .50, .75, or 1.0) within-subjects design was used. Other than the fact that size rather than color was manipulated, the design and procedure of Experiment 2 were identical to those used in Experiment 1.

Results

The results of Experiment 2 were similar to those found in Experiment 1. The mean correct RTs for target-present trials are shown in Figure 3. The data were collapsed across target size. The same pattern that was observed in Experiment 1 occurred again in Experiment 2. A one-way repeated measures ANOVA with probability (1.0, .75, .25, or .50) as the variable revealed a significant effect of probability, $F(2, 30) = 36.39, MSE = 5,316.8565$, $p < .01$. Planned comparisons confirmed that responses were significantly faster for 1.0 targets than for .50 targets, $t(15) = 6.52, MSE = 83.28$, $p < .01$, and the .75/25 condition, $t(15) = 3.26, MSE = 42.89, p < .01$, neither the .25 targets, $t(15) = 1.64, MSE = 48.28$, ns, nor the .75 targets, $t(15) = 1.22, MSE = 49.90$, ns, differed reliably from the .50 targets. Once again, however, the most important comparison for our purposes was between the 1.0 targets (i.e., complete certainty concerning the target size) and the .50 targets (i.e., maximal uncertainty concerning the target size). The reliable advantage of the former over the latter demonstrates that search was aided by attending to a particular size.

The same analyses were run on the arcsine transformations of the ERs for the target-present trials collapsed across target size (ERs are given in Table 1). No significant effects that were different from those in the RTs were revealed.

Table 3 shows the RTs and ERs for the target-absent trials collapsed across target size. The same pattern that was observed in Experiment 1 occurred again in Experiment 2. A one-way repeated measures ANOVA with probability (1.0, .75, .25, or .50) as the variable revealed a significant effect of probability, $F(2, 30) = 36.39, MSE = 5,316.8565$, $p < .01$. Planned comparisons confirmed that responses were faster for the 1.0 condition than for both the .50 condition, $t(15) = 6.52, MSE = 83.28$, $p < .01$, and the .75/25 condition, $t(15) = 7.37, MSE = 72.46, p < .01$. There was no reliable difference in RT, however, between the .75/25 condition and the .50 condition, $t(15) = 0.14, MSE = 61.35$, ns. The same analyses were run on the arcsine transformations of the ERs, but, again, no reliable effects that were different from those that occurred in the RTs were observed.

Discussion

The results of Experiment 2 indicate that the apparent conflict in results between the Egeth et al. (1984) study and Farell and Pelli's (1993) study was not caused by the different types of features used in the two studies. Attending to a particular size in this experiment aided search, just as attending to a particular color in Experiment 1 aided search. Specifically, responses to targets of a known size (i.e., 1.0 targets) were reliably faster than responses to targets of a maximally unknown size (i.e., .50 targets).

Experiment 3

The results of Experiments 1 and 2 ruled out the task and the type of feature used to direct attention as reasons for the
apparent conflict between the Egget al. (1984) study and Farell and Pelli's (1993) study. A remaining difference between the two studies is the type of display and dependent measure that was used. Egget al. used displays that were present for only a short time before being replaced by another display and accuracy was the dependent measure. In Experiment 3, we tested the hypothesis that this difference is what caused the difference in results. Experiment 3-color (3c) and Experiment 3-size (3s) were similar to Experiments 1 and 2, respectively, except that the displays were presented for only a short time before being replaced with a masking display. Task accuracy served as the primary dependent measure. If the difference in results was caused by the difference in display duration, then no probability effects should be observed in Experiments 3c and 3s, as was the case in Farell and Pelli's study, in which short display durations were used.

Method

Participants. Thirty-two different students from the Johns Hopkins University undergraduate subject pool were tested in Experiment 3: 16 in Experiment 3c and 16 in Experiment 3s.

Apparatus and stimuli. The stimuli in Experiment 3c were the same as those used in Experiment 1, except that in addition to the 4 × 4 arrays of green or blue characters, 4 × 4 arrays of 1.15° × 1.72" green or blue random-dot (30% density) masks were also used. Blue stimuli were replaced by blue masks and green stimuli were replaced by green masks. The stimuli in Experiment 3s were the same as those used in Experiment 2, except that in addition to the 4 × 4 arrays of large and small characters, 4 × 4 arrays of large (2.19° × 2.57") and small (0.9° × 1.28") black random-dot (30% density) masks were also used. Small stimuli were replaced with small masks and large stimuli were replaced by large masks.

Task. The task was different in Experiment 3 than in Experiments 1 and 2. A single digit was present on every trial, and participants reported whether the digit was a 2 or a 5. In addition, responses were not speeded; participants were encouraged to take as much time as they needed to make their responses. The primary dependent measure was ER rather than RT.

Design. The designs of Experiment 3c and 3s were the same as those of Experiments 1 and 2, except that all trials were target-present trials. Participants completed ten 64-trial blocks. Collapsing across color (Experiment 3c) or size (Experiment 3s), there were 56, 112, 168, and 224 observations for each participant at each of the four different probabilities (.25, .50, .75, and 1.0), respectively.

Procedure. Most aspects of the procedure were the same as that of Experiments 1 and 2. However, rather than a single block of practice, participants participated in three 64-trial practice blocks during which the stimulus onset asynchrony (SOA) between the array of characters and the array of masks was adjusted to allow for an ER of approximately 30%. For Experiment 3c, the mean SOA was 50 ms with a range of 28–84 ms. For Experiment 3s, there was an unacceptably large difference in performance between large and small targets in a pilot experiment. Therefore, SOA was determined separately for large and small targets, yielding approximately 30% ERs for both target types. This resulted in a slight asynchrony between the onset of masks for the large stimuli (mean SOA = 110 ms; range = 28–252 ms) and the onset of masks for the small stimuli (mean SOA = 289 ms; range = 42–378 ms). The difference was not subjectively distracting, and no participant remarked on it. The SOAs that were determined during these practice blocks were used throughout the remainder of the experiment.

Figure 4 illustrates the events that occurred on a typical trial in Experiments 3c and 3s. Each trial began with the presentation of a plus sign at the center of the screen. After 500 ms, the plus sign was replaced with the 4 × 4 array of characters. After the SOA for the given participant, this display was then replaced with the array of random-dot masks that covered the locations in which the characters had appeared. In Experiment 3c, the mask in a given location was the same color (blue or green) as the character in that location had been in. In Experiment 3s, the mask in a given location was the same size (large or small) as the character in that location had been. The display of masks remained on the screen until a response was detected, at which point it disappeared. The plus sign for the next trial was presented 1,200 ms later.

Results

The mean ERs for Experiments 3c and 3s are shown in Figure 5. The data are collapsed across green and blue.
targets for Experiment 3c and across large and small targets for Experiment 3s; preliminary analyses revealed no significant effects involving color in Experiment 3c or size in Experiment 3s.

A one-way repeated measures ANOVA run on the arcsine transformations of the ERs from Experiment 3c (collapsed across target color) revealed no significant effect of probability, $F(3,45) = 0.71, MSE = 0.0065, ns$. Planned comparisons also indicated no significant difference in ERs for 1.0 targets versus .50 targets, $t(15) = 1.01, MSE = 0.0648, ns$; for .75 targets versus .50 targets, $t(15) = 0.11, MSE = 0.0557, ns$; or for .25 targets versus .50 targets, $t(15) = 1.13, MSE = 0.0632, ns$.

A one-way repeated measures ANOVA run on the arcsine transformations of the ERs from Experiment 3s (collapsed across target size) revealed a similar lack of probability effects. There was no main effect of probability, $F(3,45) = 1.49, MSE = 0.0100, ns$. Planned comparisons revealed no significant difference in ERs for 1.0 targets versus .50 targets, $t(15) = 1.70, MSE = 0.0529, ns$; for .75 targets versus .50 targets, $t(15) = 0.03, MSE = 0.0954, ns$; or for .25 targets versus .50 targets, $t(15) = 1.41, MSE = 0.0660, ns$.

**Discussion**

The results of Experiment 3 indicate that the difference in display type and dependent measure between the Egeth et al. (1984) study and Farell and Pelli's (1993) study could have caused the apparent conflict in results between these two studies. Experiments 3c and 3s were similar to Experiments 1 and 2, except that, instead of using long displays with RT as the primary dependent measure, displays were masked after a short duration and task accuracy served as the dependent measure. Under these conditions, there were no observable effects of attending to a given color or a given size, just as in Farell and Pelli's study.

As mentioned in the introduction, an important implication of these results is that attending to a feature does not directly enhance the sensory quality of items that possess that feature. Because the stimuli were presented for only a short period of time before being replaced by masks, limits in performance (i.e., task accuracy) should have reflected the extent to which participants were able to extract information ("data") from the display, before the display was replaced by the mask. Because responses were not speeded, but instead participants were encouraged to respond at their leisure, differences in performance across probability conditions should have reflected—nearly exclusively—differences in the participants' ability to extract information from the display across the different conditions. In fact, however, no reliable differences in performance were observed across probability conditions, indicating that there were no observable differences in participants' ability to extract information from the display. If attending to a stimulus feature resulted in enhanced quality of stimuli that possessed that feature, then participants should have been able to extract information more successfully from those stimuli than from stimuli that did not include the attended feature. Insofar as participants were unable to do so, there is no evidence that the sensory quality of those stimuli was enhanced.

Thus, with regard to the two possibilities outlined in the introduction, the first—that attending to a stimulus feature directly enhances the sensory quality of stimuli—can be ruled out. The second, however—that feature-based attention serves to prioritize stimuli for some process, such as spatial attention—remains viable. Experiments 4 and 5 provided converging evidence for these conclusions.

**Experiment 4**

In Experiment 4, we took advantage of the asymmetry that occurs in visual search performance when searching for Os among Qs versus searching for Qs among Os (see Figure 6). In the first case (hereinafter called the difficult condition), search time increases when additional stimuli (i.e., distractors) are added to the display (e.g., Treisman & Souther, 1985); that is, detection of the target is not independent of the number of stimuli in the display. In contrast, for the second case (hereinafter called the easy condition), search seems to be spatially parallel in that search time is unaffected by adding more distractors to the display; that is, detection of the target is independent of the number of distracting elements (e.g., Treisman & Souther, 1985).

**Figure 6.** Examples of the displays used in the easy and difficult conditions of Experiment 4. The target in the easy condition was O and the target in the difficult condition was Q. On average, half of the distractors were blue (filled), and the other half were green (outlined). The color of the target was determined by the probability condition.
If, contrary to our conclusion from Experiments 1–3, feature-based attention can act to directly enhance the sensory quality of stimuli, then feature-based attention should aid performance in both the easy condition and the difficult condition. This follows because an enhanced target should be detected more easily than a nonenhanced target regardless of its context. If, however, feature-based attention does not affect the sensory quality of a scene but does serve to prioritize stimuli, then performance should be aided in the difficult condition but not in the easy condition. This follows because the relative priority of stimuli should affect performance only when the detection of the target is affected by the surrounding stimuli—such as depending on the number of distracting elements—and a given stimulus can therefore enjoy the benefit of being prioritized. Detection of the target is dependent on the number of surrounding stimuli in the difficult condition, whereas in the easy condition it is not.

Method

Except for the differences noted below, the methods used in Experiment 4 were the same as those used in Experiment 1.

Participants. Twelve different students from the Johns Hopkins University undergraduate subject pool were tested in two 1-hr sessions each.

Apparatus and stimuli. Stimuli were irregular arrays of green and blue letters that were centered on the dark background of SVGA monitors. The letters were 0.8° × 1.0° Os and Qs. On half the trials, there were 16 characters arranged in a 4 × 4 matrix. On the other trials, there were only 8 characters, the positions of which were chosen randomly from the 16 positions in the 4 × 4 matrix. For this experiment, each character was jittered away from the center of its cell by a random 0%–25% of the height of the cell up or down from center and 0%–25% of the width of the cell left or right of center.

Task. There were two different tasks, one in each of two sessions. For both tasks, participants reported as quickly as possible whether a target letter was present in the display. For one task—the easy task—the target letter was a Q and the distractors were all Os. For the other task—the difficult task—the target letter was an O and the distractors were all Qs. A display size manipulation was included so that we would be able to assess the difference between the two tasks. RT in the easy task should be unaffected by the display size manipulation, indicating that detection of the target is independent of the number of distracting elements, whereas RT in the difficult task should increase as display size increases, indicating that detection of the target depends on the number of distracting elements (Treisman & Souther, 1985).

Design. Participants completed 10 blocks of 64 trials each for each of the two different tasks. A 2 (target: present or absent) × 2 (task: easy or difficult) × 2 (display size: 8 or 16) × 4 (probability of target being blue or green: .25, .50, .75, or 1.0) within-subjects design was used. Half the trials in each block were target-present trials, and the other half were target-absent trials. Half the trials in each block had a display size of 8, and the other half had a display size of 16. Task was manipulated across sessions, and the order in which they were done was counterbalanced across participants. Finally, probability was manipulated in the same way as in Experiments 1–3. Collapsing across color and display size, there were 32, 64, 92, and 128 observations per participant at the four probability conditions (.25, .50, .75, and 1.0), respectively, for each of the two tasks.

Procedure. Participants participated in two individual 1-hr sessions, one for each of the two tasks. All other aspects of the procedure, including the timing of the trial events, were the same as in Experiment 1.

Results

To assess whether the task manipulation had the desired effect, we compared the mean correct RT as a function of display size across the two tasks for both target-present and target-absent trials (shown in Figure 7). The data for this analysis were collapsed across the four probability conditions. A 2 (task: easy or difficult) × 2 (display size: 8 or 16) × 2 (target: present or absent) repeated measures ANOVA revealed that all sources of variance were significant (p < .01). Planned comparisons showed that RTs for Display Size 16 were longer than RTs for Display Size 8 for both the easy task, F(1, 11) = 5.62, MSE = 527.28, p < .05, and the difficult task, F(1, 11) = 75.02, MSE = 2,879.73, p < .01. However, the effect was much smaller in the easy task than in the difficult task, as the significant interaction between task and display size in the omnibus ANOVA confirmed. In particular, the slopes for the difficult task were 10.23 ms/item in the target-present condition and 23.41 ms/item in the target-absent condition. By contrast, the slopes for the easy condition were 0.97 ms/item and 2.95 ms/item, respectively. These results indicate that the number of surrounding stimuli had a substantial effect on target-detection time in the difficult condition but a much smaller effect in the easy condition. Although the effect of display size was significant in the easy task, the slopes in this condition fell within the range that is taken to reflect spatially parallel search (e.g., Treisman & Souther, 1985). The slight increase in slope could have several different causes. For example, search may have been spatially parallel
on all but a few trials, or participants may have engaged in rechecking strategies on some trials. The same analyses run on the arcsine transformations of the ERs (ERs are given in Table 4) revealed no significant effects that differed from those found in the RTs.

Figure 8 shows the mean correct RTs for the two different tasks for each of the four probability conditions, collapsed across both display size and target color. (Preliminary analyses confirmed that neither display size nor color interacted significantly with probability.) The collapsed data were submitted to a 2 (task: easy or difficult) × 4 (probability: .25, .50, .75, or 1.0) repeated measures ANOVA. Again, all sources of variance were significant: task, F(1, 11) = 53.10, MSE = 15,819.96, p < .01; probability, F(3, 33) = 30.77, MSE = 1,345.24, p < .01; and Task × Probability, F(3, 33) = 11.51, MSE = 1,573.78, p < .01. Planned comparisons confirmed that the probability manipulation had a significant effect for both tasks: easy, F(3, 33) = 6.72, MSE = 1,962.39, p < .01; difficult, F(3, 33) = 22.54, MSE = 1,439.71, p < .01. However, the effect in the easy task seemed to have been driven entirely by a cost for .25 targets. When the 1.0-target condition (i.e., complete certainty about the target color) was compared with the .50-target condition (i.e., complete uncertainty about the target color), a significant 78-ms difference was found for the difficult task, t(11) = 5.38, MSE = 35.70, p < .01, whereas the 12-ms difference for the easy task was not significant, t(11) = 1.56, MSE = 19.50, ns. These results indicate that attending to a feature color aided search in the difficult task, but not in the easy task.3

The same analyses were run on the arcsine transformations of the ERs for the target-present trials collapsed across target color and set size (ERs are given in Table 5). Again, no significant effects that differed from those found in the RTs were revealed.

Finally, Table 6 shows the RTs and the ERs for the target-absent trials collapsed across target color and set size. A 2 (task: easy or difficult) × 3 (probability: .50, .75/25, or 1.0) repeated measures ANOVA run on the RTs revealed a significant main effect of both probability, F(1, 11) = 12.05, MSE = 11,221.55, p < .01, and task, F(1, 11) = 52.81, MSE = 54,258.50, p < .01, as well as a significant interaction between these two variables, F(1, 11) = 20.59, MSE = 5,239.53, p < .01. Planned comparisons confirmed that for the difficult task, responses were faster for the 1.0 condition than for both the .50 condition, t(15) = 5.45, MSE = 115.42, p < .01, and the .75/25 condition, t(15) = 7.06, MSE = 81.09, p < .01, whereas for the easy task, there were no reliable effects of probability (ps > .05). The same analyses run on the arcsine transformations of the ERs revealed no significant effects that differed from those observed in the RTs.

Discussion

The results of Experiment 4 provide converging support for the conclusion that feature-based attention does not directly enhance the sensory quality of stimuli but that it may act to prioritize stimuli for some process, such as spatial attention. Responses were reliably faster when the target color was known (1.0 target condition) than when it was unknown (.5 target condition), for the difficult task but not for the easy task. If attending to a color had acted to directly enhance the sensory quality of stimuli, then this benefit should have occurred in both the easy and difficult conditions. Instead, the benefit was observed only when stimuli could benefit from being prioritized relative to other stimuli (i.e., in the difficult task). Experiment 5 provided another test of this hypothesis using a strategy analogous to that of Experiment 4 but with an accuracy measure instead of an RT measure.

Experiment 5

Sperling (1960) and others (e.g., Averbach & Coriell, 1961) demonstrated that when stimuli are presented for only a short period of time but are not replaced by masks, a memorial representation of those stimuli remains available from which participants can effectively "read off" information just as if the physical stimulus were still there (see Coltheart, 1980, for a review). This representation—referred to as informational persistence—however, decays over a short period of time (about 300–500 ms). Because of this decay and because of the limited capacity of short-term

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3 The cost for the .25 targets for the easy task may have been related to the small but significant increase in the RT × Display Size slope for the easy task.
Table 5
Error Rates (%) for Target-Present Trials in Experiment 4

<table>
<thead>
<tr>
<th>Task</th>
<th>Probability</th>
<th>.25</th>
<th>.50</th>
<th>.75</th>
<th>1.00</th>
</tr>
</thead>
<tbody>
<tr>
<td>Easy (Q among Os)</td>
<td>4.07</td>
<td>1.91</td>
<td>1.61</td>
<td>3.12</td>
<td></td>
</tr>
<tr>
<td>Difficult (O among Qs)</td>
<td>6.29</td>
<td>2.40</td>
<td>2.71</td>
<td>2.21</td>
<td></td>
</tr>
</tbody>
</table>

memory, the identities of only about four to five items can be reported from a briefly flashed multi-item display (e.g., Sperling, 1960). In Experiment 5, we took advantage of this limitation to further test the hypothesis that attending to a stimulus feature fails to directly enhance the sensory quality of stimuli but instead may affect the prioritization of stimuli.

The displays and task were similar to those used in Experiment 3c; arrays of green and blue letters plus one digit were presented briefly and participants were asked to identify the digit. In this experiment, however, no masks were used. The informational persistence of the display, therefore, should have allowed participants to transfer items into short-term memory for later report. As in Experiment 4, there was an easy condition and a difficult condition. In the easy condition, there were only 4 items in the display, whereas in the difficult condition there were 16 items in the display. Thus, for the easy condition, it should have been possible for all of the items to be transferred into short-term memory before the representation decayed. The priority of a given stimulus, therefore, should have been irrelevant as far as identification of the digit was concerned. In contrast, for the difficult condition, not all items in the display could be transferred into short-term memory. Therefore, if some stimuli were prioritized over others, they may have been more likely than others to be transferred and thus more likely to be identified.

The predictions, then, were the same as those of Experiment 4. If feature-based attention affects visual processing by directly enhancing the sensory quality of stimuli that possess that feature, then attending to a particular feature should aid performance in both the easy and difficult conditions. This follows because an enhanced item should be detected and identified more easily than a nonenhanced item. If, however, feature-based attention does not affect the sensory quality of the scene but does affect the prioritization of stimuli, then performance may be aided in the difficult condition but not in the easy condition. This follows because there is room for improvement because of prioritization in the difficult condition, but not in the easy condition.

Method

Participants. Sixteen different students from the Johns Hopkins University undergraduate subject pool were tested in Experiment 5.

Apparatus and stimuli. The stimuli were the same as those used in Experiment 3c, except that no masks were used.

Task. The task was similar to that used in Experiment 3c. On every trial, there was a single digit among letter distractors, and participants identified the digit. The digit in this case, however, could be any of the set {2-9}, and participants responded on the number pad of a computer keyboard. This change was introduced because participants performed at ceiling levels in a pilot experiment in which the target was always a 2 or a 5.

Design. A 2 (display size: 4 or 16) x 4 (probability of target being of a given color: .25, .50, .75, or 1.0) within-subjects design was used. Display size was manipulated within blocks of trials, and probability was manipulated as before. Participants completed 10 blocks of 64 trials each, which, collapsing across color, yielded 32, 64, 96, and 128 observations at the four probabilities (.25, .50, .75, and 1.0), respectively, for each of the two set sizes.

Procedure. Most aspects of the procedure were the same as those in Experiment 3c. Practice involved a tracking procedure in which the best stimulus duration for the given participant was determined. Rather than being the time between stimulus presentation and mask onset, however, the time determined was simply how long the stimulus was presented. The mean stimulus duration was 53 ms with a range of 14–98 ms. These stimulus durations yielded a mean (collapsing across both display sizes) accuracy level of 30%.

As in other experiments, each trial began with the presentation of a plus sign at the center of the screen. After 500 ms, the plus sign was replaced with the array of characters, which remained present for the stimulus duration that was determined during practice. The stimulus array was then replaced by a blank screen that remained so until a response was made. The plus sign for the next trial was presented 1,200 ms after the response.

Results

The mean ERs from Experiment 5 are shown in Figure 9. The data are collapsed across green and blue targets; preliminary analyses revealed no significant effects involv-
Discussion

Once again, there were reliable effects of attending to a particular color in the difficult condition, but not in the easy condition. These results converge with those of Experiment 4 in providing support for the hypothesis that attending to a stimulus feature does not directly affect the sensory quality of items within the display but that it may affect the prioritization of items with regard to some other process, such as spatial attention.\(^4\)

In addition to the usual benefit of obtaining converging evidence, the convergence of results between Experiments 1–3 and Experiments 4 and 5 allowed us to rule out the possibility that there is something about RT versus accuracy per se that caused the difference in results across the different experiments. Effects of feature-based attention were observed in both RT (Experiments 1, 2, and 4) and accuracy (Experiment 5), and significant effects failed to occur in both RT (Experiment 4) and accuracy (Experiments 3 and 5). Thus, there was nothing peculiar about either of the measures that allowed or disallowed the observation of feature-based attention effects.

General Discussion

The results of this research allowed us to rule out the hypothesis that feature-based attention affects visual processing by directly enhancing the sensory quality of items that possess the attended feature. Attending to a stimulus feature failed to aid performance in a visual search task under data-limited conditions (Experiments 3c and 3s; Farell & Pelli, 1993), although performance was aided when conditions were not data limited (Experiments 1 and 2; Egget al., 1984). Moreover, feature-based attention failed to aid performance in the easy conditions of both Experiments 4 and 5. The fact that performance was not aided under any of these conditions led us to conclude that the sensory quality of stimuli remains unaffected by feature-based attention. Instead, we suggest that feature-based attention may result in the prioritization of stimuli that possess the attended feature over stimuli that do not, perhaps by directing spatial attention to those stimuli before others. We discuss this alternative suggestion further, below.

Two previous lines of research have yielded results that might be taken as indicating that, contrary to our conclusion, attending to a stimulus feature does directly enhance the sensory quality of stimuli that possess that feature. Our conclusion that it does not is based on failures to reject the null hypothesis, these alternative sources of evidence are especially important to consider. The first is concerned with the observation that attention to a particular feature of a stimulus can increase the accuracy of reports about that feature relative to when it is unattended (e.g., Kulpe, 1902; Yokoyama, cited in Boring, 1924; see Egget, 1967, and Pillsbury, 1908, pp. 1-11, for reviews). The second is concerned with the observation that in experiments using monkeys, attending to stimulus features resulted in increased firing rates of cells in area V4 of the visual cortex to stimuli that possessed the attended feature (Motter, 1994a, 1994b). These topics are considered in the next two sections.

Reports of Attended Aspects of Stimuli

Kulpe (1902) and others (e.g., Chapman, 1932; Yokoyama, cited in Boring, 1924) have reported greater accuracy for reports of attended attributes of complex stimuli than for unattended attributes. For example, Yokoyama reported an experiment in which displays containing pairs of rectangles that differed in either length or brightness were presented tachistoscopically. Participants were asked to make comparison judgments concerning one or the other of the two dimensions. Occasionally, after making the primary judgment, participants were asked to make a judgment about the irrelevant dimension. The results showed that report accuracy was higher when a given attribute was attended than when it was unattended.

A long and heated debate has surrounded the question of how results like those of Yokoyama (cited in Boring, 1924) should be interpreted. On the one hand, the advantage for the attended attribute may be attributable to an enhancement of the sensory quality of the attended attribute. On the other hand, the advantage may be attributable to differential memorial processes. For the Yokoyama example, participants were always asked about the unattended attribute after

\(^4\) It could be argued that there was a floor effect in the easy condition of Experiment 4 and a ceiling effect in the easy condition of Experiment 5, in which case it would be unreasonable to expect probability effects in these conditions. This argument would be more persuasive, however, if mean RT were closer to simple RT (i.e., approximately 200 ms) than it was in Experiment 4 and if mean accuracy were closer to perfect (i.e., 100% correct) than it was in Experiment 5. It is not clear what the concept of “floor” means for RTs that average approximately 500 ms, or what “ceiling” means for accuracy levels that average approximately 85%, as these values did in the easy conditions of Experiments 4 and 5, respectively. In any case, these two experiments are not intended to stand alone but are instead offered as converging evidence in support of our hypothesis.
they were asked about the attended attribute. Therefore, the advantage in report accuracy may have reflected the relative strength of the memory trace rather than enhanced perceptual processing.

In fact, little evidence has emerged over the years to support the interpretation that the advantage in report accuracy for attended attributes in experiments like Yokoyama’s (cited in Boring, 1924) were caused by enhanced perceptual processing. Several researchers have reported that accuracy was not reliably greater when the to-be-attended attribute was announced to the participant before stimulus presentation relative to when it was announced after stimulus presentation (e.g., Brown, 1960; Lawrence & Coles, 1954; Lawrence & LaBerge, 1956). If the advantage in report accuracy were attributable to enhanced sensory quality, then the advantage should have been greater when the instruction was received before rather than after stimulus presentation. If, on the other hand, the advantage were attributable to memorial differences between the two instructional conditions, then it might not make much difference whether the instruction was received before or after stimulus presentation. This being said, these studies have not gone without criticism, and the final answer may be complicated (see Egeth, 1967, for a review). We suggest that the failure to find an advantage in masked accuracy for attended features over unattended features in the present study, however, contributes to the pool of evidence suggesting that attention to stimulus features does not directly enhance the sensory quality of stimuli (see also Prinzmetal, Amiric, Allen, Nwachuku, & Bodanske, 1995, for a related conclusion).

The Effects of Attending to Features
on Cell Firing Rate in V4

The second line of research that might lead to a conclusion that conflicts with ours is concerned with the effects of feature-based attention on the firing rate of cells in area V4 of the primate visual cortex. In particular, Motter (1994a; see also Motter, 1994b) found that some cells in area V4 fired more vigorously to stimuli that possessed attended features (e.g., color or brightness) than to stimuli that did not. V4 is a region in visual cortex that is thought to be involved in fairly early visual processing (e.g., Schiller & Lee, 1991). "Firing more vigorously," then, might suggest that, contrary to our conclusion, there may be an enhancement of the sensory quality of attended features relative to unattended features.

Increased cell firing rates, however, do not necessarily translate into enhanced sensory quality. In fact, Motter’s (1994a) own interpretation of his results is consistent with our claims concerning the effect of feature-based attention:

The consequence of the selective process is that on any given trial the clear majority of the highly activated V4 neurons preferentially represent those separate stimuli in the visual scene that correspond to potential targets as defined by their match to the cue’s color or luminance. A sequential combination of these two processes, initially a full-field prefocal attentive selection based on features followed by a spatially restricted focal attentive process, offers an interesting physiological model of selective attention expressed within single neurons. (p. 2188)

Thus, according to this interpretation, attending to a feature serves to effectively highlight the representations of stimuli that possess that feature, so that spatial attention is directed first to those stimuli. This may be translated as stating that stimuli that possess attended features are prioritized relative to stimuli that do not.

What Does Feature-Based Attention Do?

As mentioned earlier, although the results of our research are inconsistent with the hypothesis that feature-based attention directly enhances the sensory quality of the stimuli, they are consistent with the alternative possibility that feature-based attention affects the prioritization of stimuli, but prioritized with respect to what? One possibility is that spatial attention—like eye movements—must be allocated to individual items in turn and that attending to a particular feature ensures that items with that feature will be attended before other items. Benefits attributable to this prioritization would be much more evident when displays are long in duration—and there is time to allocate attention to multiple items—than when displays are short in duration—and there is little time to attend to more than one item. This is exactly what we observed in Experiments 1–3.

In a recent article, Shih and Sperling (1996) arrived at this same conclusion (i.e., that attending to a feature serves to direct spatial attention to locations with items that possess the relevant feature). They used a visual search task in which, like in our experiments, the likelihood that the target would possess a particular stimulus feature (e.g., red) was manipulated from 0 to 1. In this way, their participants were induced to attend to that stimulus feature. Unlike in our experiments, however, the displays involved RSVP of multiple six-item circular arrays of stimuli, only one of which contained the target. In one experiment, each display included all red stimuli or all green stimuli. Thus, the attended color was reliably correlated with a moment in time, but not with a location in the array; every location had both red and green items equally often across the course of a trial. Their hypothesis was that feature-based attention does not serve to filter out items that do not possess the attended feature but that it instead serves to direct spatial attention to locations that contain items that possess the relevant feature. If this hypothesis is correct, then knowing what color the target will be could not aid search in their experiment. This follows because such knowledge would not provide a reliable location toward which to direct attention. If, instead, feature-based attention does serve to filter out items that do not possess the attended feature, then knowledge of what color the target would be should greatly aid search, and it would be possible to filter out half of the displays. Consistent with Shih and Sperling’s (1996) hypothesis, search was unaffected by the probability manipulation.

In a second experiment, each of the displays in the RSVP stream contained a singleton stimulus that was defined within the dimension of the attended feature (e.g., all red stimuli except for one green stimulus or all green stimuli except for one red stimulus). The target was always one of the singletons. Thus, unlike in their Experiment 1, color was
strongly correlated with a location. Consistent with their hypothesis, search was aided by the probability information in this second experiment.

Shih and Sperling's (1996) data and conclusion converge nicely with those reported here. There is no evidence that attending to a feature serves to directly enhance the sensory quality of stimuli (e.g., by allowing items that do not possess the relevant feature to be filtered out), but instead it may serve to direct spatial attention to locations that contain items that possess the relevant stimulus. This conclusion is also consistent with the body of evidence suggesting that search is "guided" through a visual scene on the basis of stimulus attributes (e.g., Wolfe, 1994; Wolfe, Cave, & Franzl, 1989).

**What Does Location-Based Attention Do?**

The main conclusion of this article is that feature-based attention does not directly enhance the sensory quality of stimuli that possess that feature. As discussed in the preceding section, however, it is possible that attending to a feature serves to direct spatial attention to locations that contain stimuli that possess the attended feature. Spatial attention may in turn enhance the sensory quality of stimuli that appear in the attended location. In this final section, we consider the evidence surrounding this possibility.

Attending to a location can improve the speed, accuracy, or both with which participants respond to stimuli at that location (although it does not necessarily do so). Although that much is clear, there is still disagreement about the theoretical interpretation of this finding. In particular, we focus here on the question of whether the attentional effect of a location cue is better conceived of in terms of (perceptual) resource allocation or (decisional) uncertainty.

One interpretation of an attentional cuing effect is that the perceptual processing system is limited in capacity; according to this interpretation, a valid location cue permits a concentration of resources that can in turn lead to improved perceptual coding (e.g., Broadbent, 1958; Posner et al., 1980). As Palmer (1994) pointed out, one way to instantiate such a theory is with a sample-size model in which perception is developed from several internal samples of sensory representations (e.g., Green & Luce, 1974; Lindsay, Taylor, & Forbes, 1968; Shaw, 1980). The more samples that are devoted to a particular stimulus, the better the perceptual representation of that stimulus.

In contrast, other theorists have proposed a class of models based on the principles of decisional statistical theory (e.g., Eriksen & Spencer, 1969; Palmer, 1994; Shiu & Pashler, 1994; Sperling, 1984). According to these models, it is assumed that nontargets are at least somewhat confusable with the target. Suppose the participant is given the task of determining whether a target, call it $T$, is present in a display. Consider the effect of increasing the number of stimuli in a display. If each element in the display has some nonzero probability of being mistaken for $T$, then the greater the number of elements in the display, the greater the probability of a false alarm (i.e., indicating that $T$ is present when it is not). Now imagine a display with a large number of stimuli that is preceded by a valid cue indicating that the target, if present, will appear in one particular location. To the extent that such a cue can direct the participant to information that is relevant for a decision, information from potentially confusable distractors can be excluded from consideration. This can result in improved performance (compared with a no-cue or invalid-cue condition) without changing the amount or quality of the information extracted from a stimulus. Performance may even be equal to the case in which just a single stimulus was present in the display (e.g., Palmer, 1994). Such models are usually called **decision models**, but Luck, Hillyard, Mouloua, and Hawkins (1996) pointed out that this terminology connotes a late, post-sensory level of selection that is not necessarily appropriate. They argued that such a model can just as well be applied to, say, a retinal ganglion cell. For this reason, they suggested referring to it as the **uncertainty model** because it proposes that performance can be impaired by uncertainty about which sources of information are relevant for a particular decision.

Note that both the resource allocation and the uncertainty models predict that performance will be improved when attention is directed to a location that contains decision-relevant information. Note also that the models are not incompatible; it is possible that both the principles of statistical decision theory and the principle of resource allocation are applicable in perceptual experiments (see also Cheal & Gregory, 1997). Indeed, the logic of most tests between these models is to assume the validity of the uncertainty model and to try to determine whether resource allocation has any discernible effect above and beyond that caused by uncertainty.

Perhaps the most refined tests of the resource allocation model involve the presentation of a single stimulus preceded by a cue that can be either valid, invalid, or neutral. If a poststimulus mask is used, the single stimulus is followed by a single mask in the same location as the stimulus (see Henderson, 1996, and Shiu & Pashler, 1994, for discussions of methodological issues pertaining to masking). If resources can be marshaled and brought to bear at a single location in the visual field, one would expect performance to be better when that location has been cued validly than when it has been cued invalidly or when it has been cued along with all of the other potential locations (i.e., a neutral cue condition). In contrast, if uncertainty reduction is the only operative principle, little benefit from precuing would be expected because, with stimuli well above threshold, there is essentially no uncertainty to resolve when a single stimulus is presented in an otherwise blank field.

The empirical results are somewhat mixed. On the one hand, Shih and Pashler (1994) found that precuing had little effect, which they took as evidence in support of an uncertainty or decisional model. On the other hand, there are several reports of significant precuing effects in the literature (e.g., Bacon, Johnston, & Remington, 1994; Henderson, 1991, 1996; Luck et al., 1996).

Experiments attempting to demonstrate resource allocation in the visual field via cuing differ from one another in a variety of ways. For example, in some the dependent
variable is accuracy; in others it is RT. In some the task is detection; in others it is discrimination. Although it seems too early to try to reach a final answer that will be true in all of the conditions that have been studied, the weight of the evidence does make it reasonable to conclude tentatively that attention to location produces effects on the sensory quality of the representation of a stimulus and in this way differs from attention to other kinds of stimulus features.

Conclusions

The research reported here provides evidence that feature-based attention (e.g., attending to red or to large) does not act to directly enhance the sensory quality of stimuli that possess the attended feature. Instead, it may act to prioritize the allocation of spatial attention, such that items possessing the attended feature are attended before items that do not. It is possible that this marks a fundamental difference between feature-based attention and location-based attention, in that location-based attention may enhance the sensory quality of stimuli in attended locations. This latter claim, however, requires further investigation.

References


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