

Postattentive Vision

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Much research has examined *preattentive* vision: visual representation prior to the arrival of attention. Most vision research concerns attended visual stimuli; very little research has considered *postattentive* vision. What is the visual representation of a previously attended object once attention is deployed elsewhere? The authors argue that perceptual effects of attention vanish once attention is redeployed. Experiments 1–6 were visual search studies. In standard search, participants looked for a target item among distractor items. On each trial, a new search display was presented. These tasks were compared to repeated search tasks in which the search display was not changed. On successive trials, participants searched the same display for new targets. Results showed that if search was inefficient when participants searched a display the first time, it was inefficient when the same, unchanging display was searched the second, fifth, or 350th time. Experiments 7 and 8 made a similar point with a curve tracing paradigm. The results have implications for an understanding of scene perception, change detection, and the relationship of vision to memory.

When people first gaze upon a scene, they see more than they know. Imagine that you are looking for your keys on a jumbled desktop. The keys may be in plain view. Nevertheless, it can take an appreciable amount of time before you find them. You attend to various items that might be the keys until attention falls upon the desired object. Only after attention is deployed to the keys are they recognized as keys. Applying Neisser's (1967) influential formulation to this example, one can say that you perceived something *preattentively* and that you perceived something more (the key) when attention was deployed to the object. What happens when attention is redeployed away from the object? What is the *postattentive* visual representation of an object? The folk psychological notion and the starting point for scientific discussion of this topic hold that a rich visual representation accumulates out of a series of deployments of the eyes and/or of the "spotlight" of attention (e.g., McConkie & Rayner, 1976). This view was summarized (no doubt accidentally) by Dante in Canto 31 of the *Inferno*: "Little by little vision starts picking out shapes that were hidden in the

misty air" (Alighieri, 1994). In this view, as attention does its work, increasing numbers of the objects in a scene come to be concurrently recognized.

The data to be presented here argue for a different view. They indicate that the effects of attention have no cumulative effect on visual perception. Attention to one object after the other may cause an observer to learn what is in a visual display, but it does not cause that observer to see the visual display in any different manner. Objects appear to be recognized one at a time. When the attention that is required for recognition is withdrawn from one object and deployed to another, the postattentive visual representation of that first object becomes indistinguishable from the preattentive state of the object. Intuition holds that a familiar scene is populated by multiple objects, all recognized at the same time. Our data suggest that intuition is wrong. Even in a familiar scene, only the current object of attention is currently recognized.

It is important that the nature of our claim be clear from the outset. We are not denying that the observer may know more about an attended object after attention has been redeployed. We are arguing only that the visual representation of the world does not show cumulative effects of attention. The knowledge that could remain is the sort of knowledge that would remain even if the visual stimulus were removed. Once you attend to the keys, you know that they are keys, and you know their location. That knowledge can be preserved when attention is deployed elsewhere. It can be preserved if you close your eyes, eliminating the visual representation entirely. You could use that knowledge to find the keys again. The claim of this article is that, whatever you may know about those keys, the postattentive visual representation of the keys is indistinguishable from the preattentive representation. It appears that attention alters the perception of an object only while it is directed to that object. Once attention has moved elsewhere, its visual effects melt away.

This article is organized as follows. In the initial section,

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we argue that if the effects of attention are cumulative, then multiple objects should be recognized simultaneously. We then discuss the implications of this argument. In the experimental section, we present a series of experiments that reject the multiple-recognized-objects hypothesis. Instead, they support the hypothesis that repeated and sustained attention to a single display has little or no cumulative effects on the perception of that display. In the first part of the General Discussion section, we discuss the relationship of our findings to the accumulating body of data showing that observers are very insensitive to changes in a display unless attention is specifically directed to the possibility of change of an object (Irwin, 1996; Mack & Rock, 1998a; Rensink, 1998; Simons & Levin, 1997). We then attempt to reconcile the data that show recognition of only one object at a time with our subjective visual experience of a world containing multiple recognized items. Finally, we address issues relating to visual learning, automaticity, and memory search.

What Would It Mean to Recognize More Than One Object?

What happens when an observer attends to an object and recognizes it? Visual recognition of an object must entail an active link between the representation(s) of an object in the visual system and the representation(s) of that object in long-term memory (LTM). Visual representation alone would be vision without recognition. This is possible but pathological: a visual agnosia (e.g., Farah, 1992). An LTM representation could be active in the absence of a visual stimulus and might constitute some nonvisual mental experience of recollection or thought. It might even be a visual image (Kosslyn, 1988), but it would not be visual perception as this term is commonly understood. Thus, one can think about Paris without actually seeing Paris at that moment. If recognition of an object requires a link between representations in vision and LTM, then it would seem to follow that recognition of more than one object must entail more than one active link between vision and LTM. The links become important because independent visual and LTM representations without links should not constitute recognition. For instance, an active LTM representation of Paris and a concurrent, but independent, visual representation of a cat should not lead to the mistaken recognition of the cat as Paris.

One can test for the presence of multiple recognized objects using a visual search paradigm, because search through recognized and unrecognized objects should produce very different results. Visual search through unrecognized items is known to be relatively inefficient. In contrast, if multiple objects are recognized and can be accessed through their representations in LTM, then search through recognized objects should be very efficient because access to information in LTM is very efficient. There may be limits on the capacity to search LTM (e.g., Collins & Quillian, 1969), but that search is very efficient when measured against other comparable searches. For instance, compare LTM search with a limited-capacity search through short-term or work-

ing memory. If an individual is asked to hold words in working memory, the time required for that individual to confirm that a probe word was or was not included increases with memory set size at a rate of about 30–40 ms per word (e.g., McElree & Doshier, 1989; Sternberg, 1969). However, if asked to determine whether *ostrich* is a word, the same individual would answer far too quickly to have searched through the tens of thousands of words in LTM at anything resembling the working memory rate.

Standard visual search is comparable in efficiency to working memory search. The standard visual search experiment presents a novel display on each trial. A substantial body of literature shows that search through these unattended and unrecognized objects proceeds in a relatively inefficient, item-by-item manner at a rate equivalent to one object approximately every 40–50 ms, even for easily identified objects (e.g., *Ts* among *Ls*; Kwak, Dagenbach, & Egeth, 1991; Treisman & Gelade, 1980). Visible objects are not selected for recognition in a completely random fashion in standard visual search. Preattentive feature information can be used to “guide” attention (Wolfe, 1994; Wolfe, Cave, & Franzel, 1989). That is, if the target item is known to be red, preattentive processes can prevent the wasting of attentional resources on items that are not red (e.g., Egeth, Virzi, & Garbart, 1984; Green & Anderson, 1956). Details of preattentive guidance by up to a dozen features were reviewed by Wolfe (1998a). Preattentive guidance helps, but once it has narrowed the search down to the set of, for example, facelike objects, search through those objects proceeds at a rate of about 40–50 ms each (Brown, Huey, & Findlay, 1997; Nothdurft, 1993; Suzuki & Cavanagh, 1995; von Grunau & Anston, 1995).

Now, however, consider the hypothetical case of several simultaneously recognized objects. Perhaps the scene contains an ostrich, an emu, and an aardvark. If there are multiply recognized objects, then each of those visual objects would be linked to its representation in LTM. When the observer is asked whether there is an ostrich in the scene, the *ostrich* representation in LTM should be activated in the efficient manner of LTM search. If the LTM *ostrich* is directly linked to the visual *ostrich*, then the individual should be able to confirm the presence of an ostrich in a time that is not dependent on the number of other items in the visual stimulus. The observer would find that the *ostrich* node in LTM is directly linked to something in the visual representation. If asked about an antelope, the observer would find no link from LTM to the visual stimulus. This should produce a fast no response in the same way that individuals can respond no quickly if asked whether *norp* is a word. One can conclude, therefore, that if multiple items are recognized, search should be an efficient by-product of LTM access. If not, search will be an inefficient visual search. If an account proposes a limitation on this LTM access, such as cross talk between vision–LTM links, then it is proposing a limit on the ability to simultaneously recognize multiple visual objects.

In addition to inefficient search through the visual stimulus and efficient access via LTM, there is a third way for individuals to respond to queries about the contents of an

attended scene. With attention to the scene, some account of ostrich, emu, and aardvark will be stored in a limited-capacity working memory. An individual, queried about *ostrich*, could search working memory for the answer. As just noted, that search will proceed in a relatively inefficient manner. Although it might prove difficult to distinguish inefficient working memory search from inefficient visual search, either should be easily distinguished from the efficient search predicted if multiple objects are recognized at one time. The experiments described subsequently were intended to distinguish between these alternative accounts of postattentive vision. The results will falsify the predictions made by the hypothesis that observers can recognize more than one object at a time.

Experiment 1: Previewing the Search Stimulus

In a visual search task, observers search for a target item among a number of distracting items. The usual dependent measures are reaction time (RT; the amount of time required to respond yes or no) and accuracy. In RT studies, a favorite measure is the slope of the function relating RT to the number of items included in the display (set size). The slope of the RT \times Set Size function is a measure of search efficiency (for reviews, see Treisman, 1986; Wolfe, 1997, 1998a). How much additional time is required for each additional item in the display? In the most efficient search tasks, the slopes are near zero, suggesting that the target item can be found with little or no interference from the distracting items. Examples would include many searches for targets defined by single basic features (e.g., a red dot among green, vertical among horizontal, or big among small). In less efficient tasks, each additional item imposes a substantial cost. Slopes of 20–30 ms per item on target-present trials and 40–60 ms per item on target-absent trials would be typical of inefficient search for items that can be processed without the need for foveating eye movements. Examples would include a search for a rotated *L* among rotated *T*s (Humphreys, Quinlan, & Riddoch, 1989) and a search for a smiling face among frowns (Nothdurft, 1993; Suzuki & Cavanagh, 1995). Different search tasks produce different slopes falling on a continuum of efficiency (Wolfe, 1998a, 1998b).

There are a variety of theoretical approaches to understanding variation in slopes. The most efficient slopes are generally held to indicate that all items are processed in parallel without any relevant capacity limitation. Steep slopes are often held to reflect the need to attend to individual items one at a time (serial search; Treisman & Gelade, 1980). Alternatively, all items could be processed in parallel but in a noise-limited or capacity-limited manner that results in RT being dependent on set size (e.g., Bundesen, 1990; Duncan & Humphreys, 1989; Grossberg, Mingolla, & Ross, 1994; Palmer, 1994, 1995; for a review, see Bundesen, 1996). Models such as feature integration (Treisman, 1988) and guided search (Wolfe, 1994) propose that serial attention is needed to bind features to objects. That is, before the arrival of attention, the visual system may know that an item has the features red, green, vertical, and

horizontal but will not be able to determine whether red goes with vertical or with horizontal. Nor would it know whether the vertical and horizontal parts formed a *T* and *L* or something else (Wolfe & Bennett, 1997). Parallel processing models may not make this binding assumption, but they generally assume that some sort of information is accumulating over time that eventually makes it possible to respond to the presence or absence of the target (see Ratcliff, 1978, for a clear description of this class of model, albeit in the context of memory).

Thus, both serial and parallel models assume that something is happening over time to the representation of the visual stimulus. In a standard visual search experiment, the story ends with the observer's response. At that point, the visual stimulus vanishes, and a new stimulus appears. In the real world, this does not happen. For example, you sit down to dinner. You attend to the plate. You shift attention to your drink, then to the salt, and then back to your plate. If you are searching for the pepper, you are searching through a scene that is already familiar. Many if not all of the objects in that scene will have received attention. Does this matter? Is visual search through attended objects more efficient than visual search through new and thus unattended objects?

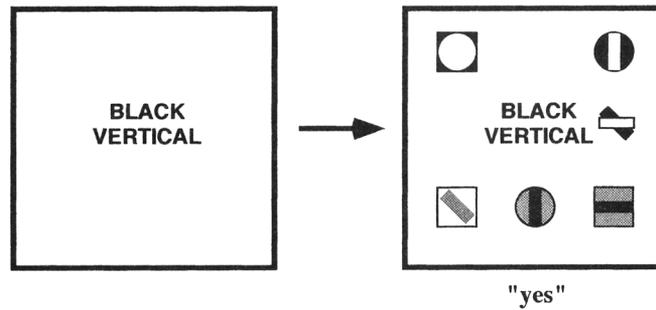
In Experiment 1, this issue was addressed by varying the time between the appearance of the "scene" (here, the search display) and the announcement of the identity of the target for that trial. In standard search, the target is identified for an entire block of trials and is, thus, known long before the appearance of the stimulus. In this experiment, a new target probe was presented on each trial. If it appeared after the search stimulus had appeared, one might expect the preview of the stimulus to permit more efficient search.

Method

The basic method of Experiment 1, shown in Figure 1, was quite similar to Pashler's (1984) study of preview effects. The search display consisted of 4, 5, 6, 7, or 8 items presented in a 3×3 array. No items were permitted at the center of the display. The center-to-center distance between items was 4.1° . Items were double conjunction stimuli, such as a red circle with a yellow vertical line on top of it. The target probe was a written description of a conjunction (e.g., "red circle") presented at the center of the screen. This eliminated any physical match between the target and the probe. The five shapes used were circle, square, vertical line, horizontal line, and oblique line. Seven colors were used: red, purple, white, green, gray, yellow, and blue. All pairings were possible. No conjunction of color and shape occurred more than once in any display. Nor did any conjunction of two shapes occur more than once in any display. These stimuli were chosen because previous research suggested that search for any specific conjunction of a color and a shape in this sort of display would be inefficient (Wolfe & Bennett, 1997).

Participants saw each search display only once. The critical variable in this experiment was the stimulus onset asynchrony (SOA) between the presentation of the target probe words (e.g., "white circle") and the presentation of the search display. Negative SOAs indicate that the target probe appeared first, whereas positive SOAs indicate that the search stimulus appeared first. The positive SOAs, therefore, represent the "postattentive" conditions, the

NEGATIVE SOA: Target probe appears before search stimulus.



POSITIVE SOA: Search stimulus appears before target probe

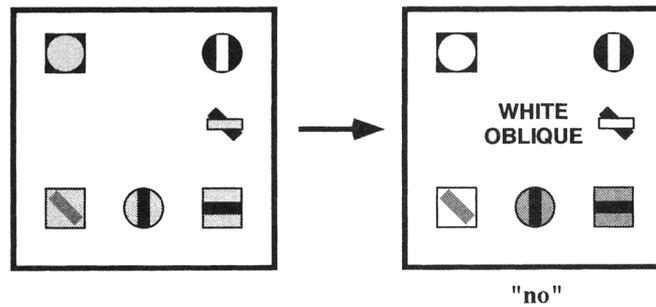


Figure 1. Stimuli for Experiment 1. Is search more efficient with positive SOAs, when the participant can attend to the stimuli before knowing the target? SOA = stimulus onset asynchrony.

conditions in which previous attention to the display could make search more efficient.

Two ranges of SOAs were tested in separate but otherwise identical versions of the experiment. In the first version of the experiment, SOAs were -60 , -40 , -20 , 0 , 20 , 40 , and 60 ms. Search displays had set sizes of 4, 5, 6, 7, or 8 items. Participants were tested for 1,000 trials; SOA and set size were randomly chosen on each trial. In the second version of the experiment, the range of SOAs was expanded to -100 , 0 , 100 , 200 , and 300 ms. Set sizes were 4, 6, and 8. Again, participants completed 1,000 trials. Experiments were run on Macintosh computers with MacProbe software (Hunt, 1994). Ten participants were tested in each version. All had normal or corrected-to-normal acuity and could pass the Ishihara color test. All gave informed consent and were paid for their time. RTs were timed from the point at which both search stimulus and target probe were visible.

Results

Figure 2 shows the average RT and slope results for both versions of Experiment 1. Averages and slopes were based on log RT to decrease the influence of RT outliers and to compensate for the usual positive skew of the RT distribution.

In regard to RTs, there was a clear pattern of variation with SOA that was seen with most participants. RTs were shortest for SOAs less than 0 ms (probe precedes search display), longer for SOAs greater than 0 ms (search display precedes probe), and longest for SOAs of 0 ms. Analyses of variance (ANOVAs) showed a significant effect of SOA on RT for target-present and target-absent trials in both versions of the experiment: small SOA version, $F_s(6, 54) > 35$, $p_s < .001$, in both cases, and large SOA version, $F_s(4, 36) > 38$,

$p_s < .001$, in both cases. SOAs could be grouped into three categories: those less than, equal to, or greater than 0 ms. All of the differences between these groups were highly significant according to unpaired t tests. By contrast, there were no reliable changes in efficiency of search as measured by the slopes of the $RT \times$ Set Size functions. ANOVAs examining the effect of SOA on slope yielded F values below 1.0 for target-present and target-absent trials in both versions of the experiment.

Errors were low except for 1 participant who had approximately 15% errors. Removal of that participant did not change the pattern of RT and slope results. Error rates for the remaining participants are shown in Table 1 as a function of SOA and set size. There was little effect of these variables. What effect there was mimicked the RT results, with slightly larger numbers of errors at the longest SOA and at the higher set sizes.

Discussion

For the purposes of this article, the important result is that there was no evidence that preview of the stimulus provided any advantage in these two experiments. RT performance was worse with the positive SOAs than with the negative SOAs. One may assume that this merely reflects the fact that the time required to read the probe words would have been part of the positive SOA RTs. With negative SOAs, participants could begin to read the probe before the RT timer started with the onset of the search stimuli. When the SOA was 0 ms, the words were presented simultaneously with the surrounding ring of stimuli. This produced the longest RTs,

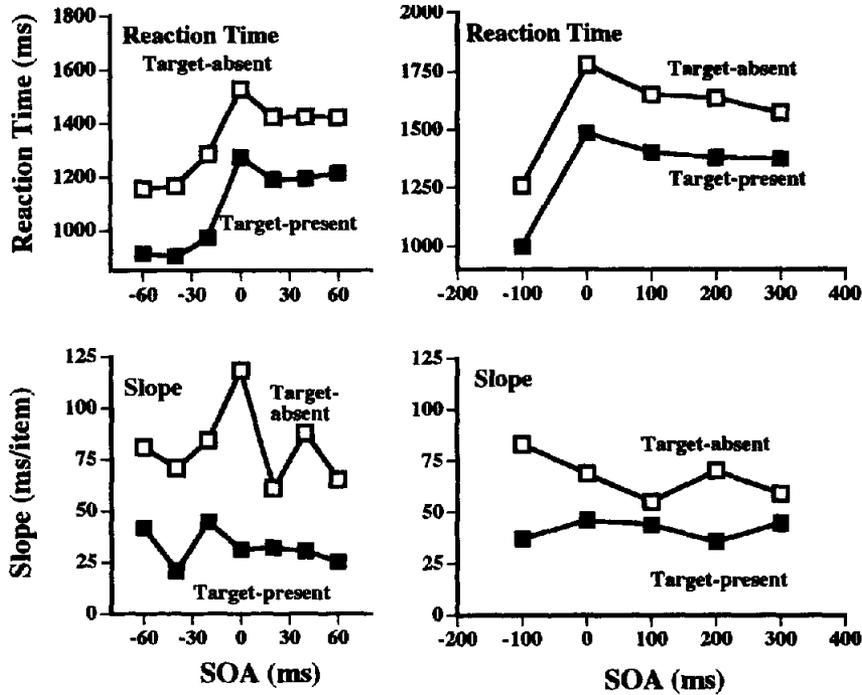


Figure 2. Reaction times and slopes as a function of stimulus onset asynchrony (SOA) for the two versions of Experiment 1.

probably because of some form of lateral masking. Again, there was no reliable impact on search efficiency. The details of any masking effects are tangential to the interests of this article, although this artifact of the simultaneous appearance of probe and surrounding search stimuli appears again in other experiments.

Of more interest for present purposes is our failure to find any improvement in the slope measure of search efficiency with positive SOAs. Target-present slopes were in the neighborhood of 25–40 ms per item. In a serial model, this means that one of these items could be processed every 50–80 ms (or every 25–40 ms if there is no marking of rejected items in search; see Horowitz & Wolfe, 1998). If one assumes that participants attended to the search display during the preview, one might expect some benefit at least at

the longest SOAs. However, there was no such effect on the slopes of the RT × Set Size function. Given that this study was very similar to that of Pashler (1984), it is encouraging that the results are similar to Pashler’s results showing no effect of preview on search efficiency (see also Pashler, 1994).

Of course, even 300 ms is a fairly minimal preview of the search stimuli. The subsequent experiments described in this article provided longer and longer periods of exposure to search stimuli. To anticipate the results, in none of the experiments did previous attention to the search stimulus make search efficient. None of these experiments, alone, is adequate to make the case against a cumulative effect of visual attention. When the experiments are taken as a whole, however, it seems difficult to avoid the conclusion that there is little or no cumulative effect.

Table 1
Error Rates for Experiment 1

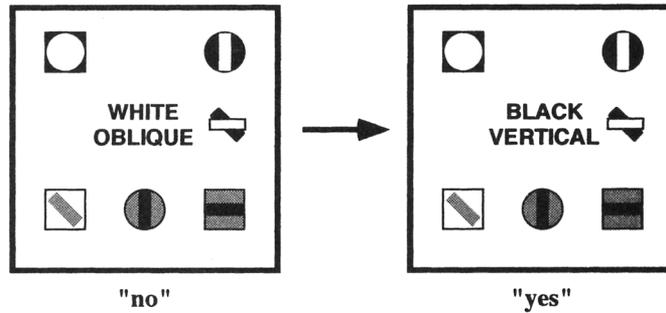
SOA (ms)	Error rate	Set size	Error rate	SOA (ms)	Error rate	Set size	Error rate
-60	.034	4	.0404	-100	.052	4	.047
-40	.045	5	.0383	0	.059	6	.061
-20	.044	6	.0378	100	.052	8	.061
0	.043	7	.0489	200	.045		
20	.037	8	.049	300	.074		
40	.049						
60	.047						

Note. These averages exclude the data from 1 participant with high error rates. Including this participant adds roughly 1% to each error value. SOA = stimulus onset asynchrony.

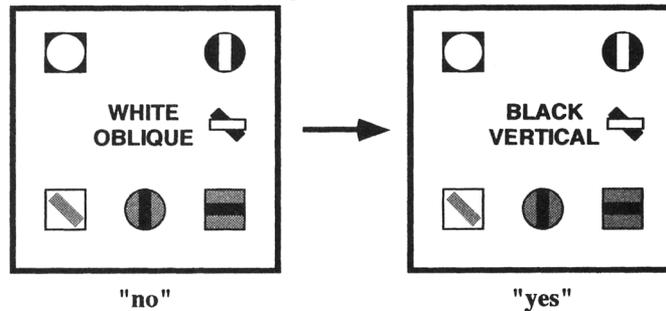
Experiment 2: The “Repeated Search” Method

In Experiment 2, exposure and attention to the search stimulus were extended by having participants search repeatedly through the same stimulus. This *repeated search* method is illustrated in Figure 3 (top panel). The search stimulus remains statically present on the screen. Only the probe, indicating the target, changes from trial to trial. Thus, on the first trial illustrated in Figure 3, the participant searches for a white oblique. No white oblique is present. Presumably, the participant must search, in series or in parallel, to determine that the correct answer is no. On the second trial, the target is a black vertical, and the answer is yes. The central question is whether search is any more

REPEATED SEARCH: only probe changes between frames



"FLOOR" CONDITION: Target, if present, is at cued location.



"CEILING" CONDITION: Stimuli and target probe change every trial.

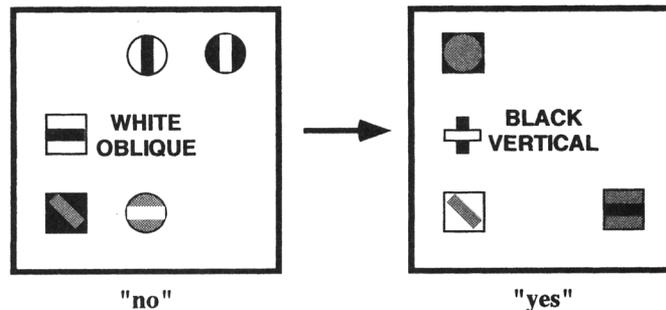


Figure 3. Stimuli for the repeated search condition and two control conditions in Experiment 2.

efficient on Trial 2 as a result of the attention paid to the stimulus in Trial 1. Of course, participants can search through the same stimulus again and again. In this experiment, participants performed five repeated searches of each search stimulus.

Two control conditions were used to establish "floor" and "ceiling" baselines for the best and worst performances that might be expected in the repeated search condition. In the floor condition (Figure 3, middle panel), a white box cue appeared around one element in the search array at the same time that the target probe appeared. If a target was present on that trial, it was present at the cued location. No search of the display would be required. Suppose that, in the repeated search condition, repeated deployments of attention form multiple links to stimuli represented in LTM. In that case, activation of a "white oblique" node in memory should be linked to the "white oblique" item in the visual stimulus, and, again, no search should be required. The box cue in the

floor condition was intended to simulate a maximally effective link between vision and memory.

In the worst performance ("ceiling") condition (Figure 3, bottom panel), the search stimuli were changed every trial as they would be in a standard visual search task. Information from the previous trial would be irrelevant to the next trial, and thus no postattentive benefit could be seen.

Method

Stimuli were identical to those used in Experiment 1. Set sizes were 4, 5, 6, 7, and 8.

On the first repetition, the target probe, the search display, and the cue (if present) all appeared simultaneously. Note that Experiment 1 suggests that this will elevate RTs but should not have any impact on search efficiency as measured by slopes. Feedback was given between trials. Its 500-ms duration served as an intertrial interval (ITI) between a participant's response and the presentation

of the next probe. The search stimulus remained visible during the interval. Participants completed 1,000 trials (5 repetitions × 200 blocks) of the repeated search and floor conditions and 500 trials of the ceiling condition.

Fifteen participants between 18 and 37 years of age were tested. All gave informed consent, had better than 20/25 acuity, and passed the Ishihara color vision test. All were paid for their time.

Results

As in Experiment 1, the RTs in this task were positively skewed but were approximately normally distributed following a log transformation. Accordingly, data presented are the anti-logs of the means of log RT. In practice, this yields essentially the same result as using median RT. RT × Set Size functions for target-present trials are shown in Figure 4. The pattern of target-absent data was similar. Effects of repetition on RT and on slopes are summarized in Figures 5 and 6. Error rates are summarized in Figure 7.

Several aspects of the data presented in Figure 4 are worth further examination. First, the repeated search function for Repetition 1 was clearly slower and steeper than the functions for Repetitions 2–5. Results for Repetition 1 resembled the results for the ceiling condition. This makes sense, because all ceiling condition trials were Repetition 1 trials. Repetitions 2–5 produced somewhat better results. This would seem to be evidence for a postattentive advantage. However, note that the floor condition also produced results for Repetition 1 that were clearly slower and steeper than Repetitions 2–5 of that condition. Whatever was hindering search on the first repetition of the repeated search condition seemed to be having a similar effect on the floor condition. Note that this experiment involved a 0-ms interstimulus interval, meaning that the target probe and the search stimulus appeared simultaneously on Repetition 1 but not on subsequent repetitions. Recall that 0-ms SOAs produced elevated RTs in Experiment 1. To anticipate the next few experiments, when this 0-ms SOA effect was avoided, the penalty on Repetition 1 vanished. Note also that the results for the repeated search condition were always slower and less efficient than the results for the floor condition. These points are further illustrated in Figures 5 and 6.

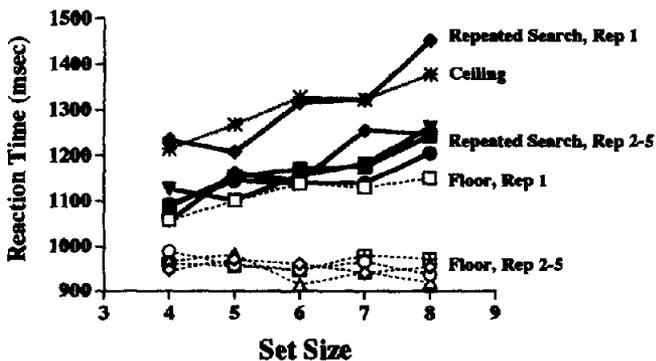


Figure 4. Reaction Time × Set Size functions for Experiment 2. Rep = repetition.

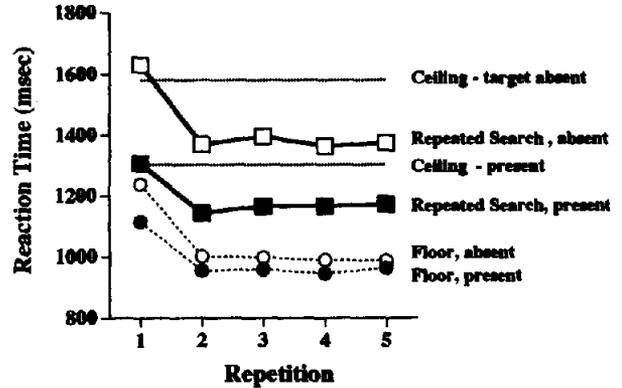


Figure 5. Reaction times (averaged over set size) for Experiment 2.

Figure 5 shows average RTs for all participants in the various conditions of Experiment 2. RT was elevated at Repetition 1. Again, Experiment 1 suggests that this was due to the 0-ms SOA that was present only on Repetition 1. Of more importance are the slopes of the RT × Set Size functions shown in Figure 6. An ANOVA on individual participants' slope data showed that there was a significant difference between repeated search and floor condition slopes, $F(1, 14) = 70.0, ps < .001$, for target-present and target-absent cases. There was no significant difference between the repeated search slopes and the slope of the ceiling condition for either target-present trials, $t(74) = 0.29, p > .75$, or target-absent trials, $t(74) = 1.42, p > .15$.

The apparent decrease in slope as a function of repetition in the repeated search condition was not a statistically reliable effect. In an ANOVA, the main effect of repetition almost reached the .05 level of significance for target-present data, $F(4, 56) = 2.45, p = .06$, but was not significant for target-absent data, $F(4, 56) < 1$. Examination of the change from the first to the second repetition reveals that the difference was significant for target-present trials, $F(1, 14) = 6.54, p < .05$ (although this was not corrected for multiple comparisons), and insignificant for target-absent trials, $F(1, 14) = 2.39, p > .05$. The effect of repetition was significant

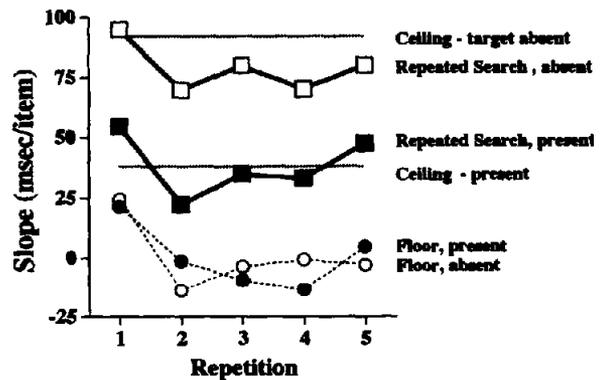


Figure 6. Slopes of the Reaction Time × Set Size functions for Experiment 2 as a function of repetition.

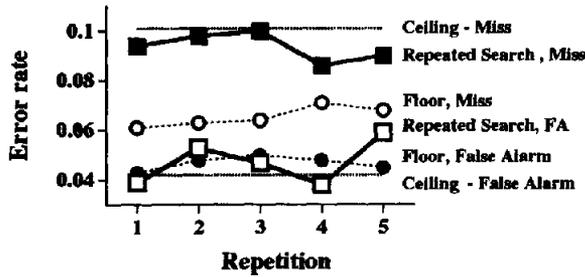


Figure 7. Error rates for Experiment 2 as a function of repetition, condition, and error type.

for the floor condition trials, $F_s(4, 56) > 4.0$, $p_s < .01$, for target-present and target-absent trials.

Error data are shown as a function of condition and repetition in Figure 7. There was no effect of repetition on error rate.

Discussion

In Experiment 2, there was some weak evidence in favor of an improvement in search performance as a function of exposure to the search stimulus. Specifically, RTs on the first search through the stimulus were longer than RTs on the second and subsequent searches. Slopes showed the same trend, but not reliably. However, very much the same pattern of results was seen in the floor condition. In this condition, only the cued item was relevant, and search was not needed. This suggests that the improvement from Repetition 1 to Repetition 2 was due to some factor unrelated to search efficiency. The effects of simultaneous onset of the target probe and the search display are probably to blame, as indicated in the 0-ms SOA data of Experiment 1. In any case, the efficiency in the repeated search leveled off at a rate similar to that of classic "serial" searches and never approached the efficiency of the floor condition. In the floor condition, the cue informed the participant where to direct attention. No effect of previous attention provided similar information in the repeated search condition.

Some objections might be raised to Experiment 2. First, there is the matter of the 0-ms SOA on Repetition 1. Second, the compound conjunction stimuli illustrated in Figure 3 might be too complex or too novel to show postattentive benefits. Third, the need to read a two-word description of the target on each trial might have required some sort of task switching that disrupted the internal visual representation. Experiments 3 and 4 replicated the basic findings of Experiment 2 with enough variations in methodology to address these concerns.

Experiment 3: Replication of the Repeated Search Method

A variant of the repeated search method is shown in Figure 8.

Method

Figure 8 shows an example with stimuli that can be described as simple (rather than compound) conjunctions of color, orientation, and size. Starting with Repetition 1, the item in the circle at the center of the display is the target probe. Thus, on this first repetition, participants are searching for a "big, black, vertical item." In the actual experiment, an item could be either big or small; could be vertical, horizontal, or oblique; and could be red, green, yellow, or blue. This generated a set of 24 possible items. Big items were $2.4^\circ \text{ long} \times 1.0^\circ \text{ wide}$. Small items were $1.5^\circ \times 0.5^\circ$. The x, y CIE coordinates of the colors were as follows: .62, .34 for red; .26, .51 for green; .42, .50 for yellow; and .19, .25 for blue. The luminances (cd/m^2) were 15.8 for red, 23.8 for green, 69.7 for yellow, and 27.9 for blue. Each trial consisted of five separate searches through the same display. On each trial, a random set of either 3 or 6 of 24 possible items was presented to the participant. The center of each object was situated on an invisible circle of radius 2.6° . The target probes, presented in the circle at the center of the display, were drawn from the same set. On 50% of the trials, the probed item was present in the search display. Participants responded by pressing one key if the target was present and another if the target was absent. RT and accuracy were recorded. Accuracy feedback was given by coloring the central circle after each repetition (blue for correct responses and red for incorrect responses). Participants completed 20 practice trials and 100 test trials (or $5 \times 20 = 100$ and $5 \times 100 = 500$ total searches).

The first test probe appeared 300 ms before the onset of the search display. Subsequent probes appeared 50 ms after the previous response had been recorded. The search display remained continuously visible with no change during the five search repetitions. We used the 300-ms preview of the test probe to prevent the interference effects seen in the RTs for 0-ms SOAs in Experiment 1 and, probably, Experiment 2. Note that preview of the target probe results in Repetition 1 being a bit more similar to a standard search trial in which the participant knows the target but not the stimulus before the start of the trial. Note also that the 300-ms preview had no impact on search efficiency in Experiment 1. Ten participants, 21 to 48 years of age, took part. All had normal or corrected-to-normal vision and passed the Ishihara color test. Participants gave informed consent and were paid for their time.

Results

Figure 9 shows the average results based on averaging log RT data for individual participants and then averaging the anti-logged results across participants. The top panel of

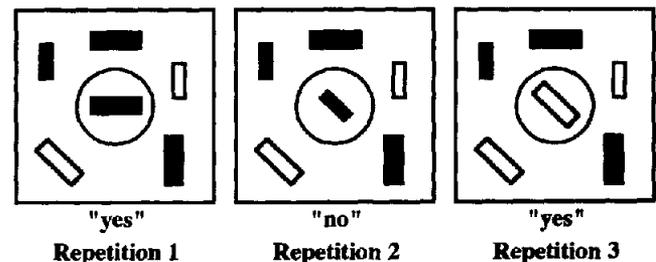


Figure 8. Variant of the repeated search method. Here observers search displays for the presence of the probe item in the central circle. On successive repetitions, only the probe changes. The surrounding search display is static.

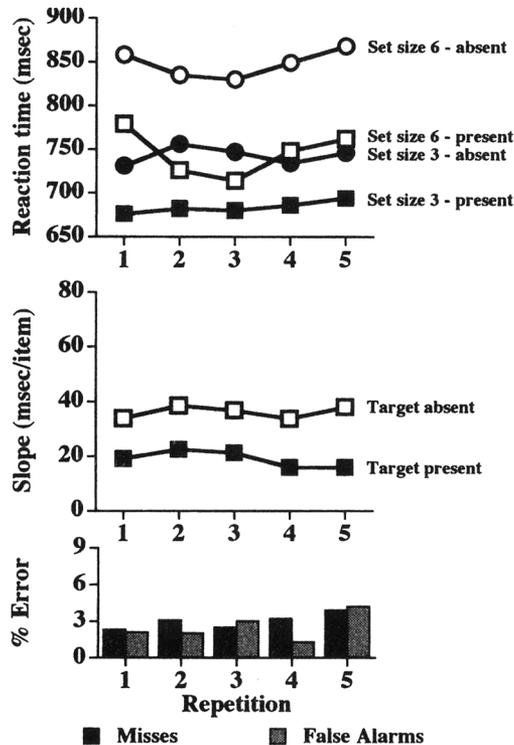


Figure 9. Mean reaction times, slopes, and errors as a function of repetition in Experiment 3: Conjunction stimuli.

Figure 9 shows RTs for each set size (3 and 6) as a function of repetition. The middle panel shows the slopes of the RT \times Set Size functions derived from the data in the top panel. The bottom panel shows error rates as a function of repetition. Error rates were low, and there was no evidence of any improvement in accuracy with repetition.

Repetition 1 can be thought of as a fairly standard conjunction search with high distractor heterogeneity producing relatively inefficient search (Duncan & Humphreys, 1989). The important point to take away from this experiment is that there was no evidence of any improvement in visual search efficiency as a function of repetition. Search was inefficient on Repetition 1 and was just as inefficient on Repetition 5.

These conclusions are supported by ANOVAs run separately on target-present and target-absent trials. Only correct trials were analyzed. The few, very large outlier RTs were eliminated by removing RTs that were more than 3 standard deviations above the mean. This eliminated 1% of the data. For target-present trials, there was no significant effect of repetition, $F(4, 36) = 1.58$, *ns*. There was a highly significant effect of set size, reflecting the inefficiency of the search, $F(1, 9) = 69.7$, $p < .001$. The interaction of repetition and set size was not significant, indicating that the slope of the RT \times Set Size functions did not change with repetition, $F(4, 36) < 1$, *ns*. The pattern was the same for target-absent trials: repetition, $F(4, 36) < 1$, *ns*; set size, $F(1, 9) = 69.7$, $p < .001$; and Repetition \times Set Size, $F(4, 36) = 1.15$, *ns*. Perhaps all of the effect of repetition

occurred between Repetitions 1 and 2. This hypothesis could be tested by restricting the analysis to only those repetitions. This reanalysis did not change the results. Nor did pooling target-present and target-absent trials into a single analysis change the results.

Discussion

Experiment 3 failed to show any improvement in search efficiency over the course of five repeated searches of the same display. Except for an anomalously low target-absent trial slope at Repetition 1, blank trial slopes were roughly twice target trial slopes. Across repetitions, these slopes were similar to other searches deemed to be "serial" (Wolfe, 1998b). The slopes might be surprisingly inefficient for conjunction search given that there is extensive evidence for more efficient, "guided search" for conjunctions (e.g., Dehaene, 1989; Egeth et al., 1984; Nakayama & Silverman, 1986; Quinlan & Humphreys, 1987; Wolfe et al., 1989). However, those more efficient results were obtained with a constant target and changing search displays. When target identity changes from trial to trial, inefficient search is the rule (e.g., the inconsistent mapping studies of Schneider & Shiffrin, 1977, and Shiffrin & Schneider, 1977). Moreover, as noted earlier, heterogeneous distractors decrease the efficiency of search (Duncan & Humphreys, 1989).

Note that the RT penalty on Repetition 1 observed in Experiment 2 was not seen in Experiment 3, suggesting that methodological factors and not the cumulative effects of attentional deployment were responsible for that effect in Experiments 1 and 2. Perhaps conjunctions of basic features such as color, size, and orientation represent a uniquely bad choice of stimuli. After all, one does not routinely search for meaningless combinations of these features. In Experiment 4, participants performed repeated searches for letters and closed curve shapes.

Experiment 4: Repeated Search for Letters and Shapes

Method

Experiment 4 was essentially identical to Experiment 3. Only the stimuli changed. In Experiment 4a, stimuli were the set of 26 uppercase letters (48-point Geneva font). In Experiment 4b, the stimuli were drawn from a set of 1,200 meaningless closed curves described in Wolfe (1997). These were black outlines on a white background. Ten participants took part.

Results

Letter stimuli. Figure 10 shows the results for the letter stimuli. There was no change as a function of repetition for target-present trials. Although RTs and slopes declined somewhat on target-absent trials, almost all of this effect was due to a single participant.

These impressions of the data were borne out by an ANOVA. As before, only correct trials were analyzed, and RTs greater than 3 standard deviations above the mean were removed. For target-present trials, there was no significant

effect of repetition, $F(4, 36) < 1$, *ns*; a highly significant effect of set size, $F(1, 9) = 141$, $p < .001$; and no significant interaction of repetition and set size, $F(4, 36) < 1$, *ns*, indicating no reliable change in slope with repetition. The target-absent trials showed a weakly significant effect of repetition, $F(4, 36) = 2.85$, $p < .05$; the usual strong effect of set size, $F(1, 9) = 40.6$, $p < .001$; and an insignificant interaction of repetition and set size, $F(4, 36) = 1.2$, *ns*. Looking at only the first two repetitions, the effect of repetition on blank trials failed to reach significance, $F(1, 9) = 2.8$, $p > .05$.

Meaningless object stimuli. The meaningless closed curve data are shown in Figure 11. In this case, there appeared to be some decline in RT and slope with repetition. Some of this effect was due to 1 participant who performed searches that were much slower and more inefficient than those of the other participants. She was particularly slow on the first repetition and may have been fixating each item in turn.¹ Again, an ANOVA was performed on the correct trial RTs. For target-present trials, there was a significant effect of repetition, $F(4, 36) = 5.1$, $p < .01$; a highly significant effect of set size, $F(1, 9) = 62$, $p < .001$; and no significant interaction of repetition and set size, $F(4, 36) = 1.5$, *ns*. The target-absent trials showed no significant effect of repetition, $F(4, 36) = 1.2$, *ns*; the usual strong effect of set size, $F(1, 9) = 175$, $p < .001$; and an insignificant interaction of repetition and set size, $F(4, 36) < 1$, *ns*. Restricting the ANOVA to the first two repetitions produced the same pattern of results.

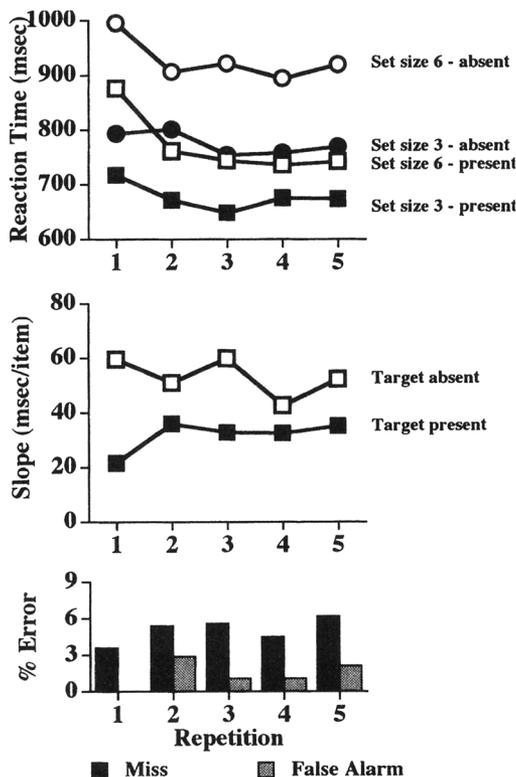


Figure 10. Mean reaction times, slopes, and errors as a function of repetition in Experiment 4a: Letter stimuli.

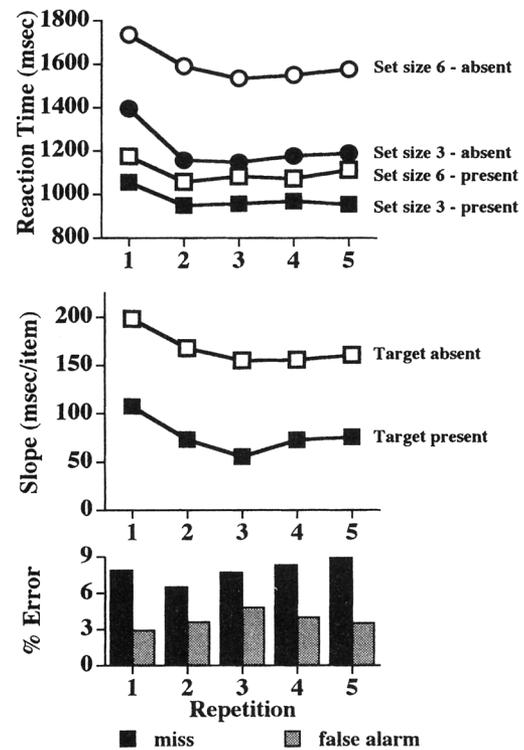


Figure 11. Mean reaction times, slopes, and errors as a function of repetition in Experiment 4b: Meaningless object stimuli.

Discussion

Three different sets of search stimuli produced quite similar results in the repeated search paradigm. There were no dramatic changes in search performance with repetition. The only significant changes related to repetition involved the RT results. Changes in the slope would be reflected in the interaction of repetition and set size variables. These variables never reached significance. Of course, failure to detect a change could simply mean that we did not look hard enough. The object data shown in Figure 11 do suggest declines in RT and slope with repetition. Perhaps the results would have been significant if the experiment had had greater power (e.g., via more participants or more trials). For the sake of argument, suppose that the apparent declines revealed in Experiment 4b are real. Note that this "improvement" in search efficiency never brought the target-present slope below 50 ms per item or the target-absent slope below 150. This search remained very inefficient after five searches through the same stimulus.

Experiments 3 and 4, taken together, show that the failure to find significant improvements in search efficiency in Experiment 2 was not a simple artifact of the stimuli used in that experiment. The same results were obtained with

¹ This was the same participant who produced most of the same effect in Experiment 4a, and her error rates were relatively high. We would have eliminated her from the data set, but eliminating participants with uncomfortable results seemed like a slippery slope.

simpler conjunction stimuli (Experiment 3), objects that were defined by shape (Experiment 4b), and overlearned stimuli (in this case, letters; Experiment 4a). The letter results are particularly significant. It might be objected that none of the other stimuli necessarily had representations in LTM. Perhaps novel shapes or conjunctions fail to show evidence of multiple links to LTM because there is nothing to link to. Presumably, this is not the case with letters. Letters must be well represented in LTM, and yet the fifth search through a small set of letters was no more efficient than the first. Pilot data involving "natural" (clip-art) objects produced the same pattern of results.

There remain a number of possible objections to the design of these experiments. First, in Experiment 2, the first repetition trial was different from Repetitions 2–5. In Experiments 3 and 4, the first test probe appeared before the initial presentation of the search display. This eliminated the 0-ms SOA problem of Experiment 2, but the preview of the central test probe may have given Repetition 1 an unfair RT advantage. Second, feedback was given between repetitions. Interpreting the feedback could add a processing load that would slow all but the first RT. Third, in Experiments 3 and 4 (but not in Experiment 2), the new test probe appeared very quickly (50 ms) after the response to the preceding repetition. Short ITIs of this sort can produce a speed–accuracy trade-off (e.g., Wilkinson, 1990). Given that accuracy remained high, this might cause an increase in all RTs except the first. Finally, in Experiments 3 and 4, the probe at the center of the screen was physically identical to the target in the search display (if present). This eliminated the need to read the target probe as in Experiment 2, but perhaps participants were performing some sort of grouping or matching of the two identical items rather than a standard visual search.

One can imagine any of the first three objections adding a constant to the RTs of Repetitions 2–5 or (in the case of the first objection) subtracting a constant from Repetition 1. It is hard to see why any of these cases would have the effect of keeping a search inefficient that would have otherwise become efficient with repeated search. Nevertheless, it is worth eliminating these variables as sources of concern. Experiment 5 was a modification of the repeated search task designed to eliminate the special status of the first repetition.

Experiment 5: Targets of Different Age

Method

A modified repeated search task is shown in Figure 12. The basic task was the same as before. Participants determined whether the search display contained the target shown as the probe letter at the center of the display. In this version, the probe was presented in lowercase Times font, and the search display was presented in uppercase Helvetica bold. This modification addressed the objection regarding probe–target matching (although, for example, an *x* and an *X* remain very similar).

The first trial was a standard letter search trial. On half of the subsequent trials, one of the letters was replaced by a new letter. Thus, in Trial 2 in Figure 12, the *V* is replaced by a *P*; in Trial 3, the *F* is replaced by a *T*. Each letter in the search display was moved by

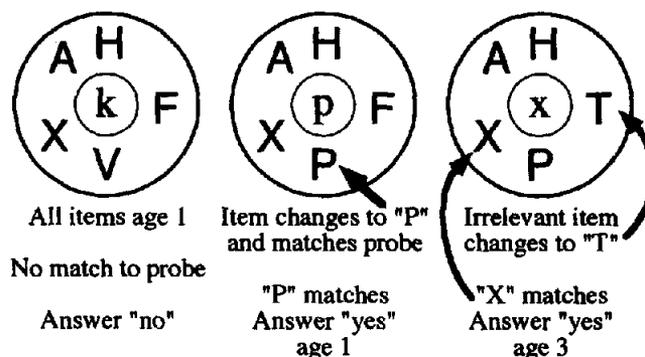


Figure 12. A modified repeated search task.

a small fraction of the letter width. Otherwise, transients accompanying the presentation of a new letter might attract attention (Yantis, 1993; Yantis & Johnson, 1990). It is unlikely that the jiggle caused each repetition to be regarded as a completely new display. The subjective appearance was of a set of *N* objects in motion. Visual search in moving stimuli is certainly possible (Dick, Ullman, & Sagi, 1987; Horowitz & Treisman, 1994; McLeod, Driver, Dienes, & Crisp, 1991), and moving stimuli are generally not treated as new objects (Hillstrom & Yantis, 1994).

With this method of replacing single items during a series of searches through an otherwise unchanging display, every letter in the display can be described in terms of its "age." Age is measured as the number of repetitions in which a letter has been visible. Thus, all of the letters in Trial 1 have an age of 1. In Trial 3, *T* has an age of 1, *P* has an age of 2, and *A*, *H*, and *X* have an age of 3. Using this paradigm, we could measure RT as a function of the age of the stimulus. This eliminated the special status of Repetition 1, because targets of age 1 could appear at any point in the sequence of trials.

No feedback was given between repetitions (in response to the second objection outlined earlier), and there was a 500-ms interval between the response to trial *N* and presentation of the target probe for trial *N* + 1 (in response to the third objection). On target-present trials, each element in the display had an equal chance of being the designated target.

After 30 practice searches, each participant completed two sets of 800 trials, one with set size 3 and the other with set size 5. All locations were reset to age 1 after 25 trials. Because target-present trials were the trials of interest in this paradigm, targets were presented on 60% of trials. Twelve participants, 24 to 49 years of age, took part. All had normal or corrected-to-normal vision. All gave informed consent and were paid for their time.

Results

Data from 1 participant were excluded from analysis because she used the incorrect response key for 800 trials. Two other participants had data sets that were corrupted, leaving 9 participants. Because of the issues surrounding the first appearance of the stimulus, the first trial of each block was eliminated from analysis. In this way, targets of age 1 came from trials that were no different from any other trial.

Ages of target items were not distributed uniformly. The rule for changing letters made targets of age 1 more common than targets of age 2, and so forth. There were 1,360 trials in which the target was of age 1 and 1,086 trials in which the target was of age 2; this fell to 403 by age 7. Mean RTs and

slopes become less stable as the number of trials drops, and so analysis was restricted to target ages of 7 or younger. Means of log RTs were obtained for each participant on target-present trials of these ages (target age was meaningless for target-absent trials). The anti-logs of these means were averaged, and the results are displayed in Figure 13 (top), along with slopes derived from these values. Error rates are shown at the bottom of Figure 13.

An ANOVA showed no significant effect of the age of the target on the RTs for either set size 3 or set size 5, separately or taken together, $F_s(6, 48) < 2$ in all cases. The interaction of age and set size (providing a measure of slope change) was likewise not significant, $F(6, 48) = 1$. The main effect of set size was highly significant, $F(1, 8) = 16.5, p < .001$. Thus, as in the previous experiments, the inefficient search showed no sign of becoming significantly more efficient with repetition.

Target-absent trials could not be analyzed by age of the target. Instead, the data were examined for an effect of the presence or absence of a new item in the display. On 66% of target-absent trials, one of the letters was new. Were these trials systematically different from the trials on which all of the items remained the same? The mean RTs were within 10 ms of each other, and the slopes were an identical 88.7 ms per item in the two cases. The blank trial error rate was 2%.

Discussion

The conclusions to be drawn from this modified repeated search task are essentially the same as the conclusions from the other repeated search tasks. Attention to items during one visual search did not seem to alter the efficiency of subsequent visual search through the same items. Again, we

found no evidence of any cumulative effects of the actions of attention. In this case, the age of an item had no effect. Indeed, participants behaved as if they did not notice the change of a letter when it occurred. This was reflected in the failure to find a significant difference between RTs for target-absent trials with and without a new item. Several other paradigms have produced similar failures to notice a substantial change in a display. McConkie and Currie (1996) found that large changes in a scene went unnoticed if the changes were made during a saccade. Rensink and O'Regan have reported similar findings even without eye movements. They found that colors can change and objects can be moved or deleted without being noticed, as long as something is done to mask the visual transients that can capture attention (O'Regan, Rensink, & Clark, 1996; Rensink, 1998, in press-b; in press-c; Rensink, O'Regan, & Clark, 1996; see also Pashler, 1995; Phillips, 1974). Even with simple letter arrays, the ability to report a change in an array of letters is no better than the ability to report letters in a whole report task (Pashler, 1988). That is, if participants can report on 4 of 12 letters, they can determine only whether 1 of those 4 letters changed (see also Eriksen, Webb, & Fournier, 1990). This topic is considered in more detail in the General Discussion section.

Experiment 6: From Repeated Search to Memory Search

Perhaps seven repetitions is not enough. Perhaps only after many searches through the same display can participants maintain multiple links between items in a visual display and their representations in memory. Research on automaticity has shown that some tasks can become much more efficient with extensive practice (e.g., Logan, 1992; Shiffrin & Schneider, 1977). Perhaps this sort of overlearning can sustain multiple links between vision and LTM. In Experiment 6, with these thoughts in mind, we asked participants to search 350 times through the same sets of three or five letters. Several other conditions were compared with this *repeated search* condition. First, there was a *memory search* condition. In this condition, participants committed three or five letters to memory and responded to the visual letter probes on the basis of that memory rather than having the visual search stimulus available. A memory search condition was needed because participants in the *repeated search* condition would be very likely to memorize the letters in the display after only a fraction of the 350 trials. It is important to know whether participants could save time in the repeated search condition by switching to a memory search strategy. Memory search, whether one regards it as serial (Sternberg, 1969) or limited capacity parallel (McElree & Doshier, 1989) search, will proceed at a rate of 30–40 ms per item. With extended practice, that slope may drop to near 0 ms per item (e.g., Logan, 1992). For present purposes, the critical question is whether having the visual stimulus present in the repeated search condition ever makes search for an item more efficient or faster than searching through memory.

A third condition was the *ceiling* condition. In this condition, the letters in the search display changed after each

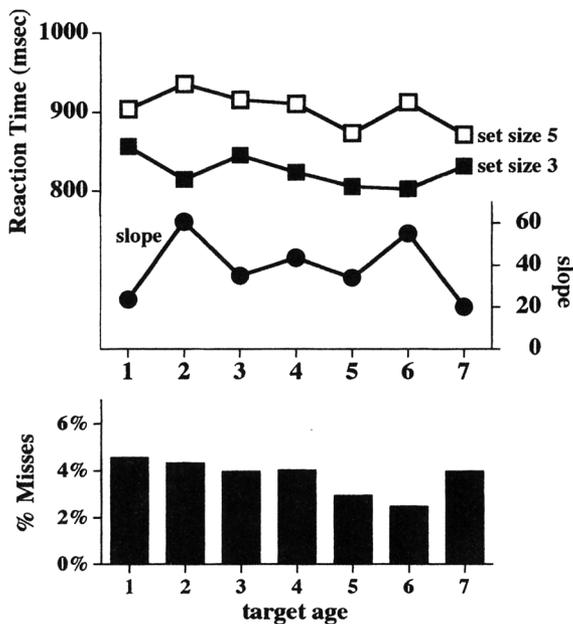


Figure 13. Mean reaction times, slopes, and error rates for target-present trials as a function of target age in Experiment 5.

trial. As a consequence, no postattentive visual representation and no memory for the display could aid in search. In this case, the question is whether repeated search ever produces a search more efficient than a search with no possible postattentive benefit.

Finally, there was the *instance* condition. One difference between the ceiling and repeated search conditions is that there were onset transients in the search display on every trial in the ceiling condition but not in the repeated search condition. The instance condition was identical to the repeated search condition except that a 400-ms blank interval was inserted between each trial. The letters in the search display did not change, but they were transiently offset and onset as in the ceiling condition. Because simultaneous onset of search stimuli and target probe produced an RT cost in previous experiments, it was useful to have an estimate of that cost here. Moreover, the instance condition might provide an interesting bridge to the learning-automatization literature. The condition was labeled in honor of Logan's "instance" theory (Logan, 1988). The core of instance theory is the idea that each instance of an association or event is recorded and that it is the accumulation of these instances that produces automatization of tasks. We speculated that participants might more readily learn to make the letter search task automatic if the task was broken into 350 discrete instances rather than the more continuous repeated search condition. Note that the insertion of the blank screens between each trial resulted in the instance condition being less like a postattentive vision task and more like a learning task.

Method

Eleven participants completed two blocks of 350 trials in each of the four conditions just described. Set sizes were three letters in one block and five in the other. The search display was drawn in 48-point Geneva uppercase. The probe items were 48-point Times lowercase. The probe item was present in the search display on 50% of trials.

In the memory condition, letters appeared before the start of the 350 trials, and participants were instructed to press a key to start testing when those letters had been committed to memory. In the ceiling condition, new letters were presented at the same time as a new probe. In all conditions, there was a 400-ms interstimulus interval between the response to one probe and the presentation of the next. In the repeated search condition, the search stimulus letters remained visible during the interstimulus interval. In the instance condition, the screen was blank for that period. Feedback was given between trials by coloring the central region blue for correct responses and red for incorrect responses.

Results

To analyze the results of this experiment, we needed estimates of mean RT and slope of the RT \times Set Size functions for the different conditions as a function of trial number. We averaged data across 50-trial epochs to obtain reasonably stable estimates. As in previous experiments, RT

estimates were based on the averages of log RTs because they provided a better estimate of central tendency of the RTs in this experiment. Error rates for the visual search conditions were below 5% for all conditions and did not change systematically with trial number. Error rates in the memory search condition were approximately 7% and, again, did not change with trial number. Data were analyzed via ANOVAs on mean RTs or derived slopes, with participants representing the error term; the effects of trial number, target presence, set size, and experimental condition were examined. Note that a large number of possible effects and interactions could be described. In the interests of space, we confine ourselves to discussing the data that have implications for the questions of primary interest in this article.

Did RT change as a function of trial number? Average RT data for Experiment 6 are shown in Figure 14. Main effects of trial number on RT were significant for target-present and target-absent trials in the repeated search, memory, and instance conditions, $F_s(6, 60) > 9.0$, $p_s < .001$, in all cases. These effects were not significant for the ceiling condition, $F(6, 60) = 1.06$, *ns*. It is clear that the repeated search condition was faster than the ceiling condition. This was confirmed by ANOVAs comparing the two conditions (repeated search and ceiling). The main effect of condition was significant for target-present and target-absent trials, $F_s(1, 10) > 61$, $p_s < .001$. The interaction of condition and trial number was also significant, $F_s(6, 60) > 4.4$, $p_s < .01$, reflecting the growing difference between the conditions. Although it increased in speed, the repeated search condition was never as fast as the memory condition, $F_s(1, 10) > 24$, $p_s < .001$, for target-present and target-absent trials. This interaction of condition and trial number was also significant, $F_s(6, 60) > 3.6$, $p_s < .01$, here reflecting the growing similarity between the conditions. The comparison between the instance condition and the repeated search condition showed their RTs to be indistinguishable, $F_s(1, 10) < 1.1$, *ns*.

Did search efficiency (slope) change as a function of trial number? Figure 15 shows the slopes of the RT \times Set Size functions as a function of trial number. Here the results for the repeated search condition followed the pattern from previous experiments. For the target-present trials, regression analysis showed no significant decline in slope as a function of trial number, $F(1, 75) = 0.8$, *ns*. The apparent decline in the target-absent trials likewise failed to meet the usual criteria for statistical significance, $F(1, 75) = 3.4$, $p = .066$. The same pattern held for the memory condition: target-present trials, $F(1, 75) = .004$, *ns*, and target-absent trials, $F(1, 75) = 3.4$, $p = .066$. It held for the ceiling condition as well: target-present trials, $F(1, 75) = 1.2$, *ns*, and target-absent trials, $F(1, 75) = 0.4$, *ns*. Interestingly, the instance condition showed a significant improvement: target-present trials, $F(1, 75) = 5.4$, $p = .022$, and target-absent trials, $F(1, 75) = 9.1$, $p = .004$.

Discussion and Comparisons Between Conditions

It is clear that none of the visual search conditions became better than the memory search. Whatever attention does, it

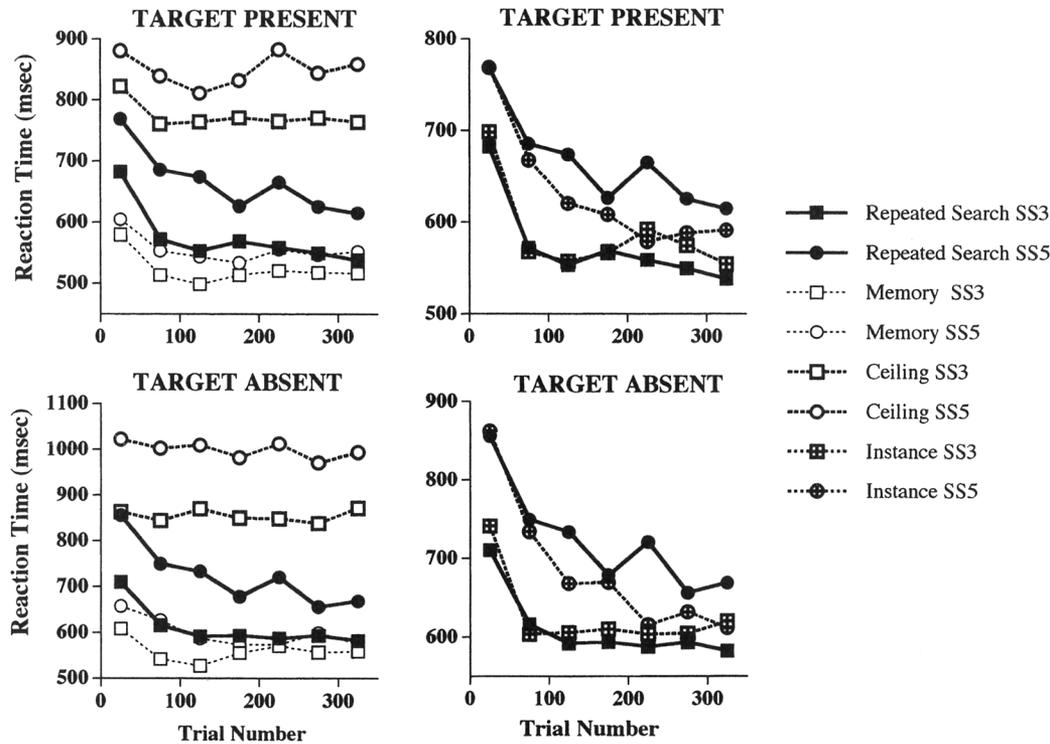


Figure 14. Reaction times as a function of repetition for the four conditions of Experiment 6.

does not create a representation of the visual world that can be used to support efficient visual search. Participants performed at least as well without the presence of the visual stimulus. This experiment does show a benefit of repeated search over the ceiling condition. As noted, RTs were faster in the repeated search condition, and this effect increased as a function of trial number. However, the efficiency of the repeated search slopes was no better than that of the ceiling condition. Even when the analysis was restricted to the last 150 trials, there was no significant difference between the slopes, $F_s(1, 64) < 2.9$, $p_s > .09$, for target-present and target-absent trials. The RT difference probably reflects the same 0-ms SOA effect shown elsewhere. There is a cost when the probe and search stimuli appear at the same time. This occurred on every trial of the ceiling condition but only on the first trial of the repeated search condition. The search processes in the repeated search and ceiling conditions were comparably inefficient.

It might seem curious that participants in the repeated search condition did not switch to the more efficient memory search, but note that the difference in RT between the two conditions was only 100 ms or so. Perhaps 100 ms is simply not subjectively salient enough to force a change in strategy. In any case, even for the last 150 trials, the slopes in the repeated search condition were markedly less efficient than those in the memory condition, $F_s(1, 64) > 10$, $p_s < .002$, for target-present and target-absent trials. In contrast, participants do seem to have been pushed to switch to the more efficient memory search in the instance condition. Slopes over the last 150 trials were more efficient than the repeated

search slopes, $F_s(1, 64) > 14$, $p_s < .0004$, for target-present and target-absent trials, and were not significantly different from the memory condition slopes, $F_s(1, 64) < 2.0$, ns , for target-present and target-absent trials. Why did participants change strategy in the instance condition but not in the repeated search condition? The instance condition had visual transients similar to those of the ceiling condition. Assuming that this would have produced the same, relatively large RT costs observed in other experiments, the instance RTs might have been at the ceiling level, 300 or 400 ms slower than memory search. In this case, a switch to a memory search could have saved the participant, something like a third of a second on each trial. This might have been enough to provoke a change in the instance condition. Alternatively, it may be that breaking the display in the 350-trial instance condition encouraged learning in the manner proposed by Logan's instance theory (Logan, 1988). Distinguishing between these hypotheses is an interesting direction for future research. For present purposes, the important conclusion is that, even after 350 searches through the same set of three or five letters, visual search never became more efficient than memory search through the same letters. In the repeated search condition, it remained markedly less efficient. In the instance condition, it was comparable in efficiency to the memory search.

There is no evidence of the development of automaticity in this repeated search task, nor is there any particular evidence of automaticity in the memory condition. The failure to find significant efficiency gains in the memory search is curious but, again, beyond the scope of this article.

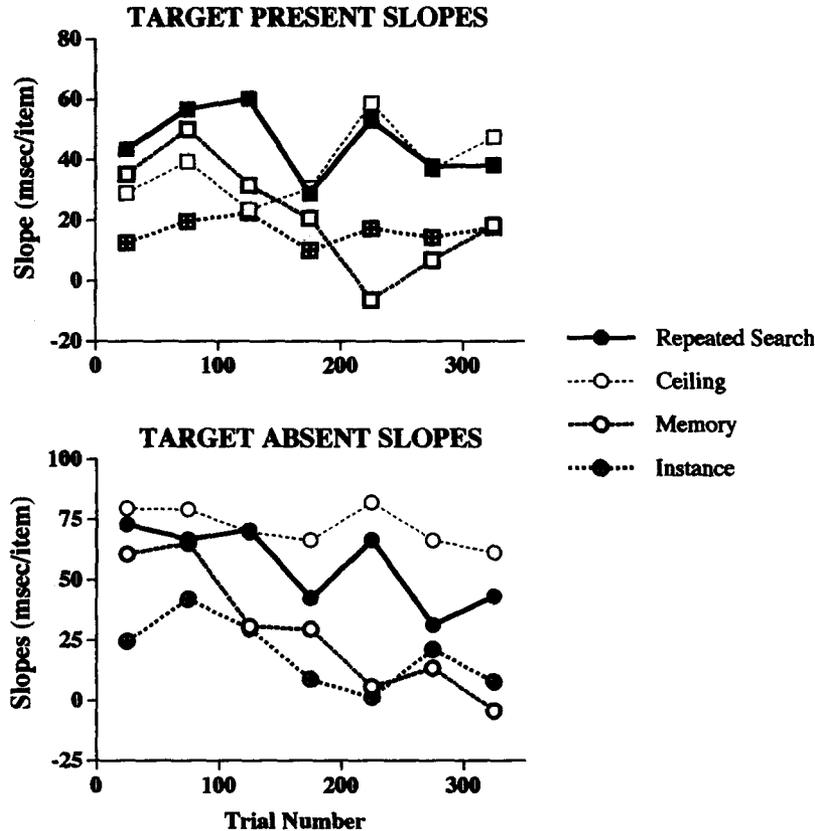


Figure 15. Slopes as a function of trial number in Experiment 6.

With this experiment, we have reached the logical limit of the repeated search paradigm. Participants completing this task spent many minutes looking at and working with the same overlearned, easily recognized stimuli. Nevertheless, the capacity limitations, seen when those search stimuli were preattentive and new, remained when the stimuli became postattentive and old. It seems probable that we could find evidence of automaticity if we were to have participants complete thousands of trials with the same stimulus. However, it does not seem reasonable to suppose that this would reflect a change in visual processing. A capacity limitation exists, perhaps, as we have suggested, at the intersection of vision and memory. It is not eliminated by sustained attention to the visual stimulus. In the remaining experiments, we pursued the same point with a different visual task.

Experiment 7: Postattentive Curve Tracing

The central argument of this article is that attention produces no lasting change in the visual representation of a stimulus. What is seen when the stimulus first appears is the same as what is seen after extended examination of that stimulus. Only the current object of attention is altered by attention, and that alteration ends when attention is deployed elsewhere. This is a rather strong and sweeping claim, and, to this point, we have supported that claim exclusively with

data from visual search experiments. It would seem prudent to seek converging evidence from other realms. Accordingly, in the remaining two experiments, we turned to a curve trace paradigm used by Jolicoeur, Ullman, and MacKay (1986, 1991). The basic paradigm is shown in Figure 16.

Participants were presented with two curves that weaved among each other without crossing. Two probe dots were presented on the curves, both on one curve or one dot on each curve. The participant's task was to respond with one keypress when the dots were on the same curve and another when they were on different curves. The lower portion of Figure 16 shows an idealization of the typical pattern of results. The time taken to make a "same" response is dependent on the distance along the curve and not on the linear separation between the spots. It is as if a small vehicle must drive from one spot to the other, with RT determined by the distance traveled. Indeed, Jolicoeur et al. (1986) argued that an attentional operator of some sort is deployed on the curve. In a series of clever experiments, Jolicoeur and his colleagues defended the proposition that the size of this operator is scaled up or down to make the biggest "vehicle" that can fit on the road. For instance, it cannot be so large as to overlap the other curve or be unable to fit around the bends in the road (Jolicoeur & Ingleton, 1991; Jolicoeur et al., 1991; McCormick & Jolicoeur, 1991, 1992, 1994). Given that it is a task that appears to require the deployment

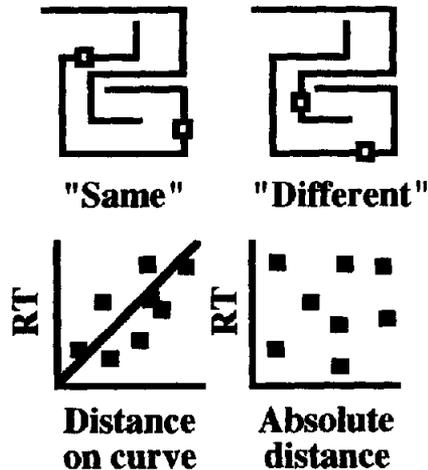


Figure 16. The Jolicoeur, Ullman, and MacKay (1986) curve tracing paradigm. Participants must determine whether the dots lie on one curve or two curves. The data shown in the lower panels are illustrative of the standard results. They are not actual data points. RT = reaction time. From "Curve Tracing: A Possible Basic Operation in the Perception of Spatial Relations," by P. Jolicoeur, S. Ullman, and M. MacKay, 1986, *Memory & Cognition*, 14, pp. 129–140. Copyright 1986 by the Psychonomic Society. Adapted with permission.

of attention, curve tracing could be used to test the postattentive hypothesis of this article with a paradigm very different from visual search. The basic strategy is illustrated in Figure 17.

In repeated curve tracing, the two curves remain unchanged on the screen for multiple trials (in the Jolicoeur experiments, the curves changed after each trial). If one imagines that some form of attention is being deployed along the curve to do the task, then one might imagine that the dependence of RT on curve distance would decline over trials. Presumably, the heart of the difficulty in curve tracing is that the curves are not clearly individuated from each other. If the two objects were easily individuated, one presumes that tracing would not be needed. This allowed us to create a *floor* condition for purposes of comparison. It is schematized in the lower portion of Figure 17. If the two curves are different colors, then, one may presume, the need to trace will be reduced or eliminated. If one of the roles of

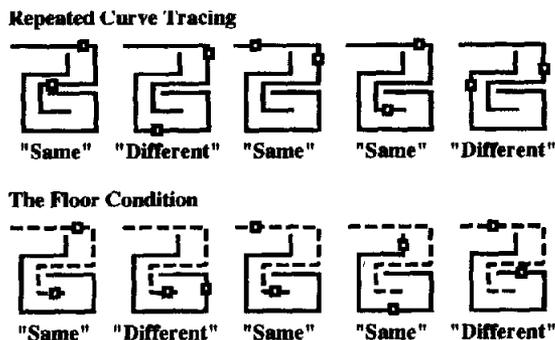


Figure 17. The repeated curve tracing paradigm.

attention is taken to be the individuation of objects (as in the creation of "object files"; Kahneman & Treisman, 1984), then it might be expected that repeated curve tracing would individuate the two curves to the point that the slopes of the RT \times Curve Distance functions in the repeated condition would come to resemble the slopes in the floor condition. This hypothesis was tested in Experiment 7.

Method

Ten pairs of curves were created, similar in appearance to those in Figure 17. These curves fit inside a region that subtended 6.8° at the 57.4-cm viewing distance. Each curve remained on the screen while five trials were run. During these five trials, the curves did not change in any way. Only the position of the probe dots was changed. After five repetitions, a new set of curves appeared. Each participant was tested for 100 blocks of five repetitions. Because there were only 10 sets of curves, each pair of curves appeared many times during the experiment. In the repeated condition, the curves were both yellow. In the floor condition, one curve was yellow, and the other was blue. Curves were presented on a dark gray background. Because useful data could be obtained only from "same curve" trials, dots were placed on the same curve on 67% of the trials and on different curves on the remaining 33%.

Ten participants were tested. All passed the Ishihara color test and had normal or corrected-to-normal vision. All gave informed consent and were paid for their time.

Results

RTs greater than 5,000 ms were coded as errors. These were extremely rare. Error rates were 3% or less for each participant in each condition. Error rates were slightly lower in the floor condition. The results replicated the basic findings of Jolicoeur et al. (1986) in that RT was dependent on the distance along the curve but not on the absolute distance between probe dots. For our purposes, the critical data are effects of condition and repetition on the slopes of the RT \times Distance functions. These data, averaged over all participants are shown in Figure 18.

An ANOVA with condition and repetition as variables confirmed the impression given by the results shown in Figure 18. The slopes in the repeated condition were steeper

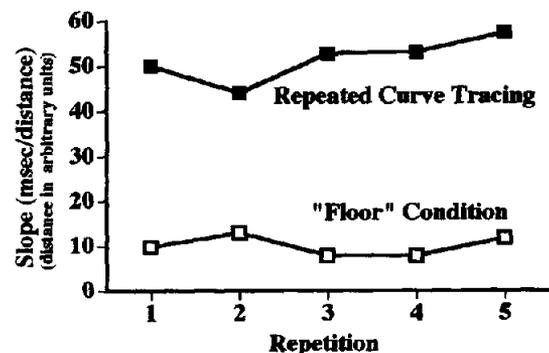


Figure 18. Results of repeated curve tracing experiment. Slopes of the Reaction Time \times Distance functions are shown. Distance is defined in arbitrary screen units.

than the slopes in the floor condition, $F(1, 9) = 34.4, p < .01$. There was no effect of repetition, $F(4, 36) < 1$, and no interaction of condition and repetition, $F(4, 36) = 1.2$. It might seem odd that the floor slopes were above zero; however, Pringle and Egeth (1988) found evidence of residual slopes even with very simple stimuli in experiments of this sort. Details of floor performance are outside the scope of this article.

Discussion

These results are consistent with the picture of postattentive vision that was developed in the visual search experiments. Repeated and sustained attention did not seem to change the representation of the stimulus in any way that could facilitate performance, in this case, of the curve tracing task. However, as mentioned earlier in regard to the visual search experiments, one could argue that five repetitions do not allow participants sufficient time to adequately attend to the stimuli. Accordingly, in Experiment 8, participants completed 100 curve tracing trials on a single pair of curves.

Experiment 8: Extensively Repeated Curve Tracing

Method

Experiment 8 was similar to Experiment 7 except that, in Experiment 8, participants were tested for 10 blocks of 100 trials rather than 100 blocks of 5 trials. This meant that a single set of curves remained unchanged on the screen while 100 probe spot trials appeared on those curves. Participants were tested in a repeated condition and a floor condition. Ten participants took part. All had normal or corrected-to-normal acuity, all passed the Ishihara color test, and all were paid for their time.

Results

One participant was removed from data analysis because of an unacceptably high error rate of 38%. Error rates were otherwise under 4% in both conditions. Errors did not vary with trial number. For purposes of analysis, results were averaged over groups of 10 trials. Only trials in which the two dots were on the same curve were analyzed. With long runs of curve tracing, we can look at three independent variables of interest. Condition (repeated or floor) and trial number were the variables of interest in Experiment 7. The new variable was the relationship to the previous trial. If both of the dots on trial N were on one curve, one can ask whether the dots on the preceding trial were both on the same curve (same curve), whether they were both on the other curve (changed curve), or whether there was one on each curve (neutral). One might expect different results in the same curve category because, in that situation, attention might not be deployed away from the relevant curve. Average RT results, as well as the slope giving the "speed" along the curve, are shown in Figure 19.

The slope measures were somewhat noisy because relatively few trials entered into the estimate of each slope for each participant and each condition plotted. Nevertheless, it

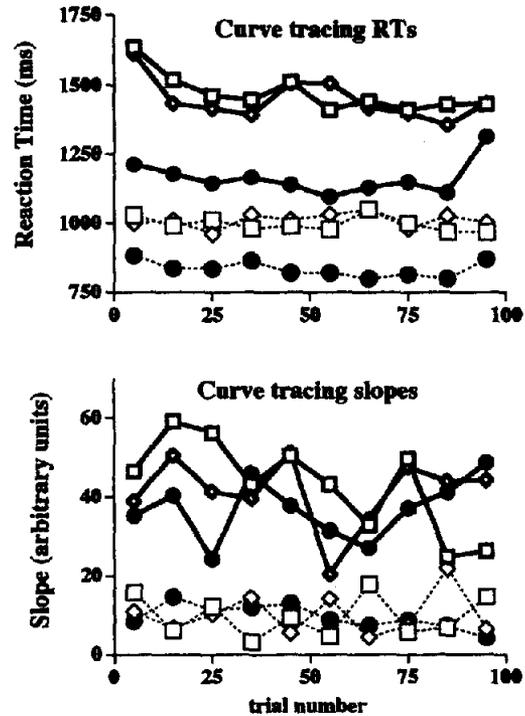


Figure 19. Curve tracing does not become more efficient, even after 100 trials. Squares = neutral trials; diamonds = changed curve trials; circles = same curve trials; solid lines = repeated curve tracing; dashed lines = floor condition. RT = reaction times.

is clear that performance in the floor condition was always better than performance in the repeated condition in regard to slopes and RTs, $F_s(1, 8) > 40, p_s < .001$, for the two measures. RTs were substantially faster when the dots appeared on the same curve on two successive trials, $F(1, 8) = 88.1, p < .001$. This advantage appeared to be greater for the repeated condition than for the floor condition, $F(1, 8) = 7.6, p < .01$. There was no such effect for the slopes, $F(1, 8) < 1$.

There was a small but significant effect of trial number in the RTs for the repeated condition ($R^2 = .001, p = .01$). This was due to a decline during the first 10 trials. After that point, there was no significant change in repeated condition performance ($R^2 = .00002, p = .77$). There was no significant effect of trial number in the floor condition. There was no significant effect of trial number on slope in either condition.

Discussion

The primary conclusion from this experiment is the same as the conclusions from the other experiments. There was no evidence that sustained attention to an unchanging visual stimulus does anything to make that stimulus more useful, in this case, in supporting curve tracing. It is worth commenting on the large RT advantage seen in the case in which the dots in two successive trials were presented on the same curve. This was not simply an effect of using the same hand to respond on two successive trials. Recall that, in both the

same curve and changed curve situations shown in Figure 19, the participant is making a "same curve" response because the dots on that trial were on the same curve. The distinction between the same curve and changed curve trials is defined by the location of the dots on the previous trial. When the dots were presented on the same curve twice in a row, there was a substantial improvement in RT. Perhaps attention does not need to shift to a new object in the same curve condition. Note, however, that even in this case, there was no improvement in the slope. The speed of attentional curve tracing was not improved even by two scans of the same curve.

General Discussion

Visual selective attention is required for a wide range of visual tasks. The results reported here are consistent with the hypothesis that the perceptual consequences of attention are fleeting. An attended visual stimulus can support a behavior such as object recognition or curve tracing. A previously attended stimulus cannot support that behavior unless attention is redeployed back to that stimulus. On the basis of this set of experiments, it would be too sweeping for us to conclude that there are no persistent visual consequences of selective attention. We can conclude that sustained attention to visual search stimuli did not make visual search more efficient in our experiments. We can also conclude that sustained attention to a pair of curves does not make attentional tracing of those curves any more efficient. It is always possible that some other aspect of attentionally modulated perception remains in its attended state when attention is redeployed. If so, we have not found it.

What are the implications of this failure to find persistent effects of attention? In the final section of this article, we consider three questions: (a) How do these findings help provide an understanding of the phenomena known as *change blindness*, *inattention blindness*, and the *attentional blink*? (b) If people can recognize only one item at any given moment in time, why do they experience a perceptual world filled with multiple recognized items? and (c) Why does practice produce improvements in other tasks but not in the efficiency of the tasks we used here?

Change Blindness in Scene Perception

To restate a point made at the outset, it would be foolish to assert that prolonged attention to a stimulus has no consequences whatsoever. Attention to a stimulus allows some information about that stimulus to enter memory. The claims of this article are that (a) only one link from vision to memory can be active at any given moment and (b) the state of visual perception after attention has left a stimulus is similar (perhaps identical) to the state before attention arrived. It is unquestionable that people know more about a stimulus after they have examined it for a while. However, what one knows about an object is what one learned when one attended to it. If something happens to that object after one's attention has turned elsewhere, one would not know that. Consider the implications of this claim for scene

perception. If only one object is recognized at any given moment, then it should be possible to change the appearance or even the identity of an unattended object without that change being noticed. Even if the observer knows with great certainty that a particular object was, in fact, present a moment ago, she or he does not know whether or not it is currently present until attention is redeployed to that object.

This prediction of an insensitivity to change is supported by the growing research on change blindness (Rensink et al., 1996; Simons & Levin, 1997). A variety of paradigms reveal striking limits on the ability to identify a change. Change goes unnoticed unless one of three conditions is met. First, a change will be noticed if it produces a visual transient that summons attention (Jonides & Yantis, 1988; Yantis, 1993; Yantis & Johnson, 1990). In the change blindness literature, these visual transients are hidden in various ways. A blank frame can be added between the old and new stimuli (Rensink et al., 1996). A more salient local transient can be presented at the same time as the change (O'Regan et al., 1996). Changes can be made during the suppression of vision caused by a saccade (Blackmore, Breilstaff, Nelson, & Troscianko, 1995; Irwin, 1996; Irwin, Yantis, & Jonides, 1983). Changes can be made away from the known focus of attention (as in reading studies; e.g., Rayner & Fisher, 1987). Changes can be made during a "film cut" or even while a door is physically moved between participant and stimulus (Levin & Simons, 1997; Simons, 1996; Simons & Levin, 1997).

The transient supports detection of the change. It does not give the viewer information about the original contents of the display. Brawn, Snowden, and Wolfe (in press) had participants' view a field of red and green dots. At one moment, a transient summoned attention to one dot. At that same moment, the dot either did or did not change color. It was then quickly removed and masked. Participants had little trouble identifying the color of the dot in the interval between cue and mask. However, they were at chance level in regard to naming that color before the attention-summoning cue (Brawn et al., in press). Change blindness is a catchy misnomer. As a general rule, the visual system is not blind to change. Rather, it is amnesic for the contents of the stimulus before the change.

Second, a change will be noticed if it occurs at the current locus of attention. For instance, in Rensink's experiments, two scenes alternate with a blank frame between them to eliminate transients. It may take a while to find the difference between the two scenes, many seconds in some cases. However, the task is possible. It appears that the solution is found when attention rests on the correct object while that object changes.

Third, a change will be noticed if it causes a detectable mismatch between the new scene and memory for the old scene. Most obviously, change will be noticed if the "gist" of a scene is changed. The exact definition of *gist* is elusive (Mandler & Ritchey, 1977; Rensink, in press-a). However, if a picture of a field of flowers is replaced by a picture of a field of cows, the change will be noted. More subtle changes will be noted if the correct items are coded into memory. Thus, a change from a display of the letters *DLKF* to a

display of *DLXF* will be noted if the original letters are coded into some form of working memory. Normal memory processes can cover for the amnesia of the visual processes.

Because changes in a stimulus are not necessarily noted even when attention is redirected to the changed object (Simons & Levin, 1997), it follows that attention does not lead to an automatic encoding of all stimulus attributes into a durable memory. The perceptual effects of attention are fleeting, and the mnemonic effects are often incomplete.

Inattentional Blindness or Amnesia

The inattentional blindness paradigm of Mack and her colleagues (Mack & Rock, 1998b; Mack, Tang, Tuma, & Kahn, 1992) can be interpreted in a similar manner. In this paradigm, participants perform a single task (e.g., a line length judgment). On one critical trial, unknown to them, a stimulus is flashed near to the task-relevant stimulus. In many cases, participants fail to report the presence of the task-irrelevant stimulus when queried after the trial. It is as if they did not see it. This task permits only one critical trial per participant because, after a single alerting trial, the task-irrelevant stimulus is noticed on subsequent trials. As the paradigm label indicates, Mack and colleagues argued that participants do not see the task-irrelevant stimulus. They argued, more generally, that participants see only attended stimuli. Unattended stimuli are unperceived stimuli.

As with change blindness, it is possible to consider inattentional blindness to be a case of visual amnesia. The critical trial and the subsequent question about it can be seen as a reduced form of a change blindness experiment. The participant is looking at a scene very briefly. The scene is removed and replaced with another scene (a mask), and the participant is queried about the contents of the now-vanished scene. If the task-irrelevant stimulus is not coded into some sort of working memory, then, even if every aspect of the critical stimulus has been seen, no report can be made about it because no record has been preserved. Rather than inattentional blindness, one might label this phenomenon "inattentional amnesia." This account of the inattentional blindness paradigm is very close to that of Moore and Egeth (1997), who backed up their account with the experimental finding that "unseen" stimuli can influence perceptual judgments about Ponzo illusion figures. The general idea of inattentional amnesia also has antecedents in the work of Kevin O'Regan: notably, his concept that the world acts as an "external memory," making an extensive internal memory unnecessary (O'Regan, 1992).

The Attentional Blink

The attentional blink (Raymond, Shapiro, & Arnell, 1992; Shapiro, 1994) is another phenomenon in which the name implies a failure to see, whereas the data suggest a failure to remember. In the attentional blink, participants must attend to and report on two targets in a rapidly presented stream of items. Identification of Target 2 is severely impaired if it is presented 100–300 ms (or more) after Target 1 (Chun & Potter, 1995; Raymond et al., 1992; Shapiro, 1994). It could

be that Target 2 is not seen. Alternatively, suppose that the act of identifying Target 1 as a target prevents encoding of the next few stimuli, including Target 2, into memory. Under this amnesic account, "blinked" stimuli might be seen and recognized but unable to produce a response. Event-related potential data from work by Luck, Vogel, and Shapiro (1996) are consistent with this amnesia account. Luck et al. studied event-related potentials during an attentional blink experiment. They found electrophysiological evidence that word meanings are accessed for Target 2 words even when those words cannot subsequently be reported. This is consistent with the notion that the items were seen and identified. Shapiro, Driver, Ward, and Sorenson (1997) found evidence of priming by "blinked" stimuli, again consistent with the idea that the stimuli were processed but were unavailable for report.

A similar analysis can be made of the work of Joseph, Chun, and Nakayama (1997). They found that the attentional blink caused by an attentionally demanding Rapid Serial Visual Presentation (RSVP) task at fixation results in participants missing the presentation of a "pop-out" in a surrounding visual search stimulus. One could argue that, in the absence of attention, participants do not "see" the search stimulus. Alternatively, one could argue that, in the absence of attention, participants cannot respond to the stimulus. When attention is otherwise engaged with the RSVP task, the search task cannot be performed. When attention is released, the physical search stimulus is gone. Because vision has no memory, no trace of the search stimulus remains, and no search response is possible.

Subjective Visual Experience and the Assembly Line of Attention

The argument of this article is that only one object is recognized at any given time. Other visual stimuli may be seen, and other nodes in memory may be active, but only one link between vision and memory is active at any given moment. Some of the accounts of blinks and blindnesses provide an even more impoverished view of perception, arguing that the unattended is not seen at all. The notion that people perceive only the current object of attention flies in the face of the impression that they can attend to one object without experiencing the disappearance of the rest of the visual world. Our notion, that one can recognize only the current object of attention, fares only slightly better. It contradicts the impression that the perceptual world is populated by a collection of stable, recognized objects and not by one recognized object and an assortment of unrecognized preattentive objects. Subjective visual experience is a slippery topic but one that has returned as a matter for serious discussion (e.g., Crick, 1994; Dennett, 1991; Kihlstrom, 1999; Koch & Braun, 1996; Ramachandran & Blakeslee, 1998). How can we reconcile our experience with the postattentive data and the data from the paradigms described in the previous section?

On the basis of visual search experiments, attention seems to be deployed from item to item at a rate of one item every 40–50 ms (twice that rate according to Horowitz & Wolfe,

1998). What does this mean? It cannot mean that all of the attention-demanding processing of a visual stimulus is accomplished in 40–50 ms. That is much faster than even the fastest estimates of the time required to recognize an object (e.g., Thorpe, Fize, & Marlot, 1996). Discussions of attentional “dwell times” (e.g., Duncan, Ward, & Shapiro, 1994; Ward, Duncan, & Shapiro, 1996) usually fail to note that this 40–50 ms per item is a rate and not a duration. It might require 300 or 500 ms to take a bundle of loosely affiliated features and turn them into a recognized object. However, the search data suggest that multiple objects can be put through this process at the rate of 40–50 ms per item.

An assembly line is a useful analogy. Suppose that an assembly line is capable of delivering a new car every 10 min. It does not follow that it takes 10 min to produce a car. It could take hours or days. All that is known is that materials are loaded into the front end and cars come out the back end at a rate of 10 min per car. In this analogy, the act of recognition, the tie between perception and memory, would be just one step in the process (e.g., painting the car). Even though all of the cars have been or will be painted, only one is actually being painted at any given time.

Note that the assembly line architecture all but dissolves the distinction between serial and limited-capacity parallel models of attention. As just described (see Figure 20), the process is clearly serial in the sense that one car is being painted at any given time and one car is rolling off the assembly line at any given time. However, multiple cars are being assembled in parallel. Returning to something closer to visual search, if items are being processed at a rate of one every 40–50 ms and it takes, say, 400 ms to move from the preattentive representation to a recognized object, then something like 10 items would be simultaneously undergoing different aspects of processing that require attention. Because the visual system is not an auto plant, one can readily imagine small changes that make the serial-parallel distinction even less compelling. Suppose that multiple items can be loaded on the assembly line at the same time. Or suppose that “easy” items can pass “harder” items,

starting later but finishing earlier. Even without such modifications, it is easy to see how an assembly line architecture could produce data consistent with either the serial or parallel models that have been so extensively discussed in the literature.

To relate the data on postattentive vision to the subjective experience of multiple recognized objects, it is useful to resurrect James’s (1890) discussion of the “sensible present.” According to James (1890), “the practically cognized present is no knife-edge, but a saddle-back, with a certain breadth of its own on which we sit perched, and from which we look in two directions into time (p. 609). There are multiple ways to conceive of and to measure the duration of this sensible present (Allan, 1979; Fraisse, 1984). Most produce estimates well in excess of 50 ms. Thus, whereas one might have mental processes operating at 20–25 Hz, one’s experience of those processes integrates over some larger period. For the sake of argument, imagine that the sensible present lasts for a second. With one act of recognition every 50 ms, that could give rise to the impression of some 20 recognized objects in the present. The fact that science might show that only one is really recognized at any given time is a little like science showing that the atom is largely empty space. It may be true, and it may be provable, but it does not represent experience. People can afford the illusion of a stable world filled with a multiplicity of recognized objects because, under most circumstances, they operate in a world containing a multiplicity of stable objects. Strangers on the street change identity only in the experiments of Simons and Levin. Bridges vanish and reappear only in the demonstrations of Rensink et al. In the real world, people can have a cluster of nodes in memory active as their thoughts about the current state of affairs, even if only the nodes for one object are in contact with the percept of that object at any given moment. With objects moving along the attentional assembly line at a rate of 20–25 Hz, people’s understanding of the world can be repeatedly reconfirmed and continually updated, even if their perception of the world does not change (see also Rensink, in press-a).

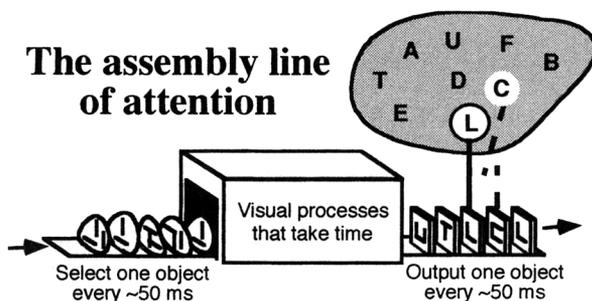


Figure 20. Assembly line of attention. Slopes of visual search experiments do not show that vision takes, say, 50 ms from features to recognition. Slopes are rates showing that items can move through the system at 50 ms per item. Many items may be on the assembly line at any given time, even if only one item (here an *L*) is recognized at any given time. This blurs the distinction between serial and parallel processes.

Postattention, Perceptual Learning, and Automaticity

One final question needs to be addressed. How can it be that hundreds of repetitions of visual search through the same set of items fail to improve search efficiency when other paradigms show substantial effects of perceptual learning or of a transition from attention-demanding capacity-limited performance to apparently unlimited automatic performance? A variety of studies shows that training makes previously difficult perceptual tasks significantly easier (Ahissar & Hochstein, 1995; Beard, Levi, & Reich, 1995; Gilbert, 1994; Karni & Sagi, 1991; Sagi & Tanne, 1994). Even letter search tasks such as those used here seem to benefit from learning to the point of becoming efficient, “parallel” searches in some cases (Caerwinski, Lightfoot, & Shiffrin, 1992; Schneider & Shiffrin, 1977; Sireteanu & Rettenbach, 1995). Why should experience help in these tasks but not in the repeated search tasks reported in this

article? The perceptual learning effects reported in other articles are unlikely to represent an enhancement of a postattentive visual representation. The time course is usually quite slow (Karni & Sagi, 1993) and may consolidate during REM sleep (Karni, Tanne, Rubenstein, Askenasy, & Sagi, 1994). As such, it cannot represent a change in the appearance of a stimulus caused by attention to some aspect of that stimulus. It is more probable that perceptual learning represents the development of new strategies for extracting information from the stimulus. To take letter search as an example, perceptual learning probably reflects an improved ability to recognize letters in the visual representation rather than reflecting an improved quality of the visual representation itself.

The literature on automaticity presents a different challenge. To paraphrase Pashler's (1997) definition in his useful chapter on the subject, two changes in performance are at the core of the concept: Automatic behavior is held to no longer demand attentional capacity, and automatic behavior is no longer subject to voluntary control. Putting aside the issue of voluntary control, there is evidence of substantial increases in letter search efficiency with extensive practice (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Why is no improvement seen with repeated search, even after 350 trials (Experiment 6)? There are, at least, two stages in processing in which one might see an improvement: visual processing and memory access. According to Logan (1992), citing LaBerge and Samuels (1974), Schneider and Shiffrin (1977), and his own earlier work (Logan, 1978), "the modal conception of automatization is the development of the ability to bypass attention" (p. 320). As Logan realized, if this is an argument that it is possible to make tasks "preattentive" with practice, then it is flawed. This is not the place for an extended discussion (see Logan, 1992; Pashler, 1997), but an example or two will suffice. For an intuitive example, consider that years of practice in reading might have made some aspects of the task automatic, but one still cannot read preattentively if that implies an ability to read multiple streams of text at the same time. The face search literature, cited earlier, suggests that years of experience with those stimuli have not rendered face recognition "preattentive." The notion is a vestige of an outdated version of "early selection" models. Another experimental example can be found in Logan (1994).

A more plausible place to look for liberation from the demands of attention would be in the link between vision and memory. Specifically, one might expect repeated search to cause a shift from a capacity-limited examination of working memory to a more nearly parallel search of LTM (again, see Pashler, 1997; see also Juola, Fischler, Wood, & Atkinson, 1971). The memory search results of Experiment 6 move in the direction of automaticity. Slopes decline as a function of trial number. Why do the repeated visual search slopes not show a decline? Even if the processing of visual stimuli could not be made "preattentive," why did subjects not switch to the more efficient memory search? As suggested in the discussion of the experiment, it may be that the small RT advantage that could be gained by the switch was not adequate to drive the system to switch strategies. The

implied demand characteristics of this task are visual. Participants had a stable visual stimulus before them. It might not have occurred to them, either explicitly or implicitly, that they would do well to ignore that stimulus. This raises an interesting area for future research: Under what conditions can the development of automaticity be blocked? This, however, is beyond the scope of this article. For present purposes, we can draw the following conclusions. Our results, like other previous results, fail to support the notion that visual selective attention can be rendered unnecessary by practice (at least, not by the amounts of practice used here). Changes in the attentional demands of memory access do occur here and elsewhere. However, any such changes failed to alter the efficiency of repeated visual search.

Summary

The six repeated search experiments reported here show that attention to items in a search array does not fundamentally alter subsequent search for the same items in the same search array. Two further experiments on repeated curve tracing, likewise, show no improvement in efficiency of curve tracing with continued attention to the stimulus. The post-attentive visual representation that remains after attention has been deployed away from a previously attended item appears to be the same as the preattentive visual representation that was present before attention arrived. This finding has the usual disadvantages of a negative finding. Perhaps postattentive effects will be found elsewhere. For the present, however, we conclude that people see a preattentive visual representation. They may know more than they can see, but that knowledge does not alter the visual representation.

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