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Repetition Blindness in Rapid Lists: Activation and Inhibition Versus Construction and Attribution

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The authors examine the repetition blindness effect—the failure to report one of the occurrences of a word presented twice in a rapid list. This phenomenon has been ascribed to inhibitory processes that prevent immediate tokenization of the 2nd occurrence of a repeated word. The authors present several kinds of evidence against that account, including observations that repetition blindness (a) does not occur when repetitions are not embedded in a list of familiar orthographic units, (b) is alleviated by precuing the subject with the identity of the word that may repeat within a rapid list, and (c) can be caused by cues presented after the list, when the opportunity for inhibition has passed. It is proposed that repetition blindness can better be understood through the principles of construction and attribution.

Our purpose in this article is to contrast two very different approaches to explaining phenomena of mind. One is based on the metaphors of activation and inhibition. In accounts using these ideas, it is generally assumed that stimuli well known to the person have representations in a semantic memory system that retains information about their abstract identity (e.g., Collins & Loftus, 1975); activation of representations in that system also creates a record in a separate episodic memory system that retains information about the context (time and place) in which the event occurred (e.g., Tulving, 1983). By such accounts, the relationship between the representations in memory and conscious experience is direct: Perception and identification of a stimulus in the moment occurs through activation of the corresponding semantic representation. A corollary of such accounts is that failure of perception and identification when the stimulus is processed with full attention results from inhibition. Thus, inhibition of a semantic representation can cause failure to encode an episodic record of the occurrence of a stimulus, resulting in failure to detect or remember it later (e.g., Chialant & Caramazza, 1997; Chun & Potter, 1995; Kanwisher, 1987; Kanwisher & Potter, 1990).

The alternative approach we wish to consider instead uses the metaphors of construction and attribution. It posits a less direct relationship between a person's consciousness of past and present events and the representations in memory: It denies that perception and remembering are simply the activation and elevation to consciousness of the contents of a memory trace. In it, representations in memory are of processing experiences, not stimulus structures or their abstract meanings. In consequence, memory has no organization beyond that given by the similarity of current and prior processing experiences (see also Landauer, 1975). As a further consequence, there are no abstract, semantic representations to activate or inhibit. Rather, it suggests that performance (including the mental generation of percepts and cognitions) is controlled by distributed representations in memory in interaction with the current stimulus, task, and context (the *stimulus complex*: Whittlesea, 1997, 2003). This interaction results in the construction of a percept or cognition, through the imposition of meaning and organization on the stimulus, guided by the mass of previous experiences of performing similar activities on similar stimuli, and attribution of the construction to the event itself. This process of construction and attribution certainly is affected by the physical properties of the stimulus, but it often also takes into account aspects of the context and can be based on interpretations of the stimulus controlled by intuitive theories of cause and effect.

In the experiments in this article, we examined the ability of these two different accounts to explain the phenomenon of repetition blindness. This effect consists of the failure to report one occurrence of a word that appears twice in a rapidly presented sequence of items (Kanwisher, 1987; Kanwisher & Potter, 1990). Other variants of repetition blindness involve repeated pictures (Kanwisher, Yin, & Wojciulik, 1999), individual alphanumeric characters (Bavelier & Potter, 1992), or even pairs of orthographically similar, but not identical, words (Bavelier, Prasada, & Segui, 1994; Chialant & Caramazza, 1997). In our experiments, we examined cases of repetition blindness arising from word repetition and from orthographically similar word pairs.

Repetition blindness is a robust effect, occurring predictably when words are presented in rapid serial visual presentation (RSVP) lists at about 150 ms or less per word and when the second occurrence of a repeated word occurs following only one or two intervening words. Kanwisher (1987, 1991; Kanwisher & Potter, 1990; Park & Kanwisher, 1994) argued that the effect reflects the difference between *type* and *token* representations. Types are representations of generic concepts, resident in a hypothetical semantic network, whereas tokens are episodic markers attached to types

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when the relevant concept occurs in some actual life event. Types are used to understand the meaning or identity of a word or object, and tokens are used to remember the occurrence of that type on a later occasion. Kanwisher and her colleagues argued that when asked to seek repetition within an RSVP list, subjects are as likely to tokenize the first occurrence of a repeated word as they are any other word, permitting later remembrance that it occurred once, but that the creation of an individuated token of the second occurrence is inhibited when the word is presented again too soon after its first occurrence is tokenized. In consequence, the second occurrence becomes assimilated into the episodic representation of the first occurrence. It is argued that this inhibition occurs because the tokenization process has a refractory period during which it cannot be reapplied to the same type representation. In support of that idea, inserting longer lags (and more items) reduces the size of the blindness effect. Also supporting the argument is the observation that double presentation of a word within a rapid list can increase the likelihood of reporting that the word occurred within the list (without being able to report that it occurred twice; cf. Morris & Harris, 2002). According to this account, that result occurs because the type is activated twice and thereby achieves a degree of activation greater than that of its neighbors. Moreover, repetition blindness is not assumed to be a strategic or resource-dependent effect but an automatic and fundamental feature of the tokenization process (and presumably of the biological substrate that supports tokenization).

Since the original demonstrations by Kanwisher and her colleagues (Kanwisher, 1987, 1991; Kanwisher & Potter, 1990; Park & Kanwisher, 1994), the repetition blindness phenomenon has been made more complex by demonstrations and counterdemonstrations suggesting that the locus of the effect is in retrieval failure (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995), reconstruction (Masson, 2004; Masson, Caldwell, & Whittlesea, 2000; Whittlesea, Dorken & Podrouzek, 1995; Whittlesea & Podrouzek, 1995; Whittlesea & Wai, 1997), or online perceptual integration processes (e.g., Johnston, Hochhaus, & Ruthruff, 2002; Morris & Harris, 2002). Much of this theoretical work, however, is outside of the scope of this investigation, which is intended to contrast the usefulness of accounts on the basis of assumptions of activation and inhibition versus construction and attribution. We demonstrate several kinds of evidence that are incompatible with the inhibition explanation of repetition blindness (Experiments 1–3). We then propose and test an alternate explanation of repetition blindness on the basis of the construction and attribution account (Experiments 4 and 5).

Experiment 1: The Role of Context

Repetition blindness has been investigated in many paradigms, all of which involve embedding repetition within an RSVP list of some other stimuli. We believe that the role of the nontarget stimuli has not received enough attention in characterizing the question posed by repetition blindness. We designed our first experiment to bring that point under careful examination.

Method

Subjects. All of the subjects tested in the experiments reported here were students at Simon Fraser University, Burnaby, British Columbia, Canada, who participated for extra credit in an undergraduate course or for a chance to win a Can$400 (U.S.$320) lottery. We tested 14 subjects in Experiment 1.

Materials and procedure. The word stimuli used in this study comprised a stock of five-letter words with a Kučera-Francis frequency rating of 50 or above. On all experimental trials, subjects saw words presented in RSVP format at 120 ms per word. The task was to report whether the RSVP sequence contained the repeated occurrence of a word. In the first condition, the subject saw only two words printed in uppercase letters, each presented for 120 ms, with a 120-ms blank between presentations. On half of the trials in this condition, the same word was presented on both occasions, and different words were shown on other trials. In the second condition, the symbol string @#$%& was presented before, between, and after the two critical words, which were again either repeated or not repeated. That is, in this condition, the subject saw an RSVP list of five stimuli, the symbol string @#$%& occurring in Locations 1, 3, and 5, and two words (repeated or not repeated) in Locations 2 and 4. Condition 3 was similar, except that the word *WHITE* was substituted for the symbol string so that the word *WHITE* appeared in Locations 1, 3, and 5, and two other words (repeated or not repeated) appeared in Locations 2 and 4. Finally, in the fourth condition, the two critical words (repeated or not repeated) appeared as usual in Locations 2 and 4, whereas different words, taken at random from the stock of five-letter words, appeared in Locations 1, 3, and 5. We conducted 30 trials of each type (15 of which had a repetition and 15 of which did not); the 120 resulting trials were presented in an order independently randomized for each subject.

The subjects were informed about the composition of the four conditions and were further informed that repetition would occur on half of the trials. They were told that the word *WHITE* would often occur repeatedly, but that in those cases, they should be looking for repetition of a different word. Likewise, they should ignore repeated presentation of the symbol string @#$%& when it occurred, claiming repetition only when a word occurred twice on such trials. The subjects were provided with two keys; they were instructed to hit one key if they thought they had seen a repeated word and the other key if they thought they had not. Type I error probability was set at .05 for all tests reported in this article.

Results and Discussion

As shown in Table 1, subjects rarely committed false alarms (claiming repetition when none had occurred) in any condition. The only exception was slightly higher false alarms in the *WHITE* context condition but, when tested against the mean of the other three conditions, that comparison was not reliable, $F(1, 13) = 2.67$, $MSE = 0.01$, $p = .126$. Of greater interest, repetition was accurately reported on almost 100% of cases when only two words were shown. Introduction of symbol masks before, between, and after critical words reduced accurate reports by 21%, $F(1, 13) = 24.79$, $MSE = 0.05$, $\eta^2 = .66$. Presentation of *WHITE* as a mask before, between, and after critical words reduced accurate reports

Table 1
Experiment 1: Probability of Claiming Repetition in Varied List Contexts

	Repetition status	
Context of critical words	Repetition	No repetition
Blank × 3	.99	.02
Symbol string × 3	.78	.04
WHITE × 3	.58	.10
Three unrelated words	.10	.01

by a further 20%, $F(1, 13) = 11.52$, $MSE = 0.03$, $\eta^2 = .47$. Finally, presenting three different words as masks before, between, and after critical words reduced accurate reports by a further 48%, $F(1, 13) = 99.98$, $MSE = 0.03$, $\eta^2 = .89$. In that condition, the subjects were substantially blind to the repetition, reporting it on only 10% of trials when it occurred.

It seems clear from these data that repetition blindness is not in any simple way a problem associated with time: The presentation durations of critical words and the offset time between critical words was identical in all four conditions. At those durations and offsets (120 ms each), the subjects had no evident problem in detecting repetition when no masks were presented. Instead, masking the critical words with other stimuli appears to be the direct source of the phenomenon. Comparison of the two middle conditions demonstrates that familiar orthography causes more problems for detecting repetition than do symbols that ordinarily are not seen as a unit, and comparison of the last two conditions demonstrates that variable orthography causes even more problems.

These observations suggest that three factors in addition to restricted presentation duration are responsible for the repetition blindness effect, namely pre- and postmasking of critical stimuli, using masks consisting of familiar orthographic units and using different familiar units as masks on each occasion. Although those four factors are present in almost every examination of repetition blindness that we have seen, those factors (other than the restricted presentation duration) are not mentioned as the cause of repetition blindness, but only as the circumstances under which the effect is observed prior to an attempt to discover its cause. The present observations demonstrate that repetition blindness, when it occurs, is not just a function of repetition coupled with high speed but also critically depends on the nature of the masking stimuli.

This experiment demonstrates specific factors that contribute to the effect—but not the mechanism by which those factors prevent the person from realizing there is repetition. We attempt to outline that mechanism in Experiments 2–5. It is already clear, however, that a refractory-period explanation would have great difficulty in explaining these results without incorporating additional mechanisms. Given that there is no blindness when no context is supplied around critical words, but that massive blindness occurs when they are surrounded by three other words using the same temporal parameters, the explanation that blindness results only from a refractory period for retokenization simply does not work. Instead, to preserve that inhibitory account, one would have to devise a way to explain how activating the type representations of the context words can inhibit the retokenization of the critical word or why presenting three context words as masks causes inhibition of tokenization when no inhibition occurs in their absence.

On the assumption that some way can be found to rescue the inhibition hypothesis, at least for cases in which repetitions are embedded within other stimuli, we do not rest our case on only those observations. Instead, we now attempt to demonstrate the inadequacy of that hypothesis even in cases in which repetitions are embedded in other stimuli.

Experiment 2: Search for Repetition

In a paradigm commonly used to investigate repetition blindness, subjects are given a list of words, one of which may be repeated. Following the list, they may be asked whether any word was repeated (as in Experiment 1), to identify which word was repeated, or how often a particular word occurred (as in Experiments 2–5). In such paradigms, subjects are aware that there may be repetition but are not aware in advance of what stimulus may be repeated. This latter detail of the experimental procedure is neutral under the assumptions of the type-token account. According to that account, repetition detection, when successful, is a passive accomplishment. Each successive word activates its respective type and also sets a flag on that type; repetition is automatically registered when a second occurrence sets a second flag. To know that repetition has occurred, the subject has only to review the presented words and count the flags. Under those assumptions, the fact that the subject is unaware in advance of the identity of the word that may repeat is irrelevant, because that knowledge is not part of what causes the detection of repetition.

Under the assumptions of the construction-and-attribution account, however, that detail of the experimental procedure is not neutral but may actually be the source of the blindness effect. According to that account, no stimulus simply *registers* in mind by activating a representation. Instead, on every occasion of encountering a stimulus, the person must construct a percept and a cognition about it. This construction is guided by representations of prior experiences of doing the same job on similar stimulus structures in the past, allowing construction to be performed efficiently on well-known stimuli. But even with such stimuli, perception is always an act of interpretation and attribution. Under that assumption, knowledge that a stimulus is repeated, like knowledge of the identity of the stimulus, must be constructed and attributed to the stimulus.

To know that a stimulus is repeated requires additional processing beyond that needed to know its identity. This additional processing might involve one of a variety of activities (as discussed in the General Discussion section), perhaps including a comparison of the identity of the stimulus just constructed with the identities of stimuli recently processed. When subjects know in advance which stimulus might be repeated, they can withhold this additional processing until a stimulus with the critical identity is encountered. When subjects do not know this information in advance, however, the additional processing must be performed on every stimulus, testing whether each successive word is a repetition. This requirement means that the repetition blindness effect observed with rapid lists may not be a problem specifically associated with detecting the reoccurrence of a particular stimulus but may instead be caused by the need to perform such additional processing on all of the nonrepeated stimuli.

We examined this idea in three steps (Experiments 2A–C). In Experiment 2A, we presented six-word lists at a duration of 120 ms each. After each list, the subject was shown a probe word, which could have been in the list zero, one, or two times; the subject was asked how often it had occurred. In Experiment 2B, we replicated Experiment 2A in complete detail, with only one change: The probe was presented in advance of each trial, instead of after it. As in Experiment 2A, subjects were asked to make a frequency judgment about the occurrence(s) of the probe. Experiment 2C was the same as Experiment 2B, except that on one quarter of the trials, after making the frequency judgment about the probe, the subject was also asked to make a recognition judgment about some other word. We intended these catch trials to make sure that the subjects examined the whole list.

Method

Subjects. We tested 21 subjects in Experiment 2A, 20 in Experiment 2B, and 15 in Experiment 2C.

Materials. Because these experiments originally were conducted as part of a larger study of false memory, we used stimuli taken from the Appendix presented by Stadler, Roediger, and McDermott (1999). These stimuli consisted of lists of 15 associates generated from words such as *ANGER*, with associates (in descending order of associative frequency) *MAD*, *FEAR*, *HATE*, *RAGE*, *TEMPER*, *FURY*, *IRE*, *WRATH*, *FIGHT*, *HATRED*, *MEAN*, *CALM*, *EMOTION*, and *ENRAGE*. We split each list of associates into two smaller lists, one consisting of the odd-numbered associates, beginning with the third word (e.g., *HATE*, *TEMPER*, *IRE*, etc.), and the other consisting of the even-numbered items, beginning with the fourth word (e.g., *RAGE*, *FURY*, *WRATH*, etc.), with the 15th word discarded. The two highest associates were reserved for special treatment, as described below. This resulted in 72 lists of 6 words each. We assigned 18 of these lists to each of four test conditions at random, except that we assigned the 2 lists belonging to any particular generating word to different conditions. Assignment of lists to conditions was independently randomized for each subject.

Procedure. In each trial of each experimental condition, the subject was shown a list of six words, always in descending order of associative strength. Each trial began with a *READY* prompt. On a keypress by the subject, the screen blanked for 250 ms. Following that, each word was presented in uppercase letters at the center of the screen for 120 ms followed immediately by the next word. These six words will be referred to as the RSVP list. Immediately following the list, a probe word was displayed on the screen, with the instruction *Did this word occur zero, one or two times?*

In the three experimental conditions of interest, one of the reserved words (*MAD* or *FEAR*, in the example, depending on whether the list was made up of the odd- or even-numbered associates) was presented as a recognition probe. In the first condition, that probe was not shown in the preceding RSVP display; in the second, it was presented once, at random from the second through the fifth list position, replacing the word that would otherwise have been shown in that location. In the third condition, the target word was shown twice in the RSVP list, replacing either the second and fourth or the third and fifth words in that list (i.e., repetition with a lag of one). The placing of the associate in the list at those locations ensured that it was always pre- and postmasked by at least one other word. In addition, each list was pre- and postmasked by a row of symbols (e.g., *#$%*, etc.) presented for 250 ms. A fourth condition was also conducted, in which the generating word was presented as a target but not within the RSVP list. We included this condition because of our interest in the false-memory effect (cf. Roediger & McDermott, 1995). However, it is not relevant to the issue of repetition blindness, and we do not discuss it here.

The procedure of Experiment 2B was similar to that of Experiment 2A, with the exception that the word to be used as a probe after the trial was presented for 2 s in advance of the trial, with a 250-ms blank ensuing before the RSVP list began. Experiment 2C was identical to Experiment 2B, except that on one trial in four, at random, after making the frequency judgment on the probe word, the subjects were required to perform recognition on a second probe. This probe was a word that would ordinarily be presented in Position 2, 3, 4, or 5 of the RSVP list. It was actually shown in the RSVP list in the first test condition (when the first probe was not shown in the RSVP list) but not in the others (being replaced by the main probe).

Results and Discussion

In Experiment 2A, when a probe word had not been shown in the list, the subjects produced few false alarms and never reported the word as having occurred twice (see Table 2). When the probe was shown once in the list, subjects reported it as having occurred at least once on 69% (.68 + .01) of the trials, otherwise claiming that it did not occur. If repetition detection consisted simply of the independent encoding and reporting of two occurrences, one might expect subjects to detect both occurrences of a repeated word on $.69^2$, or 48%, of trials. When the probe had been presented twice, subjects reported seeing it at least once on 88% of trials, but they reported seeing it twice on only 11% of trials. The difference between predicted and actual report of repetition (37%) was reliable, $F(1, 20) = 122.70$, $MSE = 0.01$, $\eta^2 = .86$. By that criterion, the subjects were therefore substantially blind to the occurrence of repetition.

As discussed earlier, Experiment 2B was an exact replication of Experiment 2A, with the sole exception that the probe was presented before instead of after the RSVP list. Experiment 2C was identical to Experiment 2B except for the extra recognition test on catch trials. If repetition blindness results from inhibition of tokenization because of a refractory period for retokenizing a particular type representation, then it must be as large under these circumstances as it was in Experiment 2A, because the time course of list presentation was identical in the three studies. The subjects in Experiments 2B and 2C, however, were made aware in advance of each list which word might occur repeatedly. Thus, if repetition blindness is due to the need to test the repetition status of nonrepeated stimuli, then the effect should be substantially reduced or abolished under these circumstances.

The data for those studies are also shown in Table 2. Whereas the subjects in Experiment 2A succeeded in identifying repetition on only 11% of trials, subjects in Experiments 2B and 2C succeeded on 45% and 46% of trials. These between-group compar-

Table 2
Experiment 2: Presentation Frequency Claims in Recognition and Search Tasks

	Claimed presentation frequency of probe								
	Experiment 2A			Experiment 2B			Experiment 2C		
Actual frequency	0	1	2	0	1	2	0	1	2
0	.91	.09	.00	.96	.04	.00	.93	.06	.01
1	.31	.68	.01	.10	.87	.03	.18	.81	.01
2	.12	.77	.11	.05	.50	.45	.10	.44	.46

isons were highly reliable: $F(1, 39) = 32.39$, $MSE = 0.04$, $\eta^2 = .45$; and $F(1, 34) = 27.65$, $MSE = 0.04$, $\eta^2 = .45$, respectively. In Experiment 2C, subjects achieved 62% hits and only 19% false alarms on catch trials, $F(1, 14) = 117.44$, $MSE = 0.01$, $\eta^2 = .89$, indicating that they were indeed attending to the whole list rather than to just the cued word.

Although repetition detection was substantially better in Experiments 2B and 2C than it was in Experiment 2A, one might argue that this change was simply due to improved overall ability to detect the critical item when it was preidentified for subjects before the list. For example, the probability of detecting the target when it occurred once improved from .69 in Experiment 2A to an average of .85 in Experiments 2B and 2C. One way to compare the relative size of the failure to detect repetition across Experiments 2A–C that takes into account differences in overall target detectability is to compute the probability of detecting both occurrences of a target, given that one of its occurrences is detected. For Experiment 2A, in which subjects did not know in advance which item might repeat, this probability was .11/.88, or .12. For Experiments 2B and 2C, the probabilities were .45/.95, or .47, and .46/.90, or .51, respectively. That analysis shows that the increased success in detecting repetition was not just an artifact of better ability to detect a probe word.

It is difficult to explain the 35% increase in detection of repetition produced by presenting the probe in advance of the RSVP list through the inhibition account, because nothing about the relationship between first and second occurrences of words within the list was changed. The only way to use the type-token account of repetition blindness to explain this result is to assume that inhibition of tokenization of a second occurrence within a rapid list can itself somehow be inhibited by a prior presentation of the word in advance of the list. But at this point, the account provides no mechanism by which the assumed refractory period can be bypassed.

Instead, we suggest that the repetition blindness observed in Experiment 2A was not a consequence of repetition per se. Rather, it was due to subjects' uncertainty about which word could potentially occur repeatedly. Without knowing that, the subjects could become aware of repetition at an uncertain location only by attempting to track the occurrences of all of the words, treating each as a potential first occurrence to be compared with later occurrences and also checking whether each was a repetition of a word already encoded. The search task simplifies the subjects' problem by allowing them to skip such extra processing for occurrences of other words, thereby performing it more efficiently on occurrences of the target word.

We do not mean to imply that the detection of repetition, when successful, necessarily consists of counting occurrences. In fact, we suggest that people can become sensitive to repetition in several qualitatively different ways, including reconstructing the contexts of different occurrences of the same stimulus and by experiencing a feeling of repetition (akin to the feeling of familiarity) when encountering a word repeated in the past on yet another occasion (see Whittlesea et al., 1995; and Whittlesea & Podrouzek, 1995, for an examination of these bases of decision). We argue only that observations of repetition blindness occurring when people do not know in advance which word may repeat do not provide sound evidence of inhibitory processes or of the idea that successful detection of repetition consists of repeated tokenization of a type representation.

Experiment 3: Repetition Blindness and Attentional Blink

The subjects in Experiments 2B and 2C were by no means completely able to detect repetition—they failed to do so on over 50% of trials. Their performance in detecting the occurrence of a single presentation of the probe (85% on average across the two studies) was also not perfect, even given the search cue. The relationship between performances in those two conditions, however, raises an interesting problem. On the assumption that subjects detect repetition in the search paradigm by detecting the separate occurrences of the repeated stimulus, it should be possible to predict the data for the repetition trials from the data obtained on single-presentation trials. Given a .85 probability of detecting each of the occurrences of a repeated word separately, and assuming that detection of two separate occurrences of the probe occurs independently, repetition should be detected on about $.85^2$, or 73%, of trials. The actual success (about 45%), however, was 28% lower than that estimate, or, to express it as a fraction, only about 62% of the predicted rate. That is, there is a residual deficit in detecting repetition compared with detecting single occurrences that is not alleviated by precuing.

We suspected that the residual deficit in Experiments 2B and 2C is a form of attentional blink (e.g., Raymond, Shapiro, & Arnell, 1992; Shapiro, Raymond, & Arnell, 1994). In this phenomenon, detection of the occurrence of the second-presented member of a predesignated target set often fails when the second target is presented in close temporal proximity to the first. Of particular interest to the current article, Chun (1997) demonstrated that both attentional blink (a nonspecific deficit in reporting any stimulus downstream from an attended stimulus) and repetition blindness (a specific deficit in reporting repetition of a particular stimulus) can occur on the same trials in the same study. He further demonstrated a case in which attentional blink continued to occur while repetition blindness was reduced through presentation of background items in black and the critical items in contrasting colors.

A similar situation—attentional blink leading to failure to detect a second target, but unrelated to target repetition—could be the appropriate characterization of the results of Experiments 2B and 2C. A common way of inducing attentional blink is to designate the search target(s) prior to list presentation. We suggest that, in introducing search cues in advance of the lists to reduce repetition blindness, we had also inadvertently induced an attentional blink for repeated occurrences.

We conducted three studies to determine whether the residual deficit in reporting repetition in Experiments 2B and 2C was an attentional blink. To test that possibility, we compared reports of one word presented twice with two words presented once each. To prepare for this examination, we conducted Experiment 3A as a near replication of Experiment 2B, changing the design slightly to make it more appropriate for comparing effects of repetition with effects of searching for two different targets. Subjects were given one word as a search target, which then was presented zero, one, or two times within the list. After the list, subjects were required to report how often the search target had occurred. Experiment 3B used a similar design, except that that there was no repetition in this study. Instead, subjects were asked to search for two different target words. Across the conditions of the study, neither, one but not the other, or both target words were shown in the subsequent list. As in Experiment 3A, subjects were asked to say whether zero, one, or two occurrences of search targets occurred. Experiment 3C was identical, except that instead of being asked after the list how many target events had occurred, subjects were interrogated separately about whether each search target had been shown.

Method

Subjects. Twenty subjects participated in each of Experiments 3A, 3B, and 3C.

Procedure. We used the lists from Experiment 2 again in Experiment 3A. As in Experiment 2B, a different search target was presented in advance of each trial. The major change was that we parametrically manipulated the list location of words that would later be shown as probes rather than allowing them to vary at random, to provide a control for Experiment 3B. On one quarter of the trials (18 trials total), the search target was not presented in the subsequent list. On half of the trials, it was presented once, equally often, in Locations 2–5. On the remaining quarter of trials, the search target was shown twice, equally often, in Locations 2 and 4 or 3 and 5. Assignment of lists to conditions and order of trials was independently randomized for each subject. As in Experiment 2B, after list presentation, subjects were asked to say whether the target had occurred zero, one, or two times in the list.

We used the 72 lists from Experiment 2 in Experiment 3B also. At the beginning of each trial, two words were presented side by side as search targets. We refer to these as the A and B targets, the A target being the word shown on the left, and the B target being the word shown on the right. Needing extra words to serve as search targets for trials on which only one or neither of the targets appeared in the list, and to preserve the relatedness of targets and lists that held for Experiments 2 and 3A, we sometimes presented the list-generating words and their two highest associates as search targets, but they were never shown in the subsequent list.

On one quarter of the trials (18 trials total), neither of the search targets appeared within the RSVP list. In that case, we used the generating word and its highest associate as A and B search targets, respectively. On another quarter, we assigned the word that would subsequently be presented in Location 2 or 3 of the list (9 trials for each location) as the A target. In these cases, we used the highest associate as the nonreoccurring B search target. On another quarter, we used the B target as the word that would subsequently be presented in Location 4 or 5 of the list (9 trials for each location) and the highest associate as the nonreoccurring A search target. On the remaining trials, both the A and B targets would be presented within the list; either the A target was shown in Location 2 and the B target in Location 4, or the A target was shown in Location 3 and the B target in Location 5 (nine trials of each). After the list, subjects were asked to judge whether neither, only one, or both of the targets had occurred. When they reported that only one had occurred, subjects were not asked which target had appeared in the list.

Experiment 3C was identical to Experiment 3B, except that after the list, subjects were shown the A target and asked whether it had occurred anywhere in the list, in a yes–no decision. In a separate judgment, they were then shown the B target and asked whether it had occurred anywhere in the list, in the same way.

Results and Discussion

Reports of the occurrence of search targets in Experiment 3A are shown in Table 3 in summary form and broken down by actual location of list presentation. Examination of the lower panel shows that presentation location had little effect on reports of either single presentations or repetitions. Inspection of the upper panel shows that the subjects had little trouble discriminating between nonpresented and once-presented items. Moreover, like subjects in Experiment 2B and 2C, the subjects in this search paradigm reported many more occurrences of repetition (56%) than did subjects in the nonsearch paradigm of Experiment 2A (11%). Comparing the rates of reporting the occurrence of single versus repeated search targets, however, subjects again had a deficit in reporting repetitions relative to single occurrences. Using the rates of reporting a single presentation as having occurred either once or twice (.88 + .03, from Table 5, upper panel, second row) as an index of the likelihood of detecting a single occurrence, one could expect subjects to report $.91^2$, or 83%, of repetitions if repetition detection consisted of counting occurrences. The actual rate of report (56%, from Table 3, upper panel, third line) was 27% less than this predicted rate. Put another way, the observed rate of repetition detection was only 67% of the predicted rate, which is very similar to the deficit observed in Experiments 2B and 2C, in which repetition detection achieved 62% of the predicted rate.

Table 3
Experiment 3A: Frequency Reports

Presentation	Claimed presentation frequency: 0	1	2
Frequency			
Search target not shown in list	.92	.08	.00
Search target shown once in list	.09	.88	.03
Search target shown twice in list	.02	.42	.56
Location			
Search target shown once in 2	.10	.88	.02
Search target shown once in 3	.12	.85	.03
Search target shown once in 4	.08	.88	.04
Search target shown once in 5	.11	.84	.05
Search target shown twice in 2 and 4	.01	.43	.56
Search target shown twice in 3 and 5	.02	.42	.56

To investigate whether this deficit was a residual repetition blindness effect (i.e., directly associated with repetition) or instead an example of attentional blink (resulting from search for two targets that happened to be the same word), in Experiment 3B, we required subjects to search for two different targets rather than the same one twice. As seen in Table 4, the subjects in this study had more difficulty in reporting the dual occurrence of two words (40%) than subjects in Experiment 3A had in reporting two occurrences of one word (56%). Inspection of Table 4 also shows that, in contrast with the results of the last study, location affected the probability of reporting the single occurrence of a word when subjects searched for two targets but only one target word was shown in the list: Erroneous reports of "zero occurrences" (complete misses) were 12% greater for A targets shown in Location 3 than for A targets shown in Location 2 and 13% greater for B targets shown in Location 5 than for B targets shown in Location 4: $F(1, 19) = 11.91$, $MSE = 0.01$, $\eta^2 = .38$; and $F(1, 19) = 12.06$, $MSE = 0.01$, $\eta^2 = .39$, respectively. Further, more erroneous reports of "zero occurrences" occurred for B items in general than for A items, $F(1, 19) = 12.97$, $MSE = 0.01$, $\eta^2 = .41$. These observations suggest that searching for two targets is not the same as searching for a single target that may occur repeatedly: The former is affected by serial position, but the latter is not. We do not

Table 4
Experiment 3B: Frequency Reports

Presentation	Claimed presentation frequency 0	1	2
Frequency			
Neither target shown in list	.93	.06	.01
One target shown in list	.24	.71	.05
Both targets shown in list	.09	.51	.40
Location			
A target shown in 2	.12	.81	.07
A target shown in 3	.24	.72	.04
B target shown in 4	.22	.71	.08
B target shown in 5	.35	.60	.05
A target shown in 2 and B target shown in 4	.07	.48	.45
A target shown in 3 and B target shown in 5	.12	.53	.35

have evidence to explain this difference, but we speculate that it reflects different attentional strategies. In searching for a single item that may occur twice, subjects might adopt a relatively passive "wait until you see it" approach, whereas in searching for two different words, they respond to the perceived extra difficulty of the dual search by attempting also to encode each successive item.

The critical issue, however, was whether a deficit in reporting double target presentations would also occur in this study, in which the two presentations involved different words. We again used the rates of reporting a single-target presentation trial as having presented either one or two targets as an index of the likelihood of detecting the occurrence of a word at some particular location. For the A target, the probability of its detection when the B target was not shown was (from Table 6, lower panel, first two rows): $(.81 + .07 + .72 + .04)/2 = .82$. The probability of detecting the B target when the A target was not shown was (from Table 4, lower panel, third and fourth rows): $(.71 + .08 + .60 + .05)/2 = .72$. Those probabilities lead to a prediction that the detection of both targets, when both were shown, should occur with probability $.82 \times .72 = .59$, which is clearly an overestimate of the obtained .40. In fact, the observed probability of correctly detecting a dual target presentation is only $.40/.59 = 68\%$ of the predicted size. That fraction is nearly identical to the corresponding fraction computed for report of actual repetition in Experiment 3A, which was 67%. The similarity between these two values suggests that whatever causes a deficit in reporting double occurrence of a repeated word also causes a deficit in reporting the dual occurrence of two different words. In turn, this observation suggests that the deficit in Experiment 3A has little, if anything, to do with repetition, but instead shows an attentional blink.

We tested that idea further in Experiment 3C, in which subjects were interrogated separately about the occurrence of each target after the list. Examination of Table 5 shows, as in Experiment 3B, decreasing sensitivity to the single occurrence of one of the targets with increased serial position: Accurate claims of the A target in Locations 2 and 3 were .92 and .82, and accurate claims of the B target in Locations 4 and 5 were .73 and .67. The differences between report of A and B targets and between earlier and later serial positions were reliable: $F(1, 19) = 36.18$, $MSE = 0.02$, $\eta^2 = .66$; and $F(1, 19) = 7.63$, $MSE = 0.02$, $\eta^2 = .29$, respectively.

More important for current purposes, Experiment 3C demonstrated the source of the report deficit seen in Experiment 3B when both search targets were presented in a list. As shown in Table 5, report of the A target was about the same size regardless of whether the B target was also shown (.87 vs. .84;, $F < 1$), whereas report of the B target was substantially smaller when the A target was also shown (.43) than when it was not shown (.70), $F(1, 19) = 37.41$, $MSE = 0.02$, $\eta^2 = .66$ (Table 5, upper panel, third and fourth rows). This deficit in report of the B target, again computed as a fraction, is .43/.70, or 62%, which is very similar in magnitude to the deficits observed in Experiments 3A and 3B. The deficit in report of the B target is most easily understood as an attentional blink, a case in which detection of the first target interfered with detecting the second. Comparing the results of Experiment 3A (containing repetition) with Experiments 3B and 3C (with no repetition), it seems clear that the residual difficulty in repetition detection observed in Experiment 3A was not due to repetition per se but was, instead, due to an attentional blink caused by presenting recognition probes ahead of the lists.

Taken together, Experiments 2 and 3 lead to the conclusion that a major factor causing the phenomenon of repetition blindness is the subject's lack of awareness of what potentially could repeat. When subjects are aware of the identity of that stimulus, detecting repetition appears to be no more difficult than is detecting the cooccurrence of two quite different precued stimuli. In contrast, the attentional blink can occur even when subjects are fully aware of the identities of both stimuli for which they are searching. It is thought to reflect a limited capacity for processing incoming information (e.g., Raymond et al., 1992). People appear to have the capacity to scan a series of stimuli, acquiring some information about each but without committing sufficient resources to any of them to produce full identification in the moment. In that case, they can later reproduce a number of the stimuli, as illustrated, for example, by the catch trials of Experiment 2C. When one of those stimuli engages full attention, however, it initiates a process of consolidation, leading to conscious identification of the stimulus in the moment (Potter, Staub, & O'Connor, 2002). This process takes time; it also appears to tie up all available processing resources, so that little if any information about a stimulus presented during this period is registered (but see Martens, Wolters, & van Raamsdonk, 2002, for a provocative exception).

Table 5
Experiment 3C: Frequency Reports

Presentation	Claims of occurrence A target	B target
Frequency		
Neither target shown in list	.06	.08
A target shown in list	.87	.09
B target shown in list	.10	.70
Both targets shown in list	.84	.43
Location		
A target shown in 2	.92	.06
A target shown in 3	.82	.11
B target shown in 4	.08	.73
B target shown in 5	.11	.67
A target shown in 2 and B target shown in 4	.92	.43
A target shown in 3 and B target shown in 5	.76	.43

It is clear that the attentional blink can be described as inhibitory, in the sense that it prevents processes that otherwise would be conducted. It is a nonselective effect, however, as any stimulus presented during the critical period suffers the same fate, regardless of its relationship or lack of relationship to the attended stimulus. As such, this mechanism is of a completely different kind than the inhibitory process argued to underlie repetition blindness, which is supposed to be specific to the second occurrence of a repeated word. That is, repetition blindness is thought to occur not by making available or tying up general resources but by activating or suppressing the activation of a memorial representation of a particular stimulus. For that reason, we prefer to use the more neutral term "interference" to describe the attentional blink, just as we prefer to use the term "facilitation" rather than "activation" to describe the effects of nonspecific factors such as attentiveness.

Although we have emphasized the consequences of not knowing the identity of the target word in advance of the trial, that is not the only source of repetition blindness. Johnston et al. (2002) presented a case in which substantial repetition blindness occurred, although the subjects were not in much doubt about the targets. Subjects viewed RSVP lists composed of letters, searching for the letters *A* and *B*. Lists could contain one instance of either *A* or *B*, or two instances, either repeated or not repeated (*AA*, *BB*, *AB*, *BA*). Subjects were to report the number of targets (one or two) following each list. Nonrepeated dual targets (*AB*, *BA*) were correctly reported 92% of the time, whereas repeated targets (*AA*, *BB*) were reported only 66% of the time. The problem in this case is clearly not one of uncertainty about the targets in advance. Compared with our experiments, however, Johnston et al. made the situation more difficult for the subjects in a different way. In our Experiments 2B, 2C, and 3A, one word was preidentified, and subjects were asked to determine whether it occurred zero, one, or two times. In Experiments 3B and 3C, two words were preidentified, and subjects were asked to determine whether either, neither, or both occurred. That is, in the earlier studies, the subjects were responsible for knowing how often a single target occurred; in the latter studies, the subjects were responsible only for knowing how many targets occurred, but they were not responsible for repetition detection. The task presented by Johnston et al. combined these demands, requiring the subjects to know both how many target words were presented and how often each was presented. They thus created a different kind of uncertainty by presenting outcomes varying on two dimensions, increasing the number of possible outcomes to six. That subjects had difficulty accurately determining number of occurrences while also keeping track of which targets were shown is not surprising. Increasing the demand further, by adding in variation on another dimension (e.g., the case or font in which stimuli are presented) would predictably cause even more difficulty in tracking repetition.

Our approach to the problem of what causes repetition blindness is simple. We suggest that knowledge of repetition must be constructed by comparing each occurrence of a candidate word with other candidates. Any task factor that facilitates that comparison will reduce the effect; any additional requirements that complicate or distract from that process will increase the effect. As we have shown here, one factor that simplifies repetition detection is knowledge of what will repeat. Another is knowledge of where in the list repetition might occur (Whittlesea & Hughes, in press). Another factor that can help, independent of the other two, is that distractor stimuli all be the same or that they not be familiar units that attract attention (see Experiment 1). When none of these resources is available, constructing a perception of repetition requires the person to process every stimulus to a depth sufficient to be able to compare it with all other stimuli already seen or yet to be seen. Under those circumstances, strong repetition blindness will be observed. These resources are less necessary to know that a word has been seen once, and later to report its identity, because that knowledge does not require the comparison processes necessary to know that it occurred repeatedly.

Experiment 4: Orthographic Repetition Blindness

Thus far, we have concentrated on refuting the inhibition explanation of repetition blindness. We now attempt to demonstrate how the ideas of construction and attribution can help to understand both interference and facilitation effects in the processing of words within and after rapid lists.

In a procedure related to that used to show the repetition blindness effect, subjects can be presented with two stimuli (such as *BENCH* and *BUNCH*) that are orthographically related, but not identical, within a rapid list. This procedure changes the subject's problem (and the dependent variable) from detecting the number of occurrences of a particular stimulus (i.e., repetition detection per se) to realizing that two orthographically similar stimuli have occurred. In recalling the list, subjects in such studies often selectively fail to report the second member of such pairs, although they can report a word presented at that same list location if it is not related to the first word (e.g., Bavelier et al., 1994; Morris & Harris, 2002). In an alternative, they may report the second but selectively fail to report the first (Bavelier et al., 1994; Masson et al., 2000).

According to the activation–inhibition account of rapid list processing, when two different but orthographically related stimuli, C1 and C2, are presented in close temporal proximity, one of two things can occur. One is that C1 is encoded well enough to cause token individuation and later report of that occurrence. In that case, identification of C2 is inhibited, either because it is mistakenly encoded as C1 (Bavelier et al., 1994) or because in the course of identifying C1, lexical neighbors such as C2 are inhibited (Chialant & Caramazza, 1997). In an alternative, C1 is not encoded well enough to cause token individuation and subsequent report. Its type representation, however, may be somewhat activated, causing preactivation of the representation of C2 and consequent facilitation of individuation (and later report) of C2 when it occurs (Morris & Harris, 2002). That is, the account may be able to explain the occurrence of either repetition blindness or repetition priming for C2 at short intervals.

In contrast, according to the construction-and-attribution account that we favor, the restricted processing of stimuli presented in rapid lists often causes the encoding of incomplete representations of their structures. A subject is likely to encode some portion of the orthographic structure of the stimulus but not have enough to become aware of the identity of that stimulus before beginning to process the next. (The exception is when partial processing of one stimulus attracts enough attention to make the subject break off processing subsequent list items and instead complete identification of that item. This type of processing shift takes time and cognitive resources and causes the attentional blink, as discussed earlier.) Such fragmentary representations are to a greater or lesser extent ambiguous. In using them later to report the occurrence of particular words, subjects are forced to interpret the partial information they were able to encode, imposing on it an identity that seems plausible. The decision process used in such cases can be biased by cues made available in the test (Masson et al., 2000). In the same way, the processing performed online during presentation of the list can be biased by contextual information made available prior to the list. In Experiments 4 and 5, we attempted to demonstrate both of these sources of variation in construction and attribution.

Method

Subjects. Twenty-three subjects participated in Experiment 4A, and 18 subjects participated in Experiment 4B.

Procedure. We generated a set of 72 words, each of which was six letters long (e.g., *RECENT*, *SPRAIN*, *ACCORD*). Each of these words contains a four-letter word across its last four letters (*CENT*, *RAIN*, *CORD*) that has no semantic connection to the larger word. We used the same six-word lists as in Experiments 2 and 3 but inserted one of the six-letter words into each. Pairing of the lists with the six-letter words was random and rerandomized for each subject, so that there was no overall connection between a target word and the rest of the items in its list. One of these six-letter words was inserted into each list, with equal frequency at Location 4 or 5 (replacing the word that would ordinarily occur there). On half of the trials, the orthographically related four-letter word was also inserted into the list, always two locations earlier than the longer word (i.e., at Location 2 or 3), creating a lag of one between the pair.

In Experiment 4A, the subjects were tested in a search paradigm on half of the trials. On these trials, the critical six-letter word was presented for 2 s ahead of the list. The same word was shown again after the list as a recognition target. On the remaining trials, the subjects were tested in a simple recognition paradigm: No search cue was given, but the critical word was presented after the list in the same way as on search trials. (In both cases, the word presented as a search and/or recognition cue always occurred within the list. This procedure was followed to keep the number of conditions at a minimum. The subjects were instructed that it occurred in only 50% of lists. That the subjects believed this false instruction is demonstrated by the rates of claiming to recognize it, which are well below 100%, as shown in Experiment 4's *Results and Discussion* section.) Presence versus absence of a four-letter word and presentation versus nonpresentation of a search cue were manipulated orthogonally; trials of the various conditions were presented in an independently determined randomized order for each subject. Experiment 4B was similar, except that no words were presented as cues after any list: The remembering task was changed to free recall of the entire list. As in Experiment 4A, the critical six-letter word was presented as a search cue before the list on half of the trials (and the subjects were again instructed that that word occurred with only 50% probability in any list); that factor was again manipulated independent of presentation of the four-letter word.

In both studies, the subjects were told about the orthographic similarity of four- and six-letter critical words, using *ACCENT* and *CENT* and *RECORD* and *CORD* as examples. They were correctly informed that the four-letter words would occur on half of the trials but falsely told the same thing about the six-letter words, so that they understood that both, neither, or one but not the other might be presented on a particular trial. In both studies, the subjects were also warned that the orthographic similarity could cause them to misidentify words, so they were to attempt to avoid confusing these items.

Results and Discussion

In Experiment 4A (Table 6), on trials when subjects performed simple recognition (without a search cue), including a related four-letter word in the RSVP list increased recognition hits by 11%, $F(1, 22) = 8.05$, $MSE = 0.02$, $\eta^2 = .27$. This increase in recognition can be explained in two ways. By the logic of activation and inhibition, the four-letter word (e.g., *VINE*) was not well encoded and not tokenized during the list. Its type, however, was activated to some extent, causing preactivation of the type representations of orthographically related words (e.g., *RAVINE*). This activation allowed subjects to encode the latter word more effectively when it occurred. In an alternative, by the logic of construction and attribution, with no cue for search, the subjects processed every word but achieved only a fragmentary encoding of each. In consequence, when *RAVINE* was presented as a test probe, subjects sometimes erroneously attributed an incomplete encoding of *VINE* to a prior experience of *RAVINE*. That is, by this account, orthographic similarity increased recognition hits on the six-letter word by causing mistaken identification of the four-letter word rather than enhanced detection of the six-letter word.

Table 6
Probability of Claiming a Critical Item Was Present in a Rapid Serial Visual Presentation List (Experiment 4A) and Probability of Recall of Critical Items (Experiment 4B)

Experiment 4A

Presentation	6-letter word: Recognition	6-letter word: Search
Four-letter word present	.53	.70
Four-letter word absent	.42	.88

Experiment 4B

Presentation	6-letter word: Free Recall	6-letter word: Search	4-letter word: Free Recall	4-letter word: Search
Four-letter word present	.21	.57	.32	.13
Four-letter word absent	.18	.50	.00	.00

In that same study, on trials in which subjects were precued in advance of the list, presenting a related four-letter word two locations earlier than the critical six-letter word had the opposite effect, decreasing recognition hits by 18%, $F(1, 22) = 15.54$, $MSE = 0.02$, $\eta^2 = .41$. As with the enhancement effect seen in the recognition task, the orthographic repetition blindness effect found in the search task could also be explained in two ways. Both explanations begin with the assumption that on some occasions, when searching the list after seeing a cue like *EXPORT*, the orthographic similarity of a four-letter word like *PORT* captured the subjects' attention. By the activation–inhibition account, it might be asserted that capture of attention caused tokenization of *PORT*, which in turn inhibited tokenization of the type representation of *EXPORT*, so that it could not be detected when presented two locations later. We instead interpret the effect to mean that, in attending to *PORT* and processing that word to complete identification, subjects realized that that was the lure word and so did not use it to make a claim about *EXPORT*. The capture of attention invoked by *PORT*, however, also produced an attentional blink, resulting in failure to process *EXPORT* extensively enough to report its occurrence. There is no evidence within Experiment 4A that would justify a preference for one explanation over the other.

In Experiment 4B (Table 6), no cue word was given after the list. On simple recall trials (no advance search cue), the four-letter word was correctly recalled on 32% of trials. Presence versus absence of a related four-letter word, however, had little if any effect on report of the six-letter word ($F < 1$). There was power of .80 to detect a difference of 8% in recall as a function of presence versus absence of the four-letter word. This result contradicts the idea that encoding the four-letter word prevented tokenizing the six-letter word. Further, the lack of effect of the four-letter words on remembering the six-letter words in the case of uncued recall supports the idea that the increased rate of claiming to remember six-letter words preceded by related four-letter words in Experiment 4A (cued recognition) occurred because presenting the test cue biased the recognition process.

On trials in which a search target was provided in advance of the list, two important results occurred. First, the six-letter target was reported about 7% more often when a related four-letter word was included, $F(1, 21) = 25.87$, $MSE = 0.01$, $\eta^2 = .25$. This result is the reverse of the effect of that factor in the recognition task of Experiment 4A. Second, the four-letter word was reported 19% less often when a search target was provided than when there was no search target, $F(1, 21) = 6.18$, $MSE = 0.01$, $\eta^2 = .79$. To explain the first of these results using the type-token account, one would have to argue that the four-letter word preactivated the six-letter word. It is difficult, however, to understand why the change from cued to uncued remembering, with all other factors held constant, would cause a switch from inhibition to activation. It is also difficult to use the type-token account to explain the lowered rate of reporting the four-letter word when a search target was provided. Within the assumptions of that account, this decrement would have to be explained as being due to inhibition caused by the search cue. Yet if that were true, the same inhibition of the four-letter word should have occurred on search trials in Experiment 4A. This constraint creates the difficulty of explaining how, in that study, an inhibited representation of the four-letter word in turn was able to inhibit the representation of the six-letter word.

To us, a simpler explanation of the effects obtained in Experiment 4B is that the subjects, knowing they were responsible for reporting all of the words, did not search selectively for the precued word. In support of that idea, the subjects were about 25% less likely to report seeing the six-letter word on trials that included a search target in this study compared with such trials in Experiment 4A. In consequence, the four-letter words less often attracted attention and so less often caused an attentional blink. To explain the decreased report of the four-letter words on cued compared with noncued trials, we again assume that the subjects achieved only a fragmentary encoding of each word in the list. We further assume that in making their reports after the list, subjects were again biased by awareness of the six-letter word presented as a search cue. In consequence, partial encoding of the short word would sometimes be interpreted as evidence that the longer word had occurred, thereby increasing reports of the longer word and decreasing reports of the shorter word.

Experiment 5: Search and Recognition With Two Targets

Experiment 4 demonstrated that the type of remembering test (recognition vs. free recall) can have dramatic effects on how orthographic repetition influences what subjects are able to report about their experiences. We investigated this idea further in Experiments 5A and 5B. In these studies, we used both the simple recognition and detection paradigms, as in Experiment 4A. The departure was that both critical words (the six-letter word and its related four-letter word) were presented as recognition test cues, and also as search cues on detection trials. In both studies, the search cues were presented side by side but were shown sequentially in test. In Experiment 5A, the six-letter word was shown in the left position of the cue display, and at test it appeared before the four-letter word. Experiment 5B was identical, except that the position of critical words in the cue display and in the test was reversed relative to Experiment 5A.

Method

Subjects. Twenty subjects participated in Experiment 5A, and 18 subjects participated in Experiment 5B.

Procedure. Experiment 5A was similar to Experiment 4A, except that in its search trials, two words (the pairs of critical six- and four-letter words) were presented together for 2 s ahead of the list. The six-letter word was always shown on the left (e.g., the subject might be shown *AUGUST–GUST* as a cue). The two words were presented again after the list, one at a time, with the six-letter word tested first. Subjects were asked whether each word individually had occurred within the list. Within the lists, the four- and six-letter words were shown at the same locations as in Experiment 4 (i.e., the four-letter word appeared before the six-letter word, separated by one intervening word). On the remaining trials, the subjects were tested in a simple recognition paradigm: No search cue was given, but both words were presented after the list in the same way as on search trials. Experiment 5B was identical, except that the four-letter words were presented on the left in search cues (e.g., *PACT–IMPACT*) and in test were presented and judged before the six-letter words. Within the lists, they were presented in the usual positions (four-letter words in Location 2 or 3 and six-letter words in Location 4 or 5). Subjects in both studies were given the same instructions about the frequency of occurrence and relationship between four- and six-letter words as subjects in Experiment 4. Thus, although subjects were told that each type of critical word (four-letter and six-letter) would appear in 50% of the lists, the six-letter critical words actually appeared in every list.

Results and Discussion

In the search trials of Experiment 5A (Table 7), the four-letter word was well-recognized (75% of trials). Presenting a related four-letter word caused a very large deficit in reporting the six-letter words, such that the six-letter word was judged old 46% less often when the four-letter word was presented in the list, $F(1, 19) = 74.18$, $MSE = 0.03$, $\eta^2 = .79$. The activation–inhibition account would explain this deficit in much the same way as in

Table 7
Probability of Claiming a Critical Item Was Present in a Rapid Serial Visual Presentation List

	6-letter word		4-letter word	
	Recognition	Search	Recognition	Search
	Experiment 5A			
Four-letter word present	.48	.40	.52	.75
Four-letter word absent	.46	.86	.21	.09
	Experiment 5B			
Four-letter word present	.43	.32	.59	.91
Four-letter word absent	.53	.67	.13	.14

Experiment 4A: Presenting the four-letter word as a search target made it easy to find in the list (hence the high recognition hits for that word); successful detection of that word caused inhibition of the representation of the related word. That account has difficulty, however, with the simple recognition trials. The subjects reported the four-letter word on about half of those trials, which, within the assumptions of that account, should often cause inhibition of the representations of the six-letter words on those trials. In contradiction of that idea, presenting versus not presenting the four-letter word in the list had little effect (2%) on report of the six-letter word ($F < 1$).

In contrast, the construction-and-attribution account again explains the deficit in the search task as an attentional blink, which we made larger than in Experiment 4A by actually presenting the shorter word as a search cue. That account would also predict that the effect seen on simple recognition trials in Experiment 4A (greater hits on six-letter words when related four-letter words were shown) would be less likely to occur, because the subjects knew, when judging the six-letter words, that they were about to judge the four-letter word contained within the six-letter word. They would thus be less likely to misattribute experience of the four-letter words to the occurrence of the six-letter words. Unlike the activation–inhibition account, this account has no reason to predict blindness in this task for six-letter words that follow related four-letter words in the RSVP lists.

In Experiment 5B (Table 7), the only change was the order of presenting four- and six-letter words in test and also in the precue, if one was shown (but not within the lists, which were presented in the same way as in Experiments 1–4 but not 5A). The four-letter words were better reported in this test than in Experiment 5A, $F(1, 36) = 11.62$, $MSE = 0.02$, $\eta^2 = .24$, in simple recognition (7%) but particularly in search (16%). This result suggests that in scanning for two items, subjects prioritized their search, primarily looking for whichever word was presented as the left member of the search cue pair.

In the search task, we again observed a large (35%) deficit in reporting six-letter words preceded by related four-letter words, $F(1, 17) = 76.49$, $MSE = 0.01$, $\eta^2 = .82$. (The absolute magnitude of the effect was smaller than in Experiment 5A, owing to the decreased hits on six-letter words occurring in the absence of a related four-letter word. That is again likely an effect of the prioritized search.) The striking effect, however, was that for the first time, we also observed repetition blindness in the simple recognition test. The six-letter words were judged old about 10% less often when preceded by a related four-letter word, $F(1, 17) = 5.00$, $MSE = 0.02$, $\eta^2 = .22$.

The fact that repetition blindness occurred in simple recognition in Experiment 5B but not in Experiment 5A is very problematic for the activation–inhibition account. The only difference between the two experiments was the order of test cues after the lists. This variation should make no difference whatsoever under the assumptions of the activation–inhibition account, because it asserts that repetition blindness is a consequence of inhibition of encoding during the speeded list. The order of test cues could not affect such within-list processing. Our alternative suggestion is that performance varied with order of test cues because of two factors. First, in reconstructing their fragmentary experiences after the lists, subjects were biased toward whichever word was presented first as a test cue. That factor by itself would tend to increase hits on six-letter words in Experiment 5A when the related four-letter word was present in the list (as actually observed in the simple recognition task in Experiment 4A). The bias in favor of the six-letter word would create a tendency to attribute mistakenly the fragmentary evidence from the four-letter word to the six-letter probe. The second factor, however, was that the six-letter words were less well encoded than the four-letter words. Evidence of this can be seen in Experiment 4B (Table 6: free recall). With no cues in test, the four-letter words were reported about 11% more often than the six-letter words, $F(1, 17) = 9.55$, $MSE = 0.01$, $\eta^2 = .35$. This difference in encoding would make the four-letter words relatively resistant, and the six-letter words relatively vulnerable, to biased interpretation under the presentation of dual test cues, resulting in the observed asymmetry in the bias of report. These results are thus also consistent with the construction and attribution account.

General Discussion

In the experiments reported in this article, we have attempted to provide evidence that at once supports the idea that construction and attribution processes lead to repetition blindness and challenges accounts that are based on the concepts of activation and inhibition. In Experiment 1, we demonstrated that repetition blindness is not a direct consequence of repetitions occurring close together in time, but instead is also critically dependent on embedding the repetition within orthographic stimuli that are familiar as units and on presenting different such units in each masking location. That evidence is at least difficult for the type-token account, which is based on the idea of a refractory period for retokenization, without consideration of the nature or effect of the distractors within the list.

Second, in Experiment 2, we observed a large repetition blindness effect when the target that might repeat was not presented until after the list, whereas presenting a target item in advance of the RSVP list substantially increased success in detecting repetition of that item in the list. That increase cannot be explained by the type-token account, which imputes repetition blindness to a refractory period for encoding a second occurrence. Instead, we suggest that when no target is specified in advance of the list, as in the standard repetition blindness paradigms, subjects do not know which word might repeat. They are therefore forced to attend not only to repeated occurrences but also to the identities of all words in the list. That additional load, not mentioned in type-token accounts, is one source of the difficulty in detecting repetition. Even when presenting the target in advance of the RSVP list, however, we observed a residual deficit in reporting repetition. In Experiment 3, by presenting two different search targets in advance of a list and measuring the probability of detecting each one, we observed a general impairment in reporting the second member of a pair of targets. This impairment is a form of attentional blink that occurs for unrelated targets as well as pairs of identical stimuli; it implies that the residual difficulty in detecting repetition blindness effect found when a target is presented twice in an RSVP list with a search cue has nothing to do with repetition per se.

In Experiments 4 and 5, we examined the effects of presenting orthographically similar word pairs. We found that the presence of an orthographically similar word in an RSVP list led to enhancement of, interference with, or no effect on report of a target, depending on how the target was tested and whether it appeared as a search target prior to list presentation. It is particularly difficult to use activation–inhibition accounts to explain how an orthographically similar word can have opposite effects, either activating or inhibiting the subsequently presented target, as a function of the type of test (recognition vs. recall: Experiment 4) or as a function of a test cue presented after the list (Experiment 5).

Instead of appealing to inhibition to explain the repetition blindness effect, we have argued for an understanding in terms of construction and attribution. In our view, repetition blindness is not a problem specifically associated with detecting repetition at short lags, as it is usually described by variants of type-token theory. Instead, it is a problem of detecting repetition under uncertainty of what may repeat. Under these circumstances, and lacking an automatic repetition counter, subjects must test each successive stimulus against each previous occurrence. That task is more difficult, and demands more resources, than merely encoding enough of the orthographic structure of some of the words to regenerate their identities later; it is therefore not surprising that under severe time constraints, subjects can often succeed in the latter but have difficulty doing the former. Further, the insensitivity to repetition of words observed under such conditions is probably not an isolated case; instead, it is likely to be found when subjects are asked to detect any relationship among the stimuli when operating under uncertainty about which stimuli possess the relationship. Consider an analogous situation in which a list of six words is presented, each of which has a strong antonym (e.g., *LEFT, BIG, HOT, UP, COLD, SQUARE, WHITE*). One of the items in that list may be an antonym of an earlier list item, but the subject is not informed in advance which word to check. We strongly suspect that such a task would lead to great difficulty in detecting the presence of an antonym pair, despite the ability to report a number of words from the list. This difficulty arises because it takes more processing steps to detect an antonym relation than simply to encode each word. (Even without time constraint, we suspect that many readers will have failed to detect the fact that an antonym pair occurred within that list.) We certainly would resist the temptation to refer to this phenomenon as antonym blindness and to propose an antonym-based inhibitory mechanism, by which the meaning rather than the occurrence of the word is suppressed, although that logic is parallel to that of the type-token account. We would also expect that the ability to detect an antonym pair would be substantially enhanced if subjects were given a search cue (e.g., look for *HOT* and its antonym) in advance of the list, thereby reducing uncertainty and the need to test all members of the list (cf. Experiments 2 and 3).

Rejecting inhibition as an explanation for repetition blindness contributes to an important debate about how to conceive of the fundamental operation of mind (see, for example, MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003). Advocates of accounts that emphasize inhibition typically depend on structural accounts of memory on the basis of the idea that mind has an architecture and that that structure has implications for task performance. For example, accounts based on the assumption of separate memory systems, such as semantic versus episodic (e.g., Tulving, 1983), procedural versus declarative (Cohen & Squire, 1980; Knowlton & Squire, 1995) or implicit versus explicit (Gabrieli, 1998; Graf & Schacter, 1987) assume that different parts of mind operate by different principles, which produce dissociated patterns of performance in different tasks. In such accounts, remembering is thought to involve encoding and retrieval of contextually defined experiences, whereas other activities such as conceptual learning and perception are proposed to occur through qualitatively different principles, including automatic abstraction and network organization, resulting in stable mental representations differing in strength and connectedness. In consequence, phenomena such as repetition blindness are thought to be transitory perturbations within a stable system, in which temporary activation and inhibition of representations drive behavior but leave little residual effect.

In contrast, processing accounts of the kind we have proposed suggest that all cognitive and perceptual operations are performed through a common set of principles. Each encounter with a stimulus is a learning event that both draws on prior learning and can have consequences for later processing. Each such processing event consists of a specific combination of task, context, stimulus properties, and cued memory traces (cf. Whittlesea & Leboe, 2000). It is assumed that each such experience is preserved intact, rather than being aggregated into a stable, abstract representation in a hypothetical semantic memory system. As a result, the representation of any thing the person knows about is distributed across a multitude of experiences, some similar, some widely different in content (operations performed, aspects of stimulus and context on which the operations are performed). Lacking any single, consolidated representation of the stimulus such as a type, there is nothing to inhibit. Instead, representations of particular processing experiences, cued selectively or in parallel with many other traces by the current combination of task, stimuli, and context, guide the construction of the current stimulus event, giving it organization and meaning, and guide the attributional process, giving rise to a specific subjective reaction to the processing. Such guidance may facilitate performance relative to some goal, when the cued representations of prior processing experiences act in synergy to impose an appropriate characterization on the current stimulus event. It may also cause interference, if the cued representations guide the construction toward an organization inappropriate to the goal.

Facilitation and interference are phenomena—real differences in observable performance. However, activation and inhibition are not. Instead, they are hypothetical constructs, explanations based on as-yet-unproven assumptions about representation and processing. They are appealing constructs, because the assumed mechanism directly resembles the effect: Facilitation is an increase in performance, and so must result from activation of whatever is responsible for that performance; interference is a decrease in performance, and so must result from inhibition of that same agency. Such reasoning, inferring mechanism directly from effect, is revealed clearly in inhibition accounts of repetition blindness. As illustrated by the experiments in this article, however, we suggest that such reasoning is misleading. Instead, we believe that repetition blindness (and also successful report of the occurrences of stimuli) can best be understood as resulting from the processes of construction and attribution. Because these processes do not directly resemble their outcomes, it is more difficult to understand how they produce such phenomena. However, given the evidence that these processes can explain critical aspects of the data that the more obvious mechanisms cannot, we suggest that they are worth the effort.

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Appendix

Six- and Four-Letter Words Used in Experiments 4 and 5

SPRAIN–RAIN	UPROAR–ROAR	EXPERT–PERT	RESORT–SORT
ACCORD–CORD	RAVINE–VINE	EXPOSE–POSE	REWARD–WARD
ADVENT–VENT	REFILL–FILL	EXTENT–TENT	SEARCH–ARCH
ALCOVE–COVE	SCROLL–ROLL	FEMALE–MALE	SPLASH–LASH
AMBUSH–BUSH	REVOLT–VOLT	GREASE–EASE	SPREAD–READ
ASSENT–SENT	MALADY–LADY	INCOME–COME	SPRING–RING
ATTEST–TEST	MOHAIR–HAIR	INSIDE–SIDE	STABLE–ABLE
ATTIRE–TIRE	ORDEAL–DEAL	ISLAND–LAND	DEFILE–FILE
AUGUST–GUST	ACCENT–CENT	NOBODY–BODY	THRUST–RUST
DETOUR–TOUR	ADVICE–VICE	POLICE–LICE	STRAIN–RAIN
DOMAIN–MAIN	AFFAIR–FAIR	RECALL–CALL	STREAM–REAM
ENCORE–CORE	APPEAL–PEAL	RECORD–CORD	BEHALF–HALF
ESTEEM–TEEM	ARREST–REST	REFORM–FORM	STROLL–ROLL
IMPACT–PACT	EFFORT–FORT	REFUSE–FUSE	TALENT–LENT
INMATE–MATE	ELEVEN–EVEN	REMARK–MARK	BREACH–EACH
INSTEP–STEP	EMPLOY–PLOY	REMOVE–MOVE	CLOVER–OVER
INTAKE–TAKE	ESCAPE–CAPE	REPAIR–PAIR	PLEDGE–EDGE
INWARD–WARD	TWITCH–ITCH	REPORT–PORT	STRIPE–RIPE

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