

Sentence Context, Word Recognition, and Repetition Blindness

Alison L. Morris and Catherine L. Harris
Boston University

When the sentence *She ran her best time yet in the race last week* is displayed using rapid serial visual presentation, viewers sometimes misread *rice* as *race* (M. C. Potter, A. Moryadas, I. Abrams, & A. Noel, 1993). Seven experiments combined misreading and repetition blindness (RB) paradigms to determine whether misreading of a word because of biasing sentence context represents a genuine perceptual effect. In Experiments 1–4, misreading a word either caused or prevented RB for a downstream word, depending on whether orthographic similarity was increased or decreased. Additional experiments examined temporal parameters of misreading RB and tested the hypothesis that RB results from reconstructive memory processes. Results suggest that the effect of prior context occurs during perception.

Questions concerning the role of sentence context in the process of visual word recognition continue to be some of the most contentious in the field of cognitive psychology. Although the idea that sentence context can influence visual word recognition is widely accepted, there is considerable controversy over just how, and when, context has its effects. Competing models of word recognition can be broadly classified as modular or interactive, with the role of sentence context providing a crucial difference between the two types. In modular models (e.g., Fodor, 1983; Forster, 1979), word recognition is strictly a bottom-up process; it is solely dependent on stimulus information and impervious to the influence of higher-level context. Sentence context influences postperceptual stages, such as integration of the word with the remainder of the sentence. However, context can also fill in when the perceptual module fails to identify a word, as may occur under degraded conditions. Modular models account for the well-documented effects of semantic priming on lexical access by positing that spreading activation between associated word representations occurs entirely within the lexical module.

In contrast, interactive models propose that constraints arising from sentence context combine with stimulus information during early stages in word recognition, even before the perceptual module has completed its identification of the word. In some interactive models, information from higher levels (i.e., sentence context)

feeds back to influence processing at lower levels such as visual feature analysis and activation of lexical candidates (e.g., McClelland & Rumelhart, 1981).

Hybrid models, or modular-interactive models (e.g., Norris, 1986; Potter, Moryadas, Abrams, & Noel, 1993) preserve the principle of autonomy while accounting for the facilitative effects of sentence context. In the modular-interactive model of word recognition proposed by Potter et al. (1993), the first, autonomous stage outputs a set of word candidates (generally orthographic neighbors of the stimulus word) with weights attached to them; weights are determined by the relative amount of stimulus evidence for each word. Although sentence context does not influence this first stage, it does play a role in selecting a single word from the set of candidates in a second stage. The selected word, which is then consciously perceived, is the word that maximizes weight value while conforming to the constraints of sentence context. The model thus preserves bottom-up priority, as in modular models, but allows sentence context to have an effect prior to the identification of a unique lexical item.

Investigations into the mechanisms underlying the effects of sentence context on visual word recognition have used a wide variety of methodologies. These include lexical decision (Fischler & Bloom, 1979; Schuberth & Eimas, 1977; Schwanenflugel & Shoben, 1985), speeded target naming (Duffy, Henderson, & Morris, 1989; Simpson, Peterson, Casteel, & Burgess, 1989; Stanovich & West, 1981), immediate recall of rapidly presented sentences (Potter et al., 1993; Potter, Stiefbold, & Moryadas, 1998), and eye-movement monitoring in normal reading (Ehrlich & Rayner, 1981; Morris, 1994) among others. The ideal technique for studying sentence-context effects would come as close to natural reading as possible, while allowing the experimenter to control, and thus separately probe, different stages of processing. Because the ideal technique remains elusive, it is worthwhile to continue to develop new methodologies and seek converging evidence.

Immediate recall of sentences displayed using rapid serial visual presentation (RSVP; Forster, 1970) has several desirable characteristics as an experimental technique. In RSVP, words are displayed one at a time in the same location, each word overwriting the previous word. Reading in RSVP, typically at rates of 10–12

Alison L. Morris and Catherine L. Harris, Department of Psychology, Boston University.

The experiments in this article were presented as a doctoral dissertation by Alison L. Morris under the direction of Catherine L. Harris at Boston University. Portions of this work were presented at the 10th Annual City University of New York Conference on Human Sentence Processing, Santa Monica, California, March, 1997, and at the 40th Annual Meeting of the Psychonomic Society, Los Angeles, California, November, 1999.

We thank the other dissertation committee members, Jackie Liederman, Mark Reinitz, and Kay Coleman. Thanks are also extended to Wayne Dinn for testing participants and to Molly Potter for helpful discussion.

Correspondence concerning this article should be addressed to Alison L. Morris, who is now at the Department of Psychology, Boston College, 140 Commonwealth Avenue, Chestnut Hill, Massachusetts 02467. E-mail: morrisai@bc.edu

words per second, resembles normal reading in that the task involves comprehending and recalling an entire sentence rather than naming a particular word or making a lexical decision in response to it. A critical question concerns the degree to which viewers process each word as it appears, as opposed to storing the words in a buffer for later processing. A study by Potter, Kroll, Yachzel, Carpenter, and Sherman (1986) supported immediate processing. Participants were shown RSVP sentences of 8, 10, 12, and 14 words and asked to make plausibility judgments about the sentences immediately after reading them. An increase in response time with increasing sentence length would be expected if participants needed to process much of the sentence offline. Although the time to make the plausibility judgments was a bit shorter for the 8-word sentences, the longer sentences showed no pattern of increase in response time or error rate with increasing length; this finding therefore suggests that processing of RSVP sentences appears to be largely online. RSVP also has some advantages over self-paced reading; in RSVP, the experimenter controls the duration of each stimulus word, and because each word is followed immediately by the next, processing time for each word is limited (Potter et al., 1993). This technique can therefore allow the investigator to probe the early stages of word recognition.

An interesting study by Potter et al. (1993) illustrates the utility of RSVP sentence recall in studying the process of word perception as it unfolds over time. These authors used a nonword-conversion paradigm to test predictions of the various classes of word-recognition models discussed above. *Nonword conversion* refers to the tendency to misread a nonword as a similar-looking word (e.g., Ehrlich & Rayner, 1981; Forster, 1989). In the Potter et al. study, nonwords (or incongruous words) were embedded in RSVP sentences, which made them particularly vulnerable to conversion. Leading sentence contexts were created to bias reading of the nonword or incongruous word as a word compatible with the context. For example, when the sentence *She ran her best time yet in the roce last week* was presented and participants were asked to recall the sentence immediately after reading it, they frequently reported *roce* as *race*. In contrast, when shown the sentence *For supper she cooked roce with vegetables, roce* was often reported as *rice*. (A similar phenomenon, though to a lesser degree, was noted when the incongruous words *rice* and *race* were substituted in the above two sentences, respectively). It is interesting to note that misreading of words and nonwords as words compatible with the sentence context was also demonstrated when context occurred only after the critical item (e.g., *In the roce she ran her best time yet last week*), although the context effect was smaller. Potter et al. (1993) suggested that this result is most consistent with a modular-interactive model of word recognition, in which a set of possible word candidates generated by the stimulus remains active until relevant selective context becomes available.

However, the nonword-conversion results are open to a different interpretation. Although evidence suggests that much of the processing of RSVP sentences is online (Potter, 1984; Potter et al., 1986) the nonword-conversion situation represents a particular ambiguity that may instead be resolved reconstructively. It is possible that the nonwords were converted only after the sentence was read, on the basis of postperceptual problem solving. The results of this experiment would therefore not provide evidence against a modular model. Although the finding of greater bias for the context-before condition might suggest that processing was

occurring online (Potter et al., 1998), the positions of both the relevant context (before or after) and the critical items (nonword or incongruous word) were confounded with serial position. That is, in the context-before condition, the context was always near the beginning of the sentence, whereas the critical item was later in the sentence; whereas in the context-after condition, the critical item was earlier in the sentence, with the context coming later. Words appearing later in an RSVP sentence have a lower probability of being recalled (Potter et al., 1993). If earlier serial position also improves encoding, it is therefore possible that the apparent decrease in the biasing properties of the context, when the context was shifted from before to after the critical item, was due to improved veridical perception of the critical item coupled with less effective encoding of the relevant context.

Thus, a crucial issue in experiments relying on serial report from RSVP sentences concerns whether the effects in question arise during perception or during recall. The phenomenon of repetition blindness (RB; Kanwisher, 1987) may prove useful in assessing the time-course of effects such as nonword conversion. RB has stimulated a great deal of interest since the publication of Kanwisher's seminal article, and RB paradigms have recently been used in investigations of visual attention (Kanwisher, Driver, & Machado, 1995), picture identification (Kanwisher, Yin, & Wojciulik, 1999), hemispheric differences in visual word processing (Harris, Morris, & Duncumb, 1999), and properties of the orthographic units involved in word recognition (Harris & Morris, 2001b). The experiments reported in this article apply an RB paradigm to the question of the effects of sentence context on visual word recognition.

The Phenomenon of RB

RB refers to a difficulty in reporting the occurrence of two identical or orthographically similar words, relative to two non-similar words, when the words are briefly displayed and appear within a few hundred milliseconds of each other.¹ Some of the most dramatic demonstrations of RB occur when the two identical or similar words (which we designate as W1 and W2) are embedded in sentences displayed via RSVP. Viewers tend to omit W2 from their report of the sentence, even when this makes the sentence ungrammatical or nonsensical. For example, after viewing a sentence such as *When she spilled the ink there was ink all over*, participants might report having seen *When she spilled the ink there was all over* (Kanwisher, 1987). RB presents a paradox in that repetition of a word more typically results in increased rather than decreased ability to report the item—the well-known repetition priming effect. Kanwisher (1987) suggested that RB and repetition priming occur under different encoding conditions. When participants monitored RSVP word lists for the presence of repeated words (Experiment 1), robust RB was demonstrated; however, when participants were required to report only the final word in the list (Experiment 3), repetition priming was found. The crucial difference concerns the nature of the encoding of the first

¹ The phenomenon of RB is not limited to words; it also occurs for repetitions of pictures (Kanwisher et al., 1999), colors (Kanwisher et al., 1995), and even for mixed stimuli such as a picture of a sun and the word *sun* (Bavelier, 1994). However, in this article we limit our discussion to RB for letter and word stimuli.

critical word. Luo and Caramazza (1995) suggested that the magnitude of RB is determined by the encoding effectiveness of the first critical item; when the first critical item is poorly encoded, reliable RB will not be observed. RB occurs for identical words and orthographic neighbors, as well as words sharing three initial letters (e.g., *chalk*, *charm*), three middle letters (e.g., *shark*, *charm*), three alternating letters (e.g., *claim*, *charm*), or even a single initial letter (e.g., *clock*, *charm*; Harris & Morris, 2000). The size of the RB effect generally increases as the orthographic overlap between the two words increases.

Kanwisher's theoretical account of RB argues that the phenomenon occurs as a result of "type recognition without token individuation" (Kanwisher, 1987, p. 117; see also Kanwisher, 1991; Kanwisher & Potter, 1990; Park & Kanwisher, 1994). In this view, RB represents a limitation on the ability to bind a word's type representation to two different tokens, or event representations, occurring within a particular time window; thus, only one of the words is consciously perceived. However, because RB is defined as a relative deficit in reporting repeated or similar words compared with nonsimilar words the effect could arise from retrieval processes rather than from perceptual processes (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995). Luo and Caramazza (1995) investigated the locus of RB using two successively displayed single letters in uncertain locations; their work suggests a perceptual rather than a memory locus, because RB was found using a memory load of only two items. Also, a number of investigators (Hochhaus & Johnston, 1996; Kanwisher, Kim, & Wickens, 1996; Park & Kanwisher, 1994) have found reliable RB using signal detection methodology, in which the response demands were simplified, and again memory load was minimal. These findings make it difficult to defend the idea that RB arises solely from retrieval failure.

A somewhat different form of memory locus for RB in word lists and sentences has been proposed by Whittlesea and his colleagues (Whittlesea, Dorken, & Podrouzek, 1995; Whittlesea & Podrouzek, 1995; Whittlesea & Wai, 1997). In this view, RB in RSVP sentences results from report biases operating during attempts to reconstruct the stimulus at the time of recall rather than from any type of perceptual interference. Two key assumptions of the reconstructive view are that (a) items in RSVP are not processed extensively, so that words are not well integrated with their contexts, and (b) the two occurrences of an identical word in an RSVP sentence are encoded and recalled independently (Whittlesea, Dorken, & Podrouzek, 1995; Whittlesea & Podrouzek, 1995). Under these assumptions, it is possible that on reconstruction of the sentence both instances of the repeated word could be recalled in the position of the first occurrence. Such an early report bias would conform to the rules of English syntax. That is, *The yellow car passed our car on the left* would more likely be expressed as *The yellow car passed ours on the left* than *The yellow one passed our car on the left* (Whittlesea, Dorken, & Podrouzek, 1995). However, Downing and Kanwisher (1995) have criticized the experiments reported in Whittlesea, Dorken, & Podrouzek (1995) and Whittlesea and Podrouzek (1995) on methodological grounds (but see Whittlesea, Podrouzek, Dorken, & Williams, 1995). Thus, although the reconstructive memory account of RB seems plausible, some of the evidence in support of this view is problematic.

Other findings in support of the reconstructive memory account of RB can be equally well explained by perceptual accounts. Whittlesea and Wai (1997) demonstrated a reverse RB effect in sentences in which the first instance of a repeated word was presented in an anomalous context (e.g., *He poured some blue ink JAR into a JAR yesterday*). The RB effect for these sentences was localized to the first critical word rather than the second; thus the reverse in reverse RB. Whittlesea and Wai claimed that perceptual accounts cannot explain these results, because they always predict a forward effect. However, it is important to note that the probability of report of the first critical word was low (.28) even in the nonrepeated condition. An account of RB based on perceptual mechanisms would not predict reliable RB for the second critical word when the first critical word was poorly encoded (Luo & Caramazza, 1995). Furthermore, because the first critical word differed from the repeated to the nonrepeated condition, increased report of the first critical word in the nonrepeated condition could be due to between-word differences in frequency or contextual fit.

Two recent event-related potential (ERP) studies provide compelling evidence in favor of an online, perceptual locus for RB. Schendan, Kanwisher, and Kutas (1997) displayed RSVP lists of three words mixed with rows of symbols. The word lists either contained two identical words plus a filler word or two nonidentical words plus the filler; an RB effect was noted in the form of more incorrect responses to repeated than to nonrepeated stimuli. When participants reported both of the repeated words, a positive potential at 220 ms, associated with repetition priming, was noted (no such positivity was noted for nonrepeated lists). In comparison, when only one of the repeated words was reported (which could be due to RB), repeated trials resembled nonrepeated trials, with no early positivity. Further differences were noted in later ERP components (400–800 ms): Correct trials (whether repeated or nonrepeated) were associated with posterior positivity, whereas incorrect trials were not. This later component is thought to reflect explicit identification of the stimulus. Thus, repeated incorrect (i.e., RB) trials resembled nonrepeated correct trials in early components but resembled nonrepeated incorrect trials later. Schendan, Kanwisher, and Kutas suggested that an early perceptual operation, possibly in the basotemporal neocortex, serves to discriminate novel from repeated stimuli and that in RB (at least for identical words), repeated stimuli are misclassified as novel. Such misclassification would then interfere with explicit identification of the stimulus.

In another ERP study, Bavelier (1999) showed participants short RSVP word lists, half of which had repeated words; participants were asked to report all the words from the list. Differences in the ERP data between correct and incorrect repeated trials were noted as early as 200 ms after the onset of the second of the repeated words. Correct repeated trials showed a larger positive component over right parieto-occipital areas, whereas incorrect repeated trials showed a larger positive component over the left anterior temporal areas. No such differences were noted between correct and incorrect nonrepeated trials. The early nature of these effects suggests that RB arises from difficulties with the individuation stage of object perception; furthermore, the findings are inconsistent with the view that RB arises from confusion in short-term memory (Bavelier, 1999).

Although the precise nature of the mechanism responsible for RB remains controversial, the preponderance of the evidence to

date supports a perceptual, online locus for the effect. Therefore, if a paradigm such as nonword conversion (Potter et al., 1993) can be combined with an RB paradigm, it may be possible to determine whether context-induced conversion of nonwords to words (or misreading one word as another) occurs as part of an online process or is accomplished reconstructively. A combination of these paradigms may also provide additional information concerning the locus of RB while further increasing our understanding of the nature of the perception-memory interface.

The Misreading-RB Paradigm

The experiments reported in this article use a variation of the nonword-conversion paradigm, in which prior biasing context induces participants to misread the first of two critical words (designated W1 and W2) in an RSVP sentence, as shown in the following example:

I broke a wine class in my class yesterday. (1)

Strong RB is typically observed for identical words in sentences; therefore, if participants correctly perceive *class* in the W1 position, the second occurrence of *class* should be frequently omitted from report. The trials of particular interest, however, would be those in which the biasing context induces participants to misread the first occurrence of *class* as *glass*. RB for nonidentical words is typically of smaller magnitude than it is for identical words (Bavelier, Prasada, & Segui, 1994; Kanwisher & Potter, 1990). If prior context results in rapid online selection of *glass*, then perception of *glass* rather than *class* in the W1 position should result in reduced RB for *class* in the W2 position. On the other hand, if *glass* is selected only reconstructively, RB for *class* may still occur.

Biasing context can also be used to induce participants to misread W1 such that it becomes more similar to W2, as in the following example:

I get lots of junk maid so jail is pleasant. (2)

Here, we hypothesized that if prior biasing context induces rapid, online misperception of *maid* as *mail*, there should be more RB for *jail* than if *maid* is read correctly, because *mail* and *jail* share more letters in common than *maid* and *jail*. In contrast, if the choice between *maid* and *mail* is made only after the sentence is processed, we should not observe an increase in RB when *maid* is misreported as *mail*.

The following seven experiments explored the misreading-RB paradigm and demonstrate its usefulness in investigations of the effects of sentence context. Experiments 1 and 2 show that misreading of W1 can prevent RB for W2 when an anomalous W1 is misperceived as a word less similar to W2. Experiment 1 demonstrated this phenomenon using identical critical words, whereas in Experiment 2 the critical items were orthographically similar but nonidentical words. Experiments 3 and 4 demonstrated that misreading of W1 can also cause RB for W2 when W1 is misperceived as a word more similar to W2. Experiment 5 investigated the effects of temporal parameters on misreading RB. Because standard orthographic RB generally declines with increasing numbers of items (lag) between the critical words, if misreading RB reflects the same process, it should show a similar decline. Exper-

iment 6 examined the possibility that W2, rather than W1, was misread on some of the trials in the misreading condition. Finally, Experiment 7 reexamined the issue of whether the mechanism underlying RB is predominantly perceptual or whether it reflects reconstructive memory processes. If significant RB persists when a recognition rather than a recall procedure is used, this would provide evidence that RB represents a failure of perception rather than an artifact of processing applied at retrieval.

Experiment 1

RB for the second of two identical words in RSVP sentence displays is a surprisingly strong effect (Kanwisher, 1987; Kanwisher & Potter, 1990). In Experiment 1, participants viewed RSVP sentences containing a pair of identical words but with prior biasing context designed to induce misreading of the first critical word (W1) as an orthographic neighbor; the perceived word would therefore be less orthographically similar to the second critical word (W2). We hypothesized that because RB is a function of the degree of orthographic overlap between the critical words, if misreading of W1 occurs online, it should result in a reduction in RB for W2 compared with trials in which W1 is read correctly. In contrast, we believed, if misreading occurs as part of a later reconstructive process, strong RB for W2 would still be observed.

Method

Participants. Sixty Boston University undergraduates participated to fulfill a course requirement. Six of the participants fell below a performance criterion (a minimum of 20% correct on the conditional-probability measure in the compatible-context control condition) and were replaced. All participants learned English in the home before 5 years of age. (Eleven participants learned English simultaneously with another language.)

Materials and design. Thirty pairs of words differing by only one letter (orthographic neighbors) and prior biasing sentence contexts were selected such that the biasing contexts would be compatible with (and in many cases, highly predictive of) one of the paired words and incompatible, or less compatible, with the other. For example, the context *Wait at the bus* is compatible with the target, *stop*, but incompatible with its orthographic neighbor, *slop*. Contexts and compatible targets consisted of frequently co-occurring word pairs (e.g., *alarm clock*, *swimming pool*), familiar phrases (e.g., *prevent forest fires*, *morning rush hour*) and idioms (e.g., *down in the dumps*, *once in a blue moon*). Six sentences were written for each set of critical words.² In the orthographic-neighbor condition, the context-compatible word was placed in the W1 position, with the incompatible word in the W2 position. There were always two words separating W1 and W2, as shown in the following example:

You snap your fingers for my singers to perform. (3)

The amount of RB in sentence paradigms is typically indexed by comparing the report of both critical words (joint probability) in a repeated condition, such as that shown in Example 3 above, with the report of both critical words in a nonrepeated condition, created by substituting an orthographically nonsimilar word in the W1 position. A nonrepeated, compatible-context control condition was created from the orthographic-neighbor condition by substituting a word for W1 that would be compatible with the context (although not specifically predicted by it), as the following example shows:

² The complete set of materials for all experiments reported in this article can be obtained from Alison L. Morris.

You snap your wrist for my singers to perform. (4)

A third condition, the misreading condition, was formed by inserting the context-incompatible word in the W1 position. This resulted in identical words occupying the W1 and W2 positions, as shown in the next example:

You snap your singers for my singers to perform. (5)

If participants read the first *singers* in the above sentence correctly, the second occurrence of *singers* should frequently be omitted from report. However, if participants misread the first occurrence of *singers* as *fingers*, then the selection of *fingers* in the W1 position should result in reduced RB for *singers* in the W2 position, and the amount of RB should be of similar magnitude to that seen with the orthographic-neighbor condition.

Selecting an appropriate control nonrepeated condition for the misreading condition posed a problem. When W1 in the misreading condition is misread, it would then be compatible with the sentence context. But when W1 is read correctly, the overall meaning of the sentence would be bizarre. To make the control condition as similar to the misreading condition as possible, we used two different control conditions for comparisons with misreading-condition trials. Misreading-condition trials that were misread were compared with the compatible-context control condition shown in Example 4 above. For misreading trials that were read correctly, however, an incompatible-context control condition was created, as shown in the following example:

You snap your artists for my singers to perform. (6)

Finally, to determine the expected amount of RB for an identical W2 when W1 was not likely to be misread, sentences were created with prior contexts that should not lead to misreading of W1. Examples of this no-misreading-context repeated condition and its corresponding nonrepeated control are shown as follows:

They need forty singers for my singers to perform. (7)

They need forty dollars for my singers to perform. (8)

Thus, if RB depends on the degree of orthographic similarity between the perceived W1 and W2, no matter what the source of the evidence for the identity of W1, the amount of RB for the misreading condition should be equivalent to that for the orthographic-neighbor condition on trials when W1 is misread. In contrast, on trials when W1 is not misread, the amount of RB should be equivalent to that seen for the no-misreading-context condition.

Procedure. Each participant viewed only one of the six sentences in each set. Stimuli were counterbalanced such that each participant viewed five sentences in each condition, for a total of 30 experimental trials. Participants were informed that the sentences could "contain some words that won't make sense." Each trial began with a row of asterisks appearing in the center of the computer screen. When the participant pressed the space bar, the sentence appeared one word at a time in the same location as the asterisks, with an interstimulus interval of approximately 15 ms (because of screen refresh time). Each word was centered on the display. Participants were instructed to report each sentence immediately after viewing it and to report exactly what they saw, even if it seemed strange or ungrammatical. Experimenters recorded via keypresses whether participants misread W1 and whether participants reported both critical words, W1 only, W2 only, or neither of the critical words; word omissions and substitutions were noted on a score sheet containing the test sentences. Exposure duration for the experimental trials was set individually for each participant on the basis of two sets of five practice sentences. Half of the practice sentences contained orthographically similar words for W1 and W2, and the other half consisted of orthographically nonsimilar words. Exposure duration for the first set of practice sentences was set at 105 ms per word; duration was decreased in 15-ms increments for the subsequent practice set and the experimental trials if the participant had difficulty

reporting the orthographically similar words but was still able to report nearly all words from each nonrepeated sentence. Duration was increased if the participant had difficulty recalling the nonrepeated sentences. The average exposure duration per word for the experimental trials across the 60 participants was 104 ms. The stimuli for all experiments reported here were presented on a Macintosh IIfx (Apple Computers, Cupertino, CA), with display controlled by the PsyScope experimental control software (Cohen, MacWhinney, Flatt, & Provost, 1993). The font was 48-point Chicago. Participants sat approximately 50 cm from the screen.

Results and Discussion

Misreading on a trial was defined as a report of the misreading target word in place of the displayed W1 (e.g., *fingers* in the context of *You snap your singers. . .*). As expected, prior biasing context was highly effective in inducing misreading in the misreading condition. Participants misread an average of 51% of misreading-condition trials, with W1 reported correctly on 37% of the trials. On the remaining 12% of misreading-condition trials, participants reported some other word or simply omitted W1. Report of the misreading target word in other conditions was low; the misreading target word was reported on only 4% of compatible-context control trials, 5% of incompatible-context control trials, 1% of no-misreading-context control (repeated) trials, and 0% of no-misreading-context control (nonrepeated) trials.

Before examining the effects of misreading on RB, it is important to ascertain whether the robust misreading obtained in this experiment was in fact related to the presence of the prior biasing context; word frequency could also have been a factor, because frequency was not controlled in constructing the stimulus items. The degree of contextual constraint for each of the prior contexts used in the stimulus items was estimated by determining the cloze probability for each item (McClelland, 1987). In a norming study, 20 participants viewed 64 sentence contexts in RSVP, at a rate of 105 ms per word. The contexts ended just before W1 (e.g., *You snap your*) and were followed by a question mark, which served as a cue to report the first word that came to mind. Each trial was scored as to whether the participant reported the misreading target word or some other word; alternate responses were recorded on a score sheet. The cloze probability of the stimulus items used in Experiment 1 ranged from 0 to 1.00, with a mean of .70. We hypothesized that if misreading of the stimulus items in Experiment 1 were a function of the predictability of prior sentence context, then the probability of misreading each item would be predictable from that item's cloze probability. Frequency counts for critical words were obtained from the database of Francis and Kučera (1982). The mean log frequency of the misreading target word was 3.90 (range = 0–6.06), reliably greater than the mean log frequency of W1 in the misreading condition (2.76; range = 0–5.91), $t(29) = 2.55, p < .05$. The variables cloze probability and word frequency were entered into a multiple regression analysis, with word frequency indexed by the difference between log frequencies for W1 versus the misreading target word. The raw correlation between cloze probability and probability of misreading an item was high ($r = .55$), whereas the raw correlation between the log-frequency difference and misreading was low ($r = .11$). The probability of misreading an item in the misreading condition in Experiment 1 was therefore reliably predicted by that item's cloze probability, as determined by the norming study; the

additional variance accounted for by the frequency measure was negligible.

A series of planned comparisons were carried out to test the predictions of an online model of the misreading effect. Because different results were predicted depending on whether W1 on a misreading-condition trial was misread or read correctly, all comparisons were performed on conditional probabilities; that is, report of W2 conditionalized on report of W1 (Bavelier et al., 1994) with alpha set at .01. Results are shown in Figure 1. Turning first to the no-misreading-context control conditions, comparison of the repeated (*They need forty singers for my singers to perform*) and nonrepeated (*They need forty artists for my singers to perform*) conditions revealed a difference of 39%, significant both in the subjects analysis, $F(1, 59) = 83.5$, $\omega^2 = .30$, and in the items analysis, $F(1, 29) = 49.3$, $\omega^2 = .31$. This represents a large RB effect, as is typical with identical words in RSVP sentences at Lag 2. In contrast, there was no RB for the orthographic-neighbor condition (*You snap your fingers for my singers to perform*) when compared with the compatible-context control condition (*You snap your wrist for my singers to perform*; both $F_s < 1$). The critical comparisons involved the misreading condition (*You snap your singers for my singers to perform*). Forty-seven of the 60 participants reported W1 correctly on at least one of the misreading-condition trials, and W1 was reported correctly at least once on 26 of the 30 items. On trials in which W1 was read correctly (and thus would be incompatible with the context as well as identical to W2), comparison with the incompatible-context control condition (*You snap your artists for my singers to perform*) revealed a difference of 30%, significant both by subjects, $F(1, 46) = 27.5$, $\omega^2 = .14$,

and by items, $F(1, 25) = 29.3$, $\omega^2 = .24$. This represents a medium-to-large RB effect. A separate analysis involved misreading-condition trials that were misread. Fifty-one out of 60 participants misread at least one of the misreading-condition trials; 29 of the 30 items were misread at least once. On these trials, comparison with the compatible-context control condition revealed no RB (both $F_s < 1$). Thus, when *You snap your singers for my singers to perform* was read correctly, RB was strong, similar to the magnitude of RB for display of *You need forty singers for my singers to perform*. In contrast, when *You snap your singers for my singers to perform* was misread as *You snap your fingers for my singers to perform*, RB was eliminated, just as in actual display of *You snap your fingers for my singers to perform*.

These results are readily accounted for by an online model of the misreading effect, in which prior biasing context induces misperception of W1 as the misreading target word. Because the misreading target word is less orthographically similar to W2 than the actual stimulus word, misperception of W1 as the misreading target word prevents RB for W2. In contrast, an offline model of the misreading effect, in which the misreading target word is reported as a result of a reconstructive process, has difficulty accounting for these findings. If W1 is initially perceived correctly and then changed to the misreading target word on reconstruction of the sentence, the initial correct perception of W1 should result in strong RB for W2.

This discussion, however, has assumed an online locus for RB. If RB and misreading are instead both the products of reconstructive processes, it might be possible to account for the results of Experiment 1. Because of the poor contextual fit for W1 in the

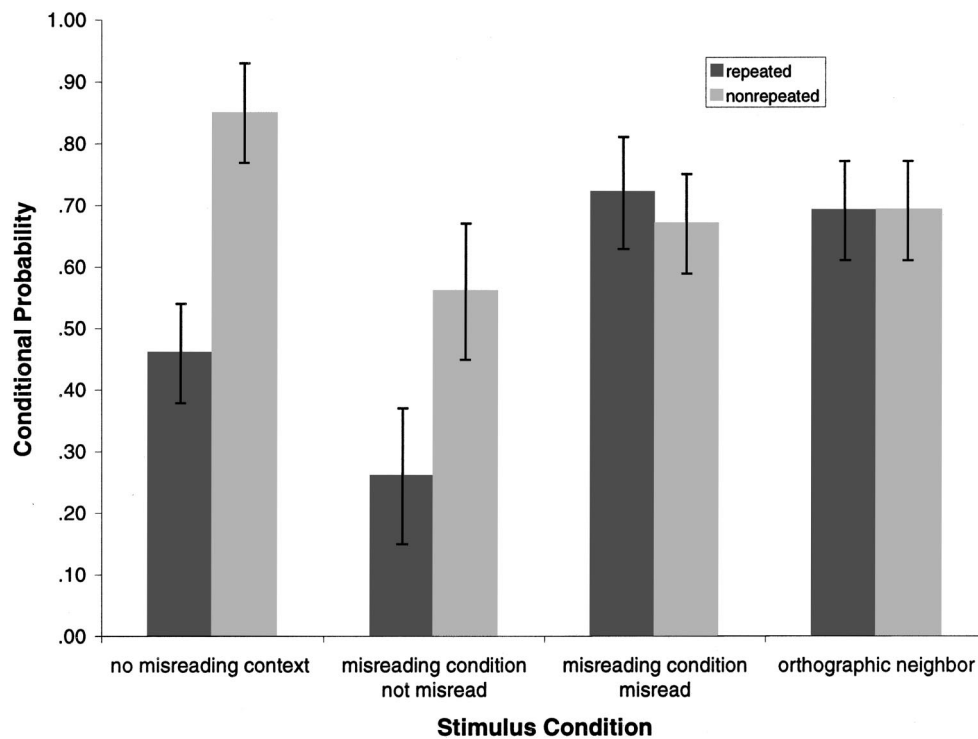


Figure 1. Conditional probabilities for stimulus conditions and nonrepeated controls in Experiment 1. Error bars represent 99% confidence intervals calculated with the method suggested by Loftus and Masson (1994).

misreading condition (e.g., *You snap your singers for my singers to perform*), a reconstructive account would predict that the RB effect would take the form of reverse RB or RB affecting W1 instead of W2 (*You snap your ? for my singers to perform*). If reverse RB occurred and context subsequently filled in *fingers* in the place of the missing *singers*, it would look as if misreading W1 had prevented RB. However, Figure 1 clearly shows a robust forward-acting RB effect, in which correct perception of W1 interfered with report of W2.

The design of Experiment 1 does have one drawback. Because W1 and W2 were identical in the misreading condition, it is difficult to be certain as to which of W1 or W2 was affected by RB and which was replaced by the misreading target word. Although these judgments were made in Experiment 1 by noting the position of the word in question with regard to local context, migrations of words in report of RSVP sentences do occur (Whittlesea, Dorken, & Podrouzek, 1995). In Experiment 2, therefore, the critical words were chosen to be orthographically similar but nonidentical.

Experiment 2

Experiment 2 again used sentences displayed in RSVP to further test the hypothesis that when a word (W1) is misread so as to be less similar to a downstream word (W2), RB is reduced relative to trials in which W1 is read correctly. Although Experiment 1 compared RB for identical words (e.g., *singers* . . . *singers*) versus orthographic neighbors (e.g., *singers* . . . *fingers*), the critical words in Experiment 2 were all nonidentical words with differing degrees of orthographic overlap (e.g., *singers* . . . *sink* vs. *fingers* . . . *sink*).

Method

Participants. Forty-eight Boston University undergraduates participated to fulfill a course requirement. Eight of the participants fell below the performance criterion of 20% correct on the conditional-probability measure in the compatible-context control condition and were replaced. All participants learned English in the home before 5 years of age. (Nine participants learned English simultaneously with another language.)

Materials and design. The design of Experiment 2 was similar to that of Experiment 1, except for the nature of the orthographic overlap between the critical words and the number of words separating them (in Experiment 2, all stimuli had only one word separating the critical words instead of two). An example of the materials used in Experiment 2 is shown in Table 1. Thirty pairs of three- to six-letter words were selected such that the first critical word contained, in the same positions, at least three of the letters in the second critical word (e.g., *singers* . . . *sink*; *maid* . . . *paid*; *free* . . . *fresh*) and prior biasing sentence contexts could be written that would be compatible with a neighbor of W1 sharing fewer letters with W2 (e.g., *fingers*; *mail*; *tree*). Contexts were similar to those used in Experiment 1. Six sentences were written for each pair of critical words. The minimal-orthographic-overlap condition was constructed by placing the context-compatible W1 following the biasing context, whereas the misreading condition was created by replacing the context-compatible W1 with the context-incompatible W1. The remaining control conditions were patterned after those from Experiment 1. Again, we hypothesized that if context influences the perception of W1, misreading of W1 to fit the context would make it less orthographically similar to W2 and thus should reduce the amount of RB for W2.

Procedure. Procedure was the same as in Experiment 1. Each participant viewed only one of the six sentences in each set. Stimuli were counterbalanced such that each participant viewed five sentences in each condition, for a total of 30 experimental trials. The average exposure

Table 1
Example of a Sentence Set from Experiment 2

Condition	Sentence
Misreading context	When she hits home <i>nuns</i> the <i>nuts</i> all cheer.
Minimal orthographic overlap	When she hits home <i>runs</i> the <i>nuts</i> all cheer.
Compatible-context control	When she hits home <i>base</i> the <i>nuts</i> all cheer.
Incompatible-context control	When she hits home <i>priests</i> the <i>nuts</i> all cheer.
No-misreading-context control (R)	When she bothers <i>nuns</i> the <i>nuts</i> all cheer.
No-misreading-context control (NR)	When she bothers <i>priests</i> the <i>nuts</i> all cheer.

Note. R = repeated; NR = nonrepeated.

duration per word for the experimental trials across the 48 participants was 99 ms.

Results and Discussion

Prior biasing context was again highly effective in inducing misreading in the misreading condition. Participants misread an average of 54% of misreading-condition trials, with W1 reported correctly on 21% of the trials; on the remaining 25% of misreading-condition trials, participants reported some other word or simply omitted W1. Report of the misreading target word also occurred on 4% of compatible-context control trials and 6% of incompatible-context control trials. No misreading was observed on either the repeated or nonrepeated versions of the no-misreading-context control condition.

The cloze probabilities of the stimulus items used in Experiment 2 (as determined by the norming study) ranged from 0 to 1.00, with a mean of .72, similar to the stimuli in Experiment 1. Unlike Experiment 1, however, the mean log frequency of the misreading target word (3.74; range = 0–6.07) was not reliably greater than the mean log frequency of W1 in the misreading condition (3.17; range = 0–5.91), $t(29) = 1.38$, $p > .15$. The variables cloze probability and word frequency were entered into a multiple regression analysis, with word frequency indexed by the difference between log frequencies for W1 versus the misreading target word. The raw correlation between cloze probability and probability of misreading an item was moderate ($r = .32$), whereas the raw correlation between the log-frequency difference and misreading was low and not in the expected direction ($r = -.10$). The frequency measure therefore did not account for any additional variance beyond that predicted by the cloze probability.

As in Experiment 1, a series of planned comparisons were carried out to test the predictions of an online model of the misreading effect. All comparisons were again performed on report of W2 conditionalized on report of W1, with alpha set at .01. Results are shown in Figure 2. Comparison of the repeated (*When she bothers nuns the nuts all cheer*) and nonrepeated (*When she bothers priests the nuts all cheer*) no-misreading-context control conditions revealed a difference of 35%, significant both in the subjects analysis, $F(1, 47) = 43.6$, $\omega^2 = .21$, and in the items analysis, $F(1, 29) = 50.4$, $\omega^2 = .23$. This represents a large RB effect. In contrast, there was no RB for the minimal-orthographic-

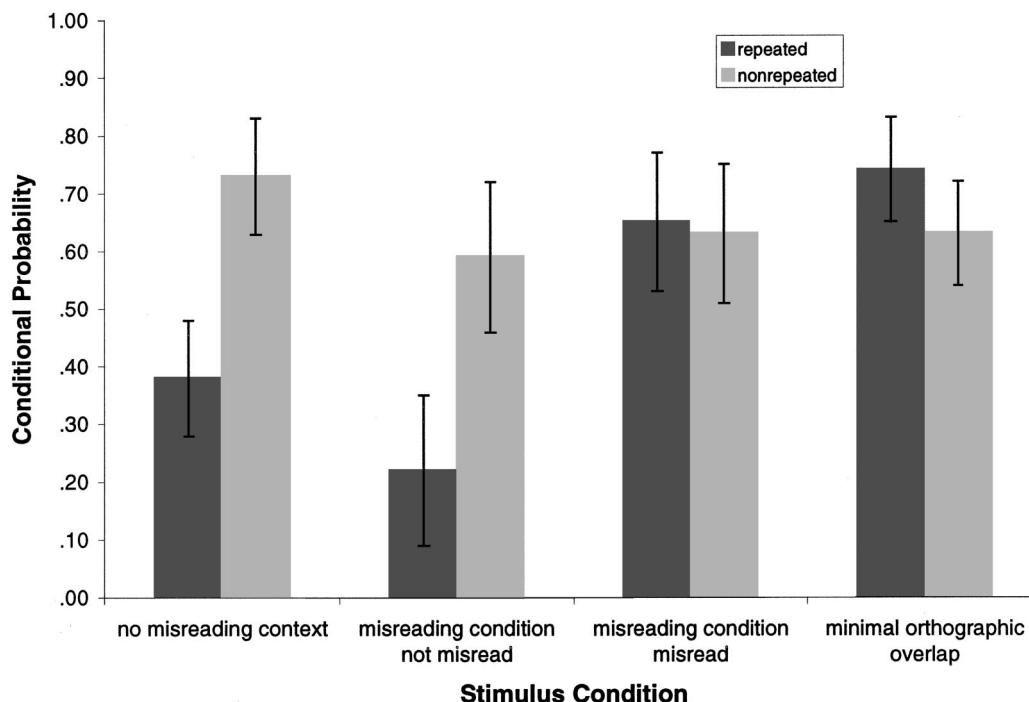


Figure 2. Conditional probabilities for stimulus conditions and nonrepeated controls in Experiment 2. Error bars represent 99% confidence intervals calculated with the method suggested by Loftus and Masson (1994).

overlap condition (*When she hits home runs the nuts all cheer*) when compared with the compatible-context control (*When she hits home base the nuts all cheer*); in fact, the subject analysis showed a marginally significant advantage in the opposite direction, $F(1, 47) = 6.2, p < .02$, whereas the item analysis was nonsignificant, $F(1, 29) = 2.1, p > .15$. Thus, overlap of just two letters, generally interior letters or nonadjacent letters, did not produce RB in this experiment.

Turning to the misreading-condition trials, all 48 of the participants misread at least one of the misreading-condition trials, and each of the 30 items was misread at least once. On trials in which W1 was misread (*When she hits home runs the nuts all cheer*), comparison with the compatible-context control condition revealed no RB (both F s < 1). Misreading-condition trials in which W1 was reported correctly were smaller in number. Thirty-four of the 48 participants reported W1 correctly on at least one of the misreading-condition trials, and W1 was reported correctly at least once on only 20 of the 30 items. On trials in which W1 was read correctly (*When she hits home nuns the nuts all cheer*), comparison with the incompatible-context control (*When she hits home priests the nuts all cheer*) revealed a difference of 30%, significant both by subjects, $F(1, 46) = 27.5, \omega^2 = .14$, and by items, $F(1, 25) = 29.3, \omega^2 = .24$. This represents a medium-to-large RB effect.

The results of Experiment 1 were replicated in Experiment 2. When participants read the misreading-condition sentences correctly (*When she hits home nuns the nuts all cheer*), they showed amounts of RB similar to that seen with the no-misreading-context control sentences (*When she bothers nuns the nuts all cheer*). However, when participants misread the misreading-condition sen-

tences (*When she hits home runs the nuts all cheer*), the resulting sentence was identical to the minimal-orthographic-overlap condition; as predicted, RB was absent in both conditions. These results are again consistent with an online model of the misreading effect; if W1 were initially perceived correctly but at a later postperceptual stage altered to fit the context, RB should still have occurred. A reconstructive form of RB in the misreading-condition sentences should take the form of reverse RB (although with nonidentical critical words); but again, Figure 2 shows a strong forward-acting effect.

One possible explanation for the results of Experiments 1 and 2 has yet to be mentioned. Many of the critical words serving as W1 in the misreading condition in Experiments 1 and 2 were of low frequency. It is possible that in many instances neither W1 nor the misreading target word was actually perceived but that for some of these trials the misreading target word was filled in later by an offline, context-based guessing process. For example, *Wait at the bus stop on slow evenings* could have been perceived as *Wait at the bus ? on slow evenings*, and on reconstruction of the sentence for report, *stop* could have been filled in by context (*Wait at the bus stop on slow evenings*). In this case, RB for W2 would not be expected to occur when W1 was "misread." To rule out this possibility, one must investigate whether misreading W1 so as to make it more orthographically similar to W2 can cause RB for W2. This question was addressed in Experiments 3 and 4.

Experiment 3

In the previous two experiments, an anomalous word in an RSVP sentence was frequently reported as an orthographically

similar word that would be compatible with prior biasing context. Although the results of Experiments 1 and 2 are most easily explained by a model in which the anomalous word is actually misperceived as its orthographic neighbor, an alternative explanation, in which the effect of sentence context occurs as part of reconstructive processes applied during recall, is also possible. When W1 fails to be encoded, RB for W2 should not occur (Luo & Caramazza, 1995). Therefore, we hypothesized that if failure to encode W1 occurred on many trials, with the misreading target word often reported in the place of W1 as a result of a postperceptual, reconstructive effect of sentence context, the results would be similar to those found in the first two experiments. To rule out this explanation, we designed Experiments 3 and 4 so that misreading W1 could cause RB for W2, rather than prevent it as in Experiments 1 and 2. In Experiment 3, the misreading target word shared more letters with W2 than did W1. Because RB between two words increases as a function of the number of shared letters between the words (Harris & Morris, 2000), we hypothesized that if prior biasing context induces misreading of W1 so as to make it more orthographically similar to W2, an increase in RB for W2 should be observed relative to trials in which W1 is read correctly.

Method

Participants. Forty Boston University undergraduates participated to fulfill a course requirement. Four of the participants fell below the performance criterion of 20% correct on the conditional-probability measure in the compatible-context control condition and were replaced. All participants learned English in the home before 5 years of age. (Seven participants learned English simultaneously with another language.)

Materials and design. An example of the materials used in Experiment 3 is shown in Table 2. Twenty pairs of words and biasing sentence contexts were selected such that the first critical word was incompatible with the prior context (e.g., *He doesn't ever read a boot*) whereas an orthographic neighbor (*book*) would be compatible with the context and, in some cases, reliably predicted by it. The second critical word (e.g., *cook*) was more orthographically similar, in terms of number and position of shared letters, to the compatible word than to the incompatible word. As in the previous experiments, to create the misreading condition, the context-incompatible word was placed in the W1 position; but, in contrast to the previous experiments, misreading the context-incompatible word (*boot*) as its context-compatible neighbor (*book*) would make it more, rather than less, orthographically similar to W2 (*cook*). This in turn would be expected to result in increased RB compared with when W1 was not misread. Four control sentences, similar to the control conditions in the previous exper-

iments, were written for each set of critical words, for a total of five sentences per set of critical words. As in the previous experiments, when W1 was misread, the appropriate nonrepeated control condition was the compatible-context control condition; however, when W1 was not misread, the incompatible-context control condition was the appropriate nonrepeated condition. Critical words were separated by zero, one, or two words in each sentence. We hypothesized that if biasing sentence context exerts its effects on perception, then there should be more RB on trials when W1 is misread than on trials when W1 is read correctly. Furthermore, the amount of RB on trials in which W1 is read correctly should be minimal and similar to the amount of RB obtained with the no-misreading-context control conditions.

Procedure. Each participant viewed only one of the five sentences in each set. Stimuli were counterbalanced such that each participant viewed four sentences in each condition, for a total of 20 experimental trials. Procedure was identical to that in the previous experiments. The average exposure duration per word for the experimental trials across the 40 participants was 106 ms.

Results and Discussion

Prior biasing context was again highly effective in inducing misreading of W1. Participants reported the misreading target word on an average of 67% of misreading-condition trials. Report of the misreading target word also occurred on 4% of compatible-context control trials, 7% of incompatible-context control trials, 2% of no-misreading-context control (repeated) trials, and 0% of no-misreading-context control (nonrepeated) trials. Cloze probabilities for the stimulus items (from the norming study described previously) ranged from 0 to 1.00, with a mean of .66. The mean log frequency of the misreading target word (4.39; range = 2.56–6.57) was significantly greater than the mean log frequency of W1 in the misreading condition (2.86; range = 0–5.69), $t(19) = 3.81$, $p < .01$. Regression analysis was not performed in Experiment 3 because of the small number of items in this experiment.

Planned comparisons were again performed on report of W2 conditionalized on report of W1, with alpha set at .01. Results are shown in Figure 3. Because there was minimal orthographic overlap in the repeated version of the no-misreading-context control condition, we predicted that there should be little-to-no RB; this prediction was confirmed. The 9% difference between the repeated (*He doesn't ever wear a boot or cook at home*) and nonrepeated (*He doesn't ever wear a shoe or cook at home*) versions of the no-misreading-context control condition was significant in the subjects analysis, $F(1, 39) = 9.5$, $p < .01$, $\omega^2 = .05$, but not in the items analysis, $F(1, 19) = 3.2$, $p > .09$, $\omega^2 = .02$. In the misreading condition, 27 out of 40 participants reported W1 correctly on at least one of the misreading-condition trials, and W1 was reported correctly at least once on 15 of the 20 items; for these trials, there was no difference between the misreading condition (*He doesn't ever read a boot or cook at home*) and the incompatible-context control condition (*He doesn't ever read a shoe or cook at home*), $F_1 < 1$; $F_2(1, 14) = 2.3$, $p > .15$. Thus, there was no RB for W2 when W1 was read correctly. In contrast, misreading W1 (*He doesn't ever read a book. . .*), thus making it more orthographically similar to W2 (*cook*), resulted in a large RB effect, both by subjects, $F(1, 39) = 44.9$, $\omega^2 = .24$, and by items, $F(1, 19) = 21.9$, $\omega^2 = .18$.

If RB is an online, perceptual process, then the above results are consistent with the view that misreading also occurred online, rather than reconstructively. Can the results of Experiment 3 also

Table 2
Example of a Sentence Set from Experiment 3

Condition	Sentence
Misreading context	He doesn't ever read a <i>boot</i> or <i>cook</i> at home.
Compatible-context control	He doesn't ever read a <i>paper</i> or <i>cook</i> at home.
Incompatible-context control	He doesn't ever read a <i>shoe</i> or <i>cook</i> at home.
No-misreading-context control (R)	He doesn't ever wear a <i>boot</i> or <i>cook</i> at home.
No-misreading-context control (NR)	He doesn't ever wear a <i>shoe</i> or <i>cook</i> at home.

Note. R = repeated; NR = nonrepeated.

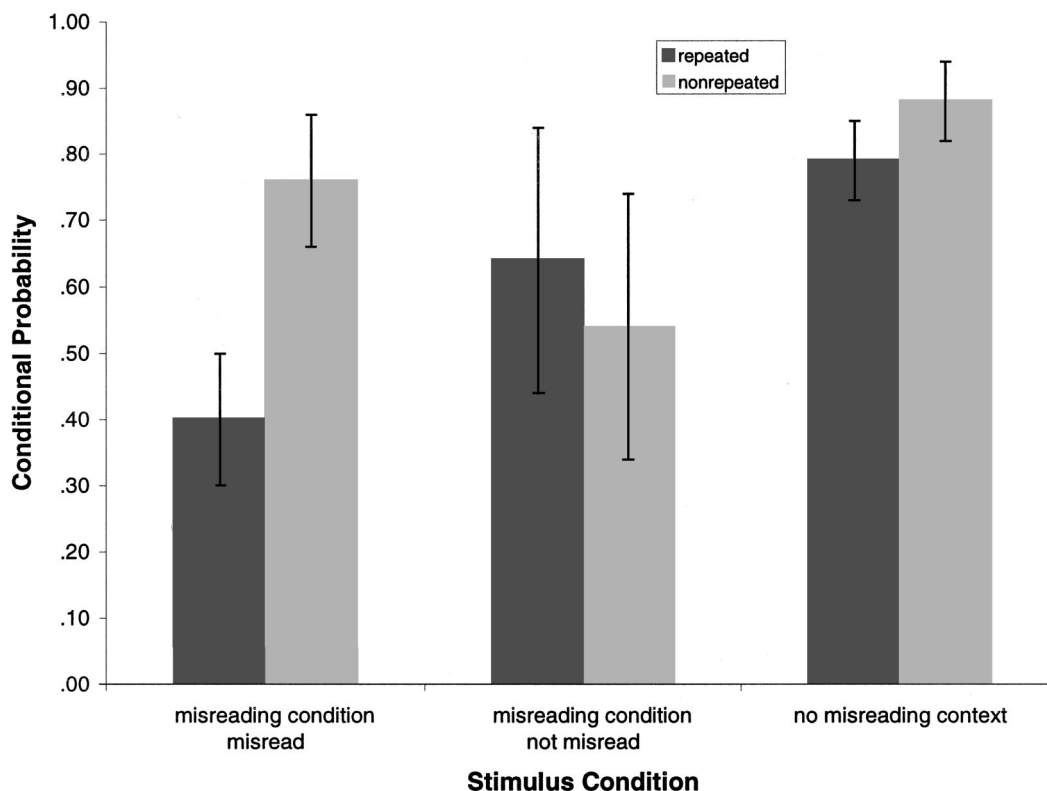


Figure 3. Conditional probabilities for stimulus conditions and nonrepeated controls in Experiment 3. Error bars represent 99% confidence intervals calculated with the method suggested by Loftus and Masson (1994).

be explained by an offline model of the misreading effect? If report of the misreading target word came about as the result of a context-based guessing or reconstruction process, there should have been no RB for trials in which W1 was “misread.” Therefore, although an offline guessing explanation of the misreading effect could explain the results of the first two experiments, it cannot explain the results of Experiment 3.

An off-line explanation of context-induced misreading could potentially account for these results, however, if RB were also assumed to be an offline process. For RB and misreading because of sentence context to both be reconstructive effects, reconstruction of W1 as the misreading target word would need to occur prior to RB affecting W2. For example, suppose that viewing of *Drink a keg of bees and jeer at the spectators* resulted in correct perception of *bees* and *jeer*, but because of the influence of the biasing context on reconstruction of the sentence for report, *bees* was changed to *beer*. Suppose further that subsequently (or perhaps simultaneously), orthographic evidence from *jeer* was misattributed to *beer*, resulting in nonreport of *jeer* (Masson, Caldwell, & Whittlesea, 2000). Such a process would mimic the results of Experiment 3. A specific prediction of this type of reconstructive account of RB was tested in Experiment 7.

Because Experiment 3 plays a potentially critical role in discriminating between online and offline models of the misreading effect, we designed Experiment 4 to replicate the findings of Experiment 3 using additional stimulus items and an additional control condition.

Experiment 4

Experiment 4 was a replication of the misreading-causes-RB effect from Experiment 3 but with over twice as many stimulus items and one additional control condition. The orthographic-overlap condition (e.g., *He doesn't ever read a book or cook at home*) was included so that RB in the misreading condition could be compared with standard orthographic RB. We hypothesized that if RB affecting W2 from a misread W1 is caused by the same process as RB affecting W2 from a displayed W1, these two conditions should show similar amounts of RB.

Method

Participants. Twenty-four Boston University undergraduates participated to fulfill a course requirement. Two of the participants fell below the performance criterion of 20% correct on the conditional-probability measure in the compatible-context control condition and were replaced. All participants learned English in the home before 5 years of age. (Eight participants learned English simultaneously with another language.)

Materials and design. An example of the materials used in Experiment 4 is shown in Table 3. Forty-eight pairs of words and biasing sentence contexts were selected such that the first critical word was incompatible with the prior context (e.g., *He doesn't ever read a boot*) whereas an orthographic neighbor (*book*) would be compatible with the context and in some cases reliably predicted by it. As in the previous experiments, the misreading condition was created by placing the context-incompatible word in the W1 position (*He doesn't ever read a boot or cook at home*). An orthographic-overlap condition was created by placing the context-

Table 3
Example of a Sentence Set from Experiment 4

Condition	Sentence
Misreading context	He doesn't ever read a <i>boot</i> or <i>cook</i> at home.
Orthographic overlap	He doesn't ever read a <i>book</i> or <i>cook</i> at home.
Compatible-context control	He doesn't ever read a <i>paper</i> or <i>cook</i> at home.
Incompatible-context control	He doesn't ever read a <i>shoe</i> or <i>cook</i> at home.
No-misreading-context control (R)	He doesn't ever wear a <i>boot</i> or <i>cook</i> at home.
No-misreading-context control (NR)	He doesn't ever wear a <i>shoe</i> or <i>cook</i> at home.

Note. R = repeated; NR = nonrepeated.

compatible word, which was also the misreading target word, in the W1 position (*He doesn't ever read a book or cook at home*). The second critical word (*cook*) was more orthographically similar, in terms of number and position of shared letters, to the compatible word (*book*) than to the incompatible word (*boot*). Eighteen stimulus items were adapted from those used in Experiment 3, whereas 30 were new. The four additional stimulus conditions were the same as those used in Experiment 3. All stimulus items had one word separating the critical words.

Procedure. Each participant viewed only one of the six sentences in each set. Stimuli were counterbalanced such that each participant viewed eight sentences in each condition, for a total of 48 experimental trials. Procedure was identical to that in the previous experiments. The average exposure duration per word for the experimental trials across the 24 participants was 104 ms.

Results and Discussion

The rate of misreading was comparable with that seen in the previous experiments; participants reported the misreading target word on an average of 56% of misreading-condition trials. Report of the misreading target word also occurred on 3% of compatible-context control trials, 3% of incompatible-context control trials, 3% of no-misreading-context control (repeated) trials, and 0% of no-misreading-context control (nonrepeated) trials. Cloze probabilities for the stimulus items, as determined by the norming study, ranged from 0 to 1.00, with a mean of .58. The mean log frequency of the misreading target word (4.07; range = 0–6.57) was significantly greater than the mean log frequency of W1 in the misreading condition (2.62; range 0–5.91), $t(47) = 5.32, p < .01$. The variables cloze probability and word frequency were entered into a multiple regression analysis, with word frequency indexed by the difference between log frequencies for W1 versus the misreading target word. The raw correlation between cloze probability of a stimulus item and probability of misreading that item was moderate ($r = .38$), whereas the raw correlation between the log-frequency difference and misreading was low ($r = .03$). The frequency measure did not account for any additional variance beyond that predicted by the cloze probability.

Planned comparisons were performed on report of W2 conditionalized on report of W1, with alpha set at .01; the results are shown in Figure 4. We believed that there should be at most a small amount of RB for the no-misreading-context condition, because the orthographic overlap between W1 and W2 was min-

imal; the 12% difference between the repeated and nonrepeated versions of the no-misreading-context control condition was marginally significant, $F_1(1, 23) = 4.7, .01 < p < .05, \omega^2 = .03$; $F_2(1, 45) = 6.3, .01 < p < .05, \omega^2 = .03$ (two items were omitted from the latter analysis because W1 was never reported correctly in the repeated condition for those items, therefore conditional probabilities could not be obtained). In contrast, the 22% difference between the orthographic-overlap (*He doesn't ever read a book or cook at home*) and the compatible-context control conditions was significant and represents a medium-to-large RB effect, $F_1(1, 23) = 26.8, \omega^2 = .20$; $F_2(1, 47) = 20.5, \omega^2 = .08$. Turning to the misreading condition, 23 of 24 participants reported W1 correctly on at least one of the misreading-condition trials, and W1 was reported correctly at least once on 30 of the 48 items; for these trials, the difference between the misreading condition and the incompatible-context control condition was marginally significant in the subjects analysis, $F(1, 22) = 4.1, p < .06, \omega^2 = .04$, but nonsignificant in the items analysis, $F(1, 29) = 1.6, p > .2, \omega^2 = .01$. In contrast, 23 of the 24 participants misread at least one of the items, thus making it more orthographically similar to W2; and, 42 of the 48 items were misread at least once. These trials, when compared with the compatible-context control condition, showed a medium-to-large RB effect, $F_1(1, 22) = 38.1, \omega^2 = .28$; $F_2(1, 41) = 29.7, \omega^2 = .14$.

The results of Experiment 3 were thus replicated in Experiment 4. When participants misread W1, making it more orthographically similar to W2, a medium-to-large RB effect for W2 was demonstrated; but when W1 was read correctly, little-to-no RB was observed. Furthermore, the size of the RB effect obtained with a misread W1 was as large as or slightly larger than that obtained in the standard orthographic-RB condition. If RB is an online effect, as a good deal of evidence suggests, then these results provide further support for the view that prior biasing sentence context affects word perception.

When the results from Experiments 1–4 are combined, some possible offline or reconstructive accounts of the effects of sentence context can be ruled out. Experiments 1 and 2 demonstrated that misreading of W1 as a word less orthographically similar to W2 prevented RB for W2, compared with trials in which W1 was read correctly (e.g., misreading *class* as *glass* prevents RB for *clay*). This rules out the possibility that W1 was initially perceived correctly but later was changed to the misreading target word when the sentence was recalled; if this had been the case, RB for W2 should still have occurred. Experiments 1 and 2 cannot rule out the possibility that “misreading” of W1 was actually the result of a postperceptual guessing process rather than an effect of context on perception, because the guessing explanation also predicts that RB for W2 will not occur when W1 is “misread.” For example, if *The morning rush sour makes soup taste bad* is perceived as *The morning rush ? makes soup taste bad*, and on reconstruction, *hour* is filled in and *The morning rush hour makes soup taste bad* is reported, there will be no RB for the W2, *soup*. However, the results of Experiments 3 and 4 contradict this guessing explanation for the misreading effect. If the misreading target word had been reported as the result of a guessing process rather than a perceptual process, no RB should have occurred for W2 in these experiments. Taken together, the results of the first four experiments are consistent with the view that misreading occurs online, during per-

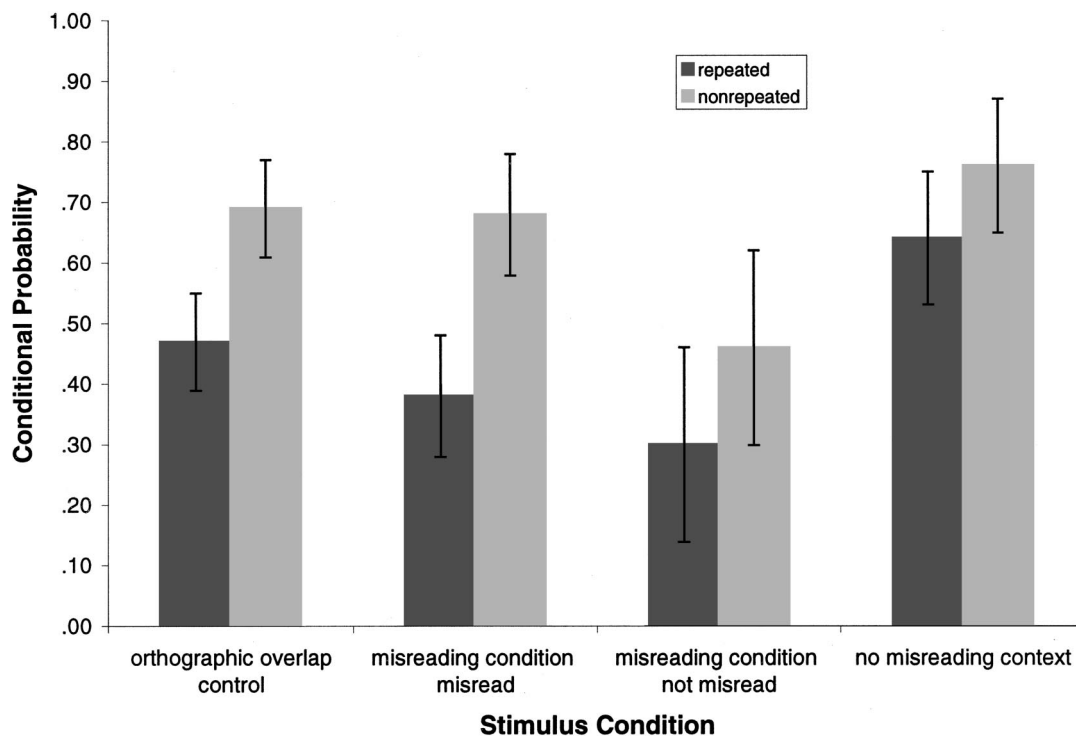


Figure 4. Conditional probabilities for stimulus conditions and nonrepeated controls in Experiment 4. Error bars represent 99% confidence intervals calculated with the method suggested by Loftus and Masson (1994).

ception of the sentence, rather than as part of reconstructive processes applied during recall.

Would the results of the first four experiments be consistent with a reconstructive account of “misreading” if RB itself were the result of a reconstructive process? The conditional-probability data demonstrate a strong, forward-acting inhibition on report of W2 when W1 was reported correctly, as opposed to the reverse RB predicted under the reconstructive view when W1 is anomalous in context (Whittlesea & Wai, 1997). In summary, the results of the first four experiments are best explained by a perceptual account of misreading and RB, in which both processes occur during online processing of the RSVP sentence. The next experiment examined the temporal parameters of the misreading-RB effect.

Experiment 5

We hypothesize that if the RB that results from misreading W1 in Experiments 3 and 4 involves the same process as standard orthographic RB, then the two phenomena should interact with temporal parameters in a similar fashion. The amount of RB between orthographically similar words has been shown to decrease with an increase in lag, or the number of items intervening between the critical words (Bavelier et al., 1994; Harris & Morris, 2001a; but see Chialant & Caramazza, 1997). If misreading RB is the result of a perceptual, online process, then it should show a similar decline with increasing lag. In Experiment 5, the lag between W1 and W2 was manipulated to examine the effects of this variable on both misreading RB and standard orthographic RB.

Method

Participants. Forty-eight Boston University undergraduates participated to fulfill a course requirement. Five of the participants fell below the performance criterion of 20% correct on the conditional-probability measure in the compatible-context control condition and were replaced. All participants learned English in the home before 5 years of age. (Five participants learned English simultaneously with another language.)

Materials and design. An example of the materials used in Experiment 5 is shown in Table 4. Materials were adapted from those used in Experiment 4. Thirty stimulus items from Experiment 4 were selected, and six sentences were created for each, a 2 (levels of lag: 1 and 3 intervening words) \times 3 (stimulus conditions: orthographic-overlap, misreading, and

Table 4
Example of a Sentence Set from Experiment 5

Condition	Sentence
Misreading context (Lag 1)	He doesn't ever read a <i>boot</i> or <i>cook</i> at home.
Misreading context (Lag 3)	He doesn't ever read a <i>boot</i> or try to <i>cook</i> at home.
Orthographic-overlap control (Lag 1)	He doesn't ever read a <i>book</i> or <i>cook</i> at home.
Orthographic-overlap control (Lag 3)	He doesn't ever read a <i>book</i> or try to <i>cook</i> at home.
Compatible-context control (Lag 1)	He doesn't ever read a <i>paper</i> or <i>cook</i> at home.
Compatible-context control (Lag 3)	He doesn't ever read a <i>paper</i> or try to <i>cook</i> at home.

compatible-context control conditions) factorial. Because the critical comparison in Experiment 5 was between standard orthographic RB (the orthographic-overlap condition) and misreading RB, the relevant trials from the misreading condition were those in which W1 was misread. The compatible-context control condition therefore served as the nonrepeated control for both the orthographic-overlap and misreading conditions, because on misread trials the reported W1 would be compatible with the sentence context.

Procedure. Each participant viewed only one of the six sentences in each set. Stimuli were counterbalanced such that each participant viewed five sentences in each condition at each lag, for a total of 30 experimental trials. The procedure was the same as in the previous experiments. The average exposure duration per word for the experimental trials across the 48 participants was 102 ms.

Results and Discussion

Participants reported the misreading target word on an average of 68% of misreading-condition trials (collapsed across lag), with W1 reported correctly on 21% of the trials. On the remaining 11% of misreading-condition trials, participants reported some other word or simply omitted W1. Misreading occurred on only 3% of compatible-context-control-condition trials. More misreading-condition trials were misread with a lag of 1 (73%) than with a lag of 3 (63%), $F_1(1, 47) = 4.1$; $F_2(1, 29) = 5.6$; both $ps < .05$. Lag did not affect misreading on the compatible-context-control-condition trials (3% at both Lag 1 and Lag 3).

Cloze probabilities for the stimulus items (on the basis of the previously described norming study) ranged from 0 to 1.00, with a mean of .66. The mean log frequency of the misreading target word (4.23; range = 1.79–6.57) was significantly greater than the mean log frequency of W1 in the misreading condition (2.74; range = 0–5.70), $t(47) = 5.32$, $p < .01$. The variables cloze probability and word frequency were entered into a multiple regression analysis, with word frequency indexed by the difference between log frequencies for W1 versus the misreading target word.

The raw correlation between cloze probability of a stimulus item and probability of misreading that item was high ($r = .55$), whereas the raw correlation between the log-frequency difference and misreading was low and in the opposite of the expected direction ($r = -.23$). The probability of misreading an item in the misreading condition in Experiment 5 was again reliably predicted by that item's cloze probability.

The probability of correct report of W2 conditionalized on report of W1 (or the misreading target word in the case of misreading-condition trials) for the orthographic-overlap, misreading, and compatible-context control conditions at both lags is shown in Figure 5. A first analysis compared the conditional probabilities for the three stimulus conditions, collapsed across lag; analysis of variance (ANOVA) revealed a main effect of condition, $F_1(2, 94) = 45.0$, $p < .01$; $F_2(2, 58) = 48.2$, $p < .01$. The mean conditional probabilities for the three conditions were further compared using Tukey's honestly significant difference. The conditional probabilities for the orthographic-overlap and misreading conditions were both significantly lower than the conditional probability for the compatible-context control condition; in turn, the conditional probability for the misreading condition was lower than the conditional probability for the orthographic-overlap condition (all $ps < .01$). The amount of RB is typically indexed by the difference between the conditional probability for a repeated condition (orthographic-overlap or misreading condition) and the conditional probability for a nonrepeated condition (compatible-context control condition). Thus, the above analysis indicated that RB was significant in both conditions, with greater RB for the misreading condition compared with the orthographic-overlap condition.

A second analysis compared the amount of RB for the misreading and orthographic-overlap conditions across the two lags by submitting the difference scores (nonrepeated–repeated) to an ANOVA with the factors stimulus condition and lag (one partic-

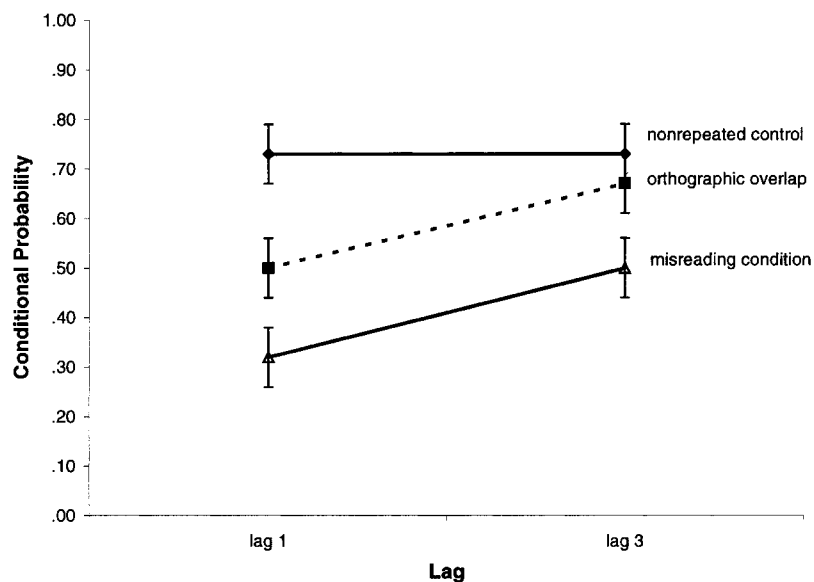


Figure 5. Conditional probabilities for stimulus conditions in Experiment 5. Error bars represent 95% confidence intervals calculated with the method suggested by Loftus and Masson (1994).

ipant was omitted from the subjects analysis because none of the misreading-condition items were misread at Lag 3). The main effect of condition was significant both by subjects, $F(1, 46) = 25.2$, and by items, $F(1, 29) = 34.7$, both $ps < .01$, again indicating more RB for the misreading condition. The main effect of lag was significant by subjects, $F(1, 46) = 15.3$, $p < .01$, and marginally significant by items, $F(1, 29) = 3.5$, $p = .07$. Most important, the Condition \times Lag interaction was not significant (both $Fs < 1$), indicating that both conditions showed a similar trend toward a decrease in RB with an increase in lag.

A third analysis compared the effect of lag on the orthographic-overlap and misreading conditions at the level of the individual items. Although some of the stimulus items showed a decrease in RB with increasing lag, others did not. We hypothesized that if misreading RB arises from the same process as standard orthographic RB, then the interaction of amount of RB with lag for each item in the misreading condition should be predictable from the orthographic-overlap condition item data. The change in RB across lags for each item in each of the two conditions was calculated by subtracting the RB score at Lag 3 from the RB score at Lag 1. Pearson product-moment correlations indicated that the change in RB with lag in the misreading condition was highly correlated with the orthographic-overlap condition item data, $r(28) = .85$, $p < .01$. These results, along with the lack of a Condition \times Lag interaction, support the idea that misreading RB and standard orthographic RB likely reflect the same underlying process.

Two findings in Experiment 5 require further exploration. There was a greater degree of misreading in the misreading condition at Lag 1 (73%) compared with Lag 3 (63%). That is, *dandy* was misread as *candy* more frequently following display of *All those chocolate dandy bars can make you fat* than following *All those chocolate dandy bars you ate can make you fat*. Although this was a small effect, it is not clear why this should have occurred, if misreading is a function of prior context interacting with stimulus information for W1. Also, RB for the misreading condition was reliably greater than RB for the orthographic-overlap condition. It is possible that both of these differences could be explained by the occasional misreading of W2 rather than W1. When the misreading target word is reported, but neither W1 nor W2 is reported, it is not always possible to know whether it was W1 or W2 that was misread. For example, if *He doesn't ever read a boot or cook at home* is reported as *He doesn't ever read a something book at home*, it may in fact have been the W2 *cook* that was actually misread as *book* (rather than the W1 *boot*), or there may have been some merging of *boot* and *cook* to create the percept *book*, as occurs in studies of letter migration with sequentially presented words (Van der Velde, 1992). W2 may be slightly more vulnerable to such misreading on Lag 1 trials compared with Lag 3 trials, because of the greater proximity of W2 to the biasing context as well as the greater proximity of the critical words (compare *He doesn't ever read a boot or cook at home* with *He doesn't ever read a boot or try to cook at home*). The important point is that whenever W2 is misread, it is by definition not reported correctly. Therefore, trials in which W2 was misread as the misreading target word (e.g., *cook* misread as *book*) would necessarily decrease the probability of report of W2 conditionalized on report of the misreading target word, compared with the standard orthographic RB situation. Occasional misreading of W2 coupled with nonreport of W1 could therefore explain the apparent increase in RB obtained

in the misreading condition compared with the standard RB condition, as well as the small increase in misreading at Lag 1 compared with Lag 3. This possibility was investigated in Experiment 6.

Experiment 6

The possibility that W2 rather than W1 can sometimes be misread as the misreading target word was examined in Experiment 6. In Experiments 3, 4, and 5, when a sentence such as *Drink a keg of bees and jeer at the spectators* was displayed, either the W1 *bees* or the W2 *jeer* could have been misread as *beer*. If participants omit W1 from their report, and W2 is sometimes misread as the misreading target word, it would appear to be a case of misreading RB. For example, if participants reported *Drink a keg of beer at the spectators*, it would not be clear whether it was actually W1 (*bees*) or W2 (*jeer*) that had been misread, or whether *bees* and *jeer* combined to produce the percept *beer*. Furthermore, because all trials in which the misreading target word was reported were included in calculations of the conditional probability for W2, misreading of W2 would tend to inflate the estimate of RB in the misreading condition. In Experiment 6, report of the misreading target word *beer* following display of *Drink a keg of bees and jeer at the spectators* was compared with report of *beer* following display of *Drink a keg of bees and shout at the spectators*. We hypothesized that if report of the misreading target word can be increased by the presence of a W2 that is orthographically similar to the misreading target word, then there should be greater report of the misreading target word *beer* when *jeer* is in the display compared with when *shout* is substituted. We also hypothesized that if W2 contributes to misreading, there should be greater report of the misreading target word for display of *Drink a keg of flies and jeer at the spectators* compared with *Drink a keg of flies and shout at the spectators*.

Method

Participants. Twenty-four Boston University undergraduates participated to fulfill a course requirement. All participants learned English in the home before 5 years of age (Four participants learned English simultaneously with another language.)

Materials and design. Stimulus items were adapted from those used in Experiment 3, with four sentences written for each item. The similar-W1-similar-W2 condition was identical to the misreading condition from Experiment 3 (e.g., *Drink a keg of bees and jeer at the spectators*). For the similar-W1-nonsimilar-W2 condition, W2 was replaced by a semantically similar but orthographically different word (e.g., *Drink a keg of bees and shout at the spectators*). The remaining control conditions consisted of the nonsimilar-W1-similar-W2 condition, which was identical to the incompatible-context control condition from Experiment 3 (e.g., *Drink a keg of flies and jeer at the spectators*) and the nonsimilar-W1-nonsimilar-W2 condition (e.g., *Drink a keg of flies and shout at the spectators*). If W2 was sometimes misread, there should have been greater report of the misreading target word in the two similar-W2 conditions compared with the two nonsimilar-W2 conditions.

Procedure. Each participant viewed only one of the four sentences in each set. Stimuli were counterbalanced such that each participant viewed five sentences in each condition at each lag, for a total of 20 experimental trials. Procedure was the same as in the previous experiments. The average exposure duration per word for the experimental trials across the 20 participants was 109 ms.

Results and Discussion

The percentage of trials on which the misreading target word was reported for the four stimulus conditions is shown in Table 5. Report of the misreading target word was greatest when W1 was orthographically similar to the misreading target word, but the presence of an orthographically similar W2 also had a significant effect. An ANOVA with the factors W1 similarity and W2 similarity showed a significant main effect of W1 similarity, $F_1(1, 23) = 75.2$; $F_2(1, 19) = 134.5$; both $ps < .01$; and a significant main effect of W2 similarity, $F_1(1, 23) = 12.6$; $F_2(1, 19) = 15.1$; both $ps < .01$; but no interaction, $F_1(1, 23) = 1.5$; $F_2(1, 19) = 1.4$; both $ps > .2$. Thus, the orthographic similarity of W2, as well as the orthographic similarity of W1, contributed to report of the misreading target word in Experiment 6.

The results of Experiment 6 may explain why RB in the misreading condition was larger than RB in the standard orthographic-overlap condition in Experiment 5. Consider the following example. Suppose an RB effect of 23% was obtained for the orthographic-overlap condition (as in the Lag 1 condition of Experiment 5) and that the true amount of RB in the misreading condition was equal to that in the orthographic-overlap condition. Further, suppose that 16% of the reports of the misreading target word in the misreading condition were actually due to misreading of W2 rather than W1 (which was the case in Experiment 6, as can be seen when the similar-W1-similar-W2 and similar-W1-nonsimilar-W2 conditions are compared). These trials would also contribute to RB in the misreading condition, but for this portion of the RB, the effect would be 100%, because the misreading target word and W2 would not be reported on the same trial. This would give us a total RB score of $[(.84)(.23) + (.16)(1.00)](100) = 35\%$. In this example, therefore, the obtained RB score overestimates the true level of RB in the misreading condition by 12%. If the results of Experiment 6 are representative of the level of misreading of W2 in Experiments 4 and 5, the greater degree of RB found in the misreading condition compared with the orthographic-overlap condition (8% in Experiment 4 and 18% in Experiment 5) is readily explained by the inflation of the RB score in the misreading condition caused by occasional misreading of W2 instead of W1. The slight increase in misreading at Lag 1 compared with Lag 3 in Experiment 5 could also be explained by occasional misreading of W2, because W2 may be more vulnerable to misreading at Lag 1 because of its greater proximity to the biasing context (and a greater probability of merging with W1). It is important to note, however, that this

difference is small and does not account for the decrease in RB with increasing lag for the misreading condition.

Could misreading of W2 have contributed to report of the misreading target word in Experiments 1 and 2? These experiments would be less vulnerable to misreading of W2, because the misreading target word and W2 were less similar than in Experiments 3, 4, and 5 (compare *pool* and *cook* with *book* and *cook*). In addition, if W2 were misread frequently, there should have been a significant RB observed on misread trials. Instead, there was no RB for W2 when the misreading target word was reported. In summary, although occasional misreading of W2 (or a merging of W1 and W2) may have contributed to the overall level of misreading in the misreading conditions in Experiments 3, 4, and 5, the major factor producing misreading was the effect of the biasing context on perception of W1. Also, even though misreading of W2 tended to inflate the estimate of RB for misread trials in these experiments, misreading of W2 cannot account for the entire misreading-causes-RB effect.

Experiment 7

The results of the previous experiments demonstrate that RB for the second of two identical or orthographically similar words in an RSVP sentence depends on the perceived identity, rather than the actual identity, of the first critical word. Furthermore, the results suggest that biasing sentence context presented prior to the first critical word can either cause or prevent RB for the second critical word by altering the perception of the first critical word. These conclusions, however, are based on the assumption that RB itself is a perceptual effect, rather than an artifact of reconstructive memory processes applied at the time of recall. Experiment 7 provides a test of this crucial assumption.

Although there is a fair amount of evidence indicating an online locus for RB, the issue of whether RB reflects perceptual or memory processes is still the subject of debate. In the most recent challenge to perceptual accounts of RB, Masson et al. (2000) investigated the role of reconstructive memory processes in recall of RSVP lists containing orthographically similar but nonidentical words. According to Masson et al., words presented in RSVP lists undergo limited processing, so that orthographic information is incomplete, especially for words appearing later in the list. This means that on recall of lists containing orthographically similar words, orthographic information from the later word may be attributed to the earlier word, resulting in reduced report of the later word. RB is therefore a result of the way the word list is reconstructed, rather than a result of a failure of perception. In Masson et al.'s Experiment 5, participants viewed RSVP word lists (e.g., *toad went cuff west trim*), half of which were followed by a probe word (e.g., *EAST*). The two critical words (C1 and C2) always occupied the second and fourth list positions, respectively. On probe trials, participants were instructed to report a word from the list that was semantically related to the probe (always C2). On the nonprobe trials, participants reported the entire list. Both tasks involved explicit recall of words from the list, but the probe task was designed to promote reconstruction of the list in such a way as to favor report of C2. On the full-report trials, C2 was reported less often in the similar (repeated) condition than in the different (nonrepeated) condition, a difference of 14%, whereas the reverse occurred for C1, in which report in the similar condition was 13%

Table 5
Percentage of Trials on Which the Misreading Target Word Was Reported in Experiment 6

W1 similarity	W2 similarity			
	Similar		Nonsimilar	
	<i>M</i>	<i>SE</i>	<i>M</i>	<i>SE</i>
Similar	54	5	38	5
Nonsimilar	10	3	3	1

Note. W1 = first critical word; W2 = second critical word.

higher than in the different condition. However, no difference was noted in cued report of C2, even when a correction for guessing was applied. According to Masson et al., because a significant RB effect for C2 was demonstrated on the nonprobe trials, the finding that C2 was equally available for report in the similar and different conditions when a cued-recall task was used suggests that RB does not reflect a failure to encode C2. Rather, it is the type of reconstructive processes used at recall that influences whether C2 is likely to be reported.

To test the hypothesis that the RB effects observed in the experiments reported in this article were the result of reconstructive memory processes, a variant of Masson et al.'s (2000) probe task was used in Experiment 7 but in an even stronger form. Rather than presenting a probe word that was semantically related to W2, the probe word was identical to W2, and the participant's task was simply to decide whether that word had appeared in the sentence just presented. When the probe was presented, participants could decide whether it appeared in the sentence by making a judgment based on a feeling of familiarity; this basis for recognition judgments has been previously well established (e.g., Mandler, 1980) and is an integral part of reconstructive accounts (Whittlesea & Podrouzek, 1995). If RB is not a perceptual effect, and W1 and W2 are fully encoded as separate events, re-presentation of W2 as a probe should result in fluent processing of the probe, in turn triggering a feeling of familiarity (Whittlesea & Podrouzek, 1995). Thus, according to the reconstructive account, RB for W2 under these circumstances should be eliminated or at least substantially reduced. If, however, a robust RB effect for W2 persists in the form of an accuracy advantage for nonrepeated control conditions over conditions containing orthographically similar critical words, this finding would support a perceptual locus for RB.

Method

Participants. Thirty-two Boston University undergraduates participated to fulfill a course requirement. Five of these participants fell below chance level on a performance criterion (50%, or 3 of 6 hits) for the compatible-context control sentences and were replaced. All participants were monolingual English speakers.

Materials and design. An example of the materials used in Experiment 7 is shown in Table 6. The orthographic-overlap, misreading, compatible-context control, and incompatible-context control conditions from Experiment 4 were adapted for this experiment. On each of the 48 experimental trials, the RSVP sentence was followed by a probe word; the participants were instructed to respond *yes* (by pressing the appropriate button) if the probe word had been in the sentence or *no* otherwise. Two versions of each stimulus condition for each sentence were written: a *yes* version and a *no* version. For the *yes* version, the probe word was identical to W2; for the *no* version, the probe word was identical to that in the corresponding *yes* version, but W2 was changed to an orthographically nonsimilar word that fit the context. Thus, for half of the experimental trials, the participant's correct response to the probe was *yes*; on the other half, the correct response was *no*. In addition to the experimental trials, participants viewed 48 filler trials. On these trials, the probe word corresponded to the first critical word from the sentence (W1); as in the experimental trials, the tested word, in this case W1, was either identical to the probe word (*yes* version) or a different word was substituted in the sentence (*no* version). These trials were included to encourage participants to attend to the entire sentence rather than focusing only on the area of W2. Six of the 24 filler trials in the *yes* version contained orthographically similar words; these were designated as repeated filler trials. Nine filler trials in the *yes* version contained nonsimilar but context-compatible words

Table 6
Example of a Sentence Set From Experiment 7

Condition	Sentence	Probe
Misreading condition (Y)	He doesn't ever read a <i>boot</i> or <i>cook</i> at home.	cook
Misreading condition (N)	He doesn't ever read a <i>boot</i> or <i>sleep</i> at home.	cook
Orthographic-overlap control (Y)	He doesn't ever read a <i>book</i> or <i>cook</i> at home.	cook
Orthographic-overlap control (N)	He doesn't ever read a <i>book</i> or <i>sleep</i> at home.	cook
Compatible-context control (Y)	He doesn't ever read a <i>paper</i> or <i>cook</i> at home.	cook
Compatible-context control (N)	He doesn't ever read a <i>paper</i> or <i>sleep</i> at home.	cook
Incompatible-context control (Y)	He doesn't ever read a <i>shoe</i> or <i>cook</i> at home.	cook
Incompatible-context control (N)	He doesn't ever read a <i>shoe</i> or <i>sleep</i> at home.	cook

Note. Y = "yes" version; N = "no" version.

and were designated nonrepeated filler trials. Because the probe word for the filler trials matched W1, the amount of RB affecting W1 could also be estimated. Such RB might be observed if participants are biased against responding *yes* to a probe word when they have viewed a sentence containing a word orthographically similar to the probe. For the remaining 9 filler trials in the *yes* version, W1 was chosen so as to be incompatible in the sentence context. Six of the *no* version filler trials also contained orthographically similar words, but the probe word did not match either word. The remaining *no* version fillers contained a W1 that was either compatible (13 of 24) or incompatible (5 of 24) with the sentence context. The same filler trials were used in all eight versions of the experiment.

Procedure. Each participant viewed only one of the eight sentences in each set, plus 48 filler trials, for a total of 96 trials. Viewing conditions were similar to those in the previous experiments, with the following exceptions. The initial warning signal (a row of asterisks) was displayed for 1,000 ms, followed immediately by the sentence, one word at a time in the center of the computer screen. After an interval of 500 ms, the probe warning signal, *in sentence?*, was displayed for 1,000 ms (in a blue font that was smaller than the font used for the sentence words) followed immediately by the probe word (in the same font and color as the sentence). The probe word remained on the screen until the participant pressed either of two buttons on a response box, one labeled *yes*, the other labeled *no*. Once the response was made, the probe word was erased, and a new trial began after an interval of 1,000 ms. The average exposure duration per word across the 32 participants was 98 ms.

Results and Discussion

The probability of a false alarm was low (.04–.05), and false alarms were evenly distributed across conditions; all further analyses were therefore based on the probability of a hit (correct *yes* response) in each condition. The probability of a hit also provides the most comparable measure to the probability of reporting W2 in a serial recall task. Figure 6 shows the probability of a hit for the compatible-context control, orthographic-overlap, incompatible-context control, and misreading conditions in Experiment 7. The probability of correct report of W2 in the same conditions from Experiment 4 is also included for comparison purposes. Experiments 4 and 7 used the same stimulus items for the four main conditions, and the mean stimulus durations were similar (104

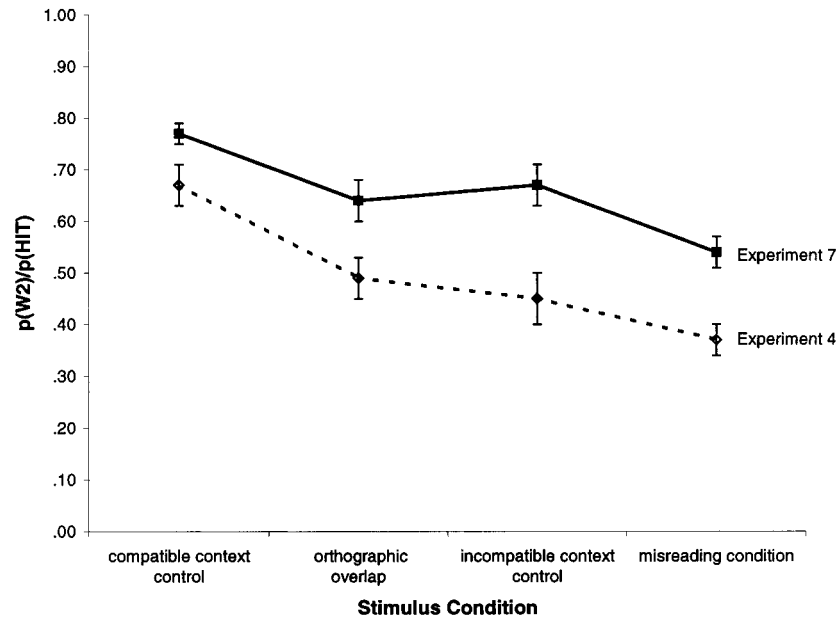


Figure 6. Probability of a hit (Experiment 7) and probability of correct recall of W2 (Experiment 4) for the four stimulus conditions of Experiment 7. Error bars represent ± 1 standard error.

and 98 ms, respectively). As can be seen in Figure 6, the probability of correctly recognizing W2 as having been displayed in the just-viewed sentence (Experiment 7) was higher than the probability of correctly reporting W2 in serial recall (Experiment 4) by a more-or-less constant amount across the four stimulus conditions. This was confirmed by a 2×4 ANOVA with stimulus condition as the within-subject factor and Experiment–task as the between-subjects factor. The main effect of condition was significant, $F_1(3, 162) = 25.9$; $F_2(3, 141) = 16.7$; both $ps < .01$; as was the main effect of Experiment–task, $F_1(1, 54) = 20.3$; $F_2(1, 47) = 59.1$; both $ps < .01$; but the interaction was not significant, $F_1(3, 162) = 1.2$; $F_2(3, 141) = 1.0$; both $ps > .3$. Thus, although scores for all the conditions were higher with the recognition than with the recall measure, the pattern of performance across conditions was not significantly different across the two experiments.

A second analysis specifically examined whether the magnitude of RB was different in Experiment 4 compared with Experiment 7 by submitting the orthographic-overlap and compatible-context control conditions to an ANOVA with the factors stimulus condition and Experiment–task. Again, the main effect of condition was significant, $F_1(1, 54) = 31.3$; $F_2(1, 47) = 18.1$; both $ps < .01$; as was the main effect of Experiment–task, $F_1(1, 54) = 8.9$; $F_2(1, 47) = 14.3$, both $ps < .01$; but there was no hint of an interaction (both $F_s < 1$). Thus, not only was a significant RB effect obtained using the recognition procedure but the amount of RB was not significantly diminished in comparison with the amount of RB obtained using the more standard recall measure. The probability of a hit on both the repeated and nonrepeated filler trials, which tested recognition of W1, was high (.95 and .97, respectively). The effect of repeatedness on the filler trials was nonsignificant, $F(1, 31) = 1.4$, $p > .2$, indicating no RB affecting W1 for these stimuli.

One note of caution is in order regarding cross-experiment comparisons. Because different tasks (recall vs. recognition) were

used in the two experiments, it was difficult to equate the performance criteria used to exclude participants who may have been run at too brief an exposure duration and thus had undue difficulty in reading the control sentences. The performance criteria have the disadvantage of removing some degree of randomness from participant selection. If, for example, the performance criterion used in Experiment 7 was more stringent than that used in Experiment 4, then the improved overall performance noted in Experiment 7 could have been due to subject factors alone. In other words, the participants included in Experiment 7 (after those falling below the performance criterion were excluded) may simply have been superior at reading and recalling rapidly displayed sentences, compared with the participants in Experiment 4. However, if this were the case, the participants in Experiment 7 should have also demonstrated significantly less RB than the participants in Experiment 4, assuming a reconstructive locus for the RB effect. According to the reconstructive view, RB results from a combination of fragmented orthographic information and poor integration with local context (Masson et al., 2000), both of which should be less of a problem for participants with better overall ability to read and recall RSVP sentences. Instead, the results of Experiment 7 suggest that even significantly more accurate recall (whether as the result of presentation of a recognition probe or from subject factors) does not appreciably reduce RB.

The findings of Experiment 7 provide strong support for the view that RB represents a failure of conscious perception rather than an artifact of reconstructive memory. If W2 had been perceived as well in the orthographic-overlap condition as in its nonrepeated control (the compatible-context control condition), then re-presentation of W2 as the probe word should have triggered equal levels of recognition of W2 in the two conditions and eliminated the RB effect. Instead, a significant RB effect was

observed, similar in magnitude to that demonstrated in Experiment 4 using serial recall.

The results of Experiment 7 run counter to those obtained by Masson et al. (2000, Experiment 5), in which a probe that was semantically related to C2 triggered similar levels of cued recall of C2 in the *similar* and *different* conditions. The discrepancy might be explained by the differing levels of baseline performance in the two experiments. The hallmark of RB is the reporting of two words as separate events in the nonrepeated condition but only reporting one in the repeated condition. Thus, the best index of RB is either a *both* score (joint report of W1 and W2) or a conditional-probability measure (report of W2 given report of W1). When report of W1 is high (as in Experiment 7 of this article) the simple probability of reporting W2 will resemble the joint probability, but when report of W1 is low there is no guarantee that a decrease in report of W2 from a nonrepeated to a repeated condition represents a decrease in the ability to report the joint occurrence of two events. In Masson et al. (Experiment 5) baseline performance on the serial recall task was low (C1 was reported 42% of the time, and C2 was reported 32% of the time in the baseline condition), and neither joint probabilities nor conditional probabilities were reported. Indeed, it is not clear that RB actually occurred in that experiment; the apparent trade-off between report of C1 and C2 in the *similar* and *different* conditions may represent some phenomenon other than RB, as such a trade-off can occur without any change in the joint report of the two critical words.

General Discussion

The experiments presented in this article introduce a novel paradigm, known as *misreading RB*. In this paradigm, a variant of the nonword-conversion task (Potter et al., 1993) is combined with a repetition-blindness paradigm to determine whether the effects of prior biasing sentence context can be localized to online, perceptual processes or whether the effects arise only upon reconstruction of the sentence for recall. The biasing sentence context frequently induces misreading of a first critical word (W1); it is then the perceived word, rather than the displayed word, that determines whether RB affects the downstream word (W2). In Experiment 1, leading context induced participants to misread the first of two identical words as an orthographic neighbor, reducing the perceived similarity between the two critical words; this in turn prevented RB for the second critical word. Experiment 2 replicated these findings using nonidentical critical words. The finding that misreading W1 prevented RB for W2 suggests that the misreading occurred online, rather than as part of a postperceptual problem-solving process. Experiments 3 and 4 demonstrated the opposite phenomenon; that is, that misreading W1 so as to make it more similar to W2 can cause RB for W2. The results of Experiments 3 and 4 make a guessing explanation for the findings of Experiments 1 and 2 unlikely and provide further support for the view that misreading of W1 occurred during perception of the sentence rather than during recall.

Experiment 5 examined the interaction of RB with lag for both misreading RB and standard orthographic RB; a trend toward decreasing RB with increasing lag was found for both the orthographic-overlap condition and for the misreading-condition trials in which the misreading target word was reported in place of W1. In addition, the amount of change in RB with increasing lag

for each stimulus item in the misreading condition was correlated with the comparable data in the orthographic-overlap condition; that is, those items showing a large decrease in RB from Lag 1 to Lag 3 in the orthographic-overlap condition also showed a large decrease in the misreading condition, whereas those showing no decrease with lag in the orthographic-overlap condition showed no decrease in the misreading condition. Thus, misreading RB interacted with lag in the same manner as standard orthographic RB. Experiment 6 investigated two unexplained findings from Experiment 5: The amount of misreading was greater for Lag 1 trials compared with Lag 3 trials, and the size of the RB effect was reliably greater for the misreading condition compared with the orthographic-overlap condition. Experiment 6 demonstrated that both of these findings can be explained by the occasional misreading of W2, rather than of W1 (or a merging of the two critical words), in the misreading condition.

Finally, Experiment 7 examined the possibility that standard orthographic RB, as well as misreading RB, results from reconstructive processes applied during recall, rather than from perceptual processes. In Experiment 7, a recognition probe was displayed following each RSVP sentence, and the participant's task was simply to indicate whether the probe word had been in the just-viewed sentence. On critical trials, the probe was identical to W2. Re-presentation of W2 should serve as a powerful retrieval cue, if W2 had been consciously perceived. Because the stimulus items were identical to those used in Experiment 4 (except for the addition of the probe word) it was also possible to compare the recognition procedure used in Experiment 7 with the serial recall procedure used in Experiment 4. Although the overall level of report of W2 was higher across all conditions with the recognition compared with the recall procedure, there was no significant reduction in RB between the two experiments. The failure of the probe procedure to eliminate or significantly reduce RB rules out an explanation based on reconstructive memory processes and provides further support for the perceptual nature of RB.

Furthermore, a recent experiment conducted in our laboratory (Morris & Harris, 2001) reinforces the idea that RB represents a failure of conscious perception. Participants monitored RSVP sentences for a specified target word and pressed a button as soon as they detected the target word in the sentence. Accuracy was near ceiling for nonrepeated trials; however, when the target word was the second of two orthographically similar words, participants frequently failed to press the button. Thus, a large RB effect was found in a simple word-detection task, in which no reconstruction of the sentence was required.

Implications for Models of Word Recognition

The experiments presented in this article constitute a preliminary exploration of the misreading-RB effect. The finding that prior biasing context in RSVP sentences can induce misreading of a word, which can then either cause or prevent RB for a downstream word, would be consistent with either interactive (e.g., McClelland, 1987) or modular-interactive (Potter et al., 1993) models of visual word recognition. In the case of interactive models, context serves as an additional source of lexical activation in its own right, supplementing that from the stimulus; whereas in modular-interactive models, context merely selects from a set of weighted candidates offered up by the autonomous word-

recognition machinery; such selection is said to be postlexical. The crucial distinction is between predictive and selective forms of context (McClelland, 1987; Swinney & Hakes, 1976). According to McClelland (1987), context needs to be highly constraining to have interactive effects; that is, it needs to be predictive, rather than merely selective. Although the experiments reported in this article were not designed to discriminate between predictive and selective effects, there are some reasons to suspect that these results reflect the workings of an interactive rather than a modular-interactive process. Experiment 4 used a large number of stimulus items, with a wide range of predictiveness as indexed by cloze probability. Six of the 48 contexts actually had a cloze probability of zero, and thus might be classified as selective rather than predictive. It is interesting to note that although these items were still misread at a fairly high rate ($M = 46\%$) they did not show significant RB in the misreading condition, although the same items did show strong RB (30%) in the orthographic-overlap condition. In contrast, items with some degree of predictability, as demonstrated by a cloze probability above zero, showed greater RB in the misreading condition (39%) compared with the standard orthographic-overlap condition (20%). Although the number of items is small, this suggests the possibility that the misreading produced by context that is merely selective, and thus has postlexical rather than prelexical effects, does not result in RB for a W2 that is orthographically similar to the misreading target word. Further investigation of this question should take the form of within-item comparisons, contrasting predictive with selective context (e.g., comparing *I can't see in the dart so bark or yell* with *I can't move in the dart so bark or yell*). Directly comparing these two types of context in their ability to cause RB for *bark* on misread trials may provide further clues as to the mechanisms involved in predictive versus selective context.

Could our findings be explained by a modular model of word recognition? The effect of context on misreading a critical word, at least for most of the stimulus items used in these experiments, appears to be perceptual rather than reconstructive. To account for these results in terms of a modular model, one would have to attribute the misreading-RB effect to an intralexical priming mechanism rather than to top-down priming from the sentence context. Many of the sentence contexts used in these experiments contained words strongly associated to the misreading target word (e.g., *birthday. . . cake; ice. . . cream; alarm. . . clock*). Lexical priming likely played a role in inducing misreading in these stimuli. Other stimuli, however, contained no single strong priming word, but rather the misreading target word was a completion of an idiom (*my goose was cooked; read between the lines; once in a blue moon*) or a common phrase (*snap your fingers; in a bad mood; take a deep breath*). Therefore, if the misreading-RB effects are to be attributed to intralexical mechanisms, common phrases as well as idioms would need to be stored in such a way that constituent word representations could activate, and be activated by, entrenched phrase- or idiom-level representations (Harris, 1998). Stretching the concept of the lexicon in such a way as to permit this type of interlevel interaction risks blurring the distinction between autonomous and interactive models of word recognition. Nevertheless, if one wishes to claim that the misreading effects found in these experiments are intralexical, then a systematic study of different types of phrase-level contexts using the misreading-RB paradigm

may prove useful in formulating and testing models of lexical organization.

Using Sentence Context to Investigate the Mechanisms Underlying RB

The experiments described in this article demonstrate how the misreading-RB paradigm may be used as a tool to increase our understanding of the mechanisms underlying sentence-context effects. But manipulations of sentence context in RB paradigms might also further our understanding of the phenomenon of RB. Although sentence context has been manipulated in some RB experiments (e.g., Whittlesea & Wai, 1997, Experiment 3), these experiments have focused on the context surrounding W2. The experiments in this article have uncovered a potentially interesting finding regarding the effect of constraining context on W1. Previous experiments investigating RB for orthographic neighbors in sentences have shown that such RB is generally robust at Lag 2; in experiments run at similar exposure durations to Experiment 1, Harris and Morris (2001a, Experiment 1) found a 33% RB effect for orthographic neighbors at Lag 2, whereas Chialant and Caramazza (1997, Experiment 1) found a 28% RB effect. In contrast, there was no RB for orthographic neighbors at Lag 2 in Experiment 1 of this article. The stimulus sentences in the experiment by Harris and Morris (2001a) did not use particularly biasing context prior to W1 (e.g., *Chris was stiff from that stuff he moved yesterday*) and neither did those used in the experiment by Chialant and Caramazza (e.g., *I saw two people leap over a leaf beside me*). This discrepancy in otherwise comparable experiments (stimulus durations and other display characteristics were similar) suggests the possibility that predictability of W1 reduces RB for W2. Some eye-movement studies (e.g., Ehrlich & Rayner, 1981) have demonstrated that words predictable from context are less likely to be fixated than nonpredictable words. In addition, when the predictable word is misspelled, participants are less likely to notice the misspelling. These findings indicate that less visual analysis is performed on predictable words, which in turn raises the intriguing possibility that the amount of RB for W2 is directly affected by the amount of visual analysis applied to W1. This hypothesis could be confirmed by comparing the same set of critical words in predictive versus neutral context sentences.

Conclusions

Despite several decades of research, many questions remain concerning the role of sentence context in visual word recognition. Similarly, whereas the phenomenon of RB has been studied for only a few years, it has been the subject of intense scrutiny, and competing models are hotly debated. Some of the unresolved questions concerning both sentence-context effects and RB might be answered by systematically investigating interactions between the two phenomena. The experiments presented in this article illustrate this approach through an exploration of the effects of misperceiving a word on RB for a downstream word in an RSVP sentence. The resulting new paradigm, known as *misreading RB* can be used to investigate whether effects seen in serial recall of RSVP sentences are the results of perceptual or reconstructive processes.

References

- Armstrong, I. T., & Mewhort, D. J. K. (1995). Repetition deficit in rapid-serial-visual-presentation displays: Encoding failure or retrieval failure? *Journal of Experimental Psychology: Human Perception and Performance*, *21*, 1044–1052.
- Bavelier, D. (1994). Repetition blindness between visually different items: The case of pictures and words. *Cognition*, *51*, 199–236.
- Bavelier, D. (1999). Role and nature of object representations in perceiving and acting. In V. Coltheart (Ed.), *Fleeting memories: Cognition of brief visual stimuli* (pp. 151–179). Cambridge, MA: MIT Press.
- Bavelier, D., Prasada, S., & Segui, J. (1994). Repetition blindness between words: Nature of the orthographic and phonological representations involved. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *20*, 1437–1455.
- Chialant, D., & Caramazza, A. (1997). Identity and similarity factors in repetition blindness: Implications for lexical processing. *Cognition*, *63*, 79–119.
- Cohen, J. D., MacWhinney B., Flatt, M., & Provost, J. (1993). PsyScope: A new graphic interactive environment for designing psychology experiments. *Behavioral Research Methods, Instruments, and Computers*, *25*, 257–271.
- Downing, P., & Kanwisher, N. (1995). Types and tokens unscathed: A reply to Whittlesea, Dorken, and Podrouzek (1995) and Whittlesea and Podrouzek (1995). *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *21*, 1698–1702.
- Duffy, S. A., Henderson, J. M., & Morris, R. K. (1989). Semantic facilitation of lexical access during sentence processing. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *15*, 791–801.
- Ehrlich, S. F., & Rayner, K. (1981). Contextual effects on word perception and eye movements during reading. *Journal of Verbal Learning and Verbal Behavior*, *20*, 641–655.
- Fagot, C., & Pashler, H. (1995). Repetition blindness: Perception or memory failure? *Journal of Experimental Psychology: Human Perception and Performance*, *21*, 275–292.
- Fischler, I., & Bloom, P. A. (1979). Automatic and attentional processes in the effects of sentence contexts on word recognition. *Journal of Verbal Learning and Verbal Behavior*, *18*, 1–20.
- Fodor, J. A. (1983). *The modularity of mind*. Cambridge, MA: MIT Press.
- Forster, K. I. (1970). Visual perception of rapidly presented word sequences of varying complexity. *Perception & Psychophysics*, *8*, 215–221.
- Forster, K. I. (1979). Levels of processing and the structure of the language processor. In W. Cooper & E. Walker (Eds.), *Sentence processing: Psycholinguistic studies presented to Merrill Garrett* (pp. 27–85). Hillsdale, NJ: Erlbaum.
- Forster, K. I. (1989). Basic issues in lexical processing. In W. Marslen-Wilson (Ed.), *Lexical representation and process* (pp. 75–107). Cambridge, MA: MIT Press.
- Francis, W. N., & Kučera, H. (1982). *Frequency analysis of English usage: Lexicon and grammar*. Boston: Houghton Mifflin.
- Harris, C. L. (1998). Psycholinguistic Studies of Entrenchment. In J. Koenig (Ed.), *Conceptual structure, discourse, and language* (pp. 55–70). Stanford, CA: California State University, Los Angeles.
- Harris, C. L., & Morris, A. L. (2000). Orthographic repetition blindness. *Quarterly Journal of Experimental Psychology: Human Experimental Psychology*, *53(A)*, 1039–1060.
- Harris, C. L., & Morris, A. L. (2001a). Identity and similarity in orthographic repetition blindness: No cross-over interaction. *Cognition*, *81*, 1–40.
- Harris, C. L., & Morris, A. L. (2001b). Illusory words created by repetition blindness. *Psychonomic Bulletin & Review*, *8*, 118–126.
- Harris, C. L., Morris, A. L., & Ducumbs, S. (1999, April). *Repetition blindness in the cerebral hemispheres*. Poster session presented at the annual meeting of the Cognitive Neuroscience Society, San Francisco.
- Hochhaus, L., & Johnston, J. C. (1996). Perceptual repetition blindness effects. *Journal of Experimental Psychology: Human Perception and Performance*, *22*, 355–366.
- Kanwisher, N. (1987). Repetition blindness: Type recognition without token individuation. *Cognition*, *27*, 117–143.
- Kanwisher, N. (1991). Repetition blindness and illusory conjunctions: Errors in binding visual types with visual tokens. *Journal of Experimental Psychology: Human Perception and Performance*, *17*, 404–421.
- Kanwisher, N., Driver, J., & Machado, L. (1995). Spatial repetition blindness is modulated by selective attention to color or shape. *Cognitive Psychology*, *29*, 303–337.
- Kanwisher, N., Kim, J. W., & Wickens, T. D. (1996). Signal detection analyses of repetition blindness. *Journal of Experimental Psychology: Human Perception and Performance*, *22*, 1249–1260.
- Kanwisher, N., & Potter, M. (1990). Repetition blindness: Levels of processing. *Journal of Experimental Psychology: Human Perception and Performance*, *16*, 30–47.
- Kanwisher, N., Yin, C., & Wojciulik, E. (1999). Repetition blindness for pictures: Evidence for the rapid computation of abstract visual descriptions. In V. Coltheart (Ed.), *Fleeting memories* (pp. 119–150). Cambridge, MA: MIT Press.
- Loftus, G. R., & Masson, M. E. J. (1994). Using confidence intervals in within-subject designs. *Psychonomic Bulletin & Review*, *1*, 476–490.
- Luo, C. R., & Caramazza, A. (1995). Repetition blindness under minimum memory load: Effects of spatial and temporal proximity and the encoding effectiveness of the first item. *Perception & Psychophysics*, *57*, 1053–1064.
- Mandler, G. (1980). Recognizing: The judgment of previous occurrence. *Psychological Review*, *87*, 252–271.
- Masson, M. E. J., Caldwell, J. I., & Whittlesea, B. W. A. (2000). When lust is lost: Orthographic similarity effects in the encoding and reconstruction of rapidly presented word lists. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *26*, 1005–1022.
- McClelland, J. L. (1987). The case for interactionism in language processing. In M. Coltheart (Ed.), *Attention and performance XII: The psychology of reading* (pp. 3–36). Hillsdale, NJ: Erlbaum.
- McClelland, J. L., & Rumelhart, D. E. (1981). An interactive activation model of context effects in letter perception: 1. An account of basic findings. *Psychological Review*, *88*, 375–407.
- Morris, R. K. (1994). Lexical and message-level sentence context effects on fixation times in reading. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *20*, 92–103.
- Morris, A. L., & Harris, C. L. (2001, November). *Repetition blindness: Out of sight or out of mind?* Poster presented at the 42nd Annual Meeting of the Psychonomic Society, Orlando, FL.
- Norris, D. (1986). Word recognition: Context effects without priming. *Cognition*, *22*, 93–136.
- Park, J., & Kanwisher, N. (1994). Determinants of repetition blindness. *Journal of Experimental Psychology: Human Perception and Performance*, *20*, 500–519.
- Potter, M. C. (1984). Rapid serial visual presentation (RSVP): A method for studying language processing. In D. Kieras & M. Just (Eds.), *New methods in reading comprehension research* (pp. 91–118). Hillsdale, NJ: Erlbaum.
- Potter, M. C., Kroll, J. F., Yachzel, B., Carpenter, E., & Sherman, J. (1986). Pictures in sentences: Understanding without words. *Journal of Experimental Psychology: General*, *115*, 281–294.
- Potter, M. C., Moryadas, A., Abrams, I., & Noel, A. (1993). Word perception and misperception in context. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *19*, 3–22.
- Potter, M. C., Stiefbold, D., & Moryadas, A. (1998). Word selection in reading sentences: Preceding versus following contexts. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *24*, 68–100.
- Schendan, H. E., Kanwisher, N., & Kutas, M. (1997). Early brain potentials link repetition blindness, priming and novelty detection. *Neuroreport*, *8*, 1943–1948.

- Schubert, R. E., & Eimas, P. D. (1977). Effects of context on the classification of words and non-words. *Journal of Experimental Psychology: Human Perception and Performance*, 3, 27–36.
- Schwanenflugel, P. J., & Shoben, E. J. (1985). The influence of sentence constraint on the scope of facilitation for upcoming words. *Journal of Memory and Language*, 24, 232–252.
- Simpson, G. B., Peterson, R. R., Casteel, M. A., & Burgess, C. (1989). Lexical and sentence context effects in word recognition. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 15, 88–97.
- Stanovich, K. E., & West, R. F. (1981). The effect of sentence context on ongoing word recognition: Tests of a two-process theory. *Journal of Experimental Psychology: Human Perception and Performance*, 7, 658–672.
- Swinney, D. A., & Hakes, D. T. (1976). Effects of prior context upon lexical access during sentence comprehension. *Journal of Verbal Learning and Verbal Behavior*, 15, 681–689.
- Van der Velde, F. (1992). Effect of orthographic preactivation on letter migration. *Journal of Experimental Psychology: Human Perception and Performance*, 18, 449–459.
- Whittlesea, B. W. A., Dorken, M. D., & Podrouzek, K. W. (1995). Repeated events in rapid lists. Part 1: Encoding and representation. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 21, 1670–1688.
- Whittlesea, B. W. A., & Podrouzek, K. W. (1995). Repeated events in rapid lists. Part 2: Remembering repetitions. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 21, 1689–1697.
- Whittlesea, B. W. A., Podrouzek, K. W., Dorken, M. D., & Williams, L. D. (1995). Types and tokens: Unscathed or unscathable? Reply to Downing and Kanwisher. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 21, 1703–1706.
- Whittlesea, B. W. A., & Wai, K. H. (1997). Reverse “repetition blindness” and release from “repetition blindness”: Constructive variations on the “repetition blindness” effect. *Psychological Research*, 60, 173–182.

Received July 20, 2001

Revision received March 1, 2002

Accepted March 25, 2002 ■

New Editors Appointed, 2004–2009

The Publications and Communications Board of the American Psychological Association announces the appointment of five new editors for 6-year terms beginning in 2004.

As of January 1, 2003, manuscripts should be directed to the following individuals. Authors are strongly encouraged to submit manuscripts electronically as of January 1, 2003, through the journal’s Manuscript Submission Portal (see the Web site listed with each journal title).

- For *Psychology and Aging* (<http://www.apa.org/journals/pag.html>), submit manuscripts to **Rose T. Zacks, PhD**, Department of Psychology, Michigan State University, East Lansing, MI 48824-1117.
- For *Psychological Assessment* (<http://www.apa.org/journals/pas.html>), submit manuscripts to **Milton E. Strauss, PhD**, Department of Psychology, Case Western Reserve University, Cleveland, OH 44106-7123.
- For *Journal of Family Psychology* (<http://www.apa.org/journals/fam.html>), submit manuscripts to **Anne Kazak, PhD**, ABPP, Oncology Psychosocial Research, The Children’s Hospital of Philadelphia, Room 1486 (Market Street), 34th and Civic Center Boulevard, Philadelphia, PA 19104. For overnight couriers: Room 1486, 3535 Market Street, Philadelphia, PA 19104.
- For *Journal of Experimental Psychology: Animal Behavior Processes* (<http://www.apa.org/journals/xan.html>), submit manuscripts to **Nicholas Mackintosh**, Department of Experimental Psychology, University of Cambridge, Downing Street, Cambridge, CB2 3EB, United Kingdom.
- For *Journal of Personality and Social Psychology: Personality Processes and Individual Differences* section (<http://www.apa.org/journals/psp.html>), submit manuscripts to **Charles S. Carver, PhD**, Department of Psychology, University of Miami, P.O. Box 248185, Coral Gables, FL 33124-2070.

Manuscript submission patterns make the precise date of completion of the 2003 volumes uncertain. Current editors Leah L. Light, PhD, Stephen N. Haynes, PhD, Ross D. Parke, PhD, Mark E. Bouton, PhD, and Ed Diener, PhD, respectively, will receive and consider manuscripts through December 31, 2002. Should 2003 volumes be completed before that date, manuscripts will be redirected to the new editors for consideration in 2004 volumes.