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When Lust is Lost: Orthographic Similarity Effects in the Encoding and Reconstruction of Rapidly Presented Word Lists

Michael E. J. Masson and Judy I. Caldwell
University of Victoria

Bruce W. A. Whittlesea
Simon Fraser University

A reconstructive account of memory is presented to explain the finding that report of a word (C2) appearing in a rapidly presented list is reduced when it is orthographically similar to an earlier word (C1) in the list. On this account, the effect arises when reconstructing the list from memory, not at the time of list presentation as proposed by accounts based on failure of encoding or tokenization. The reconstructive account is supported by a series of experiments that show: a retroactive effect in which report of C1 is enhanced by similarity to C2; a nonword C1 can either interfere with or enhance report of C2, depending on how accurately C1 is encoded; manipulation of reconstructive processes can eliminate or enhance the effect of orthographic similarity; and a bi-directional trade-off in the report of an orthographically similar C1-C2 pair, whereby report of one member compromises report of the other.

In a radical departure from established thinking, Bartlett (1932) argued that remembering is a creative, reconstructive process rather than reduplicative of past experiences. He illustrated this claim with demonstrations that the act of remembering a story suffers from a variety of distortions, including omission of detail that conflicts with the general theme of the story and addition of detail that conforms to that theme. In his account of remembering, fragments of information actually retrieved about an event are fleshed out with consistent or logically implied detail, under the control of the rememberer's expectations about the usual nature of experience. In effect, Bartlett thought of remembering as a process of deciding what the past must have been, rather than a process of retrieval.

Much of the research on remembering, however, has used lists of unrelated items rather than stories, and these lists have no overall meaning or general coherence that could guide later reconstructive processes. Under these circumstances, distortions in remembering based on relationships among components of the list are rarely observed, and investigators have had little reason to be concerned with after-list reconstructive processes. Instead, each item of a list is analyzed as an independent unit; the investigator is interested in the ways in which specific item properties (e.g., frequency, neighborhood size, concreteness, etc.) or specific encoding or retrieval tasks (e.g., levels of processing and retrieval context manipulations) influence the remembering of particular items. Under these circumstances, remembering is thought of as the engagement of a memory trace by some environmental cue, producing a mental event that is reported by the observer as a recollection. A subject's decision strategies in reporting events that come to mind are treated as bias, and not considered part of the remembering process per se; true remembering is the subject's sensitivity to the prior occurrence of an item. It thus became convenient to think of remembering simply as retrieval of representations of past events, and to direct investigation toward the factors that influence the success of that retrieval.

Some investigators, however, have argued that the retrieval metaphor for remembering is too simple, even for remembrance of events with no thematic relationship. Instead, these investigators follow Bartlett (1932) in suggesting that the act of remembering consists of two aspects, the production of a mental event and the adoption of an attitude toward that event (e.g., Whittlesea, 1997). The act of constructing this mental event is often influenced by representations of events laid down in prior experiences and cued by present circumstances (true remembering). The mental event, however, is not a copy of the representation of the prior experience elevated to consciousness (as suggested by the retrieval metaphor); instead, it is constructed through a dynamic interaction between the

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Michael E. J. Masson and Judy I. Caldwell, Department of Psychology, University of Victoria, Victoria, British Columbia, Canada; Bruce W. A. Whittlesea, Department of Psychology, Simon Fraser University, Burnaby, British Columbia, Canada.

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Correspondence concerning this article should be addressed to Michael E. J. Masson, Department of Psychology, University of Victoria, P.O. Box 3050, Victoria BC V8W 3P5, Canada. Electronic mail may be sent to mmasson@uvic.ca.

cue properties of the current environment (stimulus, context and intention) and the representations in memory. In consequence, the content of the mental event may closely resemble some actual prior experience; alternatively , it may be an amalgamation of multiple prior experiences, and may also incorporate detail from the person's present circumstances. The constructive nature of this act of producing mental events as remembrances has been extensively documented in studies of eyewitness testimony (Loftus, 1979; Loftus & Palmer, 1974; Lindsay, 1990), autobiographical memory (Hyman & Pentland, 1996) and list memory (Roediger & McDermott, 1995).

Moreover, the act of production does not itself constitute remembering. To qualify as a remembrance, the person further has to attribute the experience of the mental event to a source in the past. For example, a person encountering a statue might think "Isn't that ugly!". Seeing it again on a later occasion, they may reduplicate that mental event, and do so more readily because of the influence of the prior experience. That reduplication may be experienced as a remembrance ("I've thought that before") or as an original thought. The occurrence of one or the other of these outcomes appears to depend on an unconscious inferential process, that evaluates the fluency (Jacoby, Kelley, & Dywan, 1989) or vividness (Johnson & Raye, 1981) of the mental event and attributes it to whatever source seems likely (Marcel, 1983). These inferential processes interact with the productive processes, causing the person to reject some aspects of a mental event as actual remembrance but to elaborate on other aspects, producing further postulated detail.

According to theorists espousing this reconstructive view, including ourselves, every occasion of remembering is controlled by the processes of production and evaluation. These processes are not avoided in standard list-learning and word-recognition studies: they are simply not obvious in such studies, because the variety of factors that could lead to production of various mental events is highly constrained, as is the variety of potential sources of influence in the present and the past. Moreover, the reconstructivist account does not apply only to the act of remembering. It suggests that all interactions with stimuli, including acts of perception, identification, knowing, liking and using stimuli, have the same general nature: a stimulus is encountered in some context for some purpose; together, the stimulus, context and purpose cue some memory representations and cause some mental events to occur; the person then evaluates that processing and makes some inference about the source, utility or veracity of the mental event. The critical aspect of this account is that the conclusion at which the person arrives, the mental representation of their current experience of a stimulus, is never a direct consequence of the stimulus of itself. The mental event is inevitably an interpretation of the stimulus, supported by the physical properties of the stimulus, but influenced by the person's intentions, attentional constraints, beliefs about the world and contextual circumstances.

Investigations of the "repetition blindness" effect illustrate the potential importance of the reconstructive notion for functions other than remembering. The effect consists of the observation that people experience more difficulty in reporting occurrences of repeated than nonrepeated words when the words appear in short lists displayed using rapid serial visual presentation (RSVP). Specifically, with presentation durations briefer than about 150 ms per word, observers are less likely to report both occurrences of a repeated word than they are to report two unrelated words (e.g., Kanwisher, 1987). This phenomenon has aroused great interest among investigators of attention and memory because of its apparent implications for the nature of encoding and representation.

Kanwisher (1987; Park & Kanwisher, 1994) explained this "repetition blindness" effect by making a distinction between "type" and "token" representations of experience (see Anderson & Bower, 1973; Simon & Feigenbaum, 1964). According to this account, each object that a person encounters is identified as an example of some concept (its type), and is also individuated as a particular occurrence of that type; a token is formed to represent that particular occurrence. If observers are shown a list of words, they can later report the list by retrieving the set of tokens formed during presentation of the list. Applying this idea to the "repetition blindness" effect, Kanwisher proposed that formation of a token representing the second occurrence is inhibited when the second occurrence occurs too soon after the first, resulting in selective failure to report the second occurrence of a repeated word. Unfortunately, when observers report a repeated word only once, which occurrence of the repeated word (first or second) was reported must be inferred from the location of that item relative to other reported words. In consequence, it may be unclear whether an instance of "repetition blindness" results from selective inability to report, say, the first occurrence of a repeated word, or instead from a nonselective inability.

A similar interference effect occurs, however, when an RSVP list contains two orthographically similar words, such as *lost* and *lust*. Kanwisher and Potter (1990, Experiment 5) observed that the first item of a related pair (which we will refer to as C1) was reported about as often as the first word of an unrelated pair, but the probability of reporting the second member of a related pair (which we will refer to as C2) was lower than the probability of reporting the second member of an unrelated pair. Because the members of the pair are different words, it is possible to know unambiguously which member of the pair is not reported. In this experiment, it is clear that the similarity of the target words resulted in a selective inability to report the second member of a related pair (C2). Kanwisher and Potter attributed this orthographically based version of "repetition blindness" to misperception of C2. In particular, when C2 is misperceived as a repetition of

C1, the mechanism responsible for "repetition blindness" then prevents tokenization of C2. Kanwisher and Potter also considered the possibility that misperception and failed tokenization could operate at the level of letter clusters (see also Bavelier, Prasada, & Segui, 1994).

The type-token account thus explains "blindness" effects in report of RSVP lists purely as a deficit in encoding. To make that argument, one must assume there is a mechanism that prevents the encoding of repetitions and near-repetitions. It must further be assumed that the formation of a stable memory trace of the occurrence of a word is a necessary and sufficient condition for report of that occurrence. As a corollary, possession of two separate traces of a repeated word is a necessary and sufficient condition for reporting the word twice; the observer can know that the word was presented repeatedly because each token supports retrieval of that word, so that the observer remembers the word twice. Therefore, if a repeated word is reported only once, it can be concluded that the observer possesses only one representation of that word.

Fundamentally, by the type-token account, it is assumed that remembered events are isomorphic with the representations of those events, such that each separate trace can produce a separate mental event; remembering consists of elevating memory traces to consciousness. In contrast, Whittlesea, Dorken, and Podrouzek (1995) and Whittlesea and Podrouzek (1995) explained "repetition blindness" through a reconstructive account, in which it was assumed that encoding was not affected by repetition. Instead, the availability of two encodings of a repeated word is claimed to affect the retrieval and decision processes performed at the time of recall.

According to this reconstructive account, the relationship between memory representations and remembering is less direct than that suggested by the type-token account. When cued, memory can cause events to come to mind, but these mental events are not necessarily isomorphic with the representations in memory, either in number or content. The occurrence of a conscious mental event is thus not a direct indicator of the past. Instead, the person is in the position of trying to make decisions about the nature of their past experience, based on the evidence of what comes to mind now. That is, according to this account, the observer does not have direct access to memory contents, but instead must judge the past on the basis of the influence of memory on current performance (e.g., Jacoby & Whitehouse, 1989).

By this reconstructive account, it is assumed that during presentation, each occurrence of a repeated word is as likely to be encoded as is an occurrence of a nonrepeated word presented in the same list location. Under RSVP, however, list items are not processed extensively. More important, neither is the association between list items and their particular contexts (adjacent words). As a consequence, the representation of a word's occurrence does not contain much distinctive information. This situation holds whether or not a word is a member of a repeated pair. In the case of a repeated pair of words, however, cuing either of its two representations will produce much the same result, namely the identity of that item. Thus, even if both occurrences are encoded and later produce a mental event, those mental events will often not be distinctive enough for the subject to realize that they are produced by different sources, and so reflect repeated occurrences of the same word. Subjects are likely to decide that they saw that word only once, even though their recall of that word is supported by two separate representations. Moreover, the first occurrence of the repeated word, presented earlier in the list, is likely to be more extensively processed with its context than is the case for the second occurrence of that word (Whittlesea et al., 1995, Experiment 2b). Subjects are more likely to be able to retrieve that context than the context of the second occurrence, and will therefore more likely report that word as having occurred in the first context than in the second. A "repetition blindness" effect arises, then, not simply because of a lack of distinctive encoding—which is equally problematic for nonrepeated words—but because that lack of encoding makes it unlikely that subjects will have adequate evidence to support the report of two separate occurrences of a repeated word.

In extending the reconstructive account of "repetition blindness" to explain orthographic similarity effects, we further assume that orthographic information about words presented in an RSVP list is not extensively processed. Therefore, representations of the orthographic patterns of the words can be incomplete or fragmented. At the time of recall, orthographic and other available information associated with the occurrence of a word can be combined to construct a plausible candidate word. The availability of a similar set of orthographic information (due to the occurrence of an orthographically similar word) may be misconstrued as part of the occurrence of the earlier word, rather than a separate event. This confusion arises because of incomplete orthographic information and because of the lack of contextual distinctiveness that contributes to "repetition blindness." As indicated above, we expect that the first occurrence of an orthographically similar pair is likely to be more extensively processed with its context and may therefore be reported prior to later words in the list. Successful report of the first member of an orthographically similar pair places retrieved information about the second member at risk; instead of interpreting that information as evidence for a separate event, it may be attributed to the earlier event.

In the experiments reported in this article, we examined the idea of reconstructive recall of RSVP lists, and the potential for such an idea to explain the interference effect that arises from orthographic similarity. Because that effect clearly consists of a selective failure to report the second word of a related pair, it seems to stand as powerful evidence for the utility of a type-token account and its corollary assumption of interference with the encoding of identical or near-identical items. The evidence we obtained, however, appears incompatible with fundamental assumptions of that account. Instead, our results favor the idea that failure to report both of two orthographically related words results from decisions made after the list has been presented, in the act of recalling it.

In Experiments 1 and 2, we replicate the basic orthographic similarity effect and also show how evidence regarding the occurrence of one member of a pair of orthographically similar words can be misattributed to the other member, depending on the relative accuracy of the encoding of the two items. Thus, it is possible for orthographic similarity to produce enhanced report of C2 rather than interfere with it. The purpose of these two experiments was primarily to test predictions of the reconstruction account of orthographic similarity effects. Results with direct implications for type-token accounts of interference effects are provided in Experiments 3-6, in which we show that the influence of orthographic similarity upon the report of C2 can be eliminated or enhanced depending on how report of C2 is cued. Finally, Experiment 7 demonstrates a trade-off in the report of C1 and C2, such that report of one member of the pair interferes, even retroactively, with report of the other member of the pair.

Experiment 1

Experiment 1 was designed primarily to replicate the effect of orthographic similarity on the report of a pair of similar words that appear in an RSVP list. In addition, we assessed the report of the first member of the pair to determine whether it received some benefit from its relation to the second member of the pair. This benefit is expected by the reconstructive account inasmuch as orthographic evidence associated with C2 is assumed to have the potential to be misattributed to C1. In some previous studies of orthographic similarity, no information about the report of C1 was provided (e.g., Bavelier et al., 1994; Bavelier & Potter, 1992), although Morris and Harris (1999, Experiment 4) obtained an advantage for report of C1 when it was orthographically similar to C2.

Method

Subjects. Sixteen undergraduate students at the University of Victoria participated in return for payment of $5. Their ages ranged from 20 to 36 years, with a median age of 23 years. All subjects were native speakers of English.

Materials and design. Thirty-two word lists containing seven words each were constructed, each of which contained a pair of critical words (C1 and C2) that were orthographically similar (e.g., *slam purr bike atom bite carp null*). Orthographically similar word pairs in all experiments reported here were of the same length and differed by one interior letter (e.g., *bike bite*). All of the words within a list were of uniform length: four letters in some lists and five letters in others. The items adjacent to a member of the critical pair of words in a list did not share with that member any letters in the same position. The location of C1 in the list varied from the second to the fourth position, and C1 and C2 were always separated by one intervening item. The 32 lists were divided into two sets of 16 lists. A second version of each list was created by moving the C2 item to a different word list that contained a C1 that was orthographically different from it and the movement of C2 to a new list was done within the set of 16 lists (e.g., *have sung pork fuss bite cubs glum*). Although we did not counterbalance the assignment of items to the C1 and C2 positions, the comparisons of critical interest were within position. Therefore, counterbalancing items across C1 and C2 positions was not necessary. Critical items assigned to the C1 position ranged in frequency from 1 to 967 per million (Kucera & Francis, 1967), with a median frequency of 18.5; critical items assigned to the C2 position ranged in frequency from 1 to 395, with a median of 26.

Four practice lists, none of which contained orthographically similar word pairs, were also created. Each subject was presented the orthographically similar version of one of the sets of 16 lists and the orthographically different version of the other set. The assignment of sets of 16 lists to subjects was counterbalanced so that each list was tested equally often.

Procedure. Subjects were tested individually in the presence of the experimenter. Instructions and materials were displayed using a Macintosh II computer with two monochrome monitors with a refresh cycle of 15 ms. Characters appeared on the monitors in black on a light gray background. At a viewing distance of 40 cm, a four-letter word subtended approximately 1.2 degrees of visual angle. The subject viewed one monitor while the experimenter viewed the other. Subjects were told that they would be presented a series of word lists, and that each list would be shown one word at a time. The subject's task was to report orally, in the correct order, all of the words that were presented in each list. The four practice lists were presented, followed by the 32 critical lists in random order.

Each trial began with the word READY presented in the center of the subject's monitor. The subject pressed a button mounted on a button box to begin the trial. The READY signal was erased and a row of 8 ampersands was displayed for 495 ms. The ampersands were then erased and the word list was presented, one item at a time, each appearing in the center of the monitor for 120 ms and immediately replaced by the next word. To allow subjects to adapt to the rapid presentation, the display duration for each word in the first and second practice trials was 300 ms and 210 ms, respectively. The list was followed by a row of 8 ampersands for 495 ms acting as a postmask. The postmask was then replaced by a numeral indicating the trial number, which indicated to subjects that they were

Table 1
Mean Proportion of Correctly Reported Critical Words in Experiment 1

	C1		C2		C1&C2		C2 | C1	
Condition	M	SE	M	SE	M	SE	M	SE
C1, C2 Similar	.52	.05	.16	.03	.02	.01	.02	.01
C1, C2 Different	.34	.04	.21	.03	.08	.03	.17	.05

Note. C1&C2 represents joint report of C1 and C2. C2 | C1 represents report of C2 conditionalized on correct report of C1.

to respond. The experimenter recorded the subject's oral response on a sheet of paper.

Results and Discussion

The reports recorded by the experimenter were scored for the correct report of C1 and C2. Although subjects had been instructed to report the words in their correct order, report of C1 and C2 was considered correct, regardless of where these items appeared in the subject's report of the list. The mean proportion of these items reported in each condition is shown in Table 1.

Separate ANOVAs with orthographic similarity as a repeated measures factor were used to test the effect of similarity on the report of each type of critical word. The analysis of C1 report indicated that there was a reliable increase in report of C1 due to orthographic similarity, $F(1, 15) = 12.13$, $MSE = .023$. Report of C2 items was examined in three ways. First, the simple probability of reporting C2 showed no reliable effect of orthographic similarity, $F(1, 15) = 1.52$, $MSE = .014$. Second, the joint probability of reporting C1 and C2 was assessed. That analysis showed that the probability of joint report was reliably lower when C1 and C2 were orthographically similar, $F(1, 15) = 6.67$, $MSE = .005$. We note that when C1 and C2 were jointly reported in the orthographically different condition, they were almost always (19 times out of 20) reported in the correct order relative to one another. There were only four instances of joint report in the similar condition and in just one of these was the order of reporting C1 and C2 correct. Finally, the conditional probability of reporting C2, given that C1 was reported was analyzed, following Bavelier et al. (1994, Experiment 1). We report this measure only because it was the one used by Bavelier et al. and we wish to demonstrate comparability across our respective experiments. This analysis showed that the conditionalized report of C2 was lower in the orthographically similar condition, $F(1, 15) = 10.02$, $MSE = .017$.

The analysis of the joint report of C1 and C2 and the analysis of C2 conditionalized on report of C1 both showed an interference effect due to orthographic similarity. Moreover, as expected by the reconstructive account, the report of C1 was increased as a result of orthographic similarity to C2. An advantage for reporting C1 when it was orthographically similar to C2 has also been found by Morris and Harris (1999), but in most other cases, typically involving repetition rather than orthographic similarity, no effect on C1 has been obtained (e.g., Kanwisher, 1987; but see Luo & Caramazza, 1996). We suspect that enhanced report of C1 in repeated or orthographically similar conditions may arise when reconstruction of list members is not strongly supported by contextual cues such as grammatical sentence structure. With impoverished contextual cues and a demand to report items in the correct order, as in Experiment 1, subjects may actively analyze information about downstream items to help determine a candidate word's proper location. In seeking this information, subjects may recruit orthographic evidence made available by presentation of a related word later in the list and use it to support reconstruction of C1. This misuse of orthographic information would improve C1 report and contribute to failed C2 report.

Experiment 2

In Experiment 2, we further examined the interaction between a pair of orthographically similar items by using nonwords in the C1 position. Nonwords should be particularly difficult to identify when presented briefly, although orthographic information about such items might be available. By the reconstructive account, the orthographic evidence available from an unidentified nonword could be used to support reconstruction of an orthographically related C2 word. Thus, by using a nonword C1, orthographic similarity should lead to enhanced report of C2. Indeed, Potter (cited in Park & Kanwisher, 1994, p. 517) found that this arrangement led to frequent reports of a C2 word in the serial position of an orthographically similar C1 nonword. Enhanced report of C2, however, should occur only so long as C1 is not identified and remembered. Under conditions that would permit subjects to identify and remember C1, evidence produced by an orthographically similar C2 might be misattributed to the C1 item, producing an interference effect instead. These possibilities were tested by presenting the nonword C1 either at the same duration as other items in a list (120 ms) or at an extended duration (600 ms) that made identification and later recollection very likely.

In anticipation of finding enhanced report of C2 when paired with an orthographically similar nonword, we were concerned with the problem of distinguishing valid report of C2 from an intrusion of that word prompted by an orthographically related nonword. Such intrusions might occur because the nonword's orthographic information could be used to reconstruct an orthographically similar word. These intrusions would contribute to report of the orthographically similar word, perhaps generating an artifactual enhancement effect. To address this concern, we included a set of lists that contained the critical nonword and an unrelated filler word in the C2 position. This third condition, referred to as the "control" condition, was used to estimate the probability of an intrusion consisting of a critical word when an orthographically similar nonword is present in the list. By using a filler word instead of a critical C2 item in these lists, we ensured that the nonword's critical orthographic partner was not presented at any time during the experiment. The probability of reporting the nonword's partner could then be used to correct the report of C2 in the similar condition to compensate for intrusions of C2 prompted by the orthographically similar nonword C1.

Method

Subjects. The subjects were 36 undergraduate students at the University of Victoria who participated in the experiment for extra credit in an introductory psychology course. Half of the subjects were randomly assigned to the 120-ms duration condition and the other half were assigned to the 600-ms condition. The ages of the subjects ranged from 18 to 43, with a median age of 19 years. All subjects were native speakers of English and none had taken part in Experiment 1.

Materials and design. Sixty word lists were constructed, each of which contained a critical, orthographically similar nonword-word pair (e.g., *chiof chief*). The nonword member of each pair was constructed by changing one interior letter of its word partner to form a pronounceable nonword. The 60 critical words ranged in normative frequency from 1 to 938 per million, with a median of 48. The word and nonword members of each pair of critical items were of the same length: four or five letters. The remaining items in each list were 4 to 7 letters in length, and each list contained six items. In creating the lists, C1 (the nonword) occupied the second or third position in the list, and C2 (the word) followed C1 with one intervening word between them (e.g., *black chiof horses chief ground snakes*). Two additional versions of each list were also constructed. In the orthographically different version of the lists, each C2 item was reassigned to a different list, such that the C2 item was orthographically different from C1 and from either of the immediately adjacent words in the list (e.g., *black chiof horses drill ground snakes*). The 60 word lists were treated as three sets of 20 lists, and the movement of C2 to a new list was done within these sets. In the other version of the lists, used in the control condition, an unrelated word that was not used in any other list, replaced the C2 word (e.g., *black chiof horses water ground snakes*). The probability of an intrusion error (erroneously reporting a C2 word—i.e., *chief* in the orthographically different and control condition examples here) based only on misperception of the C1 item (*chiof* in the examples) could be obtained either from the orthographically different or the control condition. We used the latter, simply because the C2 word associated with the control version of each list never occurred in any list seen by the subject.

Each subject was shown all 60 lists consisting of three sets of 20 lists; one set was from each of the three versions (similar, different, and control). The assignment of versions of lists to subjects was counterbalanced so that each version of a list was tested equally often. Three practice lists were also created, each of which contained one nonword. All practice lists were shown to each subject.

Procedure. Testing was done using the same equipment and general procedure as in Experiment 1. The subjects' task was to orally report all of the items in the list, although they were not instructed to report items in their order of presentation. Subjects were tested with the practice lists, followed by the critical lists, shown in random order. Each list began and ended with a 120-ms presentation of a row of six @ symbols that served as pre- and postmasks for the list. Each item in a list was presented for 120 ms, except that in the extended C1 duration condition, the C1 item was presented for 600-ms. The post mask was replaced with a blank screen which remained until the experimenter had finished recording the subject's report of the list. To enable subjects to acquaint themselves with the task, items in the first practice list were presented for 150 ms each (750 ms for C1 in the extended C1 duration condition).

Results and Discussion

Verbal reports were scored for report of C1 and C2, as in Experiment 1. The mean proportions of correctly reported C1 and C2 items are shown in Table 2. These means clearly show that report of C1 was much more likely when it was presented for a longer duration, although its similarity to C2 had no effect. An ANOVA with orthographic similarity (similar and different) as a repeated measures factor and duration of C1 (120 and 600 ms) as a between subjects factor indicated that the only significant effect was that of duration, $F(1, 34) = 409.00$, $MSE = 0.053$.

The means in Table 2 also show that the influence of orthographic similarity upon the report of C2 depended on the duration of the nonword C1. Similarity was an advantage when C1 duration was 120 ms, but a disadvantage when the duration was 600 ms. An ANOVA with similarity and duration as factors was applied to the probability of reporting C2. This analysis revealed a main effect of C1 duration favoring the 120-ms group over the 600-ms group (.32 vs. .24), $F(1, 34) = 9.70$, $MSE = 0.010$. More important, there

Table 2
Mean Proportion of Correctly Reported Critical Items in Experiment 2

	Duration of C1											
	120 ms				600 ms							
	C1		C2		C1		C2		C1&C2		C2 | C1	
Condition	M	SE	M	SE	M	SE	M	SE	M	SE	M	SE
C1, C2 Similar	.06	.01	.42	.03	.69	.03	.13	.02	.04	.01	.05	.01
C1, C2 Different	.05	.01	.21	.02	.69	.03	.35	.02	.23	.02	.33	.03
Control	.04	.01	.09	.02	.63	.03	.07	.01				

Note. C1&C2 represents joint report of C1 and C2. C2 | C1 represents report of C2 conditionalized on correct report of C1 (this measure was not computed for the 120-ms condition because C1 was rarely reported).

was a significant, cross-over interaction between orthographic similarity and C1 duration, $F(1, 34) = 63.47$, $MSE = 0.013$. When C1 was presented for 120 ms, orthographic similarity to C1 led to enhanced report of C2, $F(1, 17) = 27.54$, $MSE = 0.014$. In contrast to that effect, when C1 was presented for 600 ms, report of C2 was substantially reduced by orthographic similarity to C1, $F(1, 17) = 36.83$, $MSE = 0.011$.

For subjects in the 120-ms group, we also computed corrected C2 scores for the similar condition because the similarity advantage seen with those subjects might have resulted from an intrusion of C2 induced by an orthographically similar C1 nonword. The corrected scores were computed by subtracting the probability of an intrusion of C2 in the control condition from correct report of C2 in the similar condition. The mean corrected proportion of trials on which C2 was reported in the similar condition was .33 (SE = .03), which was still reliably greater than the proportion of reported C2 items in the different condition (.21), $F(1, 17) = 7.51$, $MSE = .015$. Therefore, we can be confident that the orthographic similarity advantage was not entirely due to C2 intrusions based on an orthographically similar C1.

With a 600-ms duration for C1, that item was more fully encoded and orthographic information associated with it was unlikely to be used to support reconstruction of a subsequently presented word. Instead, it appears that the evidence in support of C1 was so powerful that it attracted evidence more properly associated with the presentation of an orthographically similar C2, leading to reduced report of C2. This orthographic similarity effect also appeared when joint report of C1 and C2 and when report of C2 conditionalized on report of C1 were considered (see means in Table 2), $F(1, 17) = 40.14$, $MSE = 0.008$, and $F(1, 17) = 50.28$, $MSE = 0.013$, respectively. Analyses of joint report of C1 and C2 and conditionalized report of C2 were not conducted for the 120-ms duration condition because C1 was rarely reported in that condition.

The results of Experiment 2 clearly demonstrate that orthographic similarity between C1 and C2 can lead either to enhanced or reduced report of C2, depending on the encoding of C1. We contend that a brief presentation of a nonword C1 leads to an impoverished encoding, leaving encoded information susceptible to misattribution to other, more supportable events such as the presentation of an orthographically similar word. By extending the duration of C1 we were able to establish a more coherent set of evidence in support of the nonword, inoculating it against misattribution and even making it an attractor for evidence from a similar, but less well encoded C2. In contrast to our proposal, Luo and Caramazza (1995) reported that increasing the duration of C1 reduced the probability of reporting C2 when it was a repetition of C1 and attributed this deficit to a refractory period associated with activation of the type representation for C1. When C2 is presented within this refractory period, it often fails to be encoded and therefore is unavailable for report. An account of this form is not plausible for the results of Experiment 2, where C1 and C2 were not identical, unless one argues that the relevant type representations are letter groups (e.g., Bavelier et al., 1994; Morris & Harris, 1999). The viability of such an account is considered further in the General Discussion, after the results of the remaining experiments are presented.

The lack of an effect of orthographic similarity on report of C1 when it was presented for an extended duration suggests that report of the nonword was not strongly influenced by the reconstructive processes that were used to generate other list items. We conjecture that the extended duration of the nonword allowed subjects to encode it with high reliability, even to bring that item to a reportable state, before presentation of the remainder of the list began. Therefore, report of the extended duration item would be unlikely to benefit from the reconstructive processes thought to be responsible for the orthographic similarity effect on words. Imperfect report of the extended duration nonword might be due to errors in encoding the item while it was in view or to forgetting during the interval between presentation of the nonword and its report.

Experiment 3

In Experiment 3, we tested the hypothesis that C1 and C2 are encoded independently. Because our objective was to assess the initial encoding of list items, we deemed it important to do so in a manner that would not invoke reconstructive processes. This goal was accomplished by not requiring subjects to report any contents of the RSVP list. To assess encoding of C2, we presented this item as the last member of the list. The C2 word was then immediately replaced by a probe word in uppercase letters. The task was to read the probe word silently and report the first word that came to mind. On half of the trials, the probe word was associatively related to C2 (e.g., *elder brass glove fever globe WORLD*), and on the other half, it was unrelated to any item in the list (e.g., *elder brass glove fever globe SWEET*).

We expected that when the probe was related to the C2 item in the list (e.g., *globe-WORLD*), responses to the probe would be affected in two ways. First, latency to produce a response to the probe should be reduced, relative to when C2 and the probe are not related, because the probe word would be primed by presentation of a related C2. Second, C2 should be highly available as a response to the probe when the two are related. That same C2 item, however, should be less available when it is not actually present in the list (e.g., *cheek bacon puppy faint poppy WORLD*).

In addition to manipulating the relation between C2 and the probe, we again used orthographically similar or different C1-C2 pairs in the word lists. If the hypothesis that C1 and C2 are encoded independently—despite their possible orthographic similarity—is correct, the influence of C2 on responding to the probe should not be affected by the relation between C1 and C2. This was the crucial prediction for Experiment 3.

Given that there was no requirement to respond to the RSVP lists and that the lists we used were of uniform length, there might be concern that subjects would simply ignore the lists or perhaps be able to selectively attend to only the last item in a list. Therefore, we conducted a companion experiment (Experiment 3A) that was identical to the main experiment (Experiment 3B) to demonstrate that under the presentation conditions of interest, subjects were not able to selectively attend only to the last item in a list. In Experiment 3A, a postmask was presented instead of the probe word and subjects were asked to report the last word in the list. If subjects are able to selectively encode the final word in a list while ignoring all earlier items, then no effect of orthographic similarity between C1 and C2 should be found. On the other hand, if it is not possible to ignore earlier items in the list, even when list length is held constant, we should find lower report of C2 when it is orthographically related to C1. This interference effect should arise because of reconstructive processes like those operating during full-list report, as in Experiments 1 and 2. Even though subjects are required to report only the last word of a list, they would need to engage in a reconstructive process to determine the relative positions of words in the list, enabling them to discern which word was the last one. This reconstruction should involve both C1 and C2, giving rise to possible confusion between them when they are orthographically similar.

Method

Subjects. Eight undergraduate students participated in Experiment 3A and 24 participated in Experiment 3B. Subjects were obtained from the same pool as those in Experiment 2. Their ages ranged from 18 to 30 years, with a median age of 19 years. All subjects were native speakers of English and none had taken part in the earlier experiments.

Materials and design. Eighty lists of five words each were created for Experiment 3A. All words in a list were of the same length (either four or five letters). Each word list contained an orthographically similar word pair. The 80 C1 items from these pairs ranged in normative frequency from 1 to 1290 per million, with a median of 21; the 80 C2 items ranged in normative frequency from 1 to 1160, with a median of 52.5. The critical pair was situated at the end of the list, such that C2 was the last item in the list and C1 was the third to last item (e.g., *hall grey cook lips cork*).

A second version of each list was created by moving the C1 item to a different word list containing a C2 item that was orthographically different from it (e.g., *hall grey neat lips cork*). The 80 word lists were treated as four blocks of 20 word lists, and the movement of C1 to a new list was done within these blocks. Four practice lists were also created, none of which contained an orthographically similar word pair.

The two sets of word lists from Experiment 3A were also used in Experiment 3B. In addition, a probe word that was an associate of C2 was selected for each list (e.g., *hall grey cook lips cork WINE*). These two sets of lists constituted the materials used in the similar-related and different-related conditions. To create the corresponding similar-unrelated and different-unrelated conditions, the probe words were reassigned to different lists such that each probe word was unrelated to the C2 item (e.g., *hall grey cook lips cork POND*; *hall grey neat lips cork POND*). This reassignment was carried out within blocks of 20 word lists. The four practice lists from Experiment 3A were used and each was assigned a probe word. For two of these lists, the probe word was associatively related to the last word in the list; for the other two practice lists the probe word was unrelated to any item in its list.

The design of Experiment 3A consisted of a single, repeated measures factor, orthographic similarity (similar and different). Each subject was presented with 40 lists from each of the two conditions. The assignment of blocks of 40 lists to conditions was counterbalanced so that each list was tested equally often in each condition.

The design of Experiment 3B was a factorial combination of two repeated measures factors, orthographic similarity (similar and different) and the relation between C2 and the probe word (related and unrelated). Each subject was presented with 20 lists from each of the four conditions. The assignment of blocks of 20 lists to conditions was counterbalanced so that each list was tested equally often in each condition.

Procedure. The equipment was the same as that used in Experiment 2 with the exception that a Gerbrands voice-operated relay was interfaced with the computer for the purpose of recording response latency. For both Experiments 3A and 3B, the four practice lists were presented first, followed by the 80 critical lists presented in random order. Each list was preceded by a 495-ms premask of eight ampersands, and each item was presented for 120 ms.

In Experiment 3A, the subject's task was to report the last item presented in the word list. In this experiment, a series of eight question marks was presented immediately after the last word in the list (C2), and remained on the screen until the subject made a verbal response. The experimenter recorded the word the subject reported. Once the response was recorded, the experimenter pressed a key, and a row of three asterisks was presented. This indicated to subjects that the trial had ended and that they could press any button to begin the next trial.

In Experiment 3B, the procedure was identical to that used in Experiment 3A with the exception that the last word in the list (C2) was followed by an uppercase display of the probe word. This word remained on the screen until the subject responded. The subject's task was to say the first word that came to mind upon seeing the probe word. The voice key was triggered by the subject's vocal response and served as the basis for measuring response latency. The experimenter pressed a key to record whether the subject responded with the C2 item that was related to the probe word. Note that for lists in the unrelated condition, the C2 item related to the probe word was not in the list, but had been replaced by an unrelated C2.

Results and Discussion

The proportion of correct reports of C2 and of intrusions of C1 and the intervening word for Experiment 3A are shown in Table 3. As expected, the proportion of correct reports was significantly higher in the different condition than in the similar condition, $F(1, 7) = 9.41$, $MSE = 0.008$. These results indicate that the orthographic similarity effect is obtained even when lists are of uniform length. Moreover, intrusions of C1 and the intervening word were more common in the similar than in the different condition. The high rate of C1 intrusions in the similar condition supports our claim that recovery of information about the relative positions of words within a list (a prerequisite to reporting the final word) depends on a reconstructive process in which confusion among orthographically similar items can arise, just as in the case of full report.

Table 3
Mean Proportion of Correctly Reported C2 Items and Intrusion Errors in Experiment 3A

	C2		C2–1		C1	
Condition	M	SE	M	SE	M	SE
C1, C2 Similar	.64	.06	.22	.05	.10	.02
C1, C2 Different	.78	.06	.16	.04	.02	.01

Note. C2–1 is the item intervening between C2 and C1.

In addition, the observed effect of orthographic similarity when only C2 was to be reported rules out a simple reporting or output bias account of the effect, whereby subjects are biased against reporting any item that is orthographically similar to another reported item.

The effect of orthographic similarity seen in the final word report task is consistent with that reported by Kanwisher and Potter (1990, Experiment 6), but contrasts with Kanwisher's (1987) finding that when the final word of a list is a repetition of an earlier word, its report is enhanced. In the Kanwisher and Potter and the Kanwisher experiments, the duration of the final word was reduced relative to the rest of the items in the list. We suggest that such a change in duration can enhance the distinctiveness of the encoding of a repeated item, allowing it to escape the interference effect and perhaps even benefit from its earlier presentation (see Chun, 1997, Experiment 3, for a similar outcome).

It appears that subjects in Experiment 3A were not able to ignore the elements of an RSVP list that occurred prior to the final item, and were prone to a large interference effect when C1 and C2 were similar. Although this outcome is not surprising given that the entire list was presented in just 600 ms, it is important with respect to the interpretation of Experiment 3B. In particular, if subjects show evidence in Experiment 3B of having processed C2, it is doubtful that they could have ignored other items in the list.

In Experiment 3B, subjects were allowed to respond to the probe word with any word they chose, so any response was accepted as valid when measuring response latency. The latency data were summarized by computing the median latency in each condition for each subject. The means of these medians are shown in Table 4. An ANOVA with orthographic similarity and probe relatedness was computed for the latency data. This analysis revealed that only the effect of probe relatedness was significant, with shorter latencies in the related condition than in the unrelated condition (1259 vs. 1394), $F(1, 23) = 18.02$, $MSE = 24{,}488$. No other effect approached significance, Fs < 1.

The latency advantage for the related condition indicates that subjects attended to the word lists and that C2 was encoded as a word because it increased the availability of plausible responses to the subsequent, associatively related probe word. It is important that

Table 4
Mean Response Latency (in Milliseconds) and Mean Proportion of C2 Responses in Experiment 3B

	Latency				C2 responses			
	Probe related to C2		Probe unrelated to C2		Probe related to C2		Probe unrelated to C2	
Condition	M	SE	M	SE	M	SE	M	SE
C1, C2 Similar	1253	47	1405	61	.38	.04	.12	.02
C1, C2 Different	1264	56	1383	54	.38	.04	.11	.02

Note. When the probe was unrelated to the C2 actually presented in the list, C2 responses refer to the critical C2 that was related to the probe, even though that item was not presented in the list.

the relatedness effect did not interact with orthographic similarity between C1 and C2—if anything, the relatedness effect was slightly greater when C1 and C2 were similar. The lack of an interaction means that the influence of C2 on response latency to the probe was independent of the relation between C1 and C2. Thus, orthographic similarity did not appear to hamper the encoding of C2, at least not in any way that could be measured by the influence of C2 on response latency to the probe.

The mean proportions of trials on which subjects responded with the C2 word that was related to the probe are shown in Table 4. In the unrelated-probe condition, the C2 of interest was the one related to the probe, even though it had not been presented in the list. Report of C2 in this condition served as a baseline against which the effect of presenting a related C2 in the list could be assessed. An ANOVA with orthographic similarity and probe relatedness as repeated measures factors found that the proportion of C2 report was significantly greater in the related-probe condition than in the unrelated condition (.38 vs. .12), $F(1, 23) = 88.05$, $MSE = 0.020$. The main effect of orthographic similarity and the interaction were not significant, Fs < 1. Report of C2 was far more likely when it had been presented to subjects as an element in the list, yet the lack of an interaction means that availability of C2 was unaffected by its relation to C1. The C2 report findings are especially compelling because they are based on producing C2 as a response, just as the tasks in our earlier experiments assessed responding with C2.

The comparison between the C2 report results of Experiments 3A and 3B is especially informative. In Experiment 3A, report of C2 was substantially reduced by the presence of an orthographically related C1 word. In Experiment 3B, we sought to remove any inducement to engage in reconstructive processes by removing all requirements to report on list elements. We expected that information about list items would still be recruited by presentation of an associatively related probe word and would influence selection of a response to that probe (e.g., Whittlesea & Jacoby, 1990), but that this would occur without engaging reconstructive processes that potentially lead to interference from an orthographically similar C1. Our efforts appear to have been successful in that (1) when the C2 item that was related to the probe was actually in the list, the likelihood of reporting that item more than tripled relative to when it was not in the list, and (2) this enhancement effect was unabated by the presence of a similar C1 in the list.

The crucial results of Experiment 3B were the lack of an interaction between orthographic similarity and probe relatedness on the latency and the C2 report measures. Because these were null effects, we conducted power analyses to determine whether the experiment had adequate power. Estimates of power were computed only for the related-probe condition because no effect of orthographic similarity was found, nor would be expected, in the unrelated-probe condition. For our estimate of effect size, we used the effect of orthographic similarity obtained in Experiment 3A, which was $d = .8$ standard deviations.

In the latency data, an orthographic similarity effect of $d = .8$ could not have been obtained—if response latency in the similar-related condition were increased by that amount, it would exceed the latency found on unrelated trials. The largest possible effect of orthographic similarity would be one in which latency in the similar-related condition turned out to be equal to latency in the similar-unrelated condition. That effect size is $d = .57$. The power to detect an effect of that size, .98, represents an upper bound on the power estimate. The power to detect an effect of half that size was .56. For the C2 response data, an effect of $d = .8$ could have been obtained because the relatedness effect on that measure was more than a full standard deviation. The power to detect an orthographic similarity effect of $d = .8$ on the C2 report data was estimated to be .98. In general, then, Experiment 3B had adequate power, at least in the C2 response data, to detect an effect of orthographic similarity if one had been present.

Taken together, the latency and C2 report results provide strong evidence for the conclusion that C2 was encoded equally well in the similar and different orthographic conditions. There is good evidence that the lack of influence of C1 was not a consequence of subjects ignoring the RSVP list. If they had ignored the list, there would have been no effect of presenting C2 on either latency or C2 report. Further, it is not plausible that subjects were capable of selectively processing the last item of each list while ignoring earlier items. The results of Experiment 3A clearly demonstrated that when subjects were required to report only the last item of the list, they were unable to escape the influence of the earlier C1 item. We conclude that it was the removal of the requirement to report directly on list contents that freed subjects from the effects of list reconstruction. It is these effects, we contend, that are responsible for producing the orthographic similarity effect so clearly seen in Experiment 3A, yet absent in Experiment 3B.

Experiment 4

A possible concern with respect to Experiment 3 is that we provided only indirect evidence that subjects encoded C1 when their task was to respond to a cue word that followed the list containing C1. That evidence came from the finding that a different group of subjects who were required to report the final word of each list (i.e., C2) were less able to do so when C2 was orthographically related to C1. In Experiment 4, we sought to obtain more substantial evidence that C1 is encoded but does not influence free association responses to a post-list cue chosen to elicit report of C2. To do so, we tested a single group of subjects using a mixture of trials like those used in Experiments 3A and 3B. That is, each trial began with an RSVP word list ending with C2. On some trials, subjects were cued at the end of the list to report the last word, and on other trials the list was followed by a probe word and subjects responded with the first word that came to mind. By randomly mixing these two types of trials and presenting the reporting cue after the list had been presented, we ensured that encoding of C1 would be the same for both reporting tasks. We expected that, as in Experiment 3, a strong effect of orthographic similarity on the report of C2 would be found in the final-word report task, but that no such effect would emerge in the free-association task.

Method

Subjects. Twenty-four subjects were drawn from the same pool as in Experiment 2. Their ages ranged from 18 to 20 years, with a median age of 18 years. All were native speakers of English and none had taken part in the earlier experiments.

Materials and design. The materials were the same as those used in Experiment 3B and consisted of four blocks of word lists, with four versions of each block corresponding to a factorial combination of orthographic similarity between C1 and C2 (similar vs. different) and relatedness of the cue word to C2 (related vs. unrelated). Each subject was presented with four blocks of 20 lists with one block in each version. Assignment of blocks of lists to subjects was counterbalanced so that each block was presented in each version equally often and each subject saw only one version of a particular list.

For each subject, half of the lists in each block were followed by a row of eight ampersands, which indicated the task was to report the final word in the list. The remaining lists were followed by the presentation of a probe word in uppercase letters, which indicated the task was to report the first word that came to mind in response to the probe. The lists within a block that were assigned to the final word report and free association trials were counterbalanced across subjects.

Procedure. The procedure was similar to that used in Experiment 3. The subject was told that there would be two kinds of trials involving RSVP word lists. If the list was followed by a row of ampersands, the task was to report the last word in the list; if it was followed by an uppercase word, the task was to say the first word that came to mind. Each subject was presented with the four practice lists (two final word report trials and two free association trials) followed by the presentation of the 80 critical lists in a random order.

For each trial, the word READY was presented followed by a premask, consisting of a row of ampersands, and the word list. The word list was presented one word at a time with each word in view for 120 ms. The last word in the list was followed either by a second row of ampersands, which indicated a final word report trial, or an uppercase word, which indicated a free association trial. The experimenter recorded the subject's response. No latency data were collected.

Results and Discussion

The mean proportion of final word report and free association trials on which C2 was reported is shown in Table 5. Intrusions of C1 and the word intervening between C1 and C2 in the final-word report task are also shown in Table 5. The final word report data replicate those found in Experiment 3A. First, there was a reliably greater probability of correctly reporting C2 when C1 was unrelated to it, $F(1, 23) = 33.46$, $MSE = .006$. Second, intrusions of C1 and the intervening word were more common in the related than in the unrelated condition. These results show that C1 was encoded by subjects and that its similarity to C2 interfered with the ability to report accurately C2 as the last item in the list.

In the free-association task, the probability of reporting the C2 item related to the probe was strongly affected by whether that item was in the list. An ANOVA with orthography (similar versus different) and probe relatedness (related versus unrelated) as repeated measures factors found only a significant effect of probe relatedness, $F(1, 23) = 39.23$, $MSE = .023$. Thus, responding with the C2 item related to the probe was reliably more likely to occur when that C2 item was in the list. The main effect of orthographic similarity of C1 and C2 and the interaction were not significant, *F*s < 1.6. A separate analysis of the report of C2 in the related condition showed that there was no reliable effect of orthographic similarity, $F < 1$. Based on the orthographic similarity effect size of $d = .71$ found in the final-word report task, the power to detect a

Table 5
Mean Proportion of Correctly Reported C2 Items and Intrusion Errors in Experiment 4

	Final-word report						Free association			
	C2		C2–1		C1		Probe related to C2		Probe unrelated to C2	
Condition	M	SE	M	SE	M	SE	M	SE	M	SE
C1, C2 Similar	.60	.04	.13	.02	.15	.02	.34	.04	.12	.02
C1, C2 Different	.73	.04	.10	.02	.02	.01	.35	.04	.18	.02

Note. In the final word report task, C2–1 is the item intervening between C2 and C1; C2–1 and C1 are intrusion errors in this task. In the free-association task, when the probe was unrelated to the C2 actually presented in the list, C2 responses refer to the critical C2 that was related to the probe, even though that item was not presented in the list.

similarity effect of that magnitude in the free-association task was estimated to be .95.

The results of Experiment 4 are consistent with the those of Experiment 3 and support the proposal that subjects encoded C2 equally well regardless of its orthographic similarity to C1. When reconstructive processes were invoked in a task involving the report of C2 (final word report), however, the similarity between C1 and C2 led to interference in reporting C2. When reconstructive processes were suspended by using the free-association task, the effect of orthographic similarity on report of C2 disappeared.

Experiment 5

In Experiment 5, we reintroduced procedures that were intended to induce subjects to engage in reconstruction of list items. At the same time, however, we provided a retrieval cue that was intended to influence reconstructive processes in a manner that would favor, rather than interfere with, the report of C2. If we succeeded in doing that, we could expect that the orthographic similarity effect could be reduced or eliminated. As usual, C2 was orthographically similar to C1 on half of the trials and orthographically different from C1 on the other half. We selected C1-C2 pairs so that the C2 member of the pair was of lower word frequency than the C1 member. This frequency differential was expected to create a strong orthographic similarity effect on the report of C2 in the full-report task (Bavelier et al., 1994).

Crossed with the orthographic similarity factor, on half of the trials, subjects were required to report the entire list. On the remaining trials, a cue word was presented after the list and subjects were required to report a word from the list that was semantically related to the cue. These two types of test were randomly interleaved so that subjects did not know, while encoding the list, which type of test they would face. In consequence, they could not perform different kinds of encoding to prepare selectively for the two tests.

On trials when a cue word was presented after the list, the cue was semantically related to C2 on half of the trials, and to no word in the list on the other half of the trials. The latter (unrelated) case was included to assess the guessing rate induced by the cue. The former case (related) was the critical one. On those trials, we expected that the cue would lead subjects to emphasize the report of C2, thereby reconstructing the list in a different manner than when reporting the entire list. The question was what influence, if any, this cue-induced change in reconstruction would have on the orthographic similarity effect. If the encoding of C2 following an orthographically similar C1 was disadvantaged in some way, we should find that cuing the report of C2 has no influence on the orthographic similarity effect—in either the cued or full-report condition, the encoding of C2 would be compromised by following an orthographically similar C1 in the list. In contrast, if the encoding of C2 were not disadvantaged by the experience of a similar C1, then changing the way in which subjects reconstruct the list by cuing the report of C2 should enable subjects to report C2 as often when it was similar to C1 as when it was not.

In summary, if the orthographic similarity effect results from poor encoding of C2 when it is orthographically similar to C1, we should observe an equally strong effect of that manipulation in both full- and cued-report tests. In contrast, if the orthographic similarity effect arises from list reconstruction processes, we should find a similarity effect in full report but a much smaller effect, or no effect at all, in cued report.

Method

Subjects. Twenty-four undergraduate students were drawn from the same pool used in Experiment 2. Their ages ranged from 18 to 21 years, with a median age of 18 years. All subjects were native speakers of English and none had taken part in the earlier experiments.

Materials and design. A set of 120 orthographically similar word pairs was created such that the first member of each pair (C1) was of higher frequency (e.g., *went west*). The median frequency for C1 words was 143 and the median frequency for C2 words was 14 (Kucera & Francis, 1967). These words were either four

Table 6
Mean Proportion of Critical Items Reported in Experiment 5

	Full report				Cued report of C2					
	C1		C2		Cue related to C2		Cue unrelated to C2		Corrected	
Condition	M	SE	M	SE	M	SE	M	SE	M	SE
C1, C2 Similar	.55	.03	.18	.02	.37	.03	.07	.01	.30	.03
C1, C2 Different	.42	.04	.32	.03	.36	.03	.02	.01	.34	.03

Note. In the cued-report task, when the cue was unrelated to the C2 actually presented in the list, C2 responses refer to the critical C2 that was related to the cue, even though that item was not presented in the list. Corrected report of C2 was computed by subtracting report in the unrelated-cue condition from report in the related-cue condition.

or five letters in length. Each pair was embedded in a word list that consisted of three additional words of the same length as the two critical words (e.g., *toad went cuff west trim*). The words next to the critical pair in the list shared no more than two letters in common with its critical neighbor. The C1 item occupied the second position in the list and the C2 item occupied the fourth position. In addition, a word judged to be related to the C2 item was selected for use as a cue word to be presented following the list (e.g., *EAST* was the cue for the pair *went west*). The 120 lists were divided into six blocks of 20 for counterbalancing purposes. A second version of each list was created by reassigning the C1 items to different lists, thereby creating an orthographically different C1-C2 pair within each list. This reassignment was done within blocks of 20 items.

Each subject was presented all six blocks of 20 lists, three in the orthographically similar version and three in the orthographically different version. Within these two sets of three blocks, one block was presented with no cue word and subjects were required to report the full list, one block was presented with the related cue word, and one block was presented with an unrelated cue word obtained by reassigning cue words to lists within the block of 20 lists so that the cue words were not related to any word their new list. Thus, the design of the experiment was a 2 X 3 factorial with orthographic similarity (similar and different) and report cue (related cue, unrelated cue, and no cue) as repeated measures factors. Assignment of the six blocks of lists to the six conditions of this design was counterbalanced across subjects so that each block was used equally often in each condition.

Four practice lists were also created. Two of these practice lists were followed by a cue word; for one of these lists, the cue was related to a word in the list and for the other the cue was not related to any words in the list. The remaining two practice lists were used for full report trials.

Procedure. Each subject was presented with 124 lists: four practice lists presented in random order, followed by 120 critical lists, also presented in random order. There were two kinds of trials: when the list was followed by a row of question marks the subject was to report as many words from the list as possible, when the list was followed by an uppercase word the subject was to report a word from the list that was related to the cue word. Subjects were told that the cue word may or may not be related to a word in the list and were given the option of saying "I don't know" if they did not recall seeing such a word.

Presentation of trials was very similar to that used in the earlier experiments. The word lists were preceded by a premask, consisting of a row of ampersands. Each word in the list was then presented one word at a time for 120 ms, followed by a postmask consisting of a row of ampersands. The postmask was then followed either by a row of question marks, which indicated a full report trial, or by an uppercase word, which indicated that subjects were to report a word from the list that was related to the uppercase word. The question marks and cue remained on the screen until the subject made a response which was then recorded by the experimenter.

Results and Discussion

The proportions of correctly reported C1 and C2 items in the full-report task and of C2 items reported in the cued-report task are shown in Table 6.[1] Separate ANOVAs were used to test the effect of orthographic similarity on report of C1 and report of C2. Report of C1 in the full-report task was significantly greater when C2 was orthographically similar to C1, $\underline{F}(1, 23) = 20.04$, $MSE = .010$. This enhancement of the report of

[1]For report of C2, we examined only the simple probability of reporting that item, rather than also assessing joint report of C1 and C2 and report of C2 conditionalized on report of C1. We restricted our analysis in this way because our objective in this experiment was to compare report of C2 across two different reporting tasks, one of which did not involve report of C1 and so did not permit application of these alternative measures. Analysis of C2 report was similarly restricted in Experiment 6 for the same reason.reconstruction account that evidence from C2 can be misappropriated in support of C1 report.

C1 is consistent with the comparable effect found in Experiment 1 and confirms the prediction from the

Report of C2 in the full-report task showed clear evidence of interference due to orthographic similarity, but no such effect was apparent in the cued-report task. To take into account the influence of guessing prompted by the recall cue in the cued-report task, we applied a correction to C2 report in that task. The correction consisted of subtracting for each subject the proportion of C2 items reported in the unrelated-cue condition from the proportion of C2 items reported in the related-cue condition. This correction is a particularly conservative one and results in smaller corrected scores than other procedures such as one that assumes independence between correct report of C2 and guessing. Given that guessing is higher in the orthographically similar condition, this correction works against the mitigation of an orthographic similarity effect.

An ANOVA comparing C2 report in the full- and cued-report tasks as a function of orthographic similarity, using the corrected scores for the cued-report task, revealed a significant effect of task, with higher probability of report in the cued task, $F(1, 23) = 9.93$, $MSE = .011$, a significant interference effect due to orthographic similarity, $F(1, 23) = 8.55$, $MSE = .020$, and an interaction between task and orthographic similarity, $F(1, 23) = 9.77$, $MSE = .007$. The interaction effect shows that the orthographic similarity effect was larger in the full-report task than in the cued-report task. In fact, that effect was significant only in the former case, $F(1, 23) = 24.45$, $MSE = .010$, and not in the latter ($F < 1$). The estimated power to detect a similarity effect in the cued-report task that was of the same magnitude as that found in the full-report task ($d = 1.14$) was .98.

The C2 report results show that we were successful in our attempt to the interference effect due to orthographic similarity by a cued-report task that targeted C2. By modifying the nature of the reconstructive process used in reporting list items, we were able to prevent the potential interfering effects of an orthographically similar C1 from influencing report of C2. Changes in reconstructive processes are strongly implicated in Experiment 5 because the two types of report task were randomly mixed during testing. Subjects did not know the reporting requirements on a given trial until the list had been completely presented. Therefore, all aspects of list encoding would have been identical in the two reporting tasks. Although the elimination of the interference effect in Experiment 5 is the same as the outcome obtained in Experiment 4, two different methods of eliminating interference were used in these two experiments. In Experiment 5, we modified the nature of reconstructive processes, whereas in Experiment 4, we prevented those processes from being invoked. Both methods led, as predicted, to the elimination of the orthographic similarity effect.

Experiment 6

In Experiment 6, we used a different method of altering reconstructive processes in a further demonstration of how such alterations can influence the orthographic similarity effect. Rather than cuing subjects with a single word related to C2 and comparing that to a full-report task, we presented a report cue consisting of a set of words at the end of every trial. This cue contained all the words in the just-presented list but with C1 and C2 or just C2 missing. The task was to report the missing item(s). With C1 and C2 both absent from the report cue, we expected to see an orthographic similarity effect like that observed in the full-report task of Experiments 1 and 5; in attempting to report both C1 and C2, it is likely that subjects would first reconstruct C1, possibly usurping evidence for C2 in the process when C1 and C2 are orthographically similar. This misattribution of evidence would lead to a lower probability of reporting C2 as well as increasing the probability of reporting C1.

When only C2 is absent from the report cue, however, the presence of an orthographically similar C1 as part of the cue should serve as a powerful attractor of evidence that would otherwise support recall of C2. As a result, the orthographic similarity effect should be increased in this condition, relative to the condition in which C1 is not present as part of the report cue but instead must be generated as part of the report.

Method

Subjects. Eighteen undergraduate students participated in this experiment and were recruited from the same pool as those in the earlier experiments. Their ages ranged from 18 to 23 years, with a median age of 19 years.

Materials and Design. Eighty word lists, five words in length, were used in this experiment. All of the words within a list were of the same length and were four to six letters in length. Each list contained a critical pair of orthographically similar words. The first critical item of the pair (C1) occupied the second position in the list, and the second member (C2) occupied the fourth position (e.g., *here neat grow nest full*). The C1 items ranged in frequency from 2 to 895 with a median of 85 and the C2 items ranged in frequency from 1 to 173 with a median of 14 (Kucera & Francis, 1967). As in Experiment 5, this frequency imbalance between C1 and C2 was intended to favor the occurrence of an orthographic similarity effect. Two report cues were created for each list in which all noncritical items from the list were included. For one cue, both C1 and C2 were missing and for the other cue only C2 was missing (e.g., C1 and C2 missing: *here ____ grow ____ full; C2 missing: here neat grow ____ full*).

A second version of each of the 80 word lists was created by reassigning C1 items among the lists to create an orthographically different C1-C2 pair in each list. This reassignment was done with blocks of 20 lists and with the constraint that both members of each resulting C1-C2 pair be of the same length. For these

Table 7
Mean Proportion of Critical Items Reported in Experiment 6

	C1 and C2 missing				C2 missing	
	C1		C2		C2	
Condition	M	SE	M	SE	M	SE
C1, C2 Similar	.69	.04	.13	.02	.05	.02
C1, C2 Different	.43	.05	.25	.05	.27	.04

Note. In the recall cue, both C1 and C2 were missing and were to be reported or only C2 was missing and to be reported.

orthographically different lists, the report cue with only C2 missing was altered to include the reassigned C1 item rather than the original C1 item.

Each subject was presented four blocks of 20 lists, with one block from each of four conditions defined by a factorial combination of orthographic similarity between C1 and C2 (similar or different) and report cue (both C1 and C2 missing or only C2 missing). The blocks of 20 lists assigned to each condition were counterbalanced across subjects so that each block of lists was presented equally often in each condition. Four practice lists were also created, two with cues with both the second and fourth words in the list missing and two with only the fourth word in the list missing.

Procedure. Each subject was presented with the four practice lists followed by the 80 critical word lists, which were presented in random order. Subjects were instructed to read each list carefully and to report the missing word or words as indicated by the cue that was presented at the end of each trial. On each trial, a premask consisting of a row of eight ampersands was displayed, followed by the presentation of the word list. Each word was displayed for 120 ms at the center of the computer monitor. The list was followed by a postmask, consisting of a row of eight ampersands for 120 ms, then the report cue appeared. The cue consisted of a row of words with either second and fourth or only the fourth word replaced by a set of underscore characters. The cue remained on the screen until the subject responded. The subject responded orally and responses were recorded by the experimenter.

Results and Discussion

The mean proportion of C1 and C2 items reported under the two different cuing conditions are shown in Table 7. When both C1 and C2 were missing from the cue, report of C1 was substantially more likely if it was orthographically similar to C2, $F(1, 15) = 63.00$, $MSE = .009$. As in Experiments 1 and 5, it appears that evidence provided by presentation of C2 was misattributed to C1, thereby enhancing the probability of its report.

Report of C2 was analyzed in an ANOVA with type of recall cue and orthographic similarity as factors. Although the main effect of recall cue was not significant, there was a reliable interference effect due to orthographic similarity, $F(1, 15) = 23.63$, $MSE = .019$. More important, the interaction between recall cue and orthographic similarity was significant, $F(1, 15) = 5.71$, $MSE = .007$, indicating that the effect of orthographic similarity on the report of C2 was increased by including C1 as part of the recall cue. As expected, when clear evidence was made available that an orthographically similar C1 had been presented, report of C2 suffered particularly strong interference. When subjects had to rely on reconstruction of C1, however, the interference effect was somewhat less. This susceptibility of the orthographic similarity effect to changes in the recall cue supports our proposal that the orthographic similarity effect arises primarily from operations that take place during the reconstruction of the list rather than during list presentation.

Experiment 7

In the final experiment, we attempted to obtain evidence regarding the reconstructive process that we propose forms the basis for the report of RSVP word lists. In particular, we examined the potential trade-off involved in reporting one or another member of an orthographically similar pair of words. We have proposed that for a subject to report both members of such a pair, it is necessary to avoid misattributing evidence arising from one word's presentation to the other member of the pair, as well as establishing contextual evidence for the occurrence of two separate but similar events. Our strategy in Experiment 7 was to demonstrate shifts in attributions of evidence between two orthographically similar candidates for list membership. We contend that these shifts are illustrative of the dynamics involved in the reconstructive processes used during report of list items.

We attempted to induce shifts in attributions about C1 and C2 by varying the evidence available to the subjects after the list. In Experiment 7A, we asked subjects to report the list of words; we then presented a single word as a recognition probe. On critical trials, the word that was presented as a recognition probe (C2) was similar to a word in the list (C1), but was not itself presented within the list (nor did the list contain any pair of orthographically similar words). On other (filler) trials, a pair of orthographically similar words actually was presented within the list. Therefore, subjects would sometimes have the experience of seeing two related words in the list, making it plausible on critical trials that the probed item had been in the list.

In Experiment 7A, our expectation for critical trials was that if subjects correctly reported C1 from the list, they would be less likely later to produce a false alarm to the C2 probe because they would have attributed the relevant evidence associated with C1 to that item in the act of reporting it. Having reported C1, there would be little if any evidence to support C2 as having appeared in the list. On the other hand, if C1 were not reported, evidence associated with its appearance on the list might still be available and unattributed. That evidence could be marshaled in support of accepting the orthographically similar C2 probe as having been in the list, thereby increasing the probability of a false alarm.

In fact, the opposite happened: Subjects were more likely to produce a false alarm to C2 if they had correctly reported C1 than if they had not. Trying to understand this outcome from a reconstructive standpoint, we reasoned that, at the time of making their full report of the list, subjects sometimes had some evidence about the occurrence of C1—enough to report its identity, but not enough to be completely sure that it was the correct word. On being shown C2 as a recognition probe, they concluded that they had made an error in reporting C1, and opted instead to report that they had seen C2. That is, the same evidence that had been used to recall C1 was now reinterpreted as evidence that C2 had occurred instead. When shown a clear and related alternative possibility, subjects mistakenly abandoned their correct report of C1 and shifted instead to C2. In contrast, on trials when subjects did not have enough evidence to report C1 in the full-report test, they also did not have evidence that could be reinterpreted to support a claim of recognizing C2.

If this account is correct, we should be able to reverse the data pattern by giving the subject confidence in their correct report of C1 prior to presenting the C2 probe. In Experiment 7B, we provided subjects truthful feedback about which items they reported actually were in the list before presenting the recognition probe. It was expected that this reinforcement of the report of C1, when it occurred, would make it particularly likely that subjects would avoid making false alarms to an orthographically similar C2 probe.

In Experiment 7C, no feedback was given prior to the recognition probe but after a response had been made to the probe, one of the items from the subject's full report of the list was re-presented to the subject. The subject was asked to rate his or her confidence that the selected item had actually appeared in the list. The selected item on critical trials was always C1, as long as it had been reported by the subject. We expected that if the subject had made a false alarm to C2 after having earlier reported C1, the subsequent rating of confidence in that report of C1 would likely be low—the subject might even recant the earlier claim that C1 had been presented.

Method

Subjects. Twelve different undergraduate students participated in each of the three versions of Experiment 7. These subjects were recruited from the same pool as those in the earlier experiments. Their ages ranged from 17 to 37 years, with a median age of 18 years. All subjects were native speakers of English.

Materials. A set of 100 word lists was constructed, with each list containing five words. All words within a list were of the same length, consisting of four or five letters. Half of the word lists were used for as filler trials and the remaining lists were used for critical trials. Each list used for the filler trials contained an orthographically similar pair of words. The first member of the pair was located in the second position of the list and the second member was located in the fourth position. For each of these lists, the word located in the third position in the list (i.e., between the two orthographically similar items) was selected for presentation as a probe word subsequent to presentation of the list (e.g., *hurry sheep frown sleep under*; probe: *frown*).

The set of critical word lists also involved an orthographically similar critical pair of words. For these lists, however, the first member of the pair (C1) was located in the second position of the list. The second member (C2) was not included in the list but instead was used as the probe word presented after the list (e.g., *tear wash blow pest fuel*; probe: *wish*). The critical C1 words ranged in frequency from 8 to 1,599 with a median of 210.5 and the critical C2 words ranged in frequency from 1 to 281 with a median of 12 (Kucera & Francis, 1967). Relatively high frequency words were used as C1 items to achieve a reasonably high probability of reporting those items during full report of the lists.

Four practice lists were created, none of which contained orthographically similar word pairs. The probe used for a practice list was the word in the third position in that list.

Procedure. Each subject was presented with the four practice items followed by the 50 filler and 50 critical word lists, presented in random order using the same procedure as in earlier experiments. The equal mixture of filler and critical trials meant that the recognition probe presented after each list was actually present in the list on half of the trials (i.e., on the 50 filler trials). For each trial, presentation of the word list began and ended with a row of ampersands that served a pre- and postmasks. Each item in the list was presented at the center of the computer monitor for 120 ms. The final row of ampersands was followed by a row of question marks that cued the subject to report the words that appeared in the list. After the subject completed an oral report, the probe word for that list was then presented on the monitor. In Experiment 7A, the subject's task was to decide whether the probe word appeared in the list. The experimenter recorded the subject's response.

In Experiment 7B, before presentation of the recognition probe word, subjects were given feedback on their report of the list words, indicating which items they had correctly reported. This feedback was presented orally by the experimenter who read out each of the words reported by the subject that had actually been in the list. The probe word was then presented and the subject made a recognition decision as in Experiment 7A.

In Experiment 7C, subjects reported list items then responded to the recognition probe, as in Experiment 7A. Next, one of the words included in the subject's report of the list was selected by the experimenter. On

Table 8
Mean Proportion of C1 Items Reported and False Alarms to C2 Items in Experiment 7

			False alarms to C2			
	C1		C1 reported		C1 not rep.	
Exp. version	M	SE	M	SE	M	SE
7A	.43	.07	.40	.08	.21	.05
7B	.63	.06	.16	.06	.37	.09
7C	.54	.07	.17	.05	.15	.03

Note. Proportions of false alarms to C2 are shown separately for cases in which C1 was reported and not reported.

critical trials the selected word was always C1, as long as it had been reported. On critical trials on which C1 was not reported, and on all filler trials, the experimenter arbitrarily selected one of the reported words. The experimenter read the selected word aloud and asked the subject to rate their confidence in their report of that word. Confidence was classified as "sure," "uncertain," or "not present" (meaning the subject no longer thought it was in the list). The latter category constitutes a repudiation of the subject's earlier free report of the item.

Results and Discussion

The mean proportion of C1 items reported on critical trials and the mean proportion of false alarms to C2 for each version of Experiment 7 are shown in Table 8. The data of primary interest in Experiments 7A and 7B are the false alarm rates to C2 as a function of whether C1 was reported during full report of the word list. For Experiment 7A, contrary to our prediction, subjects were more likely to make a false alarm to C2 if the orthographically similar C1 had been reported, $F(1, 11) = 13.31$, $MSE = .017$. We had expected that a false alarm to C2 following report of C1 would be reduced because the orthographic evidence that might have supported the false alarm would already have been attributed to C1. We suspected, however, that this relation between report of C1 and false alarms to C2 was due to subjects' lack of confidence in their report of items included in full report. When an orthographically similar C2 was presented as a recognition probe, subjects tended to mistakenly abandon their belief in the presentation of C1 and shifted to C2 instead.

Experiment 7B represented a test of this suspicion in which we instilled in subjects confidence in their full reports by providing accurate feedback on the correctness of their responses. The false alarm rates shown in the second row of Table 8 indicate that this procedure was effective inasmuch as false alarms were now much more likely to occur when C1 was not reported, $F(1, 11) = 4.95$, $MSE = .053$. This outcome suggests that when C1 was not reported, evidence for its presentation was still available to subjects and served as the basis for elevated false alarms to C2. The contrasting effect of report of C1 on the false alarms made to C2 in Experiments 7A and 7B was supported by a significant interaction in an ANOVA that included Experiment (7A, 7B) and C1 report (reported, not reported) as factors, $F(1, 22) = 13.89$, $MSE = .035$.

In Experiment 7C, report of C1 did not have a reliable effect on false alarms to C2 ($F < 1$). The lack of an effect in this case might be due to a balance between trials in which subjects were unsure of their report of C1 and were therefore likely to accept C2 (as in Experiment 7A), and cases in which subjects were confident in their report of C1 and were unlikely to accept C2 (as in Experiment 7B). To assess this possibility we examined the probability of making a false alarm to C2, conditionalized on confidence in the report of C1. When subjects had low confidence in their report of C1 (i.e., ratings of "uncertain" or "not present"), the probability of making a false alarm to C2 was .34 ($SE = .10$), which was significantly larger than the false alarm rate of .04 ($SE = .01$) found when subjects rated their confidence in report of C1 as "sure," $F(1, 10) = 9.37$, $MSE = .055$ (one subject was excluded from this analysis because of missing data).

We were also interested in the retrospective rating of confidence in C1 reports as a function of whether a false alarm had been made to C2. The mean proportion of responses in each rating category and the mean overall rating (based on the assignment of 3 for a "sure" response, 2 for an "uncertain" response, and 1 for a "not present" response) are shown in Table 9. Data are shown separately for trials on which C1 was reported and served as the target for the rating and trials on which C1 was not reported so that an alternative word was the target of the rating. The pattern of ratings suggests that confidence in report of C1 was undermined when subjects made a false alarm to the orthographically similar C2 after having reported C1: Only in that case was there a substantial rate of retraction of the earlier C1 report (i.e., making a "not present" rating response). Although reported confidence was somewhat reduced by making a false alarm to C2 even when the rated item was a word other than C1, that effect was much smaller than when the rated word was C1 (and, therefore, orthographically similar to C2).

The pattern of confidence ratings is captured reasonably well in the overall rating shown in the last column of Table 9 (based on data from 9 subjects who contributed data to all four cells of the design). An ANOVA applied to the overall ratings with word reported (C1, alternative word) and C2 response (false alarm, rejection) as factors revealed a main effect of word reported, $F(1, 8) = 25.01$, $MSE = .071$, showing that confidence generally was lower when C1 was the target of the rating than when some other item was rated. This effect could be due to differences in memorability of C1 versus other noncritical words used in the list and is not of interest here. There also was a main effect of whether a false alarm was made to C2,

Table 9
Confidence in Report of C1 or Alternative Word Conditionalized on Response to C2 in Experiment 7C

Word reported and C2 response	Confidence category: Sure M	Sure SE	Uncertain M	Uncertain SE	Not present M	Not present SE	Overall rating M	Overall rating SE
C1 reported								
C2 false alarm	.26	.12	.54	.12	.20	.07	1.95	.13
C2 rejected	.72	.08	.27	.08	.00	.00	2.66	.09
Alternative word reported								
C2 false alarm	.67	.11	.33	.11	.01	.01	2.62	.11
C2 rejected	.91	.03	.08	.03	.01	.01	2.88	.04

Note. Overall rating was computed by assigning the values 3, 2, and 1 to the "Sure," "Uncertain," and "Not present" confidence categories, respectively. Overall rating data are based on results from nine subjects who contributed scores to all four cells of the design.

$F(1, 8) = 38.24$, $MSE = .056$, indicating that lower confidence coincided with making a false alarm. This relation could reflect a distinction between trials on which subjects were more versus less successful at encoding and remembering list items. Finally, and most important, these two factors interacted, $F(1, 8) = 10.81$, $MSE = .044$, showing that the impact of making a false alarm to C2 on rated confidence in the subject's report of the targeted item was significantly larger when that reported item was C1.

The results of Experiment 7 provide evidence for our proposal that the report of two orthographically similar items involves a form of competition for supporting evidence. Depending on the dynamics of the reporting procedure that is used, either C1 or C2 may fare better in that competition. In Experiment 7A, presenting C2 as a recognition probe appeared to combine with the evidence that subjects had just finished using to support the report of C1, thereby elevating false alarms to C2. But in Experiment 7B, when report of C1 was confirmed, that same evidence was firmly associated with C1 and false alarms to C2 were reduced. The implications of making a false alarm to C2 were clearly seen in Experiment 7C, where such a false alarm tended to undermine confidence in the earlier report of C1. We propose that this trade-off in the ascription of evidence to two orthographically similar items is inherent in the reconstructive processes responsible for the orthographic similarity effects seen in the experiments reported here.

In Experiments 7A and 7C, subjects abandoned a correct report of C1 on rather flimsy evidence. An analogy to the situation created in these two experiments might be a case in which someone witnesses a crime under low light conditions, and later describes the face of the suspect to a police artist. This description might include some definite features, such as a mustache. On the basis of that description, a photograph of a known criminal is shown to the witness. This photograph is similar in some respects to the description that the witness has produced, but is in fact a picture of a different person. Moreover, it is critically different in some details, such as not having a mustache. Offered the clear and definite image of the photograph, however, the witness recants their earlier report, and identifies the individual in the photograph with high confidence.

General Discussion

We have put forward an account of orthographic similarity effects in memory for rapidly presented word lists that is based on a reconstructive account of memory. On this account, the effects of orthographic similarity operate not at the time of list processing, but during reconstruction of the list from memory, after the list has been presented. Orthographic similarity between a pair of items is claimed to affect the reconstructive report of the list, with evidence associated with C2 potentially being misattributed to C1. This misattribution has the potential to enhance report of C1 and often reduces report of C2. Five primary results from the experiments reported here provide support for this reconstructive account. First, a retroactive influence of orthographic similarity was obtained, in which report of C1 was enhanced by the presence of a similar C2 in the list (Experiments 1, 5, and 6). Second, a nonword C1 can either interfere with or enhance the report of C2, depending on how accurately C1 is encoded (Experiment 2). Third, when reconstructive processes are unlikely to operate, report of C2 is unaffected by its similarity to C1 (Experiments 3 and 4). Fourth, the effect of orthographic similarity on report of C2 can be eliminated or enhanced, depending on the contextual support provided for retrieval of C1 or C2 (Experiments 5 and 6). Fifth, there is a trade-off in the ascription of evidence to members of an orthographically similar C1-C2 pair, whereby report of one member compromises report of the other and can even lead subjects to recant an earlier report of a member of the pair (Experiment 7).

Comparison with Other Retrieval Accounts

In their experiments on "repetition blindness," Armstrong and Mewhort (1995) and Fagot and Pashler (1995) applied a methodological strategy similar to the one we used here. By varying the manner in which subjects reported list items (e.g., reversing the order in which list items are to be reported or providing a contextual probe to specify report of C2), these two studies showed that the deficit in reporting a repeated item effect could be eliminated. This modulation of the reporting deficit was interpreted to mean that repetition within a list did not affect encoding of items, but instead induced a form of retrieval failure. This conclusion is compatible with the reconstructive account we have proposed here.

Our reconstructive account of orthographic similarity effects, however, is distinct in an important way from the retrieval failure accounts of repetition blindness proposed by Armstrong and Mewhort (1995) and by Fagot and Pashler (1995). In those accounts, failure to report the second member of a repeated pair of items from an RSVP list results either from a form of bias against reporting the same item twice (akin to the Ranschburg effect; Crowder & Melton, 1965) or from the possibility that the two repetitions end up in two different short-term memory buffers (e.g., articulatory store and visual store), causing confusion about whether a single item or two different presentations of the same item had occurred. These accounts, like those based on the idea of failed tokenization of a repeated item (e.g., Kanwisher, 1987), treat remembering simply as the retrieval of a stored representation of a past event. Thus, the retrieval problem is characterized as one of accessibility to an intact memory trace. In contrast to this approach, and following Whittlesea et al. (1995) and Whittlesea and Podrouzek (1995), our proposal is that the act of remembering, including recall of the contents of an RSVP list, is a reconstructive one that operates on retrieved fragments of experience and inferences about what plausibly might have occurred. Thus, misattribution of evidence from one item to another, as in the inflated report of C1 when orthographically related (or identical) to C2, is a natural consequence of reconstruction. The alternative retrieval accounts discussed by Armstrong and Mewhort and by Fagot and Pashler have no clear means of explaining this "retroactive" effect of repetition nor the effect of orthographic similarity.

Implications for Encoding Accounts

There are a number of different accounts of orthographic interference effects, some stemming from accounts of repetition blindness effects, that assume that when C2 is orthographically similar to C1 it often is not properly encoded. This reduced probability of encoding C2 is responsible for the interference effect. In one such account, Kanwisher and Potter (1990) proposed that words or letter clusters are represented as types in memory and that presentation in a list creates a tokenized representation that serves as the basis for reporting items from the list. Moreover, presentation of C2 can be misperceived as a repetition of C1 when the two are orthographically related, leading to a failure both to activate the type representation of C2 and to tokenize C2. In another account based on the formation of episodic tokens from activated type representations, Bavelier et al. (1994) suggested that there is a constraint on token formation for ordered letter clusters. In this account, once a letter cluster in C1 has been tokenized, the type for that cluster cannot produce another token if the cluster reappears soon after as part of orthographically related C2. Thus C2 fails to be tokenized and is unavailable for report. Morris and Harris (1999) also proposed an account of orthographic similarity effects based on letter clusters, in which the occurrence of a letter cluster in C1 causes that same cluster when repeated in an orthographically similar C2 to be suppressed. The suppressed letter cluster is then less available for report.

Bavelier and Jordan (1992) developed an activation-based account of repetition and similarity effects in which they proposed that when a word is detected, its internal representation reaches a relatively high level of activation. The level of activation of this word's orthographic neighbors will also be raised in proportion to their similarity to that word as a result of its detection. Any subsequent presentation of that word or one of its neighbors may be interpreted as noise if these items are still above their baseline level of activation, leading to a failure of encoding. Chialant and Caramazza (1997) also put forward an account based on orthographic neighborhoods of words. In their proposal, however, identification of C1 produces inhibition of all of its orthographic neighbors, thereby impairing the identification of an orthographically related C2.

Another account of encoding difficulty associated with C2 was proposed by Bavelier and Potter (1992) and Bavelier (1994) to explain interference effects between phonologically identical or similar pairs of items (e.g., *ate* and *height*; *great* and *freight*; a picture of the sun and the word *son*) appearing in RSVP. Interference in reporting the second member of such a pair arises because short-term memory processing is constrained such that two identical or similar codes (whether phonological or of some other form) cannot normally enter short-term memory in close temporal proximity.

The common link among each of these encoding accounts is that they propose that C2 either fails to be correctly identified (i.e., its type is not activated) or it fails to form an episodic token and hence it is not available for report. Recent evidence presented by Morris and Harris (1999) suggests that accounts based on failed encoding of the entire C2 word are incorrect. They showed that subjects tend to report illusory words derived from a nonword and a segment of C2 that does not overlap with an orthographically similar C1 (e.g., *rock shock ell* yields *shell*). This result suggests that encoding of only the letters of C2 that overlap with those of an orthographically similar C1 are affected. On our reconstructive account, however, the confabulations reported by subjects in the Morris and Harris study are a result of the fragmentary nature of information available from an RSVP list. Letters from C2 that are compatible with C1 ("ock" in the example) may be mistakenly ascribed to C1, leaving the remaining letters of C2 ("sh") free to combine with the nonword to form a plausible word.

Our experiments provide further evidence against the claim that the mechanism responsible for the orthographic interference effect involves suppression of the encoding of C2 as a word. But these results also challenge the claim that encoding of a cluster of letters in C2 is suppressed and the claim that although a type representation of C2 is activated, no episodic token is formed. In particular, we found in Experiments 3B and 4 that if subjects are asked to provide an associate to a probe word presented after a word list, they are equally likely to respond with C2 (when it is related to the probe) regardless of whether it is orthographically similar to C1. If encoding or type activation of any part or all of C2 had failed (e.g., because C2 was misinterpreted as C1 as in the Kanwisher & Potter, 1990, account) C2 responses should have been less likely when an orthographically similar C1 was involved.

Although one might wish to argue that the free-association task used in Experiments 3B and 4 reflects only the consequence of type activation, but not the effects of tokenization, Experiments 5 and 6 add two further challenges. In Experiment 5, subjects attempted to recall a targeted item from the word list prompted by a cue word related to C2. Again orthographic similarity between C1 and C2 had no influence on report of C2 when cued this way. This recall task could not plausibly have been carried out using only activated type representations, but would have required subjects to retrieve tokenized representations. As evidence for this claim, we observe that in the free-association task of Experiment 4 the rate at which C2 was given as a response when it was not actually in the list was, on average, three times the rate of C2 intrusions seen in the recall task used in Experiment 5 (.15 vs. .05). From the perspective of a type-token account, subjects in Experiment 5 clearly were interrogating episodic memory rather than responding on the basis of activated word types. Moreover, in Experiment 6, we found that the orthographic interference effect was strongly modulated by whether or not C1 appeared as part of the report cue presented after the list had been seen. This retrieval cue effect is incompatible with accounts that propose that the activation or tokenization of C2 was suppressed. Had such suppression occurred and if that suppression were the only basis for the orthographic similarity effect, no such modulation should have been observed.

A fundamental implication of our results, particularly the evidence that encoding of C2 is unaffected by its similarity to C1, is that the orthographic similarity effects observed in RSVP experiments may have little to do with the word identification processes that operate during list presentation. Rather, these effects arise from the reconstructive processes invoked at the time list items are recalled.

Reconstructive Memory

The reconstructive account of remembering RSVP lists that we have developed parallels the constructivist account of depth perception put forward by Helmholtz (1866/1962). In the Helmholtz theory, the perception of depth is computed from a variety of cues in the environment, including relative size, occlusion, height in the field of view, and so on, but depth is experienced directly, rather than as a decision or computation. The observer does not become aware of the separate elements on which the perception is based, and the perception is not isomorphic with any of the elements. Similarly, mental contents produced by memory are constructed out of the contents of memory traces, and are then experienced as remembrances. This experience can go forward without awareness of the separate memory traces that support the current mental event and without awareness of the inference that directly leads to the experience of the event as one of remembering a prior episode.

In this reconstructive account, we do not need to assume a special inhibitory mechanism to explain effects of repetition or orthographic similarity in RSVP lists. Instead, it is assumed that during presentation, each occurrence of a repeated word, or each occurrence of orthographically similar words, is as likely to be encoded as each occurrence of a pair of unrelated words. Under RSVP, however, neither the items themselves nor their association with particular contexts (adjacent words) is processed extensively. After seeing the list, the observer has the problem of constructing a report, based on the evidence of what words come to mind. Memory constantly places mental events such as these into consciousness under the control of internal and external cues, without causing the function of remembering. The act of remembering itself consists of performing an unconscious decision about the source of current mental contents (Jacoby et al., 1989; Whittlesea, 1993).

Determining the source of a mental event depends on the availability of clues that implicate relevant sources (Johnson, Hashtroudi, & Lindsay, 1993; Whittlesea, Jacoby, & Girard, 1990). In the case of reporting words from a list, evidence that implicates the list as a potential source would include the fluency with which an item came to mind during the attempt to recall the list and the item's associated contextual information (e.g., adjacent words in the list also come to mind). Reporting a word as having occurred twice, or reporting two orthographically similar words, depends crucially on contextual information. It is the difference in contextual information associated with separate occurrences of a repeated word or with the occurrences of orthographically similar words that authenticates the two mental events as arising from separate events in the environment. Simply having a repeated word come to mind is not sufficient to claim it was presented twice, and having two similar words come to mind raises the possibility that one of the words is an impostor, mistakenly called forward by the earlier presentation or the coming to mind of its similar partner. This ambiguity is compounded by the possibility that mental events do not consist of intact representations of words, but instead may often be comprised of partial information. In the absence of supporting contextual information, an incomplete representation may readily be ascribed not to its true source, but to a related mental event and its corresponding source.

The problem introduced by rapid presentation of a series of items is that target words are unlikely to be integrated with their contexts. Under some conditions, alternative methods of establishing the source of mental events might serve in place of contextual information. First, repetition or presentation of two similar items in a list might lead to somewhat greater fluency when those items later come to mind, relative to other list items (Whittlesea & Podrouzek, 1995). It is probable, however, that the increment generated by a second presentation or by a similar item is too small to be a reliable basis for deciding that these mental events originated in separate stimulus presentations (Whittlesea et al., 1990). Second, observers may notice during list presentation that the second occurrence of an item is a repetition or that the second member of an orthographically similar pair is related to an earlier word. We doubt that the rapid presentation of list items would often admit this possibility. These constraints conspire to make it difficult to establish that two identical or highly similar mental events were occasioned by separate environmental events.

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