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Repetition Blindness Occurs in Nonwords

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Theorists have predicted that repetition blindness (RB) should be absent for nonwords because they do not activate preexisting mental types. The authors hypothesized that RB would be observed for nonwords because RB can occur at a sublexical level. Four experiments showed that RB is observed for word–nonword pairs (*noon noof*), orthographically similar nonwords (*glome glame*), and identical repetitions (*plass plass*). More RB was found for words than for nonwords. Prior researchers may have failed to find RB for nonwords because display conditions that allow 2 words to be reliably encoded are insufficient for nonwords, or because observers coped with low ability to encode nonwords by using guessing strategies that do not require creating a mental type or tokenizing it.

Many reading researchers have noted that words have a type of unitization that is not possessed by nonwords (M. Coltheart, Curtis, Atkins, & Haller, 1993; M. Coltheart, Rastle, Perry, Langdon, & Ziegler, 2001; Rastle & Coltheart, 1998, 2000). If nonwords do not activate mental types, then nonwords should not demonstrate the phenomenon known as *repetition blindness* (RB; Kanwisher, 1987). Several authors have recently concluded that RB does not occur for nonwords and have linked this to arguments that nonwords do not activate mental types (Campbell, Fugelsang, & Hernberg, 2002; V. Coltheart & Langdon, 2003). We demonstrate that robust RB occurs for nonwords, although RB is usually stronger for words than for nonwords.

Repetition Blindness

RB is a deficit in reporting the second occurrence of a repeated item, compared with reporting dissimilar items, when the two items are rapidly and sequentially displayed at stimulus-onset asynchronies of approximately 80 to 300 ms (Bavelier, Prasada, & Segui, 1994; Hochhaus & Johnston, 1996; Kanwisher, 1987; Kanwisher & Potter, 1990; Mozer, 1989; Park & Kanwisher, 1994). For pictures, RB occurs for different views of the same object (e.g., front/side of an iron; Kanwisher, Yin, & Wojciulik, 1997). In the realm of words, RB is not attenuated by changes in case (Bavelier & Potter, 1992) and occurs for words that share as few as two adjacent letters (as in *felt bolt*[1]) or three nonadjacent letters (*medical seminar*; Harris, 2001). The strength of the RB effect depends on number of repeated letters, with strongest RB for identical words, less RB for orthographic neighbors (*tower lower*), and the least amount of RB for word pairs repeating only their initial letter (Harris & Morris, 2000).

The predominant explanation for RB, put forward by Kanwisher (1987; Kanwisher & Potter, 1990; Park & Kanwisher, 1994) and endorsed by other researchers (Bavelier, 1994, 1999; Chun, 1997; Hochhaus & Marohn, 1991), is a temporal limitation in distinguishing the separate occurrences of a twice-activated visual type. Kanwisher referred to this as type activation without token individuation. She linked the ideas of types and tokens to the object recognition literature, including the distinction between the dorsal visual stream, which identifies the spatial location and other episodic characteristics of an object, and the ventral visual stream, which identifies the object as a unique type stored in long-term memory (Kanwisher, 1991; Kanwisher, Driver, & Machado, 1995).

It would be convenient for experimenters if token individuation theory made clear predictions about repetition of identical nonwords (such as *carn carn*) and nonidentical nonwords (*tarn carn*). Instead, predictions depend on assumptions about whether the locus of RB effects is lexical or sublexical and whether nonwords activate preexisting types. For example, if RB occurs at the level of lexical type nodes, and nonwords do not activate preexisting lexical type nodes, then there should be no RB for nonwords. This follows from basic postulates of token individuation theory: If there is no type activation, there can be no difficulty in binding a twice-activated type to two separate tokens, and thus no disadvantage in reporting two repeated words relative to two dissimilar words. A finding of no RB for nonwords would be helpful because it could be used as support not only for token individuation theory but also for the assumption that RB occurs at the lexical level and not the sublexical level, and for the assumption that nonwords do not activate preexisting lexical type nodes. Indeed, this was the conclusion recently advocated by V. Coltheart and Langdon

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[1] Researchers generally change the case between the two critical items to rule out low-level visual superposition as an explanation of failure to detect a second occurrence of a word. For ease of presentation, we present words in lowercase when listing examples of stimuli.

(2003), who found a repetition advantage for identical nonwords even though words, in the same experiment, showed RB.

Orthographic Repetition Blindness

RB occurs for words that are merely orthographically similar; this is typically referred to as *orthographic RB.* RB for *chance hand* occurs because of the *han* string within these words (Harris & Morris, 2000). Can a letter string within a word fail to be tokenized, or is tokenization strictly a lexical-level event? The existence of orthographic RB is problematic for Kanwisher's token individuation theory (Harris & Morris, 2000, 2001a, 2001b; Morris & Harris, 1999). Recognizing this, Kanwisher and Potter (1990) speculated that orthographic RB for words like *cape cap* may arise if observers misread one of the words as the other, which then allows operation of the mechanism underlying identity RB. Bavelier and Jordan (1992) have also explained orthographic RB as a weaker form of identity RB, with amount of RB being proportional to the similarity between the two critical words. Chialant and Caramazza (1997) suggested that orthographic RB and identity RB arise from different mechanisms. Orthographic RB may not be "true RB" but a result of word-to-word inhibition resulting when similar words compete for selection during lexical access. They claimed that it would be implausible for token individuation to operate at a sublexical level: "RB is an effect that occurs at the level of representation where a stimulus is encoded for recognition and recall; earlier levels of representation do not contribute to the RB effect" (p. 100). However, Chialant and Caramazza's experiments in support of a different mechanism for identity and orthographic RB did not replicate, and thus their proposal lacks empirical support (Harris & Morris, 2001a).

Considerable evidence exists that orthographic RB has a sublexical locus (Harris & Morris, 2000, 2001a, 2001b; Morris & Harris, 1999, in press). Participants frequently misreport orthographically similar words in a manner that preserves the unique letters while omitting the repeated letters. For example, participants may read *pain grain* as *pain grass.* To confirm statistics garnered from misreports, we developed the illusory words paradigm. Words and word fragments were displayed by means of rapid serial visual presentation (RSVP; Forster, 1970; Potter, 1984), as in the sequence *copy french kitty fresh bru.* If only the repeated letters are affected by RB, with the nonrepeated letters detected and capable of feeding activation to words consistent with them, then the *sh* at the end of *fresh* should activate words of similar length ending in *sh.* In combination with *bru,* this should activate (with some probability) the word *brush.* Frequency of report of the illusory word *brush* was two to three times higher in this trial compared with nonrepeated control trials (*copy window kitty fresh bru*). We concluded that repetition of the repeated *fre* string did not suppress the entire word *fresh* (in the example listed above), and thus the locus of orthographic RB effects is at the level of the repeated letters.

The sublexical view of orthographic RB makes clear predictions about RB and orthographically similar nonwords: If RB occurs at the sublexical level for words, then it should also apply at this level for nonwords. That is, for stimuli such as *meap teap,* the repetition of *eap* across the two stimuli should suppress recognition of *eap* in the second item, leading to a report deficit for this pair compared with a nonrepeated control such as *noof teap.*

We present four experiments concerning RB in nonwords. The goal of the first two experiments was to establish that nonwords are susceptible to RB and that this effect occurs at the level of letters (or letter clusters) within nonwords. Pilot experiments revealed low report of nonwords even when nonwords shared no repeated letters. It has long been known that nonwords are difficult to name out loud under conditions of brief display and masking by pattern masks or subsequent stimuli (Carr & Pollatsek, 1985). RB appears to depend on good recognition of the first of the two similar words (Luo & Caramazza, 1995, 1996). If the nonrepeated condition has poor report of both critical words, then it will be difficult to find RB, due to floor effects and to the possibility that poor recognition of the first item means that masked priming is occurring (Bavelier & Jordan, 1992). It was difficult to obtain high (at least 70%) report of both items in the nonrepeated condition in our pilot experiments. (Indeed, in V. Coltheart and Langdon's, 2003, studies, correct report of nonwords was in the 10%–20% range.) Because of readers' difficulty in reading nonwords under RSVP conditions, in the first two experiments we used word–nonword pairs, such as *noon noof.* Prior researchers found that novel objects are hard to identify when embedded in lists (Arnell & Jolicoeur, 1997). We thus displayed only two items, embedded in symbol strings, as suggested by Bavelier et al. (1994). Prior work has shown that RB for just two displayed words resembles RB obtained when words are embedded in a longer sequence (Harris, 2001; Harris & Morris, 2000). With positive findings in this paradigm, a pool of easy-to-pronounce nonwords, and an increased understanding of practice procedures to train participants in reading nonwords, in Experiment 3 we turned to orthographically similar nonword pairs such as *treal treap.*

It is well known that repetition of identical words results in an additional deficit beyond what is obtained for words that repeat all their letters but one (Ayçiçegi & Harris, 2002; Harris & Morris, 2000; Morris & Harris, 2002). If nonwords do not activate types at the level of the whole string, would nonwords show the additional RB deficit observed for words? In Experiment 4 we included word–word trials and nonword–nonword trials, and compared both identity and orthographic RB.

Experiment 1

Investigating the susceptibility of nonwords to RB is relevant to Chialant and Caramazza's (1997) claim that inhibition between words during lexical competition is the source of orthographic RB. These authors would agree with Colombo (1986) that nonwords are not listed in the lexicon and thus do not receive inhibition during lexical access. On this view, there should be no RB between a word and a subsequent nonword, or at most only weak RB. However, if Chialant and Caramazza are incorrect, and orthographic RB occurs at a sublexical level, then strong RB should be found for word–nonword pairs containing repeated letters such as *noon noof.*

Method

Design and materials. Twenty-one easily pronounceable three- or four-letter nonwords were created, and each was paired with three different words to create three versions of each stimulus item (see Appendix A). In the *neighbor* condition, the word and nonword shared all but their final letters (*tail tain*). In the *single-letter* condition, only the initial letters were shared (*tube tain*). In the *nonrepeated* condition, no letters were shared.

The words in each stimulus item were matched for frequency across conditions. The three versions of each stimulus item were counterbalanced across participants, such that each participant viewed seven stimulus items in each condition, for a total of 21 experimental trials. The word (Item 1) always appeared in lowercase letters, with the nonword (Item 2) in uppercase. The font was 36 pt. Courier. In addition to 21 experimental trials, participants viewed 10 filler trials containing no repeated letters to reduce saliency of orthographically similar items.

Procedure. Participants were instructed to report both the word and nonword on each trial. Pilot studies indicated that participants varied considerably in ability to perceive and report briefly displayed nonwords; participants were therefore instructed to report whatever letters they did perceive in the event that they were unable to report an entire nonword. A lengthy practice procedure was instituted to improve nonword report. Participants viewed 30 practice trials consisting of word–nonword pairs (with no repeated letters) prior to viewing the experimental trials. Participants pressed the space bar to initiate each trial, which consisted of sequences such as *noon ##### NOOF &&&&&.* The word was displayed for 90 ms, the masking stimulus ##### displayed for 60 ms, and the final masking string *&&&&&* was displayed for 250 ms. (Because of a 15-ms interstimulus interval, the interstimulus interval between word and nonword was 90 ms.) Exposure duration for the nonword was set at 150 ms at the beginning of the practice trials, and was subsequently decreased by 15 ms every 5 trials if the participant's accuracy in reporting both the word and nonword was greater than 60% over the preceding block of 5 trials. Exposure duration was increased if participants' accuracy fell below 45%. At the close of the practice trials, an exposure was chosen and held constant for the experimental trials. The average exposure duration for the nonword for the experimental trials across the 30 participants was 97 ms (range = 60–150 ms). The experimenter recorded participants' immediate report by pressing keys on the computer and by annotating a score sheet containing the correct sequence.

Apparatus. The stimuli for all experiments reported in this article were presented on a Macintosh IIci, with display of stimuli and collection of response times controlled by the PsyScope experimental control software (Cohen, MacWhinney, Flatt, & Provost, 1993). Participants sat approximately 45 cm from the screen.

Participants. Participants were 30 Boston University students who participated in exchange for course credit. All participants were native English speakers (8 participants acquired English simultaneously with another language).

Results and Discussion

Following Bavelier et al. (1994), we scored how often Item 2 was reported given correct report of Item 1 (percentage correct report of Item 1 averaged 98% or greater in all three stimulus conditions).[2] The percentages of correct reports of Item 2 along with omissions and misreports for each condition are shown in Table 1. (Misreports included reporting a word, another nonword, or only individual letters.) Analysis of percentage correct reports revealed a significant effect of condition, $F_1(2, 58) = 81.8$, $p < .001$; $F_2(2, 40) = 111.4$, $p < .001$.

Newman–Keuls tests showed that report of Item 2 was significantly lower for both the neighbor and single-letter conditions compared with the nonrepeated condition (both *p*s < .01). Because RB is indexed by the difference between repeated (neighbor or single-letter) and nonrepeated conditions, these results showed a substantial repetition deficit for both neighbor and single-letter conditions. In addition, report of Item 2 in the neighbor condition was lower than in the single-letter condition ($p < .01$), indicating greater RB in the neighbor condition (54%) compared with the single-letter condition (33%).

The finding of strong RB in nonwords falsifies the prediction that orthographic RB is only caused by lexical-level competition and the claim that sublexical effects, if they exist at all, will be weak. To substantiate the claim that the locus of these RB effects is sublexical, we analyzed participants' misreports. Morris and Harris (1999) argued that a hallmark of sublexical effects is that letters *unique* to Item 2 will be reported (either as part of another word or in isolation), whereas letters *shared* with Item 1 will be excluded. Explanations of RB based on inhibition at the level of the whole letter string make no predictions about what participants will report in place of Item 2. Participants' error reports are consistent with the proposal that only the repeated letters are suppressed due to the mechanism responsible for repetition deficits. The top half of Table 2 shows report of the first letter versus the remaining letters (as a percentage of all misreports) for the single-letter and nonrepeated conditions. As predicted by sublexical models, Item 2 misreports in the single-letter condition selectively omitted the letter shared with Item 1, as compared with the same letter in the nonrepeated condition, $t(19) = 4.2$, $p < .001$.

The bottom half of Table 2 shows a similar comparison involving the neighbor and nonrepeated conditions. In this case, the neighbor condition had only one unique letter, the last letter. All other letters were shared. Letters corresponding to the shared letters in the nonrepeated condition would be the first two letters in the case of three-letter nonwords, and first three letters in the case of four-letter nonwords. Again, the shared letters appeared less often in misreports in the neighbor condition when compared with the same letters in the nonrepeated condition, $t(19) = 4.6$, $p < .001$, whereas the unique letter was selectively preserved in the neighbor condition in comparison with the same letter in the nonrepeated condition, $t(19) = 3.8$, $p < .005$.[3] When individual letters were reported, these were more frequently the unique letters than the repeated letters. For example, for the stimulus *box BOT*, one misreport was "*box and something ending in T.*"

The results of Experiment 1 demonstrated that RB occurs for word–nonword pairs and is a strong effect (similar to the size of effects in reporting orthographically similar words, as in Bavelier

Table 1
Percentage of Correct Reports, Omissions, and Misreports of Item 2 Given Correct Reports of Item 1 in Experiment 1

Condition	Example: Item 1	Example: Item 2	% correct	% omission	% misreport
Neighbor	*tail*	*TAIN*	13	56	31
Single letter	*tube*	*TAIN*	34	27	39
Nonrepeated	*dull*	*TAIN*	67	8	25

Note. Standard errors of the mean ranged from 3% to 5%.

[2] Some theorists (e.g., Kanwisher, 1987; Luo & Caramazza, 1995) have proposed that the second critical item (or repeated letters within it) will not be subject to RB unless the first item is tokenized. For this reason, we needed to ensure that we were assessing performance on trials when the first item was recognized.

[3] Improved report of the unique letters in the repeated compared with the nonrepeated condition can be understood if one views RB as an attentional bias toward novel and away from repeated items, as proposed by Morris and Harris (2004).

Table 2
Percentage of Reports of Shared Versus Unique Letters in Item 2 in Experiment 1

	% report		Example	
Condition	First letter	Remaining letters	Stimulus	Misreport
Letter	33	77	*mad mup*	*mad fup*
Nonrepeated	77	48	*row mup*	*row mud*
	% report		Example	
	Shared letters	Last letter	Stimulus	Misreport
Neighbor	40	70	*noon noof*	*noon half*
Nonrepeated	67	34	*drug noof*	*drug no*

et al., 1994). Finding reliable RB when only a single letter was repeated across the two items falsifies Chialant and Caramazza's (1997) contention that any observed sublexical RB would be weak. Furthermore, the pattern of misreports points to a sublexical locus for RB.

The reader may note that the finding of more RB for the neighbor condition compared with the single-letter condition is compatible with a mechanism for RB that operates at the level of the whole letter string. That is, a mechanism sensitive to the overall similarity of two items would plausibly result in more RB for orthographic neighbors like *noon* and *noof* than for stimuli like *mad* and *mup.* Our next nonwords experiment considered an area in which the sublexical and whole-word/global similarity approaches (Bavelier & Jordan, 1992; Chialant & Caramazza, 1997) make different predictions: the effect of changing the proportion of shared letters.

Experiment 2

Experiment 2 again used word–nonword pairs to investigate whether the RB effect has a sublexical locus. In this experiment, we held the number of shared letters constant across items (in the repeated letters condition) but varied the proportion of shared letters by varying word length. This enabled us to manipulate overall similarity between Item 1 and Item 2 such that different predictions were generated for the sublexical account of orthographic RB (Harris & Morris, 2000; Morris & Harris, 1999) relative to theories that posit that amount of RB will reflect similarity at the level of the whole letter string (Bavelier & Jordan, 1992; Chialant & Caramazza, 1997).

Words and nonwords such as *jet* and *jeg* are more similar to each other than *plane* and *plosh,* although both share the same number of letters. An account of RB based on overall similarity at the whole-string level would therefore predict a greater magnitude of RB for *jet jeg* compared with *plane plosh,* whereas sublexical accounts predict approximately equal amounts of RB for these two types of stimuli. This is because the number of repeated letters in the Length 3 and Length 5 stimuli is the same. According to the sublexical account of RB, equivalent amounts of information would be missing in these two conditions. Therefore ability to report these two categories of nonwords will be equivalent.[4]

Method

Design and materials. Materials consisted of 20 word–nonword pairs, with two different lengths (three letters and five letters), each with a repeated and nonrepeated condition (see Appendix B). For the repeated condition, the word and nonword shared the first two letters; no letters were shared in the nonrepeated condition. Thus, all repeated items shared the same number of letters between Item 1 and Item 2, but the proportion of letters shared was greater for the three-letter stimuli (67%) than for the five-letter stimuli (40%). Words were matched for frequency between conditions and stimulus types. As in Experiment 1, 10 filler trials were included to decrease the proportion of orthographically similar trials.

Procedure. The procedure was the same as in Experiment 1; participants again viewed 30 practice trials prior to the experimental trials. The average exposure duration for Item 2 for the experimental trials across the 20 participants was 95 ms (range = 75–135 ms).

Participants. Participants were 20 Boston University students who participated in exchange for course credit. All participants were native English speakers.

Results and Discussion

Percentage correct report of Item 1 averaged 98% or greater in all stimulus conditions. The percentages of correct reports of Item 2 given correct report of Item 1 are shown in Table 3. An analysis of variance (ANOVA) revealed a significant main effect of repeatedness in both the participant and item analyses, $F_1(1, 19) = 26.6$, $p < .001$; $F_2(1, 18) = 67.1$, $p < .001$. The main effect of length was not significant, $F_1(1, 19) = 2.3$, $p > .10$; $F_2(1, 18) = 1.5$, $p > .20$. We also failed to obtain a significant interaction between repeatedness and length, $F_1(1, 19) = 1.2$, $p > .25$; $F_2(1, 18) = 1.1$, $p > .30$.

Participants' misreports were analyzed in a manner similar to the previous experiment. In the present experiment, the first two letters in the repeated condition were shared; the remaining letters were unique to Item 2. Table 4 shows percentage report of the first two letters versus the remaining letters for the repeated and nonrepeated conditions, collapsed across word length. As predicted by sublexical models, Item 2 misreports contained fewer letters repeated from Item 1 compared with the nonrepeated condition, $t(17) = 6.2$, $p < .001$. Also, the unique letters in Item 2 were reported at a higher rate in the repeated condition compared with the same letters in the nonrepeated condition, $t(17) = 2.8$, $p < .05$.

The results of Experiment 2 are inconsistent with global similarity explanations of orthographic RB: Even though the three-letter stimuli shared a greater proportion of letters than the five-letter stimuli, they did not demonstrate a greater degree of RB. Although one could argue that the lack of a significant interaction was a matter of insufficient power, the effect size of the interaction was exceedingly small ($\omega^2 = .001$). Equivalent RB for word–nonword pairs sharing the same number of letters, regardless of proportion of repeated letters, is consistent with the sublexical view of orthographic RB.

[4] If the stimuli were words, then sublexical accounts might make different predictions for short and long words. This is because words allow pattern completion based on the "leftover letters," that is, the nonrepeated letters that appear to be detected can be capable of participating in cohort activation of words (Harris, 2001). Short and long words could differ in the number of words that are compatible with their leftover letters. More precise predictions would depend on statistical analysis of a frequency-coded lexicon.

Experiment 3: Nonword–Nonword Pairs

The finding of strong RB for word–nonword pairs that repeated only one or two letters is consistent with the proposal that RB can occur at the level of individual letters contained inside a nonword string. An alternative view is that the word's type node (Item 1) fed inhibition down onto letter clusters in Item 2.[5] On this view, when a word is detected, it spreads transient inhibition to the letters within it, which would mean that *phone* would inhibit the *ph* sequence in *phurl,* resulting in a deficit in reporting *phurl.* A prediction of this view is that no RB will be obtained for orthographically similar nonwords, such as *meap meab.* In contrast, the sublexical account of orthographic RB predicts substantial RB for nonword–nonword pairs. The goal of Experiment 3 was to test this prediction.

In creating nonwords for this experiment, we opted to explore some differences in the characteristics of nonwords. Consider the nonword *bund,* which occurs in nine English words (e.g., *abundant*), yielding a summed frequency of 61 per million (Francis & Kucera, 1982). Unitization may occur when a string of letters frequently occurs. Nonwords that occur frequently as parts of words may thus have status as mental types, more than nonwords that have many word neighbors but that never occur as words (e.g., *plame*). To explore this idea, we designed two types of nonwords. The *word-part* nonwords were letter strings that occur frequently in English as parts of words. We also included nonwords that never (or rarely) occur as a whole string inside of English words. These were called the *novel* nonwords. As frequently occurring strings, the word-part nonwords, but not the novel nonwords, may activate mental types that can be subjected to limitations in type-token binding.

Readers may wonder why we investigated the variable of string frequency rather than focusing on a more standard measure of lexicality such as neighborhood size. Prior work has suggested that factors that influence word recognition (such as frequency and phonological regularity) have minimal or no effects on RB (Harris & Morris, 2001b; Morris & Harris, 1999; but see Bavelier et al., 1994, for an alternative view). We also saw no theoretical reason to believe that neighborhood size would influence tokenization (or failure to bind types to tokens). It is interesting that this was confirmed when neighborhood structure of nonwords was recently examined in an RB nonwords paradigm. V. Coltheart and Langdon (2003) found that number of neighbors influenced participants' ability to report words and nonwords in the control condition (no repeated items) but not in repeated trials. Their data indicate repetition effects are not influenced by neighborhood size.

Given the comparison between word-part and novel nonwords, three findings are possible:

Table 3
Percentage of Correct Reports of Item 2 Given Reports of Item 1 in Experiment 2

Condition	3 letters: Item 1	3 letters: Item 2	3 letters: % correct	5 letters: Item 1	5 letters: Item 2	5 letters: % correct
Repeated	*cup*	*CUG*	31	*phone*	*PHURL*	29
Nonrepeated	*raw*	*CUG*	72	*stone*	*PHURL*	61

Note. Standard errors of the mean ranged from 6% to 7%.

Table 4
Percentage of Reports of Shared Versus Unique Letters in Item 2 in Experiment 2

Condition	% letter type: Shared	% letter type: Unique	Example: Stimulus	Example: Misreport
Repeated	36	80	*sweet swand* *mud mup*	*sweet rand* *mud wup*
Nonrepeated	69	54	*thick swand* *row mup*	*thick swamp* *row mut*

1. No RB is found for either type of nonword. This would be consistent with recent failures to find RB for repetition of identical nonwords (Campbell et al., 2002; V. Coltheart & Langdon, 2003). Because our first two experiments found strong RB for word–nonword pairs, this finding would suggest that activation of a lexical type node is necessary to observe RB.

2. Reliable RB is found for both types of nonwords. This would be consistent with the sublexical hypothesis (Harris, 2001; Harris & Morris, 2000; Morris & Harris, 1999) that RB occurs at the level of repeated letter clusters.

3. Reliable RB is found for the nonwords that are parts of words, but no RB is found for nonwords that are not parts of words. This would undermine the proposal that RB in nonwords exclusively comes from token-individuation limitations at a sublexical level.

We also included stimuli that repeated only a single interior letter (*fep tem*). A repetition effect for a single interior letter would extend the finding of RB for a single initial letter, found in Experiment 1, and would be a novel finding for nonwords. Our laboratory has failed to find RB for repetition of a single interior letter in words, despite several attempts (Harris, 2003; Harris & Morris, 2001a). Such an effect would be noteworthy because it could indicate that orthographic RB is stronger in nonwords than in words, possibly because words promote pattern completion (Harris, 2001).

Method

Design and materials. Pairs of nonwords, three to five letters long, were constructed to implement a two-factor design, with levels of both factors counterbalanced across participants. The repeatedness factor was whether the two critical items shared none of their letters (such as *fic tem*), one of their letters (*fep tem*), or all but one of their letters (*ter tem*). The repeatedness factor was within item, meaning that the second critical item (Item 2) was held constant while the first critical item (Item 1) was varied to implement the three levels of this factor, creating triples (as in Table 5).

The word-part factor was whether the second critical item (Item 2) occurred frequently in English as part of a word. Sixteen of the triples were

[5] This idea was suggested to us by V. Coltheart (personal communication, August 1998).

Table 5
Percentage of Correct Reports of Item 2 Given Correct Reports of Item 1 in Experiment 3

	Word-part nonwords			Novel nonwords		
Condition	Item example	% correct	Diff	Item example	% correct	Diff
Neighbor	*VING vind*	42	30	*NIME nide*	33	11
Single letter	*RITH vind*	57	15	*LIRP nide*	35	9
Nonrepeated	*ROTH vind*	72		*LARP nide*	44	

Note. Difference (Diff) is nonrepeated − repeated (either neighbor or single-letter condition). Standard error of the mean for report of Item 2 ranged from 3% to 6%.

word-part nonwords and 15 were novel nonwords.[6] Classification as word part was based on string frequency, using number of occurrences in the Francis and Kucera (1982) corpus, with novel nonwords having string frequencies of 0–3 ($M = 0.5$) and word-part nonwords having string frequencies of 10–2,091 ($M = 576$). Mean bigram frequency was calculated for each string, as described by Solso and Juel (1980). To increase perceivability, we chose nonwords that had very high bigram frequencies, as bigram frequency can be understood to be a measure of orthographic regularity (see discussion in Castles, Datta, Gayan, & Olson, 1999; Solso & Juel, 1980). There was a trend for novel nonwords to have lower bigram frequency than word-part nonwords: novel nonwords, $M = 1{,}065$; word-part nonwords, $M = 1{,}619$, but this was not statistically reliable, $t(29) = 1.2$, $p > .20$. Nonwords type also did not differ in orthographic neighbors: novel nonwords, $Mdn = 9$, word-part nonwords, $Mdn = 6$. All stimuli appear in Appendix C, with summary statistics for string frequency and mean bigram frequency.

Procedure. A difference from the first two experiments was that the first item always occurred in uppercase and the second item in lowercase. The reason for this change was a desire to maximize report of the first item. RB is strongest and least variable across participants and items when the first word of two critical words is well perceived. Many RB experiments conducted over several years suggested to us that uppercase words are better perceived than lowercase items under conditions of brief display and masking (Harris, 2001). This probably occurs because capitalization attracts attention and provides a stronger visual signal. An example of an RSVP sequence in Experiment 3 was *NIME ######## nide &&&&&&&.* As in the prior experiments, duration of nonwords was set individually for each participant on the basis of a computer-automated titration procedure carried out over 35 practice trials. Items in the practice trials were orthographically dissimilar. Exposure duration was initialized at 120 ms. Every 6 trials, exposure was decreased if the percentage report of both nonwords was more than 70% and was increased if report was less than 50%. This titration procedure was overly aggressive in decreasing exposure for 3 participants. These participants performed better than 70% correct at 90-ms duration during the practice trials, but when exposure was reduced to 75 ms at the onset of experimental trials, the participants thereafter reported fewer than 35% of both critical items. The data of these participants were discarded. The average exposure duration was 110 ms (range = 75–120 ms).

Participants. Participants were 32 Boston University undergraduates who received course credit for their participation, including 4 participants who acquired English from birth in conjunction with another language.

Results and Discussion

Item 1 was correctly reported on 94% of trials, a value that did not vary by condition. An ANOVA was performed on the 2 × 3 design using percentage correct report of the second nonword, given successful report of the first nonword (see Table 5). The word-part nonwords were easier to report than the novel nonwords, as indicated by a main effect of nonword type, $F_1(1, 31) = 58.35$; $F_2(1, 29) = 15.87$, both $ps < .001$. There was also a strong main effect of repetition condition (indicating that RB was present), $F_1(2, 62) = 9.40$; $F_2(2, 58) = 10.63$, both $ps < .001$.

Separate planned comparisons were conducted on the word-part and novel nonword stimuli to determine if the neighbor and single-letter conditions differed from the nonrepeated condition. For the word-part stimuli, the neighbor condition differed from the nonrepeated condition, $F_1(1, 31) = 23.97$; $F_2(1, 15) = 29.76$, both $ps < .001$. The single-letter condition also differed from the nonrepeated condition, especially for analysis by participants, $F_1(1, 31) = 12.94$, $p < .005$; although the difference was marginally significant for items analysis, $F_2(1, 15) = 3.1$, $ps < .09$.

As shown in Table 5, repetition deficits were weaker for the novel nonwords stimuli, and indeed failed to reach statistical significance for either the neighbors, $F_1(1, 31) = 3.00$; $F_2(1, 14) = 2.20$, both *ns*, or the single letters, $F_1(1, 31) = 2.41$; $F_2(1, 14) = 2.75$, *ns*. Further statistical evidence that the two nonword types differed in amount of RB comes from a significant interaction of nonword-type and neighbor/nonrepeated condition (single-letter condition excluded), $F_1(1, 31) = 8.22$; $F_2(1, 29) = 4.15$, both $ps < .05$.

The finding of more RB for more repeated letters replicates Experiment 1. That RB was found at all for a single interior letter in nonwords (but not in words; see Harris & Morris, 2001a) suggests that nonwords may be more vulnerable to orthographic RB than words. One explanation is that words support pattern completion of “missing” letters. For example, in the stimulus *wharf dwarf*, the “blinded” *arf* sequence can be guessed from the *dw* onset (Harris, 2001).

Strong RB for orthographically similar nonword–nonword pairs extends Experiments 1 and 2 by showing that orthographic RB for nonwords is a general phenomenon rather than something obtained only in word–nonword pairs. Small and statistically unreliable RB was found for the novel nonwords, that is, for nonwords that never occur as coherent strings in English text. However, it is premature to conclude that RB in nonwords is restricted to word-part nonwords. The novel nonwords had lowered nonrepeated report, and lowered nonrepeated report depresses the possible difference between repeated and nonrepeated conditions. In Experiment 4, we attempted to achieve better nonword report by using longer exposure durations for nonwords. Most importantly, in Experiment 4, we directly compared identity and orthographic RB by including identical word and nonword repetitions.

[6] An additional triple was constructed around the item *jag*, but *jag* is a word, albeit a low-frequency one. This was considered a stimulus-construction error and removed from statistical analysis.

Experiment 4

V. Coltheart and Langdon (2003) and Campbell et al. (2002) reported finding no RB for nonwords. However, these authors used identical nonwords, which is identity RB, whereas in Experiment 3, we used nonidentical nonwords, which is orthographic RB. The striking RB found for nonidentical nonwords made us skeptical that RB would disappear simply by instituting as small a change as making the nonwords identical rather than differing by a single letter.

Could there be an additional amount of RB that occurs at the lexical level for identical repetitions, in addition to RB occurring at the sublexical level? Suggestive evidence for this comes from the observation that RB is stronger for repeated identical words compared with orthographic neighbors, but there is only a small reduction in amount of RB between neighbors and words repeating only half their letters (Ayçiçegi & Harris, 2002; Harris & Morris, 2000, 2001a). Our expectation is there will be no additional RB deriving from the whole-string level. This means that, for nonwords, the magnitude of RB will be only slightly larger in the identical compared with the neighbor conditions. In contrast, for words, RB should be much larger in magnitude for the identical condition compared with the neighbor condition.

Under conditions of brief display and masking, nonwords are more difficult to read than words. If nonword trials are generally reported more poorly than word trials, then the lower nonrepeated score for nonwords will depress the nonrepeated-repeated score, meaning that less RB will be observed. Low recognition rates may entirely remove the opportunity to observe RB, because all theories concur that the first item needs to be accurately recognized (i.e., in token individuation theory, the type needs to be activated). We thus ran two versions of this Experiment 4. In Experiment 4b, exposure duration was set separately for word and nonword trials, based on a practice block. In Experiment 4a, the same practice block was used, but a single exposure duration was selected that was judged to be midway between the optimal duration for words and the optimal duration for nonwords. Experiments were run over the same months but used different participants.

Method

Design and materials. A between-items variable with three conditions was stimulus type. These were words and the two types of nonwords used in Experiment 3. This variable was crossed within item with three types of relatedness (nonrepeated, neighbor, and identical). Words and word-part nonwords were equated for string frequency, using number of occurrences in the Francis and Kucera (1982) corpus. As described in Experiment 3, string frequency is not the same as word frequency: The string *chew* (an item in the word condition) occurs 24 times in the corpus, including *chewing* (13) and forms of *eschew* (4). *Prost* (a word-part nonword) occurs 24 times, in words like *prostate* and *prosthetic.* We desired to make the two types of nonwords more equivalent in bigram frequency. It was difficult to find three-letter "novel" nonwords, and using four- and five-letter nonwords allowed a greater range of stimuli. In the final set of stimuli, string frequency of Item 2 ranged from 0 to 2 ($M = 0.3$) for novel nonwords, 6 to 1,153 ($M = 193$) for word-part nonwords, and 3 to 1,690 ($M = 240$) for words. The string frequencies and bigram frequencies of the word-part nonwords and words did not differ significantly, although the word-part nonwords actually had slightly higher bigram frequencies (1,628) than the 16 word stimuli (1,079). Novel nonwords had nonsignificantly lower bigram frequencies than the word stimuli (961). The three types of items had similar median number of orthographic neighbors (3 to 5). Summary statistics comparing frequency and number of neighbors appear in Appendix D, along with items used.

Procedure. As in prior experiments, practice trials containing nonsimilar items were used to set exposure durations individually for each participant. Exposure duration began at 160 ms per word and was reduced every four trials by 15 ms. Experimenters made note of the fastest speed at which participants could report both nonwords, and both words, and used this to choose the speed for the experimental trials. Participants generally required 30 ms longer exposure duration to accurately read nonword trials. In Experiment 4a, which used the same duration for both words and nonwords, the mean exposure duration was 100 ms (range = 60–150). For Experiment 4b, the mean exposure duration for nonword trials was 112 ms (range = 75–150) and for word trials, 83 (range = 60–135).

Participants. Thirty-three Boston University undergraduates participated in Experiment 4a and 27 participated in Experiment 4b. The data of an additional 13 participants were excluded from analysis for the following reasons: 7 participants reported both items on fewer than 35% of the nonrepeated trials,[7] 1 participant was at ceiling on all trials, and 5 participants adopted a strategy of guessing identical repetitions[8] (as indicated by two or more occurrences of guessing an identical repetition on a nonidentical trial, as in reporting *chef chef* when the stimulus was *chef chew*).

Results and Discussion

To facilitate comparisons across conditions, we calculated difference scores for the nonrepeated-neighbor and nonrepeated-identity conditions. As shown in Table 6, these difference scores were substantially greater than zero (ranging from 23 to 49), indicating that strong RB occurred for all stimulus conditions, a finding that contradicts Campbell et al. (2002) and V. Coltheart and Langdon (2003). Importantly, more RB occurred for words compared with nonwords. This was confirmed statistically by combining the two types of nonwords into one category and examining only the identity condition. In Experiment 4b, main effects for the word–nonword comparison were $F_1(1, 26) = 20.2$; $F_2(1, 46) = 13.4$, both $ps < .001$; in Experiment 4a, they were $F_1(1, 32) = 5.7$, $p < .03$; $F_2(1, 46) = 2.94$, $p < .09$.

As expected, identical repetitions produced more RB than orthographically similar items. Main effects for the identity–similarity comparison were $F_1(1, 26) = 7.1$; $F_2(1, 46) = 12.0$, both $ps < .01$ in Experiment 4b. In Experiment 4a, they were $F_1(1, 32) = 4.8$; $F_2(1, 46) = 7.03$, both $ps < .04$.

An important theoretical question was whether words, but not nonwords, would show an additional amount of RB for the iden-

[7] Including participants who had low nonrepeated report increases variance leading to reduced power to find significant effects, and also reduces the opportunity to find significant RB by depressing the range of possible nonrepeated–repeated scores. The fact that titration for selecting exposure duration was inadequate for 8 of 73 participants (11%) reflected the difficulty of setting separate exposure speeds for words and nonwords, a novel procedure in our laboratory.

[8] As discussed in Harris and Morris (2001a), including participants who adopt the strategy of guessing identical repeated items changes the pattern of results, because it inflates report in the identical condition and depresses it in the neighbor condition (because participants most often incorrectly guess an identical repetition in the neighbor condition). However, could report of *chew chew* instead of *chef chew* occur because *chew* was actually misperceived as the word *chef*? We can determine from the nonrepeated trials (*dial chew*) that the probability of misperceiving *chew* as *chef* is very low. There was also a common profile of responses for these errors that could in principle be quantified. Specifically, these guesses were made later in the experimental session, after the participant had correctly reported identical repetitions on prior trials.

Table 6
Results for Experiments 4a and 4b

Stimulus type	Report of Item 2 given report of Item 1			Difference score		
	Identical	Neighbor	Nonrepeated	Nonrepeated-identical	Nonrepeated-neighbor	Neighbor-identical
Experiment 4a (same duration)						
Words	44	59	86	42***	27**	15*
Word-part nonwords	36	43	59	23***	16*	7
Novel nonwords	31	36	61	30***	25**	5
Experiment 4b (nonword trials 30 ms longer)						
Words	34	51	83	49***	32**	17*
Word-part nonwords	45	60	77	32***	17**	15**
Novel nonwords	51	64	74	23**	10†	13†

Note. Item 1 was correctly reported on 99% of word trials and 95% of nonword trials in Experiment 4a and on 97% and 95% of analogous trials in Experiment 4b. Statistical significance was tested by planned comparisons between the nonrepeated and repeated (neighbor, identity) conditions. For cell means reported in this table, standard error of the mean for report of Item 2 ranged from 3% to 7%. Standard error for difference scores ranged from 5 to 7.
† $p < .07$. * $p < .05$. ** $p < .01$. *** $p < .001$.

tical condition beyond the amount found for the neighbor condition. The difference scores in Table 6 reveal a trend in this direction: Words had a larger decrement in report between identical repetitions compared with orthographic neighbors, whereas nonwords showed a smaller decrement. However, identity–similarity of items did not interact with word–nonword status (all *p*s $>$ .15). This means that statistical support for the proposal that nonwords show less of an increase in RB between orthographic neighbors and identical words is lacking.

Did the two types of nonwords differ from each other in amount of RB? When the two types of nonwords were compared in a 2 × 2 ANOVA (excluding words), word-part nonwords showed similar amounts of RB as novel nonwords, in both Experiments 4a and 4b (*p*s $>$ .13). We thus infer that lack of RB for the novel nonwords in Experiment 3 was due to lowered nonrepeated report rather than lack of RB per se. This experiment showed strong orthographic and identity RB for two types of nonwords while also showing that RB is weaker in nonwords than in words.

General Discussion

It would be attractive support for token-individuation theory if RB did not occur for nonwords. This finding would unite several proposals: first, that nonwords do not have an existing stored representation or "type" and, second, that there is an early stage in visual processing in which types are activated but that binding types to tokens (or otherwise creating distinct episodic representations) requires additional time or access to a capacity-limited attention. Visual objects that activate types thus demonstrate the transient report failure known as repetition blindness, whereas visual objects that do not activate types (such as nonwords) do not.

We identified two shortcomings in the reasoning that token-individuation theory will predict a lack of RB for nonwords. The types that fail to be token individuated may be letters within words (or nonwords) rather than exclusively the word or nonword string that is processed for report. On the assumption that RB does occur at the level of letters within words (a controversial hypothesis; see Chialant & Caramazza, 1997, and rebuttal by Harris & Morris, 2001a), it then follows that RB will be found for nonwords, because nonwords contain letters. Second, processing of nonwords may result in activating words, as is proposed both by models using rules (M. Coltheart et al., 1993, 2001) and by connectionist models that emphasize activation of a distributed network of mappings between graphemes and phonemes (Plaut, McClelland, Seidenberg, & Patterson, 1996; Zorzi, Houghton, & Butterworth, 1998). Our experiments were designed to address whether RB in nonwords is restricted to the sublexical level and whether nonwords that are parts of words pattern with data for words rather than nonwords. However, the data have ended up supporting a simpler picture than we had imagined. RB in nonwords is similar to words, with the main difference being that RB in nonwords is weaker and more difficult to obtain than RB in words.

RB at Sublexical Versus Lexical Levels

The first two experiments demonstrated RB for word–nonword pairs (*plane plosh*). A sublexical locus was inferred because the unique letters in the nonword appeared in participants' misreports with higher probability than the repeated letters. Experiment 3 revealed orthographic RB for nonword–nonword pairs, demonstrating that inhibition from a lexical item is not a necessary component of RB, which is inconsistent with claims by V. Coltheart and Langdon (2003). Experiment 3 also produced the novel finding of RB for a single letter in the interior of a nonword. Because we have failed to find RB for a single interior letter inside words (Harris, 2003; Harris & Morris, 2001a), we suggest that pattern-completion processes in words compensate for suppression of single, interior letters repeated across two words in RB paradigms.

The question of why identity RB was greater for words than for nonwords is unresolved. Readers' generally poorer ability to encode nonwords under RSVP conditions depresses the opportunity to find a large difference between nonrepeated and repeated conditions. We boosted report level for nonwords by instituting a longer display duration for nonwords compared with words (Experiment 4b). However, RB diminishes with longer exposure durations (Kanwisher, 1987), and thus the nonwords in this experiment may have been partially out of the temporally narrow "RB window." Researchers could try to equate word and nonword reading in the same experiment by comparing (for example) easy-to-read nonwords with hard-to-read words.

We had hypothesized that words could show more RB than nonwords because identical repetitions of nonwords only manifest sublexical RB, whereas words display RB at two levels, sublexical and lexical. Motivation for this came from our laboratory experience in observing participants in experiments like the present set. Identical repetitions lead to a more profound "blindness" than what is observed for orthographic neighbors. Identity RB can be obtained with slower exposure durations and more intervening items than orthographic RB. RB increases with number of shared letters, but the increase between identical repetitions and neighbors is greater than the increase between three letters repeated and four letters repeated (Harris & Morris, 2000). Participants are more likely to have the "something" experience (the feeling that a word was displayed; Park & Kanwisher, 1994) for orthographically similar words than for identical repetitions. We thus hypothesized that RB in words occurs at two levels: the level of the word string and the sublexical level. Would nonwords show an additional deficit, beyond that found for neighbors, due to repetition at the level of the whole string? The trend we observed was for words, but not nonwords, to show this additional deficit. In Experiment 4a, report of the second nonword was only 5% to 7% lower for identical repetitions compared with orthographic neighbors, whereas the difference for words was 15%. However, these are small differences, and the interaction of identity–similarity with word–nonword status was statistically unreliable. We thus cannot conclude that all RB in nonwords occurs at the sublexical level. The question certainly warrants further investigation.

Note that there was also more orthographic RB for words compared with nonwords (see Experiment 4b, Table 6). One could imagine setting aside the type-token binding perspective to consider the effects of lateral inhibition between words and their neighbors, as proposed in interactive-activation models (e.g., Plaut et al., 1996) and the dual-route cascaded model of M. Coltheart et al. (2001). However, there is no evidence that competition between words plays a role in orthographic RB (Harris & Morris, 2001b). Thus, the issue of depressed nonrepeated report (Experiment 4a) and slower exposure time (Experiment 4b) appear to be the best explanations for greater orthographic RB in words compared with nonwords.

We hypothesized that nonwords that are parts of words (*tane tage*) would show more RB than nonwords that are not parts of words, because word-part nonwords could attain the status of mental type through frequent exposure. Although Experiment 3 found RB only for word-part nonwords, and not novel nonwords, Experiment 4 found equivalent amounts of RB for these two word types. We attribute the discrepant findings to participants' less accurate report of the novel nonwords in Experiment 3, as lowered recognition of nonrepeated trials depresses the opportunity to find statistically significant RB.

Conflicting Findings Regarding RB in Nonwords

Why did prior researchers (Campbell et al., 2002; V. Coltheart & Langdon, 2003) fail to observe RB in repeated identical nonwords while finding it, robustly, in repeated identical words? Indeed, both groups of researchers found a repetition advantage for nonwords. We argue that they failed to find RB for nonwords because their observers could not reliably encode the nonwords. We discuss the implications for theories of the mechanisms underlying RB.

Across V. Coltheart and Langdon's (2003) experiments with nonwords displayed via RSVP, participants had very low accuracy in reporting nonwords on trials without repeated items: 10%–20% (Experiment 1), 8%–12% (Experiment 2), 10% (Experiment 4), 20% and 30% (with familiarization training; Experiment 5), and 8%–20% (Experiment 6). In contrast, accuracy on word nonrepeated trials was 60%–70%, accuracy rates that allowed a robust RB effect of 20%–30%. However, Coltheart and Langdon found *improved* report accuracy of 10% to 20% for nonwords. They thus concluded that RB does not appear for nonwords.

No such conclusion can be drawn, because repetition priming occurs for both words and nonwords when recognition accuracy is near floor. Our laboratory experience with RB has revealed that, if exposure durations are broadly within an "RB window" of 65 ms to 135 ms per item, size of RB effects depends on participants' accuracy in reporting orthographically nonsimilar words, such that three types of effects emerge:

- 50%–85% report on nonrepeated trials → maximal difference between nonrepeated and repeated of 20% for orthographically similar words, and 30%–40% for identical repetitions
- 30%–50% report on nonrepeated trials → differences between nonrepeated and repeated conditions are greatly reduced and variable across participants, with some participants showing no RB
- under 20% report on nonrepeated trials → improved performance on repeated trials, especially for identical repetitions (a repetition advantage).

Based on this experience, our laboratory adopted the practice of individually setting exposure speeds based on participants' ability to report orthographically nonsimilar items in a practice block. We also exclude participants from data analysis when accuracy at reporting both critical items on the nonrepeated trials was less than 35%. We chose the 35% cutoff based on our prior observations that this is the place where RB begins to disappear for words. Adopting this cutoff does not threaten the validity of conclusions about RB, because the cutoff is neutral with respect to the phenomenon of RB. All that this cutoff concerns is whether participants can read two words that do not share any repeated letters.

V. Coltheart and Langdon (2003) would probably have obtained priming for both words and nonwords, if word report had been as equally inaccurate as nonword report. We can illustrate this by analyzing the 7 participants who were excluded from Experiment 4 for having accuracy for nonrepeated trials of less than 35%. As shown in Table 7, these participants demonstrate slight priming for nonwords, no RB for orthographically similar words, and a very small RB effect for identical word repetitions. Even more striking values emerge when the analysis is restricted to 5 very-poorly

Table 7
Analysis of Participants With Very Low Nonrepeated Accuracy

Condition	% nonrepeated accuracy $< 35\%$ ($N = 7$)		% nonrepeated accuracy $< 20\%$ ($N = 5$)	
	Nonwords	Words	Nonwords	Words
Identical	32	17	42	25
Neighbor	49	28	37	26
Nonrepeated	20	25	28	12

Note. Values reflect percentages of correct reports of Item 2 given correct reports of Item 1. Standard errors of the mean ranged from 4% to 17% ($N = 7$) and 4% to 25% ($N = 5$).

performing participants, those who had less than 20% accuracy on the nonrepeated trials (right panel in Table 7). These participants show priming for both words and nonwords.

Why does low nonrepeated report co-occur with priming on repeated trials? When overall report levels are low, it means that encoding of words is very poor. If the first item is not encoded to the point at which it can be reported, it is not tokenized, and thus it cannot inhibit perception of the other item, and thus no RB occurs. The situation then becomes similar to masked priming. However, in masked priming, participants are usually only required to report one item. In V. Coltheart and Langdon's (2003) experiment, like ours, participants attempted to report both items. In our analysis of the 5 least-accurate participants, these participants actually correctly reported both identical repetitions on 25% of trials. If encoding of even one item is so difficult, how are participants able to report two items better than one item?

Participants know they are supposed to report (at least two) items. Repeated letters across the two occurrences can reinforce each other, aiding perception. Participants can take advantage of their knowledge that the two items are orthographically similar or identical to guess two similar or identical words. This point was argued in Harris and Morris (2001a), in which we demonstrated a reduction in RB for identical items if participants adopt a strategy of guessing a repetition when they have an impression of similarity (see also discussion of the perception that "something" was present, in Park & Kanwisher, 1994).

V. Coltheart and Langdon (2003) argued that participants' much higher accuracy for words than nonwords does not threaten the validity of their conclusions by referring to their Experiment 3, in which participants counted how many items had been displayed. Word and nonword trials had comparable accuracy, with nonwords being slightly aided by repetition and words slightly impaired. However, note that this task does not require tokenization. With nonwords being difficult to identify to the point of conscious report, participants could have relied on the strategy of looking for visually similar patterns. Because reading words is highly automatic, participants are less likely to treat them as novel visual patterns. Therefore, words would have been more likely to have been recognized, tokenized, and subjected to RB.

We maintain that it would be easy to replicate V. Coltheart and Langdon's (2003) results by finding exposure parameters that give words a perceptual advantage over nonwords. The easiest parameter to manipulate in this fashion is exposure duration, because exposures at which reporting words is high but subceiling often produce accuracy near floor for nonwords.

There are other ways to give words a perceptual advantage over nonwords. One of these is illustrated in Campbell et al. (2002). Like V. Coltheart and Langdon, these authors found a trend for priming in nonwords while obtaining RB for words. In two of their three experiments, report of nonwords in the nonrepeated condition was very low: 8% to 15% in their Experiment 1, and 18% to 21% in their Experiment 3. Lack of RB in these experiments can thus be explained by following the foregoing discussion.

However, in one condition in Experiment 2 of Campbell et al. (2002), nonrepeated accuracy was reasonably good (51%). Why then was RB absent and a slight priming effect obtained (12%)? Instead of using a full report procedure, Campbell et al. used recognition probes, following Arnell and Jolicoeur (1997). After the RSVP display, participants were presented with three items and asked how many times each had occurred. One was always a foil. On repeated trials, two of the response probes were foils. In unpublished work (Harris, Morris, & Ayçiçegi, 2003), we obtained very strong orthographic RB for words using recognition probes. Our foils were always orthographically similar to targets. For example, if shown the two targets *slip slap,* observers would be asked for a forced-choice decision to the target *slap* or the foil word *slop.* Participants could not achieve high accuracy by using an impression of orthographic similarity between targets and probes. In contrast, Campbell et al. did not use orthographically similar foil items.

In Campbell et al.'s (2002) experiments, observers first saw a sentence displayed via RSVP. Participants then received the probes of one foil and two targets and had to type in how many times each occurred (e.g., for the nonrepeated trial, correct responding would be 0, 1, 1 and for the repeated trial, 0, 0, 2). For example:

> *After checking his old gelf the narb was closed.*
>
> or
>
> *After checking his old narb the narb was closed.*
>
> response choices: *krad gelf narb*

Our critique of this procedure is that fully encoding the nonwords is not necessary to perform well on this task. It is sufficient to encode any aspect of the nonword targets that allow them to be discriminated from the foils. Given that the accuracy for nonrepeated trials was only 50%, encoding word length, word-envelope shape, letter shape, or individual letters or letter pairs is likely sufficient to distinguish targets from foils. Indeed, one reason for participants to avoid fully encoding nonwords was that Campbell et al.'s (2002) nonwords contained rare letter combinations and appear to be difficult to pronounce. For example, their foil nonwords included *utni, dymi, etco, swun, smas, acdi, itno, jyru, cypo, pega, vewol, wehla, wrie, ceths, rutoe, brae, ertah, otuloko, duby, glof, jicue, buze, pippra, slos, oneon, symlob, nisoe, slun, tethe, mesou, dwin, reka, sclas, alvog, mepla, spelma, rega, sedli, peyne, hesou, snog, ralmevo, anget, wiwido, wetar,* and *dewm.*

Why did accuracy actually increase on repeated nonword trials? Observers likely become attuned to the presence of identical repetitions across the experiment, and when an impression of similarity was identified, participants could let information accrue across the two presentations of the nonwords, and then guess that two occurrences of that nonword had been reported. This would have produced superior accuracy in the repeated condition compared with the nonrepeated condition.

The strategy of not encoding items would be less useful for word trials, because of the relative ease and automaticity of phonological encoding of words. The contextual cues provided by sentences make reading for immediate comprehension a viable method for encoding words in RSVP sentences (Potter, 1984). On word trials, participants likely simply tried to read and encode the words in the sentences. On nonword trials, we suspect that many participants did not attempt to encode the nonwords but instead attended to salient letters or nonword shape.

As noted above, an additional reason for participants to resist encoding the nonwords is that Campbell et al.'s (2002) nonwords were orthographically strange and do not lend themselves to rapid phonological encoding. We calculated bigram frequency for their nonwords (using position-sensitive bigrams as described in Solso & Juel, 1980, as done in Experiments 3 and 4 of the present article) and found that their nonwords had lower bigram frequencies ($M = 932$) than their words ($M = 1{,}348$), $F(1, 377) = 10.77$, $p < .001$. Indeed, only 20% of their nonwords were above the mean bigram frequency for words. Furthermore, the bigram frequency of the foil nonwords ($M = 720$) was lower than the bigram frequency of the target nonwords ($M = 932$), $F(1, 311) = 4.6$, $p < .05$. Thus, participants could improve their detection of repeated words by rejecting those response choices with unusual letter combinations.

To force participants to fully encode nonwords and to stabilize them in working memory where they are in a form ready to be accessed for explicit report (which Kanwisher, 2001, has discussed as a criteria to observe RB), the task needs to be either full report or orthographically similar foils must be used. To decrease participants' propensity to guess that a repetition was present, one should include orthographically similar words, so that an impression of similarity across two items does not lead to a guess of two occurrences of one item.

Where Does This Leave Whole-Word Theories of Repetition Blindness?

Kanwisher and Potter (1990) proposed that the level at which RB occurs is the level at which attention is processed, which is the level of the whole word (or whole string). Whole-word theories dovetail with work demonstrating that attention modulates degree of RB. Using a two-letter display, Kanwisher et al. (1995) instructed observers to report either color or shape, thus making color or shape the attended dimension. Observers responded less accurately when the letters were identical on attended dimensions. Identity on the unattended dimension had no influence on report. This and related studies (Baylis, Driver, & Rafal, 1993) support the view that RB affects the level on which attention is focused.

Our findings on orthographic RB reveal that RB is sensitive to the level of letters within words. This raises the possibility that RB influences two levels, a sublexical level in addition to the level being reported. One could speculate that the level influenced by RB is not just the level on which attention is focused, but any level of information that must be processed to encode the information into a form that is helpful for future action (i.e., create an episode instantiation). Future work will be necessary to determine whether RB in identical repetitions is the sum of RB occurring at the sublexical and lexical levels.

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Appendix A

Stimuli for Experiment 1

Neighbor	Item 2	Single letter	Nonrepeated
arm	*arp*	*add*	*oil*
dog	*dob*	*die*	*fit*
fed	*fen*	*fly*	*sad*
box	*bot*	*buy*	*dry*
hit	*hix*	*hot*	*bed*
jet	*jeg*	*joy*	*bus*
mud	*mup*	*mad*	*row*
leg	*leb*	*lie*	*sky*
wet	*wep*	*win*	*bay*
cup	*cug*	*cry*	*fun*
card	*carn*	*cope*	*fist*
beef	*beel*	*bird*	*horn*
gate	*gath*	*golf*	*knee*
meat	*meap*	*mold*	*ring*
noon	*noof*	*neat*	*drug*
fall	*falk*	*food*	*meet*
pink	*pind*	*poem*	*luck*
rise	*risp*	*race*	*date*
skin	*skig*	*salt*	*cook*
tail	*tain*	*tube*	*dull*

Note. Each word appearing as the second critical word (Item 2) appeared with each of the words in the other three lists to create the three stimulus conditions. Item 2 always appeared in uppercase.

Appendix B

Stimuli for Experiment 2

Length 3 stimuli			Length 5 stimuli		
Item 1	Item 2	Nonrepeated	Item 1	Item 2	Nonrepeated
arm	*arp*	*oil*	*glass*	*glome*	*happy*
dog	*dob*	*fit*	*twice*	*twamp*	*knife*
fed	*fen*	*sad*	*drunk*	*dripe*	*shock*
box	*bot*	*dry*	*sweet*	*swand*	*thick*
hit	*hix*	*bed*	*plane*	*plosh*	*green*
jet	*jeg*	*bus*	*crazy*	*croot*	*false*
mud	*mup*	*row*	*fruit*	*frowl*	*waste*
leg	*leb*	*sky*	*phone*	*phurl*	*stone*
wet	*wep*	*bay*	*smile*	*smoft*	*truck*
cup	*cug*	*raw*	*brain*	*brust*	*chain*

Note. Items marked Item 1 shared their first two letters with Item 2. Item 2 always appeared in uppercase.

Appendix C

Stimuli for Experiment 3

Neighbor	Item 2	Single letter	Nonrepeated
		Novel nonwords	
croot (834, 0)	*croom* (820, 0)	*dripe* (654, 0)	*shipe* (2,052, 1)
cug (143, 0)	*cuv* (120, 0)	*lud* (24, 590)	*wep* (34, 45)
flate (1,482, 6)	*flade* (928, 0)	*glish* (299, 228)	*shilt* (2,624, 0)
glome (533, 7)	*glame* (732, 0)	*blop* (265, 0)	*risp* (433, 16)
hix (4,996, 0)	*hax* (3,970, 3)	*mav* (1,356, 6)	*ost* (6, 3,005)
nime (2,703, 13)	*nide* (913, 1)	*lirp* (1,187, 0)	*larp* (1,324, 1)
nurk (803, 0)	*nork* (2,202, 2)	*shof* (461, 0)	*shup* (347, 2)
phurl (174, 0)	*phurk* (193, 0)	*slunn* (513, 0)	*slenn* (348, 0)
ploss (1,262, 0)	*plass* (1,258, 0)	*beall* (2,121, 3)	*beell* (1,222, 0)
plune (899, 0)	*plute* (1,029, 0)	*slood* (715, 0)	*choss* (1,458, 0)
selt (1,392, 2)	*selp* (1,328, 0)	*nent* (1,249, 317)	*ning* (1,245, 2,883)
smoft (346, 0)	*smaft* (454, 0)	*drife* (667, 0)	*drine* (1,617, 2)
swook (292, 0)	*swood* (365, 0)	*shont* (1,774, 0)	*trape* (725, 1)
tream (1,350, 98)	*treap* (1,327, 0)	*frake* (623, 0)	*cated* (2,107, 331)
fen (850, 436)	*fek* (330, 1)	*tep* (103, 392)	*tis* (3,540, 1,018)
		Mean	
(1,204, 37.5)	(1,065, 0.5)	(801, 102.4)	(1,272, 487.0)
		Word-part nonwords	
arp (2,376, 243)	*arn* (2,374, 714)	*ort* (106, 4,462)	*oft* (323, 532)
bon (348, 355)	*bot* (2,891, 1,006)	*nom* (3,106, 751)	*nin* (94, 3,210)
carn (1,574, 37)	*carb* (1,396, 64)	*tash* (1,243, 1)	*tion* (912, 17,421)
dob (226, 6)	*dod* (250, 25)	*mok* (10, 77)	*mer* (2,162, 2,717)
foo (5,194, 466)	*fon* (4,926, 33)	*jor* (5,012, 334)	*alt* (1,502, 1,094)
gith (5,565, 0)	*gath* (3,266, 92)	*skap* (59, 0)	*skir* (140, 40)
lub (20, 235)	*leb* (318, 83)	*res* (186, 8,782)	*alf* (1,503, 554)
meap (1,413, 0)	*meas* (1,399, 337)	*nelo* (1,255, 1)	*nool* (1,450, 0)
mup (41, 1)	*mun* (264, 769)	*gur* (729, 411)	*gar* (739, 699)
pinal (928, 0)	*pital* (665, 267)	*tized* (900, 31)	*turg* (382, 3)
ral (128, 2,971)	*rel* (124, 2,091)	*sep* (687, 322)	*ime* (1, 3,182)
snat (4,369, 19)	*smat* (4,341, 8)	*pran* (926, 29)	*lork* (2,768, 0)
tant (1,650, 1,033)	*tane* (1,772, 94)	*bund* (965, 61)	*blef* (143, 8)
ter (1,808, 12,676)	*tem* (108, 1,820)	*fep* (330, 0)	*fic* (52, 2,280)
ven (521, 3,395)	*ved* (259, 1,807)	*jep* (36, 0)	*jat* (238, 0)
ving (1,273, 1,328)	*vind* (1,561, 10)	*rith* (5,490, 22)	*roth* (3,448, 166)
		Mean	
(1,715, 1,422.8)	(1,619, 576.2)	(1,315, 955.2)	(991, 1,994.1)

Note. Each nonword is followed in parentheses by its mean bigram frequency and string frequency. As noted in the text, Item 2's string frequency was used to classify items as novel nonwords or word-part nonwords. The item appearing as the second critical item (Item 2) appeared with each of the nonwords in the other three lists to create the three conditions. Item 1 appeared in uppercase.

(*Appendixes continue*)

Appendix D

Stimuli for Experiment 4

Neighbor	Item 2	Nonrepeated
	Novel nonwords	
croot (834, 0)	*croom* (820, 0)	*shipe* (2,052, 1)
calix (474, 0)	*camix* (342, 0)	*swoom* (288, 0)
cruv (122, 0)	*chuv* (149, 0)	*seaf* (1,592, 6)
doman (798, 0)	*doban* (498, 0)	*cheel* (2,808, 0)
glome (533, 7)	*glame* (732, 0)	*shenn* (2,687, 0)
mear (1,861, 5)	*meab* (1,391, 1)	*nool* (1,450, 0)
mert (2,198, 7)	*merk* (2,395, 0)	*tisp* (898, 0)
nibe (86, 1)	*nide* (913, 1)	*lart* (1,621, 0)
nurk (803, 0)	*nork* (2,202, 2)	*shup* (347, 2)
phurk (193, 0)	*phurl* (174, 0)	*slenn* (348, 0)
ploss (1,262, 0)	*plass* (1,258, 0)	*beell* (1,222, 0)
plune (899, 0)	*plute* (1,029, 0)	*choss* (1,458, 0)
selt (1,392, 2)	*selp* (1,328, 0)	*cada* (1,043, 3)
smoft (346, 0)	*smaft* (454, 0)	*teron* (472, 1)
treal (1,730, 4)	*treap* (1,327, 0)	*lork* (2,768, 0)
swok (373, 0)	*swod* (365, 0)	*tich* (1,847, 2)
	Mean	
(869, 1.6)	(961, 0.3)	(1,431, 0.9)
	Word-part nonwords	
pling (2,061, 53)	*bling* (1,951, 116)	*cated* (2,107, 331)
brill (1,688, 65)	*brell* (1,328, 11)	*pline* (1,393, 51)
calat (632, 8)	*calam* (475, 7)	*meric* (544, 939)
carn (1,574, 37)	*carb* (1,396, 64)	*tran* (1,070, 834)
gart (1,264, 23)	*garm* (1,229, 12)	*lund* (926, 14)
ling (2,135, 2,065)	*lind* (2,423, 184)	*fram* (1,737, 141)
mity (3,079, 62)	*lity* (3,732, 1,153)	*trem* (1,175, 225)
ored (786, 335)	*oved* (1,810, 519)	*ning* (1,245, 2,883)
pical (591, 111)	*pital* (665, 267)	*turge* (508, 1)
	Word-part nonwords (*continued*)	
tane (1,772, 94)	*tage* (509, 562)	*bott* (957, 207)
snat (4,369, 19)	*smat* (4,341, 8)	*plish* (526, 89)
trost (1,778, 9)	*prost* (1,627, 23)	*ation* (427, 9,113)
varn (990, 5)	*vard* (1,057, 53)	*ting* (1,791, 4,383)
vendi (424, 6)	*vindi* (680, 10)	*poral* (1,467, 10)
treak (1,378, 20)	*tream* (1,350, 98)	*blong* (1,766, 1)
olate (1,399, 84)	*flate* (1,482, 6)	*nomen* (1,092, 72)
	Mean	
(1,620, 187.0)	(1,628, 193.3)	(1,170, 1,205.9)
	Words	
acid (794, 30)	*arid* (1,064, 5)	*envy* (48, 7)
align (326, 22)	*alien* (906, 58)	*ghost* (1,584, 20)
chef (3,108, 9)	*chew* (3,360, 24)	*dial* (310, 81)
deny (1,025, 57)	*dent* (1,089, 1,690)	*hack* (3,411, 33)
fame (3,212, 25)	*fade* (1,507, 22)	*pony* (1,237, 11)
grant (1,510, 170)	*giant* (909, 44)	*chose* (2,447, 108)
idiot (156, 7)	*idiom* (142, 12)	*clash* (1,132, 8)
knob (635, 7)	*know* (1,064, 1,348)	*buck* (1,454, 97)
skin (274, 170)	*skip* (127, 26)	*wood* (2,003, 304)
slap (320, 19)	*slop* (253, 43)	*baby* (588, 73)
sleep (490, 151)	*sleek* (483, 3)	*trust* (1,237, 164)
spoke (473, 165)	*spike* (358, 11)	*train* (1,216, 440)
stain (1,936, 132)	*stair* (2,338, 111)	*hood* (2,035, 193)
unify (573, 6)	*unity* (998, 437)	*creep* (880, 24)
wrong (1,834, 143)	*wring* (2,200, 3)	*build* (2,320, 431)
yarn (978, 20)	*yawn* (478, 4)	*meek* (2,017, 24)
	Mean	
(1,103, 70.8)	(1,079, 240.0)	(1,495, 126.1)

Note. Each nonword is followed in parentheses by its mean bigram frequency and string frequency. Each item appearing as the second critical item (Item 2) appeared with each of the items in the other two lists to create the two stimulus conditions (the third condition was identical repeated). The first item appeared in uppercase. Although only Item 2 was selected to meet the criteria of low-string frequency (novel nonwords) or high-string frequency (word-part nonwords), attempts were made for the nonwords chosen as Item 1 and the nonrepeated control to have similarly low- or high-string frequencies.

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