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Effects of Lexicality and Distinctiveness on Repetition Blindness

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The repetition blindness (RB) paradigm developed by K. M. Arnell and P. Jolicœur (1997) was used to examine effects of lexicality (word vs. nonword target pairs) and target distinctiveness on RB. Distinctiveness was manipulated by having both targets (Experiments 1 and 2) or only the first target (Experiment 3) brighter than nontarget items. All 3 experiments demonstrated strong RB for word targets but no RB for nonword targets. This confirms that RB depends on pre-existing memory representations. In fact, there was repetition facilitation for nonwords in Experiments 2 and 3. These experiments also demonstrated that RB is reduced when targets are distinctive. This finding is better understood in terms of RB as a failure of memory rather than as a failure of perception.

During the last two decades, much has been learned about the basic mechanisms of visual attention by studying conditions under which specific perceptual or attentional processes fail (Coltheart, 1999a; Treisman & Kanwisher, 1998). One extensively studied phenomenon is repetition blindness (RB), which is a failure to recognize repetition of an item in a rapid visual series of stimuli. RB has been observed with rapid serial visual presentation (RSVP) displays of digits, letters, words, and pictures of objects with presentation rates of 4–12 items per second (Coltheart, 1999b). Kanwisher (1987, 1991) initially proposed the *token-individuation model* of RB, which continues to be a widely cited theoretical explanation (e.g., Bavelier, 1994; Bavelier & Potter, 1992; Chun, 1997; Coltheart, 1999c; Kanwisher, Kim, & Wickens, 1996; Kanwisher, Yin, & Wojciulik, 1999). Nonetheless, several researchers have presented evidence that RB has a memory locus rather than a perceptual locus (e.g., Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Whittlesea, Dorken, & Podrouzek, 1995). Here, we tested predictions of the token-individuation and memory accounts by examining RB for words and nonwords and by manipulating the visual distinctiveness of targets. We begin by briefly reviewing token-individuation and memory accounts of RB and then present the theoretical rationale for our experiments.

Perceptual Versus Memory-Based Theories of RB

Researchers proposing that RB has a perceptual locus have offered several possible mechanisms related to the token-individuation model proposed by Kanwisher (1987, 1991; cf. Hochhaus & Johnston, 1996). The token-individuation model proposed by Kanwisher implies that there are two primary stages involved in target recognition and report of stimuli in RSVP displays (see, e.g., Chun & Potter, 1995). Stage 1 involves rapid activation of pre-existing recognition units (e.g., phonological, orthographic, semantic, or object codes) associated with the stimulus. This activation of *type* representations provides information about the identity of a familiar object or word. In Stage 2, *tokens* are created that represent spatiotemporal and other episodic information about the visual stimulus. The type and token information are linked through type–token binding (also referred to as token individuation), which associates type activation processes with the appropriate spatiotemporal tokens. Token individuation requires focal attention and is assumed to be necessary for conscious report of stimuli.

According to Kanwisher's (1987, 1991) model, RB occurs because of limits affecting token individuation. Specifically, RB occurs when no token is created for the second instance of a repeated item. During a repetition trial, the first critical target (C1) will be both typed and token individuated. When this item is repeated as the second critical stimulus (C2), the repeated C2 is typed but not necessarily tokenized. The inability to token individuate a repeated item may occur because, when the display rate is very fast, the C2 type activation is not distinguished from residual C1 type activation. This limit on visual perception likely reflects that it is rare in nature for an object to suddenly disappear and be replaced instantly by an identical, but separate, object. Perception is more likely to be veridical if rapid visual events that activate the same recognition units are treated as a single stimulus (Chun & Cavanagh, 1997).

Several researchers have argued, however, that RB arises from a memory limit rather than a perceptual limit. For example, RB may be related to the Ranschburg effect (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995), a phenomenon that shows people are less likely to report a repeated item at even much slower item presentation rates than those used in typical RB paradigms (e.g., 500 ms per item). According to postperceptual accounts of RB, both repeated items are encoded, but there is a failure to recognize the two items as distinct at recall.

For example, Fagot and Pashler (1995) proposed that RSVP items typically are encoded in one or more storage formats. Some items may be registered in an articulatory store, others in visual or conceptual short-term memory systems. If two repeated occurrences are registered in a common store, they are easily recalled as

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distinct events. Repeated items registered in different stores, however, are difficult to distinguish from a single occurrence registered in multiple stores. This ambiguity reduces the probability that repeated items are recalled and reported as separate events. In a similar manner, Whittlesea et al. (1995) proposed that both occurrences of a repeated word are encoded, but there is not enough time within RSVP displays to encode the two occurrences distinctively by integrating them well with their separate contexts. Consequently, at recall, it is difficult to discriminate between the two representations, which reduces the probability that both occurrences of a repeated item are reported (see also Armstrong & Mewhort, 1995).

RB for Words Versus Nonwords

One purpose of the following experiments was to investigate RB for nonword stimuli. According to the token-individuation model, two identical type-activation events are interpreted as a single event under conditions of close temporal contiguity. The theoretical dependence on pre-existing types implies that RB should not be observed for nonwords, because they do not possess pre-existing orthographic or phonological representations. Thus, a basic prediction of the token-individuation model is that identical nonword pairs should generate no RB.

From the standpoint of memory theories, predictions with respect to RB for nonwords are less clear. If one assumes, however, that the failure to distinguish repeated items in short-term memory depends on pre-existing representations, then a memory theory may be consistent with the finding of no RB for nonwords. For example, people may be more likely to confuse two occurrences of a familiar item in short-term memory than two occurrences of a novel or unfamiliar item. Novel nonwords do not have well-established semantic or associative links; consequently, unlike repeated words, repeated nonwords do not activate overlapping associative or semantic structures in working memory. This may make it easier to keep track of repeated nonwords as distinct events in working memory; in contrast, the overlapping semantic or associative information activated by repeated words may make it more difficult to distinguish their episodic traces during recall. Although this is possible, the test of RB for nonwords is more telling with respect to the token-individuation model, because it explicitly relies on the activation of pre-existing type representations.

Surprisingly, there appears to be no published research examining RB for nonwords; however, Arnell and Jolicœur (1997, Experiment 3) conducted a conceptually similar experiment using nonsense pictures. They used novel pseudo-object stimuli to determine whether RB is obtained for pictorial items that have no existing phonological or semantic representations in long-term memory. In their experiments, two target pictures were presented among other pictures in RSVP streams. On half the trials, there was a repeated target, and on the other half there was no repetition. After each array of items, participants judged the frequency with which each of a set of response items had appeared within the stream (i.e., 0, 1, or 2 times). If the reactivation of a pre-existing type is necessary for RB, then RB should not have been found for nonobjects. Instead, Arnell and Jolicœur (1997) found significant levels of RB for the pseudo-object stimuli. On the basis of these findings, they argued that a type does not have to pre-exist but can be created on-line when the novel item is encountered.

Although the necessity of pre-existing representations for RB has been investigated for picture stimuli, it is worthwhile to pursue this issue in connection with nonword stimuli. First, both RB for words and RB for pictures of objects are sensitive to different influences. RB for words is associated with orthographic and phonological representations activated during word recognition. Bavelier (1999) reviewed research demonstrating that orthographic neighbors and identical words produce equivalent levels of RB. Orthographic RB apparently has a sublexical locus, perhaps arising at the level of individual letters (Harris & Morris, 2000; Morris & Harris, 1999). Pairs of homophones (i.e., words sharing the same phonology, such as *sun–son*) also produce RB. Thus, the type activation mediating RB for words can apparently involve either orthographic or phonological input lexicons, and token individuation involves binding of orthographic and/or phonological representations to episodic tokens. In contrast, neither semantic nor morphological similarity between pairs of words manifests RB. For example, Kanwisher and Potter (1990) found no RB with pairs of different words that were similar in meaning (*sofa–couch*). Bavelier, Prasada, and Segui (1994) found equivalent RB between morphologically related pairs and orthographic controls. Thus, the tokens mediating RB for words represent orthographic and phonological information but not semantic or morphological information. Yet C2 can produce semantic priming of a later word (Coltheart, 1999c; Shapiro, Driver, Ward, & Sorensen, 1997), confirming that its semantic representation is activated.

Whereas RB for words is not mediated by semantic relatedness, Kanwisher et al. (1999) demonstrated RB for pictures of two different exemplars from the same category (*cow–horse*). Furthermore, pictures of different objects with the same name did not produce RB, indicating no phonological RB for pictures of objects. These differences in RB between words and pictures of objects may reflect differences in the rates of activation of phonological and semantic codes by words and by pictures. Retrieval of object names from pictures may be relatively slow compared with retrieval of phonology from a written word. Conversely, semantic information about a picture or object may be extracted relatively quickly and associated with the picture's token (Coltheart, 1999b).

This brief review demonstrates that both RB for pictures and RB for verbal items are subject to different influences. Consequently, it is important to determine whether Arnell and Jolicœur's (1997) findings of RB for pseudo-object pictures also apply to pseudowords. Furthermore, as we explain later, a scoring artifact may have contributed to Arnell and Jolicœur's results (see the *Scoring* section). Thus, to investigate RB for nonwords, we adapted Arnell and Jolicœur's (1997, Experiment 3) procedure to examine RB for word–nonword stimuli and manipulated repetition (repeated vs. nonrepeated targets) and lexicality (word vs. nonword targets).

Target Distinctiveness and RB

We also manipulated a third factor, target distinctiveness. Specifically, targets were brighter than nontargets on half the trials, whereas on the other half, targets were the same brightness as the nontargets. For bright trials, therefore, targets were easily distinguishable from nontargets, and participants were specifically instructed that they would be tested on the bright items. Chun (1997) found that manipulating target–distractor distinctiveness (i.e., letter targets among digits vs. letter targets among keyboard symbols) had little effect on the magnitude of RB.

Nonetheless, one might expect RB to be increased by the brightness manipulation under the token-individuation model. If we assume that tokenization is costly in terms of attentional resources, then participants may be more likely to tokenize the bright targets, because they know that these items will be tested. If nontargets are less likely to be tokenized, however, then we would expect less RB in connection with these items, because C1 presumably must be tokenized for RB to occur (Arnell & Jolicœur, 1997). Thus, we expected greater RB when targets are bright relative to when all items are equally bright, if increasing the distinctiveness of targets and nontargets increases the probability of tokenization of C1.

In contrast, theoretical accounts that attribute RB to memory failures appear to predict opposite effects of making the two targets distinct from the nontargets. According to memory accounts, although both instances of a repeated target may be successfully encoded, it is difficult to recall them as separate events among the full set of items held in working memory (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995). This suggests that limiting the total number of targets needing attention to just two items increases the probability that repeated targets will be remembered as distinct events (i.e., assuming that they were encoded as distinct events in the first place). For trials with bright targets in Experiment 1, participants needed only to keep track of the two distinctive items, which should reduce the opportunity to confuse repeated targets during recall and thereby reduce RB (cf. Fagot & Pashler, 1995, p. 290).

In summary, Experiment 1 had two purposes. First, we wanted to determine if there is RB for nonwords, and second, we wanted to determine if RB increased or decreased when targets were easily distinguishable from nontargets.

Experiment 1

Method

Participants

A total of 58 volunteers (43 women and 15 men) participated as part of a research requirement for their introductory psychology course at the University of Saskatchewan. Their ages ranged from 18 to 40 years (M = 19.5). Informed consent was obtained from all participants. All participants reported normal or corrected-to-normal vision.

Stimuli and Design

Ninety-six sentences were used to construct the stimulus sequences (see Appendix A). Thirty-seven sentences were taken from Kanwisher and Potter (1990), and the other 59 were taken from Chialant and Caramazza (1997). Sentences ranged in length from 6 to 13 words, with a mean length of 9 words. All sentences included either two verbs or two nouns as the critical target words (C1 and C2). For repetition trials, the item nominally assigned as C2 appeared as both C1 and C2. The number of words that intervened between C1 and C2 (the lag) was controlled. Specifically, among the 96 sentences, 32 had a lag of one item, 32 had a lag of two items, and 32 had a lag of three items. A word was inserted between C1 and C2 in some sentences to complete the lag-of-three set. For each participant, sentences were randomly assigned to one of the eight cells defined by the Distinctiveness (targets bright or targets not bright) × Repetition (repeated targets or nonrepeated targets) × Lexicality (word targets or nonword targets) repeated measures design. Participants received two blocks of 96 trials. Each block included 12 trials representing each of the eight cells of the design, four at each of the three C1–C2 lags. Block 2 used the same core sentences as Block 1, but the opposite level of each factor was used. For example, a sentence assigned to the repetition, word target, bright target condition in Block 1 became a sentence in the no-repetition, nonword target, not-bright target condition in Block 2. The order of sentences within each block was independently randomized for each participant.

As each sentence was tested in word and nonword target conditions, both word and nonword targets (i.e., word and nonword versions of C1 and C2) were available for each sentence (see Appendix A). Nonword targets for repetition trials were constructed by rearranging the letters of the corresponding C2 word target (e.g., *barn* gave *narb*). If a pronounceable nonword could not be generated from the original C2 target word, a different word target was substituted, without altering the meaning of the sentence (e.g., changing *cab* to *taxi*). A nonword C1 for the no-repetition condition was constructed such that it had the same number of letters as C2 but no letters in common. Target distinctiveness was manipulated by having C1 and C2 brighter than the nontargets in the sequence on half the trials. For bright trials, targets appeared in bright white and nontargets appeared in a duller white. On not-bright trials, targets and nontargets all appeared in the same duller white. All items appeared in lowercase, and each character space was approximately 3 mm wide × 5 mm high. Apart from the target words in repetition trials, no other items were repeated within a trial.

After each RSVP display, participants saw three word items or three nonword items and indicated whether each item had occurred 0, 1, or 2 times in the preceding display. For no-repetition trials, the response items included C1 and C2 and a not-presented filler. For repetition trials, they included the repeated target (C1–C2) and two not-presented fillers. The two filler words generated for each sentence were always contextually relevant or semantically related to the targets (see Appendix A). When only one filler word was needed (i.e., for no-repetition trials), it was chosen at random from the two possibilities. Filler nonwords were selected randomly without replacement from the list shown in Appendix B, with the constraint that they were equal in length to the targets.

Procedure

Testing took place in a well-lit experimental room with an experimenter present. Stimuli were presented on a high-resolution monitor using an IBM PC. The screen refresh rate was 14.3 ms. The participant sat approximately 50 cm from the monitor. The experimenter and participant both had copies of the following written instructions, which the experimenter read out loud:

> This experiment is investigating basic processes of visual attention. You will receive two blocks of 96 trials. On each trial, a fixation dot will appear at the center of the screen for one second, then a dot will flash twice and be immediately followed by a very rapid series of words or word-like items presented one after the other at the center of the screen. On average, there are about 10 stimuli in each series. Your task on each trial is to indicate how often (0, 1 or 2 times) certain items appeared in the series. Bright white words will be presented among duller white words. You should try to keep track of these bright white words because you will be asked about them. Immediately following the series, three items will be listed and for each item you will be prompted to press 0, 1 or 2 to indicate the number of times that item appeared. Each possible response alternative (i.e., 0, 1 or 2) will be the correct response on some occasions. You should answer 0 if you don't think an item had been shown. A brief break will be given automatically half way through testing, but you may take a breather any time you wish. Please do not hesitate to ask any questions about the procedure, which will take about 20–25 minutes.

The participants initiated the first trial by pressing the space bar. A fixation dot then appeared at the center of the screen for 1 s, and then flashed off and on twice over a 1-s interval. The sentence appeared on what would have been the third flash in rapid serial visual presentation. The left-most character of each word appeared at the fixation dot. Stimuli were presented at 86 ms per item with a 0-ms interstimulus interval. After the last item, participants immediately saw the prompt *Were there 0, 1, or 2 appearances of the following items* . . . displayed near the top of the screen. The three response items were presented in a horizontal line across the middle of the screen. The left, middle, or right location of target and filler items was random. The participants responded from left to right, indicating 0, 1, or 2 by pushing those numbers on the keyboard. They then had the opportunity to hit the plus (+) key, which had to be pressed twice, to go on to the next trial, or the minus (−) key to erase and re-enter their responses. Once the plus sign was pressed twice, the fixation dot appeared for the next trial. There was no feedback about accuracy. A break was automatically given between the first and second blocks. There were no practice trials.

Scoring

Following Arnell and Jolicœur (1997), we adopted a scoring system (see Table 1) in which responses to the three response items on a single trial could contribute both to hits and to false alarms. Repetition and no-repetition trials required different scoring rules, because the correct response patterns were different. The correct response pattern for a repetition trial was C1–C2 = 2 and Fillers = 0. The correct response pattern for a no-repetition trial was C1 = 1, C2 = 1, and Filler = 0.

Hits and false alarms for repetition trials. For repetition trials, hits were counted for nonzero responses to the repeated item (i.e., C1–C2). The correct response *2* counted as 1 hit, a response of *1* counted as .5 hits, and a response of *0* counted as 0 hits. Thus, given random responding (i.e., if responses of *0, 1,* and *2* were equally likely), the expected number of hits per trial was .5 (i.e., the average of the three equiprobable scores of 0 hits, .5 hits, or 1 hit).

With respect to false alarms, responding *0, 1,* and *2* to each filler item counted as 0, .25, and .5 false alarms, respectively. For example, responding *1* to one filler and *2* to the other counted as .75 false alarms. Correct responses of *0* for both fillers counted as 0 false alarms, whereas responding *2* to both fillers counted as 1 false alarm. The expected false alarm rate was the sum of the expected values associated with each of the two fillers, which was .25 for each filler (i.e., the average of the three scores of 0, .25, and .5 false alarms). Thus, the expected false alarm rate given chance responding to a repetition trial was .5, matching the expected hit rate of .5.

Hits and false alarms for no-repetition trials. For no-repetition trials, hits were counted for nonzero responses to C1 and C2 (i.e., the response items corresponding to the nonrepeated targets); specifically, for each of C1 and C2, the correct response of *1* counted as .5 hits, *2* counted as .25 hits, and *0* counted as 0 hits. For example, responding *1* to C1 and *2* to C2 counted as .75 hits. Correct responses of *1* for both C1 and C2 counted as 1 hit, whereas responding *2* to both C1 and C2 counted as .5 hits. Note that the system for counting hits on no-repetition trials was symmetrical to that used for repetition trials. In both cases, a partial hit was credited for correctly discriminating 0 versus greater than 0 occurrences, and a full hit was credited for correctly discriminating 1 versus 2 occurrences. The expected hit rate per no-repetition trial was the sum of the expected values associated with each of C1 and C2, which was .25 for each target (i.e., the average of the three scores of 0, .5, and .25 hits). Thus, the expected hit rate given chance responding to a no-repetition trial was .5.

With respect to false alarms on no-repetition trials, responding *0, 1,* and *2* to the filler item counted as 0, .5, and 1 false alarms, respectively. Therefore, the expected false alarm rate for no-repetition trials was .5.

Our scoring of hits and false alarms for no-repetition trials differed from the formula used by Arnell and Jolicœur (1997, Experiment 3). Regarding hits in their system, responding *0, 1,* and *2* to C1 or C2 counted as 0, .5, and .5 hits, respectively. Therefore, the expected hit rate for each of C1 and C2 was .333 (the average of 0, .5, and .5), and the overall hit rate expected for each no-repetition trial was .667. For false alarms, responses of *2* to C1 and C2 each counted as .25 false alarms, and responding *0, 1,* and *2* to the filler item counted as 0, .25, and .5 false alarms, respectively. This yielded an expected false alarm rate of .417 given chance performance [i.e., 1/3(.25) for C1 + 1/3(.25) for C2 + .25 for the filler]. Thus, the expected difference between hits and false alarms was 25% (66.6% − 41.7%) for no-repetition trials. In contrast, the expected difference between hits and false alarms for repetition trials was 0% (50% − 50%). Thus, under the original Arnell and Jolicœur scoring system, no-repetition trials would appear to display greater sensitivity than repetition trials. This could masquerade as RB. Our alternative scoring system eliminates this asymmetry, and the expected values for hits and false alarms are exactly 50% for both repetition and no-repetition trials.

Table 1
Scoring of Hits and False Alarms

Item	Response 0	Response 1	Response 2	Expected
Repetition trial				
Hits C1–C2	0	.5	1	.5
False alarms				
Filler 1	0	.25	.5	.25
Filler 2	0	.25	.5	.25
				.5
No-repetition trial				
Hits				
C1	0	.5	.25	.25
C2	0	.5	.25	.25
				.5
False alarms				
Filler	0	.5	1	.5
No-repetition trial (Arnell & Jolicœur, 1997)				
Hits				
C1	0	.5	.5	.333
C2	0	.5	.5	.333
				.666
False alarms				
C1	0	0	.25	.083
C2	0	0	.25	.083
Filler	0	.25	.5	.250
				.416

Note. The correct response pattern for a repetition trial was C1–C2 = 2 and Fillers = 0. The correct response pattern for a no-repetition trial was C1 = 1, C2 = 1, and Filler = 0. C1–C2 = response item corresponding to the repeated target on the repetition trials; C1 = first target on the no-repetition trials; C2 = second target on the no-repetition trials; Filler = response item that had not been presented.

Results

All significance levels were less than .001 unless otherwise indicated. Appendix C presents mean percentages of the responses *0, 1,* or *2* given for C1–C2 (i.e., the repeated target) on repetition

Table 2
Mean Percentages of Hits Minus False Alarms and Accuracy as a Function of Lexicality, Target Brightness, and Repetition in Experiment 1

	Word targets			Nonword targets		
Condition	Bright	Not bright	Change	Bright	Not bright	Change
			Hits minus false alarms			
Repetition	57.0	50.3	+6.7	45.0	33.8	+11.2
No repetition	74.9	68.0	+6.9	43.1	33.4	+9.7
Change	−17.9	−17.7	−0.2	+1.9	+0.4	+1.5
			Accuracy			
Repetition	25.4	13.2	+12.2	15.7	4.4	+11.3
No repetition	61.3	50.7	+10.6	15.6	8.8	+6.8
Change	−35.9	−37.5	+1.6	+0.1	−4.4	+4.5

trials, for C1 and C2 on no-repetition trials, and to fillers for each Brightness × Lexicality × Repetition condition in Experiment 1.[1]

Hits Minus False Alarms

The overall mean hit and false alarm rates were 54.1% and 3.5%, respectively. We analyzed percentages of hits minus false alarms as a function of target distinctiveness (bright or not bright), lexicality (word or nonword targets), and repetition (repeated or nonrepeated targets) (see Table 2). Performance was substantially better overall with bright targets (55.0%) than not-bright targets (46.4%), $F(1, 57) = 74.89$, $MSE = 115.83$, and much better for words (62.5%) than for nonwords (38.8%), $F(1, 57) = 792.44$, $MSE = 82.46$. There was also evidence of RB, as indicated by relatively poor performance for repeated (46.5%) compared with nonrepeated targets (54.8%), $F(1, 57) = 73.25$, $MSE = 109.50$. These main effects were qualified by 2 two-way interactions, however. The Distinctiveness × Lexicality effect, $F(1, 57) = 9.20$, $MSE = 41.06$, $p < .01$, occurred because the performance facilitation for bright relative to not-bright targets was greater for nonwords (+10.5%) than for words (+6.9%). If we assume that nonword processing was generally slower than word processing, nonwords may have benefited more from distinctiveness, because bright targets produced more enduring visual representations or allowed attentional processes to lock on to the target more quickly. Such effects would have benefited performance most when generation of phonological or orthographic representations was relatively slow, as we expected them to be for unfamiliar nonwords.

There also was a large Lexicality × Repetition effect, $F(1, 57) = 191.36$, $MSE = 54.30$, resulting from strong RB for words but no evidence of RB for nonwords. Specifically, performance on word targets was substantially better for nonrepeated (71.5%) than for repeated targets (53.7%), confirming a strong RB effect (−17.8%). In contrast, performance for nonword targets did not differ between the repetition condition (39.4%) and the no-repetition condition (38.2%). A Distinctiveness × Repetition analysis of the nonword conditions indicated that the experiment had power of .8 to detect a nonword RB effect of 2.8% or larger, $F(1, 57) = 1.34$, $MSE = 57.31$, $p = .25$. Thus, absence of an RB effect for nonwords cannot be attributed to poor statistical sensitivity. The experiment had ample power to detect a very small RB effect in the nonword condition.

Whereas RB varied dramatically as a function of lexicality, there was no evidence in Experiment 1 that RB was affected by target distinctiveness. Neither the Distinctiveness × Repetition, $F(1, 57) = 0.31$, $MSE = 34.93$, nor the triple interaction of Distinctiveness × Lexicality × Repetition, $F(1, 57) = 0.46$, $MSE = 39.74$, approached significance. To assess the power of the experiment to detect a Distinctiveness × Repetition interaction, we conducted a separate Distinctiveness × Repetition analysis of the word conditions, as RB was observed only with word targets. For the interaction, the experiment had power of .8 to detect a difference in RB between bright and not-bright targets of 4.5% or larger, $F(1, 57) = .01$, $MSE = 36.84$. Thus, the experiment had adequate power to detect quite small effects of target distinctiveness on RB.

Accuracy

To confirm the foregoing results, we also conducted an analysis using an accuracy measure similar to that used in previous RB studies (e.g., Chialant & Caramazza, 1997; Kanwisher & Potter, 1990). Specifically, for no-repetition trials, we computed the percentage of trials on which both Cl and C2 received a score of *1* or *2*. For repetition trials, we computed the percentage of trials on which the repeated item received a score of *2*. Table 2 includes mean accuracy as a function of repetition, lexicality, and distinctiveness. The accuracy analysis produced a statistical pattern very similar to that observed in the analysis of hits minus false alarms;

[1] We do not report signal-detection analyses (i.e., A' or d') because these methods are designed for two-alternative forced-choice tasks, whereas our experiment involved three response alternatives (i.e., *0*, *1*, or *2*). In a similar manner, signal-detection-based analyses of bias (β) are not simply interpretable given three response alternatives. Also, although lag (1, 2, or 3 items between C1 and C2) was counterbalanced with the factors of distinctiveness, repetition, and lexicality, we do not include analyses of lag. Our theoretical rationale did not involve hypotheses that hinged on lag per se, and our experiments had low power to detect effects of lag. Once the data are broken down by lag, there are only eight observations per cell, and even with relatively large *N*s (greater than 50), power is very low.

the only difference was that the Distinctiveness × Lexicality effect was not significant in the accuracy analysis, $F(1, 57) = 3.12$, $MSE = 49.17$, $p = .08$. Overall, accuracy was higher with bright targets (29.5%) than with not-bright targets (19.3%), $F(1, 57) = 62.90$, $MSE = 193.27$, and higher for words (37.6%) than for nonwords (11.1%), $F(1, 57) = 347.32$, $MSE = 234.69$. RB was confirmed by lower accuracy for repeated (14.7%) compared with nonrepeated targets (34.1%), $F(1, 57) = 147.03$, $MSE = 297.92$. The Lexicality × Repetition effect, $F(1, 57) = 261.26$, $MSE = 133.09$, occurred because accuracy for word targets was substantially better for nonrepeated (56.0%) than for repeated words (19.3%), whereas accuracy for nonword targets was equivalent for repeated (10.1%) and for nonrepeated targets (12.2%). The accuracy analysis thereby confirmed strong RB for word targets and no RB for nonwords. As in the analysis of hits minus false alarms, neither the Distinctiveness × Repetition, $F(1, 57) = 2.81$, $MSE = 93.32$, $p = .10$, nor the triple interaction of Distinctiveness × Lexicality × Repetition, $F(1, 57) = 0.97$, $MSE = 61.46$, were significant.

Discussion

Experiment 1 was designed to address two theoretically important questions about RB: Is RB observed with nonword stimuli, and is RB affected by making the two targets distinct from nontargets? With respect to the effects of lexicality, the results were clear. Whereas word targets produced strong RB (−18% in the hits minus false alarms analysis), there was no evidence of RB for the nonwords (+1%). This is consistent with the hypothesis that RB depends on pre-existing memory representations. It is important to consider, however, that performance for nonword targets was quite poor compared with word targets. For example, performance for nonword targets in the not-bright condition was only about 33%, compared with 68% for words. Performance on nonwords may have been too low to observe RB: If participants frequently failed to encode C1 in the nonword condition, then there would have been reduced opportunities to observe repetition effects in connection with C2. One purpose of Experiment 2 was to increase nonword performance to levels equivalent with word performance in Experiment 1. To achieve this, we slowed the presentation rate from 86 ms to 114 ms per item. If we still observed little or no RB for nonwords under these conditions, the conclusion would be reinforced that differences in RB for words and nonwords are due to the presence and absence of pre-existing representations for words and nonwords, respectively.

Experiment 1 produced no evidence that manipulating target–distractor distinctiveness affected RB (cf. Chun, 1997). We argued that a token-individuation account predicts that RB should increase when targets are more distinctive, because this should increase the probability that C1 is encoded and thereby increase opportunities for failure to token-individuate C2. Appendix C confirms that on no-repetition trials, C1 was detected and reported more often when it was bright than when it was not bright. Nonetheless, and although the experiment had good power to detect such an effect, the relatively low level of performance may have made the experiment insensitive to the distinctiveness manipulation. In Experiment 2, in addition to slowing the presentation rate, we also made the bright targets more distinct by presenting them in bright yellow among dull white nontargets.

Experiment 2

Experiment 1, in summary, demonstrated no RB for nonwords, but performance for nonwords in Experiment 1 was quite poor. It is possible that no RB for nonwords is tied to their greater difficulty. Consequently, it is important to measure RB for nonwords under performance conditions similar to those under which RB is observed for words. One purpose of Experiment 2, therefore, was to improve performance in the nonword conditions; specifically, we slowed the per-item presentation rate to 114 ms in Experiment 2 from the 86-ms rate used in Experiment 1. A second purpose of Experiment 2 was to pursue the target distinctiveness manipulation of Experiment 1. Targets were brighter than nontargets on half the trials in Experiment 1, but this manipulation had no effect on RB, possibly because the distinctiveness manipulation of Experiment 1 was relatively weak. In Experiment 2, we strengthened the distinctiveness manipulation by having the targets appear in bright yellow (rather than a brighter white) on half the trials.

Method

A total of 51 volunteers (37 women and 14 men) were recruited as in Experiment 1. Their ages ranged from 18 to 34 years ($M = 19.9$). The method of Experiment 2 was the same as in Experiment 1, with three exceptions. First, the item presentation rate was 114 ms in Experiment 2, compared with 86 ms in Experiment 1. Second, the distinctiveness factor (targets brighter than nontargets vs. targets and nontargets the same dull white) was more strongly implemented by having the two targets appear in bright yellow on half the trials. Written and spoken instructions were modified accordingly: "Sometimes, bright yellow words will be presented among duller white words. You should try to keep track of these bright yellow words because you will be asked about them." The bright yellow targets stood out clearly in the RSVP stream and therefore should have potentially increased the potency of the distinctiveness manipulation. Finally, we replaced several of the nonword stimuli used in Experiment 1 that seemed particularly difficult to pronounce or that might have been pronounceable as an actual word. Specifically, we replaced *ovoh* with *levoh, dalbe* with *debla, dlise* with *desil, hulr* with *hule, wook* with *wouk, aldr* with *krad, lyf* with *vyf, rertal* with *tarler, onsoip* with *soonip, larsecl* with *clarsel, reagouc* with *guarace, slemls* with *sleams, tifl* with *tilf, agbr* with *barl, lek* with *kel, trafd* with *trafe, lalms* with *larms, wofl* with *wolk, mubth* with *mubet,* and *amr* with *arl*. The average time it took to complete the experiment was about 24 min.

Results

Appendix D presents the mean percentages of responses *0, 1,* or *2* given for C1–C2 on repetition trials, for C1 and C2 on no-repetition trials, and for fillers for each Brightness × Lexicality × Repetition condition in Experiment 2.

Hits Minus False Alarms

The overall hit and false alarm rates were 73.8% and 2.4%, respectively. As in Experiment 1, we analyzed percentages of hits minus false alarms as a function of target distinctiveness (bright or not bright), lexicality (word or nonword targets), and repetition (repeated or nonrepeated targets; see Table 3). Slowing the presentation rate and making the bright targets even more distinctive increased overall performance from 50.7% in Experiment 1 to 71.4% in Experiment 2. Furthermore, performance was substantially better overall with bright targets (79.2%) than with not-bright

Table 3
Mean Percentages of Hits Minus False Alarms and Accuracy as a Function of Lexicality, Target Brightness, and Repetition in Experiment 2

	Word targets			Nonword targets		
Condition	Bright	Not bright	Change	Bright	Not bright	Change
			Hits minus false alarms			
Repetition	83.2	69.2	+14.0	75.8	55.9	+19.9
No repetition	89.6	81.4	+8.2	68.0	48.3	+19.7
Change	−6.4	−12.2	+5.8	+7.8	+7.6	+0.2
			Accuracy			
Repetition	74.4	46.9	+27.5	63.6	32.1	+31.5
No repetition	84.9	69.5	+15.4	51.5	22.4	+29.1
Change	−10.5	−22.6	+12.1	+12.1	+9.7	+2.4

targets (63.7%) in Experiment 2, $F(1, 50) = 155.03$, $MSE = 157.81$. Indeed, the overall distinctiveness effect in Experiment 2 (+14.5%) was substantially larger than that in Experiment 1 (+8.6%), potentially increasing the opportunity to observe effects of distinctiveness on RB.

As in Experiment 1, performance for word targets (80.9%) was better than that for nonword targets (62.0%), $F(1, 50) = 295.56$, $MSE = 122.32$. It is important to note, however, that performance for nonwords in Experiment 2 was equivalent overall to performance on words in Experiment 1 (62.5%). Thus, we succeeded in elevating nonword performance to levels in which strong RB was observed for words in Experiment 1. Therefore, a failure to observe RB for nonwords in Experiment 2 could not be attributed to floor effects or other factors arising from low levels of performance.

In contrast with Experiment 1, there was no overall difference in performance between repeated and nonrepeated targets, $F(1, 50) = 0.62$, $MSE = 106.48$. This difference between the experiments arose from differences associated with the Lexicality × Repetition interaction. As in Experiment 1, there was a large Lexicality × Repetition effect in Experiment 2, $F(1, 50) = 159.80$, $MSE = 46.25$, but the pattern was different in an important way. As in Experiment 1, performance in Experiment 2 on word targets was better for nonrepeated (85.5%) than for repeated targets (76.2%), demonstrating RB (−9.3%) for the word targets. In contrast, performance in Experiment 2 on nonword targets was better in the repetition condition (65.9%) than in the no-repetition condition (58.2%). Thus, not only did the nonwords not produce RB, there was evidence of a repetition advantage of +7.7% for nonwords. The opposing effects of repetition for words (inhibition) and for nonwords (facilitation) explains why there was no overall difference between repetition and no-repetition trials in Experiment 2.

As in Experiment 1, the Distinctiveness × Lexicality effect in Experiment 2, $F(1, 50) = 43.19$, $MSE = 44.38$, occurred because the performance advantage for bright targets was greater for nonwords (+19.8%) than for words (+11.2%). There was also a Distinctiveness × Repetition effect, $F(1, 50) = 5.57$, $MSE = 40.43$, $p = .02$, which occurred because the performance advantage for bright targets was slightly greater for repetition trials (+17.0%) than for no-repetition trials (+14.0%).

These effects were qualified, however, by the significant three-way interaction, $F(1, 50) = 6.26$, $MSE = 31.66$, $p < .02$. To decompose the three-way effect, we performed separate Distinctiveness × Repetition analyses of variance (ANOVAs) on the word and nonword trials. The analysis of words confirmed an advantage for bright targets (+11.2%), $F(1, 50) = 98.99$, $MSE = 64.08$, an RB effect (−9.3%), $F(1, 50) = 66.45$, $MSE = 66.58$, and a Distinctiveness × Repetition interaction, $F(1, 50) = 15.01$, $MSE = 28.17$. As Table 3 shows, this interaction occurred because the word RB effect was smaller with bright targets (−6%) than with not-bright targets (−12%). The nonword analysis demonstrated an overall distinctiveness effect favoring bright targets (+19.8), $F(1, 50) = 145.09$, $MSE = 138.11$, a significant repetition advantage (+7.7%), $F(1, 50) = 35.19$, $MSE = 86.15$, but no Distinctiveness × Repetition interaction, $F(1, 50) = 0.01$, $MSE = 43.92$. Thus, the repetition advantage for nonwords did not vary with target distinctiveness.

Accuracy

Table 3 includes mean accuracy scores. A Distinctiveness × Lexicality × Repetition ANOVA of accuracy reproduced the same pattern of significant effects as did the analysis of hits minus false alarms. Accuracy for word targets (68.9%) was higher than that for nonword targets (42.4%), $F(1, 50) = 295.37$, $MSE = 243.09$. Note that nonword-target accuracy in Experiment 2 was equivalent to or slightly higher than word-target accuracy in Experiment 1 (37.6%). The Lexicality × Repetition effect, $F(1, 50) = 161.47$, $MSE = 119.36$, occurred because accuracy for word targets was higher for nonrepeated (77.2%) than for repeated targets (60.7%), whereas accuracy for nonword targets was higher for repeated (47.9%) than for nonrepeated targets (36.9%). Thus, repetition impaired accuracy for words but facilitated accuracy for nonwords. The accuracy facilitation for bright relative to not-bright targets was greater for nonwords (+30.3%) than for words (+21.4%), $F(1, 50) = 20.39$, $MSE = 98.25$. The accuracy advantage for bright targets also was somewhat greater for repetition trials (+29.5%) than for no-repetition trials (+22.2%), $F(1, 50) = 13.03$, $MSE = 104.63$.

As in the analysis of hits minus false alarms, these effects were qualified by the significant three-way interaction, $F(1, 50) = 8.16$, $MSE = 73.84$, $p < .01$. Separate Distinctiveness × Repetition ANOVAs of the word and nonword trials confirmed a Distinctiveness × Repetition interaction for word targets, $F(1, 50) = 20.50$, *MSE* = 92.16, but not for nonword targets, $F(1, 50) < 1$, *MSE* = 86.13. For word targets, RB was smaller with bright targets (−10.5%) than with not-bright targets (−22.6%).

Discussion

Experiment 2 demonstrated two theoretically important features of RB. First, whereas the word targets produced RB, there was no evidence of RB for nonwords in Experiment 2, even though nonword performance in the bright condition was at a level in which strong RB was observed for words in Experiment 1. Therefore, the absence of RB with nonwords cannot be attributed to floor effects or other factors associated with low performance. In fact, in contrast with RB, performance was better for repeated nonword targets relative to nonrepeated nonword targets. This effect might have occurred because two occurrences of a nonword target increased the chances that at least one of them was detected, and there was little or no RB to counteract this. Alternatively, there might have been repetition priming across repeated nonword items that permitted more-rapid construction of a phonological or orthographic representation of the second item.

Whereas the repetition advantage for nonwords did not vary with target brightness, the RB effect for words was smaller when the targets were bright. The fact that RB was observed only for words and that the Distinctiveness × Repetition interaction occurred only for words supports that the interaction reflects a genuine effect of distinctiveness on RB. Nonetheless, there may be reasons to doubt that this interaction represents less RB for bright targets. First, performance was very high for bright word targets in the no-repetition condition (hits minus false alarms = 90%). Because the no-repetition condition provides the baseline for RB, the performance being close to the ceiling of 100% raises the possibility that the Distinctiveness × Repetition interaction was due to a ceiling effect. Second, the reduced RB with bright targets might be attributed to the fact that making C2 bright simply makes it easier to attend to and remember C2 independently of C1 (cf. Chun, 1997).

To resolve these issues, we conducted a third experiment identical to Experiment 2, except that on bright target trials, only C1 was bright; thus, we eliminated any effects that having C2 bright might have had on RB in Experiment 2. Furthermore, making C2 the same dull white as the nontargets should reduce performance and thereby reduce the possible contribution of ceiling effects. Chun (1997, Experiment 3) eliminated RB by enhancing the episodic distinctiveness of targets—specifically by presenting letter targets in different colors (red and green) among black letter distractors. This is consistent with a token-individuation model, because making the targets visually distinct from one another should enhance token individuation and reduced RB. The situation in our Experiment 3 was different from Chun in a critical way, however. In Chun, both C1 and C2 were visually distinct from distractors, and therefore both targets would have popped out in the RSVP stream. Presumably, this greatly enhanced their episodic distinctiveness. In contrast, because only C1 was bright in the following experiment, bright trials provided little enhanced episodic distinctiveness between C1 and C2. Instead, the primary effect should have been to increase the probability that C1 was encoded on bright trials. Under the token-individuation model, this should tend to increase RB.

In contrast, memory-based accounts again predict reduced RB. For example, according to Fagot and Pashler (1995), RSVP items may be stored in multiple formats (e.g., articulatory, phonological, and visual), and RB is reduced when repeated items are encoded in a common form. When C1 is bright and C2 is not bright, we can expect that C1 will activate more encodings than C2. Nonetheless, because there is a high probability that a bright C1 is encoded in multiple formats, it is more likely that both C1 and C2 will be registered in a common form when C1 is bright than when it is not bright. Therefore, we expected reduced RB when C1 was bright according to Fagot and Pashler's theory. That is, making a to-be-reported item distinctive should reduce the number of items to keep track of in working memory and should reduce opportunities to confuse C1 and C2 during recall.

Experiment 3

Method

A total of 57 volunteers (44 women and 13 men) were recruited as in the previous experiments. Their ages ranged from 17 to 38 years ($M = 19.4$). The method of Experiment 3 was the same as in Experiment 2, with the exception that on bright target trials, only C1 was bright. C2 always appeared in the same dull white as the nontargets in the RSVP stream.

Results

Appendix E presents the mean percentages of responses *0, 1,* or *2* given for C1–C2 on repetition trials, for C1 and C2 on no-repetition trials, and for fillers for each Brightness × Lexicality × Repetition condition in Experiment 3.

Hits Minus False Alarms

The overall hit and false alarm rates were 66.1% and 2.4%, respectively. Table 4 presents the mean percentages of hits minus false alarms as a function of target distinctiveness (C1 bright or not bright), lexicality (word or nonword targets), and repetition (repeated or nonrepeated targets). Performance was better overall with C1 bright (65.2%) than with not-bright targets (62.3%), $F(1, 56) = 16.10$, *MSE* = 57.00, but the overall distinctiveness effect (+2.8%) was very small compared with Experiment 2 (+14.5%). There was also a Distinctiveness × Repetition effect, $F(1, 56) = 19.06$, *MSE* = 43.60, which occurred because the performance advantage with C1 bright occurred only for repeated targets (+5.5%) and not for nonrepeated targets (+0.1%). The negligible overall effect of distinctiveness suggests that performance on C2 in Experiment 3 was suppressed when C1 was bright. Examination of performance on C1 and C2 in the no-repetition trials confirms this (see Appendix E). Whereas detection of C1 (i.e., responses of *1* or *2*) was better when it was bright (87.2%) than when it was not bright (75.1%), performance on C2 was lower (48.3%) when C1 was bright than when it was not bright (59.6%). These counteracting effects resulted in little overall benefit of having C1 bright.[2]

[2] For no-repetition trials to provide an appropriate baseline for measuring RB, we must assume that the factors underlying these performance differences on C1 and C2 in the no-repetition trials also affect repetition trials similarly. This is true of all RB experiments.

Table 4
Mean Percentages of Hits Minus False Alarms and Accuracy as a Function of Lexicality, Target Brightness, and Repetition in Experiment 3

	Word targets			Nonword targets		
Condition	Bright	Not bright	Change	Bright	Not bright	Change
			Hits minus false alarms			
Repetition	72.1	68.6	+3.5	59.3	51.8	+7.5
No repetition	78.0	80.5	−2.5	51.1	48.4	+2.7
Change	−5.9	−11.9	+6.0	+8.2	+3.4	+4.8
			Accuracy			
Repetition	51.1	44.2	+6.9	34.5	28.2	+6.3
No repetition	64.2	69.4	−5.2	18.6	21.0	−2.4
Change	−13.1	−25.2	+12.1	+15.9	+7.2	+8.7

As in Experiment 2, performance was much better overall for word targets (74.8%) than for nonword targets (52.7%), $F(1, 56) = 511.21$, $MSE = 109.00$, and there was a large Lexicality × Repetition interaction, $F(1, 56) = 102.05$, $MSE = 59.62$: Performance on word targets was better for nonrepeated (79.2%) than for repeated targets (70.4%), whereas performance was worse for nonrepeated nonword targets (49.8%) than for repeated nonword targets (55.5%). The −8.8% RB effect for word stimuli was similar in magnitude to that observed with the word stimuli in Experiment 2 (−9.3%). The +5.7% facilitation for repeated relative to nonrepeated nonwords was also similar to the corresponding effect in Experiment 2 (+7.7%). Thus, all three experiments found clear evidence of RB for words but no evidence of RB for nonwords.

It is important to note that the experiment replicated the Distinctiveness × Lexicality effect found in Experiment 2, $F(1, 56) = 11.93$, $MSE = 50.48$, and a separate Distinctiveness × Repetition ANOVA of the word trials confirmed the interaction for word stimuli, $F(1, 56) = 15.36$, $MSE = 33.12$. As Table 4 shows, this interaction occurred because the word RB effect was smaller with C1 bright (−6%) than with C1 not bright (−12%). The magnitudes of these RB effects were similar to Experiment 2 (as can be seen in a comparison of Tables 3 and 4). Furthermore, the interaction cannot be attributed to a ceiling effect, because performance on the no-repetition baseline trials was practically equivalent for word targets in the bright (78.0%) and in the not-bright (80.5%) conditions. Experiment 3 indicates, therefore, that increasing the detectability of C1 decreases the magnitude of RB. As we discuss below, this result appears to be more consistent with a memory-based than a perceptual account of RB.

In contrast with Experiment 2, however, there was also a Distinctiveness × Repetition effect for the nonwords, $F(1, 56) = 6.43$, $MSE = 51.61$, $p = .01$, and there was no evidence for the Distinctiveness × Lexicality × Repetition interaction observed in Experiment 2, $F(1, 56) = 0.23$, $MSE = 41.13$. We are inclined to think that the Distinctiveness × Repetition interaction for nonwords is a Type I error. A comparison of the nonword means in Table 4 (Experiment 3) and in Table 3 (Experiment 2) shows that the repetition facilitation effect was essentially the same for the bright condition in the two experiments (+8.2% and +7.8%), whereas the effect with not-bright targets tended to be smaller in Experiment 3 (+3.4%) than that in Experiment 2 (+7.6%). In other words, the difference between the nonword conditions in the two experiments was associated with the not-bright conditions, which were methodologically identical in the two experiments. Moreover, when the data from Experiments 2 and 3 were combined, the triple interaction was present in the hits minus false alarms analysis, $F(1, 107) = 3.84$, $MSE = 36.94$, $p = .05$.

Accuracy

Table 4 includes mean accuracy scores as a function of target distinctiveness, lexicality, and repetition. Unlike the analysis of hits minus false alarms, there was no overall effect of distinctiveness, $F(1, 56) = 2.57$, $MSE = 87.93$, $p = .12$, and no Distinctiveness × Lexicality interaction, $F(1, 56) < 1$, $MSE = 67.21$. All effects involving the repetition factor, however, were the same in the two analyses. The Distinctiveness × Repetition effect, $F(1, 56) = 24.27$, $MSE = 127.42$, occurred because there was an accuracy advantage with C1 bright for repeated targets (+6.6%) but not for nonrepeated targets (−3.8%). Accuracy was much higher overall for word targets (57.2%) than for nonword targets (25.6%), $F(1, 56) = 367.73$, $MSE = 310.22$, and there was a large Lexicality × Repetition interaction, $F(1, 56) = 159.94$, $MSE = 168.36$. Specifically, accuracy for word targets was higher for nonrepeated (66.8%) than for repeated targets (47.6%), whereas accuracy was lower for nonrepeated nonword targets (19.8%) than for repeated nonword targets (31.4%). As in the analysis of hits minus false alarms, the overall Distinctiveness × Repetition effect was significant, $F(1, 56) = 24.27$, $MSE = 127.42$, but unlike in Experiment 2, there was no three-way interaction, $F(1, 56) = 1.00$, $MSE = 84.41$. When the data from Experiments 2 and 3 were combined, however, the triple interaction was present in the accuracy measure, $F(1, 107) = 6.93$, $MSE = 79.93$, $p = .01$.

General Discussion

Lexicality and RB

Our experiments confirmed under conditions that produced strong RB for word targets that there was no RB for nonword targets. This finding is expected according to the token-individuation model of RB (e.g., Chun & Potter, 1995; Park & Kanwisher, 1994). In this view, when a type node has been tokenized in connection with C1, it is briefly unavailable for a second token individuation. Consequently, in an RSVP display in which C2 is identical to C1, C2 may not be tokenized, leaving the participant unaware of the repeated item. A critical feature of this account is the assumption that RB reflects a failure to token-individuate C2 because the recognition units it activates (i.e., type information) cannot be distinguished from type information activated by C1. This implies that if there is no pre-existing type in memory, there should be no RB. That is, tokenization of C2 fails because the similar type activation produced by C1 and C2 is difficult to discriminate from the activation produced by a single event.

This view of RB accounts naturally for the findings of RB for words and no RB for nonwords: Recognition of words exploits type–token binding, and thus, failures to tokenize repeated words reduce performance relative to nonrepeated words. In contrast, because nonwords do not activate pre-existing recognition units, there is nothing readily available to bind tokens to, and thus, detection and recall of nonwords cannot exploit type–token bindings. Consequently, there is no performance deficit for repeated relative to nonrepeated nonword targets. Indeed, performance was better for repeated nonword targets than for nonrepeated nonword targets. This effect may have occurred because two occurrences of a nonword target increased the chances that at least one of them was detected, and there is little or no RB to counteract this repetition facilitation effect.

The finding of no RB for nonwords is clearly predicted by the token-individuation model, but this result is also potentially consistent with a memory-based account. According to memory-based theories of RB, both instances of a repeated item are encoded successfully but are confused during subsequent recall and report. With respect to the finding of RB for words but not for nonwords, people may be likely to confuse two occurrences of a familiar word in short-term memory but unlikely to confuse two occurrences of a novel nonword item. Nonwords do not activate well-established semantic or associative links; consequently, unlike repeated words, repeated nonwords are unlikely to activate overlapping associative or semantic retrieval cues in working memory. This may make it easier to keep track of repeated nonwords as distinct events in working memory, whereas the common, overlapping retrieval structures activated by repeated words make it difficult to distinguish their episodic traces during recall.

Distinctiveness and RB

Whereas the effects of lexicality on RB are potentially consistent with both perceptual and memory-based accounts of RB, the effects on RB of making C1 bright in Experiments 2 and 3 are more easily reconciled with a memory theory. Specifically, the magnitude of RB was smaller when C1 was distinct and therefore more likely to be attended to and encoded. According to token-individuation theory, making C1 distinct should have increased the probability that C1 was tokenized; therefore, we expected greater RB when targets were bright relative to when all items were equally bright. In contrast, according to a memory account, increasing the distinctiveness of one or both targets should have increased the probability that repeated targets were encoded and remembered as distinct events. Increasing target distinctiveness therefore should have reduced RB.

The results of both Experiments 2 and 3 support the latter prediction; RB was smaller when both C1 and C2 were bright (Experiment 2) and when only C1 was bright (Experiment 3). We have argued that this effect is predicted by memory-based theories of RB, but can it be reconciled with the token-individuation model? Chun (1997, Experiment 3) found that presenting letter targets in different colors (red and green) among black letter distractors eliminated RB. He argued that this was consistent with the token-individuation model, because enhancing the episodic distinctiveness of targets should facilitate token individuation and reduce RB. In a similar manner, if the brightness manipulation in the present experiments enhanced the episodic distinctiveness of C1 and C2, then reduced RB with bright targets relative to not-bright targets would be consistent with the token-individuation model.

The results of Experiment 3 seem to rule out this possibility, however. On bright trials in Experiment 3, only C1 was bright and C2 appeared in the same dull white as the nontargets. Having C1 bright on word trials cut RB in half relative to not-bright trials (Table 4), but there was little difference in performance overall between bright and not-bright trials. If making C1 bright substantially increased the episodic distinctiveness of C1 and C2, then we would expect overall performance to improve substantially. Instead, as demonstrated by the no-repetition trials, having C1 bright facilitated C1 processing but impaired C2 (Appendix E). Thus, having C1 bright reduced RB, even though it also tended to interfere with performance on C2. This makes it difficult to argue that reduced RB in the bright condition occurred because episodic distinctiveness enhanced detection of C2. Given that only C1 performance was enhanced, it follows that the token-individuation model should predict increased RB rather than reduced RB, as observed. The results therefore appear to be more consistent with memory-based theories of RB.

RB for Nonwords Versus Pseudo-Object Pictures

In contrast to our finding of no RB for nonwords, Arnell and Jolicœur (1997, Experiment 3) did find evidence of RB with pseudo-object pictures using a similar paradigm. This raises the possibility that type representations can be created on-line for pictures of novel objects but not for novel words. It is also possible, however, that Arnell and Jolicœur mistakenly concluded there was RB in their Experiment 3. As explained previously, their system for scoring hits and false alarms incorporated a bias that would promote the appearance of RB. Nonetheless, they also observed RB in analyses of accuracy (i.e., percentages correct) that did not depend on their method of scoring hits and false alarms. Furthermore, their Experiment 4 also indicated RB for pseudo-objects, and this experiment did not depend on the scoring system used for Experiment 3. Thus, despite the scoring difficulties associated with Arnell and Jolicœur, their results collectively still suggest that RB is obtained for novel pseudo-objects. The evidence that novel words produce no RB while novel objects do extends the range of differences found in factors that affect RB for verbal materials versus those that affect RB for pictorial materials (see Coltheart, 1999b).

Conclusions

Our finding that RB does not occur for novel words confirms that RB for familiar words depends on activation of pre-existing type information. This result is potentially consistent with both perceptual and memory-based theories of RB, although it seems to be more clearly predicted by perceptual accounts such as the token-individuation model and related explanations (e.g., Chun, 1997; Chun & Potter, 1995; Kanwisher, 1987; Luo & Caramazza, 1996). It is not clear, however, how to reconcile the absence of RB for nonwords with claims that orthographic RB has a sublexical locus, perhaps arising at the level of individual letters (Harris & Morris, 2000; Morris & Harris, 1999). Our results suggest that even if RB arises from processes that affect sublexical components, those processes only produce RB when the entire stimulus has a pre-existing lexical entry. We should be cautious about concluding that there are no conditions under which RB would be observed with nonwords. The evidence of RB for pseudo-object pictures (Arnell & Jolicœur, 1997) indicates that type recognition units do not necessarily need to preexist for RB to occur. Furthermore, our finding of repetition facilitation for nonwords in Experiments 2 and 3 indicates that finding RB for nonwords is complicated by counteracting facilitation effects of repetition. One important goal for future research should be to attempt to isolate the inhibitory and facilitatory effects of repetition in this paradigm.

Finally, our experiments demonstrated that increasing the detectability of C1 (without necessarily enhancing the episodic distinctiveness of C1 and C2; cf. Chun, 1997) can reduce RB. This was observed when both C1 and C2 (Experiment 2) were distinct from nontargets and when only C1 was distinct (Experiment 3). In perceptual accounts, RB depends on C1 being encoded and tokenized; consequently, increasing the probability that C1 is tokenized should increase RB. In contrast, flagging targets by making them distinct should reduce RB according to memory-based accounts, because this would reduce the number of items to keep track of in working memory. In balance, therefore, our distinctiveness results appear to be more consistent with memory-based than with perceptual accounts of RB.

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Appendix A

Stimulus Sentences Used in the Experiments

Below are the 96 stimulus sentences used in the experiments. Bracketed items following each sentence correspond to C1 and C2 word targets, word filler items, and C1 and C2 nonword targets, respectively. For repetition trials, C2 was repeated. Items identified with an asterisk are from "Repetition Blindness: Levels of Processing," by N. G. Kanwisher and M. C. Potter, 1990, *Journal of Experimental Psychology: Human Perception and Performance, 16,* pp. 44–46. Copyright 1990 by the American Psychological Association. Adapted with permission of the author. The unmarked items are from "Identity and Similarity Factors in Repetition Blindness: Implications for Lexical Processing," by D. Chialant and A. Caramazza, 1997, *Cognition, 63,* pp. 104–116. Copyright 1997 by Elsevier Science. Adapted with permission.

1. after checking his old C1 the C2 was closed [cow barn] [oxen hay] [gelf narb]
2. while stepping over a C1 the C2 popped up [sewer grate] [metal gutter] [flumb trage]
3. as i looked at his C1 my C2 got bigger [smirk grin] [smile sneer] [zape nirg]
4. when in need to C1 they C2 for two hours [relax rest] [calm recline] [fady erst]
5. when parents C1 they C2 their children gently [punish slap] [spank smack] [herg plas]
6. as i C1 they C2 their dirty clothes [wash clean] [scrub filthy] [forob lecan]
7. i had so much C1 and C2 is bad [avidity greed] [envy wrong] [choth dreeg]
8. after sharpening my C1 the C2 still looked dull [skate blade] [edge blunt] [lovoh dalbe]
9. i modelled her C1 his C2 and the hand [nose scalp] [cheek eyes] [ferge plasc]
10. when i start to C1 they C2 with me [sweep dust] [polish broom] [gorp tuds]
11. even when trying not to C1 they C2 on ice [stumble slide] [skid slick] [trona dlise]
12. when training to C1 they C2 stones [heave hurl] [throw fling] [tafe hulr]
13. i want to C1 and C2 immediately [compare rent] [buy contrast] [wook nert]
14. paper boats will C1 and C2 quickly [flip sink] [drop fall] [aldr insk]
15. he piles C1 upon C2 on his truck [blocks tires] [bricks logs] [fuflo stire]
16. they wish to C1 and C2 alone [go fly] [soar sail] [zun lyf]
17. have them C1 then C2 everyone else [pass board] [plane load] [snife dorab]
18. the box of C1 has C2 painted on it [cigarettes matches] [smokes lighter] [blurgoz chemsat]
19. the company's new C1 might C2 many people accidentally [product poison] [toxin venom] [rertal onsoip]*
20. we worked in the C1 until C2 covered our clothes [garden dirt] [clay soil] [meep trid]*
21. the slaves wanted C1 although C2 wasn't attainable [money freedom] [rescue liberty] [bliskil dreefom]*
22. she was terrified in C1 because C2 have spiders [basements cellars] [downstairs pantry] [bittuzi larsecl]*
23. we bought the C1 while C2 was on sale [pattern cloth] [fabric sewing] [bakis tholc]*
24. when he lost his C1 suddenly C2 seemed very important [glasses vision] [eyesight lens] [zemtal sivion]*
25. she always manages to have C1 where C2 is required [strength courage] [bravery nerve] [blikily reagouc]*
26. the worst C1 are C2 at school cafeterias [memories smells] [lunch stink] [torday slemls]*
27. we added C1 to C2 in the bowl [peas carrots] [corn beets] [zilm sepa]*
28. they asked C1 although C2 were unwelcome [things questions] [inquired unanswered] [lamiziwac stoquesti]*
29. to use C1 the C2 must have batteries [headphones radios] [walkman stereo] [wazuch siodar]*
30. there wasn't much C1 although C2 was desired [wine water] [liquor thirsty] [zuplo terwa]*
31. her jacket was C1 because C2 is conspicuous [pink red] [blush ruby] [jun der]*
32. we were C1 although C2 was unnecessary [dining eating] [dessert supper] [lopobo geatin]*
33. my favorite C1 killed another C2 in the tank [bass fish] [hook lake] [porl shif]
34. my socks C1 but they C2 only to their mate [stick cling] [static pair] [farbe glinc]
35. when i C1 the books C2 the shelf [arrange lift] [adjust hoist] [zeeg tifl]
36. some women like to C1 hair and C2 eyelashes [comb curl] [face perm] [neek clur]
37. i had a C1 and the C2 was correct [sensation hunch] [insight notion] [blord chunh]
38. skirts are a new C1 after the C2 for shorts [fad craze] [idea rage] [bluny zarce]
39. i found a red C1 and orange C2 beside me [candy leaf] [bench purple] [vors efla]
40. i will first C1 burgers then C2 hot dogs [broil flip] [buns grill] [ruve plif]
41. i will gladly C1 before you C2 for the picture [perform pose] [flash dance] [marb sepo]
42. when we C1 books they C2 the paper [grasp grab] [clutch data] [moop agbr]
43. to C1 well they C2 mentally [add count] [math sum] [greep contu]
44. set the C1 by that C2 on my table [time watch] [clock stand] [lurpe chawt]
45. a horse that hated its C1 spat the C2 out [food bit] [bridle oats] [oog tib]
46. he noticed my C1 as the C2 looked new [tool saw] [screw axe] [hig swa]
47. some workers C1 slowly but C2 well [hammer pound] [nail fist] [herge dopun]
48. he C1 conductors and C2 them well [tests trains] [teach tutor] [guffle snarit]
49. we saw an C1 though the C2 was far away [animal elk] [deer fox] [maz lek]
50. when paul C1 coffee he C2 it finely [prepared ground] [beans perked] [mambet droung]
51. we were anxious for C1 well before C2 arrived [apples autumn] [winter spring] [weeble nutuma]*
52. what does it C1 if you C2 to me [mean matter] [value worth] [conodo tretam]

(*Appendixes continue*)

53. the new C1 worked with C2 who could help them [girls students] [physics pupil] [wabornia stentuds]*
54. his C1 was a C2 of hard work and success [life tale] [fate pain] [porp leat]*
55. his C1 are long C2 about war [favorites films] [evil video] [tordy smilf]*
56. we couldn't see a C1 because the C2 was being rearranged [painting display] [artwork sculptor] [bleetor plisday]*
57. the administrator demanded a C1 although no C2 was forthcoming [raise reply] [answer award] [sobin prely]*
58. he poured in C1 until the C2 reached a liter mark [oil fluid] [gas quart] [gleep lufid]*
59. we will go C1 whenever good C2 is available [visit skiing] [snow route] [anobat isking]*
60. yesterday's C1 and this C2 were truly disgusting [pie soup] [salad dish] [kilk pous]*
61. the C1 weather was C2 than last summer even [bad hotter] [heat scorch] [jablip roteth]*
62. he chased her C1 and the C2 ran away [dog cat] [car fled] [riv zat]*
63. when she spilled the C1 there was C2 all over [liquid ink] [splash pen] [wab nid]*
64. we got into this C1 and another C2 for the commute [vehicle van] [drive bus] [moj nav]*
65. my C1 soared over his C2 last term [standing rank] [station order] [gleg karn]
66. i copied a C1 from the rough C2 yesterday [essay draft] [story trace] [leeze trafd]
67. i cut the C1 and one small C2 today [hedge shrub] [plant bush] [druve lalms]
68. water will first C1 fast but then C2 slowly [squirt flow] [spray rush] [narn wofl]
69. artists very often C1 water colors to C2 their paintings [darken shade] [tinge cloud] [hilgy desha]
70. adults never C1 chairs but children C2 them often [whirl spin] [twirl rotate] [yorg insp]
71. my left C1 ached but his C2 was feeling better [elbow heel] [knee sore] [kurm lehe]
72. the chocolate C1 and white coconut C2 were both good [frosting glaze] [icing lemon] [sonta zlage]
73. my C1 and his left C2 were hurt [elbow thumb] [wrist ankle] [snige mubth]
74. to C1 behaviors one must C2 early childhood actions [mould shape] [forge model] [murli peash]
75. if you suddenly C1 then i will C2 too [trip slip] [fast shift] [zarn plis]
76. to pick the C1 she put her C2 down [flower rose] [tulip posy] [plub reso]
77. she dropped her C1 when taking the C2 off [keys ring] [bell chain] [ploo nirg]
78. there was a C1 but soon the C2 went out [alert fire] [alarm blaze] [buld refi]
79. i C1 once and then C2 again [push press] [mash thrust] [gloob sreps]
80. laws come to this C1 before the other C2 approves them [congress court] [document judge] [fifle trouc]
81. her right C1 and his left C2 were broken [foot arm] [leg toe] [pib amr]
82. she came to C1 them but her C2 was unwanted [rescue help] [save assist] [drok pelh]
83. we had to C1 the dog and C2 him well [brush groom] [stroke bathe] [fufle gorom]
84. i C1 twice and then C2 once more [knock beep] [thump bang] [grud peeb]
85. there was a C1 but soon the C2 sold out [line movie] [flick cinema] [natun viemo]
86. those who tried to C1 me last year C2 students frequently [hurt scare] [bully shock] [toupl cresa]
87. the C1 store sold a C2 from way far east [department rug] [carpet west] [sal urg]*
88. she wandered along the C1 before discovering a C2 home [beach path] [sand trail] [smib thap]*
89. that C1 passed by our C2 very quickly [truck cab] [semi taxi] [gix dac]*
90. the birthday C1 was an unexpected C2 from his parents [cake gift] [mail tape] [plob figt]*
91. the squirrel had just hopped from one C1 to that other C2 nearby [branch tree] [limb leap] [dilb reet]*
92. to drink C1 you will need C2 with twist tops [them beers] [bottle glass] [vilmi sreeb]*
93. she read C1 whenever really good C2 came her way [stories books] [novel volume] [hutte skobo]*
94. the brown C1 and a black C2 were stolen [sofa couch] [ebony lounge] [bling chouc]*
95. his collection of C1 will include more C2 about nature [things art] [earth hobby] [hoy tra]*
96. they read C1 about travel and C2 of history [articles books] [space world] [clute skoob]*

Appendix B

Nonword Fillers Used in Experiment 1

amry, antu, brae, doby, buze, cish, caly, cypo, dymi, vode, dewm, etco, caft, fasy, folu, firk, geft, glof, hira, heor, hepo, itno, jyru, lemp, lano, smas, mati, nila, teno, orve, sape, reka, rige, relu, sohe, safo, losu, tusi, telp, turo, utni, voet, wral, yird, acdi, lant, beto, obok, nect, celd, tade, doro, duby, eged, fera, freg, lahl, heda, lihl, eadi, claf, leni, slos, culk, mide, misk, swun, pega, taph, piln, pulm, lopo, rega, riks, miro, sede, seta, slun, snog, sete, tepa, tets, tolk, tepy, vete, wrie, wolo, dwin, alvog, brith, cinah, ceths, dontu, cevru, derss, ertah, fitha, frest, govel, holet, lithg, mojar, mesou, nesru, oneon, pesal, pinao, porof, rhane, rilef, sevel, sheck, sedli, sepit, stels, soret, tethe, terch, trake, turch, vitis, wetar, wahte, anget, berak, bolce, crath, sclas, casto, dirll, evten, foldo, gosit, geren, hesou, jicue, mepla, meyon, nisoe, fofer, prape, peyne, pelan, negue, ripad, rutoe, saher, sorth, silan, sporp, selet, temeh, tochu, terno, udner, vewol, wehla, wilod, aticon, cehuch, eatest, lareed, morhet, pippra, ruselt, stirse, symlob, wido, bettib, dernag, fitheg, ganlec, halden, nitoon, presti, spelma, setris, veylal, cherbam, etidino, masuree, ralmevo, sintato, jaldurn, otuloko, pargmor, tublore, warfeel, dreknass, senlenet, murtaity, and retavile

Appendix C

Mean Percentages of Responses 0, 1, or 2, to C1, C2, and Fillers, for Each Brightness × Lexicality × Repetition Condition in Experiment 1

Repetition, lexicality, and response item	Response 0	1	2
Bright			
Repetition, word targets			
C1–C2	3.4	71.3	25.4
Fillers	92.2	7.7	0.1
No repetition, word targets			
C1	17.5	81.3	1.3
C2	23.8	74.9	1.3
Filler	92.5	7.3	0.2
Repetition, nonword targets			
C1–C2	20.1	64.2	15.7
Fillers	94.3	5.7	0.0
No repetition, nonword targets			
C1	46.4	52.4	1.2
C2	60.0	39.4	0.6
Filler	93.5	6.4	0.1
Not bright			
Repetition, word targets			
C1–C2	4.9	82.0	13.2
Fillers	92.4	7.6	0.1
No repetition, word targets			
C1	23.3	76.0	0.9
C2	31.4	67.5	1.1
Filler	91.7	8.1	0.1
Repetition, nonword targets			
C1–C2	32.0	63.7	4.4
Fillers	95.4	4.4	0.2
No repetition, nonword targets			
C1	58.5	41.2	0.4
C2	68.2	31.5	0.3
Filler	93.7	6.3	0.0

Note. The correct response pattern for a repetition trial was C1–C2 = 2 and Fillers = 0. The correct response pattern for a no-repetition trial was C1 = 1, C2 = 1, and Filler = 0. C1–C2 = response item corresponding to the repeated target on the repetition trials; C1 = first target on the no-repetition trials; C2 = second target on the no-repetition trials; Filler = response item that had not been presented.

Appendix D

Mean Percentages of Responses 0, 1, or 2, to C1, C2, and Fillers, for Each Brightness × Lexicality × Repetition Condition in Experiment 2

Repetition, lexicality, and response item	Response 0	1	2
Bright			
Repetition, word targets			
C1–C2	1.9	23.7	74.4
Fillers	94.1	5.7	0.2
No repetition, word targets			
C1	5.6	93.6	0.8
C2	10.6	88.3	1.1
Filler	96.6	3.4	0.1
Repetition, nonword targets			
C1–C2	6.6	29.7	63.6
Fillers	94.7	5.2	0.1
No repetition, nonword targets			
C1	19.9	78.4	1.7
C2	37.4	61.8	0.8
Filler	94.9	5.0	0.2
Not bright			
Repetition, word targets			
C1–C2	3.5	49.6	46.9
Fillers	95.1	4.9	0.1
No repetition, word targets			
C1	11.9	86.3	1.9
C2	20.7	78.9	0.4
Filler	96.5	3.4	0.1
Repetition, nonword targets			
C1–C2	16.3	51.6	32.1
Fillers	96.1	3.8	0.1
No repetition, nonword targets			
C1	38.3	60.5	1.1
C2	59.3	40.3	0.4
Filler	95.1	4.8	0.1

Note. The correct response pattern for a repetition trial was C1–C2 = 2 and Fillers = 0. The correct response pattern for a no-repetition trial was C1 = 1, C2 = 1, and Filler = 0. C1–C2 = response item corresponding to the repeated target on the repetition trials; C1 = first target on the no-repetition trials; C2 = second target on the no-repetition trials; Filler = response item that had not been presented.

(*Appendixes continue*)

Appendix E

Mean Percentages of Responses 0, 1, or 2, to C1, C2, and Fillers, for Each Brightness × Lexicality × Repetition Condition in Experiment 3

Repetition, lexicality, and response item	Response 0	Response 1	Response 2
Bright			
Repetition, word targets			
C1–C2	1.4	47.5	51.1
Fillers	94.7	5.2	0.2
No repetition, word targets			
C1	6.4	91.2	2.4
C2	30.7	68.4	0.9
Filler	94.9	5.0	0.1
Repetition, nonword targets			
C1–C2	10.9	54.6	34.5
Fillers	95.1	4.9	0.0
No repetition, nonword targets			
C1	19.2	78.5	2.3
C2	72.7	27.1	0.2
Filler	95.5	4.5	0.0
Not bright			
Repetition, word targets			
C1–C2	2.8	53.1	44.2
Fillers	95.8	4.1	0.0
No repetition, word targets			
C1	12.9	85.5	1.5
C2	20.0	79.2	0.9
Filler	95.0	5.0	0.0
Repetition, nonword targets			
C1–C2	20.5	51.3	28.2
Fillers	95.9	4.0	0.1
No repetition, nonword targets			
C1	36.8	62.2	1.0
C2	61.0	38.7	0.3
Filler	95.4	4.6	0.0

Note. The correct response pattern for a repetition trial was C1–C2 = 2 and Fillers = 0. The correct response pattern for a no-repetition trial was C1 = 1, C2 = 1, and Filler = 0. C1–C2 = response item corresponding to the repeated target on the repetition trials; C1 = first target on the no-repetition trials; C2 = second target on the no-repetition trials; Filler = response item that had not been presented.

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