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Repetition Blindness, Forward and Backward

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In these experiments, 2 letters were presented sequentially to the left and right of fixation, followed by pattern masks. Report was cued by spatial location (Experiments 1a, 1b, 2, 4, and 5) or temporal position (Experiments 3, 4, and 5). In all experiments, 2 identical letters on a trial resulted in reduced accuracy of report (repetition blindness; RB) for both the 1st and 2nd presented letters. This decrement was greater for the 2nd letter if subjects expected temporal cues, but tended to be greater for the 1st letter if they expected spatial cues. Analyses of errors and responses on catch trials indicated no bias against report of repetitions, and the repetition decrement did not interact with output order. The data are inconsistent with both type-refractoriness and memory-retrieval accounts of RB. A modified version of N. G. Kanwisher's (1987) token-individuation theory is proposed to account for the results.

As a general rule, cognition thrives on repetition: A repeated stimulus usually results in better memory (Ebbinghaus, 1885/1913), more accurate perception (Haber, 1965; Haber & Hershenson, 1965), and faster reaction time (Bertelson, 1963; Keele, 1969). However, there are quite a few exceptions to the rule: For example, proactive and retroactive interference with cued recall results if the same cue stimulus is studied with multiple targets (McGeoch, 1932; Melton & Irwin, 1940; Underwood, 1945). In reaction-time studies, a repeated stimulus may yield delayed responding relative to an unrepeated stimulus if its initial presentation was as a distractor to be ignored (i.e., negative priming; Neill, 1977; Neill & Westberry, 1987; Tipper, 1985; Tipper & Cranston, 1985). Neill and Mathis (1998) reviewed a number of similar phenomena in which the initial processing of a stimulus may be transfer inappropriate to the processing of its reappearance.

One controversial performance decrement due to repetition is the *repetition blindness* (RB) phenomenon reported by Kanwisher (1987). In the most common demonstration of RB, a rapid sequence of visual stimuli is presented at a single spatial location. If two items in the sequence share the same identity, report of the second occurrence is often depressed relative to an unrepeated item in the same serial position. This effect can occur despite strong sequential constraints on item predictability. In one experiment by Kanwisher (1987), a presented sequence of words formed a grammatical sentence. If a word appeared twice in the sequence (e.g., *She likes music because music is relaxing*), the probability of reporting the second occurrence (22%) was much lower than the probability of reporting the same word when not a repetition (e.g., *She likes jazz because music is relaxing*; 79%), even though the omission resulted in reporting an anomalous sentence (*She likes music because is relaxing*).

RB has similarly been found in sequences of unrelated words (e.g., Bavelier & Potter, 1992; Kanwisher, 1987), digits and letters (Armstrong & Mewhort, 1995; Bavelier & Potter, 1992; Chun & Cavanagh, 1997; Luo & Caramazza, 1995, 1996), pictures (Bavelier, 1994), complex nonsense shapes (Arnell & Jolicouer, 1997), and simple shapes and colors (Kanwisher, 1991; Kanwisher, Driver, & Machado, 1994, 1995). The effect can be found even when repeated items are spatially separated (Fagot & Pashler, 1995; Kanwisher, 1991; Kanwisher & Potter, 1989; Luo & Caramazza, 1995, 1996) and when only two items are presented in a trial (Hochhaus & Johnston, 1996; Hochhaus & Marohn, 1991; Luo & Caramazza, 1995, 1996).

Kanwisher (1987) proposed that RB occurs as a result of failure to differentiate between different instances of the same type of event if they happen too close together in time. More specifically, she argued that different instances may successfully access their abstract conceptual representation in memory (i.e., they activate a common *type node*) but may fail to be encoded as separate *tokens* in episodic memory. In support of the type–token distinction, she found facilitation rather than RB from word repetition in a task that required subjects to report only the last word in a rapid sequence. Presumably, no token individuation was required for the first occurrence of the word, and identification of the second occurrence benefited from activation of the type node. Neill and Cestaro (1993; see Neill & Mathis, 1998) found similar facilitation for reporting an uppercase target word imbedded in a sequence of ignored lowercase words if that word appeared earlier as a lowercase word. (See Downing & Kanwisher, 1995, Whittlesea, Dorken, & Podrouzek, 1995, and Whittlesea, Podrouzek, Dorken, Williams, & Wright, 1995, for debate over such effects.)

Because RB is usually demonstrated in tasks requiring retrospective report, much debate has centered on whether it really is due to interference with the on-line perceptual processing and encoding of the repeated stimulus or instead is due to interference with subsequent memory retrieval. For example, in the Ranschburg effect (Crowder, 1968; Ranschburg, 1902), short-term recall

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of a repeated item in a near-span list is depressed even though the presentation rate is slow enough to ensure adequate perceptual encoding. In many RB experiments, subjects are implicitly or explicitly expected to report list items in order of occurrence. Thus, it is possible that the overt report of the first occurrence of a repeated stimulus interferes with subsequent retrieval of the second. Fagot and Pashler (1995) noted that report of the first occurrence may also introduce a response bias against the second, for either of two reasons: (a) If subjects fail to recognize the second stimulus, they may tend to guess an item not yet reported (*guessing bias*), or (b) when repeated stimuli are both recognized, subjects may still be reluctant to emit the same response twice (*censorship bias*).

Consistent with either output interference or response bias, Fagot and Pashler (1995, Experiment 1) reported that RB for the second occurrence was eliminated when subjects were required to report the list items in reverse order. However, backward recall should also have produced RB for the first presented item. Because subjects made many serial-position errors in backward recall, Fagot and Pashler had difficulty in attributing RB specifically to the first or second occurrence of a repeated target. In addition, Fagot and Pashler manipulated report order between groups of subjects, raising the possibility that different encoding strategies, rather than different retrieval order, might alter the pattern of RB.

Whittlesea, Dorken, and Podrouzek (1995; see also Whittlesea & Podrouzek, 1995) argued that RB could occur because of a failure to differentiate between instances of a repeated item at the time of retrieval, as opposed to time of encoding. According to Whittlesea, Dorken, and Podrouzek's explanation, list items are all encoded independently of each other; consequently, there are two separate memory traces (tokens) for a repeated item. However, under the conditions of rapid-serial-visual presentation, contextual information that would distinguish similar memory traces may be poorly encoded. In the process of reconstructing the sequence of events for retrieval, the memory trace for the second occurrence may be misassigned to the position of the first occurrence (or vice versa). Such migration would be less likely to occur for unrepeated events because the inconsistency of identity information assigned to a common position would be more likely to be noticed.

In general, proponents of memory-retrieval accounts have emphasized the effects of manipulations at the time of report on the magnitude of RB. Several studies have found RB to be eliminated if recall of the second occurrence of a repeated item was specifically cued. For example, Fagot and Pashler (1995, Experiment 4) presented sequences of six letters in which one was colored red. After a sequence, the subject was cued to either first report the red letter or first report the whole sequence. Whole report yielded RB for the second occurrence of a repeated letter, even if it was the one shown in red. However, if subjects were cued to first report the red letter, report of a repeated letter did not differ from report of an unrepeated letter.

Similarly, Armstrong and Mewhort (1995) either required subjects to report all of a five- or seven-letter list or they provided one letter from the sequence and required subjects to recall the next letter in the sequence (cued report). Although RB was found in whole report, none was found in cued report for a repeated letter. Lewandowsky, Neely, VerWys, and Amos (1997) also failed to find RB when the repeated stimulus was specifically cued for report. If RB were due to impaired encoding of the repeated item, one might expect recall to be impaired even when that item was specifically cued. On the other hand, it could be argued that effects on encoding will always interact with retrieval conditions in determining report (e.g., Tulving & Pearlstone, 1966). In cued recall, the cues provide some information about the target stimulus; this may compensate for impaired encoding in a nonlinear manner that reduces the magnitude of RB.

Proponents of perceptual–encoding accounts of RB have emphasized (a) the presence of RB in tasks that do not require retrospective report of the individual items and (b) RB under conditions of minimal memory load. As an example of the former, Park and Kanwisher (1994; see also Kanwisher, Kim, & Wickens, 1996) found RB when subjects were required to determine whether one or two vowels occurred in a sequence of consonants. In principle, counting two target items should result in identical memory load ("two") and retrieval demands regardless of whether the targets share the same identity. However, the procedures used by Park and Kanwisher did not distinguish whether false "two" reports resulted from misperception of two identical items or two nonidentical items. Indeed, we have found that subjects rarely false alarm to detecting two identical targets, which raises the possibility that discrimination of two identical items from one item is not impaired relative to discriminating two nonidentical items from one item (Anderson, 1999; Anderson & Neill, in press).

Luo and Caramazza (1995) demonstrated RB in a procedure in which only two letters were shown on a trial, sequentially to the left and right of fixation. Subjects had more difficulty reporting both letters if they happened to be identical. Luo and Caramazza (1995) argued that with such a minimal memory load, difficulties with memory retrieval were unlikely; therefore, RB must be due to impaired perception and/or encoding of the repeated stimulus. In a second experiment, Luo and Caramazza (1995) found RB when subjects were required to report only the second of the two letters. This result cannot be explained by output interference or guessing–censorship bias. However, the result also contradicted Kanwisher's (1987) finding of facilitatory priming when subjects only had to respond to the last word in a list. On the assumption that tokenization of the first letter was not required, Luo and Caramazza (1995) argued that RB was due to type refractoriness; that is, once the first letter was perceptually categorized, it was difficult to reactivate the same category for a second occurrence of that letter. On the other hand, it could be argued that the requirement to report the second of two stimuli does necessitate the tokenization of the first stimulus in order to know that the second is indeed second.

To date, the empirical data are arguably inconclusive as to whether RB is due to encoding impairment (either typing or tokenizing), retrieval impairment, or both. (Or at least, neither side of the debate has been convinced by the other.) We suggest that progress in understanding RB has been impeded by a persisting assumption that the effect is primarily proactive—that is, it is only the processing of the second occurrence of a repetition that is impaired by processing of the first. If report of the first occurrence of a repeated item is impaired, this would surely impose constraints on the kinds of explanation that could be offered for RB. Substantial RB for the first occurrence of a repeated item would be especially problematic for the type-refractoriness theory. Because processing times are variable, the second of two closely presented instances might sometimes be categorized first. However, the first target should be identified sooner than the second on a majority of trials, and therefore RB for the first target should never equal or exceed RB for the second target.

Downing and Kanwisher (1995) suggested that a second stimulus may sometimes be individuated first if "(a) a short lag occurs between the two occurrences, (b) the instructions emphasize report of the end of a sequence . . .; or (c) the [second] item is more salient" (p. 1700). However, the first occurrence should again be individuated sooner than the second on a majority of trials; hence, tokenization failure should occur more often for the second occurrence than for the first. (We will, however, propose a modification of the token-individuation theory that can account for substantial RB on the first occurrence.)

In contrast, memory-retrieval explanations easily accommodate RB for the first presented item. Output interference and guessing–censorship bias both predict that RB should depend more on order of report than on order of presentation (Fagot & Pashler, 1995). Whittlesea, Dorken, and Podrouzek (1995) argued that the direction of memory migration in sentence-like sequences can be influenced by subjects' expectations. In sentences, a referent appearing twice is much more likely to be explicitly named in the first position. (Compare *She likes music because it is relaxing* vs. *She likes it because music is relaxing*.) Consequently, the memory trace for the second occurrence of a repeated word is more likely to migrate to the position of the first than vice versa. By extension, other experimental contexts might bias migration away from, rather than toward, the first position.

But, it is surprisingly difficult to ascertain from the published literature whether RB does occur for the first of two repeated items, for a variety of reasons. If a presented list of words constitute a sentence, it is relatively easy to determine where an error has occurred. However, guessing biases may favor reporting a repeated item in the first position, as suggested by Whittlesea, Dorken, and Podrouzek (1995). Second, such lists are likely to be reported in forward order, confounding presentation order with output order. Third, although researchers (e.g., Kanwisher, 1987) have been careful to counterbalance words used in the second position (e.g., *music* in both the repeated and nonrepeated conditions), they often have not counterbalanced words in the first position (e.g., *jazz* vs. *music*).

If list items are randomized (e.g., unrelated words or letters), subjects are still likely to favor reporting them in forward order in whole report, unless they are explicitly instructed otherwise. Therefore, presentation order again is likely to be confounded with output order. In addition, if the subject reports only one occurrence, it is often unclear whether it should be scored as a correct report of the first occurrence or the second (see discussion by Downing & Kanwisher, 1995; Fagot & Pashler, 1995; Kanwisher, 1991; Whittlesea, Dorken, & Podrouzek, 1995). For this reason, many researchers have given up trying to distinguish between report of the first occurrence and report of the second, and they rely on comparing the probability of reporting two instances (*both* report) to the probability of reporting both of two unrepeated stimuli presented at the same serial positions (e.g., Bavelier & Potter, 1992; Fagot & Pashler, 1995; Kanwisher, 1991; Luo & Caramazza, 1995; Park & Kanwisher, 1994). Of course, this makes it impossible to judge whether an impairment occurred for the first or second occurrence.

Direct cuing of individual items for report provides a means of controlling output order, and it provides an objective measure of performance at a specified position. It does not preclude the possibility of migration, that is, misassignment of an instance from another position, but it does allow a means for estimating the likelihood that this occurs (see below). On the other hand, RB often does not occur with cued report, which is a major argument made by proponents of retrieval interference as the cause of RB in whole report (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Lewandowsky et al., 1997). Yet, the fact that Luo and Caramazza (1995, Experiment 2) found RB when subjects were instructed to report only the second of two stimuli strongly suggests that RB can be found with directly cued report in some paradigms.

The present experiments differ from most previous experiments on RB in being designed to test effects of repetition on the report of the first presented item as well as the second. We used a modification of the two-letter procedure introduced by Luo and Caramazza (1995): A randomly selected letter (*A*, *B*, *C*, or *D*) was presented to the left or right of fixation for 50 ms. It was then replaced by a pattern mask, and (on most trials) a second letter was displayed for 50 ms in the opposite location. By chance, the second letter was identical to the first letter on 25% of such trials. Pattern masks then occupied both positions for 100 ms, followed by a cue to report one of the targets. In Experiments 1a, 1b, and 2, the report cue was a bar marker indicating a target location; in Experiment 3, the report cue was a phrase ("first letter?" or "second letter?") requesting report of either the first or second letter; in Experiments 4 and 5, we directly compared the two kinds of report cues. In Experiments 2–5, the response to the first cue was followed by a cue to report the other item, allowing an assessment of output-order effects.

It may be noted here that substantial RB for the first occurrence of a repeated letter would be problematic for five of the six accounts of RB discussed above: Output interference, guessing bias, and censorship bias cannot account for RB in the absence of prior report of the other item. Perceptual–encoding accounts (type refractoriness or tokenization failure) can accommodate some degree of RB for the first presented item, because processing times are inherently variable, but RB for the first target should never equal or exceed RB for the second target. On the other hand, the memory-migration hypothesis advanced by Wittlesea, Dorken, and Podrouzek (1995) allows for the possibility that the memory token for the first target (T1) might sometimes migrate to the remembered position of the second target (T2) more frequently than T2 migrates to the T1 position, given an appropriate expectancy set at the time of retrieval.

To assess guessing strategies and memory migration, we also included *catch trials* in which no second target was presented. Perceptual–encoding accounts of RB make no direct prediction about guesses when the *blank* location is cued. In contrast, memory migration would account for RB on the first item because the T1 token sometimes migrates to the T2 position. Therefore, subjects should be prone to guessing the first letter when cued to report the item that occurred at the blank location. On the other hand, guessing bias and censorship bias predict that subjects should be biased against reporting the first letter (i.e., the only letter presented) as having occurred at the blank location, especially if the letter has already been reported.

It is possible that tests of guessing strategies are invalidated on catch trials because subjects detect the nonoccurrence of a target at the cued blank location. However, all experiments provided an even stronger test of guessing biases by an analysis of errors on trials in which two different letters were presented. Specifically, if an error was made on a cued target, the migration hypothesis predicts that intrusions of the other target should occur more often than chance (33%). On the other hand, guessing bias and censorship bias predict that intrusions of the other target should occur less often than chance if the other target has already been reported.

To summarize, these experiments provide tests of six theoretical explanations of repetition blindness: type refractoriness (Luo & Caramazza, 1995), tokenization failure (Kanwisher, 1987), memory migration (Whittlesea, Dorken, & Podrouzek, 1995), output interference (Fagot & Pashler, 1995), guessing bias (Fagot & Pashler, 1995), and censorship bias (Fagot & Pashler, 1995). To anticipate the discussion of our findings, all six theories fail (although memory migration fares better than the rest). In the General Discussion, we provide an alternative account initially proposed by Neill and Mathis (1998), which is most closely allied to the tokenization-failure hypothesis originally advanced by Kanwisher (1987).

Experiments 1a, 1b, and 2

Method

Subjects. Undergraduate students at the University at Albany, State University of New York participated to fulfill an experiment-participation requirement for the introductory course on psychology ($n = 29$, 28, and 33, in Experiments 1a, 1b, and 2 respectively). Each subject was tested individually in a session lasting approximately 0.5 hr.

Stimuli and apparatus. Stimuli were presented on a Vivitron 1572 color monitor controlled by a Gateway 2000 PC-compatible microcomputer. The *S*, *C*, *L*, and *M* keys of the computer keyboard were relabeled *A*, *B*, *C*, and *D*, and were used to register subjects' responses. Micro Experimental Laboratory II (MEL II; Schneider, 1988) was used to program the experiment. The stimuli were uppercase letters drawn from the set *A*, *B*, *C*, and *D* presented in the default MEL II letter font approximately 1° to the left or right of a plus sign (+) at the center of the monitor screen. Pound signs (#) were used as pattern masks in the same locations. An upward-pointing arrow below the location of a letter was used to cue subjects' report.

Procedure. On each trial, subjects first saw a plus sign presented as a warning signal for 100 ms. A letter from the stimulus set was then displayed either to the left or right of the plus sign for 50 ms. The letter was then replaced by a pound sign; at the same time, either another letter appeared on the opposite side of the plus sign for 50 ms (four fifths of trials) or that location was left blank for 50 ms (one fifth of trials). A pound sign then appeared in that location, and both pound signs remained in view for 100 ms followed by a vertical arrow presented below either the left or right target position, randomly selected, indicating the target to be reported. The report cue remained in view until the subject pressed one of the four response keys.

Because of a programming error, the blank location was never cued for report on the single-target trials of Experiment 1a. Experiment 1b was therefore run as an exact replication, except with random sampling of the blank location as well. (The data for both experiments are reported here for replication of the two-target trial results.) In Experiment 2, after the subject responded to the first report cue, an arrow then appeared under the other target location for another response. After the final response, the display was then cleared for 2.5 s before initiation of the next trial. Subjects received 320 trials, with rest breaks every 40 trials.

Results

Data analysis. In all experiments, the dependent measure of primary interest was proportion of correct responses at a cued location when two letters had been presented. In Experiment 2 and all subsequent experiments, this proportion was conditionalized on correct report of the target that occurred in the opposite location. This provided a conservative estimate of RB, insofar as reversals due to either misperception or memory confusion of locations would be scored as correct for repeated targets but incorrect for unrepeated targets. In addition, the type-refractoriness and token-individuation theories would not predict RB to occur if the other target was not appropriately typed or tokenized.

The condition means for proportion of correct responses in all experiments are displayed in Table 1.

On two-letter nonrepetition trials in which an error was made to the cued target, we calculated the probability that the reported letter was the one that appeared in the opposite location (chance = .33). In Experiment 2 and subsequent experiments, this proportion was not conditionalized on whether the other target was reported correctly when it was cued. The rationale here was to provide a liberal test of the memory-migration hypothesis, insofar as migration of the opposite-location target would be less probable if that target was correctly reported at its location. This analysis is biased against output-interference and guessing–censorship-bias explanations of RB because they predict a less-than-chance intrusion of the opposite target only on those trials in which the opposite target has already been reported. However, these hypotheses are already tested in the primary analysis by whether RB occurs in the absence of a prior report. (As noted above, perceptual accounts of RB make no direct prediction about guesses when the target is misperceived.)

Finally, on catch trials, we examined the proportion of guesses that corresponded to the opposite-location target when the blank location was cued (chance = .25). In Experiment 2 and subsequent experiments, the latter proportion was again not conditionalized on correct report of the target at the opposite location, by the same rationale as for the analysis of errors on two-letter nonrepetition trials.

The unconditionalized means of guessing proportions on both catch trials and nonrepetition error trials for all experiments are displayed in Table 2.

Experiment 1a. On trials with two presented targets, performance was better on nonrepetition trials (proportion = .84) than on repetition trials (.66), $F(1, 28) = 44.949$, $MSE = .021$, $p < .001$, and marginally better on the second presented target (.77) than on the first presented target (.73), $F(1, 28) = 3.81$, $MSE = .012$, $p = .061$. Target (first or second) interacted with repetition, $F(1, 28) = 18.278$, $MSE = .005$, $p < .001$, such that the nonrepeated minus repeated difference (RB) was greater for the first target ($.85 - .61 = .24$) than for the second target ($.83 - .71 = .12$).

On nonrepetition trials in which the cued target was misreported, the probability of reporting the letter that appeared at the other location was .38 when the first target was cued and .37 when the second target was cued. Neither value was significantly greater than chance (.33).

Experiment 1b. On trials with two presented targets, performance was again better on nonrepeated targets (.80) than on repeated targets (.62), $F(1, 27) = 73.00$, $MSE = .001$, $p < .001$,

Table 1
Summary of Results of Experiments 1–5: Proportion of Correct First and Second Target Reports for Nonrepeated and Repeated Targets and Difference (Repetition Blindness; RB), as a Function of Report Position and Type of Cue

	First target			Second target		
Report	Nonrepeated	Repeated	RB	Nonrepeated	Repeated	RB
Experiment 1a						
First (spatial)	.85	.61	.24	.83	.71	.12
Experiment 1b						
First (spatial)	.80	.58	.21	.80	.65	.16
Experiment 2						
First (spatial)	.87	.59	.28	.85	.58	.27
Second (spatial)	.88	.67	.22	.88	.64	.24
Experiment 3						
First (temporal)	.76	.67	.09	.74	.63	.10
Second (temporal)	.72	.63	.09	.74	.60	.14
Experiment 4						
First (spatial)	.75	.46	.29	.76	.48	.28
Second (spatial)	.75	.45	.30	.72	.49	.23
First (temporal)	.68	.50	.18	.69	.48	.21
Second (temporal)	.67	.48	.19	.66	.47	.19
Experiment 5						
First (spatial)	.77	.42	.35	.76	.53	.23
Second (spatial)	.74	.41	.32	.75	.47	.29
First (temporal)	.69	.43	.26	.67	.41	.26
Second (temporal)	.64	.44	.20	.67	.39	.28

Note. Proportions are conditionalized on correct response to the opposite-location target. All RB effects are significantly different from zero, $p < .001$, two-tailed.

and better on the second presented target (.73) than on the first presented target (.69), $F(1, 27) = 5.04$, $MSE = .001$, $p < .05$. These two variables again interacted significantly, $F(1, 27) = 4.47$, $MSE = .020$, $p < .05$, such that RB was greater on the first target (.80 − .58 = .21) than on the second target (.80 − .65 = .16).[1]

On nonrepeated trials in which an error was made, the probability of guessing the target that appeared in the opposite location was .32 when the first target was cued and .35 when the second target was cued. Neither differed statistically from chance (.33). On catch trials, the probability of guessing the target at the opposite location was .27, not significantly different from chance (.25).

Experiment 2. On trials with two presented targets, performance was better on nonrepeated targets (.87) than on repeated targets (.62), $F(1, 32) = 31.29$, $MSE = .136$, $p < .001$; better on the second reported item (.76) than on the first reported item (.72), $F(1, 32) = 12.36$, $MSE = .009$, $p < .001$; and marginally better on the first presented item (.75) than on the second presented item (.74), $F(1, 32) = 3.36$, $MSE = .003$, $p = .076$. In addition, repetition interacted significantly with report position, $F(1, 32) = 5.89$, $MSE = .005$, $p < .05$, because of a greater RB effect on the first reported item (.86 − .59 = .27) than on the second reported item (.88 − .65 = .23). Notably, the Repetition × Item interaction was not significant, $F(1, 32) < 1$, reflecting approximately equal RB for the first presented letter (.88 − .62 = .25) and the second presented letter (.86 − .61 = .25).

On two-target trials in which an error was made to a cued target, the probability of reporting the opposite target was .35 for the first target cued first; .47 for the first target cued second; .44 for the second target cued first; and .34 for the second target cued second. Although the first and last proportions did not differ significantly from chance (.33), the second and third proportions were greater than chance, $t(32) = 4.17$, and $t(32) = 3.49$, $ps < .05$, two-tailed. In other words, when report order was opposite to presentation order, subjects had a tendency to guess the opposite target.

On catch trials, the probability of reporting the target letter when the blank location was cued was .38 if cued first and .36 if cued second. Both probabilities were significantly greater than chance (.25), $t(32) = 5.02$, and $t(32) = 3.54$, $ps < .01$, two-tailed.

Discussion

The result of primary importance is that very large RB occurred for both the first and second presented letters, with the decrement actually greater on the first presented target in the first two experiments and approximately equal for first and second targets in Experiment 2. The sizeable RB for the first presented target in all three experiments is problematic for the proposed perceptual accounts of RB (i.e., type refractoriness, tokenization failure), insofar as perception of the first target is assumed to be independent of subsequent events and to be the proactive cause of misperception of a repetition.

By itself, the existence of a large RB effect on the first presented target is consistent with memory-retrieval explanations of RB, which do not assign any special status to presented stimuli accord-

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[1] All means and differences between means were initially calculated to three decimal places before rounding; any discrepancy of .01 in the difference scores is due to rounding error in the reported condition means.

Table 2
Summary of Results of Experiments 1–5: Proportion of Opposite-Target Guesses on Catch Trials (Chance $= .25$*) and on Nonrepetition Error Trials (Chance* $= .33$*), as a Function of First or Second Target, Report Position, and Type of Cue*

	Cued target		
Report	First target	Second target	Blank location
Experiment 1a			
First (spatial)	.38	.37	—
Experiment 1b			
First (spatial)	.32	.35	.27
Experiment 2			
First (spatial)	.35	.44*	.38
Second (spatial)	.47*	.34	.36
Experiment 3			
First (temporal)	.79**	.81**	.59**
Second (temporal)	.79**	.77**	.52**
Experiment 4			
First (spatial)	.30	.65**	.72**
Second (spatial)	.27*	.36	.30
First (temporal)	.57**	.68**	.51**
Second (temporal)	.55**	.63**	.50**
Experiment 5			
First (spatial)	.46**	.49**	.44*
Second (spatial)	.46**	.38*	.19
First (temporal)	.69**	.64**	.28
Second (temporal)	.68**	.62**	.24

Note. Proportions are not conditionalized on correct response to the opposite-location target.
* $p < .05$, two-tailed. ** $p < .001$, two-tailed.

ing to their order of presentation. However, the details of the effect are also not consistent with specific memory-retrieval explanations: The output-interference, censorship-bias, and guessing-bias explanations suggested by Fagot and Pashler (1995) do not predict any RB for a target that is not preceded by overt report of a similar target. As such, they cannot account for RB in Experiment 1a or 1b or for RB on the first reported target in Experiment 2. More generally, the results belie the assertion made by memory-retrieval proponents that RB does not occur for cued report (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Lewandowsky et al., 1997).

That RB was actually greater for the first reported letter than for the second in Experiment 2 might, at first consideration, suggest that memory retrieval is somehow contributory to RB. However, as noted in the introduction, encoding effects may interact with retrieval conditions. A simple explanation of greater RB for the first reported item may be that subjects were quite certain that they saw one instance of the repeated item. Because they expected two letters to be shown, they may have adopted a strategy of reserving their certain choice for the second cued report and guessing more often on the first cued report.

Of the considered theoretical explanations of RB, only the memory-migration hypothesis advanced by Whittlesea, Dorken, and Podrouzek (1995) easily accommodates the results described above: That is, the memory trace for the first presented letter might migrate as frequently, or more frequently, to the remembered position of the second letter as the memory trace for the second presented letter would migrate to the remembered position of the first letter. Further, insofar as subjects should be quite certain of having seen at least one instance of a repeated target, they might again choose to defer their certain choice until the second cued report. The analyses of guesses in Experiment 2 are also somewhat consistent with memory migration, insofar as the probability of guessing the letter opposite to the cued location was greater than chance both for errors on two-target trials and for blank locations on single-target trials.

On the other hand, the analyses of guesses do not support the hypothesis of memory migration in Experiments 1a and 1b. The probability of guessing the target opposite to the cued location did not differ significantly from chance in either experiment. More to the point than a statistically null result, the magnitude of the differences from chance were small compared with the magnitude of the RB effect. In Experiment 1a, the overall difference from chance on two-target error trials was .04, in comparison to an RB effect of .18; in Experiment 1b, the difference from chance was .002, in comparison to an RB effect of .19. In the catch trials, the difference from chance when the blank location was cued was only .02.

Even in Experiment 2, significant differences from chance occurred only if report order was opposite to presentation order. When report order was congruent with presentation order, the overall probability of reporting the opposite letter on two-target error trials was .01, in contrast to an overall RB effect of .26. When report order was incongruent with presentation order, the difference from chance was .12, still small in contrast to an RB effect of .25. The fact that the RB effect did not change with report order (i.e., the Presentation Position × Report Position × Repetition interaction was $F < 1$), despite the change in guessing rates, provides additional evidence that RB cannot be attributed to memory migration as manifested in the guessing rates.

To summarize the guessing analyses, there was a tendency for subjects to guess the letter opposite to the cued location, consistent with the memory-migration hypothesis. However, the guessing bias was small relative to the magnitude of the RB effect; furthermore, conditions associated with increased guessing bias (output order reversed of presentation order) were not associated with increased RB. In other words, none of the extant theories of RB provide an adequate account of the data from the first three experiments (1a, 1b, and 2).

Experiment 3

In Experiment 3, subjects were required to report the letter targets by temporal position rather than by spatial position. Spatially cued report is an atypical task in RB studies (but, see Lewandowsky et al., 1997). In RB studies requiring whole report, subjects are likely to rely more on temporal-order information to organize their report; the cued-report procedure used by Armstrong and Mewhort (1995) required subjects to report the letter that followed a cue letter in a list and therefore also required retrieval of temporal-order information. Furthermore, Experiment 2 of Luo and Caramazza (1995) required subjects to report the second of two presented letters (in displays similar to those used in our study) and therefore required subjects to encode order of presentation. In principle, direct cuing of the second presented letter in our study should provide a conceptual replication of Luo and Caramazza's (1995) experiment.

Method

Subjects. Subjects were 48 University at Albany undergraduates who participated to fulfill an experiment-participation requirement in an introductory psychology course. None had participated in the previous experiments.

Stimuli and apparatus. Stimuli and apparatus were identical to those used in the previous experiments, except that the phrases "first letter?" and "second letter?," centered on the computer monitor, were used instead of arrows to cue report.

Procedure. Apart from the report cues, the only difference in procedure from Experiment 2 was that subjects received visual feedback after their second response about the correctness of each response.

Results

The condition means are displayed in Table 1. Subjects reported the first presented letter (.69) more accurately overall than the second presented letter (.68), $F(1, 47) = 4.25$, $MSE = .006$, $p < .05$, the first cued letter (.70) more accurately than the second cued letter (.67), $F(1, 47) = 13.69$, $MSE = .004$, $p < .001$, and nonrepeated letters (.74) more accurately than repeated letters (.63), $F(1, 47) = 3.50$, $MSE = .308$, $p = .068$. RB was significantly greater for the second presented letter ($.74 - .62 = .12$) than for the first presented letter ($.74 - .65 = .09$), $F(1, 47) = 6.74$, $MSE = .003$, $p < .02$. No interactions with output position were significant.

On nonrepetition trials in which an error was made to a cued target, the probability of an intrusion by the opposite target was .79 for the first target cued first, .79 for the first target cued second, .81 for the second target cued first, and .77 for the second target cued second. All of these proportions are significantly higher than chance (.33) by t test, all $t\ (47) > 18.8$, $p < .001$.

On catch trials, the probability of reporting the target when the blank position was cued first was .59 and .52 when cued second. Both proportions are significantly greater than chance (.25), $t(47) = 11.52$, and $t(47) = 8.56$, respectively, both ps $< .001$.

Discussion

The results of this experiment differ most markedly from the previous experiments in that greater RB occurred for the second presented letter than for the first. As such, the results suggest that the relative magnitude of RB on the first or second letter may depend on whether spatial or temporal report cues are used. (The procedures here also differed from the previous experiments in that feedback was provided on correctness of responses. The possibility that presence of feedback is critical to the pattern of RB will be addressed in Experiments 4 and 5.)

The results also differed from the previous experiments in showing a very strong bias toward guessing the letter that appeared in the opposite location, if either an error was made to a cued target or a blank location was cued in a catch trial. As discussed in the introduction, the memory-migration hypothesis predicts such a bias, whereas the output-interference, guessing bias, and censorship bias accounts of RB all predict a bias against reporting the item that occurred in the opposite location (particularly if the item in the opposite location was already reported).

As noted in the *Results* section of Experiments 1a, 1b, and 2, the analyses of guesses were not conditionalized on whether the target in the opposite location was reported correctly; this provided a liberal test of the memory-migration hypothesis but a conservative test of the output-interference, guessing-bias, and censorship-bias hypotheses. In addition, errors of temporal order judgment during encoding would inflate the estimate of guessing the opposite target here (but not when spatially cued). To test memory migration more conservatively, and to eliminate effects of temporal order reversals, we carried out an additional analysis of nonrepetition-trial errors conditionalized on correct report of the target in the opposite location. The probability that an error was an intrusion of the opposite target was .56 for the first letter cued first, .48 for the first letter cued second, .54 for the second letter cued first, and .57 for the second letter cued second. All proportions were still significantly greater than chance (.33), all ts$(47) > 2.96$, $p < .01$.

These results are clearly inconsistent with output interference and guessing–censorship bias, which explicitly predict less-than-chance intrusion from the opposite target when the current target is cued second. At first glance, memory migration appears to have passed a particularly stringent test. However, it may have passed it too well: That subjects show a strong tendency to guess the opposite target even if that target was also correctly reported indicates that they are willing to use the same perception or memory trace for responding to both of two report cues. There is nothing in the concept of memory migration that inherently prohibits this. However, it must be recalled that the proposed explanation of RB is that migration of a memory trace from one position to another results in that trace being unavailable for report at the initial position. Thus, although Experiment 3's guessing results can be explained by memory migration, they also undermine its usefulness for explaining RB.

A more general explanation of the guessing results may be that when subjects are certain of having seen one letter but have some uncertainty about temporal position, they may tend to guess the same letter when cued to report either position. It should be noted that this guessing strategy would hurt performance on nonrepetition trials but facilitate performance on repetition trials. Therefore, the measure of RB would necessarily underestimate the true magnitude of impairment due to repetition, or in other words, guessing strategies at the time of report may tend to ameliorate RB rather than cause it.

Experiment 4

When subjects were cued to report by spatial position, they exhibited more RB on the first presented letter than on the second (Experiments 1a and 1b) or approximately equal RB on both (Experiment 2). In contrast, they exhibited greater RB on the second letter if cued to report by temporal position (Experiment 3). The contrasting patterns suggest that the kind of information to be associated with the letter identity, spatial or temporal, has consequences for the mechanism underlying RB. However, because the two kinds of report cue were used in different experiments, it is unclear whether the effect of report cue was due to different encoding strategies or to different retrieval strategies on the part of the subject.

Experiments 4 and 5 were designed to more directly compare RB with the two types of report cue. In Experiment 4, the type of report cue was randomized from trial to trial. Because subjects could not know what kind of report would be required, they were obliged to adopt the same encoding strategies in both conditions. A difference in RB pattern for the types of report cue would then strongly implicate retrieval processes as the source of RB.

Method

Subjects. Twenty-two University at Albany undergraduates participated to fulfill an experiment-participation requirement in an introductory psychology course. None had participated in the previous experiments.

Stimuli and apparatus. Stimuli and apparatus were identical to those used in the previous experiments.

Procedure. The procedure was similar to that of Experiments 2 and 3, except that type of report cue, spatial (Experiment 2) or temporal (Experiment 3), was randomized from trial to trial. Within a trial, the same type of cue was used for both targets; that is, both were cued spatially or both were cued temporally. Feedback was provided after the second response, as in Experiment 3. Subjects received 20 practice trials, followed by 320 experimental trials. A rest break was provided after every 40 trials.

Results

The condition means are displayed in Table 1. Report accuracy was higher overall when subjects were cued spatially (.61) than when cued temporally (.58), $F(1, 21) = 4.46$, $MSE = .019$, $p < .05$. In addition, overall performance was higher on nonrepetition trials (.70) than on repetition trials (.48), $F(1, 21) = 6.90$, $MSE = .704$, $p < .02$. The magnitude of RB was greater for spatial report cues (.75 − .47 = .28) than for temporal report cues (.67 − .48 = .19), $F(1, 21) = 7.30$, $MSE = .021$, $p < .02$. No other main effects or interactions were significant. Notably, approximately equal RB occurred for the first letter (.71 − .47 = .24) and the second letter (.71 − .48 = .23), $F(1, 21) < 1$. There was a slight tendency for spatial cuing to result in greater RB for the first presented letter than the second (.30 vs. .26), whereas there was little difference for temporal cuing (.19 vs. .20). However, the interaction of Repetition × Presentation Position × Cue Type failed to approach significance, $F(1, 21) = 1.63$, $MSE = .009$, $p > .20$.

Table 2 displays the proportions of guesses corresponding to the opposite-location target when an error was made on a nonrepetition trial. With temporal cues, all guessing proportions were significantly greater than chance (.33), all ts$(21) > 8.18$, $p < .001$. In addition, with spatial cues, the guessing proportion was greater than chance if an error was made to the second target cued first, $t(21) = 8.63$, $p < .001$. However, the guessing proportion was significantly lower than chance for the first target cued second, $t(21) = -2.13$, $p < .05$.

As in Experiment 3, we also analyzed opposite-target guesses on errors, conditionalized on correct report of the opposite-location target. These proportions ranged from .24 (first target cued first, spatially) to .45 (second target cued first, spatially). None of these proportions differed significantly from chance (.33; all ps $> .10$).

On catch trials, the unconditional probability of guessing the opposite-location target when the blank location was spatially cued was .72 if cued first and .30 if cued second; when temporally cued, the probabilities were .51 if cued first and .50 if cued second. The three larger probabilities were significantly greater than chance (.25), all ts$(21) > 6.80$, $p < .001$; the smaller probability was not, $t(21) = 1.18$. The corresponding probabilities conditionalized on correct report of the opposite-location target were .56, .25, .33, and .37, respectively. Only the first probability differed significantly from chance, $t(20) = 3.55$, $p < .002$.

Discussion

Both the first and second presented letters showed strong RB, to an approximately equal degree. The magnitude of RB was significantly greater for spatial cuing than for temporal cuing. However, this appears to reflect greater accuracy in the nonrepetition condition when spatially cued than when temporally cued (.75 vs. .67). Not surprisingly, the ability to distinguish between the spatial locations of two different targets was better than the ability to judge their temporal order.

Whereas Experiments 1a, 1b, 2, and 3 suggested that spatial versus temporal report cues affected whether RB would occur more strongly for the first or second presented letter, this experiment failed to find strong evidence for a difference in the pattern of RB effects when encoding conditions were held constant. That is, if spatial and temporal report cues induced different retrieval strategies that in turn altered whether RB more affected the first or second presented letter, then a significant Repetition × Presentation × Cue interaction would have been expected. This did not occur. On the other hand, if a first–second difference in RB effects depends on encoding strategies, then this difference should emerge only when subjects are afforded the opportunity to use different encoding strategies. We tested this in Experiment 5 by varying type of report cue in separate blocks.

As in Experiment 3, the proportions of errors corresponding to the opposite-location target were high, especially when subjects were cued temporally, if not conditionalized on correct report of the opposite-location target. The one case in which opposite-target guessing was less than chance, as predicted by the output interference and guessing–censorship hypotheses, occurred for first targets spatially cued second. However, insofar as RB for these targets (.30) did not differ from similar targets cued first (.29), this result offers little support for either output interference or guessing–censorship bias. When conditionalized on correct opposite-location report, the guessing proportions generally did not differ significantly from chance, in contrast to the results of Experiment 3. As such, the memory-migration hypothesis may accommodate these guessing results a bit more easily.

Experiment 5

Method

Subjects. Twenty-nine University at Albany undergraduates participated to fulfill experiment-participation requirements in an introductory psychology course. None had participated in the previous experiments.

Stimuli and apparatus. Stimuli and apparatus were identical to those used in Experiment 4.

Procedure. The procedure was similar to that of Experiment 4, except that type of report cue was varied between separate blocks rather than randomized trial by trial. Subjects were randomly assigned to an order of cue types, resulting in 14 subjects receiving temporal cues first and 15 receiving spatial cues first. They received instructions for one type of report cue and 10 practice trials at the beginning of a cue condition; they then received 160 trials in that condition, subdivided into four blocks of 40 trials each. Subjects could rest at will before initiating the next block.

Results

The condition means are displayed in Table 1. As in Experiment 4, subjects performed better when spatially cued (.61) than when temporally cued (.54), $F(1, 27) = 8.77$, $MSE = .055$, $p < .01$, and better on nonrepeated targets (.71) than on repeated targets (.44), $F(1, 27) = 17.48$, $MSE = .489$, $p < .001$. In addition, report accuracy was greater on the first reported target (.59) than on the second reported target (.57), $F(1, 27) = 7.26$, $MSE = .008$, $p < .02$. Report of the second target (.63) was better than the first (.59) when spatially cued, but report of the first target (.55) was better than the second (.54) when temporally cued, resulting in a significant Cue Type × Target interaction, $F(1, 27) = 7.52$, $MSE = .013$, $p < .02$. Most important, the Cue Type × Repetition × Target interaction was highly significant, $F(1, 27) = 11.13$, $MSE = .009$, $p < .005$, reflecting significantly greater RB on the first presented target when spatially cued (first target: .34; second target: .26), $F(1, 27) = 5.78$, $MSE = .008$, $p < .05$, but nonsignificantly greater RB on the second presented target when temporally cued (first target: .23; second target: .27).

Table 2 displays the mean proportions of errors on nonrepetition trials that were guesses of the opposite-location target. All proportions were significantly greater than chance (.33), $ts(28) > 3.75$, $p < .001$, except for second targets spatially cued second, $t(28) = 2.05$, $p < .05$. It may be noted, however, that this guessing bias was much greater for temporal cues than for spatial cues. A post hoc analysis of variance (ANOVA) on the guessing proportions indicated greater opposite-target guessing for temporal cues than for spatial cues, $F(1, 28) = 50.19$, $MSE = .050$, $p < .001$, as well as for first presented targets relative to second presented targets, $F(1, 28) = 11.21$, $MSE = .008$, $p < .002$, and for first cued targets relative to second cued targets, $F(1, 28) = 6.75$, $MSE = .011$, $p < .02$. In addition, the guessing bias was stronger when targets were cued opposite to the presentation order (i.e., the Report × Target Position interaction), $F(1, 28) = 7.46$, $MSE = .006$, $p < .02$.

When we analyzed opposite-target guesses conditional on correct opposite-target report, no guessing proportions were significantly greater than chance. (The highest proportion was .40, for first targets temporally cued first.) Indeed, opposite-target guessing was significantly less than chance for first targets spatially cued first (.18), $t(27) = -3.93$, $p < .001$, and for second second targets spatially cued second (.23), $t(28) = -2.42$, $p < .05$. This difference also approached significance for first targets (.24), $t(28) = -2.04$, $p = .051$. A post hoc ANOVA confirmed that the proportion of opposite-target guesses was significantly lower for spatially cued trials than for temporally cued trials, $F(1, 23) = 7.81$, $MSE = .036$, $p < .02$. (No other effects were significant.)

The unconditionalized proportions of opposite-target guesses on catch trials are shown in Table 2. Only the proportion for blank locations spatially cued first differed significantly from chance, $t(28) = 2.74$, $p < .02$. Proportions of opposite-target guesses, conditionalized on correct opposite-target report were .45 for blank locations temporally cued first, .43 for blank locations temporally cued second, .70 for blank locations spatially cued first, and .24 for blank locations spatially cued second. The first three proportions were all significantly greater than chance (.25), $ts(28) > 7.70$, $p < .001$.

Discussion

Unlike Experiment 4, this experiment yielded clear evidence for a difference in the relative magnitude of RB on the first and second targets, depending on which type of report cue was used. Because subjects in Experiment 4 could not anticipate the type of report cue, they were obliged to encode the targets the same way, on average, for both report conditions. In contrast, the blocking of report-cue condition in Experiment 5 allowed subjects to anticipate whether spatial or temporal information was more important for report. We presume that this in turn allowed subjects to adopt different encoding strategies appropriate to the expected retrieval cue. Thus, it appears that greater attention to target location tends to cause more RB for the first presented letter, whereas greater attention to temporal order causes more RB for the second presented letter. A theoretical mechanism that may account for this effect is proposed in the General Discussion section.

At the same time, it may be worth noting that the magnitude of RB was very large for both first and second presented targets regardless of the report-cue condition. Thus, although subjects may be able to bias encoding in favor of one target or the other, their ability to do so may be limited.

The unconditionalized guessing analyses for nonrepetition trials again show a general bias toward, rather than against, reporting the target appearing opposite to the cued target. This bias was stronger for temporally cued targets than for spatially cued targets. When conditionalized on correct report of the opposite target, guessing was not biased toward the opposite target and was actually biased against the opposite target in the spatial-cue blocks.

Although the general pattern of a positive guessing bias in the unconditionalized analyses, but not in the conditionalized analyses, is consistent with the memory-migration hypothesis, it must be noted that the magnitude of RB was actually greater when spatially cued than when temporally cued, whereas the positive guessing bias was greater for temporally cued blocks. Therefore, the pattern of guessing biases does not support the memory-migration hypothesis. The presence of a significant bias against reporting the opposite target, when spatially cued, is more supportive of the output interference and guessing–censorship accounts of RB. However, given that RB did not depend on report order, this result too does not support those hypotheses.

General Discussion

In our experiments, subjects were shown two letters sequentially to the left and right of fixation, followed by masking patterns. They were then cued to report a letter by either spatial position (Experiments 1a, 1b, 2, 4, and 5) or temporal position (Experiments 3, 4, and 5). By chance, the two letters were identical on 25% of the trials. The result of primary importance is that robust RB—poorer report performance for repeated letters than unrepeated letters—occurred for both the first and second presented letters and for both the first and second reported letters. In addition, the type of report cue appeared to influence whether the first or second presented letter was more influenced by RB. When subjects could anticipate spatial cues, the first letter exhibited more RB (Experiments 1a, 1b, and 5) or roughly the same RB (Experiment 2) as the second letter. In contrast, when subjects could anticipate temporal cues, the second letter exhibited more RB

(Experiments 3 and 5). However, when subjects could not anticipate the type of cue (Experiment 4), type of cue did not affect the relative RB for first and second letters.

To the best of our knowledge, this is the first reported study to provide a controlled comparison of RB on first and second presented targets. That RB can be as large or larger on the first presented letter as on the second is problematic for theories that explicitly attribute RB to an aftereffect of encoding the first occurrence. For example, Luo and Caramazza (1995) argued that perceptual encoding of the first letter (in displays similar to those here) results in type refractoriness, that is, difficulty in reactivating the same conceptual category. Kanwisher (1987, 1991) argued that type activation is unaffected but that the creation of an episodic token for the first target interferes with the creation of an episodic token for an identical second target. Because processing times are inherently variable, it is possible that the second target is sometimes processed faster than the first target, causing RB to occur on the first target (cf. Downing & Kanwisher, 1995). However, assuming that the first target is identified sooner on average, the overall magnitude of RB should be greater for the second target.

On the other hand, the findings of RB on the first reported target and as-great or greater RB on that target as on the second reported target are problematic for explanations that attribute RB to aftereffects of the first report. Specifically, Fagot and Pashler (1995) suggested that the second report of a repeated target might be impaired by the effect of output interference on retrievability, by a bias toward guessing a target not already reported (guessing bias) or by a bias against making the same report twice even if both items were identified (censorship bias). Of the four experiments that cued both letters on a trial for report, only one (Experiment 2) found RB to interact with report position, and that interaction was opposite to the predictions of output interference, guessing bias, or censorship bias; that is, greater RB occurred for the first cued target than for the second cued target. We speculate that this interaction may have occurred because subjects were fairly certain of having seen one instance of a repeated letter and chose more often to reserve the "certain" letter for the second report.

The memory-migration hypothesis (Whittlesea, Dorken, & Podrouzek, 1995) accommodates the major findings above reasonably well, insofar as it makes no necessary commitment to greater RB on the first presented target versus the second presented target or to the first reported target versus the second reported target. According to this hypothesis, the targets are encoded independently of each other; thus there are two memory traces (tokens) for a repeated letter, just as there are for two unrepeated letters. However, all traces have relatively impoverished contextual information (here, spatial or temporal position). At the time of reconstructing the events for report, a token may be misassigned to the wrong position; this is more likely to occur for a repeated target because a conflict of two similar tokens assigned to the same position is less likely to be noticed than a conflict of two dissimilar tokens. Whittlesea et al. (1995) suggested that grammatical constraints make migration of the second token to the first position more likely than the reverse if the list constitutes a sentence. Similarly, it is possible that spatial cues encouraged greater migration of the first token to the second position, whereas temporal cues encouraged greater migration of the second token to the first position—although it is not clear why this difference should have occurred.

A critical assumption of the memory-migration hypothesis is that migration must occur frequently enough to account for the magnitude of the RB effect. However, a close inspection of the guesses when either a blank location was cued or an error was made to a cued target suggests that memory migration cannot account for RB in these experiments. In Experiments 1a, 1b, and 2, the probability of a guess corresponding to the target at the opposite location differed little from chance. In Experiment 3, the probability of such a guess was significantly higher than chance, consistent with the memory-migration hypothesis. However, this probability was high even when conditionalized on correct report of the opposite target. The memory-migration explanation for RB supposes that migration of a token to a different position leaves that token unavailable for report at the original position; this does not appear to have been the case in Experiment 3.

In Experiments 4 and 5, the probability of an opposite-target guess, conditionalized on correct report of the opposite target, was generally not higher than chance, despite higher-than-chance guessing in the unconditionalized analyses. As such, these analyses are more consistent with the memory-migration explanation. However, memory migration runs into difficulty when comparing guessing biases to overall RB effects when subjects are spatially or temporally cued: RB is generally greater for spatial cuing, but the tendency to guess the opposite-location target (unconditionalized or conditionalized on correct report of that target) is generally greater for temporal cuing.

Perhaps the most compelling observation here is that the guessing biases, although varying widely across experiments, are not systematically related to the magnitude of the RB effects. Insofar as the memory-retrieval accounts of RB make strong predictions of biases either against opposite-target guessing (output interference, guessing–censorship bias) or toward it (memory migration), any dissociation of guessing biases from the RB effect necessarily favors more perceptual–encoding explanations of the RB effect. It may be noted that perceptual–encoding explanations do not provide any account at all of the guessing data. However, they are also not obliged to do so. Subjects may, of course, adopt a variety of decision strategies at the time of test, but the effects of such strategies are overlaid on the availability of encoded information.

It might be argued that guesses on single-target trials and errors on nonrepetition trials may not be representative of the conditions under which memory migration is presumed to especially occur, namely, repetition trials. The analyses of guesses and errors may then simply be regarded as suggestive, rather than a definitive test of the memory-migration hypothesis. However, we offer below an alternative account of the RB data that is based on a modification of Kanwisher's (1987, 1991) tokenization-failure hypothesis suggested by Neill and Mathis (1998).

In Kanwisher's (1987, 1991) theory, tokenization of the first of two repeated targets is not presumed to differ from tokenization of an unrepeated target. Hence, RB is assumed to operate in a forward direction only, impairing the tokenization of a second occurrence of the same target. Tokenization of the first target should be impaired only if identification of the second target was by chance completed sooner. Assuming that first-presented targets are identified sooner on the average than second-presented targets, the magnitude of RB on the second target should never be equal to or greater than RB on the first. We suggest that tokenization should logically be regarded as two separate processes: (a) recognition that an instance of a conceptual category has occurred (*instantiation*) and (b) assignment or attribution of that instance to a specific context, such as list position (*contextualization*). Whereas instantiation may be determined by the first occurrence of a repeated target, contextualization may be influenced by the second occurrence. This might occur either because contextualization is slow and thereby sensitive to new information or because it is fast but can be undone by new information.

In agreement with Kanwisher (1987, 1991), we suggest that subjects may fail to discriminate between two occurrences of the same type and therefore instantiate only one token. However, it remains for the subject to decide whether that token originated from the first or second position. (See Bavelier, 1994, for a similar suggestion.) Just as Whittlesea, Dorken, and Podrouzek (1995) argued that the direction of memory migration can be influenced by expectations and constraints, we similarly argue that the contextualization decision can be so influenced. Here, we appeal to fairly straightforward ecological constraints: If one wants to know when an object appeared, it is its onset (not its persistence or offset) that is most relevant to that decision. But, if one wants to locate an object, it is the last location in which it was seen (not the first) that is most relevant.

When subjects are expecting spatial report cues (Experiments 1a, 1b, 2, and 5), it is likely that they attend especially to location during encoding of the targets. When a target letter is repeated, subjects may encode only one instance of that letter as having occurred. Because the most recent location is more salient, the instance is attributed to that location, leaving the subject more uncertain about what occurred in the first location. On the other hand, when subjects are expecting temporal report cues (Experiments 3 and 5), the initial appearance is more salient, and therefore the token is attributed to the first presentation, leaving the second target more uncertain.

Both the memory-migration hypothesis and the present version of tokenization failure attribute RB to a failure to assign a token to the appropriate context, that is, its list position. However, whereas Whittlesea, Dorken, and Podrouzek (1995) argued that two tokens for repeated targets are as likely as two tokens for two different targets, our account retains Kanwisher's (1987, 1991) basic premise that two tokens are less likely for repeated targets. The difference between (a) two tokens that cannot be distinguished and (b) a single token may be a rather subtle (and possibly useless) distinction. However, consistent with Kanwisher (1987, 1991), the present account also asserts that the mechanism underlying RB is on-line, affecting how the stimuli are encoded. In contrast, memory migration is assumed to occur at the time of retrieval, when subjects are attempting to reconstruct the events for report. Insofar as the different pattern of RB for spatial versus temporal cues failed to occur when subjects could not anticipate the type of report cue during encoding, we believe that the evidence favors an on-line account of RB.

In any case, our study's data are clearly most difficult for the other five proposed explanations of RB. That substantial RB can occur for the first of two identical targets discounts type refractoriness (Luo & Caramazza, 1995) as an explanation and cannot be explained by tokenization failure (Kanwisher, 1987, 1991) without a distinction between instantiation and contextualization. That substantial RB can occur for the first of two reported targets discounts any explanation that depends on prior report of a similar item, such as output interference, guessing bias, or censorhip bias (Fagot & Pashler, 1995).

More generally, our results are problematic for studies that have concluded that RB does not occur in a cued-report paradigm (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Lewandowsky et al., 1997). We acknowledge, however, that a more perceptual basis of RB in our experiments does not preclude the possibility of an additional cause of RB in other paradigms. We note that the experiments that found RB in whole-report but not cued-report procedures used many more items per trial and hence a greater memory load than the present experiments. Interference between similar memories at the time of retrieval certainly seems more plausible with greater memory loads (as argued by Luo & Caramazza, 1995); it is possible that such experiments were sufficiently sensitive to memory-retrieval interactions but lacked the power to detect perceptual effects that presumably would affect cued report as well as whole report. Nonetheless, if there are perceptual causes of RB, then those causes should remain in effect even when memory-retrieval effects are superimposed on them. Thus, it will remain a challenge for researchers to adequately distinguish between on-line and off-line effects in the same experiment.

The temporal parameters of RB must also be considered in generalizing our results to those of other studies. In our procedures (modeled on Luo & Caramazza, 1995), the first and second targets were each presented for 50 ms, with a zero interstimulus interval (ISI). In contrast, most studies of RB have used a slower rate of presentation—typically around 100 ms per item—with at least one intervening item. (Quite obviously, a zero ISI for repetitions is not practicable in experiments that present all stimuli at the same location.) RB in such studies clearly diminishes with longer delays between repetitions (e.g., Kanwisher, 1987), and it is likely that all theories of RB predict a maximum RB effect with minimum temporal separation of repetitions. Nonetheless, it is possible that the very close succession of critical stimuli in the present experiments, with consequently greater temporal overlap of processing, introduced a cause of RB that is not present in experiments with slower presentation rates.

In closing, we note here the usefulness of transfer-inappropriate processing (Neill & Mathis, 1998) as a framework for understanding performance decrements due to repetition, such as RB. On a purely empirical level, the presence of such a decrement automatically implies some kind of negative transfer of processing between similar events, just as any facilitation due to repetition (i.e., priming) necessarily implies transfer appropriateness. However, just as the transfer-appropriate processing framework (Blaxton, 1989; Morris, Bransford, & Franks, 1977; Roediger & Blaxton, 1987) has usefully focused attention on the importance of similar processing requirements between separate instances, transfer-inappropriateness focuses attention on how processing requirements differ. Thus, negative priming (Neill, 1977; Neill & Westberry, 1987; Tipper, 1985; Tipper & Cranston, 1985) can be understood as the conflict between ignoring one instance of some object and the requirement to respond to another instance of the same object. Similarly, inhibition of return (Posner & Cohen, 1980, 1984) might be understood as the conflict between an instance of withdrawing attention from a particular location and the subsequent requirement to reallocate attention to that same location.

Superficially, the processing requirements for repeated stimuli seem very similar in experiments on RB; that is, the subject must usually identify both stimuli (in addition to any other stimuli that might occur in the same sequence) and encode them in a manner suitable for report. However, the orientation of transfer inappropriateness led us to question, how do they differ? The answer supported by these experiments is that they conflict in the requirement to assign an instance to one position in a list versus assigning it to a different position. Furthermore, an important role of ecological constraints is strongly suggested by the finding that the relative magnitude of RB on the first or second target depended on whether subjects had to attend to presentation order or location. That is, as noted earlier, if one needs to know where an object is, the last instance of its occurrence is obviously the one that should be favored; conversely, if one needs to know when an object appeared, it is the first instance that is most relevant.

Finally, we began this article with the observation that facilitation of processing by repeated stimuli often appears to be the general rule of cognition. As such, phenomena like RB, negative priming, and inhibition of return appear to be aberrant and unrelated exceptions to the general rule. However, the possibility of a common framework for describing such phenomena suggests that facilitation by repetition may not, in fact, be the general rule. Rather, transfer-appropriate processing is just one side of the coin, and transfer-inappropriate processing may be of just as much importance as a general principle for understanding cognition.

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