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When Words Collide: Facilitation and Interference in the Report of Repeated Words From Rapidly Presented Lists

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Inhibited encoding is the basis of some accounts of repetition blindness—impaired report of the second occurrence of a repeated word in a rapidly presented word sequence. The author presents evidence for the claim that repetition effects arise from constructive processes of perception and memory that occur to some extent after the word sequence has been presented. Unpredictable postlist cues prompted subjects to report either the entire list or just the final word in the list. Repetition impaired the report of the second occurrence of a repeated word under full report but facilitated the report of such items when only the final word had to be reported. The author modulated this dissociation by presenting repeated words in sentences rather than unrelated word lists. The sensitivity of the effects of repetition to postlist cues supports a construction rather than an encoding inhibition account of repetition blindness.

Perception and memory of events have been characterized by some as acts of construction or reconstruction (Bartlett, 1932; Gregory, 1970; Helmholtz, 1866/1962; Neisser, 1967, 1976). A crucial aspect of such a view is that experience arises from a dynamic interaction between environmental stimuli and representations in memory. Powerful examples of this interaction have been provided by demonstrations of the malleability of memory under suggestive influences (E. F. Loftus & Palmer, 1974; Roediger & McDermott, 1995) and compelling visual illusions (Coren & Girgus, 1978). The constructive nature of memory and perception carries substantial implications for how one might interpret phenomena such as the varied influence of stimulus repetition on performance. On one hand, stimulus repetition can lead to improved identification performance (e.g., Forster & Davis, 1984; Jacoby & Dallas, 1981), but on the other, it can also produce a substantial deficit in the ability to report the occurrence of an event (e.g., Kanwisher, 1987). The latter phenomenon, typically referred to as *repetition blindness,* is of interest in part because of its counterintuitive nature—it is odd that a repeated stimulus should be particularly difficult to perceive or report. This negative effect of repetition is also interesting because of the propensity to attribute repetition deficits to some form of inhibitory process (Chun, 1997; Kanwisher, 1987; Luo & Caramazza, 1996).

The original and most compelling inhibition-based account of repetition blindness is the *type-token account* proposed by Kanwisher (1987; Kanwisher & Potter, 1990; Park & Kanwisher, 1994; see also Chun, 1997). According to this account, presentation of a word in a rapid serial visual presentation (RSVP) list activates that word's type representation in a mental lexicon. For the word to be reported, however, the subject must encode its presentation as a unique episode by forming a token of its specific occurrence in the RSVP list. Repetition blindness is attributed to an inhibitory process whereby for some period of time after a type representation has participated in tokenization of a word, that type cannot create an additional token. Thus, tokenization of the second presentation of a repeated word fails, and it cannot be reported. In the type-token account of repetition blindness, then, the impaired report of a repeated item is attributed to a failure of encoding or perception that occurs at the moment a repeated word is presented in an RSVP list (see also Hochhaus & Johnston, 1996; Johnston, Hochhaus, & Ruthruff, 2002).

An alternative explanation of repetition blindness situates the impairment at the stage of reporting from memory. Armstrong and Mewhort (1995) and Fagot and Pashler (1995) proposed that the inability to reliably report the second member of a repeated pair of items is due to a failure in retrieval from memory rather than to a failure of perception or encoding. Both studies showed that if subjects were cued to report a single critical item from an RSVP list rather than the entire list of items, no repetition blindness effect would be found. Thus, repetition impairment was linked to the requirements of list recall and the possible influence of reporting or guessing biases that may have operated during recall.

There is now substantial evidence, however, indicating that repetition blindness is due neither to a mechanism specifically associated with recall nor to one involving a form of systematic reporting bias. Search tasks involving designation of a target set (e.g., vowels or two specific letters) in advance of an RSVP list led to a form of repetition blindness in which detection of two occurrences of the same target within a list was less likely than detection of two different targets within a list (Johnston et al., 2002; Kanwisher, Kim, & Wickens, 1996; Park & Kanwisher, 1994). This task has no postlist recall demands and provides no opportunity for reporting biases such as a bias against reporting an identified item more than once.

Although results from search tasks provide a strong case against the notion that repetition blindness is due to some form of report-

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ing bias, they fail to suggest any compelling explanation for the elimination of repetition blindness found by Armstrong and Mewhort (1995) and Fagot and Pashler (1995). These researchers compared full and cued report trials that were mixed so that subjects could not anticipate which type of report would be required on a particular trial. Therefore, it was reasonable to assume that the status of list items would be the same when the postlist cue indicating what should be reported (the full list or a single target) was presented. In these experiments, researchers eliminated repetition blindness by selective cuing of a repeated item that would have shown repetition blindness if full list report had been attempted. This dependence on reporting cues and requirements suggests a basis for repetition blindness that is not restricted to perceptual encoding.

A constructivist approach to perception and memory provides a promising framework for understanding how repetition blindness arises and how it can be modulated by factors such as postlist cues and reporting processes. Whittlesea and his colleagues (Masson, Caldwell, & Whittlesea, 2000; Whittlesea, 1997; Whittlesea, Dorken, & Podrouzek, 1995; Whittlesea & Podrouzek, 1995; Whittlesea & Wai, 1997) have articulated this framework and applied it to repetition blindness and the related phenomenon of *orthographic blindness* (the failure to report both members of a pair of orthographically similar words presented in an RSVP list). According to this construction account, conscious experiences of perception (e.g., identifying an object as a teapot) and memory (e.g., recognizing that this particular teapot is the one that leaks because of a crack) both arise from two fundamental processes: production and evaluation (see Whittlesea, 1997, for an elaborate discussion of these processes). The production of a mental event (e.g., a word coming to mind while viewing an RSVP list) can be influenced by representations of prior experiences (as in repetition priming) and cued by present circumstances (the actual stimulus being viewed). But the mental event is not a copy of a prior experience or a copy of the stimulus currently being viewed. Rather, it is a construction based on an interaction of elements of the current environment (stimulus, task, and context) and representations in memory. Therefore, the content of a mental event may or may not closely fit the current stimulus (in the case of perception) or some prior experience (in the case of remembering). Thus, a briefly viewed word may be misperceived as some other, related, more common, or recently presented word (e.g., Allport, 1977), or, during a recall test, a nonpresented word may come to mind after exposure to a word list consisting of its strongest associates (e.g., Roediger & McDermott, 1995).

The production of a mental event (e.g., a word coming to mind) does not itself qualify as a conscious experience of perceptual identification or of remembering. A second process, evaluation, consists of an attribution regarding the source of that mental event and defines the event as an experience of perception of a current stimulus, as remembering a past event, or perhaps as a product of one's imagination or a mistaken interpretation of evidence. These attributions are assumed to be a form of unconscious inference (Helmholtz, 1866/1962) that is affected by factors such as the fluency with which the mental event was constructed (Jacoby & Whitehouse, 1989; Whittlesea & Williams, 2000) and the availability of likely sources for the event (Marcel, 1983; Whittlesea, Jacoby, & Girard, 1990).

The crucial point in the construction framework is that the conclusion an observer generates as a result of producing and evaluating a mental event is not a direct consequence of the nominal stimulus that was presented. Instead, an experience of perception or remembering (and other conscious experiences) is a constructed interpretation influenced by intentions, attentional constraints, context, and prior experiences. This stance is fundamentally different from a type-token account of perception and memory. In that account, a direct relation is assumed between a representation of an event (a token) and perception or memory of that event (elevating the token to consciousness): A failure of perception or memory implies a failure of tokenization.

In a construction account, however, impaired report of a repeated item arises from the indirect nature of the relation between the production of a mental event and conscious experience of perception or memory mediated through evaluation. In particular, the evaluation of the mental event arising from the encoding of a repeated word is likely to be affected by prior events. For the observer to attribute a second experience of an item (whether in the context of recalling a list or detecting targets online) to a repeated occurrence of that item, there must be contextual evidence to support the differentiation of this occurrence from the earlier one. In an RSVP list, that support would consist primarily of the items occurring in positions adjacent to the two instances of the repeated item. Because of the rapid presentation of items, however, the representation of an item's context is likely to be highly impoverished and frequently will fail to adequately distinguish the two separate occurrences of a repeated word. The result is impaired report of a repeated pair—only one member of the pair is reported, even though that report is supported by two separate mental events. The sometimes-strong tendency for the missed item to be the second member of the pair is due to the propensity to reconstruct a list in its original temporal order, particularly if the list comprises a meaningful sentence (Whittlesea et al., 1995).

It is important to distinguish the construction account of repetition blindness from the retrieval failure accounts proposed by Armstrong and Mewhort (1995) and Fagot and Pashler (1995), particularly because the construction account has been mischaracterized in recent articles as predicting that repetition blindness should occur only in a recall task (Johnston et al., 2002; Morris & Harris, 2002). In contrast to retrieval failure accounts, constructive processes are not restricted to memory retrieval operations associated with recall tasks. Recognition memory is also a constructive process (e.g., Roediger & McDermott, 1995), as is online, conscious perception of word identities. As in recall tasks, recognition and search tasks also require that subjects evaluate the production of mental events and attribute those events to a relevant source. Here, too, the lack of contextual support for two separate occurrences of a repeated item can lead to repetition blindness.

The distinction between the construction account of repetition blindness and the type-token account is less clear than the differences between memory retrieval and type-token accounts, given that in the construction account, conscious perception as well as memory processes are deemed to be influenced by constructive processes. Nevertheless, the construction and the type-token accounts continue to be clearly separated by the reliance on the concepts of activation (of types) and inhibition (tokenization failure) in the latter case versus task-dependent construction in the former case. I designed the experiments reported here to capitalize on this separation and to build on the fact that word repetition in an RSVP list can lead to either of two opposite effects: repetition priming or repetition blindness.

Kanwisher (1987) demonstrated that repetition blindness, obtained when subjects were required to report an entire list of words, reverted to repetition priming if subjects were instead instructed to report only the final word from an RSVP list (the final word was or was not a repetition of an earlier word). Kanwisher's explanation for this task-dependent change in the influence of repetition, on the basis of the type-token account, was that when reporting only the final word of a list, subjects were not likely to attempt tokenization of words occurring early in the list. Those early words would still activate their corresponding type representations, but the events would not be tokenized as instances that would support later memory. Because the first occurrence of a repeated word was not likely to be tokenized, the tokenization of its second occurrence (the last word in the list) would not be subjected to inhibition, and so no repetition blindness would occur. Moreover, the earlier activation of the repeated word's type by the first occurrence of that word would increase the probability that the final word would be encoded and correctly reported, leading to repetition priming.

Repetition priming is not always found when subjects are required to report only the final word of an RSVP list (Kanwisher & Potter, 1990; Luo & Caramazza, 1995; Whittlesea et al., 1995). Researchers have explained this inconsistency according to the type-token account by noting that two opposing factors operate on repeated list items: repetition priming at the stage of activating a word's type representation and repetition blindness at the stage of token individuation (Downing & Kanwisher, 1995; Park & Kanwisher, 1994). The balance of these two factors determines whether the final-word report task shows priming.

In a construction account, repetition priming can arise in the final-word report task because subjects reconstruct the list in a very different way to meet these task demands as compared with the way they reconstruct it when the entire list is to be reported. In particular, in final-word report, reconstruction of the list no longer gives precedence to early list items (among them the first occurrence of the repeated item). With emphasis on constructing a conscious identification of the final word, it is possible that evidence derived from the first presentation of a repeated word would be mistakenly attributed to the final word, enhancing its probability of report and generating a priming effect.

The crucial distinction between the type-token and construction accounts of repetition blindness versus priming is that the type-token account is confined to processes operating at the time of word encoding, whereas the construction account emphasizes that processes operating after list presentation can play a significant role in determining how subjects consciously perceive and report a repeated list item. In the experiments reported here, I examined this distinction between the two accounts by randomly mixing two reporting tasks across trials. Subjects were required either to report the entire list of words or to report only the final word of a list, but the reporting requirement was not cued until after the list had been presented. Therefore, subjects would necessarily be engaged in the same encoding operations (including type activation and token individuation in the type-token account) during list presentation on both types of trial. Armstrong and Mewhort (1995) and Fagot and Pashler (1995) used this postlist cuing method for mixing two different reporting tasks in their demonstrations of how cued recall can eliminate repetition blindness. The logic here was similar to that underlying those earlier studies: Only after the reporting cue was received would processing operations diverge.

In the type-token account, the fate of the second member of a repeated word pair would have to be the same in both reporting tasks because by the time the report cue was presented, opportunities for tokenization would have passed. Thus, regardless of whether repetition blindness or repetition priming is found, that pattern should occur for both reporting tasks. In the construction account, however, different reconstructive processes would likely be invoked by the two different reporting requirements, opening the possibility that repetition blindness would be seen under full report, but repetition priming would occur when only the final word was to be reported.

An additional factor was manipulated in these experiments, namely, the duration for which the second member of a critical word pair was presented when subjects were cued for final-word report (same duration as other list items vs. shorter duration). In her original demonstration of repetition priming in the final-word report task, Kanwisher (1987) used a shorter duration for the final word than for other list items, presumably to avoid ceiling effects in reporting accuracy. In the present experiments, half of the subjects were shown the last word on final-word report trials for a short duration as well. It might be suggested that the use of a short duration for the final list item would provide subjects with an early cue to report requirements for such trials because no reduced durations were used on full-report trials. Therefore, another half of the subjects were always shown the final word on final-word report trials at the same duration as the other list items. This arrangement ensured that subjects had no possible hint of the reporting requirements for a given trial until the postlist report cue appeared.

Experiment 1

In Experiment 1, subjects were presented with RSVP lists of unrelated words and were cued unpredictably to report either the entire list or just the final word. The report cue was presented after the list had been shown, so whatever list-processing strategy subjects might adopt would have to be applied in both reporting conditions. Only postlist-processing operations would differ as a function of report task. In the final-word report condition, the final word was presented for a reduced duration for one group of subjects and for the same duration as the other list items for another group.

Method

Subjects. Forty students at the University of Victoria, British Columbia, Canada, participated in the experiment for extra credit in an introductory psychology course. Half of the subjects were randomly assigned to each of the final-word duration conditions.

Materials. I created a set of 64 6-word lists, each of which contained 2 critical words (C1 and C2) in the second and fourth or in the third and fifth positions. In one version of these lists, C1 and C2 were identical words (e.g., *unit deep rock thin rock last*) and in another version, an unrelated word was substituted for C1 (e.g., *unit deep jump thin rock last*). These lists were used in the full-report condition. I created two additional versions of the 64 lists for the final-word report condition by changing the location of C1 and C2 in the first two list versions. The C1 and C2 items were moved to the fourth and sixth positions in each list (e.g., *unit deep thin rock last rock*). The 128 words used as C1 and C2 items ranged from 1 to 370 per million in normative frequency of occurrence (Kučera & Francis, 1967) with a median frequency of 35. Each subject was presented with all 64 word lists, with 16 different lists appearing in each of the four versions. Assignment of list versions to subjects was counterbalanced so that each version of a list was tested equally often.

Procedure. Lists were presented to subjects on a computer display, with each word appearing in lowercase letters at a fixed, central location for 120 ms, immediately followed by the next word in the list. Each list began and ended with a 120-ms display of a row of ampersands, which served as a mask. Finally, a cue consisting of either a row of eight question marks or a single question mark was presented to cue the subject to report either the full list in the order in which the words were presented or just the last word, respectively. For subjects in the 75-ms final-word duration condition, the final word in the lists cued for final-word report (C2) was presented for 75 ms instead of 120 ms. Subjects were first presented with 4 practice lists followed by the 64 critical lists in random order.

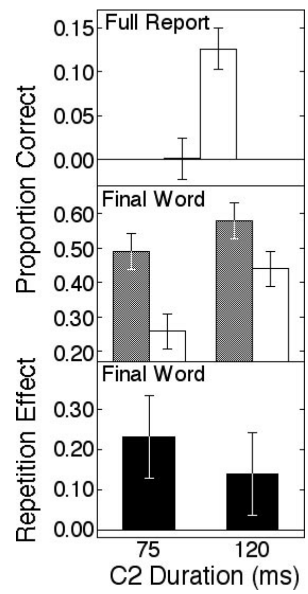

Figure 1. Mean joint report of C1 and C2 in the full-report task (top), mean C2 report in the final-word report task (middle), and mean repetition effect in C2 report in the final-word report task (bottom) for Experiment 1. Error bars are 95% within-subject confidence intervals for the top two panels and 95% between-subjects confidence intervals for the bottom panel (Masson & Loftus, 2003). Shaded bars indicate repeated condition, open bars indicate unrepeated condition, and black bars indicate repetition effects. C = critical word.

Results and Discussion

In scoring responses to full report of lists containing a repeated word, it was frequently not possible to determine which occurrence of the repeated word, C1 or C2, had been omitted when only one occurrence was reported. Therefore, report of C1 and C2 in the full-report condition was scored as the proportion of trials on which joint report of both C1 and C2 occurred (e.g., Armstrong & Mewhort, 1995; Luo & Caramazza, 1995; Park & Kanwisher, 1994). The proportion of joint report of C1 and C2 as a function of repetition is shown in Figure 1. The means are shown with 95% within-subject confidence intervals (G. R. Loftus & Masson, 1994; Masson & Loftus, 2003). Because the manipulation of duration of C2 in the final-word report condition was expected to have no influence on full report, and in fact had no such influence, full-report data in Figure 1 are collapsed across the two groups of subjects. In the repeated condition, joint report rarely occurred, whereas in the unrepeated condition, report of C1 and C2 (which were different words) was more frequent, constituting a repetition blindness effect. Scores in the repeated condition were near floor and consequently had restricted variance, so one might argue that confidence intervals based on an analysis of variance was not the most appropriate method of assessing the pattern of means. Therefore, I applied a randomization test, which makes no distributional assumptions (e.g., Edgington, 1987; Fisher, 1935), to these data. This test indicated that the two means for joint report were reliably different ($p < .01$) validating the conclusion that a repetition blindness effect occurred with full report.

The mean proportion of final-word report as a function of repetition and final-word duration is also shown in Figure 1. In contrast to the full-report results, repetition made it more likely that subjects would report C2 in the final-word report task. This repetition priming effect was of similar magnitude for both duration conditions, as indicated by the similar repetition effect sizes for the two duration conditions depicted in Figure 1.

In her assessment of final-word report data, Kanwisher (1987) considered the possibility that the enhanced final-word report that she observed was due to subjects' guessing C1 as the last word. In the repetition condition, this mistaken report would lead to a correct response, whereas in the unrepeated condition, it would be an error. Kanwisher suggested correcting for this possible artifactual repetition benefit by using report of C1 in the unrepeated condition as an estimate of C1 guessing in the repeated condition. One could then use this estimate to remove the guessing effect in the repeated condition by subtracting it from correct report in that condition.

Application of this correction, however, rests on assumptions about how subjects go about reporting list words. In particular, it assumes that subjects have words available for report and are then faced with the problem of determining their respective positions within the list, guessing if necessary. Moreover, use of this correction presupposes that the C1–C2 relation (repeated vs. unrepeated) has no impact on the conscious identification of C2 or on the ability to determine its correct position. These assumptions are problematic under a construction account of perception such as that proposed here. In that account, the C1–C2 relation potentially affects the reconstruction of C2, including impressions of where that item occurred in the list. In particular, the constructed identification of C2 is not a protected process independent of inferences about where or how often that item occurred in the list. For this reason, I do not emphasize results based on the application of the correction suggested by Kanwisher (1987) here. I do, however, present information on the proportion of C1 items in the unrepeated condition that were mistakenly reported as the last word in the final-word report task and indicate any deviations in the pattern of results observed when final-word report in the repeated condition is corrected by subtracting this proportion from correct C2 recall in that condition.[1]

In Experiment 1, the mean proportion of trials on which C1 was reported in the unrepeated condition was .08 for the 75-ms duration group and .07 for the 120-ms duration group. When final-word report in the repeated condition was corrected as described above, no changes in the pattern of repetition effects in final-word report were found.

The results of Experiment 1 clearly show that both facilitation and interference can be produced by word repetition, depending on the reporting requirements that are imposed on the subject. Contrary to the type-token account of repetition blindness and repetition priming forwarded by Kanwisher (1987), it cannot be the case that priming arises because subjects failed to tokenize the first occurrence of a repeated word. In that account, if subjects had not routinely tokenized C1, then there would have been no basis for the reduced joint report of C1 and C2 seen on repetition trials in the full-report condition.

Experiment 2

In Experiment 1, subjects were not explicitly warned about the potential repeated occurrence of a word within a list. This lack of information may have made them reluctant to report both occurrences of a repeated critical word, as witnessed by the very low joint report of C1 and C2 in the repeated condition of Experiment 1. Moreover, the use of lists of unrelated words (as compared with sentences; Kanwisher, 1987) makes it difficult and sometimes impossible to specify which of the two occurrences of a repeated critical word was reported when only one instance is included in a subject's report. I addressed these two issues in Experiment 2 by warning subjects about the possibility of repetition and by providing a report cue designed to target the report of C1 and C2 in the full-report task. The cue consisted of a list of the filler words presented on a trial with only C1 and C2 omitted from the cue. The subject's task was to report the two missing items. In the case of a repetition trial, placement of the critical word in only one of the two locations was taken as evidence for report of only the first or the second occurrence of the repeated item. Technically, this task is a part-list report task rather than a true full-report task. Nevertheless, I call it a full-report task to link it with the full-report task used in Experiments 1 and 3, and because it involves attempted report of both critical words. Final-word report trials were conducted in the same manner as in Experiment 1.

Method

Subjects. Thirty-two new subjects were drawn from the same pool as in Experiment 2, with 16 subjects randomly assigned to each of the two duration conditions.

Materials. A modified version of the critical six-word lists from Experiment 1 was used. Each list was structured so that C1 and C2 appeared in the fourth and sixth positions, respectively. This was done so that C2 occupied exactly the same list position for both the full-report and final-word report tasks. A full-report cue was constructed for each list, consisting of the list items in their original order but with two blank positions (indicated by a series of underscore characters) replacing C1 and C2. Eight filler lists were constructed, and their full-report cues were set up so that the probed list positions were the second and fourth position for four of the lists and the third and fifth position for the other four. Two lists of each type presented a repeated word in the two probed positions. These fillers were used to prevent subjects from becoming certain that the probed items would always be in the fourth and sixth positions in a list.

Procedure. I used the same procedure as I used in Experiment 1 except that (a) eight filler lists cued for full report were randomly mixed with the 64 critical lists presented to each subject; and (b) full report was cued not by a row of question marks but by the full-report cue consisting of list items with C1 and C2 missing, and the task was to report the two missing words. As in Experiment 1, the presentation duration of C2 on final-word report trials was set to 75 ms for one group of subjects but remained at 120 ms for the other group. Subjects were explicitly warned that some lists would contain two occurrences of the same word and that they were to report both occurrences if the report cue required it.

Results and Discussion

The use of a report cue in the full-report task permitted separate scoring of C1 and C2 report. Mean proportions reported for these items in the full-report task are shown in Figure 2. These means are again collapsed over the two groups of subjects, as duration of C2 in the final-word report condition did not influence full report. In addition to the usual deficit in report of C2 in the repeated condition, establishing a repetition blindness effect, there was also a benefit in the report of C1 in that condition. Effects of the latter kind have been reported before, particularly in cases in which C1 and C2 are orthographically similar rather than identical (Masson et al., 2000). Under a reconstructive memory account of repetition and orthographic similarity effects in rapidly presented lists, this benefit for C1 is explained as mistaken attribution of evidence associated with C2 to the earlier C1 item. A point of caution is in order regarding the influence of repetition on C1 report in the present case, however, because the C1 items in the unrepeated condition were not counterbalanced with the C1 items in the repeated condition in construction of the stimulus lists. Therefore, the effect of repetition on C1 seen in Figure 2 could be an artifact of differences between item sets. This caution does not apply to the results for C2 because the same words were used in that position for both repeated and unrepeated conditions in a counterbalanced

[1] An alternative correction, proposed by a reviewer, is based on the suggestion that under the type-token account, C2 in the repeated condition would not be seen (subjects are repetition "blind"). On repeated trials, then, C1 would effectively be the second-to-last item on the list. For unrepeated trials, however, C2 would presumably be seen, so functionally, the second-to-last word on these trials would be the one intervening between C1 and C2. According to this logic, the appropriate correction for intrusion of C1 into final-word report on repeated trials would be the subtraction of the probability of reporting the word intervening between C1 and C2 on unrepeated trials. The problem with this proposal is that failure to report C2 in the unrepeated condition, like failed report of C2 in the repeated condition, implies that C2 has not been accurately perceived or remembered as the final list item. Thus, in either condition, when the final word is not reported, the next candidate nominally in line would be the intervening item. Indeed, it typically is the intervening item that is the most frequent intrusion in the final-word report task. Further, in support of the idea that the intervening item has the same status for repeated and unrepeated lists, data from Experiment 1 show that the probability of reporting the intervening item as an intrusion was the same for both the repeated and the unrepeated list conditions (.21). It does not seem reasonable, therefore, to treat C1 in the repeated condition as though it occupied the same list position as the intervening item in the unrepeated condition.

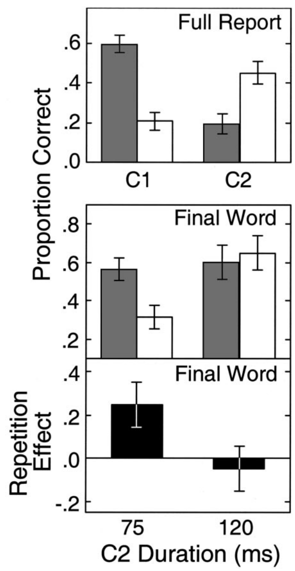

Figure 2. Mean report of C1 and of C2 in the full-report task (top), mean report of C2 in the final-word report task (middle), and mean repetition effect in C2 report in the final-word report task (bottom) for Experiment 2. Error bars are 95% within-subject confidence intervals for the top two panels and 95% between-subjects confidence intervals for the bottom panel. Shaded bars indicate repeated condition, open bars indicate unrepeated condition, and black bars indicate repetition effects. C = critical word.

assignment across subjects. Finally, when full report was scored on the basis of joint report of C1 and C2, as in Experiment 1, the results from that study were replicated in two senses. First, very few instances of joint report were observed (.01 vs. .08 for the repeated and unrepeated conditions, respectively). Second, joint report was reliably higher in the unrepeated condition by a randomization test ($p < .01$).

The mean proportions of C2 items reported in the final-word report task are also shown in Figure 2. In contrast to the repetition blindness effect seen in the full-report task, the final-word report task yielded a repetition priming effect for C2 report. This benefit was present, however, only when subjects were shown C2 for 75 ms, but there was no influence of repetition when the C2 duration was equal to that of the rest of the list items (120 ms). Application of the correction discussed in Experiment 1 did not change this pattern of results (the mean proportion of C1 intrusions in the unrepeated condition was .04 for the 75-ms group and .02 for the 120-ms group).

The dissociation between the full-report and final-word report tasks replicates the dissociation seen in Experiment 1, at least for subjects in the 75-ms duration condition. In both cases, word repetition led to repetition blindness in full-word report but to repetition priming in final-word report. One possible reason that priming was not seen in the 120-ms duration condition is that the power to detect an effect of the size seen in that condition in Experiment 1 was only .63. But even when the same duration was used for the final word as was used for other words in the list, no repetition blindness effect was seen in the final-word report task. This outcome contrasts with the clear repetition blindness effect seen for these same subjects on full-report trials. For the 120-ms condition, power to detect a repetition blindness effect in final-word report equal in size to that found in full report was greater than .95. Thus, reporting requirements clearly modulated the impact of repetition on subjects' conscious experience of list words.

Experiment 3

In Experiment 3, sentences were used in place of lists of unrelated words. There are no published reports of experiments using sentences presented under RSVP conditions for final-word report, so it is unclear whether a repetition priming effect would emerge under these conditions. Sentence frames have been shown to induce a bias toward shifting report of a repeated word toward an earlier position in the list (Whittlesea et al., 1995). This bias seems to be a result of the syntactic structure and semantic coherence provided by a sentence context. In such contexts, subjects may more readily identify words during the course of list presentation, so that more constructive perception of word identities occurs before the end of the list than is the case when lists consist of unrelated words. The greater accuracy in reporting RSVP sentences relative to unrelated word lists (e.g., Forster, 1970) is consistent with this supposition.

The tendency for report of a repeated C2 to migrate to the C1 position could undermine the repetition benefit seen in the final-word report task of Experiments 1 and 2. Thus, final-word report from sentences may fail to generate a reliable repetition advantage and could even produce a deficit like that seen in full report. As there is no basis for generating a firm prediction from the construction account, I conducted Experiment 3 as a test of the boundary conditions for observing repetition priming in the final-word report task. The type-token account of repetition blindness in full report and of repetition priming in final-word report could accommodate a repetition blindness effect in both report tasks if one assumes that subjects were attempting to individuate words in all cases. A repetition blindness effect in full report accompanied by no repetition effect or repetition priming in final-word report would be a problematic outcome for this account, as it was in Experiments 1 and 2.

Method

Subjects. Twenty-four subjects were drawn from the same pool as in Experiments 1 and 2. Half of these subjects were randomly assigned to each duration condition (75 ms and 120 ms) for the final-word report task.

Materials. I constructed a set of 48 sentences using the sentence materials described by Kanwisher (1987) as a model. Each sentence contained two occurrences of a critical word (C1 and C2), separated by one or two other words, in the middle part of the sentence. This version of the sentences was used in the full-report repetition condition. I created three additional versions of each sentence for use in the other conditions of the experiment. In one version, the first occurrence of the critical word (C1) was replaced with a different word that maintained sentence coherence, as in the following example:

The blue car and the red car had an accident

The blue truck and the red car had an accident

I created the other two versions of the sentences, which were used in the final-word report condition, by truncating each sentence after C2 as follows:

The blue car and the red car

The blue truck and the red car

The 96 words used as C1 and C2 items varied in frequency of use from 0 to 1,599 per million (Kučera & Francis, 1967) with a median frequency of 47.

Each subject was presented with 12 sentences in each of the four versions so that all 48 sentences were seen. Assignment of sentences to the four versions was counterbalanced across subjects so that each sentence was seen equally often in each version.

Procedure. The procedure was similar to that used in Experiments 1 and 2. Following 4 practice trials using four sentences that were different from the critical items, the experiment moved directly into 48 randomly ordered critical trials. As in Experiment 2, subjects were warned that some sentences would contain two occurrences of a word and that they were to report both occurrences when they noticed them. The requirement of reporting the entire sentence was signaled by a row of question marks at the end of the sentence. As in Experiment 1, subjects were responsible for reporting the full sentence—unlike in Experiment 2, no report cue containing noncritical words was provided. Final-word report was cued by presentation of a single question mark. In the final-word report task, half of the subjects saw the entire list at the same duration (120 ms), and the other half of the subjects were presented with the final word at a shorter duration (75 ms).

Results and Discussion

The use of sentences rather than lists of unrelated words meant that subjects typically reported a substantial number of words on each full-report trial. Therefore, it was usually very clear which member of a repeated pair of words had been reported when only one member appeared in a subject's protocol, allowing reports of C1 and of C2 to be scored separately. The mean proportions of C1 and of C2 report on full-report trials are shown in Figure 3. Duration of C2 in the final-word report trials had no influence on full-report results, so the data for that task shown in Figure 3 are collapsed across the two duration groups. Repetition of the critical word had no impact on report of C1, which was quite high, but it did substantially reduce report of C2, creating a repetition blindness effect. This pattern of results is very similar to that reported by Kanwisher (1987) for cases in which C1 and C2 were embedded in coherent sentences.

The report of C2 in the final-word report task, however, was quite unlike what Kanwisher (1987) found and unlike the results reported in Experiments 1 and 2. Mean proportion of C2 report in the final-word report task and the mean repetition effect in that task are shown in Figure 3. In contrast to that in Experiments 1 and 2, final-word report in Experiment 3 failed to generate any evidence of a repetition benefit in either duration condition, and there was no reliable deficit in C2 report due to repetition. When I corrected C2 report in the final-word report task for guessing by subtracting the proportion of C1 report in the unrepeated condition (.17 and .07 in the 75-ms and 120-ms duration conditions, respectively), there was a reliable repetition blindness effect. Even so, the size of the repetition blindness effect in the final-word report task (.20) was considerably less than the size of the effect in the full-report task (.32), a difference that approached significance at the .05 level, $F(1, 23) = 4.05$, $MSE = .022$, $p < .06$.

The results of Experiment 3 demonstrate the powerful nature of the context in which repeated words are shown. A coherent sentence context appears to draw evidence for C2 away from that

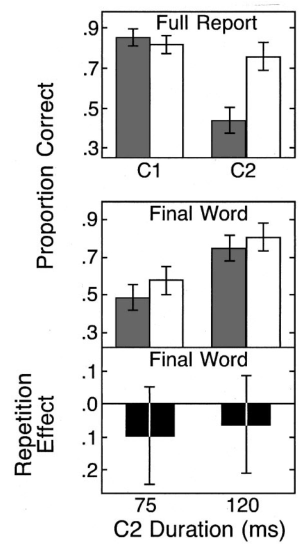

Figure 3. Mean report of C1 and of C2 in the full-report task (top), mean report of C2 in the final-word report task (middle), and mean repetition effect in C2 report in the final-word report task (bottom) for Experiment 3. Error bars are 95% within-subject confidence intervals for the top two panels and 95% between-subjects confidence intervals for the bottom panel. Shaded bars indicate repeated condition, open bars indicate unrepeated condition, and black bars indicate repetition effects. C = critical word.

item, even when C2 is the only word to be reported. That is, the mental event of C2 coming to mind is very likely to be evaluated as having its source in the earlier occurrence of that word (C1), leading the subject to report some other item (often the second-to-last word in the sentence) as the final word instead. Unlike that in the results found with unrelated word lists, then, final-word report did not benefit from repetition when sentences were used. In a construction account, one possible reason for the lack of priming is that with RSVP sentences, subjects are more likely to identify early list items, including C1, while the list is still being presented. Reconstruction of a coherent sentence might powerfully favor earlier list positions, encouraging the migration of a repeated C2 to its earlier position in the sentence (Whittlesea et al., 1995). Although this possibility was anticipated, it was not clearly predicted by the construction account. Nevertheless, the results of Experiment 3 serve to establish boundary conditions for the occurrence of repetition priming versus repetition blindness with RSVP displays.

In a type-token account, it could be assumed that the random mixture of full- and final-word report requirements led subjects to attempt tokenization of all words on all trials, thereby eliminating or even reversing (under the correction for guessing method of scoring) the repetition benefit seen in final-word report in Experiments 1 and 2. The type-token account, however, has no apparent means of explaining the dissociative effects of repetition seen in the full- and final-word report tasks of Experiments 1 and 2 or the trend for lower repetition blindness in final-word report as compared with full report obtained in Experiment 3.

General Discussion

Experiments 1 and 2 demonstrated a dissociation between two reporting tasks with respect to the influence of repetition on reporting words from RSVP lists. Under conditions of full report, repetition led to impaired performance, but when reporting was restricted to the last word in a list, repetition produced a priming effect. It is crucial that these two opposing effects of repetition occurred on randomly cued report trials with cues presented after each list, so that subjects could not plausibly have processed or encoded list words any differently as a function of reporting requirements. This dissociative pattern was predicted by the construction account of repetition blindness, but it creates a significant challenge for accounts grounded on types and tokens and the proposal that repetition blindness occurs because of inhibited token individuation. Clearly, postlist processing determined the direction of effect that repetition had on word report.

The dissociation between full- and final-word report shown here is similar in some ways to the dissociations reported by Armstrong and Mewhort (1995) and by Fagot and Pashler (1995) between full and cued report of letters presented in RSVP lists. In those experiments, repetition blindness was obtained with full report, but item-specific cuing eliminated, although it did not reverse, the effect. Johnston et al. (2002) have criticized those two earlier studies on various grounds, none of which challenge the results of the present experiments. In particular, they expressed concern about low levels of report in the unrepeated condition in the Armstrong and Mewhort data and about the possibility that subjects may have mistakenly reported C1 in the repeated condition when cued to report just C2. Erroneous report of C1 in this case would have led to credit for correct report of C2, thereby artifactually reducing the repetition blindness effect. In the present Experiments 1 and 2, a repetition priming effect was obtained in the final-word report task, so low levels of C2 report in the unrepeated condition would not have been an issue. These results held even when I applied a correction for guessing to avoid the other concern raised by Johnston et al. In any case, C2 report in Experiment 2 was quite high (over .40). The Fagot and Pashler results were questioned on the grounds that the two target items, C1 and C2, were made perceptually distinct from each other (by color, spatial location, or modality). In the present experiments, the only distinctive feature of C2 was its shorter presentation duration for half of the subjects. But even when the duration of C2 was equal to that of other list items, repetition blindness was eliminated in the final-word report task.

The results of Experiment 3, in which final-word report failed to produce a repetition priming effect when sentences rather than unrelated word lists were used, demonstrates a form of interaction between two factors that contribute to constructive processing: contextual support and task demands. The first presentation of a repeated item receives substantial contextual support from the grammatical and semantic coherence of a sentence, perhaps enabling conscious identification even before the RSVP display has been completed. Any subsequent attempt to direct constructive processes toward the final word of the presented sentence will operate under the constraints of the consciously constructed perception of earlier items. Under these conditions, the coming to mind of C2, when it is a repetition of C1, is quite susceptible to an evaluation that attributes that mental event to the earlier identification of C1.

Interpreting and Predicting Dissociations

The finding of a repetition blindness effect and a repetition priming effect within a single experiment is offered here as strong evidence in favor of a construction account of repetition effects. This interpretation, however, contrasts with Coltheart and Langdon's (2003) suggestion that such a pattern of results rules out construction and retrieval bias accounts. In particular, Coltheart and Langdon reported a repetition blindness effect for words but a repetition priming effect for nonwords presented under the same RSVP conditions as words. One might expect that retrieval bias would have similar effects on word and nonword targets, making the dissociation based on lexical status difficult to explain on that account. Constructive processes, however, could generate very different results for words versus nonwords under certain circumstances but could generate similar results under other circumstances.

For example, Whittlesea and Williams (2000) showed that in a recognition memory test, unexpected fluency associated with nonwords (items that were relatively easy to pronounce, e.g., *hension*) led to higher false alarm rates than it did for either words, which were even more fluent but whose fluency was entirely expected, or nonfluent nonwords (items that were harder to pronounce, e.g., *stofwus*). In the Coltheart and Langdon (2003) study, when subjects attempted to report an RSVP list of nonwords, the repetition benefit experienced when processing the second member of a repeated nonword pair could have had a considerable influence on one's subjective experience, particularly because there would have been little to explain that fluency other than prior occurrence in the RSVP list. Much less fluency might be expected for encoding a repeated but already highly familiar word and, in any case, that fluency could be attributed to a number of sources of prior experience other than the RSVP list. Thus, the heightened fluency of processing a repeated nonword might have led subjects in the Coltheart and Langdon experiments, who were aware that repetitions within a list were possible, to conclude that an item had been repeated.

This proposal gains plausibility from a finding by Campbell, Fugelsang, and Hernberg (2002) that involved repetition priming for nonwords but repetition blindness for words. Subjects viewed an RSVP sentence and then were presented with three probe words or nonwords and judged how many times each probe appeared in the sentence (zero, one, or two). For nonwords, subjects were more accurate in detecting two occurrences of a probe item than single occurrences of two different items (analogous to the priming effect reported by Coltheart & Langdon, 2003), but the reverse was true for word probes. Now consider that Whittlesea and Podrouzek (1995) showed that detection of repetition and illusory experience of repetition in a task like that used by Campbell et al. can be increased by enhancing the fluency with which the probe item is processed. These results suggest that the subjective experience of repetition, whether expressed in free report or in frequency judgments, can be influenced by the fluency with which an item is consciously perceived and, more particularly, the attribution that follows from that fluency.

In a construction account of the dissociative effects of repetition for words and nonwords in the Campbell et al. (2002) and Coltheart and Langdon (2003) studies, then, a different interpretation of the influence of lexical status is implied. Whereas Coltheart and Langdon took the dissociation to mean that only items with lexical representations in the form of types can invoke the inhibition of token individuation that leads to repetition blindness, a construction account instead emphasizes the nature of the production and evaluation operations involved with word and nonword targets. The construction account predicts that it should be possible to demonstrate repetition blindness for nonwords if reliance on a fluency heuristic for making repetition judgments is reduced or eliminated. Evidence consistent with this position was reported by Masson et al. (2000), who presented nonword–word pairs as C1–C2 items in RSVP lists and varied the orthographic similarity of these pairs (e.g., C1 = *chiof* and C2 = *chief*). When the duration of the nonword C1 was equal to that of other list items, report of an orthographically similar C2 item (a word) was enhanced relative to when C1 and C2 were different. But the reverse (a form of repetition blindness in reporting C2) was obtained when the duration of C1 was increased to make it readily identifiable. In experiments currently underway in my laboratory, we are testing repeated and unrepeated nonword pairs under conditions in which fluency is not likely to be a viable basis for judging repetition. Preliminary results are consistent with the prediction that repetition blindness can occur with nonword pairs.

A criticism of the construction account is that it is difficult to generate precise predictions from it. There are, however, two important points to consider when evaluating this aspect of the account. First, the account is based on a framework that emphasizes the importance of the specific conditions under which subjects are tested—the combination of stimulus, context, past experience, and a subject's intentions (e.g., Whittlesea, 1997). It is to be expected that experimental outcomes will vary when relevant components are modified. An important objective is to develop an understanding of how the basic principles of construction are shaped by the particulars of experimental contexts. As an analogy, consider Newtonian principles of motion.[2] If I hold two objects in my hand, it is possible to make a precise prediction about the speed and acceleration at which the two objects will fall to the ground if, for example, one is a marble and the other is a stone. One such prediction is that both objects, if released simultaneously, will reach the ground at the same time. But a different outcome is likely to be observed if one of the objects is a marble and the other is a feather—and yet a different outcome if the feather is wet. The basic principles of motion are not invalidated or rendered uninteresting by the case of the feather, but the account requires elaboration (e.g., to include influences of air resistance).

Second, although the full particulars are not yet in place, the account correctly predicted the existence of some conditions under which opposite effects of repetition of a target (blindness or priming) turn on the reporting requirements imposed on subjects after an RSVP list has been presented. This result is clearly not predicted by the prevailing type-token account of repetition blindness. Moreover, the construction account makes an additional prediction, as discussed above, regarding the purported lexical basis for repetition blindness. Contrary to the position taken by some advocates of the type-token account (e.g., Coltheart & Langdon, 2003), the construction account holds that repetition blindness is not lexically based and predicts that there are conditions under which it can be found with nonword targets—and the account can be used to guide the search for such conditions. In fact, as the final draft of this article was being prepared, a published report appeared verifying the prediction that repetition blindness can be obtained with nonwords (Harris & Morris, 2004).

Repetition Blindness as Perception or Memory?

Johnston et al. (2002) carefully considered the question of whether repetition blindness is more appropriately characterized as a perceptual or as a memory phenomenon. Their evidence for online influences of repetition in a target detection task supported a strong argument against the idea that repetition blindness can be attributed entirely to postlist memory recall processes. Rather, such effects appear to support a perceptual basis for repetition blindness. Nevertheless, Johnston et al. also accepted the idea that the perceptual basis for repetition blindness could be characterized as a "memory-encoding phenomenon" (p. 486). Memory encoding, however, is distinguished from recall processes such as guessing or censorship bias that would work against reporting repetitions.

In the construction account described here, it is assumed that constructive processes operate during conscious perception of stimuli as well as during conscious remembering of prior events. Thus, the distinction between perception and memory as alternative bases for repetition priming is not a central issue for this account. Rather, constructive processes can operate in either domain and can therefore be responsible for repetition-based phenomena that are detectable online and for related effects that clearly are influenced by postlist processes. Thus, whereas it is difficult to use a perceptual- or memory-encoding account of repetition blindness, such as the type-token account, to explain how the dissociative effects of repetition found in the present experiments could arise, the construction account readily accommodates such task-dependent modulation of performance.

In the case of repetition, the critical emphasis in the construction account is on the attribution or evaluation process in which an observer engages when deciding what to make of a second instance of a word coming to mind. Is the mental event a product of the original presentation of that item or the result of a second, repeated occurrence? It is the fallibility of this discrimination that lies at the heart of repetition blindness. This notion is quite similar to a modified type-token account offered by Neill, Neely, Hutchison, Kahan, and VerWys (2002). That account was motivated by the finding that, depending on how list report was cued, repetition blindness could be greater for report of the first member of a repeated pair than for the second member. This result does not fit the standard token individuation failure explanation of repetition blindness, whereby tokenization of one instance of a word prohibits its subsequent tokenization of a later instance of that same word. Neill et al., however, assumed that tokenization consists of two separate processes, instantiation and contextualization. The instantiation process involves recognition that a member of a conceptual category (e.g., a particular word) has occurred. The contextualization process consists of attributing that occurrence to a specific context (e.g., a list position). According to their account, the attribution or contextualization of an item's occurrence involves a decision that can be influenced by expectations and constraints. For example, if subjects are cued to report the item that occurred in a particular spatial location, the most recent occurrence of a repeated item tends to take precedence in the attribution process, leading to greater repetition blindness for the first occurrence. Although Neill et al. considered their instantiation–contextualization account to be a modified version of the type-token account, their approach is highly compatible with the construction account described here and its emphasis on production and evaluation processes (derived from Whittlesea's 1997 SCAPE

[2] I thank Bruce Whittlesea for suggesting this analogy.

framework). The processes of production and evaluation, respectively, seem analogous to Neill et al.'s instantiation and contextualization processes.

Another account of repetition blindness that uses the notion of evidence attribution is the responding optimally with unknown sources of evidence (ROUSE) model of short-term repetition priming developed by Huber, Shiffrin, Lyle, and Ruys (2001). The paradigm for which the model was initially designed involves the task of identifying a briefly presented, masked word target by selecting one of two alternative probes. Presentation of the target in this paradigm is preceded by two primes, one of which may be a repetition prime for the upcoming target. When the primes are presented for a relatively long interval, and subjects are encouraged to attend to them, subjects tend to prefer the foil probe if the target is repetition primed. Thus, target identification accuracy is reduced under repetition priming, an impairment that parallels repetition blindness in RSVP lists (see also Hochhaus & Johnston, 1996). In the ROUSE model, one accounts for that deficit by assuming that subjects discount evidence that potentially has been generated by an irrelevant source (i.e., evidence that could have arisen from the prime event is discounted). With a repetition prime, evidence from the target is susceptible to being mistakenly discounted via misattribution to the prime event. Huber et al. suggested that just as repeated presentation causes the same type to be activated in the type-token account, repetition causes "a failure to individuate the source(s) of activation for a given feature" (p. 177) in the ROUSE model. Huber et al. saw the removal of source confusion through discounting as analogous to failure of token individuation for the second presentation (i.e., the target). But in ROUSE, there is no inhibition of a tokenization process. Rather, the critical process involved in discounting is one of misattribution, which fits readily into the evaluation component of the construction account. Thus, as with the Neill et al. (2002) instantiation–contextualization account, processes that comprise the ROUSE model's explanation of repetition blindness are more naturally compatible with a construction than they are with a type-token account.

Given the similarity between the construction account and alternative positions that have been tied to the type-token account, why should one prefer the former? Perhaps the strongest motivation for favoring the construction account is that its emphasis on the constructive nature of both perceptual and memory processes provides significant encouragement to avoid being concerned only with representations that are formed during list presentation and instead provides for additional contributions from postlist processing. Although it is clear that influences of repetition can be observed while a list is being processed (e.g., Johnston et al., 2002), it is also clear that events occurring after list presentation can influence how reports of list contents are constructed. In particular, unpredictable postlist cuing of item report has now been shown in a number of contexts to yield dissociative effects of repetition and of orthographic similarity (Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Masson et al., 2000; Neill et al., 2002). These phenomena do not readily yield to a type-token account in which tokenization of a repeated word is inhibited. An alternative framework based on constructive processes offers a unified account of how online and postlist events can influence conscious perception and memory of RSVP list items.

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