MEMORY, 2005, 13 (3/4), 236–246

Psychology Press
Taylor & Francis Group

The time course of response suppression: No evidence for a gradual release from inhibition

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Most models of serial recall postulate that recalled items are suppressed and thus temporarily rendered unavailable. Response suppression can explain several results, for example the small number of erroneous repetitions and people's reluctance to report repeated items. Although it is clear that response suppression is not permanent (thus permitting renewed recall of an item on the next trial), nothing is known about its time course. We report two experiments that measured the time course of response suppression with a multiple cued-retrieval response-deadline method. Emphasis was on the extent of repetition inhibition for lists that contained a repeated item. Regardless of whether presentation was rapid (Experiment 1; 150 ms/item) or slow (Experiment 2; 500 ms/item), (a) the standard pattern of repetition inhibition and erroneous repetitions occurred and (b) repetition inhibition remained constant across increasing retrieval time. This suggests that the release from response suppression is a discrete, list-wide effect rather than a continuous, gradual wearing off. The latter conclusion is consistent with the operation of the SOB model (Farrell & Lewandowsky, 2002) but not with models that postulate complete suppression with gradual wearing off.

Many models of short-term serial recall assume that early list items are encoded with greater strength than later ones. This "primacy gradient" explains the pervasive primacy effect (Brown, Preece, & Hulme, 2000; Burgess & Hitch, 1999; Farrell & Lewandowsky, 2002; Henson, 1998b; Lewandowsky & Murdock, 1989; Page & Norris, 1998). Moreover, most models assume that an item, once recalled, is suppressed to prevent its repeated report. Response suppression is particularly crucial in models in which it is the strongest or most active item that is reported at each recall attempt (Farrell & Lewandowsky, 2002; Page & Norris, 1998). When this "competitive cueing" mechanism is combined with a primacy gradient, recall will automatically commence with the first list item. However, to avoid perseveration and to permit recall of the progressively less active later list items, each retrieval must be followed by (at least temporary) response suppression. (See also Vousden & Brown, 1998.)

The explanatory importance of response suppression stands in contrast to how little is known about it: On the empirical side, there is little direct evidence for the existence of suppression, and nothing is known about attributes such as its time course or whether it is under voluntary control. On the theoretical side, most models postulate the presence of response suppression without however providing a process implementation (although see Farrell & Lewandowsky, 2002). This article addresses the empirical deficit surrounding

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We thank Leo Roberts for his assistance during data collection. Preparation of this paper was facilitated by a large grant from the Australian Research Council to the second author.

http://www.tandf.co.uk/journals/pp/09658211.html DOI:10.1080/09658210344000233

response suppression and reports two experiments aimed at illuminating its time course.

THE CASE FOR RESPONSE SUPPRESSION

It has long been known that transposition errors primarily involve adjacent items, with few transpositions involving items two or more input positions away (e.g., Brown et al., 2000; Conrad, 1965; Henson, 1998b; Page & Norris, 1998). An additional contingency between successive transpositions is indicative of response suppression. Given the list A B C D E, if B is erroneously recalled first, people will tend to report sequences such as B A *x x x* or B C *x x x* (where *x* refers to any of the remaining list items) instead of BB x x x. The fact that B, once transposed, is rarely if ever recalled in its correct position suggests that its first report was followed by suppression.

The operation of response suppression is further supported by the absence of erroneous repetitions even when no transpositions have occurred. That is, if the first four items have been correctly reported as A B C D, it is unlikely that people will repeat any of those items a second time in the final output position. Instead, if they cannot recall E, they may report an extra-list item or prefer to omit the final item altogether. Empirically, even when recall is well below ceiling, erroneous repetitions constitute only between 2% (Henson, Norris, Page, & Baddeley, 1996) and 5% (Vousden & Brown, 1998) of all responses.

Finally, people's reluctance to repeat previously recalled items extends to cases in which such repetition is required; namely, when lists contain a repeated item (e.g., Henson, 1998a; Jahnke, 1969b). The failure to recall a repeated item twice constitutes *repetition inhibition* and has been studied under rapid serial visual presentation (RSVP) conditions, where each item is presented for around 100 ms (Fagot & Pashler, 1995; Kanwisher, 1987; Luo & Caramazza, 1996), and with the slower presentation rates typical of short-term memory research (Crowder, 1968b; Harris & Jahnke, 1972; Henson, 1998a; Jahnke, 1969b). With RSVP, repetition inhibition has often been attributed to encoding failure (hence it is often referred to as "repetition blindness" with RSVP). When encoding failures are prevented by slower presentation rates, repetition inhibition (often referred to as the "Ranschburg effect") is thought to be at least partially due to response suppression (e.g., Armstrong & Mewhort, 1995; Henson, 1998b; Vousden & Brown, 1998).

In the experiments reported here, we assume that repetition inhibition is not solely due to encoding failure and use it as an index of response suppression. Support for this assumption has come from Armstrong and Mewhort (1995) and Lewandowsky and Neely (1998), who eliminated repetition blindness with RSVP by altering retrieval demands. In a *whole report* condition, participants recalled all list items and showed the expected repetition blindness. In another *partial report* condition, participants were given only one list item as a cue and were asked to report the next item in the sequence. In this single-cue condition, both Armstrong and Mewhort (1995) and Lewandowsky and Neely (1998) found no trace of repetition inhibition—people's ability to report the first or second occurrence of a repeated target when cued with the previous item was indistinguishable from performance on non-repeated items on a control list. Consistent with a response suppression account, Lewandowsky and Neely (1998) showed furthermore that inhibition re-emerged even with cued retrieval when recall was probed repeatedly.

The results of Armstrong and Mewhort (1995) and Lewandowsky and Neely (1998) are particularly telling because their RSVP methodology provided a highly favourable environment for encoding deficits. The finding that encoding was demonstrably intact, but (whole) report of repetitions nonetheless impaired, strongly favours a suppression account. In addition, these results have been generalised to longer presentation durations by Jahnke (1970) who abolished the Ranschburg effect by probing recall of a single item with either the preceding item or with a position probe. The additional fact that repeated items show facilitation in probed recognition tests but inhibition during serial recall further strengthens the position that repetition effects occur at retrieval, not encoding (see Crowder, 1968a, for a related discussion; see also Jahnke, 1969b; Wolf & Jahnke, 1968). We conclude that repetition inhibition, irrespective of how rapidly the list is presented, is at least partially caused by response suppression.[1] It follows that whenever repetition inhibition is observed, the presence of response suppression can be inferred—a linkage that we exploit in the experiments below.[2]

[1] Aside from response suppression, a guessing bias has also been offered as an explanation of repetition inhibition (Greene, 1991; Mewaldt & Hinrichs, 1973). On this view, people are simply reluctant to repeat themselves, and if forced to guess, they will be more likely to produce an unrecalled item rather than a repetition. Although corrections for guessing bias can greatly reduce or eliminate repetition inhibition (e.g., Greene, 1991; Mewaldt & Hinrichs, 1973), it does not eliminate the effect. Inhibition effects persist after corrections for guessing, particularly when vocabulary size is small (see Hinrichs, Mewaldt, & Redding, 1973; Walsh & Schwartz, 1977). Repetition inhibition also persists when guesses and low confidence responses are removed from the analysis altogether (Henson, 1998a, Experiment 3). Finally, guessing cannot explain why people do not always consciously report a bias against guessing repeated items (Walsh & Schwartz, 1977), why errors in recall of repeated items are often omissions (Jahnke, 1972), or the fact that participants are known to detect repetitions in lists (Henson, 1998a). Thus, a guessing bias cannot be the sole explanation of repetition inhibition (see Henson, 1998a, for a more detailed discussion).

[2] To prevent misconceptions, we mention two potential implications that do *not* follow from our conclusion: First, we do not suggest that suppression is the sole cause of repetition inhibition. Second, we do not exclude the possibility that some repetition blindness phenomena observed with RSVP reflect encoding failures.

Concerning the former, there are aspects of repetition inhibition that response suppression alone cannot explain. For example, the extent of repetition inhibition is known to depend on separation of repeated items at input. When repetitions are adjacent or separated by only one other item, inhibition is reduced or reverses into facilitation (Henson, 1998a), an effect explained by people noticing the immediate repetitions and "tagging" the item for repeated report (Henson, 1998a). These results imply that other factors, including people's guessing strategies, contribute to repetition inhibition effects.

Concerning the latter, although there are extensive similarities between repetition blindness and other forms of repetition inhibition, the similarity breaks down in certain cases. The main issue is whether repetition blindness should be considered a special case of repetition inhibition. A common conclusion is that inhibition effects represent output processes (e.g., Armstrong & Mewhort, 1995; Crowder, 1968a; Greene, 1991; Henson, 1998a; Jahnke, 1969a). However, it has been argued that the retrieval demands of serial recall confound input and output processes (Fagot & Pashler, 1995). Furthermore, there are instances where inhibition appears to be a perceptual failure independent of output processes (Kanwisher, Kim, & Wickens, 1996). This had led to the conclusion that repetition blindness is a perceptual phenomenon distinct from other forms of inhibition (see Henson, 1998a). However, neither of these two issues compromises our main conclusion, that repetition inhibition provides a legitimate empirical index of response suppression.

THEORETICAL IMPLEMENTATIONS OF RESPONSE SUPPRESSION

Response suppression has been modelled in several ways. Here we focus on two accounts that predict the typical pattern of erroneous repetitions—a decrease across input position and a concomitant increase across output positions—using very different assumptions. The two accounts can be empirically differentiated by tracing out the time course of suppression.

One account assumes that the suppression of an item gradually wears off as recall proceeds (Henson, 1998b; Page & Norris, 1998; Vousden & Brown, 1998). Because the wearing off is gradual, items that were suppressed early will not compete again until recall is nearly complete, thus enabling the SEM model to predict the typical pattern of erroneous repetitions (see Henson, 1998b, Figure 4).[3]

A contrasting account, provided by the SOB model (Farrell & Lewandowsky, 2002), assumes that suppression is invariant over time, but incomplete and proportional to an item's original activation. In consequence, the residual activation of suppressed items parallels the primacy gradient at encoding (albeit at a much reduced level). Because suppression preserves the primacy gradient, the residual activation of suppressed early items is comparable to the activation of yet-to-be recalled (and unsuppressed) later items. Hence, early list items will compete for retrieval towards the end of recall, thus also reproducing the empirical pattern of erroneous repetitions, but without postulating that suppression wears off.

We present two experiments that differentiated between those competing accounts by tracing out the time course of response suppression using repetition inhibition as an empirical assay. To control timing, we adopted a response-deadline method (e.g., Gronlund & Ratcliff, 1989; Wickelgren & Corbett, 1977) in conjunction with multiple cued retrievals (e.g., Lewandowsky & Neely,

[3] Although in SEMs simulations the wearing off is discretised over subsequent responses for the sake of simplicity, Henson clearly identifies the process as time-based (see Henson, 1998b, Appendix B). In the Primacy model (Page & Norris, 1998), a mechanism for gradual release has not been explicitly implemented, but the authors state that modelling the pattern of erroneous repetitions would require the amount of response suppression "... to weaken during the course of further recall, ..." (Page & Norris, 1998, pp. 765).

1998). If response suppression wears off over time, repetition inhibition should decrease with increasing delay between recall of the first and second instance of a repeated item. If there is no release from suppression, repetition inhibition should remain constant across inter-response times. To maximise generality, we used RSVP in Experiment 1 and conventional serial recall presentation rates in Experiment 2.

EXPERIMENT 1

Method

Design. Three variables were manipulated within participants using a 2 × 2 × 4 design. There were two types of test (whole report and partial report), two repetition separations (repeated items two and four list positions apart), and four inter-response times (100 ms, 500 ms, 1150 ms, and 1950 ms between successive cues). Lists either contained (repetition lists) or did not contain (control lists) repeated items. However, repetition itself was not a design factor because we combined performance on both types of list into a single measure of repetition inhibition (see below).

Participants. Eight members of the campus community participated in five 1-hr sessions.[4] Participants were remunerated at the rate of A$10 per session.

Materials. Control lists were constructed by randomly sampling six items without replacement from a pool of 19 letters (all consonants except Y). Each list was duplicated to create a yoked repetition list. There were three types of repetition list, with repeated items appearing in positions 2 and 4 or 4 and 6 (separation 2), or 2 and 6 (separation 4).

Each experimental session involved 240 lists whose order was randomised anew for each session. For partial cued report, six lists were constructed for each repetition type and for each inter-retrieval time delay, yielding a total of 144 lists (72 control lists with 72 yoked repetition lists). All partial report trials involved successive presentation of items in positions 1, 3, and 5, which cued retrieval of items in positions 2, 4, and 6, respectively. The delay between cues was determined by the inter-response time variable. For whole report, inter-retrieval time could not be manipulated, and 16 lists were constructed for each repetition type, yielding a total of 96 lists (48 repetition and 48 control). A PC was used to present lists and collect responses. For whole report, responses were entered using the keyboard. For partial report, a voice key was used in conjunction with the keyboard.

Procedure. Each trial started with a 1000 ms blank interval followed by the list. Items were presented individually in the center of the screen for 150 ms with no delay between letters. A visual mask (the "#" symbol) was presented for 150 ms immediately following the last list item. Recall was prompted after a further 300 ms.

For whole report, the underscore ("_") character appeared, and participants were required to recall the list in forward order by typing each letter on the keyboard. As each response was typed, it appeared on the screen and the underscore moved to the right. Participants were required to make six responses and were instructed to guess if necessary.

For partial report, the first list item appeared on the screen and participants had to recall the second item by speaking it out loud within 450 ms. A voice key registered the response and the next cue (the third item) was presented after the appropriate inter-response delay. The second response was followed by the same delay and presentation of the fifth item as the final cue. Following the last vocal response, participants typed in the three letters they had recalled and were informed if any of their vocal responses exceeded the 450 ms response deadline. As with whole report, subjects were instructed to guess if necessary. Accuracy of typed responses was audited by the experimenter who remained present during some sessions. The audit showed that typed responses agreed with spoken recall for all but an insignificant number of responses.

Results and discussion

Dependent measure. As we were primarily interested in repetition effects, we followed relevant precedent (seeArmstrong & Mewhort, 1995; Henson, 1998a) and used a repetition index (d) as the main dependent variable. Briefly, d is

[4] We found an extremely high level of repetition facilitation for one individual who was consequently removed from the analysis. We comment on the reasons for this participant's high facilitation and its relevance to Experiment 2 in the Discussion.

obtained by first calculating the proportion of trials across all sessions on which a participant correctly recalled both repeated items (for repetition lists; P_r) or both corresponding items on the yoked control list (P_c). The *d* score is the difference between these proportions (i.e., $d = P_r - P_c$). Negative values of *d* indicate repetition inhibition whereas positive values indicate repetition facilitation.

As is standard in serial recall, an item had to be recalled in the correct position to be considered correct. Scoring correct by position is known to be a conservative estimate of repetition inhibition (see Henson, 1998a; Mewaldt & Hinrichs, 1973). To equalise guessing opportunities between list types, critical items on the control list were considered correctly recalled even if they swapped positions or were erroneously repeated (see Henson, 1998a, p. 1163). Proportions were log-odds transformed for all analyses (see Henson, 1998a).

Repetition inhibition. Mean *d* scores were computed at each separation for each type of report and were compared to 0 using a *z*-test, taking the grand mean of *d* scores as the proportion entering into computation of the variance (Cox & Snell, 1989; see Henson, 1999, Appendix A for justification of use in this context). For separation 2, partial report showed a significant effect of inhibition, $z(7) = -3.18, p < .001$, but this was not observed in the whole report, $z(7) = -1.21$, $p < .11$. However, for separation 4, both the partial report, $z(7) = -5.17, p < .0001$, and the whole report did show significant inhibition, $z(7) = -2.65, p < .005$. Clearly, conventional repetition inhibition was obtained not only with standard whole report but also with successive partial cues. The latter finding is critical, as it permits a further analysis of the time course of the underlying response suppression.

Erroneous repetitions. The proportion of erroneous repetitions in *control lists* was analysed as a function of both input position (position the item occupied during presentation) and output position (position of the erroneous *second* report of the item during recall). These data are shown in Figure 1.

For partial report, there was no apparent effect of inter-response delays. The log-odds transformed error proportions were thus analysed by one-way within-participants ANOVAs using input position, $F(2, 12) = 1.15$, non-significant, and output position, $F(2, 12) = 17.15, p < .0005, MSE = 0.25$, as independent variables. The whole-report data were analysed using a corresponding set of within-participants ANOVAs. These yielded a significant effect of input position, $F(5, 30) = 3.13$, $p < .05$, $MSE = 0.67$, as well as output position, $F(5, 30) = 36.14, p < .0001, MSE = 0.36$.

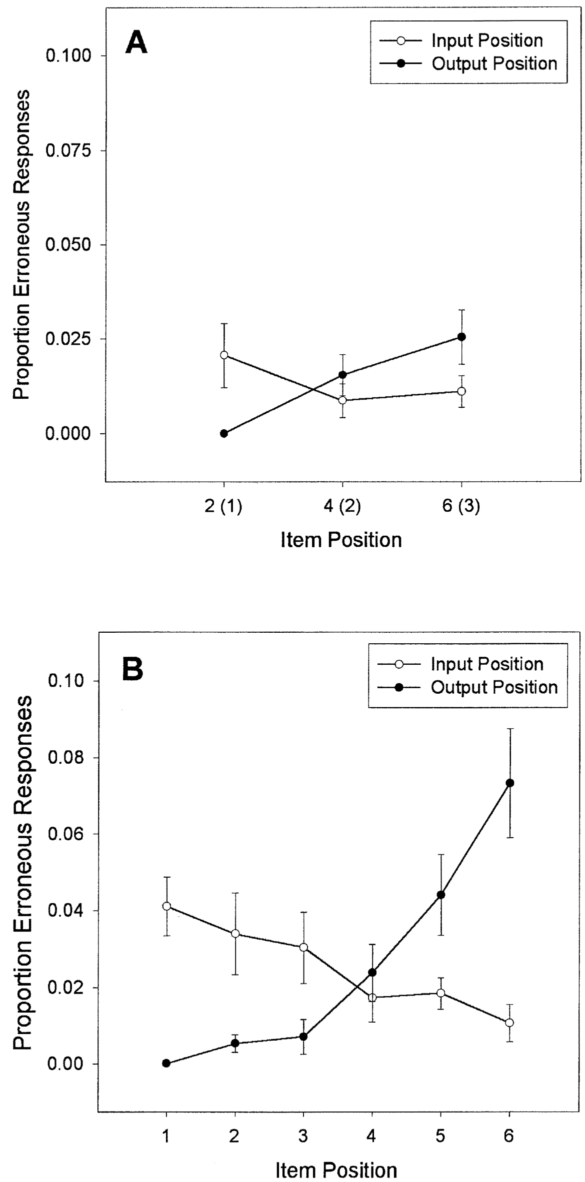

Figure 1. Mean proportion of responses containing erroneous repetitions in recall of control lists as a function of input and output positions for partial report (panel A) and whole report (panel B) in Experiment 1. In panel A, the numbers 2, 4, and 6 on the abscissa refer to input positions (light circles), whereas the numbers 1, 2, and 3 in brackets refer to output positions (dark circles). Error bars show the standard error of proportion of responses.

Overall, the erroneous repetitions conformed to the typical pattern: They increased with output position and decreased across input positions. For partial report, the lack of an effect across input position was possibly due to floor effects ($\leq 1\%$ at positions 4 and 6). However, the cross-over between input and output was still present in the partial report.

Time course of suppression. We first calculated actual inter-response times by adding each participant's response latencies to the experimentally manipulated inter-response deadlines. Inter-response times were then averaged across subjects to obtain a set of four inter-response times at each repetition separation. Figure 2 shows mean *d* score as a function of the inter-response times for both separations (note that inter-response times are greater for separation 4 because an additional delay and subsequent retrieval intervenes between the two tests of the repeated item). The figure shows that repetition inhibition is largely time-invariant for both separations: In particular, there is no clear evidence for a reduction of inhibition even at the longest inter-response times.

To confirm the pattern in the figure, each participant's *d* scores and inter-response times were entered as dependent and independent variable, respectively, into regression analyses for each repetition separation. Each participant's inter-response times were centred on 0 to ensure that tests of the intercept would correspond to the average level of inhibition without affecting slope estimates. As suggested by Figure 2, the slopes were not significant at either separation 2 or 4, $t(26) = 0.09$ and $t(26) = -0.97$, respectively. However, the intercepts at separation 2, $t(26)$ =

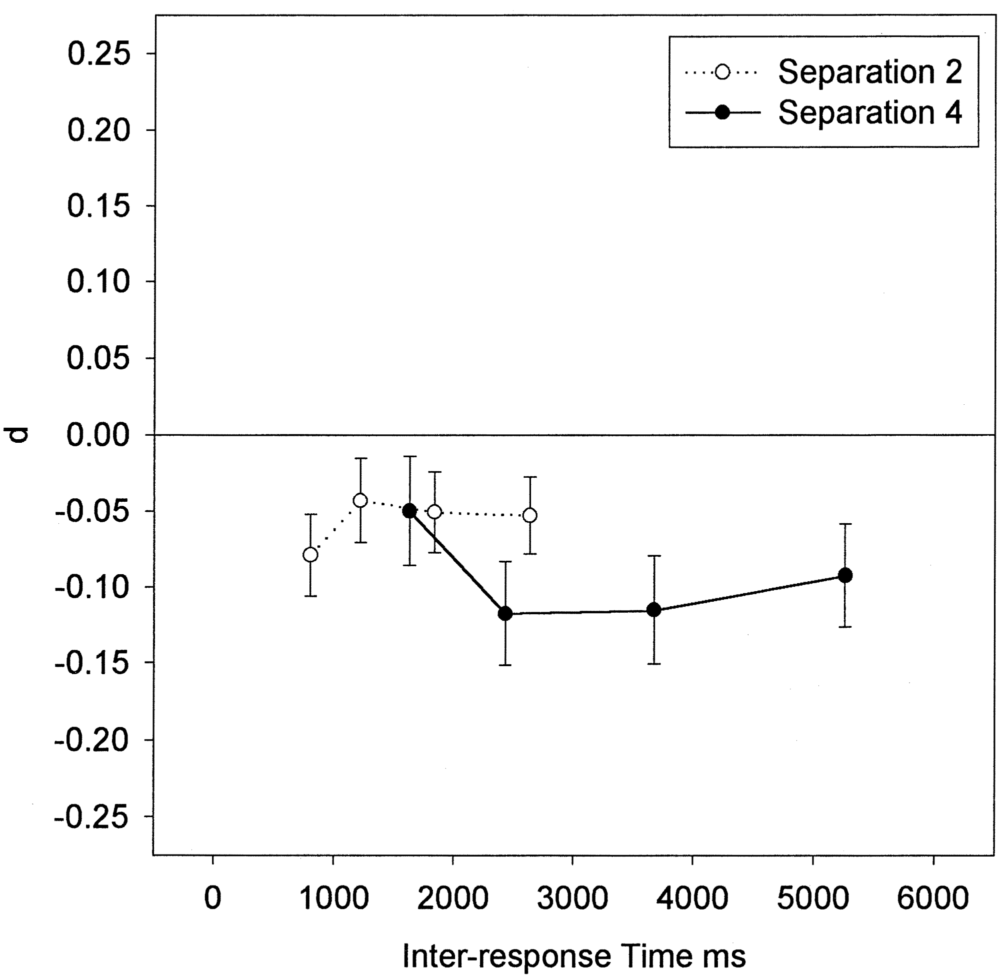

Figure 2. Mean *d* score as a function of inter-response time between recall of each critical item in cued partial report for separation of 2 (light circles) and 4 (dark circles) at input in Experiment 1. Error bars show the standard error of *d*.

-2.52, $p < .02$ and separation 4, $t(26) = -5.02$, $p < .0001$ were significant, in agreement with the earlier z-tests.[5]

The regression analysis confirmed that response suppression did not wear off over time. The observed time-invariance is consistent with SOB but is inconsistent with models that assume absolute suppression followed by gradual wearing off (e.g., SEM). Unfortunately, this conclusion is subject one major qualification: One of the participants, who did not contribute to the present analyses, showed massive repetition facilitation. Further investigation revealed that this participant had become aware that partial report always tested the same three items (in positions 2, 4, and 6). By attending only to these three critical items at study, the functional list length for that participant was three rather than six and hence most repetitions functionally adjacent; a situation known to lead to facilitation (e.g., Henson, 1998a). This problem was corrected in Experiment 2 by using all list positions as potential cues and/or responses during partial report. Experiment 2 also used a slower rate of list presentation to allow further generalisation of the results.

EXPERIMENT 2

Method

Design. This study used the same $2 \times 2 \times 4$ within-participants design as Experiment 1, except that repetition separation was instantiated differently. List length was increased to seven, and separation 2 involved lists with repeated items in positions 2–4, 4–6, 3–5, and 5–7, whereas separation 4 involved repetitions in positions 2–6 and 3–7.

Participants. Seven volunteers were paid A$10/hr for their participation in the three experimental sessions.

[5] This regression analysis confounds between- and within-participant variability, which is often inadvisable (e.g., Lorch & Myers, 1990). The data from both experiments were therefore also analysed using hierarchical regression (Busing, Meijer, & van der Leeden, 1994), which estimates regression parameters for each participant separately and ascertains statistical significance based on their overall pattern. In both experiments, the hierarchical regression analyses supported the same conclusions as the simple regressions: For ease of interpretation, we report only the latter.

Materials. All lists contained seven letters and were constructed from the set H, J, M, Q, R, V, and Z. Thus, unlike Experiment 1, all lists contained the same items, but in different random orders. However, as in Experiment 1, each repeat list had an identical yoked control. For partial report, three lists were constructed for each possible pair of repeated positions and for each inter-retrieval time, yielding a total of 72 lists (36 repetition lists and 36 yoked control lists). For whole report, there were 12 repetition lists for each pair of possible positions, yielding a total of 72 lists (36 repetition and 36 yoked control).

Procedure. Presentation of lists and collection of responses was identical to Experiment 1 except that items were presented for 400 ms with a 100 ms blank inter-item interval. The post-list mask was also omitted.

Typed partial-report responses were audited for one participant. Agreement with verbal responses was again in excess of 95%, with a tendency towards greater accuracy for repetition lists but only at the longest deadlines. The difference between repetition and control lists was statistically negligible (less than half the standard error).

Results and discussion

Repetition inhibition. The data were analysed in the same way as for Experiment 1. The z-tests of the mean d against 0 revealed that the effect of repetition was significant at separation 2 for partial report, $z(7) = -4.28$, $p < .0001$, but not for whole report, $z(7) = -0.91$. For separation 4, both partial report, $z(7) = -5.77$, $p < .0001$, and whole report, $z(7) = -2.78$, $p < .05$, showed significant repetition inhibition. This constitutes a clear replication of Experiment 1 with repetition inhibition occurring in both whole and partial report, and that inhibition decreasing for more closely spaced repetitions.

Erroneous repetitions. Figure 3 shows erroneous repetitions across input and output positions for both types of report. As in Experiment 1, separate one-way ANOVAs were conducted for partial report using output position, $F(2, 12) = 35.11$, $p < .0001$, $MSE = 0.17$, and input position, $F(2, 12) = 1.73$, as independent variables. The analysis for whole report revealed significant effects for both input position, $F(5, 30) = 4.56$, $p <$

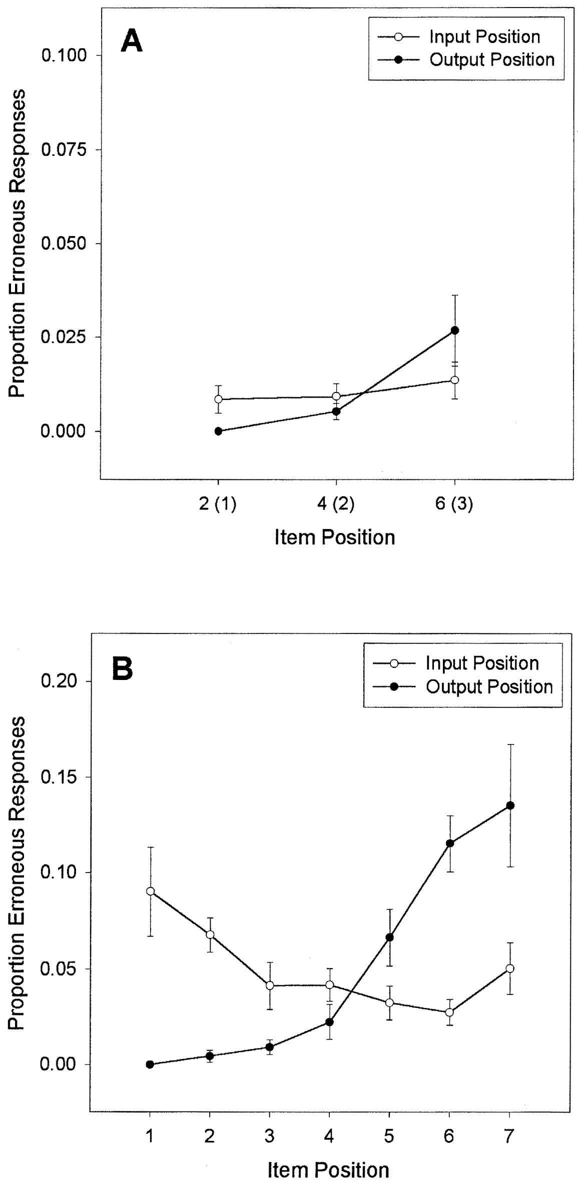

Figure 3. Mean proportion of responses containing erroneous repetitions in recall of control lists as a function of input and output positions for partial report (panel A) and whole report (panel B) in Experiment 2. In panel A, the numbers 2, 4, and 6 on the abscissa refer to input positions (light circles), whereas the numbers 1, 2, and 3 in brackets refer to output positions (dark circles). Error bars show the standard error of proportion of responses.

.005, $MSE = 0.32$, and output position, $F(5,30) = 30.90$, $p < .0001$, $MSE = 0.35$. Overall, the results completely replicated Experiment 1. Although the effect of input position for the partial report was still absent, both whole and partial report show the cross-over of erroneous repetitions between input and output positions typical of serial recall.

Time course of suppression. The extent of repetition inhibition across inter-response times is shown in Figure 4. The data were analysed by regression in the same way as for Experiment 1. As before, d scores exhibited no significant slope for separation 2, $t(26) = 0.06$ or separation 4, $t(26) = -0.21$. By contrast, intercepts for both curves were significant, $t(26) = -4.14$, $p < .0003$ and $t(26) = -5.13$, $p < .0001$, for separation 2 and 4, respectively. As in Experiment 1, the regression analysis was consistent with the earlier z-tests.

As in Experiment 1, there was no evidence of response suppression wearing off with increasing inter-response delays. Importantly, the range of inter-response times varied from about 1 second to over 5 seconds, which is well within the range for whole report of a seven-item list. Hence, if suppression were wearing off during the course of recall, a decrease in repetition inhibition should have been observed in partial-report at least for the very longest inter-response times. However, it is precisely this portion of the curve that is most convincingly flat (see Figures 2 and 4).

In summary, Experiment 2 represents a complete replication of the findings from Experiment 1 after two important methodological changes. In this study, all list positions (bar the first one) were cued to prevent a selective-encoding strategy, and lists were presented at a slower rate that ruled out any encoding contribution to repetition inhibition.

GENERAL DISCUSSION

Potential limitations

We consider two potential limitations of our studies before we examine their theoretical implications. First, our studies inevitably confounded inter-response time and retention interval. That is, at the longest intervals for separation 4, the retention interval for the second repetition was about 5 seconds greater than for the same item at the shortest intervals for separation 2. It is therefore conceivable that the observed time invariance of repetition inhibition represented a trade-off between two opposing processes: A gradual release from suppression (which by itself would have eliminated inhibition), counteracted by selective forgetting of the second repetition of an item (which exactly cancelled out the release from suppression). Although possible, we do not consider this likely: There is no suggestion in the literature that the second instance of a repeated

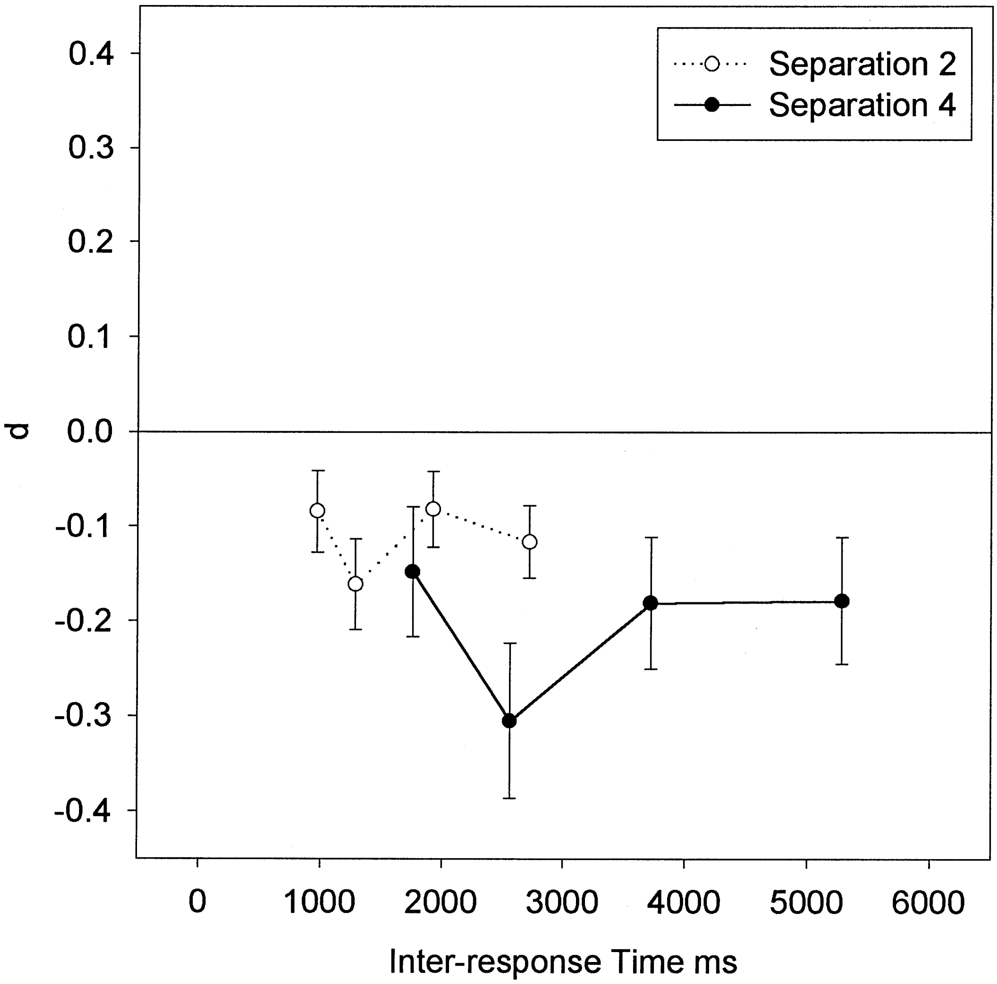

Figure 4. Mean *d* score as a function of inter-response time between recall of each critical item in cued partial report for separation of 2 (light circles) and 4 (dark circles) at input in Experiment 2. Error bars show the standard error of *d*.

item would be forgotten any faster than its non-repeated counterpart on the control list. In fact, in the study by Armstrong and Mewhort (1995), repeat and control items were recalled equally well in response to a single cue across a range of retention intervals, rendering the possibility of a trade-off between forgetting and release from suppression in our experiments highly unlikely.

The second potential limitation of our studies concerns the nature of the test used to trace out the time course of suppression. The use of multiple partial cues is not without precedent (e.g., Lewandowsky & Neely, 1998) but nonetheless rather uncommon. Moreover, in contrast to the Lewandowsky and Neely precedent, responding here was deadline-driven and people had to respond as quickly as possible. This may raise the concern that the observed time invariance of suppression was tied to a peculiar methodology, and may not apply to other, more conventional tests. This concern can be ruled out for two reasons: First, in both experiments, the partial report data replicated all known relevant findings. Inhibition was attenuated when the repetitions were closely spaced during study, and erroneous repetitions tended to increase with output position but decrease with input position (although the latter effect was not statistically significant). Second, and more important, we have replicated the time-invariance of response suppression in an unpublished experiment using paced whole report. In that study, participants were trained to recall the entire list to the beats of a metronome. By varying the timing of the beats, inter-response times were manipulated across a range similar to the one used here. Again, there was no evidence of any release from response suppression. We therefore conclude that the time-invariance observed in the present study was not an isolated phenomenon but represents a more general characteristic of response suppression.

Theoretical implications

Our results pose a challenge to theories that assume a gradual wearing-off of response suppression. This calls into question the implementation of suppression in SEM (Henson, 1998b). It also challenges the similar notion proposed by Page and Norris (1998), that wearing off is required in their primacy model. Instead, our data favour the implementation of suppression in SOB (Farrell & Lewandowsky, 2002) as a partial but time-invariant reduction of an item's availability for further report.

By challenging the notion of gradual release, our data indirectly also call into question models in which suppression is complete and time-invariant (e.g., OSCAR, Brown et al., 2000; TODAM, Lewandowsky & Murdock, 1989). This is because complete suppression is at odds with the observed pattern of erroneous repetitions: A completely suppressed item could not, with increasing likelihood, be repeated later during output. Although complete suppression could be reconciled with the erroneous repetition data by assuming that suppression wears off, this remedial mechanism runs counter to the present results. Hence, any model that postulates complete suppression, regardless of whether it is time-invariant or followed by wearing off, is difficult to reconcile with the sum total of known results.

A tacit assumption of all available theories is that the end of the recall episode also ends suppression. There has been no suggestion in the literature that a recalled item remains suppressed across multiple trials. At first glance, this might be taken to run counter to our data, which suggest that response suppression does not wear off. However, upon closer inspection, Experiment 2 indirectly showed that suppression is confined to the duration of the trial. In that study, items on all successive lists were the same. If suppression carried over between trials, then no repetition inhibition should have been observed because all (or nearly all) to-be-recalled items on trial N would already have been suppressed from having been recalled on trial N–1. The implication, that there is a list-wide release from suppression at the end of a trial, ties in with other list-wide phenomena (e.g., Lewandowsky, 1986).

CONCLUSIONS

Most theories of short-term serial recall postulate that an item is suppressed once it is recalled. This assumption is vital to some theories, such as the primacy model or SOB, which could not recall a list in order without the existence of response suppression. Notwithstanding its theoretical importance, the characteristics of response suppression have remained largely unknown. We have presented two experiments that identified the time-invariance of response suppression. This result favours theories in which suppression is partial and not followed by wearing off.

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