

Item Repetition in Short-Term Memory: Ranschburg Repeated

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In serial recall from short-term memory, repeated items are recalled well when close together (repetition facilitation), but not when far apart (repetition inhibition; the Ranschburg effect). These effects were re-examined with a new scoring scheme that addresses the possibility that repetitions are distinct tokens in memory. Repetition facilitation and repetition inhibition proved robust, and were shown to interact with the temporal grouping of items (Experiment 1), which affected the probability of detecting repetition (Experiments 2A and 2B). It is argued that detection of a repetition is necessary for repetition facilitation, attributable to the tagging of immediate repetition, whereas the failure to detect or remember a repetition results in repetition inhibition, attributable to an automatic suppression of previous responses and a bias against guessing repeated items (Experiment 3). The findings are discussed in relation to models of short-term memory and the phenomenon of repetition blindness.

The presence of a repeated item has dramatic effects on serial recall from short-term memory. When two occurrences of an item are close together in a sequence, recall of both occurrences is generally superior to recall of two different items at corresponding positions in control sequences with no repeated items (*repetition facilitation*; e.g., Crowder, 1968a; Lee, 1976b). However, when the two occurrences are separated by a number of intervening items, recall of one or both occurrences is generally inferior to recall of two different items at corresponding positions in control sequences (*repetition inhibition*, or the *Ranschburg effect*; e.g., Crowder, 1968a; Jahnke, 1969b). Though these effects have been demonstrated in many previous studies, they have been measured, and interpreted, in several different ways. The main purpose of the present study was to attempt a unified measurement and interpretation.

The issue of repeated items in serial recall is important because it raises questions about the representation of items in memory. For example, do two occurrences of a repeated item activate the same *type* representation in memory, or does each occurrence form a separate *token* representation (cf. Wickelgren, 1969)? As well as being an important theoretical issue, this distinction has implications for the measurement of repetition effects. If the two occurrences of

a repeated item are separate tokens in memory, the experimenter, who deals only with categorical responses, has no way of distinguishing them. This presents a problem in the conventional scoring of serial recall; a problem overlooked in previous studies. The present study introduces a new scoring scheme that overcomes this problem.

Another reason for the interest in item repetition is in connection with *repetition blindness*, the failure to detect repetition in rapidly presented sequences (Kanwisher, 1987). Most studies have used serial recall to index repetition blindness and are therefore potentially confounded by repetition inhibition. Indeed, several researchers have suggested that repetition blindness is no more than a memory phenomenon such as repetition inhibition (e.g., Armstrong & Mewhort, 1995; Fagot & Pashler, 1995; Whittlesea & Podrouzek, 1995), though others have argued that repetition blindness is a distinct perceptual phenomenon (Kanwisher, Kim, & Wickens, 1996; Luo & Caramazza, 1995; Park & Kanwisher, 1994). A better understanding of repetition inhibition will therefore help clarify the relationship between the two phenomena.

Repetition facilitation and repetition inhibition are simple to demonstrate in serial recall of supraspan lists, and are robust to experimental manipulations such as presentation rate or presentation modality (Mewaldt & Hinrichs, 1973). Consequently, the effects have been subject to considerable research. To summarize, the separate occurrences of a repeated item are referred to as the *repeated elements* of a *repetition list*. Nonrepeated items at the same positions in *control lists*, containing no repeated items, are the *control elements*. Repeated and control elements are the *critical elements*; the remaining nonrepeated elements in repetition and control lists are the *context elements*.

In a parametric study varying the separation between two critical elements, Crowder (1968a) compared recall on positions of repeated elements with recall on positions of control elements. He demonstrated superior recall of repeated elements one or two positions apart, and inferior recall of repeated elements three or more positions apart.

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Wickelgren (1965) showed a similar transition between repetition facilitation and repetition inhibition using item scoring rather than position scoring (i.e., whether a critical element was recalled anywhere in a participant's report). Repetition inhibition in particular stemmed from a failure to recall the repeated item more than once. The importance of recalling both critical elements was reinforced by Lee (1976b), who showed that, when estimating the probability that at least one of the critical elements was recalled somewhere, there was no repetition facilitation or inhibition for any separation of repeated elements. Only when estimating the probability that both critical elements were recalled somewhere did the effects arise.¹

Given these robust effects of item repetition in serial recall, it is somewhat surprising that their theoretical interpretation remains unclear (Jahnke & Bower, 1986). Repetition facilitation has been variously attributed to distinctiveness (Lee, 1976a), chunking (Wickelgren, 1965), and tagging (Lee, 1976b). Repetition inhibition has been attributed to output interference (Crowder, 1968b; Jahnke, 1969a), proactive interference (Jahnke, 1972b), and guessing strategies (Hinrichs, Mewaldt, & Redding, 1973). Moreover, all of these studies have overlooked the type-token distinction, potentially compromising their conclusions.

The aim of Experiment 1 was to determine whether repetition facilitation and repetition inhibition remain significant effects under a new method of scoring that addresses the type-token distinction. Experiment 1 also examined the role of temporal grouping, a common rehearsal strategy in such experiments. Grouping had striking effects on the pattern of facilitation and inhibition. Whereas Experiment 1 used serial recall, Experiments 2A and 2B used a direct test of people's ability to detect and remember a repetition, with particular interest in how grouping affected this ability. Finally, Experiment 3 examined the role of guessing in repetition inhibition, a role given much attention in the literature (Greene, 1991; Hinrichs et al., 1973; Walsh & Schwartz, 1977). The results of these experiments are combined with those of previous studies to produce a unified theory of item repetition effects in short-term memory.

Experiment 1

In Experiment 1, repetition facilitation and repetition inhibition were measured under a new method of scoring control lists, which overcomes a potential bias in favor of repetition lists. This bias arises because, unlike a transposition between two control elements, a transposition between two repeated elements cannot be detected by the experimenter. For example, consider recall of a control list represented by the sequence 123456, and a repetition list represented by the sequence 12 R_1 R_2 56, where R_1 and R_2 are the two repeated elements. A participant's report of the control list as 124356 would be marked as containing incorrect positioning of the control elements 3 and 4. However, a participant's report of the repetition list as 12 R_2 R_1 56 would be marked as containing correct positioning of the repeated elements, because the experimenter has no way of distinguishing R_1 and R_2 . Such a discrepancy in

scoring the two types of lists might lead to an overestimation of repetition facilitation and an underestimation of repetition inhibition. Indeed, given that the transposition between adjacent items in the above example is a common error in serial recall (e.g., Estes, 1972), any repetition facilitation found in such cases may be no more than a scoring bias against control elements.

One way to overcome this bias is to use an item-scoring criterion, where an element is scored correct irrespective of its recall position. Unfortunately, this does not address people's ability to recall a sequence in the correct order, for which a position-scoring criterion is required. An alternative approach is not to score a transposition between two control elements as incorrect (Wickelgren, 1965). This is a more suitable approach. However, the most general way to control for the bias is to count either control element appearing in a critical position as correct. Thus, when contrasted with the four possible reports of repetition list 12 R_1 45 R_2 in the Appendix all four reports of the corresponding control list 123456 are scored correct under this *modified control scoring*. There are several points worth noting about these reports:

1. Under conventional position scoring, such as serial position curves (e.g., Crowder, 1968a), the second, third and fourth reports of the control list will lead to errors on at least one of the Positions 3 and 6, underestimating performance on control elements relative to repeated elements.

2. Even under conventional item scoring, the third and fourth reports of the control list will reduce Lee's (1976b) probability of recalling both control elements somewhere, relative to recalling both repeated elements somewhere.

3. Though repetitions of a response in the third and fourth reports of the control list might seem rare, Henson, Norris, Page, and Baddeley (1996) showed that such repetitions can comprise around 16% of order errors, even when no list contained a repeated item. When participants are aware that lists can contain repeated items, as in the present study, a greater percentage is likely to result. A higher incidence of repetitions is also likely when lists are grouped (Henson, 1996).

In addition to the modified scoring of control lists, the present study introduces a single index of repetition facilitation and repetition inhibition. These two effects have been measured in several different ways in previous studies, often because they have been studied separately from one another. The new index, *delta*, provides a unified measure of both repetition facilitation and repetition inhibition. Specifically, *delta* is the difference between the probability of recalling two repeated elements and the probability of recalling two control elements. A positive value of *delta* implies facilitation; a negative value implies inhibition. This measure requires recall of more than one repeated element (Lee, 1976b), though it does not distinguish which repeated element benefits or suffers in recall. Again, this reflects the fact that the experimenter cannot be certain that the first

¹ The presence of repeated elements can also affect recall of surrounding context elements. Such *repetition contamination* is discussed elsewhere (Henson, 1996).

Table 1
Composition of List Types in Experiments 1–3

List type	Repetition format	Repetition separation	No. of lists (Exp. 1)	No. of lists (Exps. 2A & 2B)	No. of lists (Exp. 3)
Control	123456	—	48	40	60
Repetition	1RR456	1	12	—	—
Repetition	12RR56	1	12	20	20
Repetition	1R3R56	2	12	—	—
Repetition	12R4R6	2	12	20	20
Repetition	1R34R6	3	12	—	—
Repetition	12R45R	3	12	20	20
Repetition	R234R6	4	12	—	—
Repetition	1R345R	4	12	20	—

Note. Exp. = Experiment. Dashes indicate that the particular repetition format was not used in that experiment.

repeated element recalled is the first repeated element presented (though previous studies suggest it is mainly the second repeated element to be recalled that is affected, e.g., Crowder, 1968a; Jahnke, 1969b; Wickelgren, 1965).

If repetition facilitation and repetition inhibition reflect more than a possible scoring bias, then previous research suggests that both should be demonstrable simply by varying the separation between repeated elements. Plotting delta against repetition separation should therefore give some form of facilitation–inhibition continuum, with positive delta at short separations and negative delta at longer ones. In the present study, repetition separation was defined over several different *repetition formats*, depending on the exact positions of repeated elements. This was to examine the additional role of serial position effects.

The second aim of Experiment 1 was to examine how this continuum changes with the introduction of temporal grouping (e.g., the insertion of a pause between every third item). Strong interactions might be expected between grouping and repetition effects. For example, will repetition facilitation remain for adjacent repeated elements that straddle a group boundary? Or will repetition inhibition remain for more widely separated repeated elements that occur at the same position within groups? Furthermore, people will often spontaneously group sequences subjectively (imposing their own rhythmic and intonational cues; Henson, 1996). It is possible that the choice of grouping strategy will be influenced by the presence of repeated elements in a sequence. This possibility was noted in passing by Walsh and Schwartz (1977), who warned of “a possible source of interference which would apply to the experimental but not to the control sequences, i.e. a conflict between the subjective grouping imposed by the subject and the objective grouping imposed by the intrasequence repetition” (p. 68). The presence of objective grouping of lists should override any subjective grouping strategies triggered by particular repetition formats, and hence eliminate the interference suggested by Walsh and Schwartz.

In summary, Experiment 1 measured both repetition facilitation and repetition inhibition within the same design, by manipulating the position of repeated elements (within

subjects) and the presence or absence of temporal grouping in threes (between subjects).

Method

Participants. Twenty-four participants from the Applied Psychology Unit Subject Panel were assigned randomly to two grouping conditions. There were 10 women and 2 men in the ungrouped condition, with a mean age of 29 years, and 8 women and 4 men in the grouped condition, with a mean age of 28 years.

Materials. The consonants J, H, R, Q, V, and M were used to generate 144 lists of six items (the use of such a small vocabulary being to maximize repetition effects; Jahnke, 1972b, 1974). Two thirds of the lists (the repetition lists) contained one repeated item; the rest (the control lists) contained all six items. The repetition lists were divided into eight repetition formats, according to the positions of the repeated elements (see Table 1). The repeated elements were between one and four positions apart. Over all lists, each item was repeated equally often and occurred approximately equally often at each position. Control and repetition lists were distributed equally over six blocks of 24 trials, and the order of trials was pseudorandomized, with the constraint that two consecutive trials never contained the same repetition format nor repetition of the same item.

Procedure. Each consonant was presented in the center of a computer monitor, replacing the previous one, at a rate of two every second (400 ms on, 100 ms off). The grouped condition included an additional 500-ms pause after every third consonant. The sixth consonant was followed by a further sequence of three distractor digits (drawn randomly without replacement from the set 1–9), presented at the same rate as the consonants. These distractors were to titrate performance levels such that approximately 70% of responses were correct. Participants were instructed to vocalize each consonant and digit as it appeared, but to recall only the consonants. Vocalization of the consonants was monitored by the experimenter to ensure that participants perceived the items correctly, particularly the repeated items. (In fact, errors in vocalization were extremely rare.) Though vocalization introduced potentially complicating effects of echoic, auditory information, the vocalization of the distractor digits was to minimize such effects, each digit acting much like an auditory suffix.

Immediately after the last digit, a visual cue appeared to prompt spoken, forward recall of the consonants, which were written down by the experimenter. Participants were encouraged to guess if they

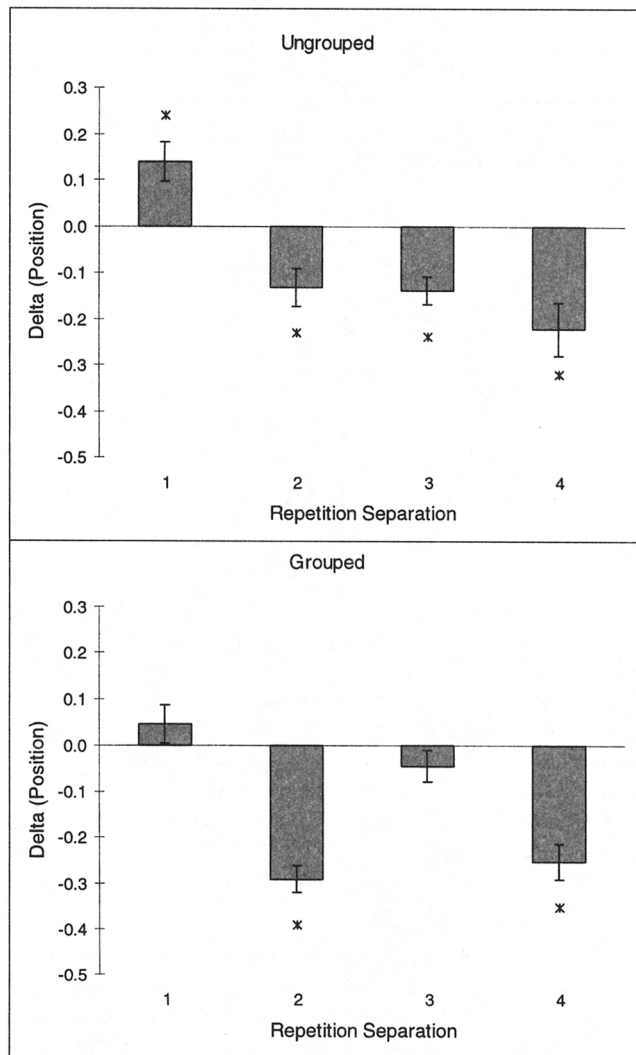


Figure 1. Delta under position scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 1. Error bars show standard error of delta values; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

were unsure, or to say "blank" if no consonant came to mind. They were alerted to the fact that some lists contained repeated consonants. After 10 practice trials, each participant attempted all six blocks, with the order of blocks counterbalanced across subjects. The whole experiment took 50 min.

Hypothesis testing. Statistical tests in the present study were made on empirical log-odds, which were weighted for the purposes of pairwise comparisons (Cox & Snell, 1989). This transform makes some allowance for floor and ceiling effects in the calculation of proportions (see appendixes of Henson, 1996, 1997). When multiple, pairwise comparisons were made, Holm's method for testing significance was used (Howell, 1992), with significance defined for familywise, two-tailed p values below .05.

Results

In brief, significant repetition facilitation and repetition inhibition were found under modified control scoring. In

ungrouped lists, repetition facilitation was found for adjacent repeated elements, and repetition inhibition was found as soon as one or more context elements intervened. In grouped lists, no repetition facilitation was found for adjacent repeated elements that straddled a group boundary, and no repetition inhibition was found for three-apart repeated elements that occurred at the end of groups. Repetition facilitation and repetition inhibition were also dissociable by different scoring criteria: Repetition facilitation reflected superior positioning of repeated elements, whereas repetition inhibition reflected inferior recall of repeated elements anywhere.

Position scoring of serial recall. The general pattern of facilitation and inhibition can be seen by calculating delta, the mean difference between the probability of correctly positioning the two repeated elements and the probability of correctly positioning the two corresponding control elements (under the modified control scoring discussed above).² Collapsing across repetition formats with the same repetition separation, delta values are shown in Figure 1. For ungrouped lists, delta was positive for adjacent repetition (a repetition separation of one), but negative for all greater separations. For grouped lists, delta was negative for repetition separations of two and four, but close to zero for separations of one and three. Indeed, tests of weighted log-odds showed that delta was significantly different from zero in all cases, $Z(12) > 3.21$, $p < .006$, except for one-apart and three-apart repeated elements in grouped lists, $Z(12) < 1.17$, $p > .24$.

The pattern of delta values was confirmed by a three-way, repeated-measures analysis of variance (ANOVA) on the log-odds of recalling critical elements on both critical positions (expressed as probabilities in Table 2). There was a significant effect of list type, $F(1, 11) = 26.24$, $p < .001$, and repetition format, $F(7, 77) = 25.51$, $p < .001$. There was no significant effect of grouping, $F(1, 11) < 1$, but grouping did interact with repetition format, $F(7, 77) = 5.54$, $p < .001$. The interaction between list type and repetition format was also significant, $F(7, 77) = 17.39$, $p < .001$, as was the three-way interaction between list type, repetition format, and grouping, $F(7, 77) = 4.13$, $p < .005$.

However, closer inspection of Table 2 revealed important differences between some repetition formats. In repetition format *1RR456*, for example, repeated elements were correct more often than control elements in both ungrouped and grouped lists. In repetition format *12RR56*, however, repeated elements were correct more often than control elements in ungrouped lists, but less often in grouped lists. Indeed, tests of weighted, delta values showed that grouping had no significant effect on delta for repetition format

² The corresponding probabilities for control elements were determined by pairing a control list with each repetition list. Given the smaller number of control lists than repetition lists, this meant each control list was paired with two repetition lists. To minimize any dependencies between the two measurements taken from each control list, the control elements in question were always from different positions. This caveat does not apply to Experiment 3, where equal numbers of control and repetition lists meant that they could be randomly paired with one another.

Table 2
*Probabilities of Correct Recall of Critical Elements Under Position Scoring
 in Experiment 1*

Condition	Repetition format							
	1RR456	12RR56	1R3R56	12R4R6	1R34R6	12R45R	R234R6	1R345R
Ungrouped								
Repeated								
<i>M</i>	.79	.78	.53	.48	.42	.65	.48	.43
<i>SD</i>	.19	.16	.26	.22	.15	.32	.22	.25
Control								
<i>M</i>	.65	.65	.64	.64	.63	.72	.66	.69
<i>SD</i>	.22	.21	.18	.20	.23	.20	.23	.24
Grouped								
Repeated								
<i>M</i>	.92	.69	.37	.37	.51	.87	.36	.47
<i>SD</i>	.08	.19	.27	.23	.15	.18	.22	.23
Control								
<i>M</i>	.74	.79	.67	.65	.67	.80	.69	.65
<i>SD</i>	.22	.17	.16	.19	.16	.20	.17	.22

1RR456, $Z(24) = 1.21$, $p = .23$, but significantly decreased delta for repetition format 12RR56, $Z(24) = 3.03$, $p < .005$. Thus, the failure to find significant repetition facilitation for one-apart repeated elements in the grouped condition of Figure 1 came from a reduction in delta when repeated elements straddled a group boundary (i.e., repetition format 12RR56): Elements repeated within a group (i.e., repetition format 1RR456) showed no such reduction.

In contrast, grouping increased delta for repetition format 12R45R, such that delta changed sign, though the change did not quite reach significance, $Z(24) = 1.88$, $p = .06$. No such trend was found for repetition format 1R34R6, $Z(24) = 0.94$, $p > .35$. Thus, the failure to find significant repetition inhibition for three-apart repeated elements in the grouped condition of Figure 1 came mainly from an increase in delta for repeated elements at the end of groups (i.e., repetition format 12R45R): Elements repeated in the middle of groups (i.e., repetition format 1R34R6) showed little change.

Other scoring of serial recall. Repetition facilitation and repetition inhibition were investigated further by using an item-scoring criterion (i.e., how often critical elements were recalled in any two positions in a report). Corresponding delta values are shown in Figure 2. Surprisingly, only repetition inhibition was evident under item scoring; there was no significant repetition facilitation. In other words, recall of two repeated elements somewhere was always less likely than recall of two control elements somewhere. Indeed, separate tests of each repetition format showed that delta was significantly below zero for all formats, $Z(12) > 2.58$, $p < .01$, except repetition format 1RR456, both grouped and ungrouped, repetition format 12RR56 when ungrouped, and repetition format 12R45R when grouped, $Z(12) < 2.16$, familywise $p > .05$. Thus, repetition inhibition was evident under all conditions except for adjacent repeated elements that did not straddle a group boundary and three-apart repeated elements at the end of groups.

If repetition facilitation did not arise through better recall of repeated elements anywhere, it must have arisen through better positioning of those elements. This was confirmed by analyzing the conditional probability of recalling critical

elements in the two critical positions, given that critical elements were recalled in two positions somewhere. In this case, delta was significantly above zero for repetition format 1RR456, both grouped and ungrouped, and repetition format 12RR56 when ungrouped, $Z(12) > 3.42$, $p < .001$, but not significantly different from zero for any other repetition format, $Z(12) < 2.70$, familywise $p > .05$. Thus, provided a repeated item was recalled twice, there was no extra difficulty in positioning its two occurrences.

Discussion

Using a single index of performance and a position-scoring criterion, the present experiment showed both repetition facilitation and repetition inhibition within the same design, simply by varying the separation of repeated elements. Indeed, there was a transition from repetition facilitation to repetition inhibition in the ungrouped condition, as soon as one or more context elements intervened between the repeated elements. Furthermore, temporal grouping removed repetition facilitation for adjacent repeated elements that straddled a group boundary, and appeared to prevent repetition inhibition for repeated elements at the ends of groups. This striking interaction between repetition and grouping is an important new finding.³

³ Given that delta is a difference score, one might attribute the pattern of delta values to variability in overall performance levels. Ceiling effects, for example, may account for the failure to find significant repetition inhibition when repeated elements occur at the end of groups. However, the probabilities in Table 2 are rarely greater than .80, and performance on control elements is quite constant, around .65 when ungrouped and .70 when grouped. Even when the data are reanalyzed using a relative measure of performance (Jahnke, 1969b), where delta is normalized by the probability of recalling control elements, the pattern of results remains unchanged. Thus, though the magnitude of delta is undoubtedly sensitive to overall performance, this is not sufficient to explain the effects of repetition separation and grouping.

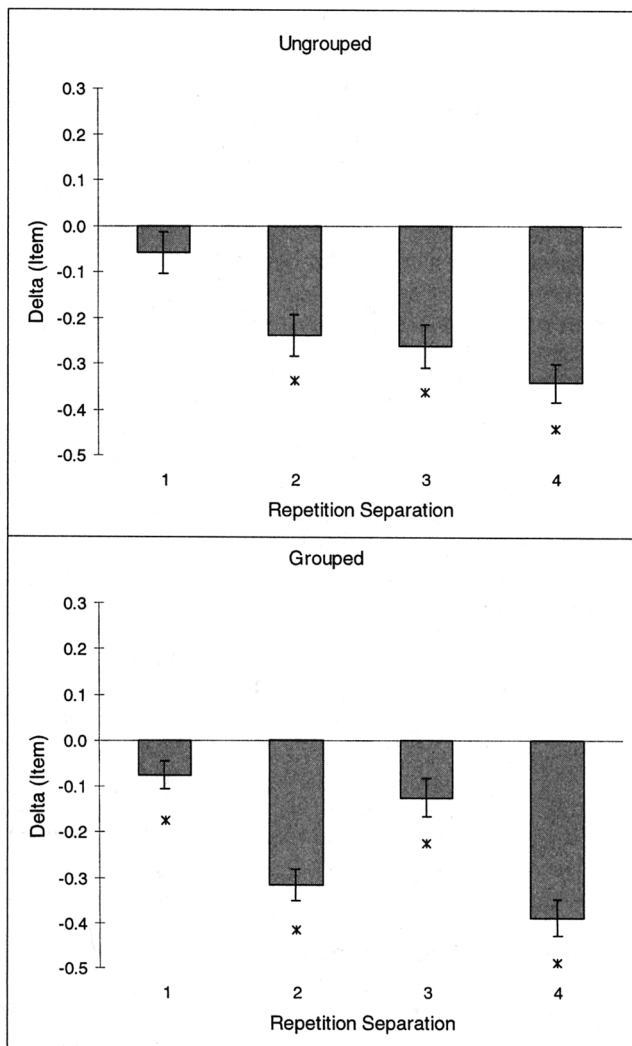


Figure 2. Delta under item scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 1. Error bars show standard error of delta values; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

The new method of scoring control lists confirmed that these effects arise over and above any potential bias in favor of repetition lists, owing to the inability of the experimenter to tell which response represents which repeated element. The results under this modified control scoring were in broad agreement with previous studies, except that significant repetition inhibition was found for two-apart repeated elements, where none has been found before (e.g., Crowder, 1968a; Lee, 1976b). This probably reflects the greater sensitivity of the delta index used in the present study. To make a more precise comparison, the data were also analyzed by calculating the probability of recalling each critical element in its correct position under conventional scoring, as if comparing positions of repeated and control elements on serial position curves (e.g., Crowder, 1968a).

The resulting differences are shown in Table 3, together with the values of delta reproduced from Figure 1.

The magnitude of repetition inhibition for nonadjacent repeated elements was considerably greater according to the delta measure than the conventional measure, with an absolute increase of 8% on average (a proportional increase of approximately 50%). Indeed, the magnitude of repetition inhibition for two-apart repeated elements in ungrouped lists increased from a nonsignificant value of 1% under the conventional measure to the significant value of 12% under the delta measure, an increase that was itself significant, $Z(12) = 2.07$, $p < .05$. The increases were not as great for three- and four-apart repeated elements because transpositions between the corresponding control elements (which are correct under modified, but not conventional, control scoring) are less likely the further apart the control elements (Estes, 1972). The magnitude of repetition facilitation for adjacent repeated elements did not differ much from that under the conventional measure, because the decrease in repetition facilitation owing to better performance on adjacent control elements under modified control scoring was counteracted by the increase in repetition facilitation owing to the requirement to recall both critical elements (see Lee, 1976b). Thus the delta measure is more sensitive than the conventional scoring of serial position curves, at least for repetition inhibition. The effect of this greater sensitivity is to transform what has previously been regarded as a smooth transition from facilitation to inhibition as repetition separation increases to one with a discrete transition from facilitation to inhibition as soon as repeated elements are no longer adjacent.

Further investigation of repetition facilitation and repetition inhibition was made possible by examining delta values under item scoring. In this case, recall of repeated elements nearly always suffered compared to control elements, no matter how far apart the repeated elements. This most probably reflected a failure to recall a repeated item more than once (Jahnke, 1969b; Lee, 1976b). By conditionalizing the positioning of critical elements on their recall somewhere, this bias was removed and repetition facilitation was shown to arise from better positioning of adjacent repeated elements, in agreement with Drewnowski (1980). The

Table 3
Difference Between Recall of Repeated and Control Elements Under Modified Versus Conventional Position Scoring in Experiment 1

Scoring scheme	Repetition separation			
	1	2	3	4
Ungrouped				
Modified	0.12***	-0.12***	-0.11***	-0.17***
Conventional	0.11***	-0.01	-0.07**	-0.10***
Difference	0.01	-0.10*	-0.04	-0.07
Grouped				
Modified	0.06	-0.23***	-0.03	-0.21***
Conventional	0.06	-0.12***	0.02	-0.13***
Difference	0.00	-0.11**	-0.06	-0.08

* $p < .05$. ** $p < .01$. *** $p < .001$.

dissociation of repetition facilitation and repetition inhibition by different scoring criteria suggests that (at least) two different factors contribute to the effects: one that reduces the probability of recalling a repeated item more than once, and another that increases the probability of positioning the repeated item, provided it is recalled more than once.

In summary, the present experiment replicated both the repetition facilitation and the repetition inhibition reported in previous studies, within a single design and under a new, unbiased scoring of control elements. Moreover, these effects were sensitive to the grouping of lists, a factor overlooked in previous studies (Walsh & Schwartz, 1977). Given that grouping is such a prevalent and powerful effect in serial recall (Henson, 1996), this sensitivity has important implications for both the measurement and interpretation of repetition effects. In particular, the role of grouping in the detection of repetition was examined in Experiment 2A.

Experiment 2A

The previous experiment demonstrated an abrupt transition from repetition facilitation to repetition inhibition in ungrouped lists as the separation between repeated elements increased. How does varying repetition separation have such dramatic effects on the probability of recalling repeated elements? One factor that covaries with repetition separation may be the probability of detecting the repetition event (i.e., the fact that an item was repeated). Lee (1976b), for example, showed that the confidence with which participants reported repetition in a list decreased as the separation between repeated elements increased. Jahnke (1972a) showed a similar effect in a recognition task: Recognition for a pair of repeated elements was worse when they were far apart in a list than when they were close together. In a striking demonstration of people's general failure to detect repetition, Malmi and Jahnke (1972) reported that even when 100% of lists contained repeated elements, participants only guessed 40% had repeated items on debriefing. Thus, one important contribution to the repetition effects in Experiment 1 may be the probability of detecting repetition: Detection may be necessary for repetition facilitation, whereas failure to detect a repetition may result in repetition inhibition. A reduced probability of detecting repetition may explain why repetition facilitation was not found across a group boundary, whereas an increased probability of detecting repetition may explain why repetition inhibition was not found at the end of groups.

Why should people fail to detect that an item was repeated? It is not simply because participants do not expect repetitions: Repetition inhibition arises even when they are told in advance to expect repetition (Experiment 1), are reminded that repetition in lists is possible (Jahnke, 1969b), experience a high frequency of repetition (Hinrichs et al., 1973; Wickelgren, 1965), or have considerable practice in recalling lists with repeated items (Crowder, 1968b). A failure of detection is not necessarily related to repetition blindness either, which is unlikely with the slow presentation rates of the above studies (Kanwisher, 1987). Moreover, repetition blindness is often viewed as a perceptual problem

and yet repetition inhibition arose in Experiment 1 when participants correctly vocalized items as they were presented (note that one can read aloud an item twice, without explicitly registering the repetition of that item).

Even if detection that an item was repeated is necessary for its correct recall, questions remain as to how detection improves its recall (repetition facilitation), or why a failure of detection impairs its recall (repetition inhibition). The latter question is dealt with in Experiment 3. As for the former question, several roles for repetition detection have been suggested. One possibility is that the distinctiveness of a repetition leads to increased attention to, or rehearsal of, the repeated elements. A similar reason is often given for Restorff isolation effects (e.g., Potts & Shiffrin, 1970). However, this type of explanation is inconsistent with Lee's (1976b) finding that the probability of recalling at least one repeated element is no greater than recalling at least one control element: If increased attention were given to repeated elements, then the probability of recalling at least one repeated element should exceed that of recalling at least one control element.

Alternatively, repeated elements may be recoded into a single unit or chunk (e.g., *double-five*), reducing the overall memory load (Wickelgren, 1965). However, such chunking, at least as defined by Johnson (1972), implies all-or-none recall of repeated elements. This is again inconsistent with Lee's (1976b) findings. In fact, Lee (1976b) proposed that detection of a repetition results in a repetition tag associated with the repeated item. Memory for this tag is independent of memory for the item that was repeated (e.g., separate but associated representations of *five* and *doubled*). Tagging produces repetition facilitation only through increasing the probability that both occurrences of a repeated item are recalled.

With these ideas in mind, the aim of Experiment 2A was to make explicit tests of people's ability to detect and remember (tag) the repetition of an item as a function of repetition format and grouping. Grouping in particular allowed a novel means of testing repetition detection independently of repetition separation. The procedure was similar to that of Experiment 1, the only difference being whether the task was to remember all the elements (Experiment 1), or just the repeated elements (Experiment 2A). Given the results of Experiment 1, two specific hypotheses were that grouping (a) improves repetition detection in repetition format *12R45R*, where repeated elements occur at the end of groups, and (b) impairs repetition detection in repetition format *12RR56*, where repeated elements straddle a group boundary.

Method

Participants. Twelve participants from the Applied Psychology Unit Subject Panel were tested, 10 women and 2 men (mean age = 33 years).

Materials. The same set of consonants was used to generate 120 lists of six items as in Experiment 1, except that only four repetition formats were used (Table 2). Control and repetition lists were distributed equally over four blocks of 30 trials and the order of trials was pseudorandomized as before.

Table 4
*Probabilities of Correct Repetition Detection Under
 Position Scoring and Item Scoring in Experiment 2A*

Condition	Repetition format			
	12RR56	12R4R6	12R45R	1R345R
Position scoring				
Ungrouped				
<i>M</i>	.71	.52	.54	.33
<i>SD</i>	.29	.21	.33	.27
Grouped				
<i>M</i>	.84	.71	.98	.66
<i>SD</i>	.21	.24	.05	.24
Item scoring				
Ungrouped				
<i>M</i>	.81	.83	.91	.88
<i>SD</i>	.21	.18	.09	.13
Grouped				
<i>M</i>	.89	.93	.99	.87
<i>SD</i>	.17	.12	.03	.11

Procedure. The same presentation of six consonants followed by three digits was used as in Experiment 1. Participants were given a response sheet with a row of six boxes for each trial, in which to write any repeated consonants (ignoring others). So if a participant thought that *Q* appeared as the second and fourth consonant in a sequence for example, they were to write *Q* in the second and fourth boxes (from left to right). If they did not remember any repeated consonants, they were to strike a line through all six boxes.

Unlike in Experiment 1, grouping was manipulated within subjects. Each participant attempted all four blocks, the order of which was counterbalanced across subjects. The order of grouping conditions was constrained such that the two ungrouped conditions were always attempted before the two grouped conditions. This was to reduce the risk of subjective grouping of the objectively ungrouped lists (Henson, 1996). The whole experiment took approximately 40 min.

Results

In general, participants were extremely good at detecting a repetition event, with a repetition reported for over 90% of repetition lists and a false-alarm rate in control lists of less than 2%. Participants were less accurate at remembering which particular item was repeated, and less accurate still at positioning the repeated elements.

Position scoring of repetition detection. The probability of placing the correct repeated item in both critical positions is shown in Table 4. Correct positioning generally decreased as repetition separation increased, though this trend was modified by grouping, which improved positioning of repeated elements, particularly at the end of groups. This was confirmed by a two-way, repeated-measures ANOVA on log-odds, which showed significant effects of repetition format, $F(3, 33) = 21.07, p < .001$; grouping, $F(1, 11) = 64.80, p < .001$; and their interaction, $F(3, 33) = 3.35, p < .05$.

In relation to the first specific hypothesis, repetition detection in repetition format 12R45R was significantly improved by grouping, $Z(12) = 7.02, p < .0001$. Contrary to the second hypothesis, however, repetition detection in

repetition format 12RR56 was not impaired by grouping. In fact, it was significantly improved, $Z(12) = 2.25, p < .05$.

Item scoring of repetition detection. The probability of remembering the correct repeated item, regardless of its positioning, is also shown in Table 4. Unlike position scoring, repetition detection under item scoring appeared little affected by repetition separation. A two-way ANOVA on log-odds showed a significant effect of grouping, $F(1, 11) = 13.18, p < .005$, but any effect of repetition format, $F(3, 33) = 2.46, p = .08$, or interaction, $F(3, 33) = 2.37, p = .09$, failed to reach significance. The effect of grouping was a general improvement in repetition detection, significantly so for repetition format 12R45R, $Z(12) = 2.23, p < .05$, and almost significantly so for repetition format 12RR56, $Z(12) = 1.74, p = .08$, again confirming the first hypothesis from Experiment 1, but not the second.

Discussion

Though there were some parallels between people's ability to position repeated elements and the delta values under position scoring in Experiment 1, other aspects of the present results were difficult to reconcile with Experiment 1. In particular, people's ability to identify the repeated item showed little effect of repetition separation, unlike the delta values under item scoring in Experiment 1, and grouping failed to impair detection of repeated elements that straddled a group boundary, contrary to what was expected from Experiment 1.

However, one problem with comparing the results of the repetition detection task of Experiment 2A with the serial recall task of Experiment 1 is the greater memory load involved in the serial recall task. The concurrent demands to remember all the elements, including the context elements, may have produced lower levels of repetition detection in Experiment 1 than measured in Experiment 2A. Further discussion is therefore postponed until after Experiment 2B, in which this problem was addressed.

Experiment 2B

In Experiment 2B, tests of repetition detection and serial recall were combined within the same trial, producing a memory load comparable with that in Experiment 1. Furthermore, the trial-by-trial correlation of repetition detection and recall allowed a more direct test of the role of repetition detection in serial recall.

Method

Participants. Twelve participants from the Applied Psychology Unit Subject Panel were tested, 7 women and 5 men (mean age = 34 years).

Materials. The same materials were used as in Experiment 2A.

Procedure. Lists were presented in the same manner as in Experiment 2A, after which participants reported aloud any repeated consonants before recalling all the consonants in order, writing each consonant in a row of boxes on a response sheet. Participants were told only to report a repeated consonant if they were sure it was repeated (or else to say aloud "none") and only to

write a consonant in a box if they were sure it occurred in that position. In view of the greater difficulty in combining the two tasks, the number of distractor digits was reduced from three to one (thus, though the memory load was higher than in Experiment 2A, the retention interval was shorter). The remaining procedure was identical to that in Experiment 2A.

Results

As in Experiment 2A, participants were extremely good at detecting a repetition event, with a repetition reported for over 90% of repetition lists and a false-alarm rate in control lists of less than 1%. There was a high correlation between detection and recall of repeated items, particularly in that correct recall in the absence of detection was rare.

Repetition detection. The probability of reporting repetition of the correct item is shown in Table 5. As in Experiment 2A, repetition detection was good for all repetition formats, though there was now a small decrease in performance with repetition separation. A repeated-measures ANOVA on log-odds showed a significant effect of grouping, $F(1, 11) = 12.37, p < .01$, and repetition format, $F(3, 33) = 5.54, p < .005$, and an interaction that approached significance, $F(3, 33) = 2.59, p = .07$. The effect of grouping was a significant improvement in repetition detection for repetition format 12R45R, $Z(12) = 2.53, p < .05$, and a small but nonsignificant improvement for repetition format 12RR56, $Z(12) = 0.90, p = .37$, again confirming the first hypothesis from Experiment 1, but not the second. The reason for the clearer effect of repetition separation under item scoring in the present experiment than in Experiment 2A is unclear, but may relate to the concurrent demand to remember all of the elements, suggesting that the results of the present experiment are more comparable to those of Experiment 1.

Correlation between detection and recall. Performance on the serial recall task must be treated with caution, given that it was likely to be contaminated by the prior detection task. Nonetheless, it is interesting to examine the correlation between the detection of a repeated item and its recall (under item scoring). As expected, the majority of trials combined correct detection of a repeated item with recall of both its occurrences (Table 6). A failure to detect the repetition of an item was likely to be accompanied by a failure to recall it more than once. Indeed, combined tests of the contingency tables for each participant (Henson, 1996), collapsed across

Table 6

Contingency Between Number of Repeated Items Detected and Number Recalled, Summed Across Subjects and Grouping Condition in Experiment 2B

Detection and recall	Repetition format			
	12RR56	12R4R6	12R45R	1R345R
Detected				
Recalled	217	168	200	149
Not recalled	5	43	21	41
Not detected				
Recalled	1	9	1	4
Not recalled	17	19	18	46

grouping, showed highly significant correlations for each repetition format, $Z(12) > 7.02, p < .001$.⁴ There was a considerable number of trials in which correct detection of a repetition was not accompanied by correct recall. This is not surprising, because the repeated item may have been forgotten by the time of its recall, or, more likely, the simple act of saying aloud the repeated item impaired its subsequent recall (Crowder, 1968b). More interesting, however, is the relative scarcity of trials in which a repeated item was recalled twice but its repetition was not detected. These data suggest that repetition detection is necessary for recall of repeated items. In other words, the data support the hypothesis that repetition detection plays an important role in the repetition effects found in Experiment 1.

Though it was uncommon for participants not to detect the correct repeated item, it was informative to consider what happened in these situations: Did they not report any repetition, or did they think a different item was repeated? The answer depended on the repetition separation. When repeated elements were close together, it was more common to report repetition of a different item. For ungrouped repetition format 12RR56, for example, 29 of the 34 total detection errors in Experiments 2A and 2B were incorrect repetitions. When repeated elements were far apart, it was more common to fail to report any repetition. For ungrouped repetition format 1R345R for example, only 4 of the 41 total detection errors in Experiments 2A and 2B were incorrect repetitions.

Discussion of Experiments 2A and 2B

Experiments 2A and 2B provided some evidence supporting the role of repetition detection in repetition facilitation and repetition inhibition. This evidence included broad parallels between the effects of repetition separation on

Table 5
Probabilities of Correct Repetition Detection Under Item Scoring in Experiment 2B

Condition	Repetition format			
	12RR56	12R4R6	12R45R	1R345R
Ungrouped				
M	.91	.89	.87	.78
SD	.07	.11	.13	.20
Grouped				
M	.94	.87	.98	.80
SD	.08	.12	.06	.24

⁴ Participant-specific and list-specific effects may have inflated this correlation (Hintzman, 1972), though contingency tables for individual participants did show similar patterns. Another reason for the high correlation may be that participants in Experiments 2A and 2B performed the repetition detection task on the basis of covert recall of the whole list. However, though covert serial recall cannot be ruled out, anecdotal evidence suggested that this play was not necessary: Participants usually reported that repeated elements "jumped out" of the sequence.

correct positioning (Experiment 2A) and identification (Experiment 2B) of repeated elements, and its effects on recall (Experiment 1). Further support derived from the high correlation between detection and recall of repeated items and, in particular, the scarcity of recall in the absence of detection (Experiment 2B). Finally, the first specific hypothesis from Experiment 1 was confirmed: Grouping improved detection of repeated elements in repetition format *12R45R*, probably owing to the distinctive nature of the end of groups (Henson, 1997). Such improved detection may ensure that the repeated item is tagged (Lee, 1976b), removing repetition inhibition for this condition in Experiment 1.

However, other evidence questioned the role of repetition detection. Foremost, detection of a repeated item was still extremely good for all repetition formats (over 85% correct on average in both Experiments 2A and 2B). In particular, the difference between detection of the repeated item in ungrouped repetition formats *12RR56* and *12R4R6* was minimal, despite the corresponding transition from repetition facilitation to repetition inhibition in Experiment 1. Furthermore, the second specific hypothesis from Experiment 1 was not confirmed: Grouping did not impair repetition detection for repetition format *12RR56* in either Experiment 2A or 2B. If anything, a repetition was detected better when separated by a group boundary. Thus the removal of repetition facilitation for repeated elements straddling a group boundary in Experiment 1 seems inexplicable in terms of poorer detection of the repetition.

These data suggest that the notion of repetition tagging is incomplete. Though the overall level of detection and tagging of repeated items in Experiment 1 may have been lower than measured in Experiments 2A and 2B (because of the explicit instructions to report repeated items in Experiments 2A and 2B, or the potential for tags to become misplaced during recall of prior elements in Experiment 1), other findings remain problematic for the tagging hypothesis. Namely, the tagging hypothesis cannot explain why recall of repeated elements in ungrouped repetition format *12RR56* differed from that in ungrouped repetition format *12R4R6* or grouped repetition format *12RR56* in Experiment 1, when the levels of repetition detection were comparable in each case in Experiments 2A and 2B. One possible explanation is given in the General Discussion, which appeals to a special form of tagging for immediate repetition.

Experiment 3

In Experiments 2A and 2B, it was suggested that failure to detect or remember the repetition of an item results in repetition inhibition. However, the question remains as to why failure to remember the repetition of an item should lead to repetition inhibition in the first place. There was no doubt that participants in the previous experiments could vocalize and therefore presumably encode both occurrences of a repeated item. Yet why did they often fail to recall more than one occurrence?

One possibility is that people fail to repeat a previous response because of output interference: The act of recalling an item in the past makes it less available for recall in the

future. As Jahnke (1969a) remarked, "[repetition inhibition] is, at least in part, a result of interference arising from the act of sequential recall" (p. 620). Jahnke supported this claim with data from backwards recall, which suggested that repetition inhibition was stronger for the second repeated element to be recalled, rather than the second repeated element presented. Output interference can also explain why repetition inhibition is absent when recall of both repeated elements is not required, such as in probe recognition (Wolf & Jahnke, 1968) or probed recall (Jahnke, 1970). If output interference is an automatic, unconscious process, repetition inhibition would be expected even when the first repeated element is a redundant, response prefix (Crowder, 1968b). Moreover, repetition inhibition would still be expected when participants are well aware that repetition of responses is necessary (Crowder, 1968b; Hinrichs et al., 1973; Jahnke, 1969b).

There is an alternative to the output interference hypothesis however. Hinrichs et al. (1973) suggested that repetition inhibition may not reflect the operation of memory per se, but rather the guessing strategies used by participants when their memory has failed. If people have a default reluctance to repeat themselves, they would be biased against guessing a repeated item. In other words, they would be more likely to guess a control element correctly than guess a repeated element correctly. Thus, repetition inhibition may arise not so much from impaired performance on repetition lists because of output interference, but from improved performance on control lists owing to more successful guessing. This guessing hypothesis can not only account for most of the findings above, but is supported by further findings that are troublesome for the output interference hypothesis.

First, Greene (1991, Experiment 1) showed that repetition inhibition disappeared when participants were instructed not to guess, by virtue of poorer recall of control, but not repeated, elements. This is exactly the pattern predicted by the guessing hypothesis. Secondly, Hinrichs et al. (1973) showed that repetition inhibition decreased as the vocabulary size was increased (see also Jahnke, 1974). According to the guessing hypothesis, a larger vocabulary reduces the probability of guessing a control element correctly. Finally, the fact that overt output is not required for repetition inhibition was demonstrated by Mewaldt and Hinrichs (1977), who found repetition inhibition in a situation where participants had to report the missing item in a modified Cloze task. This was confirmed by Greene (1991, Experiments 2 and 3), who found repetition inhibition in a partial report task, where recall of only one repeated element was required. It is important to note that this repetition inhibition was contingent on the remaining items being displayed during recall, to bias guesses against these items.

The guessing hypothesis can also be extended to the results of Jahnke (1972b). Jahnke failed to find any repetition inhibition when every trial contained different items. In other words, it appeared that repetition inhibition required intertrial repetition as well as intratrial repetition. Moreover, when there was intertrial repetition of critical elements, repetition inhibition was normally absent on the first trial, but increased over subsequent trials. These findings led

Jahnke to suggest a role for proactive interference in repetition inhibition. However, the same pattern of results can be explained by the guessing hypothesis. The large vocabulary needed to prevent intertrial repetition will necessarily reduce the probability of guessing control elements, and hence reduce repetition inhibition. With a smaller vocabulary, the build-up of repetition inhibition over trials can be attributed to participants gradually learning the vocabulary and hence constraining their range of sensible guesses.

Several puzzles remain even for the guessing hypothesis, however. One puzzle is why participants are still reluctant to guess an item they have already recalled when they are well aware that lists can contain repeated items (e.g., Jahnke, 1969b). Though Mewaldt and Hinrichs (1977; Hinrichs & Mewaldt, 1977) showed that repetition inhibition was reduced when participants experienced greater frequencies of repetition, it was clearly not eliminated. A second puzzle is that, in direct contradiction to the guessing hypothesis, participants in the Walsh and Schwartz (1977) study reported no conscious avoidance of guessing repeated items. Indeed, they often reported taking into account the presence of a repeated item when guessing. A third puzzle is why Jahnke (1972b) found that most errors on critical positions were omission errors, rather than the substitution errors predicted by the guessing hypothesis. A final puzzle concerning the guessing hypothesis is that Walsh and Schwartz, unlike Greene (1991), failed to find a significant effect of guessing instructions on repetition inhibition. Greene argued that the large vocabularies used by Walsh and Schwartz would have decreased the potential for repetition inhibition and hence reduced the chance of finding a significant effect of instructions. However, the fact remains that Walsh and Schwartz still found considerable repetition inhibition even with strict instructions not to guess. Furthermore, Experiment 7 in Henson (1996) used a very small vocabulary and not only found highly significant repetition inhibition with instructions not to guess, but also with instructions specifically to remember which item was repeated.

One possible solution to these puzzles is that people are not always sure of when they are guessing and when they are not. This is why instructions not to guess may not always be effective. In addition, when people do decide to guess, it may not be that they are consciously avoiding guesses that would repeat previous responses, but that previous responses simply do not come to mind as possible guesses. In other words, both the output interference hypothesis and the guessing hypothesis can assume that the unavailability of repeated elements comes from an automatic, unconscious bias. In this case, the difference boils down to whether this bias causes forgetting of a repeated element (the output interference hypothesis), or prevents guessing of a repeated element already forgotten (the guessing hypothesis).

The aim of Experiment 3 was to resolve the debate between the output interference and guessing hypotheses. Rather than instructing participants not to guess, they were asked to indicate which of their responses were guesses, to see whether these guesses did present a bias against repeated elements. This provides a test of the guessing hypothesis.

However, given that participants may not always be certain of what constitutes a guess, they were further asked to indicate responses that they were simply unsure about. Both of these confidence ratings (guesses and unsure responses) were measured on-line during recall, through participants moving up and down an array of response boxes according to the confidence of each response. A bias toward control elements in uncertain responses would further support the guessing hypothesis. However, if significant repetition inhibition remained even when both guesses and unsure responses were removed from analysis, then there would also be support for the output interference hypothesis.

Method

Participants. Twelve participants from the Applied Psychology Unit Subject Panel were tested; 3 were men, 9 were women and their mean age was 30 years.

Materials. The 120 lists were generated in the same manner as in Experiment 1. This time however, there was an equal number of repetition lists and control lists, and only three repetition formats were used (see Table 2). Control and repetition lists were distributed equally over four blocks of 30 trials. The order of trials was pseudorandomized in the same manner as in previous experiments.

Procedure. Each participant attempted four conditions generated from factorial combination of two levels of grouping (ungrouped and grouped) and two levels of retention interval (a short delay of one distractor digit and a long delay of three distractor digits).⁵ The order of grouping conditions was constrained such that the two ungrouped conditions were attempted before the two grouped conditions (for the same reasons as in Experiments 2A). The order of short or long retention intervals was counterbalanced within this constraint.

The remaining procedure was similar to that of Experiment 1, except that participants indicated three levels of confidence for each response: confident, unsure and guess. The confidence of a particular response was indicated by where it was placed in an array of three rows: The top row was used for confident responses, the middle row for unsure responses and the bottom row for guesses. Participants were told they could move up and down the rows as much as they liked, providing they always gave exactly one response per column. In other words, they were always required to give six responses (omissions were not allowed), even if that meant guessing randomly from the vocabulary. The whole experiment took 50 min.

Results

In brief, position scoring of all responses replicated the results of Experiment 1. More important, the pattern of repetition inhibition remained under item scoring, whether or not guesses were included in the analysis. Further exclusion of unsure responses removed repetition inhibition for some conditions, but significant repetition inhibition

⁵ The manipulation of retention interval was to test whether participants were indicating confidence levels appropriately. Preliminary analyses showed that the long retention interval did result in more guesses and uncertain responses, as expected. However, though retention interval had a detrimental effect on recall, it did not interact with other factors. Subsequent analyses were therefore collapsed across this manipulation.

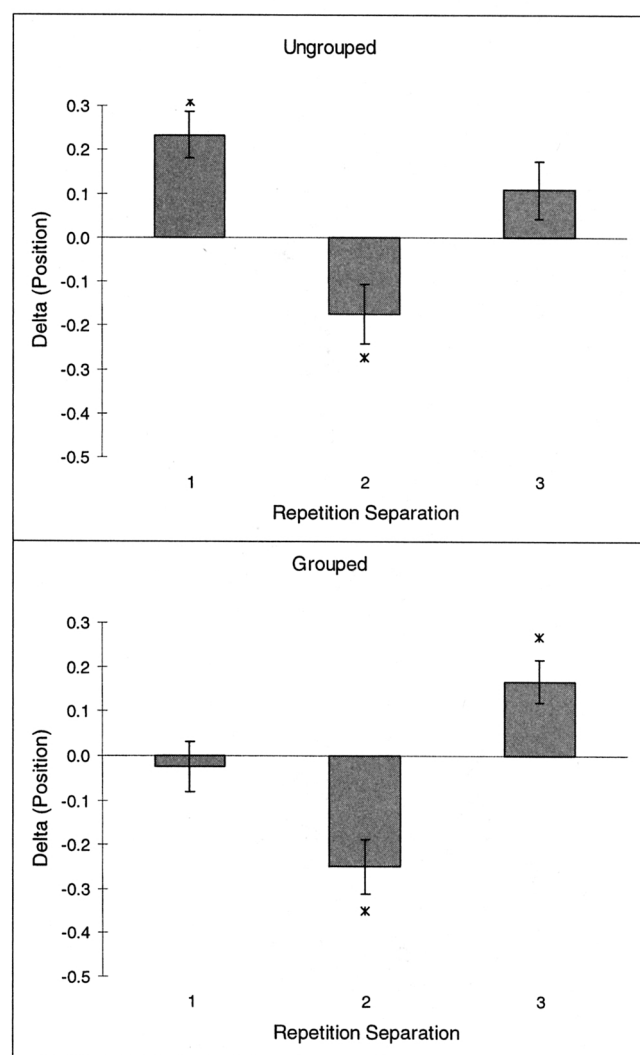


Figure 3. Delta under position scoring, including all responses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 3. Error bars show standard error of delta values; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

remained for two repetition formats when grouped. These results support a role for both guessing and output interference in repetition inhibition.

Position scoring of serial recall. With all responses included, delta was calculated under position scoring in the same manner as in Experiment 1. The basic pattern of delta values (see Figure 3) resembled that in Experiment 1 (see Figure 1), including the abrupt transition from facilitation to inhibition between adjacent and nonadjacent repeated elements in ungrouped lists. Repetition facilitation was also found for grouped repetition format *12R45R*, a nonsignificant trend in Experiment 1 (see Table 2). Indeed, tests of weighted log-odds showed that delta was significantly different from zero in all cases, $Z(12) > 2.70$, $p < .01$, except for adjacent repeated elements that straddled a group

boundary and three-apart repeated elements in ungrouped lists, $Z(12) < 1.73$, familywise $p < .05$. Further tests of weighted delta values across grouped and ungrouped conditions showed that grouping significantly reduced delta for repetition format *12RR56*, $Z(12) = 2.80$, $p < .01$, in agreement with Experiment 1. The effect of grouping on repetition format *12R45R* did not quite reach significance, $Z(12) = 1.72$, $p = .09$, but the fact the trend was in the same direction as in Experiment 1 supports the hypothesis that grouping improves recall of repeated elements at the end of groups.

The above results constitute an important, within-subjects replication of Experiment 1. Furthermore, both repetition facilitation and repetition inhibition appear robust to increased levels of guessing. However, the main purpose of the present experiment was to test whether these effects, particularly repetition inhibition, are robust to decreased levels of guessing. This was tested under item scoring, by successively removing less confident responses.

Item scoring of serial recall. Approximately 9% of responses were indicated as guesses, and an additional 24% were indicated as unsure. Interestingly, these percentages were almost identical for repetition and control lists. In subsequent discussion, the term *uncertain responses* refers to the 33% of responses that participants either guessed or were unsure about.

Delta values under item scoring were calculated in the same manner as in Experiment 1. With all responses included, the pattern of delta values (see Figure 4) was identical to that in Experiment 1. Significant repetition inhibition was found for all repetition formats, $Z(12) > 3.63$, $p < .001$, except one-apart repetition in ungrouped lists and three-apart repetition in grouped lists, $Z(12) < 2.16$, familywise $p > .05$.

When guesses were removed, delta increased for all conditions (see Figure 5). However, there was still significant repetition inhibition for all repetition formats, $Z(12) > 2.54$, $p < .05$, except one-apart repetition in ungrouped lists and three-apart repetition in grouped lists, $Z(12) < 0.95$, familywise $p > .05$. In other words, removing guesses did not change the pattern of significant results.

When all uncertain responses were removed, delta increased further still (see Figure 6). There was no longer significant repetition inhibition for any condition except for one-apart and two-apart repeated elements in grouped lists. Nevertheless, repetition inhibition for both of these conditions was still highly significant, even under a Bonferroni correction, $Z(12) > 3.18$, $p < .008$. In addition, there was significant repetition facilitation for one-apart repetition in ungrouped lists, $Z(12) = 2.94$, $p < .008$. No other delta values differed significantly from zero, $Z(12) < 1.65$, familywise $p > .05$.

The analyses of delta values were confirmed by a two-way, repeated-measures ANOVA on the log-odds of recalling two critical elements anywhere (expressed as probabilities in Table 7). The four variables were list type, repetition format, grouping, and response certainty. The two levels of response certainty were to include or exclude uncertain responses. There were significant effects of list

type, $F(1, 11) = 73.38, p < .001$; repetition format, $F(2, 22) = 8.77, p < .005$; grouping, $F(1, 11) = 56.36, p < .001$; and response certainty, $F(1, 11) = 73.38, p < .001$. As expected from Experiment 1, repetition format interacted with both list type, $F(2, 22) = 19.77, p < .001$, and grouping, $F(2, 22) = 9.51, p < .001$. In agreement with the analyses of delta values, response certainty also interacted with both list type, $F(1, 11) = 13.54, p < .005$, and grouping, $F(1, 11) = 5.69, p < .05$. Two three-way interactions were significant, that between list type, grouping, and repetition format, $F(2, 22) = 7.77, p < .005$, and that between list type, grouping, and response certainty, $F(1, 11) = 19.44, p < .001$. No other interactions approached significance.

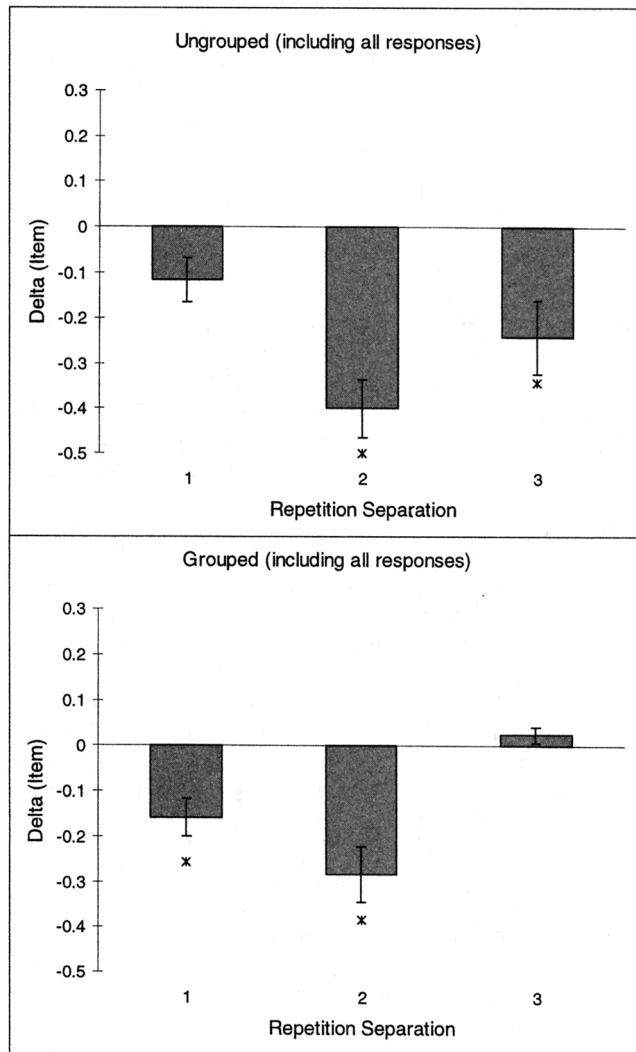


Figure 4. Delta under item scoring, including all responses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 3. Error bars show standard error of delta values; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

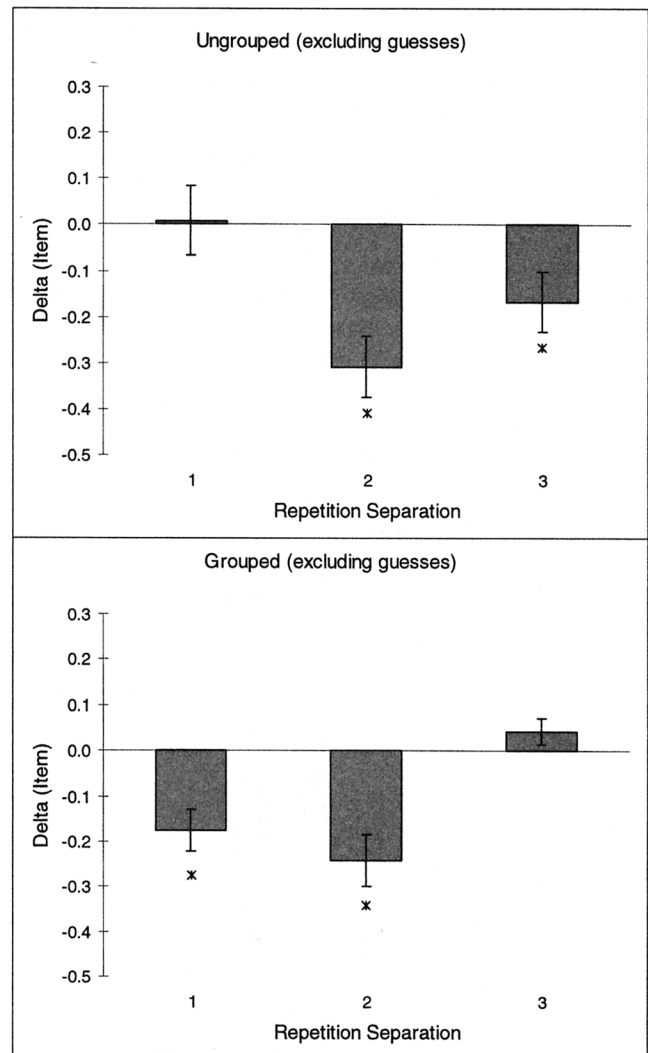


Figure 5. Delta under item scoring, excluding guesses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 3. Error bars show standard error of delta values; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

The interactions of interest concerned those between response certainty and list type. The two-way interaction between response certainty and list type reflected a greater reduction in recall of control elements than repeated elements when uncertain responses were removed, explaining the corresponding increase in delta across Figures 4–6. This interaction is consistent with the guessing hypothesis of Hinrichs et al. (1973). The three-way interaction between list type, grouping, and response certainty reflected a greater interaction between response certainty and list type in ungrouped lists than grouped lists. This interaction reflected the residual repetition inhibition in grouped lists when uncertain responses were removed (see Figure 6), suggesting an additional role for output interference.

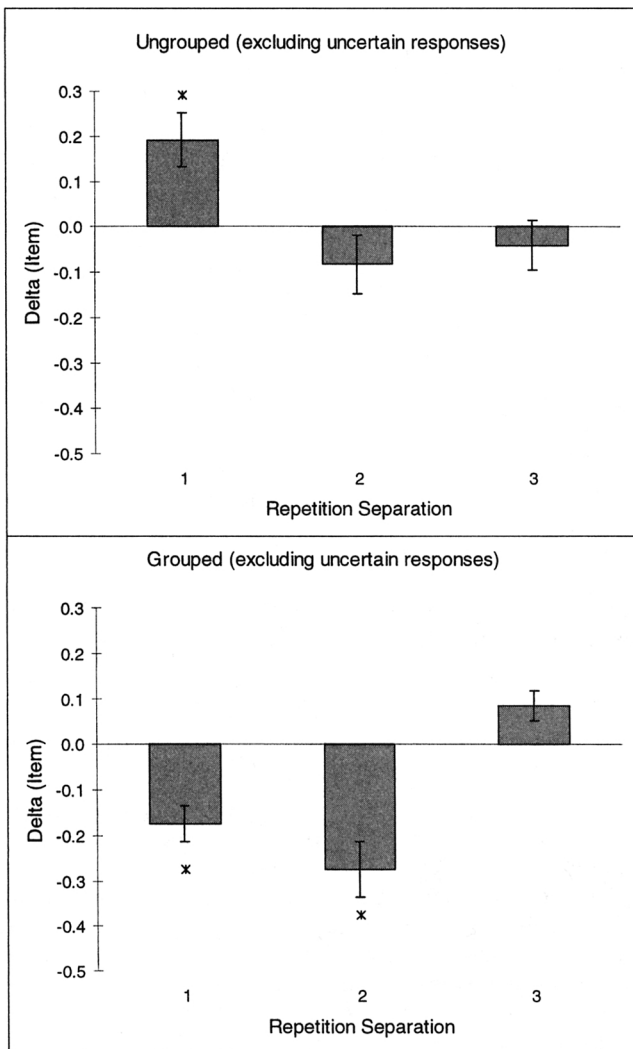


Figure 6. Delta under item scoring, excluding uncertain responses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 3. Error bars show standard error of delta; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.

Discussion

The results of the present experiment confirm the guessing hypothesis of Hinrichs et al. (1973), that guessing strategies pose a bias against repeated elements and in favor of control elements. This bias was apparent by successive removal of responses that participants guessed or were simply unsure about. Though one might not want to call all such responses "guesses," the effect of removing them was to reduce mainly the probability of recalling control elements; the probability of recalling repeated elements was not affected to the same extent. Thus, a considerable part of repetition inhibition reflects a better chance of guessing control elements than repeated elements.

However, the present data also suggest that guessing

strategies are not the only cause of repetition inhibition, because highly significant repetition inhibition remained in some conditions, even when all uncertain responses were removed. This is consistent with the significant repetition inhibition sometimes found when participants are instructed not to guess (Henson, 1996; Walsh & Schwartz, 1977). One reason why Greene (1991) did not find repetition inhibition with similar instructions may be that he only compared error rates on critical positions of serial position curves, a measure that Experiment 1 showed is not as sensitive as the delta measure. Furthermore, though the percentage of guesses in the present experiment was in line with the percentage suggested by Greene's data (no more than 10%), the exclusion of a further 24% of responses indicated as unsure means that the present experiment, as a test of the guessing hypothesis, erred on the conservative side, if at all.

How can this persistence of repetition inhibition be explained? The hypothesis outlined below is that both guessing strategies and output interference play a role. Moreover, though originally presented as competing hypotheses, both can be viewed as consequences of a more general process, that of *response suppression*.

Many models of serial recall assume that suppression of previous responses is necessary to output a sequence of items in the correct order (e.g., Burgess & Hitch, 1992; Henson, in press; Lewandowsky & Li, 1994; Page & Norris, in press). After an item is recalled once, suppression reduces its accessibility for output again in the future. This is normally advantageous in preventing erroneous repetitions and narrowing down the set of candidate responses during recall (Henson et al., 1996). However, its side effect is a bias against recalling a repeated item more than once. In addition, the suppression of an item already recalled may prevent that item coming to mind when one does decide to guess. In other words, response suppression can not only cause forgetting of a repeated item, but also prevent its guessing.

Because response suppression is assumed to be an automatic process (Henson, 1996), repetition inhibition can remain even when both guesses and uncertain responses are removed. The unconscious nature of suppression also explains why repetition inhibition remains even when people are aware that repetition is necessary (Jahnke, 1969b), and why people do not always report any conscious bias against guessing repeated items (Walsh & Schwartz, 1977). Moreover, because response suppression can prevent retrieval of a repeated item, as well as prevent its guessing, suppression explains why repetition inhibition can remain when people are instructed not to guess (Henson, 1996; Walsh & Schwartz, 1977) and why repetition inhibition can result from omission as well as substitution errors (Jahnke, 1972b).

Suppression is usually assumed to be partial rather than complete. In other words, suppression will not always prevent recall of both repeated elements: It simply reduces the probability of recalling both. In fact, many models (e.g., Henson, in press) assume that suppression is temporary, wearing off over time. This is necessary to explain why people do occasionally repeat themselves, even when there were no repeated items in the list. In the present experiment,

Table 7
Probabilities of Correct Recall of Critical Elements Under Item Scoring in Experiment 3

Condition	Responses analyzed					
	All responses			Excluding uncertain responses		
	12RR56	12R4R6	12R45R	12RR56	12R4R6	12R45R
Ungrouped						
Repeated						
<i>M</i>	.75	.53	.63	.66	.29	.33
<i>SD</i>	.16	.18	.29	.20	.23	.29
Control						
<i>M</i>	.87	.93	.88	.41	.38	.37
<i>SD</i>	.15	.10	.10	.31	.32	.32
Grouped						
Repeated						
<i>M</i>	.82	.67	.98	.59	.45	.79
<i>SD</i>	.19	.25	.04	.29	.31	.24
Control						
<i>M</i>	.98	.95	.96	.77	.73	.71
<i>SD</i>	.06	.07	.05	.27	.21	.24

for example, approximately 24% of control lists contained repetition errors. Such errors tend to be items recalled at the start of a report reappearing again at the end of a report, during which time suppression has worn off (Henson et al., 1996).⁶

Considerable numbers of erroneous repetitions were found in repetition lists too. In the present experiment, approximately 32% of repetition lists contained repetition of a wrong item. The greater percentage of such errors in repetition lists than control lists is usually taken as evidence that people sometimes detect a repetition event, but forget which item was repeated. The response suppression account suggests an alternative, or perhaps additional, reason: When people fail to recall the second repeated element due to suppression, they are likely to substitute another, less suppressed item from the list. The smaller set of such items in repetition than control lists means that a repetition error is more likely to result in the former.

The refractory nature of suppression can explain why Crowder (1968a, 1968b) found greater repetition inhibition when repeated elements were three positions apart than when they were more than three positions apart: The further apart the repeated elements, the longer the time for suppression to wear off. (The smaller range of repetition separations may explain why this trend was not found in the present study.) However, increasing repetition separation may also decrease the probability of detecting a repetition (Experiment 2B; Lee, 1976b). The trade-off between these two factors may depend on the particular repetition formats used. This reinforces the potentially complex nature of repetition effects in serial recall.

In summary, repetition inhibition may be attributed to two causes. One is output interference, which can cause forgetting of the second repeated element to be recalled. The other is a bias against guessing repeated items when an item is forgotten; a bias that may operate unconsciously as well as consciously. Nonetheless, it is possible that both output interference and guessing biases are consequences of a more general process of response suppression during serial recall. This automatic suppression of previous responses can not

only cause failure to retrieve an item more than once, but can also prevent it coming to mind should one decide to guess.

General Discussion

The present series of experiments confirmed that the presence of repeated items has important effects on serial recall from short-term memory, even under a new, conservative scoring scheme. Furthermore, these effects of repetition facilitation and repetition inhibition were shown to interact in a reliable, yet complex manner with repetition separation and grouping. The complexity of this interaction suggests that several factors play a role. This is probably why there have been numerous demonstrations of repetition facilitation and repetition inhibition in the literature, and yet no comprehensive theoretical interpretation has emerged. A summary of the empirical findings in the present study is given below, followed by one such attempt at a more comprehensive theory of item repetition effects in short-term memory. Finally, this theory is discussed in relation to current models of short-term memory and the phenomenon of repetition blindness.

Summary of Empirical Findings

All repetition effects in the present study were measured under a modified scoring scheme that treated control elements identically to repeated elements. This scheme overcomes a potential bias against the scoring of control elements, which may have caused an overestimation of repetition facilitation and underestimation of repetition

⁶ Interestingly, if the elderly were less effective at suppressing (inhibiting) previous responses (e.g., Hasher & Zacks, 1988), they would be expected to not only make more repetition errors in control lists, but, somewhat ironically, be less prone to repetition inhibition in repetition lists. The fact that repetition inhibition is a within-subject measure makes it particularly attractive to the study of developmental changes in inhibitory processes.

inhibition in previous studies. This may be why the present experiments found repetition inhibition as soon as one context element intervened between two repeated elements, whereas previous studies reported repetition inhibition only after two or more intervening context elements (e.g., Crowder, 1968a; Lee, 1976b). The new scoring scheme may also explain why significant repetition inhibition was found in the absence of guesses, where Greene (1991) failed to find such an effect. Nonetheless, other results were in broad agreement with previous studies, and the present scheme would seem appropriate as a conservative and unbiased method.

In the serial recall tasks of Experiments 1 and 3, repetition facilitation reflected mainly superior positioning of two repeated elements relative to two control elements. It was consistently found only for immediate repetition that did not straddle a group boundary.⁷ Repetition inhibition reflected inferior recall of two repeated elements anywhere in a report. It was typically found for all repetition formats that did not show repetition facilitation. Repetition inhibition was reduced by removing guesses, though some significant repetition inhibition remained even when any uncertain responses were removed from analysis.

In the repetition detection task of Experiments 2A and 2B, the probability of detecting a repetition was generally high, and decreased only slightly as repetition separation increased. The probability of detecting a repeated item also correlated highly with subsequent recall of that item (Experiment 2B). The effect of grouping was mainly to improve memory for repetition at the end of groups. Surprisingly, however, grouping did not impair memory for repetition across a group boundary.

A General Theory of Item Repetition Effects in Short-Term Memory

Repetition inhibition. The basic tenet of the theory is that repeated items face a negative bias against repetition during recall, which can be overcome only when their repetition is explicitly remembered. The negative bias, underlying repetition inhibition, has several aspects. In a task such as serial recall, the main bias arises from response suppression. This is the automatic suppression of previous responses assumed by many models of serial recall (Burgess & Hitch, 1992; Henson, in press; Lewandowsky & Li, 1994; Page & Norris, in press) and which may be a general requirement in sequential output (Houghton & Tipper, 1996; MacKay, 1987). Suppression of items after their recall can prevent repeated items from being retrieved more than once.

A second aspect of repetition inhibition arises when people resort to guessing. Response suppression may also prevent repeated items from coming to mind when one decides to guess (see *Discussion* to Experiment 3). Note that the unconscious guessing bias resulting from response suppression is not to deny an additional conscious bias in other situations. People may have a natural reluctance to repeat themselves, which may also prevent them from guessing a repeated item (Hinrichs et al., 1973). This conscious bias might apply to a range of tasks; it is not necessarily restricted to serial recall (Greene, 1991; Mewaldt & Hinrichs, 1977).

Both response suppression and a guessing bias are sufficient to explain why repetition inhibition reflects a failure to recall both repeated elements (Experiment 1) and, in particular, the second repeated element to be recalled (Crowder, 1968a; Jahnke, 1969b; Wickelgren, 1965). However, both are necessary to explain why discouraging or removing guesses decreases repetition inhibition (Experiment 3; Greene, 1991), but does not eliminate it (Experiment 3; Henson, 1996; Walsh & Schwartz, 1977). Furthermore, only an unconscious process of suppression can explain why people do not always report a bias against guessing repeated elements (Walsh & Schwartz, 1977), why many errors in recall of repeated elements are omissions (Jahnke, 1972b), and why repetition inhibition can remain unaffected by a secondary distraction task, which would presumably attenuate any conscious guessing bias (Baddeley & Andrade, 1995). On the other hand, only a guessing bias can explain the effects of vocabulary size (Hinrichs et al., 1973; Jahnke, 1974), changes in repetition inhibition across trials (Jahnke, 1972b), and repetition inhibition in tasks where only one repeated element is recalled (Greene, 1991; Mewaldt & Hinrichs, 1977). A similar two-factor account was proposed by Arbuthnott (1996) for repetition inhibition in sequential arithmetic problems. In addition, both conscious (Baddeley, Emslie, Kolodny, & Duncan, 1995) and unconscious (Brugger, Monsch, & Johnson, 1996) factors have been proposed for people's failure to give appropriate numbers of immediate repetitions in random generation tasks.

Repetition detection. Recall of repeated elements can be aided when their repetition is explicitly remembered. In order to be remembered, the repetition event must first be detected. Though people may correctly encode both occurrences of a repeated item (inferred from the present study because participants almost invariably vocalized both repeated elements), they do not automatically notice that an item has been repeated. Generally, the probability of detecting repetition is lower the more widely separated the repetition (Experiment 2B; Lee, 1976b). However, the organization of a sequence, such as its temporal grouping, can also affect the probability of detecting repetition. In particular, the distinctive nature of the end of groups (Henson, 1996) improves detection of repeated elements at these positions (Experiments 2A and 2B).

However, overcoming repetition inhibition requires not only detecting the repetition during presentation, but also remembering which particular item was repeated come recall. Sometimes one can remember the repetition event, but forget which item was repeated. This explains why repetition inhibition remains when repetition is expected (Mewaldt & Hinrichs, 1977), reminded (Jahnke, 1969b), or even monitored (Experiment 2B). When the repetition event is detected, but the repeated item forgotten, people may

⁷ Repetition facilitation was also found for repetition format 12R45R in the grouped condition of Experiment 3. However, this result must be treated with caution, because this was the only format that participants were able to describe accurately during debriefing. As such, participants may have extracted an explicit schema for this format during the grouped condition (see Henson, 1996).

guess, or try to reconstruct the missing item. Because repetition detection is more likely the closer the repeated elements, repetition of a different item will necessarily be more frequent in such cases (Experiment 2B).

Accurate memory for a repetition can counteract the general bias against repetition during recall. Correct memory, or tagging, of the repeated item (Drewnowski, 1980; Lee, 1976b) will remove any conscious bias against guessing it, and perhaps prevent its suppression during recall (at least until it has been output twice). This will reduce the difference between recall of repeated and control elements. The magnitude of repetition inhibition will therefore depend mainly on the number of trials in which a repeated item is correctly detected and remembered, relative to the number of trials in which it is not detected or forgotten. This entails a strong correlation between repetition detection and serial recall (Experiment 2B).

Repetition facilitation. Though memory for the repeated item is necessary to prevent repetition inhibition, it is not always sufficient to cause repetition facilitation. For example, memory for adjacent repeated elements that straddle a group boundary is as good as, or better than, memory for adjacent repeated elements that do not (Experiments 2A and 2B), and yet only the latter show repetition facilitation (Experiments 1 and 3). Likewise, the dramatic transition from repetition facilitation to repetition inhibition as soon as one context element intervenes between two repeated elements (Experiments 1 and 3) is not mirrored by a large change in detection of the repeated item (Experiments 2A and 2B). These somewhat paradoxical results can be explained by appealing to a special type of tagging for immediate repetition (i.e., that which does not straddle a group boundary). Such *immediate repetition tags* are associated with a particular position in a sequence, and their purpose is to cause immediate repetition of the item recalled at that position (cf. Houghton, Glasspool, & Shallice, 1994; Rumelhart & Norman, 1982). This increases the probability of recalling repeated elements above that of control elements, by ensuring that both repeated elements are recalled in adjacent positions. Indeed, such specialized coding of the immediate repetition of an action may have evolved specifically to overcome suppression of prior actions (e.g., MacKay, 1987).

If immediate repetition is tagged often enough over trials, repetition facilitation can emerge, most obviously under position scoring (Experiment 1). This is because tagging of immediate repetition means there is no possibility for the second repeated element to transpose with subsequent context elements, as there is for the second of two adjacent control elements. Such tagging also explains the sharp transition from facilitation to inhibition as soon as one context element intervenes between two repeated elements. Repetition across a group boundary is not tagged however, because immediate repetition during recall would interfere with the grouped organization of recall (e.g., *12R R56* recalled as *12RR 56*). This is why repetition facilitation does not occur across group boundaries, even though detection of the repetition may actually be improved.

There is also indirect evidence that tagging of immediate repetition is qualitatively different from the tagging of other

repetitions. It was only with repetition formats containing adjacent repeated elements in the present study that people sometimes repeated an erroneous response in two adjacent positions (e.g., *12RR56* recalled as *12R556*). In all other formats, such repetition errors were much further apart. The former type of error can be attributed to an immediate repetition tag being triggered at an incorrect position, much like the Houghton et al. (1994) account of spelling errors such as *scholl*. (An inability to suppress the immediate repetition tag itself may explain errors like *schoool* found in some dysgraphic patients; Veneri, Cubelli, & Caffarra, 1994.) The latter type of error can be attributed to people forgetting which item was repeated, and making a repetition error when all other responses have been suppressed (see *Discussion of Experiment 3*).

There are many aspects of the above theory that warrant further investigation. The most obvious questions concern the interaction between repetition detection and serial recall. How exactly does tagging of a repeated item prevent repetition inhibition? How do immediate repetition tags operate, occasionally incorrectly? These questions require a more precise model of short-term memory.

Models of Short-Term Memory and the Issue of Item Representation

Several models of short-term memory have been developed recently (e.g., Burgess & Hitch, 1992; Drewnowski, 1980; Henson, in press; Lee & Estes, 1981; Lewandowsky & Murdock, 1989; Nairne, 1990; Page & Norris, in press; Shiffrin & Cook, 1978). Some assume type representations (e.g., Burgess & Hitch, 1992), some require token representations (e.g., Page & Norris, in press), and some assume a combination of both (e.g., Henson, in press; Nairne, 1990). Surprisingly, however, not one of these models has fully addressed the issue of repeated items; their simulations have assumed lists of unique items (with the possible exception of Drewnowski, 1980, who addressed repetition facilitation, but not repetition inhibition).

The ability to detect the repetition of an item clearly demands type representations of items at some level of memory. Indeed, within the general theory of repetition inhibition outlined above, the process of response suppression is assumed to operate over type representations. However, these assumptions do not imply that serial order is stored over type representations. In fact, though present data indicate that the presence of repeated items in sequences can impair recall of those sequences, models that store order over type representations (associative models; Wickelgren, 1969) appear to face much greater problems in recalling such sequences.

For example, one class of models assumes that serial order is stored through associations between successive items (Lewandowsky & Murdock, 1989; Wickelgren, 1966). The order of items is reconstructed in a process of chaining, whereby each item becomes (part of) the cue for the next item. If these items are represented as types, a repeated item will be associated with more than one successor, making it an ambiguous cue for its successors. This ambiguity can be reduced by assuming that the cue includes a number of

previous responses (e.g., Jordan, 1986), distinguishing repeated elements with different predecessors. However, to the extent that cues following repeated elements retain some similarity, chaining models still predict a greater probability of errors following a repeated element than a control element. Experiments by Wickelgren (1966) appear to support this prediction, but more recent work has questioned this support, once the scoring scheme takes into account the type-token distinction, as in the present study, and possible guessing biases (Henson, 1996). More generally, such associative chaining models face increasing difficulties as the number of repeated elements in a sequence increases.

Another class of associative models stores order by associations not between successive items, but between positional cues and items. These models also have problems recalling sequences with repeated elements. In the Burgess and Hitch (1992) model for example, any overlap between the positional cues for repeated elements will reinforce the associations between those cues and the type representation of the repeated elements. This will cause a tendency for repeated elements to be recalled too early (a problem related to the model's difficulty with phonologically similar items; Henson et al., 1996). This is inconsistent with the results of Experiment 1, which showed that, provided people recall a repeated item twice, they have no greater difficulty in positioning repeated elements than control elements.

Yet other models store order by a gradient of item activations in memory (e.g., Page & Norris, in press). These models cannot represent serial order over type representations, because repetition of an item will increase its activation relative to surrounding items, disrupting the representation of serial order. Moreover, activation models that did assume type representations (e.g., strength models; Hintzman, 1976) would have difficulty explaining Lee's (1976b) finding that the probability of recalling at least one repeated element does not differ from the probability of recalling at least one control element.

The above problems of ordering sequences with repeated elements do not apply to nonassociative models (Wickelgren, 1969) or multiple trace models (Hintzman, 1976), in which repetitions are stored as distinct tokens. In Henson's (1996, in press) model, for example, encoding of each element produces a position-sensitive token in short-term memory (though when elements are presented too rapidly, there may be insufficient time to generate unique tokens for repeated elements, resulting in repetition blindness; e.g., see Kanwisher, 1987). At this stage, repeated elements are equivalent to control elements, and can be recalled equally well. The specific effects of item repetition arise from subsequent processes, such as the detection and tagging of tokens that match the same type representation in long-term memory. Repetition inhibition, in particular, arises later in serial recall, when the token best matching the position of recall is selected for output. This token is assumed to make contact with its type representation again, in order to produce a categorical response, and it is the suppression of these type representations that causes repetition inhibition.

In summary, as far as current models of short-term memory are concerned, those that assume order is repre-

sented over token representations seem best suited to explain people's general ability to recall sequences with repeated elements. The specific effects that item repetition has on serial recall may well arise from special mechanisms geared toward the detection of repetition (such as tagging of immediate repetition), or the output of a response (such as response suppression). The implementation of such mechanisms in more explicit models will help clarify general theories of repetition facilitation and repetition inhibition like that outlined here. Nonetheless, the present data do not decide the issue of types versus tokens, and the question of item representation in short-term memory remains an open one. Its resolution may well depend on demonstrations that particular models with particular representations can account for the empirical data, such as those from the present study.

Repetition Blindness and Repetition Inhibition

The failure to recall repeated items from short-term memory (repetition inhibition) resembles the failure to detect repeated items in rapidly presented sequences (repetition blindness). Repetition inhibition is not simply a special case of repetition blindness, however. As discussed above, repetition inhibition in the present study occurred even with slow presentation rates of 500 ms per item (rather than the 100 ms per item often used in rapid serial visual presentation), with vocalization of each item as it was presented, and for repetitions widely separated in time. Indeed, repetition facilitation, rather than inhibition, was more likely for repeated elements close in time (cf. Kanwisher, 1987).

The question more appropriate to the current debate on repetition blindness (e.g., Armstrong & Mewhort, 1995; Kanwisher et al., 1996) is whether repetition blindness is a special case of repetition inhibition. Fagot and Pashler (1995) argued that demonstrations of repetition blindness typically use serial recall to index performance, and hence may be confounded by repetition inhibition. Whittlesea and Podrouzek (1995) argued that demonstrations of repetition blindness typically fail to take into account potential scoring biases between repeated and control elements. The bias addressed in the present study is a good example. Nonetheless, more recent studies (e.g., Hochhaus & Johnston, 1996; Kanwisher et al., 1996) have used tasks such as recognition that do not require recall of more than one repeated element. The repetition deficits in these cases suggest that repetition blindness is indeed a distinct phenomenon.⁸

⁸ Further evidence for a dissociation between repetition blindness and repetition inhibition comes from a study by MacKay, Abrams, and Pedroza (1996). They found that age accentuated the repetition deficit in a spelling detection task, yet attenuated the repetition deficit in a spelling reproduction task, analogous to serial recall. This supports the distinction of repetition blindness as a perceptual or input phenomenon and repetition inhibition as a production or output phenomenon. In particular, the attenuation of repetition inhibition with age might be attributable to less effective suppression of previous responses (see footnote 5). Studies like these raise the exciting prospect of several, dissociable forms of repetition deficit.

Conclusion

The results from the present study suggest that repetition inhibition arises because people often fail to retrieve, or guess, the second occurrence of a repeated item, unless they explicitly detect and remember repetition of that item. Both retrieval failure and guessing biases may reflect a more general process of response suppression during recall. Detection of a repetition is more likely the closer or the more salient the repetition (e.g., at the end of groups). In particular, the special case of immediate repetition can be tagged so as to increase the probability of recalling both occurrences above that of control items and produce repetition facilitation. These complex yet robust findings represent important challenges for models of short-term memory.

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Appendix

Possible Reports of a Repetition List and a Control List

Repetition list					
1	2	R ₁	4	5	R ₂
1	2	R ₂	4	5	R ₁
1	2	R ₁	4	5	R ₁
1	2	R ₂	4	5	R ₂
Control list					
1	2	3	4	5	6
1	2	6	4	5	3
1	2	3	4	5	3
1	2	6	4	5	6

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