Chapter 7: Item repetition in serial recall

Repetition facilitation, inhibition and contamination

The experiments and simulations in previous chapters used lists of unique items. This chapter describes three experiments that examine serial recall of lists in which an item is repeated. The results of these experiments, together with those described in Henson (1996b), are consistent with SEM's assumptions of positional tokens and suppression of type representations. Nevertheless, the results also highlight additional assumptions required before SEM, or any other model, can fully explain item repetition effects in serial recall.

Ranschburg Repeated

The presence of a repeated item in a list has important effects on the serial recall of that list. Foremost are effects on the recall of the repeated item itself, the *Ranschburg* effects (Jahnke & Bower, 1986). When two occurrences of an item are close together in a list, recall of both occurrences is generally superior to recall of two different items at corresponding positions in control lists 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 lists (*repetition inhibition*; e.g., Crowder, 1968a; Jahnke, 1969b). Repetition can also affect recall of other, nonrepeated items in a list (*repetition contamination*), particularly those immediately following a repeated item (Wickelgren, 1966). 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 experiments 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, as in Wickelgren's associative theories (Wickelgren, 1969), or does each occurrence form a separate token representation in memory, as in Wickelgren's nonassociative theories? This question is particularly relevant to chaining theory, because associative chaining models predict a detrimental effect of item repetition (given that repeated items are associated with more than one successor; Chapter 1). One way to overcome this problem is to assume nonassociative chaining models, which chain along token rather than type representations (Wickelgren, 1969).

The representational issue is particularly apparent in explicit, computational models, whether or not they employ chaining. For example, the Articulatory Loop Model (Burgess & Hitch, 1992, 1996b) assumes type representations, whereas the Primacy Model (Page & Norris, 1996b) requires token representations. SEM also assumes token representations, though these are linked to a common type representation at output, in order to produce a categorical response (Chapter 5). Surprisingly however, none of these models has directly addressed recall of repeated items; simulations have always assumed lists of unique items.

Models that do address repetition in more detail have generally dealt with long-term memory, such as the typing model of Rumelhart and Norman (1982) and the spelling model of Houghton, Glasspool and Shallice (1994). These models assume special mechanisms for dealing with repetition (particularly immediate repetition). The repetition mechanism of Houghton et al. (1994) is supported by neuropsychological data, and is justified theoretically in order to overcome the temporary suppression of previous actions during sequential behaviour (e.g., Houghton & Tipper, 1996; MacKay, 1987). This may correspond to the suppression of SEM’s type representations during serial recall, suggesting one possible explanation for repetition inhibition.

Another reason for the interest in item repetition is in connection with repetition blindness (Kanwisher, 1987). Repetition blindness is the failure to detect or respond to repeated elements in rapidly presented sequences (of around 100 ms per item). Though repetition blindness is unlikely in the present experiments, where presentation rates are 500 ms per item or more, some previous studies have used serial recall to index repetition blindness and have been potentially confounded therefore 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, Dorken & Podrouzek, 1995; Whittlesea & Podrouzek, 1995), though others have argued that repetition blindness is a separate perceptual phenomenon (Bavelier, 1994; Kanwisher, Driver & Machado, 1995; Luo & Caramazza, 1995; Park & Kanwisher, 1994). Much of the confusion has resulted from a failure to distinguish type and token representations in the scoring of serial recall. The new scoring scheme introduced below may help resolve some of the confusion.

Repetition facilitation and repetition inhibition are simple to demonstrate in short-term, serial recall, and are robust to experimental manipulations such presentation rate or presentation modality (Mewaldt & Hinrichs, 1973). Consequently, the effects have been subject to considerable research. In order to summarise, the separate occurrences of a repeated item are the *repeated elements* of a *repetition list*. Nonrepeated items at the same positions in *control lists*, containing no repeated items, are the *control elements*. These are the *critical elements*; remaining nonrepeated elements in repetition and control lists are *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. Wickelgren (1965c) showed a similar transition between repetition facilitation and repetition inhibition using item-scoring (i.e., whether a critical element was recalled anywhere in a subject’s report) rather than position-scoring. The repetition inhibition stemmed from a failure to recall the repeated item more than once. The importance of recalling both critical elements was reinforced by Lee (1976b). Lee 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.

Repetition contamination can also be measured in different ways. Under item-scoring, context elements are usually recalled better in repetition lists than in control lists (Wickelgren, 1965c). Under position-scoring however, an advantage for context elements arises only when repeated elements are close together (i.e., under conditions of repetition facilitation, Crowder, 1968a; Mewaldt & Hinrichs, 1973). These results may be reconciled by a trade-off between two opposing factors, one which increases the recall of context items generally, and another which decreases the probability of recalling them in the correct position.

The general increase in correct recall of context elements in repetition lists may owe to a reduced set of possible responses: If an item is forgotten, there is one less item to guess from in repetition lists than control lists. This can improve recall of context elements under both position- and item-scoring. One factor that might impair positioning of context elements is the presence of *associative intrusions* following repeated elements (Wickelgren, 1966).[1] These are transpositions between the context elements that immediately follow repeated elements (and therefore only occur when repeated elements are separated in the list). When conditionalised on correct recall of the repeated element, Wickelgren found such associative intrusions to occur significantly more often than equivalent transpositions following correct control elements in control lists. Wickelgren used associative intrusions to support an associative chaining theory of serial recall (Chapter 1), stating that "...the prior item is an important cue in short-term memory for serial lists and there is only one representative of an item no matter how many times it is presented." (Wickelgren, 1966, p. 858). One question asked here is whether associative intrusions, together with a guessing bias, are a sufficient account of repetition contamination.

Thus the simple presence of repeated elements can have a number of different effects on serial recall. It is somewhat surprising therefore that theoretical interpretation of these effects remains unclear. Repetition facilitation has been variously attributed to chunking (Wickelgren, 1965c), isolation or distinctiveness (Lee, 1976a), and repetition 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). Repetition contamination has been attributed to associative chaining (Wickelgren, 1966) and grouping strategies (Henson et al., 1996). The present set of experiments aim to resolve some of these issues.

However, all previous studies of these repetition effects have overlooked the type/

1. Though these errors are not really intrusions in the present terminology (they are transpositions and a special type of associate; Chapter 1), Wickelgren's term is maintained for consistency with previous studies. Note that a similar effect of repetition contamination arises in speech errors: the "repeated phoneme effect" (Dell, 1984).

token distinction. 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.

Fortunately, it is possible to overcome this problem with a new method of scoring control lists. This method is introduced in Experiment 6 and employed in all subsequent experiments. Indeed, the main aim of Experiment 6 was to determine whether repetition facilitation and repetition inhibition remain significant effects under this new method of scoring. Experiment 6 also examined the interaction between these effects and temporal grouping of lists, another theme continued through subsequent experiments. Experiment 7 examined people's ability to detect and remember the repetition of an item, on-line with serial recall (complementing the off-line task in Experiment 2 of Henson, 1996b). There is good evidence that memory for the repetition plays an important role in Ranschburg effects, and the ability to detect and remember repetition may be affected by grouping. Finally, Experiment 8 examined the role of guessing in Ranschburg effects; in particular, to test whether repetition inhibition is more than a bias against guessing repeated items.

Experiment 6

The main aim of the first experiment was to measure repetition facilitation, inhibition and contamination under a new method of scoring control lists that overcomes any bias in favour of repetition lists. This bias arises because, unlike a transposition between two control elements, a transposition between two repeated elements can not be detected by the experimenter. This bias potentially confounds all previous demonstrations of repetition facilitation and repetition inhibition.

For example, consider recall of a control list represented by the sequence *123456,* and a repetition list represented by the sequence $12R_1R_256$, where R_1 and R_2 are the two repeated elements. A subject's report of the control list as *124356* would be marked as containing incorrect positioning of the control elements *3* and *4*. However, a subject's report of the repetition list as *12*R_2R_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 list 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 very common type of error in serial recall (the locality constraint; Chapter 4), any repetition facilitation found in such situations could be no more than a scoring bias against control elements.

One way to try to overcome the bias is to use an item-scoring criterion, where an element is scored correct irrespective of its recall position. However, this does not address people's ability to recall a list 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, 1965c). 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. For example, all four reports of the repetition list *1*R_1*34*R_2*6* in Table 7-1 will look identical to the experimenter, even though they may represent different outputs from short-term memory. To control for this, all four reports of the control list *123456*, in Table 7-1 would be judged as correct under the *modified control scoring.* There are several points worth noting about the reports in Table 7-1:

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 Position 2 or Position 5, 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. Repetitions of a response in the third and fourth reports of the control list might appear rare from the meta-analysis of Chapter 4, but that was when subjects knew that lists never contained repeated items. When subjects are aware that some lists contain repeated items, as in the present experiments, a greater incidence is likely to result. Furthermore, an even higher incidence is likely when lists are grouped (Chapter 3).

Reports of Repetition List					
1	R_1	3	4	R_2	6
1	R_2	3	4	R_1	6
1	R_1	3	4	R_1	6
1	R_2	3	4	R_2	6
Reports of Control List					
1	2	3	4	5	6
1	5	3	4	2	6
1	2	3	4	2	6
1	5	3	4	5	6

Table 7-1: Example reports of a repetition list *1R34R6* and a control list *123456* that are correct under modified control scoring.

4. If Item 6 were recalled in place of Item 3, Wickelgren (1966) would have scored it as an associative intrusion in all four reports of the repetition list, but only in the first and third reports of the control list (because the previous control element would not be correct in the second and fourth report), potentially biasing measures of repetition contamination.

In addition to the modified scoring of control lists, the present study introduces a single index of repetition facilitation and repetition inhibition. These effects have been scored 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 under modified control scoring (where correct recall is further defined under either position- or item-scoring criteria). 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 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, 1969a; Wickelgren, 1965c).

If repetition facilitation and repetition inhibition arise above possible scoring biases, then previous research suggests that both should be demonstrable in the present experiment simply by varying the separation between critical elements. Plotting delta against *repetition separation* should give some form of facilitation-inhibition continuum, with positive delta at short separations and negative delta at longer ones. In the present experiments, repetition separation was defined over several different *repetition formats*, depending on the exact positions of repeated elements. This was to examine the role of serial position in addition to repetition separation.

The second aim of Experiment 6 was to examine how this continuum changes with the introduction of temporal grouping. 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? These questions may shed further light on the nature of repetition effects. Another reason for investigating the effect of temporal grouping is that people will often spontaneously group lists subjectively (Chapter 3). It is possible that the choice of grouping strategy will be influenced by the presence of repeated elements in a list. This possibility was also noted in passing by Walsh and Schwartz (1977):

> “Both during presentation and during recall, subjects often imposed their own intonation groupings or rhythmic patterns upon the items within the sequence. This suggests 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 intrasequence repetition.” (p. 68).

The presence of objective, temporal grouping of lists should override any subjective grouping strategies triggered by particular repetition formats, and hence eliminate the bias suggested by Walsh and Schwartz.

The final aim of Experiment 6 was to measure repetition contamination, particularly errors following correct recall of a repeated element (*contamination errors*). Wickelgren's associative intrusions are contamination errors following one repeated element that are transpositions of the item that immediately followed the other repeated element. For example, recall of a list *1R34R6* as *1R63R4* contains two contamination errors (Items 6 and 4), the first of which (Item 6) is an associative intrusion.

The evidence for associative intrusions in Wickelgren's (1966) experiment was rather weak. Indeed, Wolf and Jahnke (1968) failed to find a significant incidence of associative intrusions. Moreover, associative intrusions could be the result of subjective grouping, rather than item-item chaining. The presence of repeated elements in a list might affect (and even effect) spontaneous grouping of that list, as a number of subjects in subsequent experiments reported. For example, the list *12R4R6* might be grouped as three groups of two, the repeated elements triggering groups of this size during rehearsal. If repetition lists were grouped in this manner, with repeated elements starting groups, then associative intrusions could be no more than a special case of interpositions between groups (Chapter 3). If this explanation were true, then overriding subjective grouping by objective grouping should remove any difference in the incidence of associative intrusions in repetition lists compared with control lists.

In summary, the present experiment measured repetition facilitation, inhibition and contamination within the same design, by manipulating the position of repeated elements (within subjects) and the presence or absence of temporal grouping (between subjects).

Method

Subjects

Twenty-four subjects from the APU Subject Panel were assigned to two conditions. There were ten women and two men in the ungrouped condition (mean age of twenty-nine), and eight women and four men in the grouped condition (mean age of twenty-eight).

Materials

The letters *J, H, R, Q, V, M* were used to generate 144 lists of 6 items. 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 (Table 7-2). 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.

List Type	Repetition Format	Repetition Separation	No. of Lists
Control	1 2 3 4 5 6	-	48
Repetition	1 R R 4 5 6	1	12
Repetition	1 2 R R 5 6	1	12
Repetition	1 R 3 R 5 6	2	12
Repetition	1 2 R 4 R 6	2	12
Repetition	1 R 3 4 R 6	3	12
Repetition	1 2 R 4 5 R	3	12
Repetition	R 2 3 4 R 6	4	12
Repetition	1 R 3 4 5 R	4	12

Table 7-2: Composition of lists in Experiment 6.

Control and repetition lists were distributed equally over 6 blocks of 24 trials. The order of trials was pseudorandomised, with the constraint that two consecutive trials never contained the same repetition format nor repetition of the same item. Subjects did not know in advance whether the next trial would contain a repeat, though the ratio of repetition to control lists would lead them to expect repetition more often than not.

Procedure

Each letter was presented in the centre of a VDU, replacing the previous one, at a rate of two every second (400 ms on, 100 ms off). The grouped condition included an further 500 ms pause after every third letter. The sixth letter was followed by a sequence of three distractor digits (drawn randomly without replacement from the set *1-9*), presented at the same rate as the letters. Subjects were instructed to vocalise each letter and digit as it appeared, but to recall only the letters. Vocalisation of the letters was monitored by the experimenter to ensure that subjects perceived the items correctly, particularly the repeated items. (In fact, errors in vocalisation were extremely rare.) Though vocalisation of the items introduced potentially complicating effects of echoic, auditory information, vocalisation of the distractor digits should minimise 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 letters, which were written down by the experimenter. Subjects were encouraged to guess if they were unsure, or to say *blank* if no letter came to mind. They were alerted to the fact that some lists contained repeated letters. After ten practice trials, each subject attempted all six blocks, with the order of blocks counterbalanced across subjects. The whole experiment took approximately 50 minutes.

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. Furthermore, repetition facilitation and repetition inhibition were dissociable by different scoring criteria: repetition facilitation reflected superior positioning of repeated elements, whereas repetition inhibition reflected inferior recall of repeated elements anywhere.

There was a nonsignificant trend for a greater proportion of errors following repeated elements to be associative intrusions than following correct elements, irrespective of grouping. However, this trend may have reflected a guessing bias between the two list-types, and therefore does not constitute evidence for associative chaining models of serial recall.

Position-scoring of Serial Recall

The probability of recalling critical elements on both critical positions was calculated for repetition and control lists (Table 7-3). A three-way ANOVA on log-odds showed a significant effect of list type, $F(1,330)=45.21$, $p<.001$, and repetition format, $F(7,330)=31.18$, $p<.001$. There was no significant effect of grouping, $F(1,22)<1$, but grouping did interact with repetition format, $F(7,330)=5.35$, $p<.001$. The interaction between list type and repetition format was also significant, *F(7,330)=15.91, p<.001*, as was the three-way interaction between list type, repetition format and grouping, *F(7,330)=3.01, p<.005*..

	Repetition Format							
Condition	1RR456	12RR56	1R3R56	12R4R6	1R34R6	12R45R	R234R6	1R345R
Ungrouped								
Repeated	.74 (.16)	.75 (.14)	.55 (.16)	.49 (.15)	.43 (.14)	.64 (.17)	.50 (.16)	.44 (.15)
Control	.62 (.15)	.62 (.15)	.62 (.14)	.62 (.15)	.63 (.15)	.69 (.15)	.61 (.17)	.67 (.16)
Grouped								
Repeated	.88 (.09)	.66 (.15)	.41 (.17)	.40 (.16)	.52 (.15)	.82 (.13)	.39 (.16)	.48 (.15)
Control	.70 (.15)	.75 (.14)	.64 (.14)	.62 (.15)	.66 (.14)	.75 (.15)	.67 (.14)	.63 (.16)

Table 7-3: Correct recall of critical elements under position-scoring in Experiment 6. (Calculated from weighted log-odds.)

The general pattern of facilitation and inhibition can be seen by collapsing repetition formats with the same repetition separation. To calculate delta, a repeated measure, the differences between recall of repeated elements and recall of control elements were averaged over subjects. The resulting delta values are shown in Figure 7-1, expressed as weighted, empirical log-odds. (A log-odds value of +1.0, for example, represents an increase of .20 in recall of repeated elements over recall of control elements, given recall of control elements of .60.) The upper panel shows delta for ungrouped lists; the lower panel shows delta for grouped lists. Even making a Bonferroni correction for eight pairwise comparisons, 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 the grouped condition, *Z(12)<1.17, p>.24*.

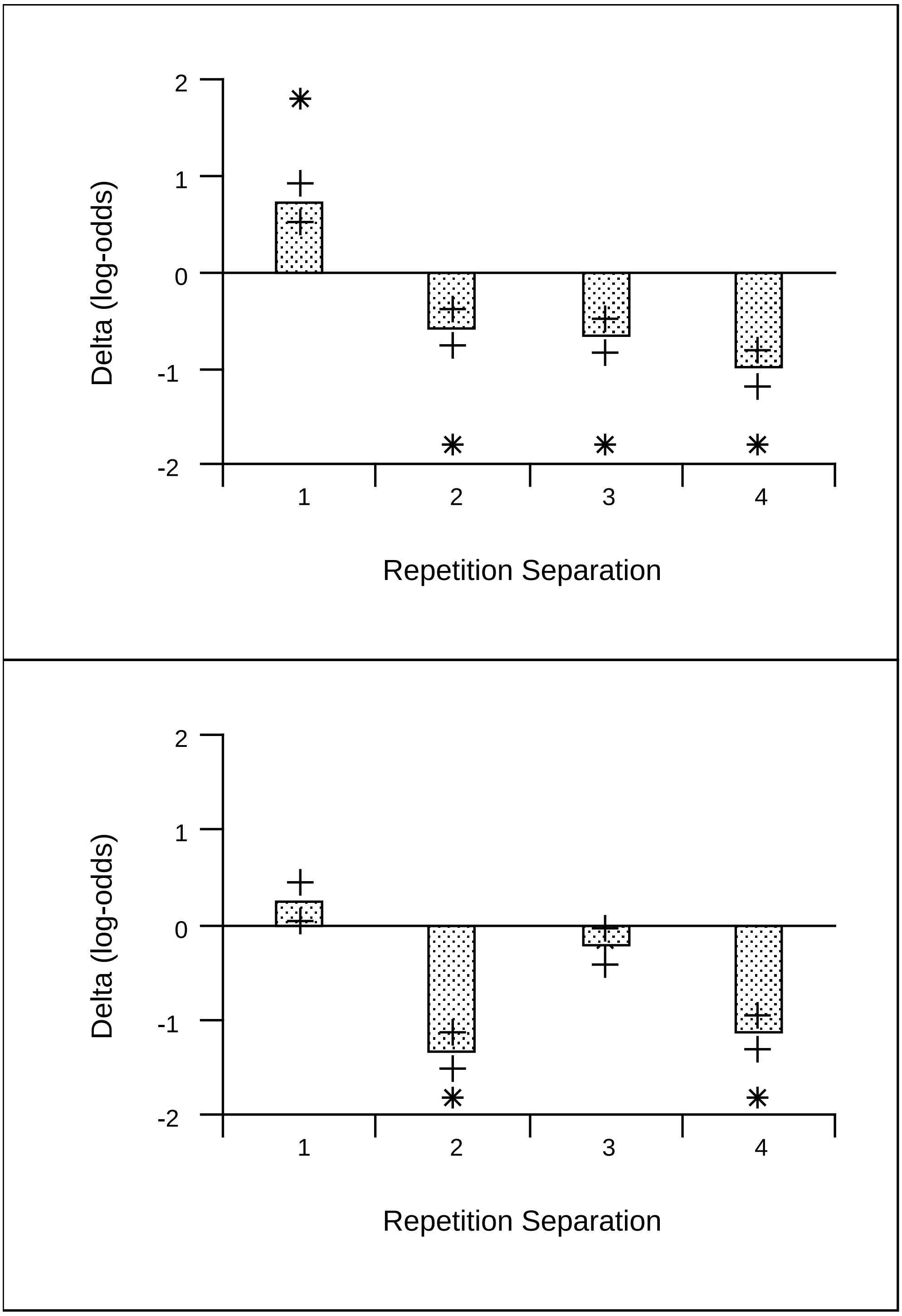

Figure 7-1: Delta under position-scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 6.
(Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

To examine the three-way interaction between list type, repetition format and grouping in more detail, delta values were tested via weighted log-odds for each repetition format in grouped and ungrouped conditions. Delta was significantly below zero for ungrouped repetition formats *1R34R6*, *R234R6* and *1R345R*, and grouped repetition formats *1R3R56*, *12R4R6*, *1R345R* and *R234R6*, $Z(12)>3.12$, $p<.005$. Delta was significantly above zero for grouped repetition format *1RR456*, $Z(12)=3.93$, $p<.0001$. No other delta values reached significance under Holm's correction for sixteen comparisons, $Z(12)<2.70$, *family-wise* $p>.05$.

Differences between weighted delta values were also tested across grouped and ungrouped conditions. Grouping had no significant effect on delta for repetition format *1RR456*, $Z(24)=1.21$, $p=.23$, but significantly decreased delta for repetition format *12RR56*, $Z(24)=3.03$, $p<.005$. Grouping also decreased delta for repetition formats *1R3R56*, *12R4R6* and *R234R6*, but not necessarily significantly, $Z(24)<2.44$, *family-wise* $p>.05$. In contrast, grouping increased delta for repetition format *12R45R*, such that delta changed sign, though again the change did not quite reach significance, $Z(24)=1.88$, $p=.06$. Repetition formats *1R34R6* and *1R345R* showed little change, $Z(24)<0.94$, $p>.35$.

One reason why some contrasts across grouped and ungrouped conditions did not quite reach significance may be because subjects in the ungrouped condition were spontaneously grouping lists in threes. Several subjects reported such grouping in debriefing. Spontaneous grouping may also explain why there was no significant repetition inhibition for repetition format *12R45R* in ungrouped lists, $Z(12)=1.14$, $p=.25$. This lack of repetition inhibition was not simply because the second repeated element in repetition format *12R45R* was the last item in the list (Crowder, 1968a), because highly significant repetition inhibition was found for repetition format *1R345R*, $Z(12)=4.28$, $p<.0001$.

In summary, the failure to find significant repetition facilitation for one-apart repeated elements in the grouped condition came from a reduction in delta when repeated elements straddled a group boundary (i.e., repetition format *12RR56*): Elements repeated immediately within a group (i.e., repetition format *1RR456*) continued to show repetition facilitation. The failure to find significant repetition inhibition for three-apart repeated elements in the grouped condition came 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, measuring how often critical elements were recalled in any two positions in a report. Delta under item-scoring is shown for each repetition separation in Figure 7-2. The most striking observation is that 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. Further tests of weighted, log-odds showed delta was significantly less than zero for all repetition formats, $Z(12)>2.58$, $p<.01$, except *1RR456*, both when grouped and when ungrouped, *12RR56* when ungrouped, and *12R45R* when grouped, $Z(12)<2.16$, *family-wise* $p>.05$. Thus repetition inhibition was clear in all cases 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 item recall of repeated elements, it must have arisen through better positioning of those elements. This was confirmed by analysing the conditional probability of recalling critical elements in the two critical positions, given that critical elements were recalled at two positions somewhere. The results under this scoring criterion are shown in Figure 7-3. There was clear repetition facilitation for adjacent repetition, but no repetition inhibition for any other repetition separation, except for two-apart repeated elements in the grouped condition. Indeed, separate analysis of each repetition format showed delta was significantly above zero for repetition format *1RR456*, both grouped and ungrouped, and repetition format *12RR56* when ungrouped, $Z(12)>3.42$, $p<.001$ in each case, but not significantly different from zero for any other format, $Z(12)<2.70$, *family-wise* $p>.05$.

Repetition Contamination

In general, context elements in repetition lists were recalled better when repeated elements were recalled better (i.e., under conditions of repetition facilitation). This was reflected in better recall of the lists as a whole. For example, approximately *61%* of ungrouped lists with repetition format *1RR456* were recalled correctly, compared with *52%* of control

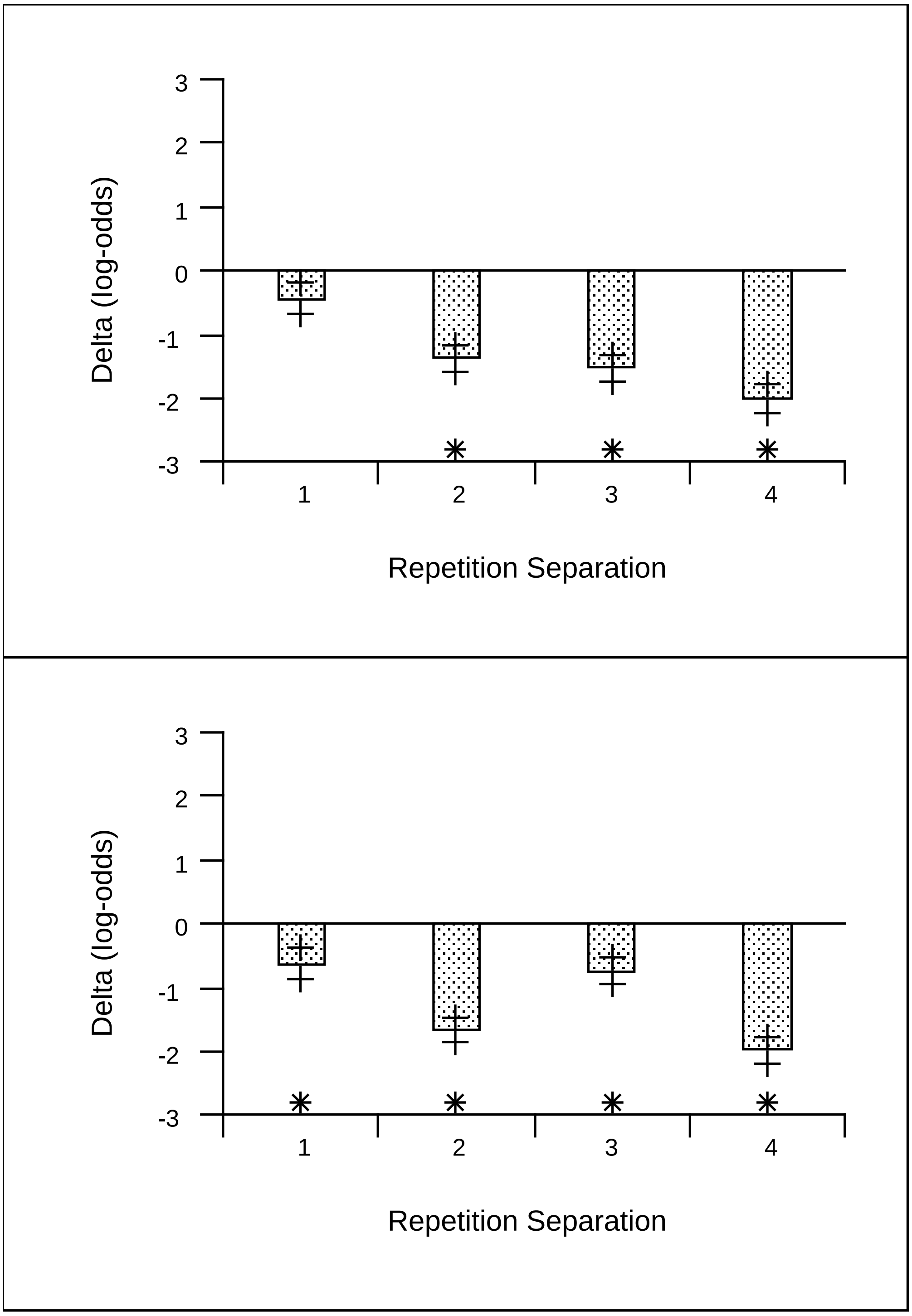

Figure 7-2: Delta under item-scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 6.
(Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

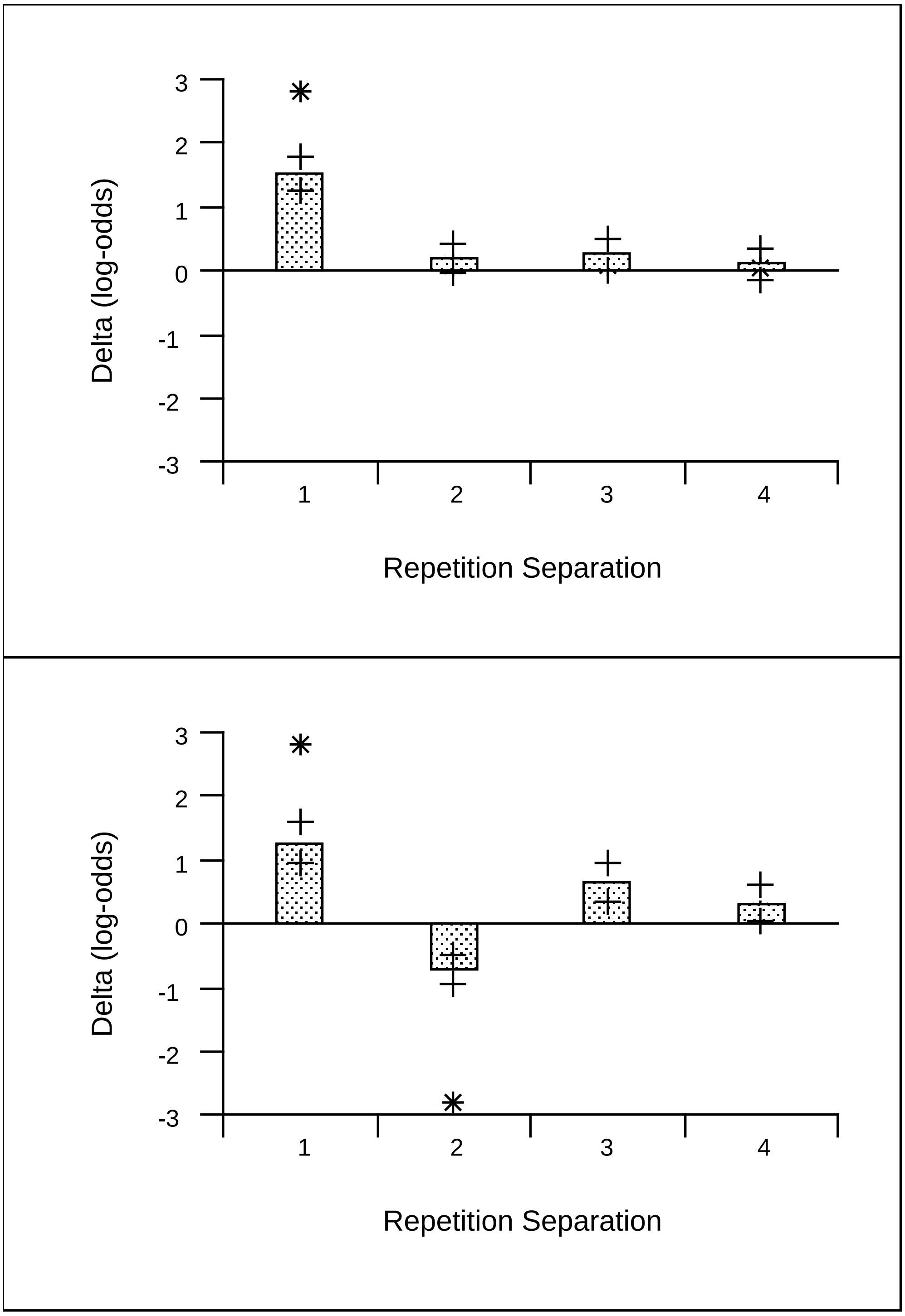

Figure 7-3: Delta under position-scoring, given correct item recall, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 6. (Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

lists. The converse was true for repetition formats that produced repetition inhibition. For example, only *35%* of ungrouped lists with format *1R345R* were recalled correctly. This mirrors previous findings (e.g., Crowder, 1968a).

A more detailed analysis of repetition contamination examined the errors immediately following correct critical elements, using modified control scoring and collapsing over repetition formats with at least one context element following each critical element (i.e., repetition formats *1R3R56*, *12R4R6*, *1R34R6*, and *R234R6*). The proportion of responses following correct critical elements that were errors and the proportion of such contamination errors that were associative intrusions (following Wickelgren, 1966) are shown in Table 7-4.

	Contamination Error	Associative Intrusion
Ungrouped		
Repeated	.18 (.05)	.36 (.18)
Control	.15 (.05)	.25 (.18)
Grouped		
Repeated	.17 (.05)	.24 (.16)
Control	.15 (.05)	.19 (.14)

Table 7-4: Proportion of responses following correct critical elements that were errors and the proportion of such contamination errors that were associative intrusions in Experiment 6. (Calculated from weighted log-odds.)

A two-way ANOVA on the log-odds of a contamination error, given correct recall of the preceding critical element, showed a significant effect of list-type $F(1,22)=4.58$, $p<.05$, reflecting a greater incidence of contamination errors following repeated elements, but no significant effect of grouping, or interaction, $F(1,22)<1.11$, $p>.30$. A two-way ANOVA on the log-odds that a contamination error was an associative intrusion showed a significant effect of grouping, $F(1,22)=6.06$, $p<.05$, reflecting a greater incidence of associative intrusions in ungrouped lists, but no significant effect of list-type, or interaction, $F(1,22)<1$.

Thus, though both ungrouped and grouped conditions showed a trend for a greater proportion of contamination errors to be associative intrusions, this trend did not quite reach significance, even under direct tests of weighted log-odds, $Z(12)<1.59$, $p>.11$. More importantly, there is an alternative reason for this trend. If subjects guessed a list item at random when unsure, the proportion of contamination errors that were associative intrusions expected by chance would be .25 in repetition lists (given the four possible erroneous items), and .20 in control lists (given the five possible erroneous items). In other words, there is a guessing bias for more associative intrusions following repeated elements than control elements. Consistent with this account, tests of weighted log-odds showed that the incidence of associative intrusions in repetition and control lists was not significantly greater than these chance levels in either condition, $Z(12)<1.02$, $p>.31$.

Nevertheless, a guessing account does not explain why the proportion of responses following correct repeated elements that were errors was greater than the proportion following correct control elements. However, this difference was accompanied by a lower frequency of a correct repeated elements ($M=.66$, $SD=.09$) than correct control elements ($M=.80$, $SD=.07$), a difference that was significant, $Z(12)=4.63$, $p<.001$. This reflected the fact that recall of repeated elements was worse than control elements for the subset of repetition formats used to examine repetition contamination (i.e. those with nonadjacent repeated elements, which showed repetition inhibition). The greater proportion of contamination errors in repetition lists may therefore reflect greater difficulty, on average, in recalling those lists. This caveat emphasises the care required of conditional analyses of errors (e.g., Bower & Springston, 1970; Healy, 1982).

Discussion

Using a single measure of performance and a position-scoring criterion, the present experiment showed both repetition facilitation and repetition inhibition simply by varying the positions of repeated elements. In the ungrouped condition, there was a transition from facilitation to inhibition 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 remove repetition inhibition for repeated elements at the ends of groups. This striking interaction between grouping and item repetition represents an important new finding.

The new method of scoring control lists confirmed that these effects arise over and above any potential bias in favour of repetition lists (owing to the inability of the experimenter to tell which response represents which repeated element). The results under this modified 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). In fact, when contrasted with conventional scoring, the modified scoring of control lists produced increases of up to 7% in the probability of recalling control elements. Though such differences are small in absolute terms, they resulted in a 50% increase in delta in some cases. Modified control scoring was therefore maintained in Experiments 7 and 8.

One might seek to explain the variability in delta as a function of repetition format by examining differences 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 7-3 are rarely greater than .80, and performance on control elements is quite constant, around .63 when ungrouped and .67 when grouped. Indeed, the largest difference in performance on control elements over all sixteen conditions is only .14. Even when the data are reanalysed using a relative measure of performance (Jahnke, 1969b), where delta is divided by the probability of recalling control elements, the pattern of results remains essentially unchanged. Thus, though the magnitude of delta is undoubtedly sensitive to overall performance levels, this is not sufficient to explain the effects of repetition separation and grouping.

Further investigation of repetition facilitation and repetition inhibition was made possible by considering different scoring criteria. Under item-scoring, 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 conditionalising 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 (Drewnowski, 1980a). The 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.

Regarding repetition contamination, the present results showed that recall of context elements generally correlated with recall of repeated elements. This is not surprising, given the interdependencies between responses in a report (Chapter 4): Better positioning of one item will necessarily improve positioning of other items, explaining why positioning of context elements benefits only under conditions of repetition facilitation (Crowder, 1968a). Conversely, context elements will be less well positioned under conditions of repetition inhibition, even though they may be more often recalled somewhere (Wickelgren, 1965c), given the smaller set of items to chose from in repetition lists than control lists.

A more specific measurement of repetition contamination showed a trend for a greater proportion of contamination errors to be associative intrusions in repetition lists than control lists, as predicted by the associative chaining theory of Wickelgren (1965c). This trend did not appear due to subjective grouping elicited by repeated elements, as suggested in Henson et al. (1996), because no significant interaction was found between this trend and the presence or absence of objective grouping. However, the trend did not reach significance in the present experiment. One reason for this may be a lack of statistical power, given the small numbers of associative intrusions and considerable variability across subjects. However, several points are worth noting in this respect.

Firstly, Wickelgren had even fewer data points in his 1966 experiment, using a sign test over subjects. Even then, there was only a significant incidence of associative intrusions in three of his eight conditions (only one of which would be significant under correction for multiple comparisons). Secondly, Wickelgren did not use the modified control scoring used in the present experiment. This marking scheme allows a greater number of control elements to be judged correct, which may increase the proportion of errors following correct control elements that are scored as associative intrusions. Though such a marking scheme may not be fair to theories assuming type representations of items (such as the chaining account of associative intrusions), it does suggest an alternative explanation of Wickelgren's results, by virtue of an underestimation of associative intrusions in control lists.

Thirdly, there is another possible explanation of Wickelgren's results, in terms of a guessing bias. Because there are a smaller number of items from which to chose a response in repetition lists than control lists, the baseline chance of an associative intrusion in repetition lists is higher than in control lists. Thus, if people occasionally recalled a critical element correctly, but forgot the item that followed it, they have a greater chance of guessing the item that followed the other critical element in repetition lists than control lists. Though such situations are probably rare, they may be sufficient to account for the small trend observed in the present experiment, and the significant trend found by Wickelgren (1966).

Finally, even if associative intrusions are more than a guessing bias, they still represent an extremely small proportion of errors (less than 2% of errors in the present experiment). Indeed, attempts to measure associative intrusions in Experiments 7 and 8 were thwarted by the scarcity of such errors. Thus, even if repeated items do represent ambiguous cues in a process of item-item chaining, this fact has an almost negligible effect on the recall of repetition lists, especially in relation to the effects of repetition facilitation and repetition inhibition on recall of context elements. Even if the alternative explanations of Wickelgren's finding prove incorrect, associative chaining models of serial recall would surely predict a much stronger effect of repetition on cuing (cf. the effect of similarity on cuing; Chapter 2).

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 shown to be 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 (Chapter 3), this sensitivity has important implications for both measurement and interpretation of repetition effects. In particular, the role of grouping in the detection of repeated items was examined in Experiment 7.

Repetition Memory

The previous experiment demonstrated a 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 is the probability of detecting the repetition event (i.e., the fact that an item was repeated). Lee (1976b), for example, showed that the probability that subjects detected 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, subjects only guessed around 40% had repeated items on debriefing. Thus one important contribution to the repetition effects in Experiment 6 may be the probability of detecting repetition: Detection may be necessary for repetition facilitation, while 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, while 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 repetition of an item? It is not simply because subjects do not expect repetitions: Repetition inhibition arises even when they are told in advance to expect repetition (Experiment 6), are reminded that repetition in lists is possible (Jahnke, 1969b), experience a high frequency of repetition (Hinrichs et al., 1973; Wickelgren, 1965c) or have considerable practice in recalling lists with repeated items (Crowder, 1968b). Repetition inhibition is not the result of repetition blindness either (e.g., Kanwisher, 1987), which is unlikely with the slow presentation rates of the above studies. Indeed, repetition blindness would predict greatest repetition inhibition for repeated elements closest together in time, in striking contrast to the repetition facilitation found for adjacent repeated elements in Experiment 6. Moreover, repetition blindness is often viewed as a perceptual problem, and yet repetition inhibition arises even when subjects vocalise repeated items as they are presented

(Experiment 6; note that one can correctly read aloud an item twice, without explicitly registering that there was repetition of that item). Thus repetition blindness is not sufficient to explain repetition inhibition.

The problem may not rest with detection either, because one still has to remember the repetition event at recall. In other words, it is possible to detect a repetition during presentation of a list, but forget that repetition during recall. Jahnke (1969b) tested this possibility by reminding subjects whether or not a repetition had occurred immediately before recall of each list. Though it reduced the amount of repetition inhibition, this reminding did not eliminate it. Thus failure to remember a repetition event during recall may contribute to repetition inhibition, but it is not a sufficient reason. A further requirement may be to remember not only that a repetition occurred, but which particular item was repeated. If correct positioning of the repeated elements is necessary, the requirement may be even more stringent: One may have to remember where the repetition occurred.

Even if memory for the repetition is an important factor in serial recall, questions remain as to how such memory might improve recall of repeated elements (repetition facilitation), or why a lack of such memory might impair recall of repeated elements (repetition inhibition). The latter question is dealt with in Experiment 8. As for the former question, several roles for detection and memory of a repetition have been suggested. One possibility is that the detection 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 has problems explaining why repetition effects are stronger when presentation rates are faster (Wickelgren, 1965c). It is also 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 overall memory load (Wickelgren, 1965c). 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. Instead, Lee proposed that detection of a repetition leads to 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 through increasing the probability that both occurrences of a repeated item are recalled. Lee used this fact to explain why the probability of recalling both occurrences was greater than that predicted if recall of each occurrence were statistically independent.

Lee (1976b) assumed further that repetition tags can become separated from a repeated item over time and become associated with a different item. This can produce repetition of the wrong item, consistent with Lee's finding of more such repetition errors in repetition lists than control lists. A proposal similar to Lee's repetition tags has been suggested for long-term memory as well. Following the model of Rumelhart and Norman (1982), Houghton et al. (1994) introduced a *geminate node* in their model of spelling, which is associated with a particular position in a word (rather than a particular letter, as with Lee's repetition tags). The job of the geminate node is to double the output of the letter at that position (and thus is used only for immediate repetition). The fact that this node can sometimes become triggered at an earlier or later position during output accounts for the common typographical errors where the wrong letter is repeated in adjacent positions (e.g, *school* typed as *schhol*, or *scholl*).

Repetition facilitation may also be related to the detection of distinctive items, another reason given for isolation effects (Hunt, 1995). Experiments that vary the acoustic similarity of critical and context elements (e.g., Jahnke & Melton, 1968; Lee, 1976a) have found *contrast facilitation* for critical elements that, though not repeated, are in contrast with context elements (e.g., two adjacent, phonologically nonconfusable items are recalled better when surrounded by confusable items than when surrounded by other nonconfusable items). Importantly, similarity between critical items does not, on its own, lead to facilitation (so repetition is not just an extreme case of phonological similarity). Nevertheless, repetition facilitation and contrast facilitation may have different underlying causes, because repetition facilitation remains over retention intervals where contrast facilitation has disappeared (Lee, 1976a) and does not appear to interact with phonological similarity of items (Drewnowski, 1980a). Contrast facilitation could certainly not arise through the mechanisms Houghton et al. (1994) proposed for immediate repetition for example.

In summary, there may be a role for a separate *repetition memory* in repetition facilitation and repetition inhibition. Detection and memory of a repeated item may improve its recall, through some form of repetition tagging for example. A failure to detect or remember the repetition may result in a failure to recall both repeated elements.

Experiment 2 in Henson (1996b) tested people's repetition memory as a function of repetition format and the presence or absence of grouping. By using a subset of the repetition formats in Experiment 6, it was possible to compare the results of this repetition memory task with the results of the serial recall task. In fact, exactly the same presentation conditions were employed in both experiments; the only difference was whether the task was to remember all elements (Experiment 6), or just the repeated elements (Experiment 2, Henson, 1996b). Two specific hypotheses of interest were (1) whether grouping improved repetition memory for repetition format *12R45R*, where repeated elements occurred at the end of groups, and (2) whether grouping impaired repetition memory for repetition format *12RR56*, where repeated elements straddled a group boundary. The former was confirmed, but the latter was not confirmed (in fact, if anything, grouping improved repetition memory for repeated elements straddling a group boundary). Thus, the fact that repeated elements were detected and remembered better when at the end of groups can explain the lack repetition inhibition for this condition in Experiment 6. However, the lack of repetition facilitation for repetition format *12RR56* when grouped in Experiment 6 seems inexplicable in terms of poorer detection or memory for the repetition. An alternative explanation was offered in terms of repetition tagging (Henson, 1996b); an account elaborated here in the General Discussion.

The other main finding of Experiment 2 in Henson (1996b) was that people were extremely good at remembering a repetition event (on over 95% of occasions for repetition formats *12RR56* and *12R4R6*). They were less accurate at remembering which item was repeated, but even then, they were correct over 75% of the time. This high level of repetition memory could be taken to question the role of repetition memory in serial recall. However, there are several reasons why dismissing such a role would be premature.

Firstly, repetition inhibition may still arise even with high levels of repetition memory. For example, if repetition inhibition arose by default whenever repetition memory failed, it would only take a 20% failure rate of repetition memory to cause a decrement of 12% in the probability of recalling repeated elements relative to control elements (if control elements were correct 60% of the time). This is comparable to the magnitude of repetition inhibition in Experiment 6. Secondly, the concurrent memory demand in Experiment 6, to remember all elements and their order, may have produced much lower levels of repetition memory than measured in Experiment 2 of Henson (1996b). These issues were addressed in Experiment 7.

Experiment 7

Experiment 7 differed from Experiment 2 in Henson (1996b) by measuring repetition memory on-line with serial recall. This tested repetition memory under a larger memory load than in the latter experiment; the additional memory load required for serial recall of all the elements in the list. Furthermore, by measuring repetition memory and serial recall of critical elements on a trial by trial basis, the two could be directly correlated. A strong correlation would support the hypothesis that repetition memory is an important determinant of repetition facilitation and inhibition. This would complement the indirect evidence for this hypothesis in Henson (1996b) and other studies (e.g., Lee, 1976b).

However, by attempting an on-line measurement of repetition memory, there are risks of contamination of one memory task (reporting the repeated item) by the other (recalling all items in order). Requiring subjects to indicate repeated items first may affect their subsequent serial recall. For example, Crowder (1968b) showed that a redundant response prefix produced “repetition inhibition” if the prefix item also occurred in the list to be recalled. Conversely, requiring serial recall before subjects indicate repeated items may allow them to base their decision about which item was repeated by simply inspecting or remembering what they had previously recalled. There seems to be no perfect solution to this dilemma.

Nevertheless, the approach taken in Experiment 7 was to ask subjects for written, serial recall, followed by a requirement to say aloud any item they thought was repeated. To minimise the risk of subjects performing the second task by inspecting their written reports, they were required to cover their responses before reporting any repeated items. The problem remained that what subjects report in the repetition memory task may still depend on what they recalled in the serial recall task. For example, a subject may repeat the wrong item in recall, and then “remember” the repetition of this item rather than the correct item. To minimise this risk, instructions for the serial recall task emphasised that subjects were only to write items they were certain about. Furthermore, a smaller subset of repetition formats was used than in Experiment 6, to increase overall performance under the dual task conditions.

In addition to investigating repetition memory, the present experiment was an important replication of Experiment 6, with a more powerful, within-subjects design. Three repetition formats were tested both ungrouped and grouped for each subject. Of particular interest was whether repetition inhibition would remain when subjects have specific reason to keep in mind a repeated item. Finally, the number of distractor digits was varied (either one or three), in order to test repetition memory and serial recall over different retention intervals.

Method

Subjects

Twelve subjects from the APU Subject Panel were tested; three were men, nine were women and their mean age was thirty-four years.

Materials

One hundred and twenty lists of six items were generated from the same set of letters as Experiment 6. This time however, there was an equal number of repetition lists and control lists, and only three repetition formats from Experiment 6 were employed (Table 7-5). Control and repetition lists were distributed equally over 4 blocks of 30 trials. The order of trials was pseudorandomised in the same manner as previous experiments.

Procedure

Each subject attempted four conditions generated from factorial combination of two levels of retention interval (a short delay of one distractor digit and a long delay of three distractor digits) and two levels of grouping (ungrouped and grouped). The order of grouping conditions was constrained such that subjects always attempted the two ungrouped conditions

List Type	Repetition Format	Repetition Separation	No. of Lists
Control	1 2 3 4 5 6	-	60
Repetition	1 2 R R 5 6	1	20
Repetition	1 2 R 4 R 6	2	20
Repetition	1 2 R 4 5 R	3	20

Table 7-5: Composition of lists in Experiment 7.

before the two grouped conditions, to reduce the incidence of spontaneous grouping (Experiment 2). The order of short or long retention intervals was counter-balanced within this constraint, as was the order blocks. The remaining procedure was similar to that of Experiment 6, except for three important differences. Firstly, subjects wrote rather than spoke their responses. Secondly, subjects were instructed not to guess, being told:

> "Most importantly, please do NOT guess at letters. In other words, only write a letter in a particular box if you are SURE that it occurred in that position. Otherwise, put a line through the box, before going on to try to recall the next one. It is better to indicate a blank than to respond with a letter which you are unsure about."

(Approximately 11% of responses were omissions, considerably greater than the figure of 4% in Experiment 6, suggesting that subjects did obey this instruction.) Thirdly, when subjects had finished the serial recall task, they were asked to cover their responses with a piece of card, before saying aloud any letter that they remember as being repeated in the list. They were told to report a repeated item even if they had not managed to recall that item correctly. If they did not remember any items as being repeated, they were told to say *none*. They were informed that half of the lists did contain a repeated item. The experiment took 50 minutes.

Results

In brief, results of the serial recall task were similar to those of Experiment 6, except that significant repetition facilitation was found for repeated elements at the end of groups. As expected, the concurrent memory demands of the serial recall task lowered performance on the repetition memory task relative to Experiment 2 in Henson (1996b). Most importantly, performance on the two tasks was highly correlated, consistent with the hypothesis that repetition memory is an important determinant of item repetition effects in serial recall.

Position-scoring of Serial Recall

As in Experiment 6, the probability of recalling either critical element on the two critical positions was calculated for both repetition and control lists (Table 7-6). Unlike Experiment 6 however, the equal number of control and repetition lists meant that each control list could be paired randomly with one of the three repetition formats (removing the risk of correlations between recall of more than two critical elements from the same control list). A four-way ANOVA on log-odds showed a significant effect of repetition format, $F(2,255)=18.43$, $p<.001$, grouping, $F(1,255)=72.57$, $p<.001$ and retention interval, $F(1,255)=23.20$, $p<.001$. Unlike Experiment 6, there was no significant effect of list type, $F(1,255)=2.46$, $p=.12$, but this factor did interact with repetition format, $F(2,255)=18.43$, $p<.001$, and retention interval, $F(1,255)=12.18$, $p<.001$. There were no further significant interactions. The lack of a main effect of list type suggested that repetition facilitation and repetition inhibition for the three different formats cancelled out overall. The interaction of list type with retention interval reflected a greater decrement in recall of control elements than repeated elements as retention interval was increased.

Collapsing across short and long retention intervals, Figure 7-4 shows delta against repetition format. Tests of weighted log-odds showed delta was only significantly different from zero for repetition format *12RR56* when ungrouped and repetition format *12R45R* when grouped, $Z(12)>2.63$, $p<.01$ in both cases; $Z(12)<1.88$, $p>.06$ for all other repetition formats. Thus, there was not only significant repetition facilitation for adjacent repeated elements when ungrouped, as in Experiment 6, but also for repeated elements at the end of groups, a nonsignificant trend in Experiment 6.

Item-scoring of Serial Recall

Though repetition inhibition was not reliable under position-scoring, it was clearly present under item-scoring. Figure 7-5 shows delta against repetition format, again collapsing across retention interval. Delta was significantly below zero for repetition format *12R4R6* in both ungrouped and grouped conditions, $Z(12)>2.65$, $p<.01$. Surprisingly, delta for grouped

Condition	Short 12RR56	Short 12R4R6	Short 12R45R	Long 12RR56	Long 12R4R6	Long 12R45R
Ungrouped						
Repeated	.68 (.21)	.39 (.22)	.53 (.25)	.59 (.23)	.35 (.22)	.66 (.24)
Control	.59 (.23)	.52 (.24)	.58 (.24)	.42 (.24)	.37 (.24)	.46 (.24)
Grouped						
Repeated	.77 (.18)	.60 (.23)	.86 (.14)	.70 (.21)	.51 (.24)	.83 (.15)
Control	.76 (.19)	.78 (.19)	.80 (.18)	.59 (.23)	.51 (.24)	.67 (.22)

Table 7-6: Correct recall of critical elements under position-scoring in Experiment 7. (Calculated from weighted log-odds.)

repetition format *12R45R* was still positive under item-scoring, thought this did not quite reach significance, *Z(12)=2.29, family-wise p>.05*. No other delta values were significant, *Z(12)<1.1, p>.27*, in all cases. Weighted tests of delta across ungrouped and grouped conditions showed that grouping significantly increased delta for repetition format *12R45R*, *Z(12)=2.30, p<.05*, but not for the other two repetition formats, *Z(12)<0.53, p>.60*.

Item-scoring of Repetition Memory

A three-way ANOVA on log-odds of memory for the repeated item showed significant effects of repetition format, *F(2,121)=12.85, p<.001*, and grouping, *F(1,121)=14.54, p<.001*, and a significant interaction between them, *F(2,121)=3.71, p<.05*. There was no significant effect of retention interval, nor any other significant interaction. Collapsing across retention interval, the accuracy of repetition memory is shown in Table 7-7. Performance was below the near-ceiling levels of Experiment 2 in Henson (1996b), probably explaining why the interaction between repetition format and grouping reached significance. This interaction arose from grouping producing a significant improvement for repetition format *12R45R*,

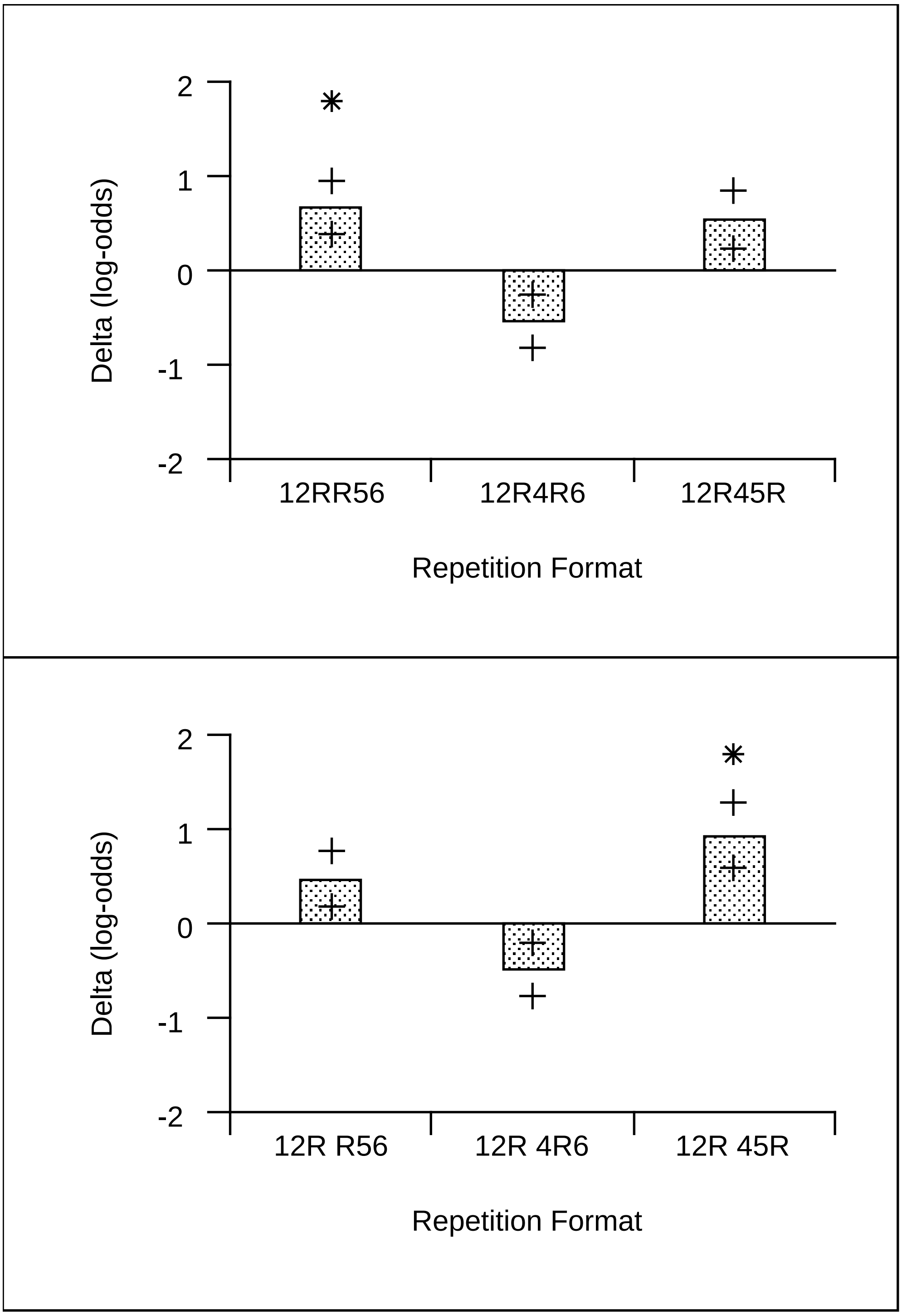

Figure 7-4: Delta under position-scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 7.
(Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

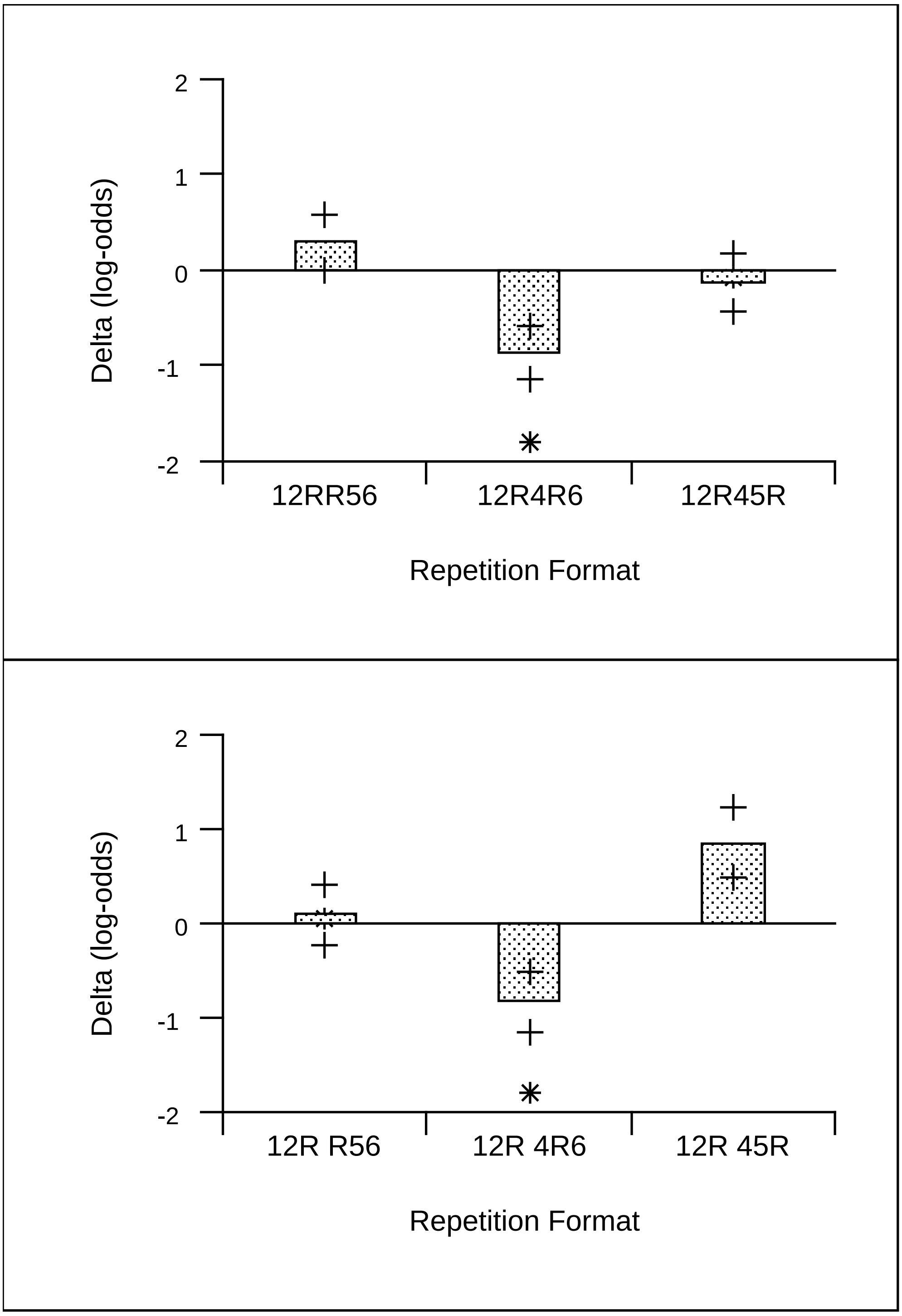

Figure 7-5: Delta under item-scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 7.
(Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

Z(12)=2.00, p<.05 (and perhaps repetition format *12R4R6*, *Z(12)=1.73*, *p=.08*), but having little affect on repetition format *12RR56*, *Z(12)=0.21*, *p=.83*, in agreement with the results of Experiment 2 in Henson (1996b). Also in agreement with that experiment, repetition of a different item was the most common error in the repetition memory task for ungrouped repetition format *12RR56*. The other repetition formats produced approximately equal numbers of incorrect repetitions and failures to detect any repetition.

	Repetition Format		
Condition	12RR56	12R4R6	12R45R
Ungrouped	.75 (.15)	.57 (.16)	.71 (.18)
Grouped	.75 (.15)	.66 (.16)	.80 (.14)

Table 7-7: Correct repetition memory under item-scoring in the Experiment 7. (Calculated from weighted log-odds.)

Correlation between Repetition Memory and Serial Recall

Collapsing across repetition formats, contingency tables for performance on the repetition memory task and the serial recall task (under item-scoring) were determined for each subject. A combined test of significance of these tables showed an extremely high correlation between recall of repeated items and memory for their repetition, *Z(12)=20.30, p<.0001*. Table 7-8 shows the contingency table summed across subjects. Unfortunately, the correlation was so high that correct recall in the absence of correct repetition memory and correct repetition memory in the absence of correct recall were so rare that the direction of causality remained unclear. In other words, there was no direct way of telling whether recall depended on repetition memory, repetition memory depended on recall, or both factors influenced each other. If, for example, performance on the repetition memory task depended on serial recall of the whole list (overtly in the present experiment; covertly in Experiment 2 of Henson, 1996b), then there would be no need to postulate a separate repetition memory.

Indirect support for a separate repetition memory came from the observation that an item incorrectly repeated in serial recall was often the same item that was repeated on the

	Repetition Memory	
	Correct	Incorrect
Serial Recall		
Correct	521	17
Incorrect	25	157

Table 7-8: Number correct and incorrect under item-scoring in the repetition memory and serial recall tasks of Experiment 7.

previous trial (and yet the same item was never repeated in the lists of two successive trials). This might be expected if a separate repetition memory were prone to proactive interference, but would not be expected if repetition memory were based solely on serial recall of a list.

Finally, a considerable number of the 22% of occasions where incorrect serial recall was accompanied by incorrect repetition memory were cases where subjects recalled the wrong item twice, and reported remembering that item as being repeated. Prima facie, this would support the hypothesis that repetition memory was based on what was recalled in the serial recall task. However, such repetition errors in serial recall could equally well arise from a failure of repetition memory. Furthermore, though there may well be contamination of repetition memory from prior serial recall in the present experiment, this is not evidence against the existence of a separate repetition memory (which may be updated during recall).

Discussion

The present experiment was an important, within-subjects replication of the repetition effects found in Experiment 6. The pattern of delta values was similar, except for a general increase in delta across all conditions. There were probably two reasons for this increase in delta: greater attention towards repetition, as required by the concurrent repetition memory task, and the smaller set of repetition formats used. Nevertheless, the robustness of repetition inhibition was confirmed by the fact that it was still found for two-apart repeated elements in a situation where subjects were required to explicitly detect and remember repetition of items.

The present experiment was also an important extension of the results of Experiment 2 in Henson (1996b), confirming that grouping increases repetition memory for repeated elements at the end of groups, but not for repeated elements that straddle a group boundary. By pulling performance off ceiling levels, the present experiment also showed that increasing repetition separation can impair repetition memory, at least across repetition formats *12RR56* and *12R4R6*. The fact that repetition format *12R45R* showed high levels of repetition memory in the ungrouped as well as grouped condition probably reflected some spontaneous grouping in the ungrouped condition (Chapter 3).

In addition to corroborating the previous experiments, the present experiment went further to correlate serial recall and repetition memory on a trial-by-trial basis. The extremely high correlation leaves no doubt that the two are interdependent. The direction of this dependency is somewhat unclear, though indirect evidence suggested that a separate repetition memory does influence serial recall. One reason was that the same item was often repeated in two consecutive trials, which would be expected if a separate repetition memory were prone to proactive interference, but not otherwise. Another reason was that the serial recall task showed a significant effect of retention interval, whereas the repetition memory task did not. This might be expected if a separate repetition memory (an item memory) were longer lasting than the memory underlying serial recall (a serial memory). Taken together, these facts support the hypothesis that repetition memory plays an important role in repetition facilitation and repetition inhibition.

The most surprising difference between the repetition effects in the present experiment and those in Experiment 6 concerned repetition format *12R45R* when grouped. Under item-scoring, this condition showed a delta value close to zero in Experiment 6, and yet showed a delta value considerably greater than zero in the present experiment. One reason could be the increased attention to repetition in the present experiment, as discussed above. However, an alternative account was offered in Henson (1996b) in terms of *repetition schema* (i.e., memory for the structure of repetition in a list); an account elaborated here in the General Discussion.

In summary, the present experiment reinforced the effects of repetition separation and grouping on repetition memory and serial recall. It also provided reasonably good support, given the problems of measuring repetition memory on-line, for the role of a separate repetition memory in serial recall. That role may be to ensure that an item is recalled twice, preventing repetition inhibition. In addition, special types of repetition memory, such as repetition tagging and repetition schemata (Henson, 1996b) may further cause repetition facilitation. Repetition facilitation at the end of groups in particular may be attributable to the ease of abstracting a repetition schema for repeated elements in these salient positions.

Repetition Inhibition

The previous experiment suggested that failure to remember which item was repeated may result in repetition inhibition. Anything that affects the probability of detecting and remembering repetition, such as repetition separation and grouping, will therefore affect the magnitude of repetition inhibition. However, the question remains as to why failure to remember the repetition of an item should lead repetition inhibition in the first place. There was no doubt that subjects in the previous experiments could vocalise 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 remarked, repetition inhibition "...is, at least in part, a result of interference arising from the act of sequential recall..." (Jahnke, 1969a, p. 620). Jahnke supported this claim with data suggesting that repetition inhibition was stronger for the second repeated element to be recalled, rather than the second repeated element presented, whether that was in forward or backward recall. Output interference also explains why repetition inhibition is absent when recall of both repeated elements is unnecessary, such as in probe recognition (Wolf & Jahnke, 1968) or probed recall (Jahnke, 1970). If output interference were an automatic 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 subjects are well aware that repetition of responses is necessary (Crowder, 1968a; 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 subjects when their memory has failed. If people have a default reluctance to repeat themselves, they would be biassed against guessing a repeated item. In other words, they would be more likely to recall a control element correctly from a lucky guess than a repeated element. 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 due 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.

Firstly, Greene (1991, Experiment 1) showed that repetition inhibition disappeared when subjects were instructed not to guess, by virtue of poorer recall of control elements, but not repeated elements. This is exactly the pattern predicted by the guessing hypothesis. Secondly, when the vocabulary size is increased, repetition inhibition is reduced (e.g., Hinrichs et al., 1973). Again, this reduction comes from poorer recall of control elements; performance on repeated elements remains unaffected (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 subjects had to report the missing item in a modified Cloze task. This was confirmed by Greene (1991, Experiments 2 and 3), who found that repetition inhibition could occur in a partial report task, where recall of only one repeated element was required. Importantly, this repetition inhibition was contingent on the remaining items being displayed during recall, to bias guesses against these items.

Jahnke (1972b) suggested that another important factor in repetition inhibition is proactive interference. He showed that when the experimental vocabulary was large enough, such that an item never occurred in more than one trial, there was no repetition inhibition. In other words, it appeared that repetition inhibition depended on intertrial repetition as well as intratrial repetition. Jahnke also showed that, when there was intertrial repetition, repetition inhibition was normally absent on the first trial, and increased over subsequent trials, again suggesting a role for proactive interference. However, the role of proactive interference is far from clear. Walsh and Schwartz (1977) showed that repetition inhibition was unaffected by a category shift across trials, unlike proactive interference. More importantly, the pattern of results of Jahnke (1972b) can also be explained by the guessing hypothesis. The larger vocabulary needed to prevent intertrial repetition will necessarily reduce the probability of guessing control elements, and hence reduce repetition inhibition. With a small vocabulary, the build up of repetition inhibition over trials can attributed to subjects 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 subjects are still reluctant to guess an item they have already recalled when they are well aware that lists can contain repeats (e.g., Jahnke, 1969b). Though Mewaldt and Hinrichs (1977; Hinrichs & Mewaldt, 1977) showed that repetition inhibition was reduced when subjects experienced greater frequencies of repetition, it was clearly not eliminated. A second puzzle is that, in direct contradiction to the guessing hypothesis, subjects in the Walsh and Schwartz (1977) study reported no conscious avoidance of guessing repeated items during debriefing. 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 omissions, rather than the substitutions predicted by guessing accounts. Jahnke (1974) also failed to find a correlation between the number of errors made by each subject and the magnitude of their repetition inhibition effect. If most errors were guesses (as would be expected from Experiments 4 and 5), a guessing bias would predict a strong correlation, with more errors resulting in a greater magnitude of repetition inhibition.

One final puzzle concerning the guessing hypothesis is that Walsh and Schwartz (1977), unlike Greene (1991), failed to find a significant effect of guessing instructions on repetition inhibition. Greene argued that Walsh and Schwartz used large vocabularies, which tend to reduce the magnitude of repetition inhibition (Hinrichs et al., 1973), and hence would have reduced the probability of Walsh and Schwartz observing 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 used a very small vocabulary and not only found 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 exactly 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 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 repeated elements (the output interference hypothesis), or prevents guessing of repeated elements already forgotten (the guessing hypothesis).

Experiment 8

The aim of Experiment 8 was to test the output interference and guessing hypotheses. Rather than instructing subjects 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 subjects may not always be certain of what constitutes a guess (Chapter 6), they were further asked to indicate responses that they were simply not sure about. Both these confidence ratings (guesses and uncertain responses) were measured on-line during recall, through subjects moving up and down an array of response boxes in the same manner as in Experiments 4 and 5. A bias towards control elements in uncertain responses would further support the guessing hypothesis. However, if significant repetition inhibition remained even when both guesses and uncertain responses were removed from analysis, then there would also be support for the output interference hypothesis.

Method

Subjects

Twelve subjects from the APU Subject Panel were tested; three were men, nine were women and their mean age was thirty years.

Materials

The same materials were used as in Experiment 7.

Procedure

The procedure was similar to that of Experiment 7, except for the following differences. Firstly, subjects were only tested on serial recall of lists; there was no additional requirement to remember which item was repeated. Secondly, subjects were not instructed to avoid guessing, but rather could indicate three levels of confidence for each response: *confident*, *unsure* and *guess*. The confidence of a particular response was indicated by where subjects wrote it 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. Subjects 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 approximately 50 minutes.

Results

In brief, position-scoring of all responses replicated the results of Experiment 6, except that significant repetition facilitation was again found for repeated elements at the end of groups, as in Experiment 7. Item-scoring revealed exactly the same pattern of repetition inhibition as in Experiment 6, whether or not guesses were included in the analysis. Further exclusion of uncertain responses removed repetition inhibition for some conditions, but significant repetition inhibition 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, the probability of recalling either critical element on the two critical positions was calculated for both repetition and control lists. A four-way ANOVA on log-odds showed a significant effect of repetition format, $F(2,255)=6.36$, $p<.005$, grouping, $F(1,255)=23.38$, $p<.001$, and retention interval, $F(1,255)=7.82$, $p<.01$. Like Experiment 7, there was no significant effect of list type, $F(1,255)<1$, but this factor did interact with repetition format, *F(1,255)=13.20, p<.001*. There were no further significant interactions. Collapsing across retention interval, these probabilities are shown in Table 7-9.

	Repetition Format		
Condition	12RR56	12R4R6	12R45R
Ungrouped			
Repeated	.65 (.16)	.36 (.17)	.49 (.19)
Control	.41 (.19)	.50 (.18)	.39 (.18)
Grouped			
Repeated	.73 (.16)	.55 (.18)	.94 (.06)
Control	.74 (.15)	.79 (.14)	.77 (.15)

Table 7-9: Correct recall of critical elements under position-scoring in Experiment 8. (Calculated from weighted log-odds.)

Figure 7-6 shows delta against repetition format. Tests of weighted log-odds showed delta was significantly different from zero in all cases, *Z(12)>2.70, p<.01*, except for one-apart repeated elements in grouped lists, and three-apart repeated elements in ungrouped lists, *Z(12)<1.73, family-wise p>.05*. Tests of weighted delta values showed that grouping significantly reduced delta for one-apart repeated elements, *Z(12)=2.80, p<.01*. Grouping did not significantly reduce delta for two-apart repeated elements, *Z(12)=1.19, p=.23*, or significantly increase delta for three-apart repeated elements, *Z(12)=1.72, p=.09*, though both these trends were in exactly the same direction as the nonsignificant trends in Experiment 6 and Experiment 7. The fact that three experiments show these trends seems to warrant the general conclusion that grouping impairs recall of repeated elements at different positions within groups and improves recall of repeated elements at the end of groups.

These results replicated those of Experiments 6 and 7, even when subjects were forced to guess. Thus repetition facilitation and repetition inhibition were robust to increased levels of guessing. However, the main purpose of the present experiment was to see if these effects,

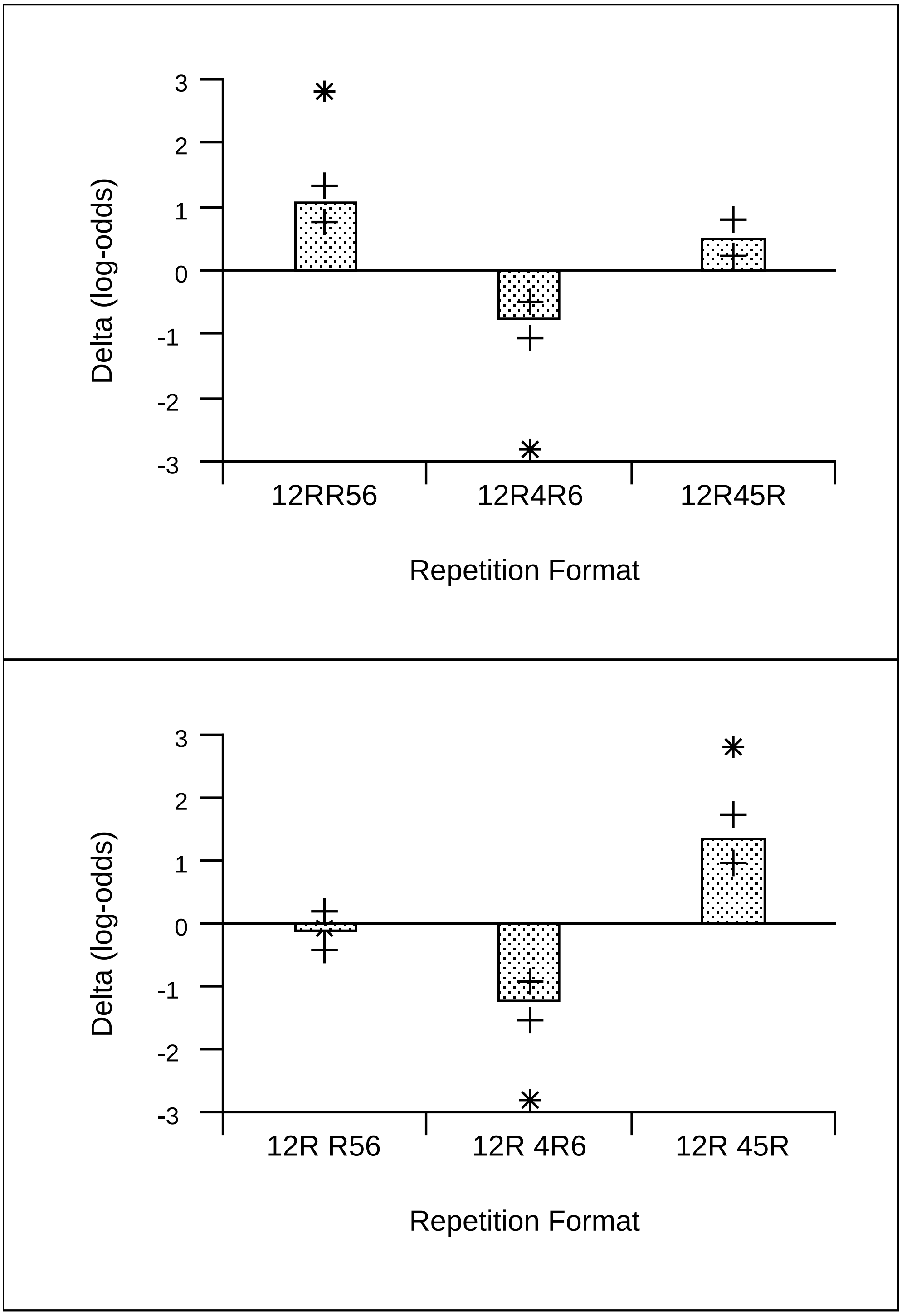

Figure 7-6: Delta under position-scoring as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 8.
(Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

particularly repetition inhibition, were robust to decreased levels of guessing. This possibility was tested under item-scoring, by successively removing less confident responses.

Uncertain Responses

Over all subjects, approximately 9% of responses were indicated as guesses, and an additional 24% were indicated as unsure. Interestingly, these percentages were identical for both repetition and control lists. In all subsequent discussion, the term *uncertain responses* will refer to the 33% of responses that subjects either guessed or were unsure about.

A three-way ANOVA on the log-odds of an uncertain response showed significant effects of grouping, $F(1,253)=121.42,\ p<.001$, retention interval, $F(1,253)=134.34,\ p<.001$, and output position, $F(5,253)=27.84,\ p<.001$. Grouping interacted significantly with output position, $F(5,253)=3.67,\ p<.005$, but none of the other interactions was significant. The longer retention interval produced a higher frequency of uncertain responses, as would be expected. Generally, uncertain responses increased towards the end of recall, though this pattern was modified by grouping, which reduced uncertain responses more for the second group than the first, and tended to equate the certainty of responses within groups (Chapter 6).

Item-scoring of Serial Recall

To investigate the impact of uncertain responses on repetition inhibition, a four-way ANOVA was conducted on the log-odds of recalling two critical elements anywhere. Collapsing across retention interval, the four factors were list type, repetition format, grouping and response certainty (Table 7-10). The three levels of response certainty were either to include all responses, to include all responses except guesses, or to include all responses except uncertain responses. There were significant effects of list type, $F(1,389)=84.38,\ p<.001$, repetition format, $F(2,389)=19.32,\ p<.001$, grouping, $F(1,389)=203.74,\ p<.001$, and response certainty, $F(2,389)=152.14,\ p<.001$. As expected, repetition format interacted with both list type $F(2,389)=21.67,\ p<.001$, and grouping, $F(2,389)=16.05,\ p<.001$. Interestingly, response certainty also interacted with both list type, $F(2,389)=9.95,\ p<.001$, and grouping, $F(2,389)=4.71,\ p<.01$. Two three-way interactions were significant, that between list type, grouping and repetition format, $F(2,389)=15.68,\ p<.001$ and that between list type, grouping and response certainty, $F(2,389)=6.07,\ p<.005$. No other interactions approached significance.

	Responses Analysed								
	All Responses			Without Guesses			Without Uncertain Responses		
Condition	12RR56	12R4R6	12R45R	12RR56	12R4R6	12R45R	12RR56	12R4R6	12R45R
Ungrouped									
Repeated	.72 (.15)	.51 (.16)	.59 (.19)	.71 (.15)	.46 (.16)	.53 (.19)	.58 (.16)	.25 (.13)	.34 (.18)
Control	.82 (.13)	.88 (.10)	.83 (.12)	.68 (.18)	.72 (.16)	.69 (.16)	.33 (.16)	.31 (.17)	.28 (.16)
Grouped									
Repeated	.78 (.14)	.62 (.18)	.94 (.06)	.75 (.15)	.60 (.17)	.89 (.09)	.59 (.18)	.44 (.18)	.74 (.17)
Control	.93 (.07)	.91 (.08)	.91 (.08)	.90 (.09)	.83 (.12)	.85 (.12)	.65 (.20)	.67 (.17)	.62 (.18)

Table 7-10: Correct recall of critical elements under item-scoring in Experiment 8. (Calculated from weighted log-odds.)

The interaction between response certainty and list type reflected a greater reduction in the probability of recalling control elements than repeated elements when guesses and uncertain responses were removed. This is consistent with the guessing hypothesis of Hinrichs et al. (1973). The interaction between response certainty and grouping reflected a greater reduction in the probability of recalling critical elements when guesses and uncertain responses were removed from ungrouped lists than from grouped lists. This is expected if grouping not only improves performance, but also increases confidence levels. The interaction between response certainty, grouping and list type reflected a greater interaction between response certainty and list type in ungrouped lists than grouped lists.

Delta for all responses is shown in Figure 7-7, delta without guesses is shown in Figure 7-8, and delta without uncertain responses is shown in Figure 7-9. With all the responses analysed, the pattern of delta values was identical to that in Experiment 6. Significant repetition inhibition was found for all repetition formats, $Z(12)>3.63$, $p<.001$,

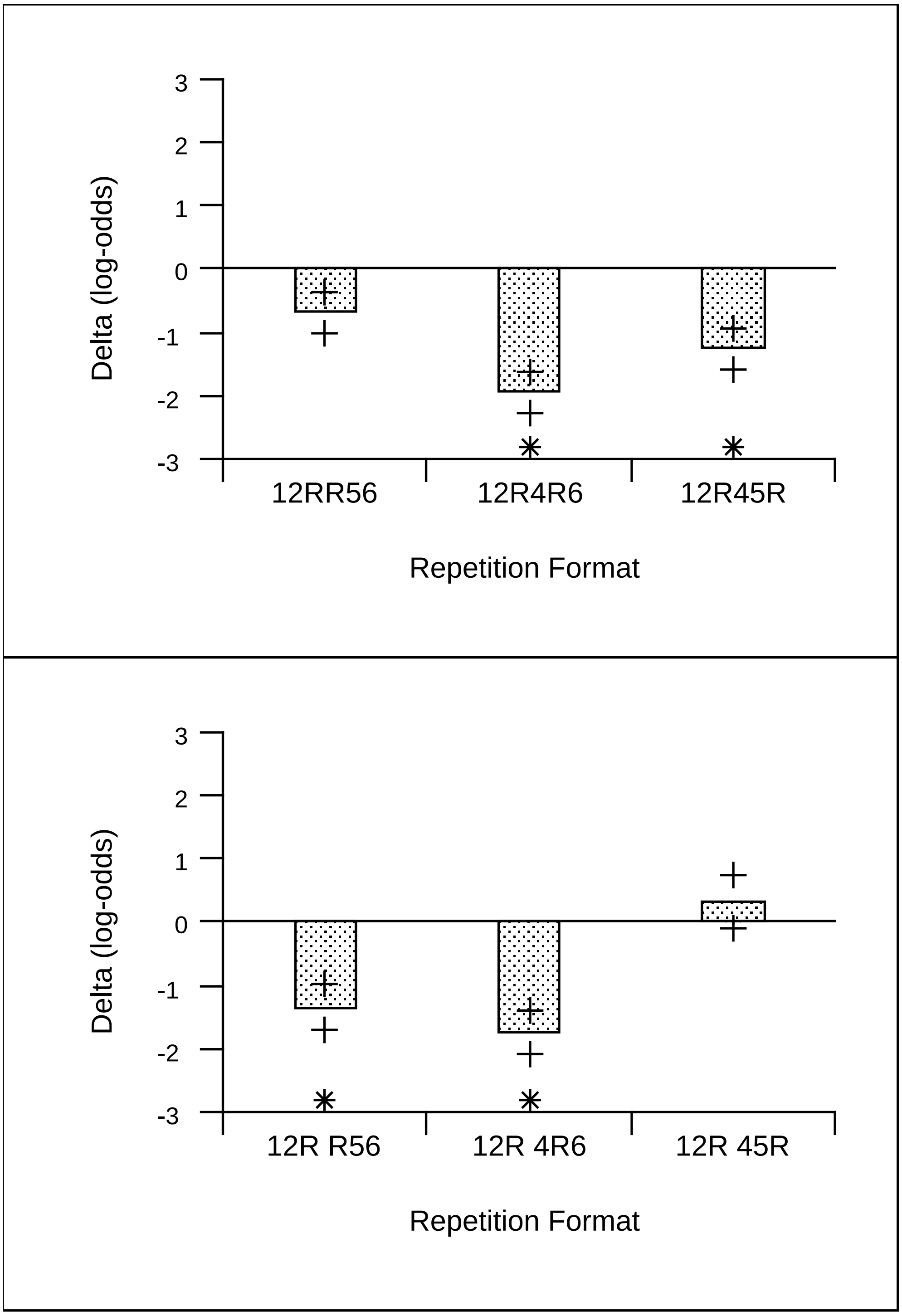

Figure 7-7: Delta under item-scoring, including all responses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 8. (Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

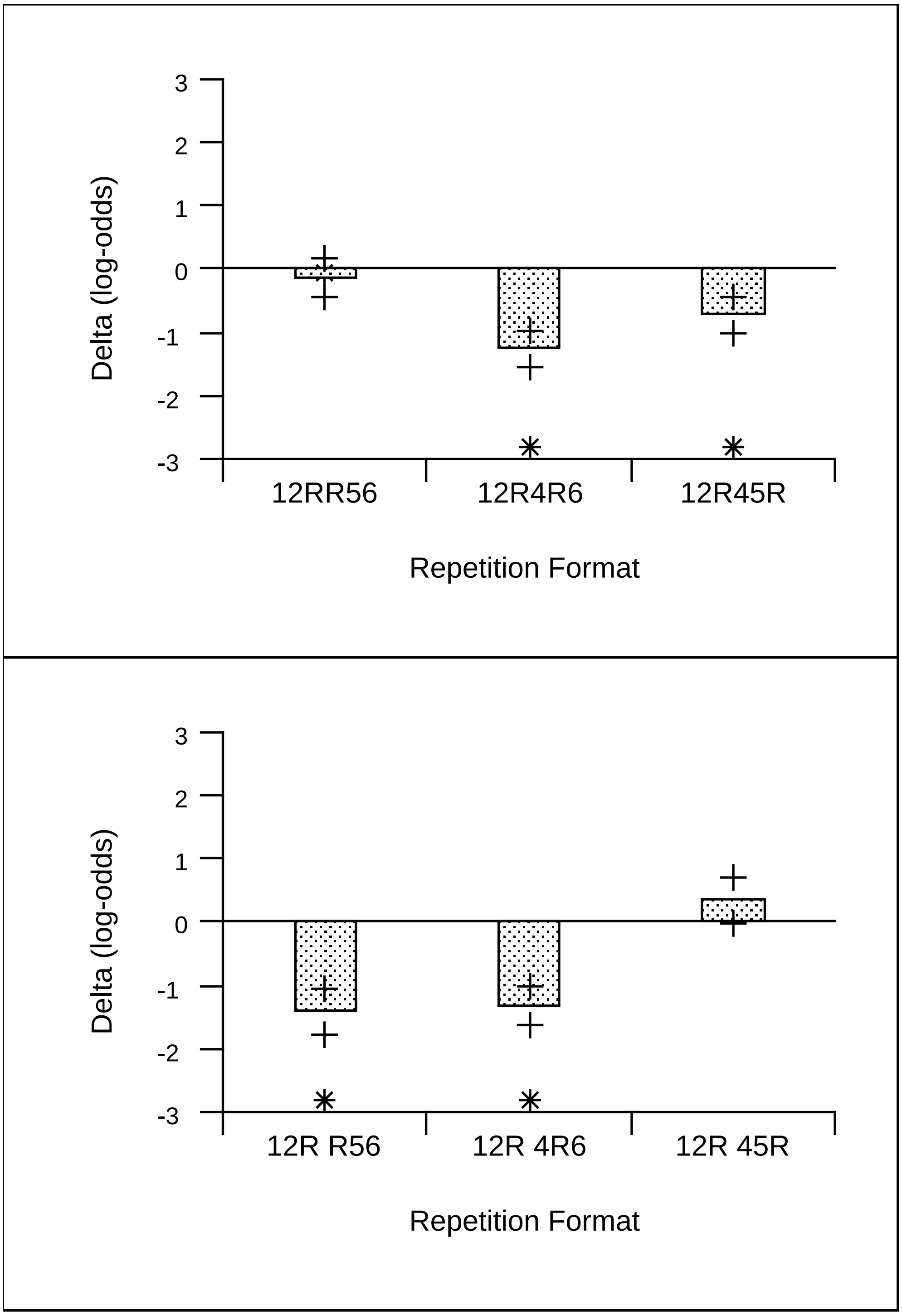

Figure 7-8: Delta under item-scoring, excluding guesses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 8. (Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm’s method of multiple comparisons.)

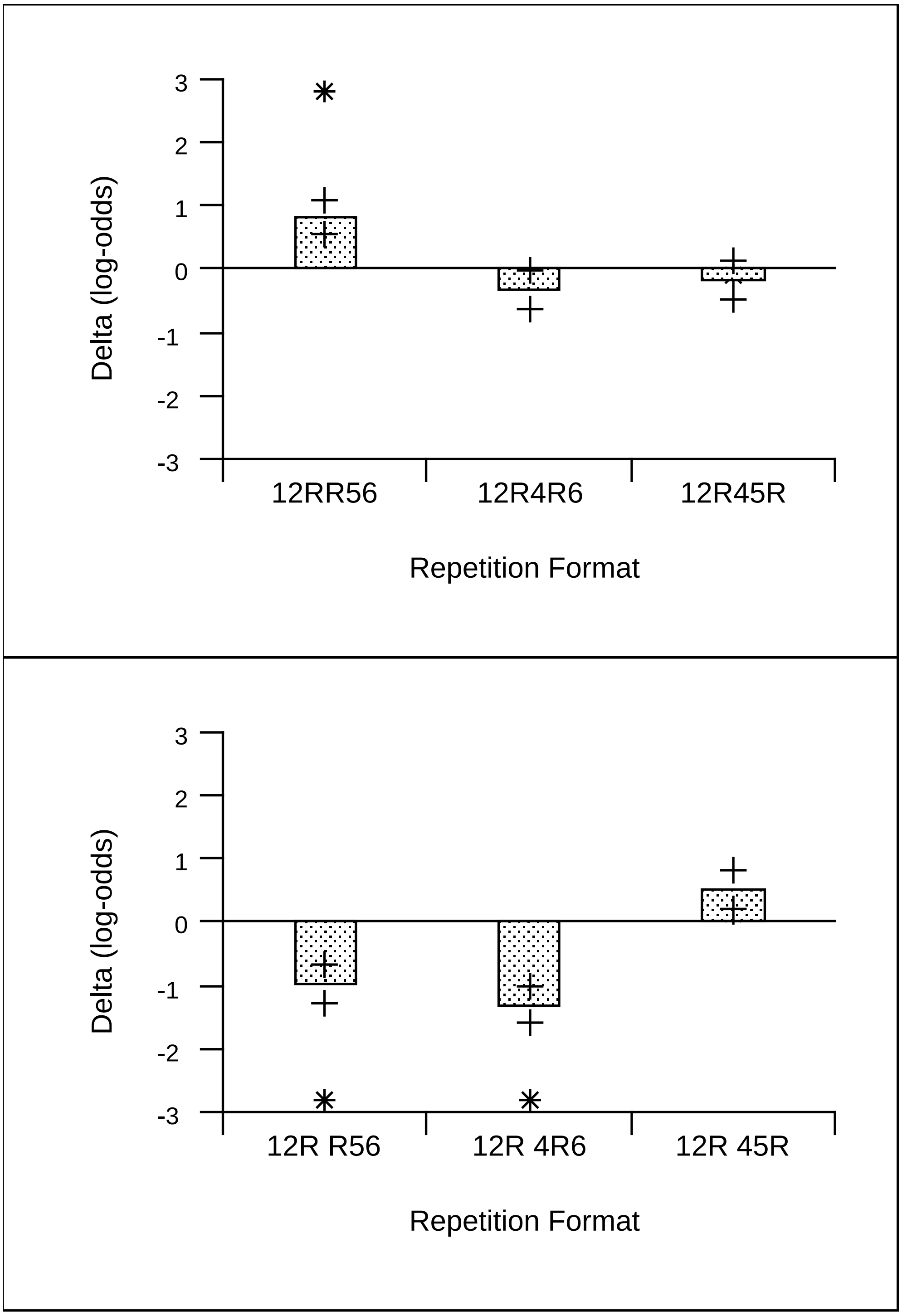

Figure 7-9: Delta under item-scoring, excluding uncertain responses, as a function of repetition separation in ungrouped (upper panel) and grouped (lower panel) conditions of Experiment 8. (Cross-hairs show standard error of delta scores above and below mean; asterisks indicate delta values significantly different from zero under Holm's method of multiple comparisons.)

except one-apart repetition in ungrouped lists and three-apart repetition in grouped lists, $Z(12)<2.16$, *family-wise* $p>.05$.

With guesses removed, delta increased for all conditions. 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, $Z(12)<0.95$, *family-wise* $p>.05$. In other words, removing guesses did not change the pattern of significant results.

With all uncertain responses removed, delta increased further still. 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 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, $Z(12)<1.65$, *family-wise* $p>.05$.

Discussion

The results of the present experiment confirm the guessing hypothesis of Hinrichs et al. (1973), that guessing strategies bias recall against repeated elements and in favour of control elements. This bias was apparent by successive removal of responses that subjects guessed or were simply unsure about. Though one might not want to call all such uncertain responses “guesses”, the effect of removing them was to reduce dramatically 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 an increased likelihood of guessing control elements relative to 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 under some conditions even when all uncertain responses were removed. This is consistent with the significant repetition inhibition found when subjects were instructed not to guess in Experiment 7. It is surprising then that this repetition inhibition was not found by Greene (1991) with similar instructions. One reason may be the more sensitive measure of repetition inhibition used in the present study. Greene compared only error rates on critical positions in serial position curves, but did not take into account the probability of recalling two critical items, nor the modified control scoring introduced in the present study. It is noteworthy that the figure of 33% uncertain responses in the present experiment was much greater than the percentage of guesses suggested by Greene's data (no more than 10%). This suggests that the present experiment, as a test of guessing theories, erred on the conservative side, if at all.

Note that the residual repetition inhibition found after removing uncertain responses is unlikely to be because subjects were simply less certain of repeated responses than control responses. For example, a natural reluctance for people to repeat themselves may make them more likely to indicate a second repeated element as uncertain than a second control element. However, by this account, repetition inhibition should have increased rather than decreased as uncertain responses were removed. Moreover, the overall numbers of uncertain responses were almost identical in control lists and repetition lists. It seems more likely therefore that people simply did not recall the second repeated element.

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 of suppression during serial recall (as in SEM; Chapter 5).

After an item is recalled once, its type representation is assumed to be suppressed, reducing its probability of output again in the immediate future, much like Crowder's (1968b) output interference hypothesis. This will cause forgetting of the second repeated element to be recalled (and perhaps its replacement by a unsuppressed context element). If this suppression is an automatic, unconscious process, repetition inhibition can remain even when both guesses and uncertain responses are removed. Moreover, suppression can also cause a guessing bias. Suppression of an item already recalled may prevent that item coming to mind when one does decide to guess (as in SEM's guessing in Chapter 6). This would explain why control elements are greater affected by the removal of uncertain responses than are repeated elements. Thus suppression can not only cause forgetting of a repeated item, but also prevent its guessing.

The unconscious nature of suppression can explain why repetition inhibition remains even when people are fully aware that repetition is necessary in order to recall correctly, and why people do not always report any conscious bias against guessing a repeated item (Walsh & Schwartz, 1977). Because suppression can prevent retrieval of a repeated item, as well as prevent its guessing, suppression also explains why repetition inhibition can remain when people are instructed not to guess (Experiment 7; Walsh & Schwartz, 1977) and why repetition inhibition can result from omission as well as substitution errors (Jahnke, 1972b). The automatic nature of suppression is further supported by an unpublished study by Baddeley and Andrade (1996). They found that the magnitude of repetition inhibition was not affected by a concurrent secondary task, which would presumably attenuate any conscious guessing bias.

In SEM, suppression is usually assumed to be partial rather than complete. In other words, suppression will not always prevent correct recall of both repeated elements: It simply reduces the probability of recalling both. More accurately, SEM assumes that suppression is temporary, wearing off over time, to explain repetitions even when there are no repeated items in a list (Chapter 5). In the present experiment for example, approximately 24% of control lists contained such repetition errors. Though a pure guessing hypothesis can appeal to a similar notion of forgetting of previous responses over time, this is problematic for its account of repetition inhibition. If such a significant baseline probability of forgetting previous responses were operating in repetition lists too, a much higher incidence of guessing would be required to explain the magnitude of repetition inhibition. Interestingly, if the elderly were less effective at suppressing or inhibiting previous responses (e.g., Hasher & Zacks, 1988; Koriat, Ben-Zur & Sheffer, 1988), they should not only make more repetitions in control lists, but, somewhat ironically, be less prone to repetition inhibition in repetition lists.[2] The fact that repetition inhibition is a within-subject measure makes it particularly attractive to the study of developmental changes in inhibitory processes.

Considerable numbers of erroneous repetitions were found in repetition lists too. In the present experiment, approximately 32% of repetition lists contained repetition of the 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

2. Preliminary evidence suggests that age does indeed reduce the repetition deficit in production tasks analogous to serial recall, but not in perception tasks (i.e., repetition blindness), in which age accentuates rather than reduces the repetition deficit (MacKay, Abrams & Pedroza, 1996).

repeated (Henson, 1996b). The 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. The smaller set of such items in repetition lists than control lists means a repetition error is more likely to result in the former.

The refractory nature of suppression may also 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. One reason why this trend was not found in the present experiments may be because of the smaller range of repetition separations tested. Also, in addition to allowing greater recovery from suppression, increasing repetition separation will reduce the chance of detecting repetition (Lee, 1976b; Henson, 1996b). The trade-off between these two factors may depend on the exact repetition formats used. This reinforces the potentially complex nature of item repetition effects in serial recall.

Finally, an alternative explanation of present results is worth discussing. The repetition inhibition remaining for some grouped repetition formats, in spite of the removal of uncertain responses, might result when people thought a different item was repeated. Henson (1996b) showed such errors of repetition memory are quite common for these formats. Can incorrect repetition memory cause repetition inhibition, by overriding repetition of the correct item with repetition of a different item? If so, there may be no need to appeal to the notion of suppression. However, this alternative account seems unlikely for several reasons.

Firstly, repetition errors were rare when uncertain responses were removed, so correct repetitions were not always replaced by incorrect repetitions. Secondly, the repetition memory errors in Henson (1996b) may have arisen when subjects detected a repetition, but forgot which item was repeated, and so resorted to guessing, or even covert serial recall of the whole list. In either case, the repetition memory errors would not cause repetition inhibition per se, but rather reflect situations where repetition inhibition would have resulted anyway. Finally, memory for an incorrect repetition can not be a sufficient reason for repetition inhibition because repetition inhibition still occurred in repetition formats where such errors were rare: where it was more likely for a complete failure to detect any repetition (e.g., repetition format

1R345R in Henson, 1996b). This is confirmed by Jahnke (1969b), who showed repetition inhibition remained even when reports containing repetitions were removed from analysis. Suppression seems the only way to explain the residual repetition inhibition in Experiment 8.

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 which may operate unconsciously as well as consciously. A similar argument for two causes underlying repetition inhibition was made by Arbuthnott (in press) for repetition effects in sequential arithmetic problems. Both conscious (Baddeley, Emslie, Kolodny & Duncan, 1995) and unconscious (Brugger, Monsch & Johnson, 1996) causes have also been suggested for people’s failure to give appropriate numbers of immediate repetitions in random generation tasks. Nevertheless, it is possible that both output interference and guessing biases are consequences of a more general process of suppression. The automatic suppression of previous responses, assumed necessary for serial recall, 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 short-term, serial recall, even under a new, conservative scoring scheme. These effects were mainly restricted to the repeated elements (repetition facilitation and repetition inhibition); there was little evidence for a direct effect of repetition on surrounding context elements (repetition contamination). Furthermore, the 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 related to the present chapter is given below, followed by one such attempt at a more comprehensive theory of item repetition effects. Finally, the effects are discussed in relation to models of serial recall from short-term memory, and SEM in particular.

Summary of Empirical Findings

All repetition effects in the present study were measured under a modified scoring scheme that treated repeated elements identically to control 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 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, where 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. Nevertheless, other results were in broad agreement with previous studies, and, given no theoretical reason to chose one scoring scheme over another (i.e., no accepted theory of whether repeated items are represented as types or tokens in short-term memory), the present scheme seems preferable as a conservative and unbiased method.

In the serial recall tasks, repetition facilitation reflected mainly superior positioning of two repeated elements relative to two control elements. It was found only for immediate repetition that did not straddle a group boundary (Experiments 6, 7, 8), and repetition at the end of groups (Experiments 7, 8). Repetition inhibition reflected mainly inferior recall of two repeated elements anywhere in a report. It was typically found for all repetition formats that did not show repetition facilitation (Experiments 6, 7, 8). Repetition inhibition was reduced by discouraging (Experiment 7) or removing (Experiment 8) guesses. Nevertheless, significant repetition inhibition remained even when all uncertain responses were removed from analysis (Experiment 8), and when subjects concentrated on remembering repetition (Experiment 7).

In the repetition memory tasks, the probability of detecting a repetition event was generally high, even with a concurrent serial recall task (Experiment 7), but decreased slightly as repetition separation increased (Henson, 1996b). The probability of reporting the correct repeated item was lower, and reporting repetition of a different item was common for small repetition separations (Experiment 7; Henson, 1996b). Correct positioning of repeated elements decreased more markedly as repetition separation increased (Henson, 1996b). The only exception to these effects of repetition separation occurred when lists were grouped: Repetition at the end of groups was then much better remembered (Experiment 7; Henson, 1996b). Surprisingly, repetition memory was not impaired for repetition across a group boundary (Experiment 7; Henson, 1996b). Memory for a repeated item correlated very highly with serial recall of that item (Experiment 7).

Apart from a general association between recall of critical elements and recall of their surrounding context elements (as would be expected given the interdependency between responses in serial recall), more specific measures showed little evidence of repetition contamination. The probability of errors following correct repeated elements was significantly greater than following correct control elements, but this was accompanied a greater probability of errors on the repeated elements themselves. There was a trend for more contamination errors to be associative intrusions in repetition lists than control lists, irrespective of grouping (Experiment 6). Unfortunately, such intrusions were so infrequent that this trend could not be tested further in Experiments 7 and 8.

A General Theory of Item Repetition Effects

There are probably several possible interpretations of the repetition effects found in the present study. Rather than trying to enumerate all of them, one possible interpretation is outlined below. This general theory attempts to incorporate results from the present study with those from previous studies in the literature. Several important components of the theory, such as repetition tagging, repetition schemata, guessing strategies and response suppression, come from Henson (1996b). What follows is an attempt to bring these ideas together.

Repetition Facilitation and Inhibition

The basic tenet of the theory is that repeated items face a negative bias against repetition during recall, which can be overcome in situations where their repetition is explicitly remembered. The negative bias, underlying repetition inhibition, has several forms. Firstly, people have a natural reluctance to repeat themselves. This will prevent them from guessing a repeated item if they have forgotten the answer to a problem (Hinrichs et al., 1973). Note that this might apply to a range of tasks; such a bias is not necessarily restricted to serial recall (Greene, 1991; Mewaldt & Hinrichs, 1977). Secondly, in serial recall, an additional unconscious bias operates. This is the automatic suppression of previous responses. Suppression of an item after its recall reduces its chances of being retrieved again, and may also prevent that item coming to mind as a guess. Such suppression is often assumed necessary for serial recall (Henson et al., 1996) and may be a general process in the sequencing of actions (Houghton & Tipper, 1996; MacKay, 1987). Repetition inhibition is therefore not necessarily found in tasks with no requirement to output both repeated elements (e.g., Jahnke, 1970; Wolf & Jahnke, 1968).

Both a guessing bias and a suppression process are sufficient to explain why repetition inhibition reflects inferior item recall of repeated elements (Experiments 6, 7, 8) and, in particular, the second repeated element to be recalled (Crowder, 1968a; Jahnke, 1969b; Wickelgren, 1965c). However, both are necessary to explain why discouraging or removing guesses decreases repetition inhibition (Experiments 7, 8; Greene, 1991), but does not eliminate it (Experiments 7, 8; Walsh & Schwartz, 1977). Furthermore, only a guessing bias can explain the effects of vocabulary size and number of trials (Hinrichs et al., 1973; Jahnke, 1969b, 1974), occasional repetition inhibition for the first repeated element (Jahnke, 1969a) and repetition inhibition in tasks other than serial recall (Greene, 1991; Mewaldt & Hinrichs, 1977). On the other hand, only an automatic suppression process can explain why people do not always report a bias against guessing repeated elements (Walsh & Schwartz, 1977), why many errors in recall of critical elements are omissions (Jahnke, 1972b), why repetition inhibition does not necessarily correlate highly with ability (Jahnke, 1974), and why repetition inhibition is unaffected by secondary distraction tasks (Baddeley & Andrade, 1996).

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 subjects almost invariably vocalised both repeated elements), they do not automatically notice that an item has been repeated. Generally, the probability of detecting repetition is lower the greater the repetition separation (Henson, 1996b; Lee, 1976b). The most important aspect of repetition separation is the number of intervening items, rather than the absolute time, because repetition memory is just as accurate for repeated elements that are separated by a pause between groups as for repeated elements that are not (Experiment 7; Henson, 1996b). Nevertheless, grouping can affect the probability of detecting repetition in other cases. In particular, the distinctive nature of the end of groups (Chapter 5) improves detection of repeated elements at these positions (Henson, 1996b).

However, accurate repetition memory depends not only on detecting the repetition during presentation, but also remembering which particular item was repeated. Sometimes one can remember the repetition event, but forget which item was repeated. Indeed, subjects in the present experiments occasionally reported “knowing” that a repetition occurred, but not being sure which item was repeated. This explains why repetition inhibition remains when repetition is expected (Mewaldt & Hinrichs, 1977), monitored (Experiment 7) or even reminded (Jahnke, 1969b). Memory for the repeated item is assumed to be an item memory separate from the memory for the list itself (Experiment 7). Like Lee’s (1976b) repetition tags, this repetition memory affects retrieval rather than storage of the list. It is also prone to proactive interference, explaining the tendency for people to perseverate the repetition of a particular item across trials (Experiment 7).

When the repetition event is remembered, but the repeated item forgotten, people may guess at an item, or try to reconstruct the item via (covert) serial recall of the list. “Memory” for repetition of a different item can result in both cases. Such errors of repetition memory do not cause repetition inhibition therefore, but arise in situations where repetition inhibition would result anyway. Because repetition detection is better the closer the repeated elements, repetition of the wrong item will necessarily be more frequent in such cases (Henson, 1996b).

Accurate repetition memory can counteract the negative bias against repetition during recall. Correct memory for the repeated item will remove any conscious bias against guessing it, and perhaps overcome its suppression during recall (at least until it has been output twice). This will reduce any difference in 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 explains why the correlation between repetition memory and recall performance is so high (Experiment 7). In order to remove repetition inhibition completely, repetition memory must be accurate over all trials. Thus, even though repetition across a group boundary may be detected and remembered on most trials (Experiment 7; Henson, 1996b), it may not be accurate enough over all trials to overcome repetition inhibition (Experiment 6, 8).

While memory for the repeated item is necessary to prevent repetition inhibition, it is not sufficient to cause repetition facilitation. Repetition facilitation requires additional forms of repetition memory. One such memory is the tagging of immediate repetition. Such repetition tags are associated with a position in a list, and cause immediate repetition of the item recalled at that position (as in Houghton et al., 1994).[3] 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 specialised coding of the immediate repetition of an action may have evolved specifically to overcome suppression during the execution of action sequences (e.g., MacKay, 1987).

If adjacent repetition is detected and tagged often enough over trials, repetition facilitation can emerge, sometimes under item-scoring (Experiment 8; Lee, 1976b), but most obviously under position-scoring (Experiment 6), given that there is no opportunity for the second repeated element to transpose with the context elements that follow it as there is for the second of two adjacent control elements. Repetition tagging is not applied to nonadjacent repeated elements however, because immediate repetition of the corresponding item during recall would result in the wrong order of items (e.g., *12R4R6* being recalled as *12RR46*). Neither is tagging is applied to adjacent repeated elements that straddle a group boundary, because immediate repetition would then interfere with the grouped organisation of recall. People tend to pause between recalling groups, and the immediate repetition of an item at the end of one group may impair retrieval of the next, by disrupting the grouped organisation of recall (e.g., *12R..R56* recalled as *12RR..56*). This explains why repetition facilitation does not occur in these situations (Experiments 6, 7, 8), even though detection and memory for the repetition may actually be improved (Experiment 7; Henson, 1996b). For further details about repetition tagging, see Henson (1996b).

3. These repetition tags differ from those of Lee (1976b) and Drewnowski (1980a), which were associated with items rather than positions, and were not restricted to immediate repetition.

Another special type of repetition memory is the repetition schema. A repetition schema represents knowledge of a recurring repetition structure in lists (Jahnke, 1969b; e.g., the particular repetition formats in the present study). In the case of repetition format *12R45R* for example, the schema might be of the form *something, something, repeat; something, something, repeat*. Though people are not normally aware of recurring patterns of repetition (Malmi & Jahnke, 1972), when the subset is very small, and repetition memory is very accurate over a number of trials (Experiments 7, 8), they may extract one or more of the underlying repetition structures. The salience of repetition at the end of groups makes it particularly likely that a schema will be extracted in such cases. The use of repetition schemata can aid both item and position recall of repeated elements (Experiments 7, 8), though they do not have to be employed by every subject on every trial to cause repetition facilitation. They must simply be used by enough subjects on enough trials to overcome the repetition inhibition arising when repetition is not detected or remembered.

Repetition Tagging and Schemata

Some parts of this theory are assumptions that may need further justification. For example, why assume that accurate memory for which item was repeated is only necessary to prevent repetition inhibition, and is not sufficient, without additional repetition tagging or schemata, to cause repetition facilitation? Could not the notions of repetition tagging and repetition schemata be subsumed within a single notion of repetition memory (such as Lee's tags, 1976b), with the presence of repetition facilitation or repetition inhibition depending simply on the accuracy of this memory? The main reason for thinking otherwise is that the present study failed to find repetition facilitation in some situations where repetition memory was very accurate, and yet did find repetition facilitation in other situations where there was comparable accuracy of repetition memory. With repetition format *12RR56* for example, there was repetition facilitation when ungrouped, but not when grouped (Experiments 6, 7, 8), even though repetition memory for both conditions was comparable and very accurate (over 75% of trials in Experiment 7 and Henson, 1996b).

There was also indirect evidence that repetition tagging and repetition schemata are qualitatively different from a simple memory for which item was repeated. It was only with adjacent repeated elements that did not straddle a group boundary where people sometimes erroneously repeated a response in two adjacent positions (e.g., *12RR56* recalled as *1RR256*, or *12RR56* recalled as *1255R6*; Henson, 1996b). In all other formats, such repetition errors were much further apart. The former type of error can be attributed to a repetition tag being triggered too early or too late, much like the Houghton et al. (1994) account of typing errors such as *schhol*. Such side-effects of repetition tagging might explain why adjacent repetition does not always improve item recall (Experiment 6). 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 in Experiment 8).

A repetition schema is a qualitatively different type of repetition memory because it is only likely to be extracted when the range of repetition formats is small. This is one reason why repetition facilitation at the end of groups was stronger in Experiments 7 and 8 than in Experiment 6. It is possible that repetition memory for repetition format *12R45R* in Experiment 6 was very accurate on a trial by trial basis, but a corresponding repetition schema was never extracted because the repetition structure was so variable across trials. This would explain why few subjects in that experiment could accurately describe any of the repetition formats during debriefing, whereas most subjects in Experiments 7 and 8 were able to describe repetition format *12R45R*, normally after they had attempted the grouped condition (Henson, 1996b). (Some subjects could also describe adjacent repetition in other formats, but could not always correctly position that repetition). This was the only repetition format with nonadjacent repetition that ever led to repetition facilitation in the present experiments. Finally, use of a repetition schema for repetition format *12R45R* can also explain why serial recall of this format was least sensitive to retention interval in Experiment 7. This was in contrast with repetition format *12RR56*, which showed greater sensitivity to retention interval, presumably because of the greater opportunity for erroneous triggering of repetition tags (above).

The present theory also suggests some ways to dissociate repetition tags and repetition schemata. Repetition tagging is assumed to be a general property of the cognitive system for ordering output of sequences from both short- and long-term memory. Thus repetition tags are assumed to be employed automatically by everyone. Use of repetition schemata on the other hand depends on how well an individual can detect and remember a recurring repetition structure. Some people may extract one or more repetition schemata; others may not. Therefore, a large group of subjects could be split after testing into those who were able to describe some repetition formats and those who were not. This post hoc division should have little effect on repetition facilitation for immediate repetition; for other types of repetition, only subjects who were able to describe a repetition format accurately should show repetition facilitation for that format.

Another means of dissociating repetition tags and repetition schemata might be to employ a secondary distraction task during presentation and recall. The added attentional demands of this task should impair the abstraction of repetition schemata and reduce repetition facilitation in such cases, whereas the automatic nature of repetition tagging should mean that repetition facilitation for immediate repetition is unaffected (Baddeley & Andrade, 1996).

There are many other aspects of the above theory that warrant further investigation. The most obvious questions concern the exact interaction between repetition memory and serial recall. How exactly does memory for a repeated item prevent repetition inhibition? How do repetition tags operate, occasionally incorrectly? How do repetition schemata act during recall to improve recall of repeated items? How much do guessing strategies affect repetition effects like repetition contamination? These questions require a more precise, computational model of serial recall.

Models of Serial Recall and Item Representation

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 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 repeated items in lists can impair serial recall of those lists, associative models that store order over type representations would seem to face much greater problems in recalling such lists.

Associative chaining models face problems because a repeated item will be associated with more than one successor, making it an ambiguous cue in chaining. This ambiguity can be reduced by assuming that the cue includes a number of previous responses, as in compound chaining models (Chapter 1). This additional "context" allows disambiguation of repeated elements with different predecessors. However, to the extent that cues following repeated elements retain some similarity, associative chaining models still predict that there will be a greater probability of an error following a repeated element than a control element (Chapter 2). Yet in Experiment 6, the evidence for errors, particularly associative intrusions, was weak, and may well have alternative explanations (e.g., guessing biases). In any case, given the predicted impairment following repeated items, it is unclear whether associative chaining models could match the high level of recall of repetition lists in the present study (at least without an explicit, quantitative model). More generally, associative chaining models would face increasing difficulties as the number of repeated elements in a list increases.

Associative positional models would also appear to face problems in recalling lists with repeated elements. In the Articulatory Loop Model (Burgess & Hitch, 1992, 1996b) 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 would appear to cause a tendency for repeated elements to be recalled too early (for reasons related to the original model's difficulty with phonologically similar items; Chapter 1).

More generally, any model that assumes that repetitions of an item produce multiple associations with the same type representation (or even increased activation of a type representation, as in *strength* models, Hintzman, 1976), would appear to 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 nonrepeated (control) element. Again, this problem may be more apparent than real, an issue that can be resolved by applying computational models to data from studies like the present one. The above problems do not apply to nonassociative models however, where repeated elements are stored as separate tokens (e.g., Page & Norris, 1996b). SEM is a nonassociative model.

Item Repetition in SEM

SEM has not been fitted to the data from Experiments 6, 7 and 8. This is mainly because many new assumptions would be needed, regarding repetition memory, repetition tagging and repetition schema, which themselves remain hypothesis to be tested further (Henson, 1996b). Nevertheless, it is worth considering how the general theory of item repetition effects outlined above might be implemented within SEM.

In SEM, auditory or visual perception of an item produces a position-sensitive token for that item in short-term memory.[4] During this process, there is no automatic registration that some items have occurred before (i.e., that some of the tokens may represent the same type). Detecting that two tokens correspond to a repeated item depends on a secondary process of comparing new tokens with older tokens in memory. The probability of detecting a repetition will therefore be a function of the similarity between tokens. This similarity is determined both by the identity of the items and their positional context (i.e., repetition detection might demand not only identical item codes, but also similar positional codes). The smaller positional overlap for items further apart in a list (Chapter 5) explains why the probability of detecting a repetition generally decreases with increasing repetition separation. In other words, a *Q* at the start of the list and the *Q* at the end of the list may appear quite different in short-term memory. When lists are grouped, the positional context for items in the same position within groups is increased, particularly for repeated elements at the end of groups, where the positional coding is very sharp (Chapter 5). At the same time, the positional codes for repeated elements at different positions within groups will reduce the chance of detecting their repetition. This is consistent with most of the results in Experiment 7.

However, the notion of positional overlap does not explain why repeated elements straddling a group boundary are well detected, because their tokens have very different positional contexts within groups. One reason why they are well detected may be the simple fact that there are no intervening items. Their repetition could then be detected by alternative means, such as residual activation of type representations. (This possibility could be tested by inserting a redundant item between groups.) Though obviously an ad hoc solution at the moment, this additional assumption allows SEM to capture fully the effects of repetition separation and grouping on repetition memory.

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4. When repeated elements are too close in time however, as in very rapidly presented sequences, it may not be possible to form two separate tokens. Such a limit to the process of token individuation (at least for visual presentation) is one reason sometimes given for repetition blindness (e.g., Kanwisher, 1987).

Once a token is selected for output during recall (Chapter 5), it makes contact with its long-term type representation again, in order to articulate a categorical response. The suppression of these type representations corresponds to the automatic suppression underlying repetition inhibition in the general theory above. This suppression can prevent a repeated item being recalled more than once (even though there are two separate tokens for that item in memory). This may lead to a transposition of an item whose type representation is not suppressed, or even an omission, if most type representations are suppressed. Alternatively, a guess might result instead. A guess chosen from the most active (least suppressed) type representation in memory (Chapter 6) will be unlikely to produce the correct repeated item, and repetition inhibition will still result. Thus SEM already provides a suppression process that can explain repetition inhibition by both failure to retrieve a repeated item and failure to guess a repeated item.

It is less clear how memory for a repeated item interacts with serial recall in SEM. One possibility is that detection of a repeated item causes its type representation to be flagged. The purpose of the flag is to prevent suppression of the type representation during output. In principle, this means that both tokens can be selected and output as effectively as if they represented two different control elements[5]. This prevents repetition inhibition. By assuming further that the flag itself can sometimes be forgotten or separated from its type representation, there is also the possibility of repetition of a different item.

As in the general theory then, the magnitude of repetition inhibition depends on how often the repetition is detected and flagged correctly. Because detection and flagging is normally quite good, repeated elements can be recalled correctly more often than not, and repetition inhibition is usually only of the order of 10%. When detection and flagging are very accurate, then repetition inhibition can be prevented completely.

Repetition tagging can be modelled as a special type of token in memory. When this “doubling” token is selected, it causes immediate repetition of the next token selected, before the corresponding type representation is suppressed. This gives adjacent repeated elements an

5. (though the exact consequences of withholding suppression depend on the equation governing the strength with which categorical representations compete for output; Equation 10-8 in Appendix 3)

advantage over adjacent control elements, producing repetition facilitation. However, this is not always the case, because if the doubling token is selected too early or too late, there can be adjacent repetition on the wrong position or of the wrong item. Modelling tags as tokens, rather than over phonological representations, may explain why effects of immediate repetition do not interact with phonological similarity (Drewnowski, 1980a).

Repetition schemata might be modelled as a structure associated with particular positional codes. When the identity of tokens with these positional codes match, this structure is triggered, and associated with the repeated item. This association suppresses the type representation of the repeated item until the corresponding positions are reached in recall. At this point, the representations are unsuppressed and the repeated item is output. This will increase recall of repeated elements at these positions above corresponding control elements, producing repetition facilitation.

Repetition Contamination

Finally, repetition contamination is assumed to have two aspects. The first is that the probability of recalling context elements in a repetition list depends on the probability of recalling the repeated elements. This means context elements are recalled better under conditions of repetition facilitation than repetition inhibition, and is a trivial consequence of competition and suppression in SEM (Chapter 5). The second aspect of repetition contamination is a slight increase in the probability of guessing context elements in repetition lists than control lists. This owes to the smaller set of items to guess from in repetition lists than control lists. This guessing bias explains why a slightly greater proportion of errors following correct recall of one critical element will be the item following the other critical element in repetition lists than control lists. Thus Wickelgren’s (1966) associative intrusions are not seen as evidence for an associative chaining theory, but as an artifact of guessing. Being a nonassociative model, SEM also predicts that, provided a repeated element is recalled correctly, the fact that it represents a repeated item has no relevance to recall of the subsequent context element (i.e. no effect of repetition on cuing). The question of whether repeated items do have any effect on recall of subsequent items must await further testing, in which performance on repeated and control elements is equated.

Many of the above ideas are speculative, and must await implementation and simulation in SEM before they can be confirmed theoretically (and subsequent experiments before they can be supported empirically). Nevertheless, they represent a first approach to modelling a tight set of constraints emerging from the present experiments; constraints that also represent a challenge for other models of serial recall, particularly associative ones.

Chapter Summary

The results from present experiments 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. Detection and memory of a repetition is more likely the more immediate or the more salient the repetition (e.g., at the end of groups). In such cases, additional tagging of immediate repetition or abstraction of repetition schemata can increase the probability of recalling both occurrences and produce repetition facilitation.

These complex yet robust findings represent important challenges for models of serial recall from short-term memory. Those that assume order is represented over token representations would seem best suited to explaining people's general ability to recall sequences with repeated elements. This is consistent with one of the core assumptions of SEM (Chapter 5). The specific effects that item repetition has on serial recall, at least in short-term memory, may well arise from special mechanisms geared towards the detection of repetition (such as repetition tagging or repetition schema), or the output of a response (such as response suppression), though the implementation of such mechanisms in SEM remains a task for the future. Indeed, the issue of item representation in both short- and long-term memory remains an open one, whose resolution may well depend on demonstrations that particular models with particular representations can account for the empirical data, such as those in this chapter.