Chapter 6: Absolute or relative position?

The nature of positional errors

Experiment 2 showed that transpositions between groups tend to maintain the same position within a group. Such interpositions were as common as adjacent transpositions. Experiment 3 showed that intrusions between trials tend to maintain the same position within a trial. Such protrusions were significantly more common than expected by chance. However, both these types of positional error were demonstrated with groups or lists of equal length. The present chapter describes two experiments examining positional errors between sequences of different length. The results of these experiments are predicted (and fitted) by SEM, but not predicted by other models of short-term memory.
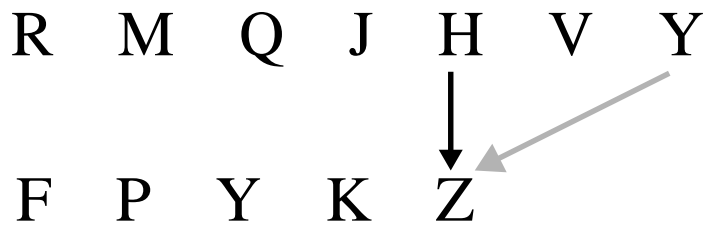
Absolute and Relative Position

What happens to errors between sequences of different lengths? Do substitutions between such sequences maintain *absolute* position from the start of sequences (e.g., first, second, third, etc.), or do they maintain position *relative* to the end as well as the start? For example, consider transpositions between a group of three items followed by a group of four: Does the third and final item of the first group tend to swap with the third item of the second group, or with the fourth and final item of the second group (Figure 6-1)? The *absolute interpositions* in the former case are transpositions respecting absolute position within a group (i.e., third to third); the *terminal interpositions* in the latter case are transpositions respecting relative position within a group (i.e., end to end). Or consider recall of a list of seven items on one trial followed by recall of a list of five items on the next: Are most intrusions on the fifth and final position of the report of five items from the fifth position of the previous report, or from the seventh and final position of the previous report? In other words, which are more common: *absolute protrusions* or *terminal protrusions*?

The nature of such positional errors is important in light of SEM (Chapter 5). Because the end marker maintains the same maximum strength and rate of change, irrespective of sequence length, the model predicts terminal positional errors will dominate over absolute

Interpositions

R M Q   J H V Y

Protrusions

R M Q J H V Y

F P Y K Z
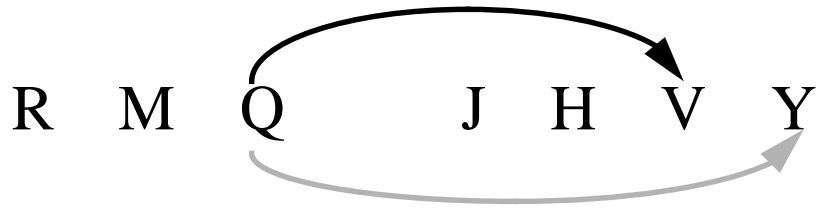
→ Absolute   → Relative

Figure 6-1: Illustration of absolute and relative positional errors.

positional errors. To take the above example of different list lengths, terminal protrusions are predicted because the cue for the last position in recall of a five item list is identical to the cue for the last position in recall of a seven item list. Thus, the positional overlap between the positional cues for the ends of the two lists will always have greater overlap than the positional cues for the fifth positions of each list.

The Articulatory Loop Model (Burgess & Hitch, 1992, 1996a, 1996b) predicts the opposite: that absolute positional errors will dominate over terminal positional errors. This is because their context window (Chapter 1) moves in the same, constant manner, irrespective of the length of the sequence. Thus the cue for the fifth position will be the same, whether there are five, six, seven, or more items, and maximum positional overlap will always arise for identical absolute positions. Indeed, this prediction would appear to follow from any model where the positional cue is derived from some regular or real-time temporal oscillation (e.g., Brown et al., 1996). Though the abstract formulation of the Lee and Estes (1981) model does

not specify the exact nature of the positional cues it employs, viewing perturbations as the results of cyclic reactivations would also seem to imply coding of absolute position.

To distinguish these models, two experiments below directly compare the incidence of absolute and terminal positional errors. In the first experiment, these errors are interpositions between groups of different sizes. In the second experiment, these errors are protrusions between lists of different lengths. These experiments are the first tests of predictions of SEM, that position is coded relative to both the start and the end of sequences, and so terminal positional errors should exceed absolute positional errors in both cases. In addition, both experiments allowed subjects to indicate the confidence of each response. Page and Norris (1996b) have suggested that positional errors, particularly protrusions, might be the result of guessing strategies, implying that subjects are less confident of positional errors than other responses. If so, positional errors should disappear once guesses are removed from analysis. If positional errors remain however, there will be further support for the integral role of positional information in serial recall.

Experiment 4

The present experiment tested whether transpositions between groups of different size maintain absolute or terminal position within groups. In the Grouped 3-4 condition, lists of seven items were split into a group of three followed by a group of four. The *critical positions* were Positions 3 and 7 (final positions within groups), and Positions 3 and 6 (third positions within groups). Terminal interpositions between the ends of groups were errors when Item 7 was recalled in Position 3, or Item 3 was recalled in Position 7. Absolute interpositions between the third position of groups were errors when Item 3 was recalled in Position 6, or Item 6 was recalled in Position 3. In the Grouped 4-3 condition, the lists were split into a group of four followed by a group of three. In this case, the critical positions were Position 3 and 7, and Positions 4 and 7. Terminal interpositions were then errors when Item 7 was recalled in Position 4, or Item 4 was recalled in Position 7, and absolute interpositions were errors when Item 3 was recalled in Position 7, or Item 7 was recalled in Position 3. Given the interdependencies between responses in a report (Chapters 4, 5), the chance probability of

terminal and absolute interpositions cannot be determined in any simple manner. Therefore, an Ungrouped condition was included to check that differences in terminal and absolute interpositions were not simply an artefact of different baseline probabilities. Finally, all conditions allowed subjects to distinguish between confident responses and less confident responses (guesses), to test whether interpositions were the result of guessing strategies.

Method

Subjects

Twenty-four students from Cambridge University were tested, twelve male and twelve female, with a mean age of twenty years.

Materials

Three blocks of thirty lists were constructed. Lists were permutations of seven single-syllable, low-frequency, phonologically nonconfusable words, drawn from a subset of those in Experiment 3: *goose, verve, latch, bathe, flown, clump* and *trout*. The order of words within lists was randomised except for the constraint that, over a block of trials, each word appeared approximately equally often at each position.

Procedure

Blocks were assigned to three conditions for each subject. In the Ungrouped condition, the seven words were presented in the centre of a VDU at a rate of just over one a second (600-ms on, 200-ms off), each word replacing its predecessor. In the Grouped 3-4 condition, there was an additional 800-ms pause between the third and fourth words; in the Grouped 4-3 condition, there was an additional 800-ms pause between the fourth and fifth words. Subjects read the words in silence and were told to use the pause to group the lists appropriately. As soon as the last item had disappeared, a cue followed for immediate, serial recall.

Subjects recalled the list by writing the first letter of each word in two rows of seven boxes provided on a response sheet. Subjects were told to write responses they were sure about in the top row, and responses they were not sure about, or which were guesses, in the bottom row. They could go up and down the rows as much as they liked, as long as they gave one and only one response in each column (i.e., gave exactly seven responses in total). All seven words were permanently on display, from which subjects could guess if necessary.

Subjects were asked to write from left to right on the response sheet, recalling the lists in a forward order. Though they only had to write the first letter of each word, subjects were told to remember the lists as lists of whole words (and all reported obeying this instruction).

Subjects received six, ungrouped practice trials, followed by the three blocks of lists. The order of blocks was counterbalanced across subjects. The order of conditions was constrained by the fact that the Ungrouped condition was always first, followed by the two grouped conditions, which alternated across subjects. This was to reduce the chance of subjects subjectively grouping the ungrouped lists, as might happen if a grouped condition preceded the ungrouped one. The whole experiment took about 40 minutes.

Results

In brief, terminal interpositions were more common than absolute interpositions, irrespective of whether guesses were included or excluded. Many terminal interpositions were repetitions of an item at the end of both groups. Confidence and accuracy of responses were highly correlated, as expected, though guesses were far from random and a considerable number of errors were not indicated as guesses.

Overall Performance

The proportion of lists correct was greater in the Grouped 3-4 (*M=.39, SD=.29*) and Grouped 4-3 *(M=.39, SD=.27)* conditions than the Ungrouped condition (*M=.22, SD=.22*). Tests of weighted log-odds showed the difference was significant in both cases, *Z(24)>7.25. p<.0001*, but no significant difference between the two styles of grouping, *Z(24)=0.09, p=.93*. Error position curves (Figure 6-2) suggested some spontaneous 4-3 grouping in the Ungrouped condition, though several grouping strategies were reported (e.g., 3-4, 2-2-3 and 3-2-2). Removing guesses reduced the number of correct responses on most positions.

Errors on Critical Positions

With the same seven items per trial, the only errors made were transpositions. With guesses removed, the frequency of errors was calculated from the number of transpositions remaining. In order to compare error frequencies on critical positions with baseline measures in ungrouped lists, the ungrouped lists were scored as if they were grouped 3-4 or 4-3.
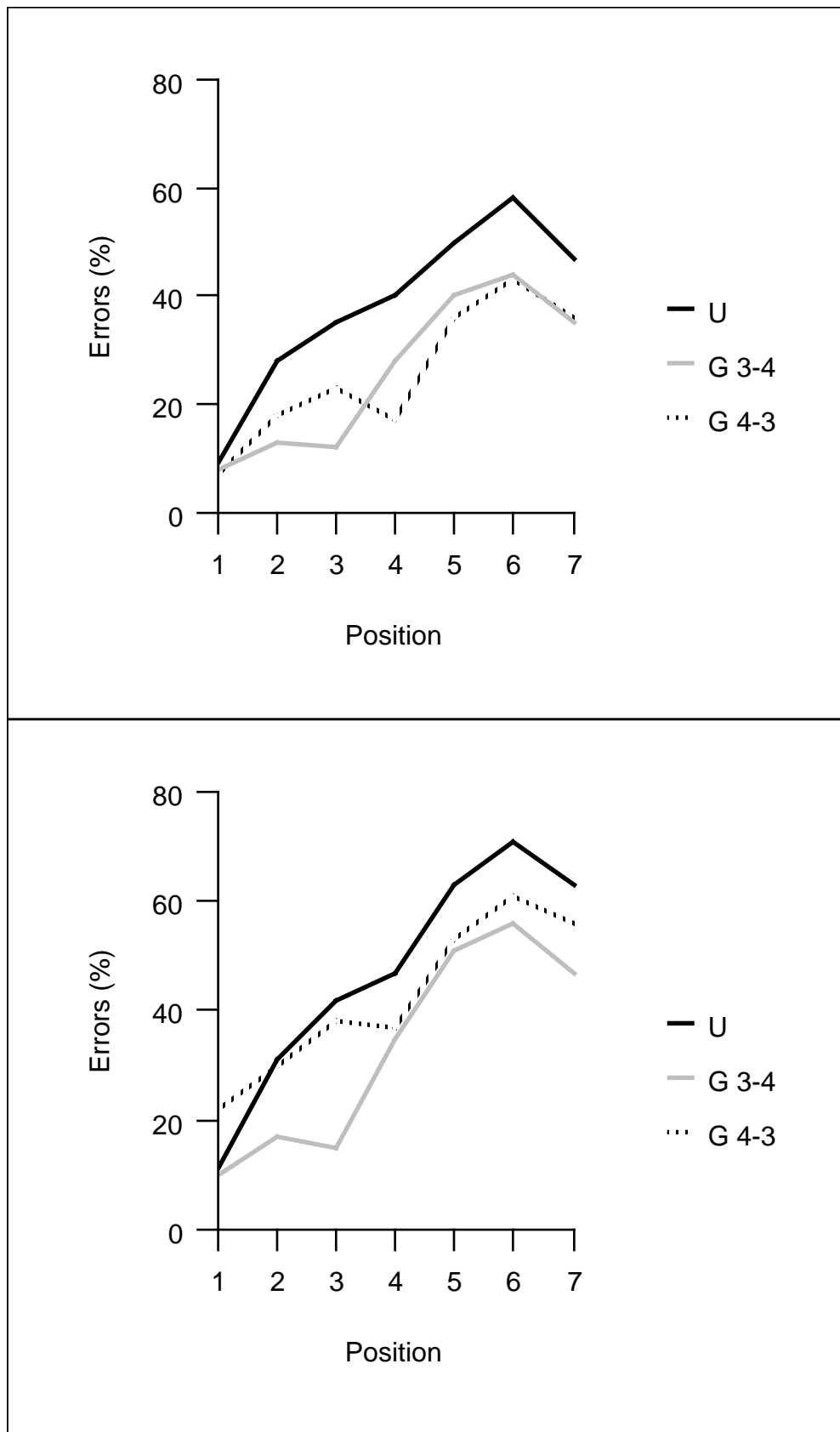
Figure 6-2: Errors by position with guesses (upper panel) and without guesses (lower panel) in Experiment 4.

(U=Ungrouped; G=Grouped.)

Under 3-4 grouping, a three-way ANOVA on the log-odds of an error on either critical position showed effects of condition (Grouped 3-4 vs. Ungrouped), $F(1,161)=128.46, p<.001$, guesses, $F(1,161)=218.90, p<.001$, and position, $F(1,161)=4.62, p<.05$, but no significant interactions, $F(1,161)<1.34, p>.25$. As expected, explicit grouping reduced error frequencies (Experiment 2), as did excluding guesses (Table 6-1). The effect of position reflected fewer errors on final positions than third positions of groups, also as expected from Experiment 2.

Under 4-3 grouping, a three-way ANOVA on the log-odds of an error on either critical position showed effects of condition (Grouped 4-3 vs. Ungrouped), $F(1,161)=83.42, p<.001$, and guesses, $F(1,161)=188.41, p<.001$, but not position, $F(1,161)=1.74, p=.19$. The interaction between condition and position approached significance, $F(1,161)=3.06, p=.08$, but no other interactions did, $F(1,161)<1.31, p>.25$. Apart from the slightly different pattern in the Ungrouped condition, the results resembled those under 3-4 grouping (Table 6-1).

	Grouped		Ungrouped	
	Third	Final	(Third)	(Final)
Grouping 3-4				
Guesses Included	.28 (.16)	.24 (.16)	.47 (.20)	.41 (.17)
Guesses Excluded	.12 (.09)	.10 (.10)	.21 (.16)	.21 (.14)
Grouping 4-3				
Guesses Included	.30 (.17)	.27 (.16)	.41 (.17)	.44 (.18)
Guesses Excluded	.15 (.12)	.12 (.11)	.21 (.14)	.21 (.14)

Table 6-1: Frequency of errors on critical positions in Experiment 4.

The proportion of errors on a critical position that were interpositions from the other critical position was calculated for the 22 subjects who made at least one error per critical position with guesses excluded. These proportions were small; the majority of errors on critical positions were from adjacent, within-group positions. Nevertheless, under 3-4

grouping, a three-way ANOVA on the log-odds showed a significant effect of position, $F(1,147)=9.49$, $p<.005$, and a significant interaction between position and condition, $F(1,147)=9.42$, $p<.005$. No other effects were significant, $F(1,147)<1.66$, $p>.20$. The interaction between position and condition reflected a greater proportion of interpositions between final positions than third positions in the Grouped 3-4 condition, but not the Ungrouped condition (Table 6-2). In other words, interpositions respected relative rather than absolute position, and this did not appear to be an artefact of different baseline probabilities.[1]

Under 4-3 grouping, a three-way ANOVA on log-odds did not show any significant effects, $F(1,147)<1.83$, $p>.18$, though the effect of condition, $F(1,147)=3.20$, $p=.08$, and interaction between condition and position, $F(1,147)=2.75$, $p=.10$, approached significance. Despite the lack of statistical significance, the pattern of results was very similar to that under 3-4 grouping (Table 6-2). A reason for the difference in significance of results in the Grouped 3-4 and Grouped 4-3 conditions is given in the Discussion.

	Grouped		Ungrouped	
	Third	Final	(Third)	(Final)
Grouping 3-4				
Guesses Included	.12 (.09)	.18 (.13)	.15 (.07)	.17 (.09)
Guesses Excluded	.12 (.09)	.20 (.18)	.18 (.13)	.18 (.13)
Grouping 4-3				
Guesses Included	.13 (.09)	.15 (.10)	.17 (.09)	.18 (.09)
Guesses Excluded	.13 (.12)	.16 (.15)	.18 (.13)	.18 (.13)

Table 6-2: Proportion of errors that were interpositions in Experiment 4.
(Calculated from weighted log-odds, $n=22$.)

---

1. It remains possible that the differences in proportions are an artefact of differences in overall numbers of errors on critical positions, with fewer errors on final than third positions within groups (Table 6-1). This possibility is discounted in the analysis below, where errors are confined to repetitions in the second group.

To confirm the possible interactions between critical position and grouping condition, pairwise comparisons were performed on the weighted log-odds of interpositions on third versus final positions in groups. The proportion was significantly greater between final positions of groups in the Grouped 3-4 condition, whether or not guesses were included, $Z(22)>2.00, p<.05$. No such differences were significant in the Ungrouped condition, $Z(21)<0.98, p>.33$. The proportion was also greater in the Grouped 4-3 condition, but this difference was not significant either with, $Z(21)=0.88, p=.40$, or without, $Z(21)=1.09, p=.28$, guesses. This was also true of the Ungrouped condition, $Z(22)<0.41, p>.78$.

Between-group Repetitions

The previous analyses demonstrated that a transposition at the end of one group was more likely to come from the end of the other group than from the third position of the other group, at least in the Grouped 3-4 condition. Closer inspection of the data revealed that many of these errors occurred at the end of the second group, and were repetitions of an item recalled at the end of the first group. In the Grouped 3-4 condition for example, Item 3 was sometimes recalled on both Position 3 and Position 7. Even if a different item was recalled on Position 3, that item was likely to be recalled again on Position 7 (e.g., Item 2 might be recalled on both Position 3 and Position 7). This suggested that many interpositions might be perseverations resulting from proactive interference from recall of the first group on recall of the second, much like the proactive interference between reports in Experiment 3. Consequently, responses in the second group were examined in more detail (in a manner parallel to immediate intrusions in Experiment 3 and Experiment 5, and which allowed comparison with chance levels). Specifically, analysis was restricted to the third and the final position within the second group.

Given a response on a critical position in the second group that was a repetition of an item recalled somewhere in the first group (a *between-group repetition*), the interest was whether that item came from the same critical position of the first group. For this analysis, the two grouped conditions were collapsed together, and, as before, the ungrouped lists were treated as if they were grouped in the corresponding manners. Given the small numbers involved, guesses were included. The proportion of between-group repetitions that were

absolute or terminal interpositions was then calculated for the 19 subjects that made at least one between-group repetition per critical position (Table 6-3). Pairwise comparisons of weighted log-odds showed a significantly greater proportion of interpositions between final positions of groups than third positions of groups in grouped lists, $Z(19)=2.68, p<.01$, but not in ungrouped lists, $Z(19)=0.08, p=.94$. This was not due to different overall incidence of between-group repetitions, which did not differ significantly in either case, $Z(19)<0.45 \ p>.65$.

	Grouped		Ungrouped	
	Third	Final	(Third)	(Final)
Between-group Repetitions	.16 (.07)	.17 (.08)	.16 (.07)	.17 (.08)
Interpositions	.24 (.19)	.40 (.24)	.31 (.19)	.31 (.19)

Table 6-3: Frequency of between-group repetitions, including guesses, and proportion that were interpositions in Experiment 4.
(Calculated from weighted log-odds, *n=19.*)

The chance probability that between-group repetitions maintain absolute or terminal position is difficult to determine exactly, because there are three responses in the first group in the Grouped 3-4 condition and four in the Grouped 4-3 condition. This means the chance probability lies somewhere between *.25* and *.33*. Taking an average value of *.29* (a value close to that in the ungrouped lists), repetitions between the final positions of groups were more frequent than expected by chance in the grouped lists, $Z(19)=2.82, p<.005$, but not the ungrouped lists, $Z(19)=0.65, p=.52$.

Finally, repetitions on the first position of the second group were still predominantly from the first position of the previous group. Indeed, the proportion of between-group repetitions on the first position of groups in the grouped list ($M=.48, SD=.27$) was significantly greater than the baseline figure in ungrouped lists ($M=.37, SD=.28$) and the figure of *.29* expected by chance, $Z(16)>2.03, p<.05$ in both cases. In other words, repetitions between groups respected both terminal positions, the start and the end, of groups.

Guesses

The proportion of responses that were guesses was greater in the Ungrouped condition (*M=.29, SD=.15*) than the Grouped 3-4 (*M=.23, SD=.16*) or Grouped 4-3 (*M=.22, SD=.14*) conditions. A two-way ANOVA on the log-odds of a guess showed a significant effect of condition, *F(2,460)=14.69, p<.001*, output position, *F(6,460)=94.17, p<.001*, and an interaction that almost reached significance, *F(12,460)=1.68, p=.07*. Guesses increased towards then end of recall, with a particularly large increase across group boundaries (i.e., the whole of the second group was often "guessed", like the omission of groups in Experiment 2).

Responses in the Ungrouped condition were split by whether or not they were correct and whether or not they were guesses, forming contingency tables for each subject. A combined test of significance of these tables showed an extremely high correlation between accuracy and confidence of responses, *Z(24)=27.97, p<.0001*, mainly owing to the large number of correct responses that were not guesses (Table 6-4). Nevertheless, almost half of the errors were not indicated as guesses (*M=.48, SD=.07*). Though some of these may have reflected a failure or reluctance to indicate guesses, such a large proportion suggests that subjects were often unaware of having made an error. At the same time, a considerable proportion of guesses were correct (*M=.31, SD=.08*), suggesting that guesses were more than random choices of list items (of which only *.14* would be correct).

	Not Guess	Guess
Correct	2673	438
Error	926	1003

Table 6-4: Number of guesses and errors collapsed across subjects in the Ungrouped condition of Experiment 4.

Discussion

The present experiment showed that interpositions between groups respect the terminal positions of groups rather than absolute position within groups. Though differences were small, the proportion of errors that were interpositions was significantly greater between the ends of groups than between the third positions of groups, particularly in the Grouped 3-4

condition. Similarly, the proportion of repetitions that were interpositions was greater at the end of the second group than the third position of that group, and significantly above chance levels. These data suggest that position within a group is coded relative to both the start and end of that group, confirming the prediction of SEM and questioning the coding of absolute position in other models, such as the Articulatory Loop Model (Burgess & Hitch, 1992) and its extension to grouping (Burgess & Hitch, 1996a, 1996b).

The proportion of errors that were interpositions was unchanged by the removal of guesses, which showed no interaction with grouping or critical position. This implies that interpositions are not simply the result of guessing strategies. The difference in proportions of terminal and absolute interpositions in grouped lists seemed to reflect a depression of absolute interpositions relative to ungrouped lists, rather than an elevation of terminal interpositions. The depression of transpositions between the same absolute position within groups resembles the depression of transpositions between groups that were not interpositions in Experiment 2. The lack of significant elevation of terminal interpositions was surprising, given that grouping increased this proportion in Experiment 2. When analysis was confined to between-group repetitions however, grouping did increase the proportion of repetitions that were terminal interpositions, as well as decreasing the proportion that were absolute interpositions.

The demonstration that interpositions respect position relative to the end as well as start of groups can be explained simply by SEM. Because grouping conditions were blocked in the present experiment, subjects knew the size of both groups in advance. Thus, it is feasible that the strength of the end marker could represent expectation for the end of a group (Chapter 5). Then the strength of the end marker at the third position in a group of three, where the final item is expected, will differ to its strength at the third position in a group of four, where the final item is not yet expected; equivalent strength of the end marker will only occur at the fourth position in the group of four. The capability of SEM to fit present data is confirmed in the General Discussion.

It is not apparent how the present results can be explained by other accounts of grouping. For example, Hitch, Burgess, Shapiro, Culpin and Malloch (1995) suggested that grouping is a rhythmic process driven by internal oscillators. This is based on the fact that the

grouping advantage for visual material may be removed by irrelevant, background tones, which they suggest entrain the internal oscillators to a different rhythm (e.g., Treisman, Cook, Naish & MacCrone, 1994). However, Henson (1996a) failed to replicate their results in two experiments with a fixed list length procedure, rather than span procedure, and a more sensitive index of grouping. In any case, internal oscillators would seem to predict absolute rather than terminal interpositions, contrary to present results. This suggests that the grouping advantage is not solely due to internal oscillators.

This is not to deny that rhythm contributes to grouping effects in other situations, such as when the group sizes are equal. The 3-3-3 temporal grouping in Experiment 2 for example conforms to a natural 4/4 rhythm in each metrical segment, whereas the 3-4 and 4-3 groupings in the present experiment have different rhythms in each segment. This may be one reason why interpositions were much more frequent in Experiment 2 than the present experiment. Thus, a rhythmic account may be necessary to explain why regular group sizes are more effective than irregular group sizes (Wickelgren, 1967), a result not necessarily predicted by SEM. Finally, there are other aspects of the possible interaction between grouping and articulatory suppression or finger tapping, such as differences between internal and external pacing (Hitch et al., 1995), which clearly warrant further investigation.

The observation that many interpositions are repetitions of an item at the end of both groups suggests that interpositions may be the result of proactive interference from recall of the first group on recall of the second. Indeed, around half of the interpositions measured on critical positions were repetitions, which is probably sufficient to explain the differences between grouped and ungrouped conditions in Table 6-2. In other words, interpositions between groups may result from the same output effects that cause protrusions between reports (Experiment 3). The only difference is that repetitions between groups must contend with the additional effect of suppression. Suppression of previous responses reduces repetition within reports, but has little effect on repetition between reports, given that it has normally worn off between trials (Chapter 5). The refractory nature of suppression also explains why terminal interpositions appeared more frequent in the Grouped 3-4 condition than Grouped 4-3 condition: When an item is recalled at the end of the first group, there is more time for

suppression to wear off before the end of the second group in the Grouped 3-4 condition, with three intervening responses, than the Grouped 4-3 condition, with only two intervening responses (Chapter 5). The issue of output effects is resumed in the General Discussion.

Experiment 5

The previous experiment demonstrated that transpositions between groups of different size tend to maintain terminal rather than absolute position. The present experiment tests whether the same is true of intrusions between lists of different length. Specifically, in the Variable condition, subjects saw either five, six or seven words on a given trial. In the Fixed condition, subjects always saw six words on each trial. Given that Chapter 4 demonstrated that output protrusions are more common than input protrusions, the former measure was used in the present experiment. The critical positions were therefore the fifth position in reports and the final position in reports. In the Variable condition, absolute protrusions were intrusions on the fifth position of a report that also occurred on the fifth position of the previous report; terminal protrusions were those on the final position of a report that also occurred on the final position of the previous report. In the Fixed condition, measurements of absolute and terminal protrusions are of course confounded, but the frequency of protrusions on the fifth position and final position were also examined, to give a comparative baseline measure of protrusions.

The Variable condition was interesting for a further reason. Precautions were taken to ensure that subjects in this condition did not know in advance the length of the list on each trial. This raises questions about interpretation of the end marker in SEM. If the end of a list is unpredictable, it is hard to see how the strength of the end marker during presentation of a list could represent the degree of expectation for the end of that list (Chapter 5). Interpretation would be particularly difficult if protrusions were found between the ends of reports, rather than between the same absolute positions. Such errors would require the end marker to grow in strength towards the end of a list in the same manner, irrespective of the list length, even when the end of the list is not known until it occurs.

Finally, both Variable and Fixed conditions used the same method of distinguishing confident responses from guesses as in Experiment 4. There is evidence to suspect that

protrusions might disappear when guesses are removed from reports. This evidence comes from research suggesting that all intrusions are guesses. For example, Dillon and Thomas (1975) showed that instructing subjects not to guess dramatically reduced the proportion of errors that were intrusions. Indeed, subjects were less confident of intrusions than other errors. Bjork and Healy (1974) found a similar result, provided intrusions were not phonological confusions. Dillon and Thomas, like Conrad (1960), used their results to argue that proactive interference results from correct items being inaccessible, and therefore being replaced by guesses, which are often items from previous trials. They argue against the notion (in SEM) that proactive interference is due to response competition. If Dillon and Thomas are correct, protrusions should be affected more by the removal of guesses than other types of error.

Method

Subjects

Thirty students from Cambridge University were tested, twenty male and ten female, with a mean age of twenty years.

Materials

Stimuli were lists of five, six, or seven single-syllable, low-frequency words, drawn from the same set as Experiment 3. The words were split into two subsets that were alternated across trials, such that no word appeared in two consecutive trials. The order of words within lists was randomised except for the constraint that, over all trials, each word appeared approximately equally often at each position.

Procedure

Two blocks of 46 lists were constructed. The first list of six items in each block was not analysed. In the Fixed block, the remaining 45 lists also contained six words. In the Variable block, there were 15 lists of five words, 15 of six words and 15 of seven words. The order of lists in the Variable block was such that no two consecutive trials had lists of the same length.

Each word was presented in the centre of a VDU, replacing the previous word, at a rate of just over one a second (600-ms on, 200-ms off). Subjects were instructed to read the words in silence. Some time after the last word had disappeared, a cue appeared to signal immediate serial recall. The pause before this cue appeared was such that the amount of time elapsing

between the onset of the first word and the onset of the cue was identical, no matter how many words in the list (i.e., the cue appeared 200-ms after the offset of the last word in seven-item lists, 1000-ms after in six-item lists, and 1800-ms after in five-item lists).

The instructions for recall were exactly the same as in Experiment 4, with the confidence of each response being indicated via two rows of boxes on the response sheet. The number of boxes in each row always equalled the number of words that were presented in that trial and all 14 words in the experimental vocabulary were permanently on display. Unlike Experiment 4 however, there was a separate response sheet for each trial. Initially, all response sheets were face down in a pile on the left of the subject. When subjects saw the cue for recall, they turned over the top response sheet from the pile, wrote their responses, and then put the response sheet face down in a pile on their right. In this way, subjects did not know in advance the length of the list on a given trial in the Variable condition until starting recall, and could not see the responses they gave in the previous trial.

Subjects received eight practice trials, two of five words, two of six words, two of seven words, and two of six words, in that order. The order of the Fixed and Variable conditions that followed was alternated across subjects. The experiment took 45 minutes.

Results

In brief, terminal protrusions were more frequent than absolute protrusions in both the Variable and Fixed conditions, irrespective of whether guesses were included or excluded. In fact, protrusions were the most common intrusion on all six positions in the Fixed condition, and were less likely to be guesses than other types of intrusion. The results were generally a close analog to those of Experiment 4.

Overall Performance

A greater proportion of six-item lists were correct in the Fixed condition (*M=.58, SD=.28*) than Variable condition (*M=.45, SD=.31*), a difference that was significant under weighted log-odds, *Z(30)=4.65, p<.0001*. As expected, the corresponding proportion was higher for five-item lists in the Variable condition (*M=.80, SD=.20*) and lower for seven-item lists in the Variable condition (*M=.20, SD=.20*). The advantage of fixed-length lists was apparent over all positions, except perhaps the last (Figure 6-3; guesses included). These
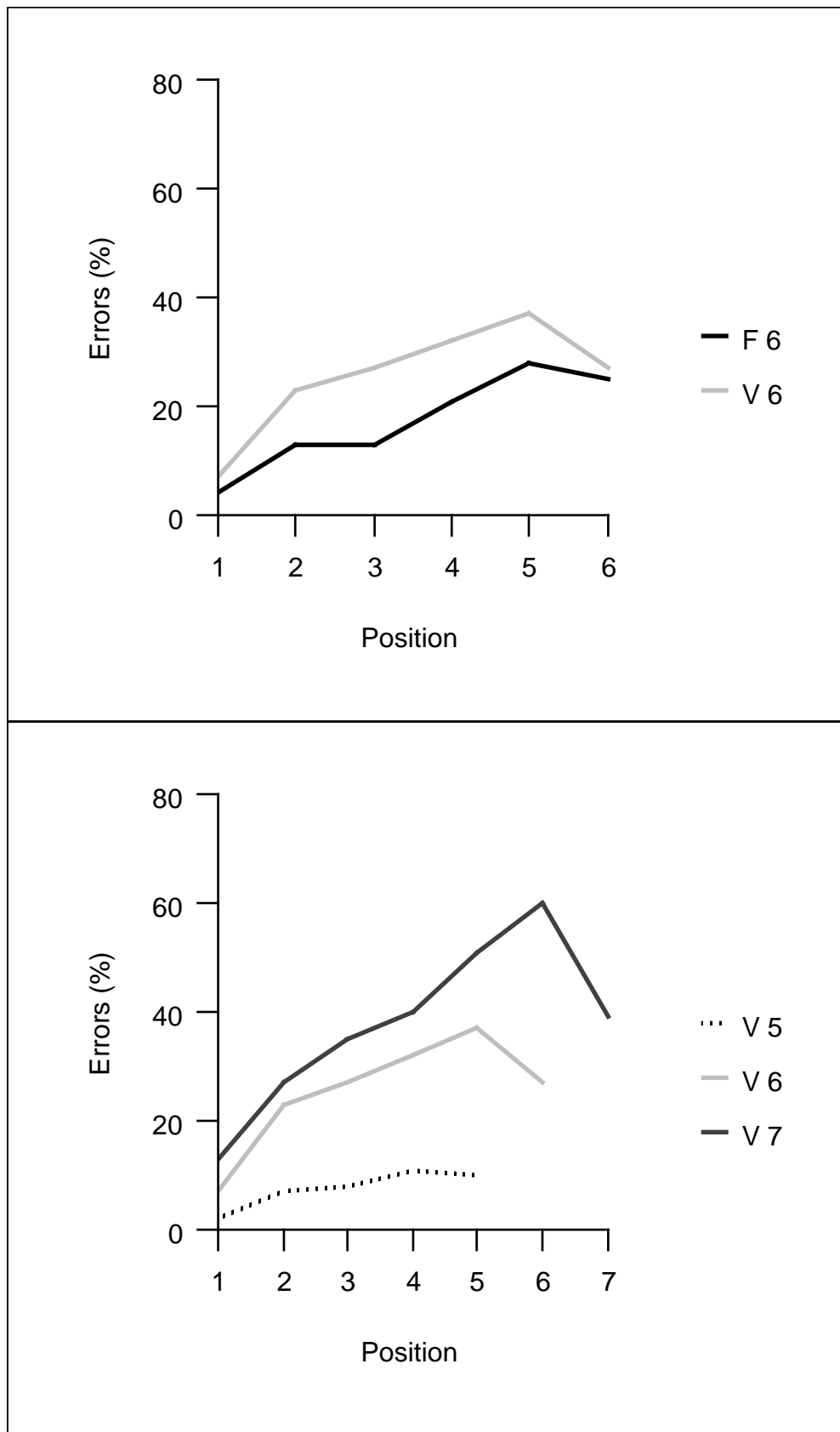
Figure 6-3: Errors by position for six-item lists in Fixed and Variable conditions (upper panel) and five-, six-, and seven-item lists in the Variable condition (lower panel) of Experiment 5. (F=Fixed; V=Variable.)

differences could be attributable to spontaneous grouping in threes, for which there was more evidence in the Fixed than Variable condition. Interestingly, recency remained strong in the Variable condition, even though the last item was not known in advance.[2]

Errors on Critical Positions

A three-way ANOVA on the log-odds of an error on either critical position showed a significant effect of condition, *F(1,203)=4.54, p<.05*, guesses, *F(1,203)=174.55, p<.001,* and position, *F(1,203)=7.35, p<.01*. There were no significant interactions, *F(1,203)<1.10, p>.30*. Errors were more frequent on the fifth position than final position, indicating a recency effect, and more frequent in the Variable than Fixed condition. Not surprisingly, excluding guesses reduced the frequency of errors (i.e., remaining transpositions and intrusions, Table 6-5).

	Variable		Fixed	
	Fifth	Final	Fifth	Final
Guesses Included	.32 (.18)	.25 (.17)	.27 (.23)	.25 (.22)
Guesses Excluded	.09 (.08)	.07 (.05)	.08 (.08)	.07 (.07)

Table 6-5: Frequency of errors on critical positions in Experiment 5.

The proportion of errors on critical positions that were protrusions from the same critical position in the previous report was calculated for the 17 subjects who made at least one error per critical position with guesses excluded. A three-way ANOVA on log-odds showed significant effects of guesses, *F(1,112)=6.48, p<.05,* and position, *F(1,112)=35.68, p<.05*, but neither an effect of condition, *F(1,112)=3.41, p=.07*, nor any interactions, *F(1,112)<2.66, p>.11*, quite reached significance. Protrusions were more frequent on final than fifth positions, and the effect of excluding guesses was to increase the frequency of protrusions, mainly on the final position (Table 6-6). Four pairwise comparisons on weighted log-odds confirmed that a significantly greater proportion of errors were terminal rather than absolute protrusions in both conditions, whether or not guesses were included, *Z(17)>2.14, family-wise p<.05*.

---

2. This is in contrast to Bunt (1976), who located the advantage of fixed length lists mainly on later positions. (Crowder, 1969, located the advantage on early positions, but with free rather than serial recall.)

	Variable		Fixed	
	Fifth	Final	Fifth	Final
Guesses Included	.13 (.09)	.20 (.14)	.16 (.13)	.22 (.14)
Guesses Excluded	.12 (.13)	.26 (.25)	.16 (.17)	.32 (.24)

Table 6-6: Proportion of errors that were protrusions in Experiment 5.
(Calculated from weighted log-odds, *n=17*.)

Immediate Intrusions

The previous analyses demonstrated that the proportion of errors that were protrusions was greater between final positions than between fifth positions. However, there were also fewer errors on final positions than fifth positions, potentially affecting the proportion that were protrusions. To overcome this problem, and compare the proportion of protrusions with chance levels, the following analyses restricted errors to immediate intrusions from the preceding report (with guesses included). The frequency of immediate intrusions, and the proportion that were protrusions, was calculated for the 22 subjects who made at least one immediate intrusion per critical position (Table 6-7).

A two-way ANOVA on the log-odds of a protrusion showed an effect of position, $F(1,63)=8.53$, $p<.005$, but no effect of condition, or interaction, $F(1,63)<1$, $p>.38$ in both cases. The effect of position was confirmed by two pairwise comparisons on weighted log-odds, which showed a significantly greater proportion of intrusions were protrusions between final than fifth positions in both conditions, $Z(22)>2.44$, $p<.05$. These differences did not owe to differences in the overall incidence of immediate intrusions, for which an ANOVA showed no significant effects of position, condition or interaction, $F(1,63)<1$, $p>.40$ in all cases.

The proportion of immediate intrusions that were protrusions was also compared to that expected by chance. In the Fixed condition, the chance proportion was .17 (given that an intruding item could come from one of six positions in the previous report); the proportion of protrusions was significantly greater than this on both critical positions, $Z(22)>4.75$, $p<.0001$. In the Variable condition, the chance proportion was not so clear (given that the previous

	Variable		Fixed	
	Fifth	Final	Fifth	Final
Immediate Intrusions	.20 (.07)	.18 (.08)	.20 (.09)	.20 (.09)
Protrusions	.27 (.19)	.37 (.23)	.30 (.23)	.42 (.24)

Table 6-7: Frequency of immediate intrusions, including guesses, and proportion that were protrusions in Experiment 5.
(Calculated from weighted log-odd*s, n=22*.)

report could contain five, six or seven items). Using the same figure of .17 as for the Fixed condition, the proportion of protrusions was significantly greater than chance on both critical positions, *Z(22)>3.55, p<.0005*. Even taking a more conservative estimate of chance of .20 (as if the previous report contained only five items), proportions of protrusions on both the fifth position and the final position were still greater than chance, *Z(22)=2.15, p<.05*, and *Z(22)=5.03, p<.0001*, respectively.

Finally, protrusions on the first position of a report in the Variable condition were predominantly from the first position of the previous report. Indeed, as a proportion of immediate intrusions (*M=.57, SD=.34*), they were significantly more frequent than the chance level of *.20, Z(23)=5.51, p<.0001*. Protrusions respected both terminal positions of reports.

Immediate Intrusions in the Fixed Condition

Immediate intrusions on all six positions in the Fixed condition, including guesses, were collapsed over subjects (Figure 6-4). Unlike Experiment 3, intrusions increased towards the end of reports (probably because subjects had to guess rather than omit in the present experiment). Otherwise, the data replicated those of Experiment 3, with protrusions being the most common intrusion for all six positions, and the proportion of immediate intrusions that were protrusions being greatest at the start and end of reports. There was some evidence for spontaneous grouping of the six items into two groups of three (e.g., many protrusions on Position 6 came from Position 3 of the previous report). This probably explains why the intrusion gradients are not as smooth as in Experiment 3. In fact, several subjects reported it
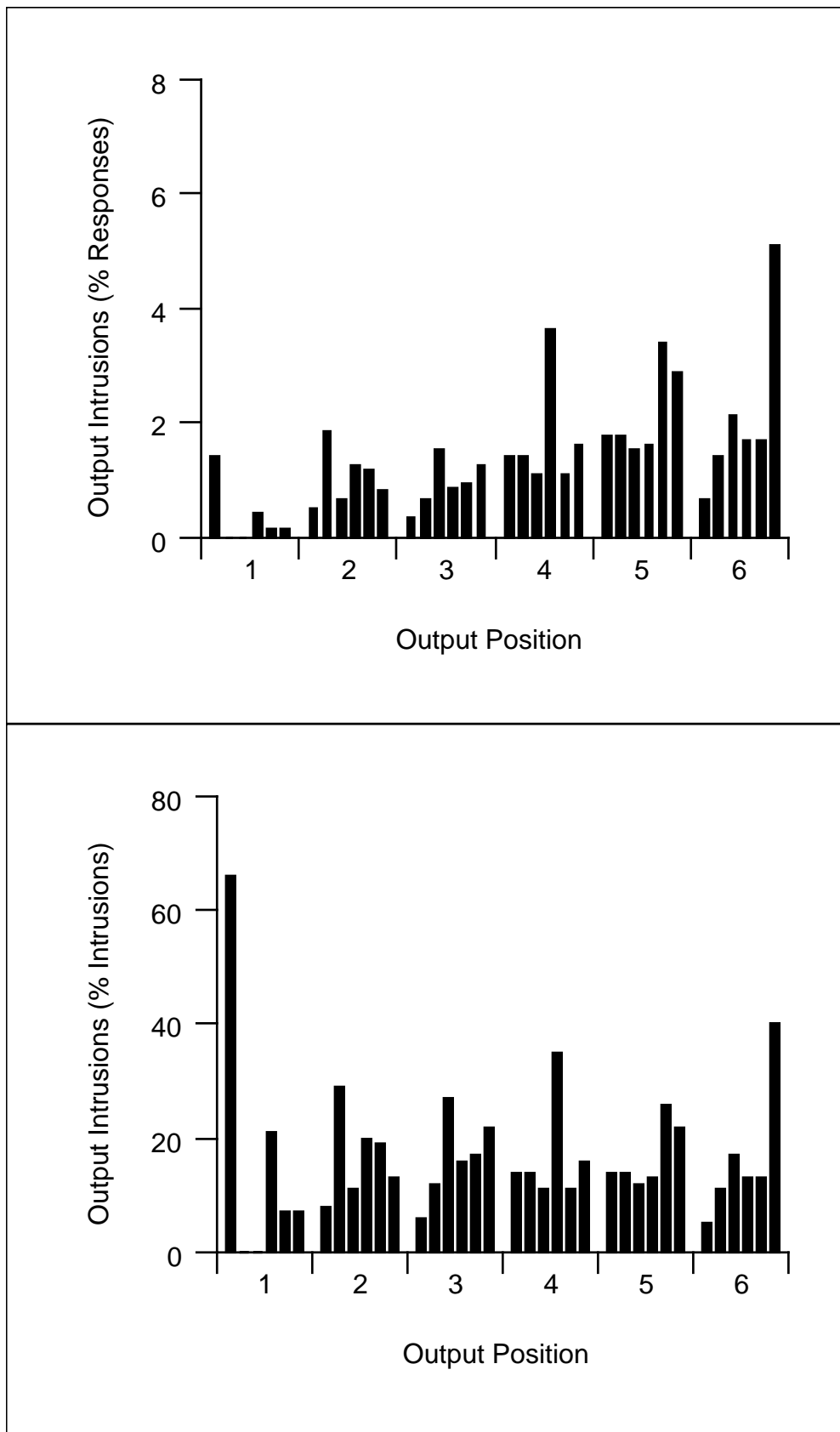
Figure 6-4: Output intrusions as a proportion of responses (upper panel) and as a proportion of intrusions per output position (lower panel) in Experiment 5.

"easier to find a rhythm" in the Fixed than Variable condition. Grouping was therefore less likely to affect the pattern of protrusions in the Variable condition.

The proportion of errors that were immediate intrusions with guesses (*M=.48, SD=.12*) was greater than without guesses (*M=.34, SD=.19*), a difference that was significant under weighted log-odds, *Z(30)=6.14, p<.0001*. This was in contrast to the proportion of errors that were protrusions with (*M=.17, SD=.09*) and without (*M=.16, SD=.13*) guesses, which did not differ significantly, *Z(30)=0.72, p=.47*. (Indeed, the proportion on final positions actually increased; Table 6-6). These results imply that subjects were more confident of an intrusion from the same position in the previous report than other types of intrusion.

Guesses

Not surprisingly, the proportion of responses that were guesses was greater in the Variable condition (*M=.22, SD=.20*) than Fixed condition (*M=.17, SD=.17*), *Z(30)=1.98, p<.05*. Again, the frequency of guesses increased towards the end of recall, paralleling the similar increase in omissions in Experiment 3, with a one-way ANOVA in the Fixed condition showing a significant effect of output position *F(5,179)=24.60, p<.001*.

As in Experiment 4, responses in the Fixed condition were split by whether or not they were correct and whether or not they were guesses. A combined test of significance of contingency tables showed an extremely high correlation between accuracy and confidence of responses, *Z(30)=39.46, p<.0001* (Table 6-8). Nevertheless, a considerable proportion of errors were not indicated as guesses (*M=.37, SD=.11*), reinforcing the conclusion of Experiment 4, that subjects are often unaware of errors, and a similar proportion were correct (*M=.37, SD=.15*), reinforcing the conclusion that guesses were more than random choices of list items (of which only .17 would be correct).

	Not Guess	Guess
Not Error	6347	505
Error	502	926

Table 6-8: Number of guesses and errors collapsed across subjects in the Fixed condition of Experiment 5.

Discussion

The present experiment showed that protrusions between reports respect terminal position rather than absolute position. This was demonstrated in the Variable condition, where the proportion of errors that were protrusions was significantly greater between the final positions of reports than between the fifth positions of reports, even when the length of lists was varied from trial to trial in an unpredictable manner. This suggests that position in a report is coded relative to both the start and end of that report, again confirming the prediction of SEM, and questioning the absolute positional information assumed in other models (e.g., Brown et al., 1996; Burgess & Hitch, 1996b).

Protrusions were also more probable between the final position of reports than the fifth position of reports in the Fixed condition. SEM predicts this because the positional uncertainty is smaller for end items, where positional coding is particularly sharp (Chapter 5). SEM also explains why the proportion of absolute protrusions, though less than that of terminal protrusions, was still greater than chance in the Variable condition: There is still considerable positional overlap between the cue for the fifth position in, say, a list of seven items and the fifth position in a list of five items (as shown in Fit 8 below).

The present results also showed that protrusions are not simply the result of guessing strategies. Removing guesses had little effect on the proportion of errors that were protrusions. Removing guesses did reduce the proportion of errors that were intrusions however. In other words, intrusions were particularly likely to be guesses, in agreement with Dillon and Thomas (1975). One reason why intrusions were not removed completely might be that some subjects were not bothering to indicate all their guesses. Alternatively, guesses may be more likely to be intrusions than transpositions simply because there is a greater chance of a guess being an intrusion than a transposition, particularly if subjects tend not guess an item they have already recalled (Chapter 7). In other words, guesses might have a higher baseline chance of being intrusions than other types of error. Both these possibilities are consistent with the distribution of guesses and errors in Table 6-8, and, unfortunately, the present experiment provides no way of clarifying this situation. What is clear is that intrusions that maintain relative position were less affected by the removal of guesses than other intrusions. This corresponds to sharpening

intrusion gradients by reducing the noise from completely random guesses (Experiment 3).

Present results represent an important replication of the output protrusions found in Experiment 3 (and Page & Norris, 1996a). Output protrusions have now been shown to occur both with and without vocalisation, with both spoken and written recall, and with immediate as well as delayed recall, confirming that positional information is ubiquitous in serial recall (Chapter 4). Moreover, the results from the Fixed condition suggest that positional coding can extend over six positions, as well as the five in Experiment 3 (though there was also evidence for spontaneous grouping of the six items in the Fixed condition). In any case, the fact that protrusions were found in immediate, serial recall of visually presented lists (a task assumed to rely predominantly on the phonological loop; Baddeley, 1986) further questions the sufficiency of the Primacy Model (Page & Norris, 1996b) as a model of immediate, serial recall. Moreover, in as far as recall of six words in the Fixed condition was within most students' spans (58% of lists being recalled correctly), this finding also questions the assumption of Tehan and Humphreys (1995). According to these authors, immediate recall is immune to proactive interference. On the contrary, proactive interference (of a positional kind) acts even on immediate serial recall of phonologically-coded, span-length lists (i.e., proactive interference is a matter of degree, rather than all-or-none).

Somewhat ironically however, the confirmation of terminal protrusions in a situation where the end of the list is unpredictable does not help interpretation of the end marker of SEM. As in Experiment 4, present results rule out any interpretation where the positional cue changes constantly over time or position, as with the internal oscillators of Brown et al. (1996) and Hitch et al. (1995). However, the present results are also problematic for an interpretation in terms of expectation for the end of a list (Chapter 5). Though an expectancy interpretation might explain better performance on six-item lists in the Fixed condition than in the Variable condition, it seems incompatible with terminal protrusions, which require an end marker that grows towards the end of list in a manner independent of list length, even when the list length is not known in advance. This issue is resumed in the General Discussion.

General Discussion

The present experiments demonstrated that substitutions between sequences of different lengths tend to maintain terminal positions rather than absolute positions, whether those sequences are reports on different trials, or groups within the same trial. These findings suggest that position within a sequence is coded with respect to both the start and the end of that sequence, confirming the prediction of the Start-End Model (Chapter 5).

The present experiments also demonstrated that positional errors are not simply the result of guessing strategies. Neither protrusions nor interpositions were any more likely to be guesses than other errors. This supports SEM's assumption that positional errors result from competition amongst responses for a particular position, rather than guesses after the correct item has been forgotten (c.f., Conrad, 1960; Dillon & Thomas, 1975). The fact that significant numbers of errors were not indicated as guesses suggests that this response competition may operate at an unconscious level, supporting the observation that people are often unaware of errors (Chapter 1). Nevertheless, it is clear that people do sometimes resort to conscious guessing when no response comes to mind, particularly in the present experiments where they had to give a response for every position. Such guesses were more accurate than would be predicted by random choices from the experimental vocabulary. The guesses in Experiment 5 were also likely to be intrusions, in agreement with Dillon and Thomas (1975), and Bjork and Healy (1974). Incorporating a role for guessing, in addition to response competition, allows SEM to explain these somewhat paradoxical results (below).

The present results support the assumption that position is coded by markers at the start and end of sequences. The start marker can explain the positional errors between the start of groups (Experiment 4) and the start of reports (Experiment 5); the end marker can explain the positional errors between the end of groups and the end of reports. Nevertheless, the important question remains: What are the psychological correlates of these markers? The start marker needs only be triggered by the first item in a sequence and could depend on any psychological variable that decreases monotonically during subsequent items, such as attention. The end marker on the other hand needs to grow steadily towards the end of a sequence. When the length of a sequence is known in advance, as for the different size groups

in Experiment 4, the end marker could quite plausibly represent expectation for the end of the group. When the length of a sequence is unknown however, as in the Variable condition of Experiment 5, such an expectancy interpretation becomes less plausible, particularly when protrusions remain between the ends of those sequences. Two possible solutions to this problem are given below.

Positional Codes Generated during Rehearsal

Positional errors between sequences are clearest when measured with respect to output position (Experiment 3; Chapter 4). In other words, they are clearest when the previous sequence reflects a recall episode rather than a presentation episode (e.g., recall of the previous group in Experiment 4, or recall of the previous list in Experiment 5). Because recall episodes are normally more recent than presentation episodes, this finding is not on its own a problem for SEM, which assumes less general contextual change for more recent episodes (Chapter 5). However, because presentation and recall are confounded with recency in this way, and because output position and input position are normally highly correlated (given that responses are usually correct), it is difficult to determine the relative influence of previous presentation episodes and previous recall episodes. Indeed, it remains possible that only recall episodes are the source of positional errors. In other words, the interpositions in Experiment 4 and the protrusions in Experiment 5 may be explained solely by proactive interference from positional codes generated during recall of the previous group or previous list. By extending the notion of recall to any form of overt or covert rehearsal, this hypothesis can even explain anticipations from later groups during recall of earlier groups (Experiment 2): Any rehearsal of the later groups before recall begins may be sufficient to generate positional codes for items in those groups, and cause interpositions during recall of earlier groups.

This *rehearsal hypothesis*, that positional codes are only generated during rehearsal, and not during presentation, has the advantage of making interpretation of the end marker easier. Because the length of a sequence is known at recall, expectation remains a possible psychological correlate. The disadvantage of this hypothesis is that it begs the question of how the order of items is stored before rehearsal begins: If positional codes are only established during rehearsal, they can not be used to order the very first rehearsal. An alternative means of

ordering items is required. Thus, if the rehearsal hypothesis were confirmed, SEM would no longer be sufficient as a model of serial recall.

There is circumstantial evidence against the rehearsal hypothesis. For example, several studies have demonstrated that people extract positional information even under incidental learning (e.g., Hintzman, Block & Summers, 1973; Toglia & Kimble, 1976; Nairne, 1991; though not as well as under intentional learning, Navey-Benjamin, 1990; Tzeng, Lee & Wetzel, 1979). However, these demonstrations used long lists and considerable delays before recall. More relevant to serial recall from short-term memory is an experiment by Estes (1991). In a condition where subjects rehearsed lists overtly during the retention interval, Estes showed that about 70% of intrusions in a rehearsal protocol were likely to be recalled at the same position in recall, in agreement with the rehearsal hypothesis. However, he also showed that about 57% of items that did not occur at the correct position in a rehearsal protocol did occur at the correct position in recall (i.e., at the same position as in the original presentation). This led Estes to propose that there are two sources of positional information, a "direct" one from the presentation episode and an "indirect" one from rehearsal episodes. (SEM can also explain this data with its assumption that every rehearsal of an item creates a new token, without the need to postulate different sources per se.) In other words, Estes assumed positional information can be generated during presentation as well as rehearsal, contrary to the rehearsal hypothesis.

However, though Estes's data suggest that two sources of information influence recall, they do not actually require both to be positional. Estes's indirect source may be positional, but his direct source need only be ordinal in order to explain why correct responses can occur in spite of incorrect positional information from the indirect source. M. P. A. Page (personal communication, 1995) observed that Nairne's (1991) data may similarly be explained by use of ordinal rather than positional information. Better evidence would come from positional errors in situations where input and output position are not positively correlated. One possibility is to use backwards recall of lists or groups (Henson, 1995), where input and output position are negatively correlated, though the processes underlying backwards recall remain unclear (Chapter 5). Another possibility is to use a part-list recall paradigm. Lee and Estes

(1981) for example showed that interpositions occurred even when recall of only one of three groups was required. However, subjects did not know in advance which group was to be recalled and so were likely to rehearse all three groups, perhaps allowing positional codes to be generated. A better approach would be to require subjects to only ever recall the first of two groups (so there is no reason to rehearse the second). If erroneous items in the first group still tended to come from the same position in the second group, such retroactive interference of positional information would refute the rehearsal hypothesis.

Finally, preliminary evidence against the rehearsal hypothesis was obtained in a recent pilot experiment by Page & Norris (1996a). Using part-list recall of one of two groups, they found evidence for positional errors even under conditions of articulatory suppression (during both presentation and recall, and with both visual and auditory presentation). In fact, positional errors seemed more prevalent than usual, and yet articulatory suppression should preclude, or at least minimise, rehearsal (Baddeley, 1986). Further experiments are required to confirm these findings and test the rehearsal hypothesis more rigorously.

Positional Codes Generated during Presentation

Given no conclusive evidence for the rehearsal hypothesis, and preliminary evidence against it, SEM's assumption that position is automatically coded during presentation, rehearsal and recall will be maintained. The question remains however as to how position is coded relative to the end of a sequence, when the end of a sequence is unknown in advance. Several possible solutions are outlined below.

One possibility is that the end marker does not grow in strength until the very last item, when the end of the sequence is finally confirmed. A similar suggestion was made by Houghton (1990), whose end node was only triggered by termination of a sequence. By assuming further that presentation of items left them transiently activated in memory, the triggering of the end node allowed it take a "snap-shot" of a recency gradient of decaying activations of the last few items in the list. By growing in strength more gradually during recall, this allowed the end node to exert an influence on items earlier in the list. The problem with this solution however is that it does not allow positional tokens to be created until the end of presentation. This is contrary to the assumption of SEM, that a position-sensitive token is

generated as soon as each new item is presented.

A more suitable approach within SEM is that, when the length of a sequence is unknown, the end marker might grow in a fixed manner. This growth is irrespective of exact sequence length (though in a manner that might make some allowance for the expected range of lengths). For example, on the very first trial, the strength of the end marker might grow by a constant amount during presentation. (Alternatively, no end marker might be employed, given that SEM can still recall a sequence correctly without an end marker; Chapter 5). During subsequent trials however, when a subject has induced the range of possible list lengths, the end marker might grow more quickly, reaching its maximum value as soon as it is possible for the list to end (e.g., after the fifth item in the Variable condition of Experiment 5). It might then stay at that maximum value during any remaining items in the sequence, reflecting the subject's expectation that the sequence will end soon.

During recall however, when the length of the sequence is known (particularly if the correct number of boxes are provided for recall), the end marker can behave as previously assumed, growing continually towards the end of recall, and reflecting more accurate expectation for the end of the report. Because the end marker behaves differently during presentation and recall, there will be a greater positional uncertainty for the last few items in the list. This is consistent with the finding in Experiment 5, that six-item lists are recalled less well in the Variable condition than the Fixed condition. However, there will still be considerable overlap between the cue for the last position and the token created at the last position in the previous report (Chapter 5). Thus, protrusions will still be more likely between terminal positions than absolute positions when measured with respect to output position.

In summary, it is possible to maintain an expectancy interpretation of the end marker, with a hazy notion of expectancy during presentation that is refined during recall. A hazy notion of expectancy can be modelled by an end marker that grows in a constant manner, irrespective of exact list length. Though this entails the end marker behaving differently during presentation and recall, the more accurate recoding of positions during recall still predicts significant numbers of terminal protrusions from the previous report. This was the approach taken below, in fitting SEM to data from the present experiments.

Fits of the Start-End Model

SEM was fitted to data from Experiment 4 and Experiment 5, including modelling the effect of guesses.[3] This entailed an additional assumption about the nature of guesses.

Modelling Guesses

In the present experiments, subjects were asked to indicate both explicit guesses and uncertain responses. In SEM, explicit guesses are assumed to arise after an item's phonological representation has been retrieved. If the activation of that representation does not exceed a guessing threshold, T_G, then an item is guessed instead. (In previous experiments, omissions may have resulted in such cases, which were modelled by activations below the omission threshold, T_O; Chapter 5.) A guess is chosen by competition amongst phonological representations, on the basis of their current activation and suppression, together with additive Gaussian noise with standard deviation G_G (Appendix 3). Thus guesses are random choices from the set of recently perceived items, with a bias against those recently recalled, owing to suppression. The bias against items recently recalled reflects the fact that people are often reluctant to repeat themselves (Chapter 7). Because phonological activations tend to be lower for later items, guesses will increase towards the end of recall, in a manner similar to omissions (Chapter 5). Furthermore, with guesses predominantly at the end of recall, suppression of previous responses will tend to preclude most list items, meaning that many guesses will be intrusions (Experiment 5).

However, not all responses indicated as guesses in the present experiments were likely to be explicit guesses. A considerable number may have been items that came to mind, but not readily enough for subjects to be certain of them. If subjects were obeying instructions, these responses would be indistinguishable from "true" guesses in the present experiments (see Experiment 8). Such uncertain responses can be modelled as phonological activations that do not exceed an uncertainty threshold, T_U (Appendix 3). Because most such responses, though weakly active in memory, will nevertheless be correct, a significant proportion of responses indicated as guesses will be correct (Experiments 4 and 5). When simulating the removal of

---

3. Modelling guesses seems more appropriate than "correcting" the data for guesses (e.g., Sperling & Melchner, 1976; Drewnowski, 1980a), because the latter would seem impossible to achieve in an atheoretical manner.

uncertain responses in SEM, T_U is set above T_G, and any item whose phonological activation drops below T_U is removed. Thus, the two new parameters, T_G and T_U, replace the old parameter T_O. By setting $T_G>0$ and $T_U=0$, SEM simulates the inclusion of guesses; by setting $T_U>T_G>0$, SEM simulates the exclusion of guesses.

Fit 7: Interpositions in Variable Groups

The multiple-trial version of SEM was fitted to all three conditions of Experiment 4, without and without guesses. Most parameters, such as C_P, C_D, C_R, and C_I, were fixed by the experimental design (Appendix 3). Specifically, $C_P=C_R=1$ reflected contextual change and phonological decay during presentation and recall of each item, and $C_D=C_I=0$ reflected the immediate recall and unfilled intertrial interval. The parameters $G_G=G_P=0.30$ and $T_O=0.00$ were fixed. This left 5 free parameters, eventually set to $D_G=0.10$, $M_G=0.95$, $G_C=0.06$ and $T_G=0.90$, while $T_U=0.00$ or $T_U=1.10$ was varied to fit the inclusion or exclusion of guesses. Remaining parameters were the same as in Fit 5.

Two simulations of SEM were run on the 720 lists given to subjects in the Ungrouped condition, one with $T_U=0.00$, to simulate the inclusion of guesses, and one with $T_U=1.10$, to simulate the exclusion of guesses. Responses were then split by whether or not they were correct and whether or not they were guesses (Table 6-9; cf. Table 6-4). Of the guesses, .28 were correct, and of the errors, .41 were not guesses (the corresponding figures over 100,000 trials were .30 and .41 respectively). These figures are close to those in Experiment 4 and Experiment 5, supporting the assumption that guesses include both explicit guesses (below T_G) and uncertain responses (below T_U). In other words, SEM's treatment of guesses appears to provide a reasonable approximation of subjects' behaviour.

	Not Guess	Guess
Correct	2670	447
Error	781	1142

Table 6-9: Number of guesses and errors in Ungrouped condition from SEM in Fit 7.

Six further simulations were run on 100,000 copies of the same lists given to subjects, to fit each condition with and without guesses. With guesses included, SEM recalled .17 of

lists in the Ungrouped condition correctly, .38 of lists in the Grouped 3-4 condition, and .37 in the Grouped 4-3 condition. With guesses excluded, the corresponding figures were .06, .26 and .26 respectively. Overall performance was therefore reasonably matched to the data, though slightly worse for ungrouped lists. This is probably attributable to spontaneous grouping in the Ungrouped condition of Experiment 4, as is common with supraspan lists (Chapter 3). SEM also reproduced the error position curves, with an RMSE over 42 data points of 7.22% (Figure 6-5).

The proportion of errors on critical positions that were interpositions showed a reasonable quantitative fit to the data (Table 6-10; cf. Table 6-2), though the pattern was more pronounced in the model than the data, particularly without guesses. This may reflect more noise in the data than was captured by SEM's assumption about uncertain responses (and, in the ungrouped case, the presence of spontaneous grouping in the data). Nonetheless, the RMSE of *10.58%* over the 16 (untransformed) data points was not a reliable difference, given the variability in the data, $T^2=62.43, F(16,6)=0.52, p=.86$. The most important point was that SEM reproduced the significant aspect of the data, that terminal interpositions were more frequent than absolute interpositions.

	Grouped		Ungrouped	
	Third	Final	(Third)	(Final)
Grouping 3-4				
Guesses Included	.12	.28	.08	.14
Guesses Excluded	.08	.38	.02	.06
Grouping 4-3				
Guesses Included	.18	.23	.14	.13
Guesses Excluded	.13	.31	.06	.06

Table 6-10: Proportion of errors that were interpositions from SEM in Fit 7.

Including guesses and collapsing across the grouped conditions, as in Experiment 4, between-group repetitions were slightly more common than in the data. Nonetheless, the
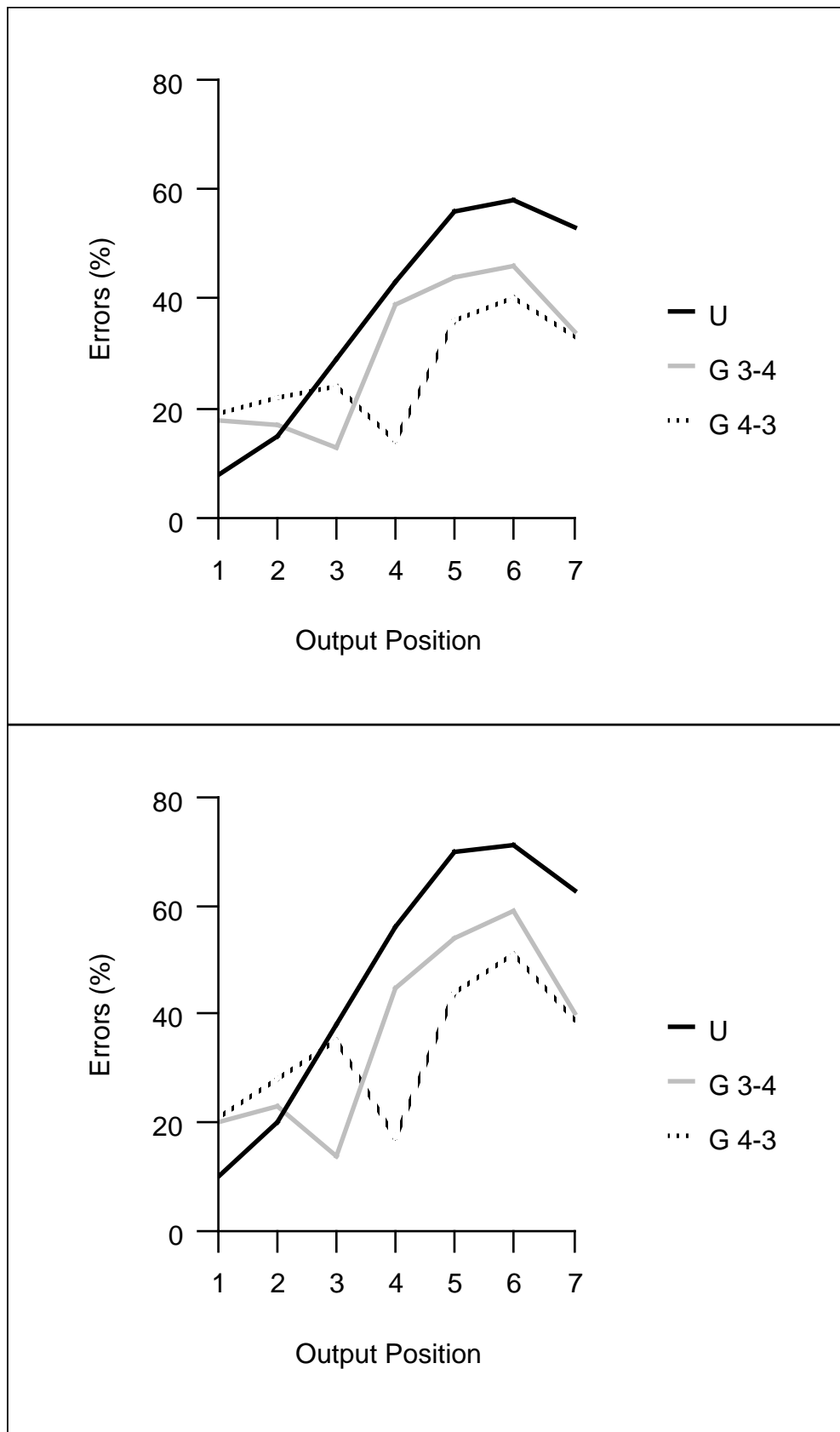
Figure 6-5: Errors by position with guesses (upper panel) and without guesses (lower panel) from SEM in Fit 7.

(U=Ungrouped, G=Grouped.)

proportion that were interpositions was very similar to the data (Table 6-11; cf. Table 6-3). The RMSE over the 8 (untransformed) data points was *8.58%*, which was extremely good, given the noise associated with the small numbers in the data. This pattern of repetitions between groups stems from SEM's assumption that each response is recoded in its output position (Chapter 5). When an item is recalled at the end of the first group, the new token created will be strongly cued again at the end of the second group. Not only does it share the same code for within-group position, but its more recent encoding means its general context will overlap more with the recall context than other, as yet unrecalled tokens. Thus, providing the token is cued strongly enough to overcome the suppression of its type representations a few responses earlier, its repetition at the end of the second group is quite likely.

	Grouped		Ungrouped	
	Third	Final	(Third)	(Final)
Between-group Repetitions	.26	.25	.23	.28
Interpositions	.23	.41	.26	.25

Table 6-11: Frequency of between-group repetitions and proportion that were interpositions from SEM in Fit 7.

Fit 8: Protrusions in Variable Lists

The multiple-trial version of SEM was fitted to both conditions of Experiment 5, without and without guesses. All parameter values were identical to the Ungrouped condition of Fit 7, except the value $G_C=0.01$. This one degree of freedom was to accommodate differences in the experimental procedure. A new parameter N_M was also introduced. The value $N_M=5$ was fixed by the experimental design and reflected the minimum list length expected by subjects in the Variable condition (given that lists varied from five to seven items). This meant that the end marker coding the positions of items in the list grew exponentially to a value $E_{0,I}=0.60$ during presentation of the fifth item, and then stayed constant at that value during presentation of any further items (Appendix 3). During recall, when the list length was known, the end marker behaved as normal (Figure 6-6).
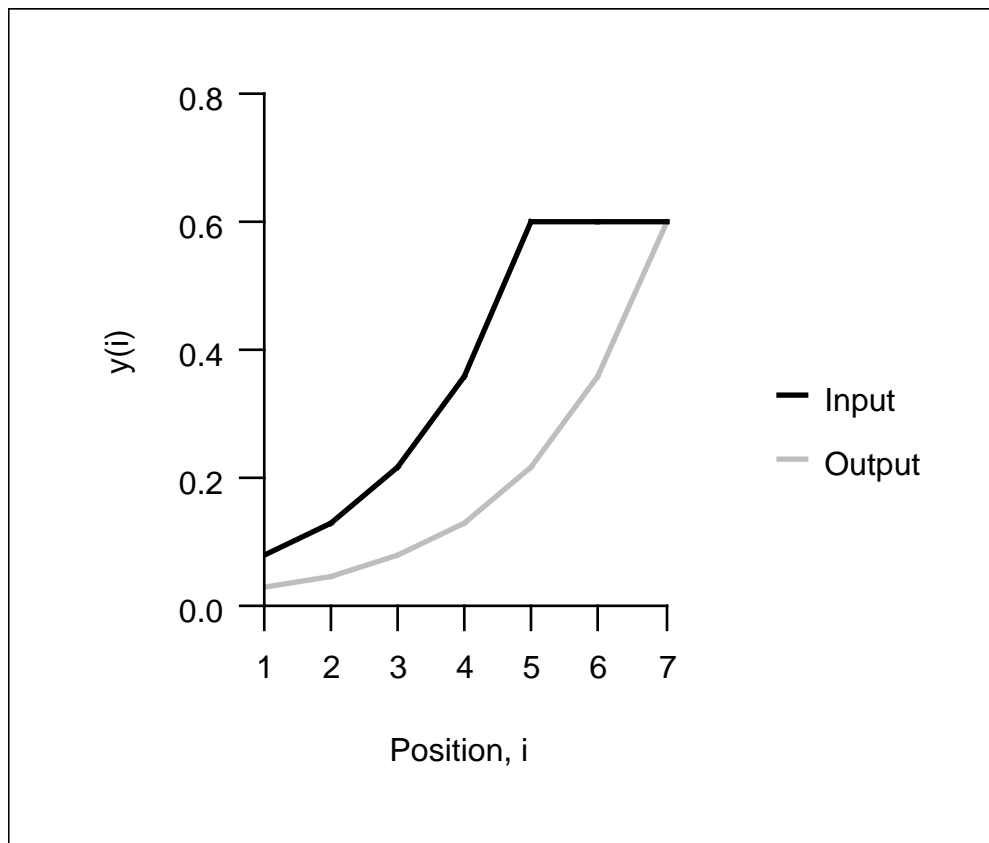
Figure 6-6: Strength of the end marker during presentation (input) and recall (output) of an unpredictable seven-item list in the Variable condition of Fit 8.

Four simulations were run on 100,000 copies of the same lists given to subjects, to fit each condition with and without guesses. In close agreement with both Fit 7 and the data, .34 of guesses were correct and .38 of errors were not guesses. With guesses included, SEM recalled .57 of lists correctly in the Fixed condition, and .80 of five-item lists, .35 of six-item lists and .04 of seven-item lists in the Variable condition. With guesses excluded, the corresponding figures were .33, .64, .18 and .01. Some of these figures were lower than in the data, but the important trends were present, including better performance on six-item lists in the Fixed condition than the Variable condition. SEM also produced similar error position curves, with an RMSE over 48 data points of *10.56%* (Figure 6-7).

In agreement with the data, removing guesses from the Fixed condition decreased the proportion of errors that were intrusions, from .38 to .26, but not the proportion that were protrusions, which increased slightly from .18 to .20. Thus SEM's assumptions about guessing
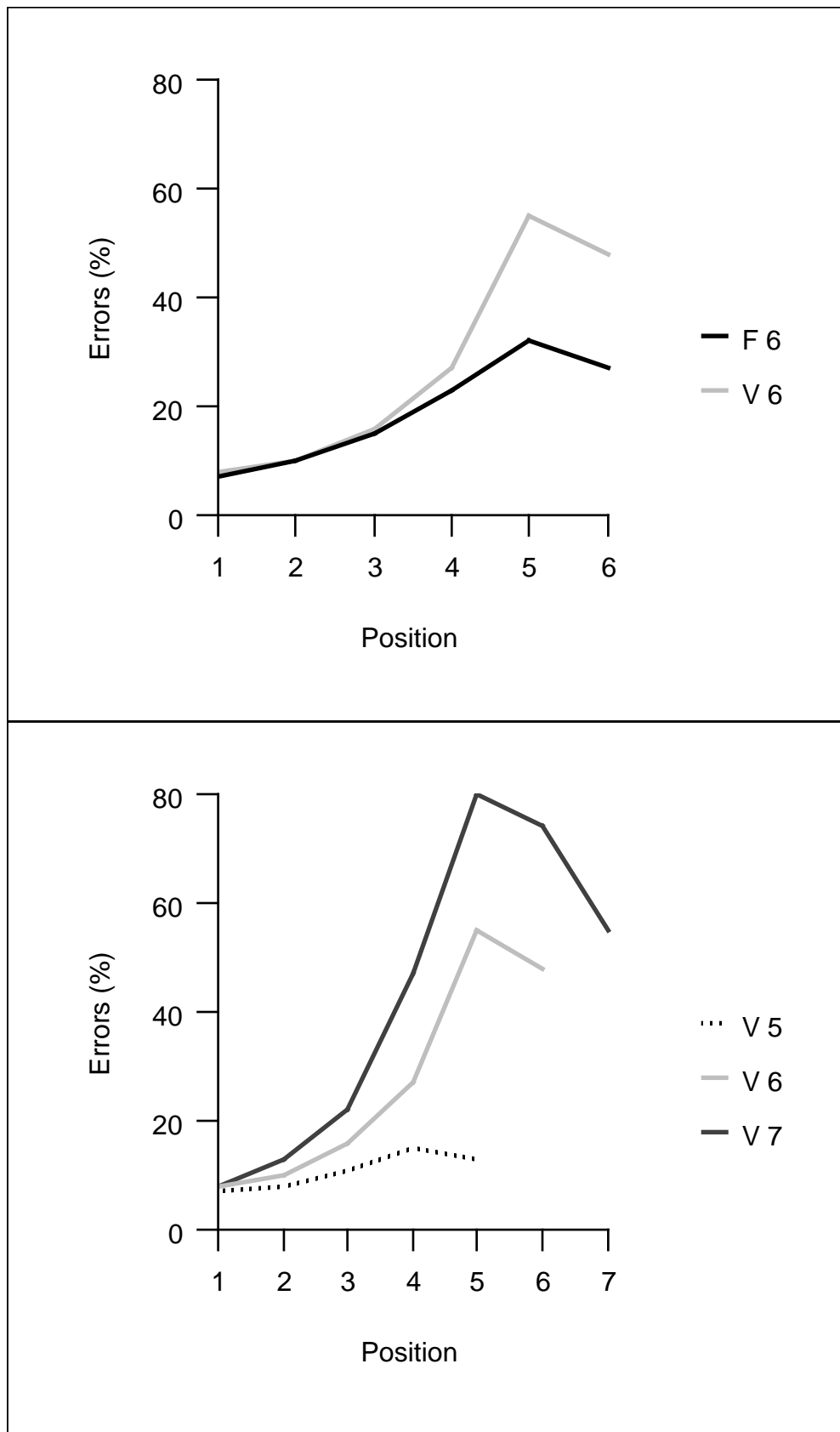
Figure 6-7: Errors by position for six-item lists in Fixed and Variable conditions (upper panel) and five-, six-, and seven-item lists in the Variable condition (lower panel) from SEM in Fit 8. (F=Fixed; V=Variable.)

can explain these somewhat paradoxical results, by producing intrusions that arise from both random guesses (most intrusions) and from response competition (mainly protrusions).

The frequency of immediate intrusions, including guesses, was slightly lower than in the data. Nevertheless, the proportion that were protrusions showed a reasonable quantitative fit (Table 6-12; cf. Table 6-7). The proportion of protrusions on the last position in the Fixed condition was higher than in the data, though again this may reflect the effects of spontaneous grouping in the data. The RMSE over the 8 (untransformed) data points was *6.67%*, a difference that was not reliable given the variability in the data $T^2=0.75, F(8,14)=0.16, p=.99$. The most important point was that SEM reproduced the significant aspect of the data, that terminal protrusions were more frequent than absolute protrusions, attributable to SEM's assumption that position in a list is coded relative to the end of that list.

	Variable		Fixed	
	Fifth	Final	Fifth	Final
Immediate Intrusions	.13	.08	.08	.13
Protrusions	.22	.33	.33	.56

Table 6-12: Frequency of immediate intrusions, including guesses, and proportion that were protrusions from SEM in Fit 8.

In summary, the assumption of an end marker geared to the minimum expected list length provided a reasonable fit to the data, together with a plausible explanation for the general impairment in the Variable condition (though a greater ease of spontaneous grouping in the Fixed condition is equally plausible). The fit was therefore a reasonable first approximation to modelling unpredictable lists in SEM, and demonstrates how an expectancy interpretation of the strength of the end marker might be maintained.

Future Work

Though the present finding that positional errors respect terminal positions of sequences suggests that relative rather than absolute position is coded, the concept of relative position includes more than just terminal positions. To demonstrate relative position more

generally, it would be necessary to test whether positions in the middle of sequences are also coded relative to the start and the end of those sequences. For example, coding of relative position predicts that the middle item of a sequence of three (Item 2) is likely to substitute with the middle item of a sequence of five (Item 3). Such a finding would confirm that present results reflect more than something special about the first and last item in a sequence. Preliminary support for relative position comes from a study by Banks, White and Mermelstein (1980), who showed that, in judgements of relative order, an item added to the middle of a four-item list immediately behaved like the middle item of a five-item list.

However, SEM does not necessarily predict that relative position is symmetrical with the respect to the start and end of a sequence. The exact overlap between positional codes for middle positions depends on the particular parameter values of the start and end markers. With an end marker weaker than the start marker (Chapter 5), the middle item of a sequence of three might be more likely to substitute with the second item of a sequence of five, because the influence of the end marker on these positions will be less than that of the start marker. Further experiments will therefore not only help clarify the issue of relative position and its symmetry, but may also help determine the relative strengths of the start and end markers.

Chapter Summary

Two experiments demonstrated that positional errors between sequences of different lengths respect both the start and the end of those sequences, confirming one of the core assumptions of SEM. These errors were not simply position-sensitive guesses. These results were predicted by SEM, and are problematic for all other models of serial recall. Nevertheless, the demonstration of positional errors between the ends of sequences, even when the ends of those sequences are unpredictable, prevents any simple interpretation of the end marker in SEM. Two more subtle interpretations were suggested, one of which was implemented in the model and fitted to the present data. Future work may help clarify the nature of the end marker and, in particular, test 1) whether position is really coded during presentation as well as recall, and 2) whether position is truly relative (i.e., extending to more than just terminal positions). The next chapter examines a different core assumption of SEM, that the order of items in short-term memory is stored over token representations.