Improving Recall by Recoding Interfering Material at the Time of Retrieval

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Our experiments demonstrate that interference of an interpolated list of items with recall of an original list can be substantially reduced by forming Ss just before testing how to reorganize and simplify the interpolated material. In Experiments 1 and 2, Ss better recalled an initial serial list of letters when informed at testing that an interpolated list spelled a certain phrase backward. Similarly, in Experiments 3 and 4, Ss better recalled an initial list of cities when told that the interpolated cities were also names of former U.S. presidents. Control experiments rule out several simple explanations. In contrast to an editing hypothesis, the postorganizing clue helped recall even when problems of list differentiation were minimized. Current memory models appear unable to explain this benefit of a postlearning clue that enables Ss to segregate the interpolated material from the to-be-remembered material.

Interference is a widely accepted cause of forgetting: people's recall of some material can be reduced if before or after learning it they learn similar material. One set is said to interfere with recall of the other and more so the more similar the two sets. Although the empirical facts of interference are widely accepted, the exact conditions and theoretical mechanisms governing such phenomena are still being clarified. The present studies add one more piece to this puzzle.

Accepted principles of interference have been summarized in the cue-overloading hypothesis (Watkins & Watkins, 1975), which states that a cue for recall of a particular unit becomes less effective the greater the number of alternative memory-units associated to that cue, and the less distinctive it is from cues associated to other units. The fact that more units produce more interference is demonstrated by the standard list length effect (e.g., Murdock, 1962) and fan effect (e.g., Anderson, 1974). Importantly, functional memory units (called chunks) produce nearly equal interference regardless of their size. A study by Brent (1965) is relevant to this point. He found that interference caused by learning an interpolated list of items with recall of an original list can be substantially reduced by forming Ss just before testing how to reorganize and simplify the interpolated material. In Experiments 1 and 2, Ss better recalled an initial serial list of letters when informed at testing that an interpolated list spelled a certain phrase backward. Similarly, in Experiments 3 and 4, Ss better recalled an initial list of cities when told that the interpolated cities were also names of former U.S. presidents. Control experiments rule out several simple explanations. In contrast to an editing hypothesis, the postorganizing clue helped recall even when problems of list differentiation were minimized. Current memory models appear unable to explain this benefit of a postlearning clue that enables Ss to segregate the interpolated material from the to-be-remembered material.

The second half of the cue-overloading hypothesis suggests that interference will be reduced the more the cues given during retention testing more clearly specify or index the desired targets rather than the interfering material. A finding of Tulving and Psotka (1971) provides an illustration: their subjects learned an initial list of categorized words then learned either 0, 1, 2, 3, or 5 other lists from different categories. When asked to recall words from the initial list, subjects' recall was worse the more intervening lists they studied. However, the decrement was largely due to subjects failing to retrieve entire categories. When cued with the unique categories of the initial list, their recall was vastly improved and showed little decrement due to the intervening lists. Thus, the retrieval cue of "the initial list" became less distinctive or more confused as more lists were interpolated; in contrast, a category cue such as "birds" mentioned in the initial list" specified or indexed more precisely the items to be recalled almost independently of the number of other lists learned.

In the Tulving and Psotka (1971) experiment, subjects were fully aware of the categorized nature of the list items during initial learning and later recall. Some evidence suggests that a more specific category cue given at recall may benefit recall even if it was not explicitly noted during initial learning. Such a finding arose in an experiment by Gardner, Craik, and Birtwistle (1972) demonstrating delayed "release from proactive interference." Their subjects studied triads of words then recalled them after 30 s of distracting activity. The words belonged to one subcategory of a large taxonomic category (e.g., garden flowers as a subset of flowers and outdoor sports as a subset of sports). On a critical trial, the presented items came from a different subcategory (e.g., wildflowers or indoor sports). Subjects who continued to encode the items during presentation as "just more flowers" (or "just more sports") recalled them poorly. Subjects who were instructed to encode the items more specifically as "wildflowers" (or "indoor
trials, but was shifted to the more specific subcategory (e.g., the time of retrieval after the distractor activity: that cue was critical. Group of subjects always received their category cue at sports."

Recalled the items far better than did the former flowers or sports, or the general category (e.g., for the first 3 groups, subjects also showed substantial release from proactive interference, recalling as well as did subjects who had been given the more specific cue during initial learning of the critical items.

This last result of Gardiner et al. (1972) shows that items originally stored or indexed in a manner that would lead to interference in their recall can avoid that fate if the cues given at retrieval provide a more specific index. This curious phenomenon illustrates one way human memory differs from mechanical information retrieval systems: for example, in a library filing system, a text originally indexed only under the general title of "engineering" would not be singled out for retrieval by a later request (cue) that stipulated more information about it, such as "structural engineering for bridges." The principle of encoding specificity (Tulving & Thomson, 1973) claims that in order for an index cue to be effective in retrieving a memory item, the relation of that index to the item must have been noticed and recorded (stored in memory) at the time the item was originally processed. The Gardiner et al. result can be made consistent with that principle if it is assumed that (a) subjects index the items during original study using a variety of general and more specific category indices; but (b) left to their own devices, at retrieval subjects cue themselves for recall using only the more available, general category indices (e.g., flowers or sports). It is the second step that produces the cumulative interference and is circumvented when the experimenter supplies a more specific retrieval cue.

In the Gardiner et al. (1972) experiment, the more specific cue indexed the items to be recalled. Reflecting on that result and the results of Brent (1965) and Bower (1969) reviewed earlier, one may wonder whether a late-arriving clue that organizes and reindexes some interfering material after it has been learned can reduce its power to interfere with recall of other, target material. If, after learning interfering material, a clue can be provided that enables subjects to reorganize that material into a smaller number of integrated memory units or to revive and reindex that material so as to distinguish it better from the to-be-recalled items, then enhanced recall of the target items might be anticipated. The question is whether such a rewriting of a learning-and-interference history is possible without the person reviving and reindexing the earlier interfering experiences. The possibility of such mental maneuvers was suggested by some intriguing results of an experiment by Zimmerman (1954).  

Zimmerman's (1954) subjects learned, in serial order, an initial list of 21 random letters and then learned a second, interfering list of 21 letters. Unbeknownst to subjects, the interfering list was the phrase wealthy bankers holiday spelled backward without spaces. Some subjects were told this clue before they studied the second list, and these subjects remembered more of the original random list on a subsequent recall test than did subjects who were not informed of the clue (70% vs. 15% of the letters recalled from List 1, respectively). This result is similar to Brent’s (1965) results and is consistent with the number of chunks hypothesis about interference. Most interesting were the results from other subjects who were not given the clue until after learning List 2 and just before they received a retention test on List 1. Surprisingly, these subjects recalled the original list nearly as well as the subjects who had received the clue before learning (62% vs. 70% of the letters, respectively).

Zimmerman’s (1954) conclusion was that clues that enabled the interfering materials to be reorganized helped segregate those materials so they were no longer confused with the original material that subjects were trying to recall. How is this possible? How could clues given after the interfering learning be used to reorganize that material unless subjects, in some sense, revive and reindex the interfering items, thus segregating them from the to-be-remembered items? One procedural point of Zimmerman's experiment might be crucial to her result: After giving the subjects the clue and before asking for List 1 recall, she briefly showed them the interfering list of letters so that they could verify that it indeed spelled wealthy bankers holiday backward. Perhaps that brief experience sufficed for subjects to completely reindex List 2 in memory, partitioning it off from the original list. The potential importance of that minor procedural feature was investigated in Experiment 1.

**Experiment 1**

Experiment 1 attempted to replicate Zimmerman’s (1954) basic experiment but with a crucial change: After hearing the clue orally, subjects were not permitted to examine and restudy List 2 visually using that reorganizing clue. Instead, after receiving the spoken clue, subjects moved directly onto recalling List 1. Conceivably, preventing subjects from explicitly reorganizing the interfering list will wipe out the effect of the post facto information.

Experiment 1 also tested another explanation of the postinformation advantage. That explanation suggests that subjects who are told the reorganizing clue realize that they would thus be able to regenerate List 2 easily, and therefore they could set aside that list (i.e., not worry about it) and instead focus all their retrieval efforts on List 1. An effect similar to this was demonstrated by Epstein (1970; see also, Bjork, 1978) in an experiment in which subjects studied two short lists of words, and then they were told whether they were to recall both lists in a particular order or only a specified one of the lists. Recall of a list when it was the only one being recalled was significantly better than recall of the same list when it was the first of two lists being recalled. However, this effect only occurred when the subject was recalling the most recently presented list from immediate memory, so it is unclear whether this short-term factor would generalize to explain
Zimmerman's (1954) result obtained at a longer retention interval.

Our experiment was designed to determine whether subjects who are told the organizing clue recall more of the original list simply because they are able to not worry about having to later recall the interfering list. To test for this effect, one group of subjects was told just before recall that they would only be tested on the original list thus relieving them of concern about having to recall the other list later. If they recall as many words from List 1 as does the group that was given the clue, then it would indicate that the clue only helps by allowing subjects to stop worrying about having to recall List 2.

Method

Subjects. The subjects were 106 Stanford University undergraduates who participated in Experiment 1 for course credit in Introductory Psychology. They were tested in groups of 6 to 10 subjects, and each group was assigned to one of three conditions. The condition of each group was randomly selected at the beginning of each session under the constraint that each condition was run one time per week for 3 weeks, and that sample sizes were approximately equated at each week's end. At the end of the experiment there were 28 subjects in the rest-control group, 23 in the informed group, 29 in the uninformed/first group, and 26 in the uninformed/both group.

Materials. List 1 was composed of 21 letters presented in a vertical column on the page in the order SOJNUGPAHWMSE-LICBQTA. List 2 was composed of 21 letters in the columnar serial order YADILOHISREKNABYHTLAEC, which spells the phrase wealthy bankers holiday when read backward without spaces (taken from Zimmerman, 1954). Eleven of the letters in List 1 also appear in List 2. Each list was typed in a vertical column on a separate sheet of 4 in. × 5 in. (10.16 cm × 12.70 cm) paper.

Procedure. Subjects performed each task on a separate page of their individual booklets. During learning, subjects initially had 30 s to study their copy of List 1. They then turned to a blank page and were given 1 min to write all the letters they could remember in a column of blank spaces provided in their answer booklet. Subjects could recall the letters in any temporal order so long as they were entered in the correct spatial location in the column of recall slots. This study-then-recall procedure was then repeated for a second trial. Immediately after learning and recalling List 1, subjects in the rest-control condition engaged in a filler task (explained in the next paragraph), and subjects in the three experimental groups proceeded to study and recall List 2 for two trials using the same procedure. These subjects then worked on the same filler task but for a shorter time than the rest-control subjects. The overall time between List 1 learning and its final retention test was equated at 6 min for all four groups.

The filler task consisted of reading The Far Side cartoons (by Gary Larson) presented using an overhead projector. Subjects were asked to rate each cartoon for degree of humor on a 10-point scale (10 = very funny and 1 = not funny). Three cartoons were shown on each overhead. Informed and uninformed subjects were given 3 min to rate 9 cartoons, whereas rest-control subjects were given 6 min to rate 15 cartoons. The 3-min difference equals the approximate learning time for List 2 for the former subjects.

After the filler task, all subjects were given 2 min to recall by writing in a column in correct order as many letters as they could from List 1. Before performing the recall task, subjects in the informed condition were told that "List 2 was the phrase wealthy bankers holiday spelled backward." Although subjects were told this, the experimenter did not show List 2 again (as had Zimmerman, 1954) or give subjects sufficient time to mentally review List 2 to verify this fact. Subjects proceeded to recall List 1 immediately after receiving this clue.

In the uninformed/first condition, subjects were told that they were to recall only List 1 and that they could "forget about List 2" as its recall would not be tested. In the uninformed/both condition, subjects were not told the organization of List 2 but were told immediately before recall that they would be responsible for recalling both List 1 and List 2, and they would start with recall of List 1. In fact, however, after they recalled List 1, the experiment was terminated, and they were not asked to recall List 2.

Following the unpaced serial-recall task, all subjects completed a paced, item-by-item serial-recall task in which they were shown successive letters from List 1 one at a time and they were instructed to write the letter that followed from the list. To start the procedure, subjects were asked to write the first letter of the list. Within 5 s they were told the correct first letter of the list, copied it on their answer sheet, and were then given 5 s to write the next (second) letter. Then the second letter of the original string was given, and the subjects copied it and were given 5 s to recall the next (third) letter. This procedure was continued through the rest of the list. After this paced recall test, subjects were debriefed and dismissed.

Results and Discussion

For each subject, unpaced serial recall was scored as the number of letters recalled from List 1 in their correct absolute serial positions. Paced recall was scored in the same absolute-position manner. Group averages are shown in Table 1.

Turning to the first question: Did telling subjects to forget List 2 improve their recall of List 1? The answer is clearly no. The last two rows of Table 1 show that unpaced recall of the uninformed group expecting to recall only List 1 was close to recall of List 1 items by uninformed subjects expecting to recall both lists, $t(52) = .13, p > .3$. We may thus conclude that subjects are not much influenced, while recalling List 1, by whether they expect to recall List 2 later. Accordingly, this conjectured apprehension (or its lack) cannot be used to explain any benefit caused by the clue that reorganizes List 2. Because the uninformed/first and uninformed/both groups had equivalent List 1 recall, their results are pooled for the later analyses.

We turn now to the primary question: Can the postinformation benefit observed by Zimmerman (1954) be replicated? Examining first the unpaced recall percentages in Table 1, an

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<th>Table 1 Percentage of Items Recalled in Correct Serial Position and Standard Deviations by Groups and Test Conditions in Experiment 1</th>
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overall analysis of variance (ANOVA) combining the two uninformed groups yielded significant group differences, \( F(2, 103) = 8.22, p < .001 \). To assess pairwise group differences, Duncan’s multiple-range test requires a difference between adjacent percentages of more than 15.9% for significance at the 5% level. By this test, the informed subjects recall significantly more than the combined uninformed subjects, though not significantly less than the rest-control subjects. An alternative perspective on this outcome is to say that the postorganizing clue reduced retroactive interference from 36% to 13%.

Turning next to the paced serial-recall scores in Table 1, we used an overall ANOVA to detect significant group differences, \( F(2, 103) = 7.52, p < .001 \). Duncan’s test required the difference between adjacent means to exceed 12.7% to be significant at the 5% level. By this measure, none of the adjacent means differ; thus, the postorganizing clue enhanced recall to an intermediate level between the other two groups. The uninformed subjects did recall reliably less than the rest-controls. Duncan’s test for means two ranks apart required a difference of 16.8% in comparison with the observed difference of 22.3%, in this case.

Note that the paced recall scores are higher than the unpaced scores, which is somewhat surprising in view of the paced test’s briefer time to respond and its requirement for strict temporal serialization of output. However, those factors appear to have been offset by the advantages conferred during the paced test of informing subjects about the preceding letters as they were trying to recall the letter at a given position. During unpaced recall, subjects occasionally remembered a correct sequence of three to five letters but located their positions incorrectly within the overall series, and our absolute-position scoring method counted all these as errors. The item-by-item cuing method of the paced recall test prevented that kind of error run by resetting subjects to the correct location for the beginning of any remembered sequence.

Among alternative explanations of the postclue benefit in unpaced recall, one suggestion is that on hearing the reorganizing clue for List 2, the informed subjects may have guessed that List 1 also exemplified some hidden message, and while trying to figure out the hidden message, subjects reviewed the List 1 items. But that account seems implausible in several respects. First, no subject in the informed condition spontaneously mentioned later searching for a hidden message in List 1. Second, there was little time for such revival and review of List 1; after receiving the clue, subjects were moved directly into recall of List 1. Third, because List 1 held no hidden message, searching for one should have caused a decrement in recall rather than the enhancement observed. Fourth, the hypothesis assumes what has to be explained, namely, that informed subjects were able to remember more items (than uninformed subjects) simply to review them for a hidden message.

To summarize our results, the essential outcomes of Zimmerman’s (1954) experiment were replicated, more strongly for uncued (unpaced) than for cued (paced) recall. Providing subjects with a delayed clue, suggesting a way to reorganize List 2, enhanced their ability to recall List 1. Although the reorganizing clue bore no relation to List 1, it nonetheless helped improve recall of that list. Our results also indicate that the List 1 recall advantage conferred by the organizing clue cannot be attributed to relief from the worry of having to later recall List 2. The equivalent List 1 recall of the uninformed/first and uninformed/both groups weakens that explanation.

**Experiment 2**

Having replicated the essentials of Zimmerman’s (1954) result, we became interested in its generality. As one small step toward generality, in Experiment 2 we had subjects learn different lists of letters using item-by-item presentations rather than Zimmerman’s method of whole list study. Accordingly, subjects learned 13 letters in List 1 and 13 letters in List 2 by the serial anticipation method using a memory drum.

A second aim of Experiment 2 was to examine how performance of the postinformed subjects compared with that of other subjects who were given the clue before they learned List 2. These preinformed subjects should learn List 2 very rapidly and should index it quite differently from List 1, so that their List 2 learning should interfere only a small amount with their memory for List 1. Of greater interest is whether List 1 recall of postinformed subjects will equal that of preinformed subjects. Accordingly, sufficient groups of subjects were tested to make these comparisons.

**Method**

**Subjects.** The subjects were 40 Stanford University students fulfilling a service requirement for their Introductory Psychology course. Subjects were tested individually, with 10 assigned in random alternation to the four experimental conditions described in the Procedure section following.

**Materials.** The original list of 13 letters, ETMPCINYMOLRA, was presented one by one at a 2-s rate on a memory drum. The second list rearranged the same letters into the order YRATNEMILP-MOC, which spells complimentary backward. Each list was preceded by the word BEGIN as a cue to anticipate the first letter. The last letter was followed by 4 asterisks (one per 2 s) providing an 8-s rest before the word BEGIN started the list again.

**Procedure.** Subjects learned the first list either for 10 trials or to a criterion of at least 10 correct serial anticipations out of 13 letters, whichever came first. Subjects in the rest-control condition then carried out two filler tasks for approximately 20 min while the other three groups proceeded to learn List 2. The filler tasks were, first, to list (for 10 min) as many uses as possible for common objects (e.g., shoe, newspaper, etc.), and, second, to rate the funniness of Peanuts cartoons (for another 10 min). One of the three groups that learned List 2 (called preinformed) was told that the second list of letters they were about to learn formed the word complimentary (as in flattery) spelled backward. The other two groups were not preinformed. All three groups were trained on List 2 until they reached a criterion of 10 out of 13 letters correctly anticipated. After reaching criterion, subjects then rated cartoons for 10 min before being tested for recall of List 1. Just before List 1 recall was requested, one of the two groups of subjects not preinformed was told that List 2 was the word complimentary spelled backward; the subjects then proceeded within 5 s to recall List 1. The other subjects received no information about List 2 before they recalled List 1.

Memory for List 1 was tested in two ways with all 40 subjects. First, subjects were given a sheet of paper with a column of 13 blank
The results of primary interest concern recall of List 1. The percentage of letters in correct absolute serial position are shown in Table 2. The paced recall scores in Table 2 reveal considerable differences in performance, \( F(3, 36) = 9.08, p < .01 \); although consistent with a warm-up or learning to learn effect, the magnitude of their speed-up in learning was unexpected. The improvement in List 2 performance by preinformed subjects was expected and indicates that even at the fast 2-s pace of testing, knowledge of the clue aided their List 2 performance. The criterion-based stop rule also caused the preinformed subjects to receive fewer study-and-recall trials on List 2 than did the other two work groups.

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Experiment 3

Experiment 3 tested the generality of the benefit of reorganizing interfering material by examining free recall of lists of words (i.e., names of U.S. cities). We hoped that both lists would be encoded simply as city names. The reorganizing clue given after List 2 learning was that most of the cities on List 2 were also the names of former presidents of the United States. As before, the question is whether this postinformation clue that recategorizes List 2 will facilitate recall of List 1.

Method

Thirty students from a Stanford University undergraduate dormitory volunteered to participate in the experiment. They were tested in the laboratory in groups of 2 to 5 students, each subgroup was assigned, at random (subject to sample-size constraints), to one of three experimental conditions: uninformed, informed, and rest-control. All subjects began by learning to free recall a list of 20 American cities of medium familiarity, such as Boise, ID, and Newark, NJ. The city names were each displayed for 5 s by a slide projector. After one study trial, subjects were asked to recall as many cities, not including states, as they could, in any order. They had 2.5 min for this recall.

Subjects in the rest-control condition proceeded to rate the funniness of The Far Side cartoons for 7 min, while the other two groups proceeded for one trial to study then recall the second list of city names. The cities on List 2 were chosen so that most were also names of former presidents of the United States, such as Tyler, TX; Jackson, MS; and Monroe, LA. Names that were singularly identifiable as presidents rather than cities (e.g., Nixon, Eisenhower) were avoided. Fifteen of the 20 cities were also presidents; to round out a 20-item list, 5 city names were added that were names of American statesmen in history but were not U.S. presidents.

The informed and uninformed subjects studied the List 2 items for one trial at a 5-s rate, then recalled the items for 2.5 min. Through a misunderstanding, the experimenter's assistant failed to collect subjects' recall sheets for List 1 and List 2, therefore no original learning data are available to analyze for this experiment. After learning List 2, the informed and uninformed subjects rated the cartoons for 3 min. Then all three groups of subjects were asked to free recall cities from List 1, and they were given 7 min to do so. In addition, just before this final recall, subjects in the informed group were told that most of the city names on List 2 were also the names of former U.S. presidents.

Results and Discussion

Regrettably, we are unable to analyze original learning to determine if groups were equated at that point; rather we must rely instead on random assignment of subjects to the three conditions and their equal treatment to argue that their initial learning was probably equated (see also Experiment 4). The main results concern subjects' later recall of List 1 cities; and these percentages are shown in Table 3. The three groups' recall scores differ reliably, $F(2, 27) = 29.69, p < .001$. All these percentages differ significantly from one another by Duncan's multiple-range test, which requires a difference of 11.5% for significance at the 1% level. Recall of the informed subjects is significantly below that of the rest--control subjects; and recall of the informed subjects is significantly above that of the uninformed subjects. We may conclude that learning the second list of cities produced significant retroactive interference, but its magnitude was significantly reduced by postinformation that reorganized (or recategorized) List 2 and that was given just before recall of List 1.

One may ask whether the recall advantage conferred by the presidents clue arises because it further differentiates the two lists thereby improving subjects' ability to edit implicitly considered candidates for recall. Perhaps uninformed subjects confuse the two lists of items, whereas informed subjects know that any candidate city they remember that is also a former president's name can be rejected because it was probably not on List 1. That editing hypothesis predicts that, in comparison with informed subjects, uninformed subjects will commit more intrusion errors (of List 2 items) when trying to recall only List 1 cities.

Unfortunately, the number of overt List 2 intrusions is too small for meaningful analysis. There were only four List 2 intrusions out of 70 total items produced in recall by the 10 uninformed subjects compared with one out of the 91 items recalled by the 10 informed subjects (i.e., Garfield, Ohio). Although the difference is in the predicted direction, the number of overt intrusions is too small to account for the recall benefit conferred by the reorganizing information.

A more subtle version of the editing hypothesis would suppose that many of the intrusions of List 2 items are covert and more easily suppressed by the informed than the uninformed subjects, leaving more time for informed subjects to conduct fruitful memory searches for List 1 items. The important implication here is that uninformed subjects may covertly remember a List 1 item but withhold recalling it either because they mistakenly believe it was a List 2 item or they cannot determine its list membership. In contrast, informed subjects would know that any covertly remembered candidate that is not a former president's name should be recalled because it probably came from List 1. The result of such suppression would be an advantage for informed subjects in recalling List 1. This analysis led to Experiment 4.

Experiment 4

One way to test the editing-plus-suppression hypothesis is to ask subjects to recall all items from both lists without regard to their list membership—the so-called modified modified free recall (MMFR) test. If the editing-plus-suppression hypothesis is correct, then the reorganizing clue for List 2 given just before recall would facilitate recall of List 1. This analysis led to Experiment 4.

<table>
<thead>
<tr>
<th>Condition</th>
<th>M</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rest-control</td>
<td>65.5</td>
<td>12.1</td>
</tr>
<tr>
<td>Postinformed</td>
<td>46.0</td>
<td>9.1</td>
</tr>
<tr>
<td>Uninformed</td>
<td>33.5</td>
<td>5.8</td>
</tr>
</tbody>
</table>

---

*This implication was pointed out to us by Eugene Winograd, Norman Slamecka, Robert Bjork, and Michael Watkins when these results were presented at the November 1990 Convention of the Psychonomics Society, New Orleans, LA.*
before the MMFR test should eliminate the difference in List 1 recall between the informed and uninformed subjects and do so by increasing the recall of the uninformed subjects.

As a second aim of the experiment, we wondered whether some subjects in the uninformed group while studying List 2 became aware of the fact that the cities were also former presidents’ names. If so, that covert categorizing should materially alter these subjects’ encoding of List 2 items, as former presidents as well as cities. Such altered encoding by subjects would be expected to boost their recall of both List 1 and List 2. Accordingly, at the end of the session, subjects in the uninformed condition were asked whether they noticed anything unusual about the items on List 2. Their answers were used to divide the uninformed subjects into those who claimed to be aware or not to be aware that the List 2 cities were also former presidents’ names.

Method

Subjects. The subjects were 110 undergraduates from San Jose State University who participated in an open experiment day to receive course credit for their Introductory Psychology class. They were tested in single condition groups ranging from 20 to 30 subjects each. Subjects selected experiments and times to participate but knew nothing about the experiment. The condition of each group was chosen randomly at the beginning of each session under the constraint that all conditions would be run that morning. Two uninformed groups were run in order to ensure that there would be sufficient numbers of aware and unaware subjects. The groups were run in consecutive 30-min sessions in the following order: uninformed, rest-control, postinformed, and uninformed. There were 24 subjects in the rest-control control group, 26 in the postinformed group, and 61 in the two uninformed groups.

Materials. List 1 was composed of 20 U.S. city names of moderate familiarity and the abbreviated state names in which they were located (e.g., Newark, NJ). List 2 was constructed similarly except that all 20 city names were also names of former presidents of the United States. Six of these city names were fabricated because we could not find enough actual cities with names of former presidents. (Lists of cities are in the Appendix.) As before, we avoided names of former presidents that were too obviously person names rather than city names.

Procedure. The procedures of Experiment 2 were followed with some minor changes. Subjects studied List 1 cities for two trials in random order at a 5-s rate, then recalled them in writing for 2 min. Immediately after recalling List 1, subjects in the postinformed and uninformed conditions proceeded to study then recall List 2 for two trials using the same procedure, while rest-control subjects rated cartoons. The postinformed and uninformed subjects then rated cartoons for 6 min. Rest-control subjects engaged in the rating task for 11 min instead of learning List 2. Thus, the overall time between List 1 learning and its final recall test was equated for all three groups.

After the filler task, subjects in the rest-control group were asked to recall in writing as many items as they could remember from the list they had initially learned (List 1), and they were given 3 min to do so. The other subjects were told to make three columns on their answer sheets and to label them as: List 1, Not Sure, and List 2. They were then asked to recall by writing as many items from each list in the appropriate column as they could remember. If they could not remember which list an item came from, subjects were to place it in the Not Sure column. To ensure that subjects listed items from the target list and to reduce output interference on List 1 relative to rest-control subjects, they were told that the experimenter was especially interested in what they could remember from List 1. At the end of these instructions, but immediately before recall commenced, subjects in the postinformed condition were told: “All the cities on the second list are also names of former U.S. presidents.” Postinformed and uninformed subjects were then given 6 min to recall the items. No subject, however, needed the entire amount of time allotted.

After the recall task, subjects in the uninformed condition were asked to turn to the next page of their booklet and write whether they had “noted anything interesting” about List 2 at the time they were studying it initially. Subjects were given only 10 s to write what they had noticed, if anything, in order to minimize the likelihood of their mentally reviewing List 2 and reporting the president’s category as noticed when in fact it had not been noticed during learning or recall. Using their answers, we divided uninformed subjects into two subgroups: those who claimed they had noticed the former presidents’ category while studying the List 2 cities and those who did not make that claim. Of course, subjects might answer from having noticed the presidents relation during the MMFR recall of List 2.

Results and Discussion

On the MMFR test, subjects overwhelmingly identified accurately the list from which a recalled item came. They hardly ever used the Not Sure category—only 0.8% of items recalled by uninformed subjects and never by postinformed subjects. Of all List 1 items recalled, words were correctly assigned to List 1 in 99.4% and 100% of the cases by uninformed and postinformed subjects, respectively. Similarly, recalled List 2 items were correctly assigned to List 2 99.7% and 98.3% of the time by uninformed and postinformed subjects, respectively. All errors were contributed by 1 subject in each group. The extreme accuracy of list identification indicates that performance can be gauged simply in terms of the number of items recalled and correctly attributed to each list.

The average items recalled for each list for each condition are displayed in Table 4. Initial analyses were carried out using all uninformed subjects regardless of their awareness reports. An overall ANOVA on List 1 recall percentages detected significant differences, $F(2, 108) = 15.55, p < .001$. Duncan’s multiple-range test (requiring an adjacent mean difference of 7.1%) declares that List 1 recall was significantly higher for rest-control subjects than for the postinformed subjects, who in turn had significantly higher recall than the full group of uninformed subjects. Thus, as in Experiment 3, learning the second list produced reliable retroactive interference (RI) with recall of the first list, but this RI was significantly relieved by the reorganizing cue. The MMFR test thus yielded results that were comparable to those for List 1 recall in our previous experiment.5

Surprisingly, the informed and uninformed subjects did not differ significantly in recall of List 2, $F(1, 84) = .25, p > .6$.

4 The experimenter said that none of the uninformed subjects spontaneously mentioned noticing the former presidents’ names in List 2. But subjects were not asked whether they noticed this fact.

5 Similar results were obtained in a replication of this MMFR experiment carried out by Barnett Hartston in Robert Bjork’s laboratory at the University of California, Los Angeles (personal communication, August, 1991).
Thus, having the organizing clue aids recall of the original material but not of the interfering material in this case.

Forty-three subjects in the uninformed condition claimed that while studying List 2 they had noticed that some of the cities were also names of former U.S. presidents. Table 4 reports results separately for the Aware noticers versus Unaware subjects. An ANOVA, carried out on List 1 recall treating the aware, unaware, and postinformed subjects as separate groups, yielded overall significance, $F(2, 84) = 6.89$, $p = .002$. Duncan’s multiple-range test (requiring an adjacent mean difference of 7.5%) declares that postinformed subjects and aware subjects recalled significantly more than unaware subjects, but the former two groups were not significantly different. A similar ANOVA on List 2 recall percentages found no significant differences, $F(2, 84) = 2.27$, $p > .10$.

Several interpretations of these post hoc awareness analyses are possible. First, aware subjects may have noticed and used presidents as a second category or index when storing and retrieving List 2 items; and that second index would facilitate recall by reducing the extent to which storage of List 2 caused overloading of retrieval cues established for List 1; and it also later provides a second cue for retrieving List 2 items. A second interpretation of those results is that conditioningual on subjects who become aware selects the smarter subjects in the sample, and their better recall of both lists is a consequence of that subject selection. The present data provide no way to distinguish these interpretations.

### Discussion

The pieces of the theoretical puzzle can now be assembled. Interference from learning a second set of material on a first set is substantially reduced by a postlearning clue enabling the learner to reorganize the interfering material. Assuming the reliability of the result (see Footnotes 2 and 4), we find that the reduction occurs both for serial recall and for free recall. The reduction occurs whether or not list discrimination is required; that is, the reduction occurred with the MMFR test as well as with the single-list recall test. Moreover, the reduction occurred with a postorganizing clue (in Experiment 4) that did not improve subjects' recall of the interfering list.

The postinformation enhancement appears to contravene several other hypotheses in memory theory. First, it contravenes Postman and Underwood’s (1973) idea of response-set suppression, which supposes that in learning List 2 the subject learns to suppress or inhibit items from List 1. It is the supposed persistence of this suppression, after List 2 learning, that is alleged to explain the usual generalized, retroactive interference. But there is no reason in that theory to expect a lessening of this List 1 suppression by simply telling subjects another fact about List 2 after it has been learned. If anything, further information about List 2 should only enhance the suppression of List 1 responses.

Our results are also unexpected in light of the interference normally produced by part-set or part-list cuing (Nickerson, 1984; Rundus, 1973; Slamecka, 1968). In those experiments, subjects who were provided with some of the unrelated items to be recalled experienced reduced chances of recalling other items in the target set. The former items may either be presented directly during the recall of the full set or may simply have been primed by their earlier presentation (see Brown, 1968). Generalizing such results to our situation, one would have expected that the reorganizing clue would cause subjects to think about the List 2 items, thus enhancing their availability and augmenting their interference with recall of List 1 items. But that is the opposite of the results to be explained.

The postinformation benefit to List 1 recall also conflicts with the implication of list-strength effects found for free recall (see Ratcliff, Clark, & Shiffrin, 1990; Shiffrin, Ratcliff, & Clark, 1990; Tulving & Hastie, 1972). The list-strength effect refers to the lowering of recall probability of some items in a set when other items in the set are strengthened by a learning manipulation applied to those other items (more presentations or more study time). Our postinformation clue (especially in Experiments 1 and 2) qualifies as a strengthening operation for half the items in the experiment because the clue presumably makes the interpolated items more available for recall. But the opposite of a list-strength effect occurred with our postinformation clue.

It is not obvious how to explain our results in terms of current memory models such as search of associative memory (SAM), free recall in an associative net (FRAN), ACT, composite holographic associative recall model (CHARM), theory of distributed associative memory (TODAM), or Minerva. In all such models, a retrieval cue aids recall primarily by its association to the target items to be recalled. For example, “California city where movies are made” would be a good cue to retrieve the presence of Los Angeles on List 1. But our reorganizing clue did not refer to the target items in List 1, but only to those in List 2.

We might suppose that on being told that List 2 items are former presidents, the postinformed subjects infer that the List 1 cities were not presidents and perhaps that cue helps their recall. For example, in the SAM model of free recall (Raaijmakers & Shiffrin, 1981), memory search is directed by a composite retrieval cue such as: List 1 + City. This composite is used to generate candidate items from memory that are then evaluated before recall. We may ask whether adding

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**Table 4**

Percentage of Items Recalled From Lists 1 and 2 and Standard Deviations by Groups in Experiment 4

<table>
<thead>
<tr>
<th>Condition</th>
<th>n</th>
<th>M</th>
<th>SD</th>
<th>n</th>
<th>M</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>List 1</td>
<td></td>
<td></td>
<td></td>
<td>List 2</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Rest-control</td>
<td>24</td>
<td>56.5</td>
<td>15.2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Informed</td>
<td>26</td>
<td>46.5</td>
<td>14.6</td>
<td></td>
<td>55.0</td>
<td>15.6</td>
</tr>
<tr>
<td>Uninformed</td>
<td>61</td>
<td>37.5</td>
<td>13.5</td>
<td></td>
<td>53.5</td>
<td>15.9</td>
</tr>
<tr>
<td>Aware</td>
<td>43</td>
<td>39.9</td>
<td>12.5</td>
<td></td>
<td>56.0</td>
<td>15.7</td>
</tr>
<tr>
<td>Unaware</td>
<td>18</td>
<td>31.4</td>
<td>14.1</td>
<td></td>
<td>47.5</td>
<td>14.8</td>
</tr>
</tbody>
</table>

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This unexpected finding suggests that our subjects may not have been able to generate or recognize large numbers of names of former U.S. presidents. That account suggests follow-up experiments to clarify the result.
"is not a president" as a cue should help generate more relevant items of List 1. However, the problem with this type of explanation is that normally what a verbal item is not is practically useless as a retrieval cue. The set of U.S. cities that are not former presidents’ names is too vast. Moreover, the set of List 1 cities is also not names of birds, not names of vehicles, and so forth, and we would not expect such negative cues to enhance recall of the List 1 cities. The point is that negative cues that set aside only a small fraction of a vast collection of items would not be expected to help in generating the remainder of the set. A similar problem arises for the other models listed, since for each model cues are alleged to improve recall primarily by enhancing the generation of target items by virtue of associations from the cue to the targets. But such associations are lacking for our clue that reorganizes the interfering material.

An early hypothesis to explain the data (especially those in Experiment 3) was a simple editing idea. As candidate cities are evoked from memory, postinformed subjects know that candidate cities that are also former president’s names were probably on List 2, whereas other candidates from memory were probably on List 1. Uninformed subjects might make errors in this discrimination, and therefore withhold possible List 1 candidates from overt recall. The problem with this editing account is that the number of overt list confusions by uninformed subjects was trivial in all experiments; moreover, the reduced interference effect occurred even with the MMFR test in Experiment 4 where list discrimination was not required. Moreover, the results of Experiments 1 and 2, which involved serial recall of overlapping lists with many common letters, complicate the hypothesis.

An alternative form of the list-confusion explanation asserts that uninformed subjects believe (wrongly) that the list-discrimination component of the recall task will be so difficult that they proceed more cautiously with their memory search, with the consequence that they run out of recall time before they have examined all the available candidates for List 1. While not wishing to dismiss all such explanations, we point out two of its shortcomings: first, the postinformation benefit occurred in Experiment 4 even when list discrimination was not required; and second, final recall periods in our experiments were very long so that subjects routinely finished their recall long before the allotted time.

In our opinion the basic flaw of the editing hypothesis is that it refers only to postretrieval processes: on that hypothesis, candidate items are first retrieved by contextual cues, are then tested for list membership, and are then overtly recalled if they pass a criterion for membership in the desired target set. The presumed editing process operates only in the second, monitoring phase of recall. But this seems insufficient to explain our results. It appears to us that the influence of the reorganizing cue is also on the first phase—the subjects’ ability to generate plausible candidates for List 1.

We suspect that the explanation of our findings will center on the ability of the reorganizing cue for interfering material to help in the generation or direct retrieval of items from the desired set. One way to achieve this is for the cue to enable the subject to set aside in some manner—perhaps by erasing, inhibiting, or unlearning—the associations of the List 2 items to the experimental context, so that they are not even generated when the subject is searching for List 1 items. In some way, the clue must enable subjects to not think about items in List 2 encompassed by that clue and thereby devote themselves exclusively to generating items from List 1. But associationist theorists are nearly unified in their belief that associations, once established, cannot be erased from memory in wholesale fashion (see, e.g., Anderson, 1983, p. 173); nor does association theory have mechanisms for setting aside interfering items attached to a given cue. One problem is that modern memory theories have progressed quite far without need for inhibition constructs (e.g., see explanations of unlearning results by Mensink & Raaijmakers, 1988, 1989) despite the widespread evidence for inhibitory processes in classical conditioning (Rescorla & Wagner, 1972), in neural networks, and in synaptic neurotransmitters.

Perhaps it is premature to offer firm hypotheses before further empirical studies have explored the nature, extent, and boundary conditions of the postinformation advantage. Many parametric variations need to be explored, inquiring whether the postinformation advantage occurs with various types of learning materials (e.g., texts), at very short or very long retention intervals, in proactive interference designs, within a single set of materials of which parts are reorganized by the clue, with recognition memory, or with paired-associated recall, and so on.

As one example, in a recent near-replication of Experiment 1, after learning both lists, uninformed subjects showed strong interference on attempting to recall List 1 but no release from that interference if immediately after the recall attempt they were given the reorganizing clue (for List 2) and asked to recall List 1 again. Apparently, having just recalled List 1 (badly), subjects simply repeat those few items without making productive use of the reorganizing clue for List 2. This unexpected null result might be different if a delay were to be imposed between subjects’ first, uninformed retention test and their later, informed test. In any event, the null result places some constraints around a proper explanation of the postinformation benefit.

An immediate goal is to better characterize the kinds of reorganizing clues that will produce the postinformation advantage and which types will prove ineffective. Our intuition is that effective postinformation clues would be those that simplify, make familiar, or bring unity into the disorganized interfering material, thereby reducing its number of chunks in memory. Similarly, we would guess that obscure orthographic clues (e.g., “All List 2 words had an E as their third letter”) would be ineffectual, as would be weak associates of the interpolated items (e.g., “List 2 words referred to white things”) or unnoticed arbitrary features of pictured items (e.g., “The pictured objects of List 2 all pointed to the left”). However, even these intuitions are not resolute and are best laid aside until we have empirical results to inform our hunches.

7 This view predicts that if List 1 had included a few former presidents’ names, they would have been recalled especially poorly.
References


(Appendix follows on next page)
Appendix

List 1

Burton, MI
Daly City, CA
Athens, GA
Mansfield, OH
Evanston, IL
Garland, TX
Boise, ID
Warwick, RI
Auburn, AL
Joplin, MO

Durham, NC
Eugene, OR
Orlando, FL
Baltimore, MD
Lubbock, TX
Seattle, WA
Pacifica, CA
Waukegan, IL
Newark, NJ
York, PA

List 2

Jackson, MS
Cleveland, OH
Lincoln, NB
Madison, WI
Garfield, NJ
Jefferson City, MO
Washington, DC
Monroe, LA
Tyler, TX
Johnson, TN

Taylor, MI
Polk, PA
Harding, CA
Wilson, NC
McKinley, ID
Grant, OR
Ford, MI
Hayes, NB
Taft, IL
Hoover, CO

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