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Influences of Intentional and Unintentional Forgetting on False Memories

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In 2 experiments, we examined the interplay of 2 types of memory errors: forgetting and false memory—errors of omission and commission, respectively. We examined the effects of 2 manipulations known to inhibit retrieval of studied words—directed forgetting and part-list cuing—on the false recall of an unstudied "critical" word following study of its 15 strongest associates. Participants cued to forget the 1st of 2 studied lists before studying the 2nd recalled fewer List 1 words but intruded the missing critical word more often than did participants cued to remember both lists. By contrast, providing some studied words as cues during recall reduced both recall of the remaining studied words and intrusions of the critical word. The results suggest that forgetting can increase or decrease false memories, depending on whether such forgetting reflects impaired access to an entire episode or retrieval competition among elements of an episode.

During most of the history of memory research there has been a decided preference for studying accuracy in memory performance, but in recent years there has been growing interest in memory errors. Two types of memory errors that have drawn considerable attention—forgetting and false memory—are functional opposites: Forgetting is the failure to remember information to which one has been exposed (an error of omission); false memory is the (incorrect) remembering of information to which one has not been exposed (an error of commission). The two experiments reported here were designed to examine the interplay of forgetting and false memory.

For both experiments we used a paradigm that has been markedly successful in creating false memories in the laboratory: the Deese–Roediger–McDermott (DRM) paradigm (Deese, 1959; Roediger & McDermott, 1995; see also Read, 1996), in which an unpresented word, the "critical item," is falsely recalled at a high rate following presentation of its strongest associates. To induce participants to forget studied words from DRM lists, we used two quite different procedures: In Experiment 1, we used directed forgetting (e.g., R. A. Bjork, 1970; see MacLeod, 1998, for an elegant and thorough review), and in Experiment 2, we used part-list cuing (e.g., Slamecka, 1968; see Nickerson, 1984, for a thorough review). These two procedures differ in some important ways, including whether the resultant forgetting is consistent or inconsistent with participants' goals, but both procedures induce a particular type of forgetting—retrieval inhibition—that takes the form of impaired access to studied items during free recall.

False Memories in the Laboratory

In the DRM paradigm, participants study a list of words that are the strongest semantic associates of a word not presented on the list—the critical item. For example, participants may study words such as *mad*, *fear*, *hate*, *rage*, *temper*, and so forth, all of which are the strongest semantic associates of the unpresented critical item, *anger*, according to word association norms. Roediger and McDermott (1995) found that participants were highly likely both to recall and to recognize the critical item falsely. Indeed, rates of false recall and recognition of the critical item equaled or exceeded the rates of correct recall and recognition for some words that actually had appeared on the list. Even more impressive is that participants who falsely recognized a critical item said they *remembered* experiencing some phenomenon in connection with its presentation (e.g., the speaker's voice, the surrounding words, or contemporaneous thought processes), as opposed to merely *knowing* that the item had been presented, with about the same frequency as they did for actually presented items (Roediger & McDermott, 1995; see also Norman & Schacter, 1997; Payne, Elie, Blackwell, & Neuschatz, 1996). The DRM memory illusion has proven especially robust in a multitude of subsequent studies (see Roediger, McDermott, & Robinson, 1998, for a review), appearing even when experimenters warned participants in advance about the illusion and trained participants with the lists (e.g., McDermott & Roediger, 1998).

Retrieval Inhibition

Like false memories, retrieval inhibition has also been investigated extensively of late (for a review, see M. C. Anderson &

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Neely, 1996). Adopting the terminology used by E. L. Bjork, Bjork, and Anderson (1998), we use the term *retrieval inhibition* to denote any of the potential theoretical mechanisms underlying an impairment of retrieval access to information that remains available in memory, as measured by alternative means, such as a recognition test (see M. C. Anderson & Bjork, 1994, for a discussion of these mechanisms). Thus, we distinguish retrieval inhibition from *suppression* in that suppression is only one particular type of retrieval inhibition mechanism, one that involves an intentional forgetting process directed at to-be-forgotten information. The terms *impairment* and *interference* denote the empirical effects of retrieval inhibition.

Directed Forgetting

In Experiment 1 we used the list-method directed-forgetting paradigm to induce retrieval inhibition. In this paradigm, participants study two lists of words and are cued after studying List 1 either to forget or to continue remembering that list while they study List 2. Before receiving the cue to forget or remember List 1, all participants have the same mindset—namely, they believe that the items in List 1 need to be learned for a later test. Typical recall results (see E. L. Bjork et al., 1998, and MacLeod, 1998, for reviews) have shown that, compared with remember-cued participants, forget-cued participants recall fewer words from List 1 but more words from List 2 (e.g., E. L. Bjork & Bjork, 1996; Geiselman, Bjork, & Fishman, 1983; but see Conway, Harries, Noyes, Racsma'ny, & Frankish, 2000, reporting inconsistent List 2 effects). Evidence has indicated that the forget cue affects only retrieval access, as forget-cued and remember-cued participants exhibit no reliable memory differences on tasks other than free recall—that is, on tasks that do not require the unaided use of retrieval processes, such as recognition (B. H. Basden, Basden, & Gargano, 1993; Elmes, Adams, & Roediger, 1970; Geiselman et al., 1983), relearning (Geiselman & Bagheri, 1985; Reed, 1970), word fragment completion (B. H. Basden et al., 1993; E. L. Bjork & Bjork, 1996), and word association (B. H. Basden et al., 1993). Of particular importance for current purposes, the lack of directed-forgetting effects on indirect memory tests indicates that the forget cue does not affect the semantic activation of studied items. In addition to impairing access to List 1, the forget cue also abates the proactive interference that List 2 normally suffers when both lists must be remembered. The proactive interference is evidenced by lower List 2 recall by remember-cued participants than by control participants who study List 2 but not List 1; the abatement of interference caused by the forget cue is evidenced by forget-cued participants' List 2 recall, which is higher than that of remember-cued participants and approximately equal to that of control participants (E. L. Bjork & Bjork, 1996; R. A. Bjork, 1989).[1]

Part-List Cuing

In Experiment 2 we used a different method of inducing retrieval inhibition, part-list cuing, which involves presenting participants with some items from a study list as cues at the time of recall. For example, in Slamecka's (1968) seminal study, participants studied lists containing exemplars from five categories. At test, one group of participants received exemplars from each category as cues for recalling the remaining noncue items (targets); another group received no such cues. Slamecka unexpectedly found that people recalled fewer of the targets when they were given cues than when they were not. As with directed forgetting, only retrieval access is impaired by part-list cuing; recall of the targets in the uncued condition demonstrates that they are available in memory at test. However, in contrast to directed forgetting, retrieval inhibition from part-list cuing is strictly unintentional in that it occurs despite participants' intention to maintain access to the studied items at all times. Part-list cuing inhibition has proven fairly robust (for reviews, see Nickerson, 1984; Roediger & Neely, 1982), occurring also with uncategorized lists (e.g., Slamecka, 1968, 1972) and with unrelated extralist items as cues (e.g., Roediger, Stellon, & Tulving, 1977). A similar part-set cuing effect has been observed with sets of items associated in long-term memory (e.g., Brown, 1968).

Practical Ramifications

The effects of directed forgetting and part-list cuing on veridical and false memories for word lists may be analogized to more complex, real-world phenomena, although the validity of any such analogy obviously depends on empirical verification. One such phenomenon is memory for autobiographical events, such as childhood sexual abuse, although the validity of such an analogy has been questioned (see, e.g., Freyd & Gleaves, 1996). In this context, directed forgetting may be seen as analogous to an instruction by an abuse perpetrator to the victim to forget that the abuse ever happened, or the victim's own self-motivated desire to forget the incident (see Cloitre, 1992, 1998; also see Cloitre, Cancienne, Brodsky, Dulit, & Perry, 1996, and McNally, Metzger, Lasko, Clancy, & Pitman, 1998, reporting varying results for directed forgetting of word lists by abuse survivors). Part-list cuing, in this context, may be seen as analogous to recovered memories that are incomplete or to focusing on a subset of details in the course of therapy. Applications to the courtroom are also possible: Judges' instructions to juries to disregard testimony are an attempt at directed forgetting, although most evidence has indicated that such instructions either have little effect in causing jurors to forget the testimony or, worse, may actually make the testimony more accessible (see Kassin & Studebaker, 1998, for a review). Part-list cuing could be analogized to interrogation of witnesses about some aspects of the witnessed event but not others, which may render the latter aspects less accessible.

The current research may bear on the occurrence of false memories in such circumstances. Of course, false memories for incidents of abuse have been the topic of considerable controversy (see, e.g., Gartner, 1997; Lindsay & Read, 1994; Loftus, 1993b, 1994; Loftus & Ketcham, 1994; Pope, 1996), although the occurrence of errors in eyewitness memory for other types of events has been well established (see, e.g., Loftus, 1993a). The type of false

[1] Another method of inducing directed forgetting—the item method—differs from the list method in that a forget or remember cue is delivered after each item rather than after an entire list. The two methods respond differently to various manipulations, and different mechanisms are thought to underlie them (see B. H. Basden et al., 1993; Basden & Basden, 1998; MacLeod, 1998; but see also Kimball & Metcalfe, 2001). Because in Experiment 1 we used the list method, we focus here on studies that also used that method.

memories that are analogous to critical-item intrusions with DRM lists would involve information that is semantically associated to actually experienced events. For example, an eyewitness may misidentify a bystander as the perpetrator (e.g., Phillips, Geiselman, Haghighi, & Lin, 1997; Ross, Ceci, Dunning, & Toglia, 1994) or may mistakenly recall having witnessed the cause of an event when he or she witnessed only the event itself and inferred the cause (Hannigan & Reinitz, 2001). Similarly, jurors may mistakenly recall that a witness's testimony included a statement that was actually only a semantically plausible inference derived from the testimony (e.g., Harris, 1978).

The issue of concern here is the effect that forgetting may have on this type of false recall: Will the false details be more, less, or equally accessible as a result of the forgetting of veridical details? On the one hand, a decline in false recall as a result of forgetting may offer something of a silver lining, ameliorating the effects of one type of memory error with a decrease in another type. On the other hand, an increase in the accessibility of false details as a result of forgetting would seem particularly troublesome, as it compounds the loss of access to veridical details. For example, an abuse victim or another type of eyewitness may forget information that would tend to exculpate the accused perpetrator (e.g., physical characteristics of the perpetrator that do not match those of the accused) but may also falsely recall information that would tend to incriminate the accused (e.g., the exact time and place of the incident, for which the accused has no alibi), resulting in a wrongful incrimination of the accused. The reverse situation would be harmful as well, with incriminating information being forgotten and exculpatory information being falsely recalled, resulting in the true perpetrator escaping punishment. Of course, no firm conclusions on these complex issues are possible on the basis of evidence acquired from experiments involving simple word lists, but some insight into arguably similar underlying cognitive processes may be gained.[2]

Experiment 1: Directed Forgetting

A key consideration in predicting the effects of directed forgetting on DRM lists is that directed forgetting appears to affect episodic access but not semantic activation, as evidenced by differences in free recall attributable to the interlist cue but an absence of differences on indirect memory tasks, such as word fragment completion and word association. The differences in episodic access are likely attributable to differential rehearsal of List 1 and List 2 following the interlist cue (see R. A. Bjork, 1970, 1972; Kimball & Metcalfe, 2001)—in particular, differential rehearsal involving interitem relational processing (see B. H. Basden & Basden, 1998)—although it is also possible that forget-cued participants may intentionally suppress episodic access to List 1 (see R. A. Bjork, 1989; Geiselman et al., 1983).

Given that semantic activation is unaffected by directed forgetting and that DRM lists contain strong semantic associations, one prediction for studied-item recall is that typical directed-forgetting effects may not occur: Forget-cued participants may obviate the impairment in episodic access by using *semantic cuing*, that is, by using early-recalled items to cue other items semantically. An alternative prediction is that any such semantic cuing would be insufficient to overcome the episodic inhibition, and a typical directed-forgetting pattern would occur.

Predictions regarding the effect of directed forgetting on critical-item intrusions depend on the roles of semantic and episodic information in the occurrence of such intrusions in the first place. Several theories that have been extended to false memories (see Roediger et al., 1998) afford different roles to these two types of information. For example, the fuzzy-trace theory (Brainerd & Reyna, 1998; Reyna & Brainerd, 1995) assumes that a semantically based gist trace and a perceptually based verbatim trace are stored in memory at the time of a study episode and that the particularly strong gist trace for DRM lists is more easily accessed than the verbatim trace, resulting in a high rate of intrusions for the gist-associated critical item.

Similarly, the source-monitoring framework (Johnson, Hashtroudi, & Lindsay, 1993; Mather, Henkel, & Johnson, 1997) explains critical-item intrusions as resulting from a greater reliance during recall on semantic information, which is highly salient for DRM lists, at the expense of episodically distinctive information that would allow rejection of the critical item as not having been presented. The result is a mistaken attribution of the critical item's strong activation to its presentation during study rather than to its internal generation through semantic associative processes (see Mather et al., 1997). In a like manner, the attributional theory of remembering (Jacoby, Kelley, & Dwyan, 1989, as extended by Roediger et al., 1998) assumes that participants heuristically misattribute the ease in accessing the critical item at test to its prior presentation on the study list. As a consequence of using the heuristic, participants typically fail to use more analytical processing, including the use of episodically distinctive source information to correct the attribution error.

These theories all assume that critical-item intrusions occur in the first place because participants recalling DRM lists rely on highly salient—but in this case misleading—semantic information. Such reliance results in the underuse of episodically distinctive information that would allow participants to reject the critical item as not having been presented. Because a cue to forget a list impairs access to the already underused, episodically distinctive information while leaving semantic activation unimpaired, these theories seem to predict that critical-item intrusions would increase as episodic access decreases with directed forgetting. This interpretation is consistent with results for older adults, who have exhibited a tendency toward lower veridical recall and greater false recall than younger adults (see, e.g., Norman & Schacter, 1997; but see Tun, Wingfield, Rosen, & Blanchard, 1998), a tendency that has been attributed to greater reliance on gist-based processing by older adults (see, e.g., Schacter, Israel, & Racine, 1999). In a similar vein, Schacter, Verfaellie, and Pradere (1996) have interpreted lower false recall among amnesic patients as indicative of a degraded semantic or gist representation.

A different prediction for critical-item intrusions seems to follow from certain spreading-activation theories (e.g., J. R. Anderson, 1983; Collins & Loftus, 1975; McClelland & Rumelhart,

[2] We are primarily concerned with the effects of forgetting on false recall, but we acknowledge, and our results show, that there is substantial veridical memory despite the forgetting. Thus, a particular detail that a witness or abuse victim recalls—including, for example, subsequently recovered memories—may indeed be veridical, and we do not intend to imply otherwise by focusing on false recall.

1981). Such theories assume that items become activated during study as a result of an unconscious and automatic spreading of activation in a semantic network and that items with the highest level of semantic activation are output during recall. Roediger et al. (1998) extended these theories to DRM lists, assuming that semantic activation spreads during study from the list words to the critical item, resulting in a high level of activation and thus a high rate of intrusion for the critical item. Inasmuch as directed forgetting does not appear to affect semantic activation, these theories appear to predict that critical-item intrusions would not be affected by directed forgetting.

Finally, a third possible prediction is that the critical item might behave like a studied item during recall, so that a decline in studied-item recall pursuant to a forget cue would be accompanied by a similar decline in critical-item intrusions. Such a prediction is consistent with the implicit associative response (IAR) theory (Underwood, 1965), which assumes that as studied words are encoded they cause implicit associative responses that bring semantic associates of the studied words—such as the critical item, in particular—into conscious awareness. Support for such conscious intrusion of the critical item during encoding is provided by evidence of perceptual priming on implicit memory tests (McDermott, 1997). The IAR theory further assumes that an associate behaves like a studied item after entering conscious awareness. The theory would seem, therefore, to predict that as studied-item recall declines with directed forgetting, so should critical-item intrusions. This prediction is also consistent with the frequently observed similarities in output levels for studied and critical items during recall and in the phenomenological experience for the two item types (e.g., Norman & Schacter, 1997; Payne et al., 1996; Roediger & McDermott, 1995; but see Mather et al., 1997). There is also evidence, however, of dissociations between studied-item recall and critical-item intrusions under some circumstances, such as with multiple tests (McDermott, 1996) and slow presentation rates (McDermott & Watson, 2001).

Method

Design. In Experiment 1 we used a mixed design. Interlist cue (forget, remember, and control) and list test order (List 1 or List 2 tested first) were manipulated between subjects. List study position (List 1 vs. List 2) was manipulated within subjects for forget-cued and remember-cued participants but between subjects for control participants, who only studied one list. We only report recall results for the first-tested list in each condition as they are uncontaminated by any previous recall.[3]

Participants. Participants were 270 University of California, Los Angeles (UCLA), undergraduates enrolled in the introductory psychology course, participating for partial course credit. The six groups were randomly assigned 30 participants each initially. Another 30 and 60 participants were added to the remember and forget groups recalling List 2 first, respectively, to increase power after sizable group differences in List 2 critical-item intrusion rates failed to achieve statistical reliability among the initial groups.

Materials and apparatus. Three pairs of 15-word lists were selected from among the 24 lists used by Roediger and McDermott (1995; see their appendix for all 24 lists). The selected pairs of lists had as their critical items the words *sweet* and *soft*, *rough* and *slow*, and *angry* and *spider*, respectively, and each list appeared only with its paired list. These selections and pairings were made with two primary criteria in mind. First, it seemed important that the two critical items for a given list pair have similar valences in order to minimize categorical differences between the two lists. We feared that lists generated from critical items that differed markedly in valence might result in lists that were too distinctive, enabling participants to segregate the lists at the time of study, which might provide alternative retrieval routes that could obviate the effects of retrieval inhibition (see, e.g., Horton & Petruk, 1980; Shebilske, Wilder, & Epstein, 1971). Second, an attempt was made to minimize semantic associations between words in one list and those in its paired list so as to minimize the likelihood of cross-cuing between lists because there is evidence that such associations can reduce and even eliminate typical directed-forgetting effects (e.g., Conway et al., 2000).[4]

Each word in the lists was printed on a separate slide for presentation to the participants by means of a carousel slide projector. The study order of the lists for each of the three list pairs was counterbalanced across participants, resulting in six combinations of list study order (with one list in each combination omitted in the control conditions). An equal number of participants was assigned to each of the six list-study-order combinations in each of the cue-test-order conditions.

Procedure. The experiment was administered to five or fewer participants at a time. Participants in the forget and remember conditions were told at the outset that they would see lists of words projected onto a screen one word at a time and that they should try to remember the words for a later memory test. The words in List 1 were then shown to the participants for 2 s each. Following Roediger and McDermott's (1995) procedure, the words were presented sequentially from the strongest associate of the critical item to the weakest.

After the last slide for the first list, participants in the remember condition were told that they should continue remembering that list and that they would be shown another list that they should also try to remember. Participants in the forget condition were told instead at this point that the first list had been for practice, so they should forget it, and that they would now be shown the list on which they would be tested. Approximately 30 s elapsed during the delivery of the interlist cues and the changing of slide trays, after which time the second word list was shown. Participants then received a blank sheet of paper and were given 2 min to recall words from a specified list, either List 1 or List 2. After 2 min had passed, the sheets were collected, new ones were distributed, and participants were given 2 min to recall the other list.

Participants in the control conditions saw only one word list, either in the List 1 or List 2 study position. In lieu of seeing the other list, they made similarity judgments for geometric shapes presented on each of 15 slides, also shown for 2 s each, and in lieu of an interlist cue, they received the instruction regarding the second task. With these exceptions, the procedure was the same for the control groups as for the forget and remember groups.

Results

The data were scored according to strict and liberal scoring criteria, but the patterns were virtually identical, so the results reported here are those obtained with the liberal criterion, which

[3] Experiment 1 is a modification and extension of a pilot experiment conducted by Thomas Murray and Aaron Benjamin as a demonstration in the UCLA cognitive psychology lab course at the suggestion of Robert A. Bjork. That experiment used the same two DRM lists for all participants, tested List 2 recall first for all participants, and did not include any control conditions. Results closely resembled those for comparable conditions in Experiment 1.

[4] An inadvertent exception to this criterion occurred in the *anger–spider* list pair. The *anger* list contains the word *fear* and the *spider* list contains the word *fright*. However, veridical and false recall for this list pair exhibited a pattern similar to that for other lists. Conway et al. (2000) also found that one cross list semantic association did not alter the directed-forgetting pattern.

permitted minor variations in spelling, tense, number, and the like. Mean percentages of studied items recalled and critical items intruded are displayed in Panels A and B of Figure 1, respectively.

Studied-item recall. The studied-item recall data were analyzed with a between-subjects analysis of variance (ANOVA), $MSE = 174.55$. Neither of the counterbalancing variables (list-pair topics and within-list-pair presentation order) interacted with any other variables. Overall, the percentages of studied items recalled for List 1 ($M = 57$, $SE = 2$) and List 2 ($M = 59$, $SE = 1$) did not differ reliably ($F < 1$). There was a reliable main effect of interlist cue, $F(2, 234) = 27.19$, $p < .0001$, with recall in the control condition ($M = 68$, $SE = 1$) being reliably greater than recall in both the forget condition ($M = 53$, $SE = 1$), $F(1, 234) = 47.33$, $p < .0001$, and the remember condition ($M = 54$; $SE = 2$), $F(1, 234) = 38.19$, $p < .0001$, but the latter two conditions did not differ reliably ($F < 1$).

Of more interest for our purposes, there was a reliable Interlist Cue × List Study Position interaction, $F(2, 234) = 14.87$, $p < .0001$, and in particular, a reliable simple interaction for the forget and remember conditions apart from the control conditions, $F(1, 234) = 27.13$, $p < .0001$. This simple interaction reflects typical directed-forgetting effects, with recall of List 1 being lower in the forget condition ($M = 46$, $SE = 2$) than in the remember condition ($M = 58$, $SE = 3$), $F(1, 234) = 11.92$, $p < .001$, but recall of List 2 being higher in the forget condition ($M = 60$, $SE = 1$) than in the remember condition ($M = 51$, $SE = 2$), $F(1, 234) = 18.11$, $p < .0001$. Mean studied-item recall for List 2 was similar for the initial and added sets of participants in both the forget condition (*M*s = 57 and 61, *SE*s = 3 and 2, respectively) and the remember condition (*M*s = 49 and 52, *SE*s = 3 and 3, respectively). The costs and benefits of directed forgetting were fairly consistent across serial positions, with 13 of 15 List 1 serial positions exhibiting numerically greater recall in the remember than in the forget condition, and 13 of 15 List 2 serial positions exhibiting numerically greater recall in the forget than in the remember condition.

In the control conditions, studied-item recall for List 1 ($M = 69$, $SE = 2$) did not differ reliably from that for List 2 ($M = 67$, $SE = 2$; $F < 1$), meaning that whether the shape-judgment task preceded or followed the studied list had no reliable effect on recall of the list. Relative to the control conditions, the remember condition exhibited both retroactive and proactive inhibition: Both List 1 and List 2 recall were lower in the remember condition than in the control condition, $F(1, 234) = 11.48$, $p < .001$, and $F(1, 234) = 30.565$, $p < .0001$, respectively.

Recall of Lists 1 and 2 in the forget condition was also impaired relative to the comparable control lists. The lower recall for List 1 in the forget than in the control condition, $F(1, 234) = 46.79$, $p < .0001$, may be attributable solely to intentional forgetting, or List 1 may also have suffered from retroactive interference in the forget condition as in the remember condition. The reliably lower recall for List 2 in the forget than in the control condition, $F(1, 234) = 6.25$, $p < .05$, is a departure from the typical directed-forgetting pattern, in which these two conditions do not differ. The lack of a complete abatement of proactive interference indicates that List 1 continued to exert an inhibitory influence on List 2 recall, which could mean that the highly interassociated DRM lists are not as susceptible to intentional forgetting as are lists of unrelated words. Alternatively, the large sample size in this experiment may have provided enough power to detect a reliable difference that earlier experiments had been unable to detect. Lending some support to the latter possibility is the similarity between the magnitude of recall in these two conditions versus that in the same conditions in E. L. Bjork and Bjork's (1996) Experiment 1, which used lists of 16 unrelated words and obtained recall rates of 61% and 69% for List 2 in the forget and control conditions, respectively, but found the difference unreliable with 18 participants in a within-subjects design.

Given (with the one exception just noted) that studied-item recall exhibited a typical directed-forgetting pattern—including, in particular, impairment of List 1 recall in the forget condition, impairment of List 2 recall in the remember condition as a result of proactive interference, and (at least partial) abatement of the proactive interference for List 2 in the forget condition—we can now address the question of most interest in this experiment: What happens to critical-item intrusions when studied-item retrieval is impaired pursuant to the intentional forgetting of List 1?

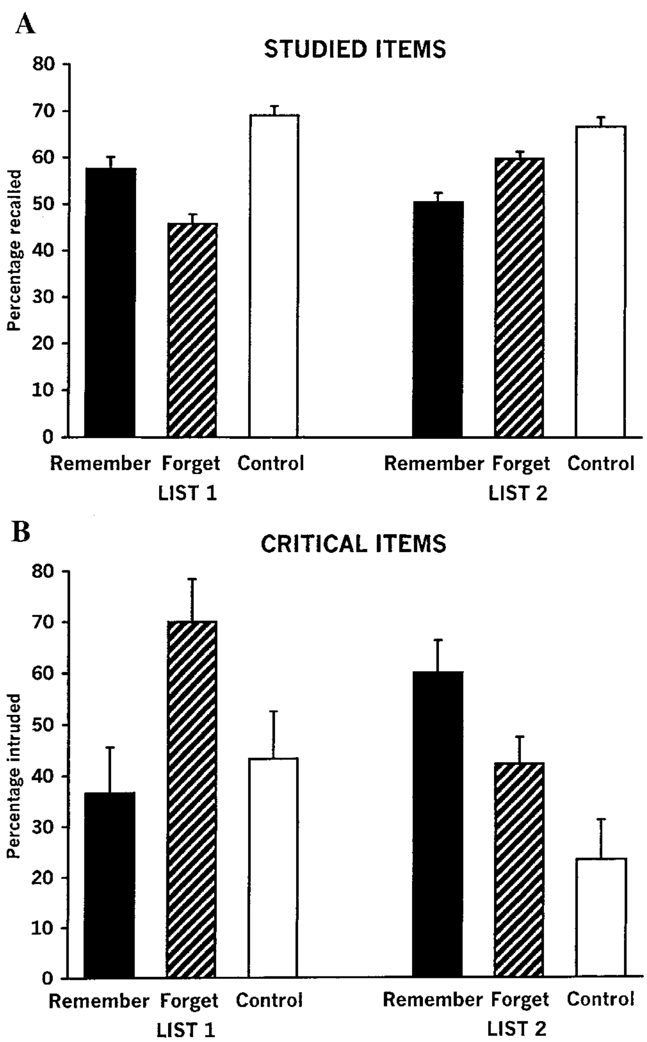

Figure 1. Mean percentage and standard error of studied items recalled (A) and critical items intruded (B) as a function of interlist cue and list study position for first-tested lists in Experiment 1.

Critical-item intrusions. As can readily be seen from Panel B of Figure 1, the critical-item intrusions exhibited a near mirror-image pattern inverse to the studied-item recall pattern. Almost without exception, lower studied-item recall was coupled with more critical-item intrusions. The statistical analyses confirmed what is apparent from observation.

Because each participant contributed only one observation to the critical-item analysis, the data were analyzed with chi-square analysis and the related weighted-least-squares method (although a separate between-subjects ANOVA yielded similar results). The analysis revealed a reliable main effect of interlist cue, $\chi^2(2, N = 270) = 8.75$, $p < .05$, reflecting fewer intrusions in the control condition (33%) than in the forget condition (56%), $\chi^2(1, N = 180) = 8.70$, $p < .005$, and marginally fewer in the control condition than in the remember condition (48%), $\chi^2(1, N = 150) = 3.48$, $p = .062$, with the forget and remember conditions not differing reliably, $\chi^2(1, N = 210) = 1.13$, $p > .10$. This pattern is essentially the inverse of the pattern for studied items.

More important for our purposes, there was a reliable Interlist Cue × List Study Position interaction, $\chi^2(2, N = 270) = 13.39$, $p < .005$, and in particular, a reliable simple interaction in the forget and remember conditions apart from the control condition, $\chi^2(1, N = 210) = 12.18$, $p < .001$. Planned comparisons revealed a pattern in the forget and remember conditions that is a mirror image of the typical directed-forgetting pattern we observed for studied-item recall: Critical-item intrusions were in this case reliably higher for List 1 in the forget condition (70%) than in the remember condition (37%), $\chi^2(1, N = 60) = 6.70$, $p < .01$, but were reliably lower for List 2 in the forget condition (42%) than in the remember condition (60%), $\chi^2(1, N = 150) = 4.55$, $p < .05$. It bears mentioning that the 70% intrusion rate for List 1 in the forget condition is the highest critical-item intrusion rate reported in the literature to date (cf. Roediger, Balota, & Watson, 2001). List 2 critical-item intrusion rates were similar for the initial and added participants in both the forget condition—43% and 42%, respectively—and the remember condition—63% and 57%, respectively.

Planned comparisons involving the control conditions yielded some further evidence of the inverse relationship between studied- and critical-item output: The intrusion rate for List 2 was reliably lower in the control condition (23%) than in the remember condition, $\chi^2(1, N = 150) = 10.78$, $p = .001$, and marginally lower than in the forget condition, $\chi^2(1, N = 120) = 3.43$, $p = .064$, and the intrusion rate for List 1 was reliably lower in the control condition (43%) than in the forget condition, $\chi^2(1, N = 60) = 4.34$, $p < .05$. However, there were also two deviations from the pattern inversion: The critical-item intrusion rate for List 1 in the control and remember conditions did not differ reliably ($\chi^2 < 1$), but was marginally higher for List 1 than for List 2 in the control condition, $\chi^2(1, N = 60) = 2.70$, $p = .10$. Both of these deviations appear to have arisen from an unexpectedly high intrusion rate for List 1 in the control condition, which indicates that critical-item intrusions can increase over a retention interval filled by an unrelated nonverbal task even if studied-item recall is not affected by such an interpolated task.

Comparisons of studied- and critical-item output percentages. To directly test the reliability of the interactions apparent from the mirror-image patterns for studied and critical items, we used a mixed-subjects ANOVA. The analysis revealed a reliable simple Interlist Cue × List Study Position × Item Type interaction for the forget and remember conditions apart from the control conditions, $F(1, 234) = 24.67$, $MSE = 1{,}120.43$, $p < .0001$, reflecting lower recall and more critical-item intrusions for List 1 in the forget than in the remember condition, $F(1, 234) = 13.62$, $MSE = 1{,}120.43$, $p < .0005$, and higher recall and fewer intrusions for List 2 in the forget than in the remember condition, $F(1, 234) = 11.84$, $MSE = 1{,}120.43$, $p < .001$. The ubiquity of the inverted relationship is indicated by the reliability of all but one of the remaining simple pairwise Interlist Cue × Item Type interactions for each list study position: List 1 in the control and forget conditions, $F(1, 234) = 16.74$, $MSE = 1{,}120.43$, $p < .0001$; List 2 in the control and forget conditions, $F(1, 234) = 6.71$, $MSE = 1{,}120.43$, $p < .01$; and List 2 in the control and remember conditions, $F(1, 234) = 25.07$, $MSE = 1{,}120.43$, $p < .0001$. Only for List 1 in the control and remember conditions did the studied- and critical-item output percentages not reliably interact ($F < 1$).[5] The abundance of these reliable, diverging interactions underscores the basic finding in this experiment: To the extent that recall of studied items is impaired, whether for List 1 or List 2, there is a greater tendency to intrude the critical item.

Noncritical-item intrusions. Intrusions of items other than the critical item are set forth in Table 1. Both interlist intrusions (studied items from one list that were recalled as having appeared on the other list) and extralist intrusions (words that had not appeared on either list) were relatively rare. Moreover, in marked contrast to studied-item recall and critical-item intrusions, the pattern of noncritical-item intrusions bore little relation to the manipulated variables, as the only reliable difference was a greater tendency to make extralist intrusions during List 1 recall than during List 2 recall, $\chi^2(1, N = 270) = 5.33$, $p < .025$. These results suggest it is unlikely that response criterion differences contributed to the diverging results for studied-item recall and critical-item intrusions.

Discussion

Using DRM lists, we found typical directed-forgetting effects for studied items and an inverse pattern for critical-item intrusions, with impaired retrieval of studied items being coupled with an increase in critical-item intrusions. This dissociation of false and veridical recall is consistent with the predictions of the source-monitoring, attribution, and fuzzy-trace theories, all of which assume dissociable effects for episodic and semantic information. Furthermore, the dissociation is not consistent with the predictions of the IAR theory, which assumes that the critical item should behave like a studied item, nor is it consistent with those spreading-activation theories that assume that output at recall is determined solely by an item's semantic activation.

[5] We also examined the key two-way and three-way interactions of instruction and list study position with item type by conducting a median split of all lists in all conditions on the basis of percentage of studied items recalled and then using the median split together with the critical-item intrusions to classify each participant for chi-square-type analyses. These analyses yielded results and conclusions similar to those from the ANOVA.

Table 1
Mean and Standard Error of Noncritical-Item Intrusions and Percentage of Participants Intruding at Least One Noncritical Item as a Function of List Study Position and Interlist Cue for First-Tested Lists in Experiment 1

List study position and interlist cue	Noncritical-item intrusions: No. per list						% of participants intruding at least 1 item		
	Extralist intrusions		Interlist intrusions		Either		Extralist intrusions	Interlist intrusions	Either
	M	*SE*	*M*	*SE*	*M*	*SE*			
List 1									
Remember	0.30	.11	0.10	.07	0.40	.12	23	7	30
Forget	0.30	.09	0.30	.12	0.60	.16	30	23	47
Control	0.33	.11			0.33	.11	27		27
List 2									
Remember	0.23	.08	0.22	.08	0.45	.12	17	15	27
Forget	0.20	.05	0.32	.08	0.52	.11	18	19	29
Control	0.17	.14			0.17	.14	7		7

Experiment 2: Part-List Cuing

Given the clear inverse pattern for false and veridical recall with directed forgetting, a logical inquiry is the extent to which this pattern prevails for other forms of retrieval inhibition. In Experiment 2, we sought to induce retrieval inhibition by using part-list cuing. We manipulated three variables within subjects: (a) cuing (part-list cues either were or were not provided at test); (b) number of cues (when provided, part-list cues numbered either four or eight); and (c) cue associative strength (when provided, part-list cues were either the strongest or the weakest associates of the critical item). These three variables yielded five cuing conditions: Recall of a list was either uncued or cued with the strongest four (Strong 4), strongest eight (Strong 8), weakest four (Weak 4), or weakest eight (Weak 8) associates of the critical item.

We presented items auditorily to avoid possible floor effects on critical-item intrusions; intrusion rates for immediate recall following visual presentation were only 23% for the List 2 single-list control condition in Experiment 1 and only 31% in Robinson and Roediger (1997), whereas auditory presentation has typically yielded much higher rates (e.g., 55% in Roediger & McDermott, 1995). We presented items on each list in descending order of associative strength, as in Experiment 1 and in Roediger and McDermott. Because such a presentation order confounds the associative strength and serial position of the studied items (cf. Kimball, Bjork, & Bjork, 2001), we consider both variables in interpreting our results. For ease of discussion, however, we identify studied items by their associative strength.

Several theories of part-list cuing have varying amounts of support in the literature (for reviews see Nickerson, 1984; Roediger & Neely, 1982), and it is beyond the scope of this article to discuss their relative merits. We based our predictions on one particular leading theory—the retrieval competition theory of Rundus (1973). This theory assumes that during study, participants induce a hierarchical structure of retrieval cues to guide later memory search. Studied items are associated to particular retrieval cues on the basis of some reason for grouping or some organizing idea. The strength of the association between an item and its retrieval cue is initially determined by preexperimental associative strength, which is then incremented on presentation of the item at study.

During recall, according to Rundus's (1973) theory, access to studied items is determined probabilistically by strength-dependent competition among all items associated to a particular retrieval cue, ensuring that the stronger associates of a retrieval cue are retrieved more frequently. Each successful retrieval of an item is assumed to increment the strength of its association to its retrieval cue, and because sampling is with replacement, repeated retrievals of the item become even more likely. Each such repeated retrieval constitutes a failure to retrieve a new item, and the participant is assumed to stop searching with a particular retrieval cue (and finally to stop searching altogether) when the search yields a criterial number of successive failures to retrieve a new item.

The retrieval competition theory explains the part-list cuing effect by assuming that on presentation of a part-list cue at test, its association to its retrieval cue becomes stronger. Because a strengthened association renders the item increasingly likely to be retrieved when its retrieval cue is used to search memory, providing the part-list cues at test introduces a bias favoring their retrieval at the expense of noncue items. In the case of the part-list cues, of course, even the first such retrieval is a failure to retrieve a new item, as is each subsequent retrieval. (It might seem odd to think of the already-present part-list cues as being "retrieved," but because sampling is with replacement, the part-list cues may be retrieved and reretrieved by means of the memory search process, just as previously recalled items may be retrieved again even though they, too, are present at the time of the subsequent retrieval.) As a consequence, the participant reaches the stopping criterion having retrieved fewer noncue items for cued lists than for uncued lists. As the number of cues increases, so does the retrieval bias and the impairment in retrieval of noncue items (e.g., Roediger, 1973; Roediger et al., 1977; Rundus, 1973; Slamecka, 1968). However, recall of noncue targets may be facilitated to the extent that a part-list cue provides access to a retrieval cue that would not otherwise be accessed (e.g., Penney, 1988; Roediger, 1973, 1974; Rundus, 1973; Slamecka, 1972).

Applying the Rundus (1973) model to our design, we predicted that the recall of noncue studied items would be impaired with cuing. By using cues drawn from blocks of adjoining serial positions, we sought to avoid providing access to retrieval cues that would not be accessed otherwise, as may occur when cues are drawn from throughout the study list (cf. Raaijmakers & Phaf, 1999; Sloman, Bower, & Rohrer, 1991; Williams & Zacks, 2000). We also expected that increasing the number of cues would increase the impairment of noncue recall. Under the retrieval competition theory, however, there does not seem to be a basis for predicting a priori that the degree of impairment owing to part-list cuing should be affected by the part-list cues' strength of association to the critical item or by their serial position.

For predictions regarding intrusions of critical items, we began by assuming that critical items are prone to false recall because they are cued by means of some association to one or more retrieval cues that a given participant induces during study. Such associations exist prior to the experiment, but we assume that they may be strengthened if the critical item is generated during the study phase, as assumed by the IAR and source-monitoring theories.

If we assume that part-list cuing affects the critical item's associations to retrieval cues in the same way as it does studied items' associations, then the model predicts that critical-item intrusions should generally decrease with retrieval inhibition induced by part-list cuing rather than increase, as occurred with retrieval inhibition induced by directed forgetting. As the number of cues increases, critical-item intrusions should decline, much like studied-item recall. In addition, and in contrast to the prediction made for studied-item recall, using cues that are stronger associates of the critical item may be expected to reduce critical-item intrusions. This prediction is based on the assumption that the critical item and its stronger associates are more likely to share a retrieval cue, and the critical item is therefore likely to suffer a greater competitive disadvantage when the stronger associates' associations to the shared retrieval cue are strengthened by their re-presentation during recall. According to similar reasoning, noncritical-item intrusions should also generally decline with cuing, although cue associative strength should not affect such intrusions, inasmuch as, like studied items, intruded noncritical items should be associated more or less evenly to retrieval cues induced throughout study of the list.

Method

Participants. Participants were 115 students, including 79 UCLA undergraduates enrolled in the introductory psychology course and participating for partial course credit and 36 psychology honors students from a local Los Angeles–area high school who participated as a class exercise. The data from an additional 19 UCLA undergraduates and 8 high school students were excluded because they did not avoid recalling part-list cues as instructed. Given evidence that permitting or prohibiting recall of part-list cues affects retrieval dynamics (see, e.g., Kimball et al., 2001; Roediger et al., 1977), we excluded participants who recalled more than a total of four part-list cue words across the eight cued lists.

Materials and apparatus. In the study phase, an audiotape presented 12 DRM lists of 15 words each, selected from the 18 (of 36) lists that Stadler, Roediger, and McDermott (1999) found were most likely to induce a critical-item intrusion (mean critical-item intrusion rate = 53%; mean studied-item recall = 59%). Lists with the following critical items were presented in the following randomly determined order: *smell*, *anger*, *sleep*, *sweet*, *cold*, *slow*, *smoke*, *rough*, *needle*, *soft*, *chair*, *window* (see the appendix in Stadler et al., for complete lists). Note that five of the lists—*anger*, *sweet*, *slow*, *rough*, and *soft*—had also been used in Experiment 1 (the *spider* list was not among the top 18 intrusion-inducing lists in Stadler et al.). Words on each list were ordered from strongest to weakest associates of the critical item.

A male voice (Daniel R. Kimball's) spoke the words at a rate of 1.5 s per word. Before the start of each list, the voice alerted participants to get ready and then paused briefly before beginning the list. After the last word in the list, a beep signaled participants to turn to the next page in the response booklet to recall the words from that list. There then followed a 90-s interval of blank tape, corresponding to the recall period, before the taped voice instructed participants to stop recalling that list and turn their answer sheet over. This sequence repeated for all 12 lists.

Each response booklet contained 24 pages—12 answer sheets interleaved with 12 sheets that served to obscure the next answer sheet while participants listened to each list on the tape. At the top center of each answer sheet, eight items appeared in a column. Each item was either a word from the list (one of the part-list cues) or a row of symbols (pound signs). On the four sheets for the uncued lists, there appeared eight rows of symbols and no words. On another four sheets, eight words appeared as cues; the eight strongest associates (Strong 8 condition) and the eight weakest associates (Weak 8 condition) appeared on two sheets each. On the remaining four sheets, four rows of symbols appeared along with four words as cues; the four strongest associates (Strong 4 condition) and the four weakest associates (Weak 4 condition) appeared on two sheets each. The assignment of the 12 lists to the cuing conditions was counterbalanced by using block randomization to ensure distribution of the conditions across the study sequence, resulting in six response booklet versions that were randomly assigned to participants.

Test cue order was determined for each list by assigning the eight strongest associates and the eight weakest associates to separate random orders, and those two orders were then used on all Strong 8 and Weak 8 answer sheets for that list, respectively. Randomization of test cue order was designed to minimize the likelihood that participants could use any correspondence between study order and test cue order to assist in deciding whether the critical item had appeared in the original list. For the Strong 4 and Weak 4 answer sheets, the four strongest or weakest associates appeared in the same ordinal position as on the Strong 8 and Weak 8 answer sheets, respectively, and the four omitted words were replaced by rows of symbols.

Procedure. The high school students participated in two groups of approximately equal size. The UCLA students participated individually or in groups of no more than 5. At the start of the experiment, the participants received a response booklet and the experimenter read instructions that described the lists on the audiotape, the answer sheets in the response booklets, and the tasks to be performed at each step. The recall instructions stated that the cue words were provided to help participants recall the other words from the list, that they were to check off all eight items at the top of each answer sheet before starting to recall each list, and that they were to recall (write down) only noncue list words. Participants were also warned not to guess whether they had heard a word but to write down only the words they were confident they had heard. After the instructions, the experimenter started the audiotape and participants worked through the lists and answer sheets in accordance with the taped sequence: For each of the 12 lists, participants listened to the list for approximately 23 s, then turned to the answer sheet for that list, checked off the eight items at the top, and wrote down words from the list for 90 s before being signaled to stop recalling that list and turn their answer sheet over.

Results

Studied-item recall. Mean percentages for studied-item recall are depicted in Figure 2. The studied-item recall data were analyzed separately for three overlapping sets of seven studied items to avoid item effects when comparing across cuing conditions. Thus, recall of the seven strongest associates of the critical item, appearing in Serial Positions 1–7, was compared for the uncued, Weak 4, and Weak 8 cuing conditions; recall of the moderately strong associates, appearing in Positions 5–11, was compared for the uncued, Weak 4, and Strong 4 cuing conditions; and recall of the seven weakest associates, in Positions 9–15, was compared for the uncued, Strong 4, and Strong 8 cuing conditions.

Within-subjects ANOVAs revealed reliable declines with part-list cuing for all three target sets using all sets of part-list cues. Recall of the strongest associates was higher for uncued lists ($M = 55$, $SE = 1$) than for either Weak 4 lists ($M = 51$, $SE = 2$), $F(1, 109) = 10.34$, $MSE = 110.42$, $p < .0025$, or Weak 8 lists ($M = 49$, $SE = 1$), $F(1, 109) = 21.34$, $MSE = 98.77$, $p < .0001$. Recall of the moderately strong associates was higher for uncued lists ($M = 43$, $SE = 1$) than for either Strong 4 lists ($M = 38$, $SE = 2$), $F(1, 109) = 10.39$, $MSE = 131.18$, $p < .0025$, or Weak 4 lists ($M = 37$, $SE = 1$), $F(1, 109) = 15.67$, $MSE = 121.72$, $p < .0001$. Recall of the weakest associates was higher for uncued lists ($M = 53$, $SE = 1$) than for either Strong 4 lists ($M = 48$, $SE = 1$), $F(1, 109) = 10.21$, $MSE = 120.27$, $p < .0025$, or Strong 8 lists, ($M = 43$, $SE = 1$), $F(1, 109) = 44.94$, $MSE = 118.86$, $p < .0001$. Impairment occurred consistently across serial positions, with average recall at every serial position being numerically lower in each cued condition than in the uncued condition. The five lists that had been used in Experiment 1 exhibited a 6% decline in studied-item recall with cuing, the same as for the seven new lists (averaging across all applicable cuing conditions for each target serial position).

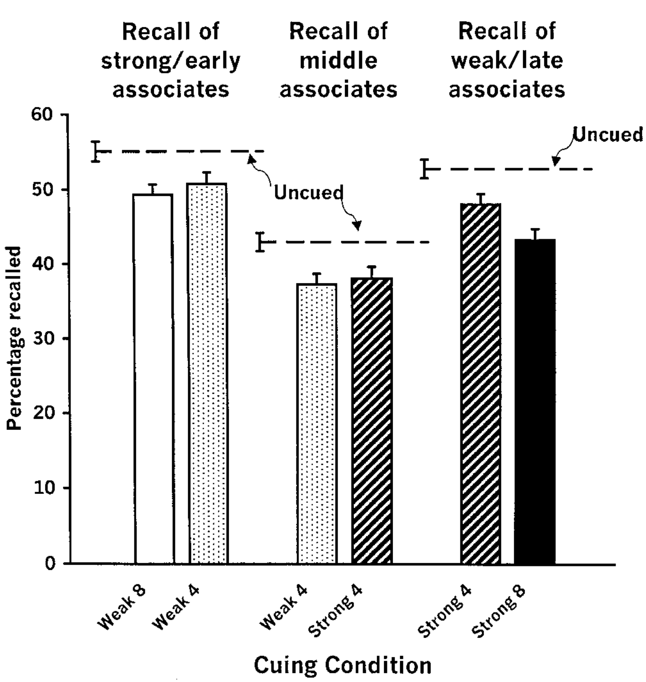

Figure 2. Mean percentage recall and standard error as a function of cuing condition for strong/early, middle, and weak/late noncue studied items in Experiment 2.

There was only equivocal support for our prediction of a negative effect on target recall of increasing the number of cues: Recall of the weakest associates was reliably lower for Strong 8 lists than for Strong 4 lists, $F(1, 109) = 9.40$, $MSE = 153.91$, $p < .005$, but recall of the strongest associates did not differ reliably between the Weak 4 and Weak 8 conditions ($F < 1$). As predicted, cue associative strength did not affect studied-item recall, as recall of the moderately strong associates did not differ reliably between the Strong 4 and Weak 4 conditions ($F < 1$).

Given that we observed the predicted retrieval inhibition of noncue targets with part-list cuing, we turn to address the question of more interest in this study: How are critical-item intrusions affected when access to studied items is impaired by part-list cuing?

Critical-item intrusions. Mean percentages of critical items intruded are depicted in Figure 3. As predicted, and in marked contrast to the directed-forgetting pattern, critical-item intrusions actually declined with cuing. A within-subjects ANOVA revealed reliably more critical-item intrusions for uncued lists ($M = 54$, $SE = 3$) than for cued lists ($M = 44$, $SE = 2$), $F(1, 109) = 14.08$, $MSE = 547.47$, $p < .0005$. The starkness of the contrast with the critical-item results from Experiment 1 is evident in the decline in critical-item intrusions with cuing for the five lists that had been used in Experiment 1: from 51% for uncued lists to 44% for cued lists (the decline was from 56% to 44% for the seven new lists).

There was a reliable effect of cue number on critical-item intrusions, $F(2, 218) = 10.20$, $MSE = 509.07$, $p < .0001$, with planned comparisons revealing reliably more intrusions for uncued lists than for lists with either four cues ($M = 47$, $SE = 3$), $F(1, 109) = 5.39$, $MSE = 622.44$, $p < .05$, or eight cues ($M = 40$, $SE = 3$), $F(1, 109) = 17.61$, $MSE = 589.71$, $p < .0001$, and reliably more intrusions for lists with four cues than for lists with eight cues, $F(1, 109) = 5.69$, $MSE = 470.67$, $p < .05$.

Strength of association between the cues and the critical item also had a reliable effect on critical-item intrusions, $F(1, 109) = 13.49$, $MSE = 550.50$, $p < .0001$, with planned comparisons indicating fewer intrusions for lists with strong associates as cues ($M = 38$, $SE = 3$) than for either uncued lists, $F(1, 109) = 26.44$, $MSE = 705.60$, $p < .0001$, or lists with weak associates as cues ($M = 50$, $SE = 3$), $F(1, 109) = 12.91$, $MSE = 553.54$, $p = .0005$. However, there was no reliable difference between uncued lists and lists with weak associates as cues, $F(1, 109) = 2.00$, $MSE = 758.36$, $p > .10$. The effects of cue number and cue associative strength did not interact reliably, $F(1, 109) = 1.57$, $MSE = 885.61$, $p > .10$.

Pairwise comparisons among individual cuing conditions (using a more conservative familywise reliability criterion of $p < .01$) indicated there were reliably fewer critical-item intrusions in the Strong 4 condition than in the uncued condition, $F(1, 109) = 8.47$, $MSE = 740.72$, $p < .005$, and reliably fewer intrusions in the Strong 8 condition ($M = 33$, $SE = 3$) than in each of the other four conditions: uncued, $F(1, 109) = 32.48$, $MSE = 762.36$, $p < .0001$; Strong 4 ($M = 43$, $SE = 3$), $F(1, 109) = 6.87$, $MSE = 889.35$, $p < .01$; Weak 4 ($M = 51$, $SE = 4$), $F(1, 109) = 15.79$, $MSE = 894.19$, $p < .0005$; and Weak 8 ($M = 48$, $SE = 4$), $F(1, 109) = 13.75$, $MSE = 1{,}176.36$, $p = .0001$. No other pairwise comparisons were

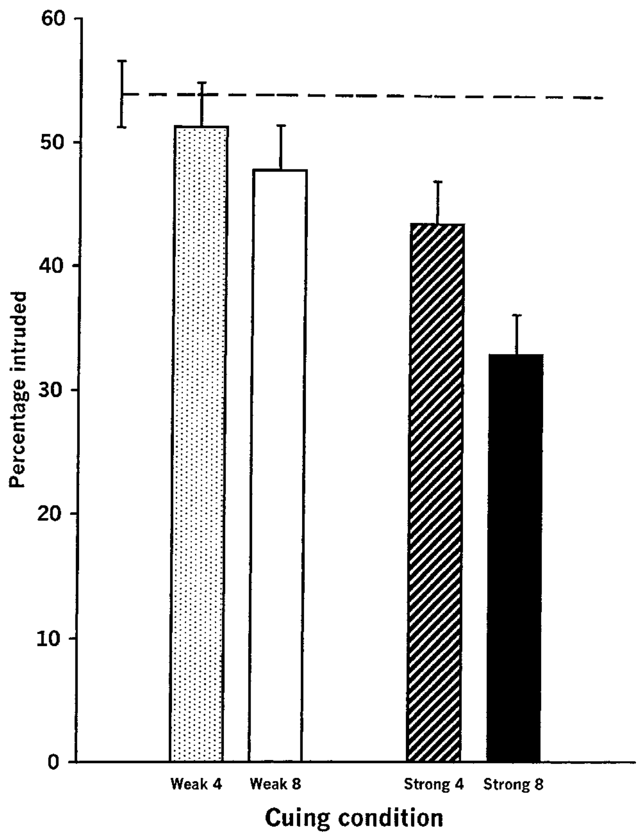

Figure 3. Mean percentage and standard error of critical items intruded as a function of cuing condition in Experiment 2.

reliable. Chi-square-type analyses with weighted least squares were also conducted on pairwise comparisons among those conditions with two or fewer observations per participant, with results similar to those obtained from the ANOVA. Note that the Strong 8 condition, the cuing condition that showed reliably fewer critical-item intrusions than in each of the other conditions, was also the condition that exhibited the largest part-list cuing decrement for studied-item recall, underscoring the departure of the part-list cuing pattern from the inverse pattern observed with directed forgetting.

The critical item also tended to be output increasingly later as cue number and cue associative strength increased, providing further evidence that these variables adversely affected the accessibility of the critical item. Critical-item output percentiles for the uncued, Weak 4, Weak 8, Strong 4, and Strong 8 conditions were 66, 63, 72, 78, and 86, respectively.

Comparisons of studied- and critical-item output percentages. The similarity of the studied- and critical-item patterns can be seen rather vividly in Figure 4, which plots the decline in studied- and critical-item output percentages as difference scores between means for each of the four cued conditions and the mean for the uncued condition. With strong associates as cues, there were consistently reliable declines in both studied- and critical-item output percentages and the magnitude of the decline increased reliably for both item types as cue number increased. With weak associates as cues, the declines in studied- and critical-item output percentages were similar in magnitude, although reliable only for the studied items, and the declines increased slightly but unreliably for both item types as cue number increased. As a result of this similarity in patterns, there was only one reliable interaction involving item type: The decline in critical-item intrusions was greater than the decline in studied-item recall on Strong 8 lists relative to uncued lists, $F(1, 109) = 6.87$, $MSE = 516.72$, $p = .01$. Notably, that interaction did not involve an inverse relationship between studied-item recall and critical-item intrusions, as had the interactions in Experiment 1, but rather involved a difference in the magnitude of declines in output percentages. The differences between the two experiments in the prevalence and character of interactions underscore the difference in the effects on critical-item intrusions engendered by impairing retrieval of studied items through directed forgetting and part-list cuing.

Noncritical-item intrusions. As with directed forgetting, intrusions of items other than the critical item occurred at a relatively low rate, as can be seen in Table 2. Interlist intrusions were extremely rare, and as predicted, extralist intrusions declined generally with cuing and declined as cue number increased but were not affected by cue associative strength, consistent with a more even distribution of their associations to retrieval cues induced throughout the input sequence.

Discussion

Using part-list cuing with DRM lists, we succeeded in impairing retrieval of studied items, as we had with directed forgetting. However, in marked contrast to directed forgetting, critical-item intrusions also declined with part-list cuing. In addition, the magnitude of the decline generally increased as cue number increased

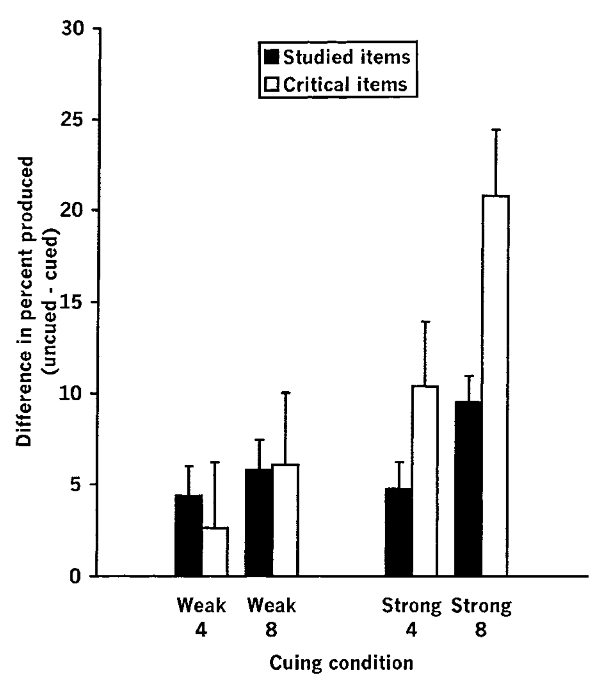

Figure 4. Difference in mean percentage and standard error of studied items and critical items produced in uncued conditions versus cued conditions as a function of cuing condition in Experiment 2.

Table 2
Mean and Standard Error of Noncritical-Item Intrusions per List and Percentage of Participants Intruding at Least One Noncritical Item per List as a Function of Cuing Condition in Experiment 2

	Noncritical-item intrusions								
	No. per list						% of participants intruding at least 1 item per list		
			Interlist					Interlist	
	Extralist		Later		Earlier		Extralist	Later	Earlier
Cuing condition	*M*	*SE*	*M*	*SE*	*M*	*SE*			
Uncued	0.30	.03	0.02	.00	0.01	.01	25	2	1
Cued									
Weak 4	0.20	.03	0.02	.01	0.03	.01	18	2	3
Strong 4	0.22	.03	0.02	.01	0.01	.01	20	1	1
Weak 8	0.14	.03	0.02	.01	0.02	.01	13	2	2
Strong 8	0.13	.02	0.01	.01	0.01	.01	12	1	1

Note. Interlist, later = intrusions from a list that was presented later; Interlist, earlier = intrusions from a list that was presented earlier.

for studied and critical items. These results are consistent with predictions based on an extension of the retrieval competition theory (Rundus, 1973).

Also as predicted by the extended retrieval competition theory, the decline in critical-item intrusions—but not in studied-item recall—was larger when the stronger, earlier presented associates of the critical item were used as part-list cues rather than the weaker, later presented associates. However, because we confounded the studied items' serial position and strength of association to the critical item, it is not clear whether this greater decline was due to episodic or semantic factors. Although the possible effect of semantic associations is straightforward, it is also possible that the critical item may have been intruded consciously early in the study phase and then become episodically associated with the early-presented studied items or their retrieval cue (for evidence favoring this latter interpretation, see Kimball et al., 2001, in which cue serial position and cue associative strength were manipulated factorially).

Although predictions based on the retrieval competition theory were largely supported by the results, we do not rule out other explanations, including theories that assume that the part-list cues lead participants to change retrieval strategies (e.g., the strategy disruption theory [D. R. Basden & Basden, 1995; D. R. Basden, Basden, & Galloway, 1977; see Reysen & Nairne, in press], and the associative sampling bias theory [Raaijmakers & Phaf, 1999; Raaijmakers & Shiffrin, 1981]). However, our results present a challenge for other theories in that they must account for co-occurring declines in output of both studied and critical items with part-list cuing.

General Discussion

Two different methods of impairing retrieval access to studied list items—directed forgetting and part-list cuing—had opposite effects on intrusions of an unstudied critical item that was strongly associated semantically to the list items. When participants intended to forget a studied list, they were less likely to recall the studied items but more likely to intrude the critical item than when they intended to remember the list. However, when participants who intended to remember a list received part-list cues at the time of test, not only did recall of the remaining studied items decline but so did critical-item intrusions.

Globally, we think the results point to differences in the effects of directed forgetting and part-list cuing on subsequent access to episodic and semantic information. For directed forgetting, evidence indicates that the cue to forget a studied list impairs episodic access to the entire list, rendering the unaided retrieval of the studied items more difficult (e.g., Geiselman et al., 1983) but leaving their semantic activation unimpaired (e.g., B. H. Basden et al., 1993; E. L. Bjork & Bjork, 1996). The observed increase in critical-item intrusions obtained in Experiment 1 is consistent with the notion that impairing access to the List 1 episode also impairs participants' ability to use episodically distinctive information to determine that the semantically activated critical item was not on the list. This interpretation is consistent with predictions made by extensions of three theories—the fuzzy-trace theory (Brainerd & Reyna, 1998; Reyna & Brainerd, 1995), the source-monitoring framework (Johnson et al., 1993; Mather et al., 1997), and the attributional theory of remembering (Jacoby et al., 1989).

The dynamics of part-list cuing, in our view, are quite different and act primarily at the item level. Providing some list items as cues does not impede access to the prior list episode per se. Instead, part-list cuing enhances episodic access to some items—the part-list cues—while impairing episodic access to other items—the noncue targets. Rundus (1973) assumed that these differences in accessibility are due to retrieval competition, with the hyperaccessible part-list cues rendering the remaining studied items less accessible through strength-dependent competition for retrieval access. We extended Rundus's theory by assuming that retrieval competition affects the critical item in the same way as it does noncue studied items, predicting correctly that output percentages for both item types would generally decline with cuing. The results also supported other predictions of the extended theory regarding the effects of cue number and cue associative strength or serial position.

Can a Single Theory Explain Both Sets of Results?

Our results present a challenge to theories seeking to explain both directed forgetting and part-list cuing. We now consider whether both sets of results can be explained by the retrieval competition theory or any of the three theories we have discussed that are consistent with our directed-forgetting results.

For the retrieval competition theory to explain the studied-item recall results for directed forgetting, it seems necessary to assume that, following the interlist cue to forget, the associations between List 2 items and their retrieval cues are strengthened at the expense of those for List 1, in contrast to the more evenly distributed strengthening of such associations for Lists 1 and 2 given an interlist cue to remember. This differential strengthening could occur through differential rehearsal alone (see R. A. Bjork, 1970, 1972; Kimball & Metcalfe, 2001), although intentional suppression of List 1 items may also make a contribution (see R. A. Bjork, 1989; Geiselman et al., 1983). Consistent with this differential-strengthening hypothesis, Conway et al. (2000) found that interfering with processing during List 2 encoding reduced or eliminated the directed-forgetting effect for List 1. To predict the diverging pattern of studied-item recall and critical-item intrusions for directed forgetting, however, the retrieval competition theory would need to assume that studied and critical items are affected differently, either by retrieval competition itself—an assumption at odds with the similar effects on both item types observed with part-list cuing—or by some other mechanism operating in addition to retrieval competition, which would threaten parsimony.

Conversely, without added assumptions, the fuzzy-trace, source-monitoring, and attribution theories do not seem to predict clearly that part-list cuing would yield declines in both the recall of noncue studied items and the intrusions of critical items. To begin with, it is not clear that the theories predict the basic part-list cuing effect on studied items, at least without added assumptions. For example, the theories might be interpreted to predict that the increase in accessibility of episodic information for the part-list cues would generalize to other list items, resulting in an increase in recall of noncued items; or if accessibility of episodic information is instead item specific, the cuing might be predicted to have no effect on noncue recall. Either prediction would clearly be contrary to the evidence.

On the other hand, part-list cuing might affect source attributions under the source-monitoring and attributional theories. From the standpoint of the source-monitoring framework, the increase in access to episodically distinctive information for the part-list cues might cause participants to shift their decision criterion, requiring that a similarly substantial amount of such episodic information must be available before they are willing to attribute activation of an item to its presentation during study rather than to its internal generation by means of semantic associative processes. Similarly, from the standpoint of the attribution theory, the increase in access to episodically distinctive information may prompt participants to use more analytical processing rather than rely on an accessibility heuristic, thus also effectively raising the source attribution criterion. Such a criterion shift would result in a decrease in both noncue studied-item recall and critical-item intrusions—consistent with our results. Of course, one might wonder why such a criterion shift would not also occur in directed forgetting, with the increased accessibility of the List 2 items in the forget condition causing participants to raise the source attribution criterion for List 1, resulting in declines in both critical-item intrusions and studied-item recall. In any event, whether such decreases in recall and intrusions with part-list cuing are due to such a criterion shift or to differences in item accessibility—as assumed by the retrieval competition theory—is an empirical question requiring further investigation.

A less parsimonious alternative that bears consideration is the possibility that both retrieval competition and the relative accessibility of episodic and semantic information may play roles in both directed forgetting and part-list cuing. According to this view, the critical difference between the two paradigms is that retrieval competition occurs between lists in directed forgetting and within lists in part-list cuing. In directed forgetting, retrieval competition could underlie the decrease and increase in the episodic accessibility of List 1 and List 2 in the forget condition, respectively (see Conway et al., 2000, for a similar account). In turn—and as predicted by fuzzy-trace, source-monitoring, and attribution theories—these differences in list-level episodic accessibility would lead to differences in the relative accessibility or use of semantic and episodic information and, consequently, to the divergent pattern of studied-item recall and critical-item intrusions. In part-list cuing, by contrast, overall episodic access to the list is unimpaired, so the overall relative accessibility of semantic and episodic information for the list would be unchanged and would not affect recall. However, the critical item would still suffer the effects of intralist retrieval competition and would be intruded less often as the part-list cues gain a competitive advantage over critical items and noncue studied items alike. Of course, this account would require empirical validation.

Comparison to Effects of Time-Dependent Forgetting

Our results can be compared with the results of DRM experiments that have manipulated retention interval, resulting in more passive, time-dependent forgetting (see Thapar & McDermott, 2001, for a review). A consistent finding among such experiments is that veridical recall is affected more adversely than false recall by increasing retention interval (see McDermott, 1996; Thapar & McDermott; Toglia, Neuschatz, & Goodwin, 1999). Less clear is the effect of retention interval on the absolute level of false recall—that is, whether false recall decreases (albeit less steeply than veridical recall), holds steady, or increases across increasing retention intervals. Thapar and McDermott found that false recall declined over 2- and 7-day intervals, although at a slower rate than veridical recall declined. By contrast, Toglia et al. found that false recall held steady over 1- and 3-week intervals. In McDermott, false recall increased somewhat (from .26 to .33) over a 1-day delay, although the increase was relative to recall on the last of five study-test trials on the 1st day, during which the critical item had been progressively edited out of recall output, so this finding appears to have limited applicability in other circumstances (see Thapar & McDermott).

Our part-list cuing results—a consistent decline in studied-item recall coupled with a decline in critical-item intrusions that was reliable in some but not all cuing conditions—seems to resemble the retention interval results. This suggests the possibility that time-dependent forgetting of information may also be fueled by retrieval competition from material processed during the retention interval, as suggested in E. L. Bjork and Bjork (1992), for example. On the other hand, our finding of a dramatic increase in critical-item intrusions with directed forgetting stands in stark contrast to the retention interval findings. Apparently, intentional forgetting can actually increase the occurrence of false memories, not just affect their incidence relative to studied-item recall. This difference in the effects of intentional and time-dependent forgetting on critical-item intrusions could reflect impaired access to episodic information in both cases, coupled with a greater loss of access to semantic information with delayed testing than with the immediate testing used in directed forgetting.

Practical Ramifications

The different effects of directed forgetting and part-list cuing on intrusion of unpresented critical items during recall point to different practical ramifications for real-world instances of instructions to forget and selective cuing. An instruction to forget an event may lead to impaired memory for actual details of the event but an increased likelihood of falsely recalling details that never occurred but that are semantically consistent with other aspects of the event. For example, a perpetrator's instruction to forget an incident of abuse—or the victim's own self-motivated forgetting—may lead the victim to forget crucial episodic details, such as the specific time and place of the event of abuse, but also to intrude incorrect but plausible information, such as identifying information about the perpetrator. Depending on whether these details tend to incriminate or exculpate a particular accused person, innocent persons may be unjustly convicted or guilty persons may go free. Similarly, instructing a jury to disregard testimony may lead the jurors to suppress details about the source of the testimony that would permit a more accurate assessment of its validity, such as the behavior of the witness, but may also increase the jurors' access to inferences that are consistent with the discredited testimony—exactly what the instruction to disregard seeks to avoid.

On the other hand, cuing someone with incomplete information about an event may render that person less able to retrieve the remaining information than if uncued free recall were used, but it may also render the person less likely to intrude false information. For example, an abuse victim provided with some of the details surrounding the abuse, such as the time and place, might be less likely to recall other true details, such as what the victim was wearing or what the perpetrator said, but might also be less likely to intrude false details, such as identifying characteristics of the perpetrator. Although the intrusion of fewer false details might be regarded as a silver lining to the clouding of veridical memory, the similar effects of part-list cuing on veridical and false recall underscore the difficulty in discerning whether recall of a particular detail is accurate.

To offer a specific example, suppose that when first recounting an event, a witness is cued with other details of the event and, as a result, omits one of the other details. Suppose further that the omitted detail is later recalled by the witness, perhaps under conditions more like free recall (see, e.g., D. R. Basden et al., 1977). In that event, it is almost certain that opposing counsel will argue that the omitted detail must not be true or the witness would have recalled it during the first recounting, especially if it is a crucial detail. A plausible counterargument premised on the basic finding of inhibition with part-set cuing is that the detail is true and access to it was simply blocked by the other information used to cue recall during the first recounting. However, this counterargument carries less weight in light of the current research because access to a false detail is also likely to be impaired by the recall cues, especially if it is a crucial detail that is highly associated to the cues. The silver lining thus becomes considerably tarnished.

Concluding Comment

The two experiments reported here suggest that the effect of forgetting on the occurrence of false memories depends critically on the circumstances surrounding the forgetting. If forgetting arises from loss of access to an entire episode—as in directed forgetting—then false memories may become more frequent. On the other hand, if forgetting arises because some elements of an episode are made more accessible than others—as in part-list cuing—then false memories may become less frequent. Thus, our results show that the interplay of these two types of memory errors, forgetting and false memories, does not take the same form in all cases but rather takes quite different forms depending on the way the forgetting occurs.

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