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Ironic Effects of Repetition: Measuring Age-Related Differences in Memory

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Four experiments examined ironic effects of repetition, effects opposite to those desired (cf. D. M. Wegner, 1994). For an exclusion task, participants were to respond "yes" to words heard earlier but "no" to words that were read earlier. Results from young adults given adequate time to respond showed that false alarms to earlier-read words decreased with their repetition. An opposite, ironic effect of repetition was found for elderly adults—false alarms to earlier-read words increased with repetition. Younger adults forced to respond quickly or to perform a secondary task while reading words showed the same ironic effect of repetition as did elderly adults. The process-dissociation procedure (L. L. Jacoby, 1991, 1998) was used to show that factors that produce ironic effects do so by reducing recollection while leaving effects of repetition on familiarity unchanged.

A friend whose mother is suffering symptoms of Alzheimer's disease (AD) tells the story of taking her mother to visit a nursing home, preliminary to her mother's moving there. During an orientation meeting at the nursing home, the rules and regulations were explained, one of which regarded the dining room. The dining room was described as similar to a fine restaurant except that tipping was not required. The absence of tipping was a central theme in the orientation lecture, mentioned frequently to emphasize the quality of care along with the advantages of having paid in advance. At the end of the meeting, the friend's mother was asked whether she had any questions. She replied that she only had one question: "Should I tip?"

Similar to the unwanted effect of repetition mentioned above, repeated asking of questions is one of the most striking and frustrating symptoms of memory impairment resulting from Alzheimer's disease (e.g., Camp & Schaller, 1989). Earlier asking of a question seems to strengthen and increase the probability of later asking the same question for someone suffering with AD, whereas people with normally functioning memory would not repeat the question because of their ability to recollect asking it earlier, as well as the ability to recollect the answer. Repetition may well have two effects, serving both to increase the strength of questioning, an automatic influence, and to increase the probability of recollection of earlier asking, a consciously controlled use of memory. Because of a deficit in recollection, the AD patient is left only with the increase in strength and so repeatedly asks the same question.

The experiments reported here examined unwanted effects of repetition in the memory performance of normal elderly and younger adults by placing automatic influences of memory in opposition to recollection. We refer to unwanted effects of repetition as *ironic effects* to highlight their similarity to the ironic effects described by Wegner (1994). Wegner has shown that attempts to avoid mental states can have the ironic effect of increasing the probability of their occurrence. As in the "tipping" example, the result is an automatic influence that can produce an effect opposite to what is desired if left unopposed by cognitive control. Ironic effects of repetition are likely much less extreme for normal elderly adults than for AD patients and might even have a different neural basis. However, older adults are more susceptible to a variety of memory distortions and illusions than are younger adults. For example, older adults are more likely than are younger adults to claim mistakenly that a recently viewed nonfamous name or face is famous (Bartlett, Strater, & Fulton, 1991; Dywan & Jacoby, 1990) and are more prone to memory distortions produced by postevent information (Cohen & Faulkner, 1989). We have used such misleading automatic influences of memory to diagnose age-related deficits in recollection, a form of cognitive control (e.g., Jennings & Jacoby, 1997).

The opposition procedure used to investigate the effects of repetition is a variant of the procedure used in our "false-fame" experiments (Jacoby, Kelley, Brown, & Jasechko, 1989; Jacoby, Woloshyn, & Kelley, 1989). In those experiments, participants read a list of names that they were told were nonfamous followed by a fame-judgment test consisting of old nonfamous names mixed with new famous and new nonfamous names. The prior presentation of nonfamous names increased their familiarity and, thereby, made it more likely that they later would be mistakenly identified as being famous, a false-fame effect. However, if participants could recollect reading the name earlier in the experimental

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setting, then any influence of increased familiarity would be opposed, allowing them to be certain that the name was nonfamous and to avoid showing a false-fame effect. Because of a deficit in ability to recollect, elderly adults show a pronounced false-fame effect (Bartlett et al., 1991; Dywan & Jacoby, 1990; Jennings & Jacoby, 1993), as do amnesiacs (Cermak, Verfaellie, Butler, & Jacoby, 1993; Squire & McKee, 1992) and patients who have sustained a closed-head injury (Dywan, Segalowitz, Henderson, & Jacoby, 1993).

In Experiment 1, I used an opposition procedure to examine the possibility that repetition produces an automatic influence of memory that is separate from its effect on recollection. In a first phase of that experiment, young and elderly adults read a list of words, with each word being read one, two, or three times. Next, they listened to a list of words that they were told to remember for a later test. At testing time, participants were instructed to identify words that they had heard and were warned that the test list would include words that they had read. They were further told that the earlier-read words were to be excluded because none of those words were in the list of words that they heard. Repeatedly reading a word was expected to increase its familiarity, and participants might misattribute the word's familiarity to its having been heard. However, recollection of having read the word would oppose its familiarity, allowing the word to be excluded, just as in the fame experiments.

Because of a deficit in recollection, the performance of elderly participants was expected to reveal the strengthening effect of repetition. Repeatedly reading a word was expected to increase the probability of mistakenly accepting the word as one that was heard earlier, showing an ironic effect of repetition. The strengthening effect of repetition, unopposed by recollection, is the same as held responsible for AD patients' repeated asking of questions and illustrated in the "tipping" example. Younger participants, in contrast, were expected to be better able to use recollection to oppose familiarity. For them, repeatedly reading a word was expected to make it more likely that they would later recollect that the word had been read, producing a decrease in the probability of mistakenly accepting the word as one that had been heard.

The purpose of Experiment 1 was to examine the effects of speed of responding on ironic repetition effects in young and elderly participants. Experiments 2 and 3 were conducted to specify further the conditions that give rise to an ironic effect of repetition. As I discuss later, placing familiarity and recollection in opposition holds advantages over other procedures as a means of gaining evidence of separate forms or uses of memory. Furthermore, results that are reported in this article show that opposition procedures sometimes provide a more sensitive measure of age-related differences in memory than does a standard test of recognition memory. However, opposition procedures do not allow one to measure familiarity and recollection separately. To do that, one must make an assumption about the relation between the two forms or uses of memory.

At issue is the question of how exclusion of earlier-read words is accomplished or, more generally, what the nature is of the deficit (or deficits) responsible for ironic effects of repetition and other memory distortions to which elderly, compared with young, participants are more susceptible. In the last experiment reported in this series (Experiment 4), I used the process-dissociation procedure (Jacoby, 1991, 1998) to show advantages of assuming that familiarity and recollection independently contribute to performance. The independence assumption was cast in the form of a multinomial model, and fits of that model were compared with those of a model that is similar to models that have been used to describe source monitoring (e.g., Buchner, Erdfelder, Steffens, & Martensen, 1997; Johnson, Kounios, & Reeder, 1994). The contrast between the two models will be used to formalize a distinction between early selection versus late correction (Jacoby, Kelley, & McElree, in press) as a means of avoiding misleading effects of the past, such as ironic effects of repetition.

Experiment 1

Familiarity has been described as being a faster basis for responding than is recollection (e.g., Atkinson & Juola, 1974; Dosher, 1984; Hintzman & Curran, 1994; McElree, Dolan, & Jacoby, in press; Yonelinas & Jacoby, 1994). Young participants in Experiment 1 were given either a moderate amount of time to make their decisions or were required to respond very rapidly. Because fast responding would not allow sufficient time for recollection, repeatedly reading a word was expected to increase the probability of mistakenly accepting the word as one that was heard. A result of this sort would raise the possibility that any ironic effect of repetition found for elderly adults was because the amount of time allowed for responding was insufficient to allow them to engage in recollection. Because of general slowing (e.g., Salthouse, 1996), elderly participants might require more time for recollection than would younger participants. To investigate this possibility, one group of elderly participants in Experiment 1 was given the same moderate amount of time to respond as were young participants, whereas a second group of elderly participants was given additional time for responding.

Method

Participants. Forty young adults and 40 older adults participated in the experiment. The young adults were New York University undergraduates enrolled in an introductory psychology course who received credit for participating in the experiment. Twenty young participants were assigned randomly to each of two between-participant test conditions, long deadline and short deadline. The older adults were community dwelling volunteers. Their mean age was 75.0 years (range = 60–94 years), and their mean number of years of education was 15.94. Twenty older adults were assigned randomly to each of two between-participant test conditions, long deadline (age: M = 75.7 years, SD = 7.7 years; education: M = 15.7 years, SD = 2.5 years) and extra-long deadline (age: M = 74.3 years, SD = 8.9 years; education: M = 16.1 years, SD = 2.9 years). All participants spoke English fluently and were tested individually.

Materials and design. Words used in the experiment were taken from a pool of 198 words that were three to eight letters in length ($M = 5.5$ letters). Eighty words were divided into four sets of 20 words each, which were rotated through the four repetition conditions: once presented, twice presented, three times presented, and new. Each set had an equal distribution of word length and word frequency as indexed by Kučera and Francis (1967). Sixty words were selected for auditory presentation. Twenty of these words were treated as critical items for purposes of analyses. A further 40 words were selected as new fillers for the test phase so as to produce an equal proportion of read, heard, and new items. The auditory words and new fillers remained constant across all formats.

To avoid primacy and recency effects, four items were presented at the beginning and end of both study lists. The multiple presentations in the visual study list resulted in a study list of 128 words with 20 presented once (20), 20 presented twice (40), 20 presented three times (60), and 6 buffers of which 4 were presented once (4) and 2 were presented twice (4). This list was constructed such that there were at least 16 items intervening between repetitions of any particular item. The auditory study list consisted of 68 words.

The test list contained 180 words corresponding to the 60 visually presented study words (20 from each of the repetition conditions), 60 new words (20 critical, 40 fillers), and 60 heard words. A short practice list of 12 items was presented prior to the beginning of the test list (4 read words—2 once presented and 2 twice presented—4 heard and 4 new items). In all phases of the experiment, order of presentation was random with the restriction that not more than three items representing the same combination of conditions could be presented in a row, and all conditions were represented evenly throughout the list.

Procedure. Materials were presented by means of IBM-compatible computers interfaced with VGA (video graphics array)-color monitors using Micro Experimental Laboratory (MEL) software (Schneider, 1990). A Sony CFM-10 radio cassette recorder was used to present words aurally. Each visually presented word appeared in white, lowercase letters (approximately 3 × 5 mm in size) on a black background in the center of the screen.

The visual study list was always presented first. The words were presented at a rate of one word/2 s which included an intertrial interval of 0.5 s, during which time the screen was blank. Participants were instructed to read the words aloud and to remember them for a later memory test. For the second phase, participants listened to a list of words played from the tape recorder at a rate of one word/2 s. Participants repeated each word out loud and tried to remember the words for a later memory test.

For the final phase of the experiment, words were presented one at a time in the center of the screen. Participants were instructed to indicate whether or not a word was heard earlier in the experiment by pressing keys labeled *Y* for "yes, the word was heard earlier" or *N* for "no, the word was not heard earlier." Participants were warned that the test list would include words that they had read earlier and were informed correctly that none of the words that they read were among those that they heard. Furthermore, participants were told that this meant that if they remembered a word as having been read in the first part of the experiment, then they could be certain that the word was not heard and should be excluded, giving a "no" response. In the case that they were unsure of whether a word was read or heard, participants were told to respond "yes" because it was important to identify all of the heard words. The four participant groups were assigned to one of three response conditions. For the young short-deadline group, participants were given 750 ms to respond after the test word appeared on the screen. For the young long-deadline and elderly long-deadline groups, participants had to wait 1,250 ms after the test word appeared before responding. At the end of that period, a row of asterisks appeared below the test word and participants were given 750 ms to respond prior to a beep that informed them that the interval in which a response was to be made had lapsed. Finally, in the elderly extra-long-deadline group, participants saw the test word and waited 1,250 ms for the asterisks to appear. These older adults were then given up to 2,750 ms to respond but were told to respond as quickly and as accurately as possible. For each test trial, after a participant responded or the computer "timed out" (the response interval had lapsed without a response being made), the program continued to the next test word with an intertrial interval of 1 s. A practice test preceded the actual test.

Results

The significance value for all tests was set at $p < .05$. Significant main effects of variables will not be reported if higher-order interactions involving those variables were significant.

A 3 (group: young, short deadline; young, long deadline; and elderly, long deadline) × 5 (item type: read words presented one, two, or three times, new words, and heard words) mixed analysis of variance (ANOVA) was conducted to examine differences in the probability of a time out (failure to respond within the response interval). Data from the elderly, extra-long-deadline group were excluded from this analysis because they made no time-outs. The time-out analysis revealed only a significant main effect of group, $F(2, 57) = 12.15$, $MSE = .12$. The probability of a time out was higher in the elderly, long-deadline group (.33) than in the young, short-deadline group (.16), which was higher than that in the young, long-deadline group (.09). For the remainder of the analyses that are to be reported, results were conditionalized on the probability of a response being made before the deadline, separately for each participant.

The mean probability of correctly responding "yes" to words that were heard earlier (hits) along with the probability of incorrectly responding "yes" to new words (false alarms) are presented in Table 1. An analysis of those data revealed that hits were more likely than false alarms, $F(1, 76) = 247.47$, $MSE = .02$. A separate ANOVA on false-alarm rates indicated a lack of an effect of group ($F < 1$), whereas an analysis of the hit rate for heard items showed a significant effect of group, $F(3, 76) = 5.21$, $MSE = .02$. Post-hoc analysis with a Tukey test revealed that the only significant difference was between hit rates for the two young groups.

Table 1
Probabilities of Responding "Yes" for Experiment 1

	Item type				
	Read				
Group	1x	2x	3x	New	Heard
Young short	.31	.40	.45	.18	.43
Young long	.35	.31	.21	.22	.63
Elderly long	.43	.53	.59	.19	.52
Elderly extra-long	.35	.42	.44	.14	.52

Of greater interest were differences among conditions in the probability of mistakenly accepting words that were read earlier as having been heard earlier (see Table 1). Young, long-deadline participants produced a pattern of results that differed from that produced by the other three groups. A 2 × 3 ANOVA of performance of the two young groups revealed a significant interaction of group and item type, $F(2, 76) = 17.81$, $MSE = .01$. Short-deadline participants made more false alarms as the number of study presentations increased, whereas long-deadline participants made fewer false alarms as the number of study presentations increased. A 2 × 3 ANOVA of performance of the two elderly groups revealed a significant main effect of repetition, $F(2, 76) = 6.50$, $MSE = .03$—false alarms increased with repetition for both elderly groups. Elderly participants in the extra-long-deadline condition made fewer false alarms than did those in the long-deadline condition, but the difference was only marginally significant, $F(1, 38) = 2.93$, $MSE = .13$, $p < .10$.

Discussion

The results of the elderly participants showed an ironic effect of repetition as did those of young adults who were required to respond rapidly. Repeatedly reading a word made it more likely that later the word would be accepted mistakenly as heard earlier. However, younger participants in the long-deadline condition showed an opposite effect of repetition. For them, repeatedly reading a word made it less likely that later the word would be accepted mistakenly as heard earlier. In contrast to results from younger participants, increasing the amount of time elderly participants were given to respond did not allow them to avoid showing an ironic effect of repetition. Although the effect was slightly less pronounced, elderly participants in the extra-long-deadline condition showed an ironic effect of repetition just as did those in the long-deadline condition. The greater susceptibility of older adults to ironic effects of repetition cannot be fully removed by appealing to general slowing of processing and providing additional time for responding so as to compensate for that slowing.

Others also have found differential effects of repetition for older as compared with younger adults. Bartlett et al. (1991, Experiment 2) used a false-fame paradigm and found that repeated viewing of a nonfamous face increased the probability of the face's mistakenly being accepted as famous by elderly adults but had an opposite effect for younger adults. Similarly, Schacter, Koutstaal, Johnson, Gross, and Angell's (1997) found that repeated viewing of a photograph depicting an event increased the probability of older adults falsely remembering the event as being from a videotaped sequence of events. Younger adults were less susceptible to such false remembering, and their performance did not show a significant effect of repetition.

Finding age-related differences in repetition errors is closely related to evidence of age deficits in source monitoring (Spencer & Raz, 1995). Source-monitoring paradigms assess memory for source by examining overt source attributions (Johnson, Hashtroudi, & Lindsay, 1993). Participants choose one of n alternatives denoting whether and in what context the test item was studied. For example, participants might hear some words and read other words during study and then later judge for each test word whether it was read, heard, or not studied earlier. Those judgments are used to compute both a measure of recognition memory and a measure of source memory, often with the intention of showing an impairment in certain types of source discrimination (e.g., Foley & Johnson, 1985; Harvey, 1985) or the dissociation of source attributions and old–new discriminations (e.g., Hashtroudi, Johnson, & Chrosniak, 1989; Mitchell, Hunt, & Schmitt, 1988). An ironic effect of repetition, as defined here, reflects a failure to recollect that a word was read, rather than heard, earlier. Results from standard tests of source memory have shown that older, as compared with younger, adults are less able to remember whether information was presented visually or aurally (e.g., Light, La Voie, Valencia-Laver, Albertson-Owens, & Mead, 1992; McIntyre & Craik, 1987).

Because of parallels between source monitoring and exclusion results, it might be argued that opposition procedures offer no information beyond what could be gained by using a standard test of source memory (Buchner et al., 1997). However, it is important that standard tests of source memory explicitly instruct participants to report the source of information, whereas the opposition procedure examines the monitoring of source as part of some ongoing task. Monitoring of source while engaging in other activities is critical for guiding everyday behavior and relies on frontal-lobe functions of a sort that likely decline in old age (Dywan & Jacoby, 1990; Jennings & Jacoby, 1997). Directly questioning source for each test item, as is done with standard tests of source memory, might reduce the magnitude of age-related differences by encouraging recollection in a way that is not done when monitoring of source serves as part of some ongoing task. Jacoby, Kelley, et al. (1989) found that earlier-read nonfamous names were more likely to be judged mistakenly as being famous than were new nonfamous names, although people were told that all the names read earlier in the experiment were nonfamous. In contrast, when participants were required to make a recognition-memory decision (judging whether the name was read earlier, as well as judging fame), old nonfamous names were less likely to be accepted mistakenly as famous than were new nonfamous names. Multhaup (1995) found a similar result with an elderly population. When only making fame judgments, elderly participants are more likely than are younger participants to accept mistakenly earlier-read nonfamous names as famous (Dywan & Jacoby, 1990; Jennings & Jacoby, 1993). Multhaup found that this difference could be reduced by giving a three-alternative source discrimination task (i.e., choose one of the following—new famous name, new nonfamous name, old nonfamous name—from the study list). A similar result was found by Lindsay and Johnson (1989) in an eyewitness suggestibility paradigm.

Findings of opposite effects of repetition for younger and older adults and as a function of response deadline provide compelling evidence for the necessity of postulating a dual-process theory to account for differences in exclusion performance. Those results show that repetition has two effects, serving both to increase the familiarity of a repeated item and to enhance recollection of the details of its presentation. For younger adults given sufficient time to respond, the increase in recollection gained from repetition successfully opposes the increase in familiarity to produce a reduction in the probability of mistakenly accepting an earlier-read word as heard earlier. However, recollection is a slower process and more vulnerable to age-related differences in memory than is familiarity.

In contrast to dissociations found using the opposition procedure, repetition would not be expected to produce dissociations between recognition and source-memory performance but, rather, would likely produce parallel effects on the two types of measures for both young and elderly participants. Interpreting effects on recognition and source memory requires that one make some assumption about the relationship between recollection (source memory) and familiarity (detection of old items) along with assumptions that serve to disentangle measures of memory from response biases (Batchelder & Riefer, 1990; Riefer & Batchelder, 1988). I consider the importance of such assumptions when reporting the results of Experiment 4. For present purposes, the important point is that reliance on opposition procedures makes it unnecessary to adopt such assumptions to demonstrate that repetition has separate effects on two forms or uses of memory. Jacoby, Jones, and Dolan (in press) provided additional evidence that repetition has two effects and discussed advantages of the opposition procedure over the Remember/Know procedure (Richardson-Klavehn, Gardiner, & Java, 1996) as a means of gaining support for a dual-process model. McElree et al. (in press) used a speed–accuracy tradeoff (SAT) methodology to examine time-course differences of the two effects of repetition and discussed the implications of such differences for models of recognition memory and source monitoring.

By placing automatic and consciously controlled processes in opposition, errors in performance allow the examination of age-related declines in recollection that cannot be compensated for by relying on familiarity. In the opposition paradigm, familiarity leads to errors, whereas familiarity is a basis for correct responding to old items on standard tests of recognition memory, just as is recollection (e.g., Jacoby & Dallas, 1981; Mandler, 1980). Consequently, for recognition-memory performance, correct responding due to familiarity may mask a deficit in recollection (e.g., Jacoby, Toth, & Yonelinas, 1993), explaining why tests of recognition memory sometimes show nonsignificant age differences in performance (e.g., Craik & McDowd, 1987; Dywan & Jacoby, 1990; Rabinowitz, 1984).

Results of the present experiment converge with those reported by Jennings and Jacoby (1997) in showing that errors in exclusion performance can be a more sensitive measure of age-related differences in memory than is a test of recognition-memory performance. In contrast to the large age-related differences revealed by exclusion performance, older and younger adults did not differ in their ability to recognize words that were heard earlier. This pattern of results is most striking when one compares the near-identical recognition performance of young participants in the long-deadline condition and older participants in the extra-long-deadline condition with the large difference in their exclusion performance (Table 1). Of course, a problem for that comparison is that older adults may have been advantaged by being given more time to respond than were younger adults. Experiment 2 compared recognition-memory performance on heard words when both older and younger adults were told to respond as soon as possible after occurrence of the response signal, the extra-long-deadline condition of Experiment 1.

Experiment 2

Experiment 2 was similar to Experiment 1, but words were presented to be read one, three, or five times in the first phase of Experiment 2. At the highest level of repetition, recollection was expected to oppose familiarity successfully even for older adults; that is, the intuition was that it would be possible to reverse the ironic effect of repetition by simply having older adults read words a larger number of times. After reading a word five times, older adults should find it easy to recollect having read the word earlier and so be able to exclude it when asked to accept only earlier-heard words. Also, the presence in the test list of words for which recollection is easy might generally encourage more sustained attempts at recollection. The ironic effect of repetition shown by comparing performance on once- and thrice-read words in Experiment 1 might, in part, reflect older adults' not attempting recollection, although they would have been able to recollect had they tried harder to do so. If so, testing items for which recollection is easy might encourage participants to attempt recollection for all test items and diminish the difference between performance on words read once versus three times. Experiment 2 included only the extra-long-deadline test condition from Experiment 1.

Method

Participants. Twenty young adults and 20 older adults participated in the experiment and were drawn from the same participant pool as in Experiment 1. The older adults ranged in age from 61 to 80 years (M = 71.1 years, SD = 6.5 years) and had an average of 17.6 years (SD = 2.5 years) of education.

Design and procedure. The design and procedure of this experiment were the same as in Experiment 1, except that the number of study repetitions was changed to one, three, and five. This change resulted in a Phase 1 study list of 188 words, 20 once presented (20), 20 three-times presented (60), 20 five-times presented (100), and 8 buffer items. Also, only the extra-long-deadline test condition from Experiment 1 was used for both groups of participants.

Results and Discussion

The mean hit and false-alarm rates are reported in Table 2 for all item types. Words that were earlier heard were more likely to be given a "yes" response than were new words, $F(1, 38) = 239.71$, $MSE = .02$. Neither the main effect of group nor the interaction approached significance ($Fs < 1$). That is, young and elderly participants did not differ in their recognition memory for words that were heard earlier.

Table 2
Probabilities of Responding "Yes" for Experiment 2

	Item type				
	Read				
Group	1x	3x	5x	New	Heard
Young extra-long	.29	.22	.13	.12	.62
Elderly extra-long	.33	.34	.34	.13	.65

An analysis of the probability of responding "yes" to earlier-read items (false alarms) revealed a significant interaction of group and repetition, $F(2, 76) = 4.63$, $MSE = .01$. Elderly participants' ability to exclude earlier-read words was not influenced by repeatedly reading the words. In contrast, young participants made fewer false alarms with increasing repetition just as they did in Experiment 1.

Results of Experiment 2 failed to replicate the ironic effect of repetition shown by comparing performance of older adults on once- and thrice-read words in Experiment 1. However, closer inspection of results for elderly participants showed striking variability in performance. I examined the results separately for relatively young older adults (mean age = 68, $n = 14$) and the most elderly participants (mean age = 79, $n = 6$). Results for the relatively young older participants showed that the probability of mistakenly accepting an earlier-read word decreased with number of repetitions (.28, .26, and .20 for one, three, and five repetitions, respectively). Despite the lack of an ironic effect of repetition, these relatively young older adults were more likely to accept mistakenly words that were repeatedly read earlier than were younger participants (e.g., .20 vs. .13 for words read five times). Participants who were most elderly did show an ironic effect of repetition (.43, .51, and .65 for one, three, and five repetitions, respectively) and were generally more likely to accept mistakenly an earlier-read word as heard earlier.

The number of participants in the older subgroup of elderly is too small for one to draw firm conclusions. Further, age by itself is not expected to fully predict whether an ironic effect of repetition will be observed. I reanalyzed results from Experiment 1 and did not find consistent differences between subgroups of elderly participants in that experiment—both groups of elderly participants showed an ironic effect of repetition in the extra-long-deadline condition. The presence of test items that could be recollected easily as read earlier (words read five times during study) may have encouraged participants to engage in recollection, making an ironic effect of repetition less likely to be found in Experiment 2 as compared with Experiment 1. Similarly, Jacoby, Kelley, et al. (1989) found that participants were less likely to show a false-fame effect when the necessity of engaging in recollection was made obvious by testing many, rather than few, nonfamous names that had been read earlier.

Results of Experiment 2 replicated those of Experiment 1 by showing that exclusion performance can provide a more sensitive test of deficits in recollection than does a test of recognition memory. Elderly and young participants did not differ in their ability to recognize words that were heard earlier. However, elderly participants were much more likely than were younger participants to accept mistakenly words that they had read repeatedly. Exclusion errors allow one to examine age-related declines in recollection that cannot be compensated for by relying on familiarity.

Experiment 3

Experiment 3 was conducted to replicate results from younger participants in the short- and long-deadline conditions in Experiment 1. As in Experiment 1, I expected young adults to show an ironic effect of repetition when they were required to respond rapidly. Results of that sort show that recollection is a slower process than is judging familiarity. A third condition was added to examine effects of dividing attention while reading words. Participants in the divided-attention condition engaged in a listening task while simultaneously reading words aloud in the first phase of the experiment. Subsequently, they were tested in the same long-deadline condition as was one of the two groups of participants who devoted full attention to reading words aloud. Dividing attention while reading words was expected to reduce recollection (e.g., Jacoby, Woloshyn, & Kelley, 1989) and, thereby, produce an ironic effect of repetition.

Method

Participants. Seventy-two New York University undergraduates participated in return for course credit, with 24 students randomly assigned to each of three between-participant conditions: full attention, long deadline; divided attention, long deadline; and full attention, short deadline.

Design and procedure. The listening task used in the divided-attention condition was one previously used by Craik (1982) and required participants to monitor a tape-recorded list of digits (1 to 9) to detect target sequences of three odd numbers in a row (e.g., 9, 3, 7). The digits were random with the exceptions that a minimum of one number and a maximum of five numbers occurred between the end of one target sequence and the beginning of the next target sequence and no more than two even numbers occurred in sequence. Digits were recorded at a rate of one digit/1.5 s.

As in Experiment 1, words were read one, two, or three times in Phase 1. Participants in the divided-attention group were told that the experiment would test their ability to perform two tasks at the same time, a listening task and a reading task. A list of single-digit numbers was played from the tape recorder, and participants were instructed to indicate when three odd digits were heard consecutively by tapping a piece of chalk on the table. The experimenter stressed the importance of not missing any of the odd-number sequences. Participants heard 20 digits before the experimenter started the presentation of the words on the computer screen. The experimenter recorded participants' responses to the number-listening task and said "miss" whenever a participant missed identifying a sequence so as to encourage attention to the listening task. The procedure for the full-attention condition was the same as in Experiment 1. Participants in both the full- and divided-attention conditions were warned that their memory would be tested for the words that they would read in Phase 1. Phases 2 and 3 for the long- and short-deadline conditions were identical to the corresponding conditions in Experiment 1.

Results and Discussion

Participants in the divided-attention condition missed an average of 18.8% of the target sequences (range = 0% to 44%) in the listening task.

An analysis of time outs revealed only a main effect of group, $F(2, 69) = 9.95$, $MSE = .03$. Participants in the short-deadline condition were more likely to allow the deadline to lapse without responding (.11) than were participants in the full- or divided-attention, long-deadline conditions (.04 and .02). For further analyses, results were conditionalized on responses made within the deadline (see Table 3).

An analysis of correct recognition of words heard earlier and false recognition of new words revealed a highly significant interaction of group and item type, $F(2, 69) = 31.58$, $MSE = .02$. Both long-deadline groups demonstrated much better discrimination of heard items from new items than did the short-deadline group. More interesting, an analysis of false alarms to earlier-read words showed that the interaction between repetition and group was significant, $F(4, 138) = 7.22$, $MSE = .01$. As in Experiment 1, the full-attention, long-deadline group made fewer false alarms to earlier-read words as repetition increased. In contrast, dividing attention during study or reducing the amount of time to respond during the test produced increased false alarms with increases in repetition; that is, dividing attention during study produced an ironic effect of repetition that is similar to that found for elderly participants.

Experiments 1–3 revealed ironic effects of repetition for elderly participants and for young participants who were required to respond rapidly or who had read words under conditions of divided attention. In each of those cases, repeatedly reading a word increased the probability of the word's later being accepted mistakenly as one that was heard earlier. In contrast, young participants who gave full attention to reading words and who were given adequate time to respond showed an opposite effect of repetition. For them, repeatedly reading a word decreased the probability of mistakenly accepting the word as heard earlier.

Experiment 4

Did factors responsible for producing ironic effects of repetition only reduce the probability of recollection, leaving familiarity unchanged? This question is an important one for understanding age-related differences in memory. Successful exclusion of earlier-read words can reflect either successful opposition of familiarity by recollection or a lack of familiarity of the earlier-read words. Perhaps the elderly participants' greater false recognition of earlier-read words, observed in Experiments 1 and 2, underestimates age-related decline in recollection because familiarity also declines in old age. For purposes of diagnosing memory deficits, what is needed is a means of separately measuring recollection and automatic influences of memory.

Toward that goal, in Experiment 4 I used the process-dissociation procedure (Jacoby, 1991, 1998) to examine age-related differences in effects of repetition along with effects of varying response deadline. An exclusion test was the same as used in Experiments 1–3. For that test, participants were instructed to exclude earlier-read words and respond "yes" only to words that they had heard earlier. In contrast, for an inclusion test, participants were instructed to respond "yes" to all earlier-studied words, accepting both earlier-read and earlier-heard words. The inclusion test is the same as a standard test of recognition memory except that instructions stressed the importance of using recollection as a basis for responding. The process-dissociation procedure combines results from inclusion and exclusion tests to estimate the contributions of recollection and familiarity.

If it is assumed that the recollection (R) and the familiarity (F) that serve as a basis for recognition memory are independent, then correctly responding "yes" to an earlier-read word in the inclusion test is due to either conscious recollection or familiarity, or both: The probability of responding "yes" given an inclusion test is equal to $R + F$, minus the overlap of the two processes, RF. More formally, $P(\text{yes}|\text{inclusion}) = R + (1 - R)F$. In contrast, on the exclusion test, participants should respond "yes" to an earlier-read word, mistakenly accepting it as earlier heard, only if the study presentation of the word is not recollected but, nonetheless, the word has gained sufficient familiarity from prior study to pass the criterion set for acceptance as earlier-heard: $P(\text{yes}|\text{exclusion}) = (1 - R)F$. To obtain estimates of recollection, one subtracts the probability of responding "yes" to earlier-read words in the exclusion condition from the probability of responding "yes" to earlier-read words in the inclusion condition. Once an estimate of R is obtained, the equations can be used to solve for an estimate of F.

If it is true that two processes make independent contributions to a particular task, then one should be able to find manipulations that affect one estimate derived from the process-dissociation procedure without affecting the other estimate, or that affect the other estimate in an opposite direction. Jacoby, Yonelinas, and Jennings (1997) summarize findings of such process dissociations. Requiring participants to respond rapidly, rather than more slowly, has been found to reduce estimated recollection but leave estimated familiarity unchanged (Yonelinas & Jacoby, 1994). Similarly, elderly, as compared with younger, participants show a deficit in recollection but unchanged automatic influences of memory (Jennings & Jacoby, 1993, 1997).

Experiment 4 included three groups of participants: young and elderly participants in the long-deadline test condition used in Experiments 1 and 3, and elderly partici-

Table 3
Probabilities of Responding "Yes" for Experiment 3

	Item type				
	Read				
Group	1x	2x	3x	New	Heard
Full short	.41	.48	.49	.27	.41
Full long	.35	.31	.24	.19	.69
Divided long	.21	.26	.27	.10	.65

pants instructed to respond as soon as possible after a response signal (extra-long deadline). Words were presented either once or three times to be read in the first phase of the experiment. The exclusion test was always given prior to the inclusion test so as to facilitate comparison of exclusion results with those from earlier experiments in which only an exclusion test was given. Combining exclusion and inclusion performance to gain estimates by means of the process-dissociation equations was expected to show that both aging and faster responding had their effects by reducing recollection, leaving familiarity unchanged. Repetition was expected to increase both recollection and familiarity.

The assumptions underlying the process-dissociation approach have been controversial, as have been the details of the particular procedures used to implement the approach (e.g., Buchner, Erdfelder, & Vaterrodt-Plünnecke, 1995; Curran & Hintzman, 1995, 1997; Graf & Komatsu, 1994; Mulligan & Hirshman, 1997; Ratcliff, Van Zandt, & McKoon, 1995). Most controversial has been the assumption that recollection and familiarity independently contribute to performance (see Curran & Hintzman, 1997; and Hintzman & Curran, 1997; along with responses by Jacoby, Begg, & Toth, 1997; and Jacoby & Shrout, 1997). Rather than independence, a redundancy relation might hold between recollection and familiarity (e.g., see Joordens & Merikle, 1993, along with the reply by Jacoby, Toth, Yonelinas, & Debner, 1994). By a redundancy assumption, the probability of correctly responding "yes" to earlier-presented words (hits) serves as a measure of familiarity, and words whose source (read vs. heard) can be recollected are held to be a subset of those that are familiar. When reporting the results of Experiment 4, I cast the two assumptions in different multinomial models and identify the independence model with an early-selection means of avoiding exclusion errors, whereas the redundancy model is identified with a late-correction means of avoiding such errors.

Method

Participants. Twenty-four young adults and 48 older adults participated in the experiment and were drawn from the same participant pools as in Experiment 1. The older adults ranged in age from 60 to 88 years (M = 71.5 years) and had a mean of 16.4 years of education. Twenty-four older adults were assigned randomly to each of two between-participant conditions, long deadline (age: M = 70.8 years, SD = 6.6 years; education: M = 16.9 years, SD = 2.1 years) and extra-long deadline (age: M = 72.2 years, SD = 6.2 years; education: M = 16.1 years, SD = 2.4 years) test conditions.

Materials and design. Although the materials and design of this experiment were basically the same as in Experiment 1, there were some changes required to accommodate the inclusion–exclusion test phases. The same stimuli were used but they were separated into six critical sets of 15 words each, which were rotated through each of the 6 experimental conditions: 2 test conditions (inclusion, exclusion) × 3 repetition conditions (0, 1, 3). Each set had an equal distribution of word length (M = 5.52 letters) and word frequency (M = 80.03 occurrences per million words) as indexed by Kučera and Francis (1967). The same 60 words as used in earlier experiments were presented in the auditory phase, but 30 of these words (two sets of 15, one set for each test) were treated as critical items for purposes of analyses, whereas 30 words remained as fillers, of which 15 were used for each test. Thirty words were selected as new fillers for the test phases (15 per test) so as to make equal proportions of visual, auditory, and new items. The auditory words and new fillers remained constant across all formats. The recency and primacy buffers in each study phase plus 8 new items were used to create two practice tests of 12 items each (one for each test).

These changes resulted in a visual study list of 128 words with 30 presented once (30), 30 presented three times (90), and 8 buffers. The auditory study list, as in the previous experiments, consisted of 68 words. Each test phase consisted of 90 words, 30 read words (15 once-presented, 15 three-times presented), 30 heard words (15 critical words, 15 fillers), and 30 new words (15 critical words, 15 fillers). The exclusion test was always presented first.

Procedure. The instructions for both the visual and auditory study phases were the same as in Experiment 1. Both the long-deadline and extra-long-deadline test procedures were the same as used in Experiment 1, as were the instructions for the exclusion test. For the inclusion test, participants were told to respond "yes" to the words they heard earlier from the tape recorder and the words they read from the screen, and to respond "no" to the new words.

Results and Discussion

Participants in the young, long-deadline group made few time-outs but were more likely to time out on the exclusion test than on the inclusion test (.04 vs. .01), $F(1, 23) = 5.86$, $MSE = .004$. Time-out data for the elderly, long-deadline participants revealed that they also timed out more on the exclusion test than on the inclusion test (.18 vs. .10), $F(1, 23) = 8.49$, $MSE = .04$. Further, the Instruction × Item Type interaction was significant for the elderly group, $F(3, 69) = 3.29$, $MSE = .006$. Visual inspection of that interaction showed that elderly participants were more likely to time out on words that were earlier read three times, rather than only once, when given an inclusion test (.14 vs. .07), but the opposite was true when they were given an exclusion test (.17 vs. .19). The probability of a time out for words that were heard earlier did not differ from that for new words on either the inclusion (.09 vs. .09) or the exclusion test (.18 vs. .16). For further analyses, data were conditionalized on responding prior to the deadline (see Table 4).

Table 4
Probabilities of Responding "Yes" for Experiment 4

	Item type			
	Read			
Group	1x	3x	New	Heard
	Inclusion			
Young long	.67	.87	.15	.60
Elderly extra-long	.55	.80	.18	.64
Elderly long	.55	.77	.17	.53
	Exclusion			
Young long	.29	.20	.18	.58
Elderly extra-long	.36	.31	.13	.60
Elderly long	.34	.44	.16	.55

Table 5
Estimates of Recollection and Familiarity for Read Words in Experiment 4

	No. of presentations			
	1x		3x	
Group	*R*	*F*	*R*	*F*
Young long	.38 (.37)	.45 (.43)	.67 (.59)	.57 (.56)
Elderly extra-long	.20 (.17)	.44 (.43)	.49 (.42)	.58 (.60)
Elderly long	.21 (.14)	.38 (.42)	.33 (.29)	.62 (.62)

Note. Numbers in parentheses are the estimates based on data when participants with *F* values of 1, 0, or undefined are removed.

Recognition-memory performance. Heard items were given a "yes" response much more often than were new words, $F(1, 69) = 342.80$, $MSE = .04$. Neither the probability of correct recognition of words that were heard earlier nor the probability of false recognition of new words differed significantly across either group or type of test. Indeed, the only effect that produced an $F > 1$ was the effect of group in the analysis of correct recognition of earlier-heard words, $F(2, 69) = 1.76$, $MSE = .04$.

The inclusion test corresponds to a standard test of recognition memory for words that were read earlier. An analysis of correct recognition of earlier-read words on the inclusion test revealed that the main effects of group, $F(2, 69) = 4.13$, $MSE = .04$, and repetition, $F(1, 69) = 122.16$, $MSE = .01$, were significant but the interaction was not, $F < 1$. For the inclusion test, young participants had a higher hit rate for earlier-read words than did both groups of elderly participants. The age-related difference in recognition memory of earlier-read words contrasts with the lack of difference in recognition memory of earlier-heard words. For all three groups of participants, correct recognition was much higher for words read three times compared with words read only once.

Exclusion performance. False alarms for the read words on the exclusion test were analyzed in a 2 (repetition: items presented one or three times at study) × 3 (group: young long deadline, elderly long deadline, elderly extra-long deadline) mixed ANOVA. Results revealed a main effect of group, $F(2, 69) = 3.54$, $MSE = .07$, and a Group × Repetition interaction, $F(2, 69) = 4.46$, $MSE = .03$. The main effect of repetition was not significant ($F < 1$). Elderly participants in the long-deadline condition showed an ironic effect of repetition, whereas for young participants given a long deadline and elderly participants given an extra-long deadline, the probability of mistakenly accepting an earlier-read word decreased with repetition. However, as in Experiment 2, results differed for a relatively younger subgroup as compared with a relatively older subgroup (age > 75, $n = 6$) of the elderly. The probability of accepting earlier-read words increased with repetition for participants in the most elderly subgroup (.39 vs. .42) but decreased with repetition for participants in the younger subgroup (.34 vs. .28) of the elderly.

Process-dissociation estimates. Estimates of recollection and familiarity (see Table 5) were calculated for each participant using the exclusion false alarms and inclusion hits for earlier-read words. The equations used to compute those estimates, described in the introduction to Experiment 4, are based on the assumption that the criterion that participants use for responding is the same for the inclusion and exclusion tests as well as across the different groups of participants. Further, because the exclusion test always was given prior to the inclusion test, it is also necessary to assume that there was not substantial forgetting across the retention interval created by giving the exclusion test first. These assumptions gain strong support from the earlier-reported findings. The lack of an effect of type of test (inclusion vs. exclusion) on recognition of heard words suggests that there was not substantial forgetting across the exclusion test. The assumption of equivalence of criteria is supported both by the lack of an effect of group on recognition of heard words and by the findings that neither group nor type of test influenced false alarms to new words.

An analysis of estimates of recollection revealed that recollection was higher for the young group than for the two groups of elderly participants, $F(2, 69) = 8.23$, $MSE = .10$. Also, words that were read three times were more likely to be recollected than were words that were read only once, $F(1, 69) = 41.26$, $MSE = .05$.[1] The interaction of group and repetition approached significance, $F(2, 69) = 2.51$, $MSE = .05$, $p = .09$. Recollection in the elderly, extra-long-deadline group was higher than that in the elderly, long-deadline group only for words that were read three times.

An analysis of estimates of familiarity revealed only a main effect of repetition (higher *F* estimates for words read three times as compared with words read once), $F(1, 69) =$

[1] Two of the participants (1 in the young, long deadline group and 1 in the elderly, extra-long deadline group) in this analysis achieved perfect performance on inclusion and exclusion tests, and so an estimate of familiarity could not be calculated for these participants. Therefore, for purposes of analysis, the mean value of familiarity from those conditions was inserted for those participants. Several other participants also achieved perfect performance in either the inclusion or exclusion conditions (but not both), which contrains the estimates. Thus, analyses involving estimates of *R* and *F* were recalculated after eliminating data from any participant who achieved a perfect score in inclusion or exclusion. The mean estimates of *R* and *F* based on the reduced sample are shown in brackets in Table 5. Results of the new analyses (based on $n = 18$ for the young, long deadline, $n = 19$ for the elderly, long deadline and the elderly, extra-long deadline) produced the same results as the original.

33.27, $MSE = .03$. Neither the main effect of group ($F < 1$) nor the interaction were significant, $F(2, 69) = 1.70$, $MSE = .03$, $p = .19$; that is, across groups, estimates of F remained invariant while estimates of R were considerably lower for the two elderly groups relative to the young group. The failure to detect an effect of group on estimates of F was not due to a lack of statistical power, which was calculated to be approximately .75 (Cohen's $f = 0.35$, $\alpha = .05$, $n' = 24$, $u = 2$). A 2 × 3 ANOVA revealed that estimates of F for words read once were higher than the false-alarm rate for new items across all three groups, $F(1, 69) = 110.37$, $MSE = .02$.

Findings of perfect performance on either the inclusion or exclusion test create problems for estimating familiarity. Perfect performance on the inclusion test mathematically constrains familiarity to be equal to 1.0, whereas perfect performance on the exclusion test mathematically constrains familiarity to be 0. Eliminating data from participants who produced a perfect score on either the inclusion or the exclusion test and reanalyzing results produced findings that were the same as those from the analysis that included data from all participants (see Table 5). The small, nonsignificant differences that were found were largely because of performance in the elderly, long-deadline group. The large number of time outs in that group gives reason to treat their results with caution. An analysis that included only the young, long-deadline and elderly, extra-long-deadline groups produced a pattern of results that was the same as was found when all three groups were included in the analysis.

Results from the aforementioned analyses lead to the conclusion that repetition increased both recollection and familiarity. In contrast, requiring fast, as compared with slow, responding and age-related differences in memory had their effects by influencing only recollection, leaving familiarity unchanged. As described in the next section, these conclusions depend critically on the assumption that recollection and familiarity independently contribute to performance.

Early Selection Versus Late Correction: A Choice Between Assumptions

In this section, a multinomial model based on the independence assumption is compared with a multinomial model that is based on an assumption of a redundancy relation between familiarity and source memory (recollection). The models differ in the way they describe exclusion performance as being accomplished. For all analyses of the multinomial models, $N = 6129$ and $\alpha = .005$.

Source-monitoring multinomial model: Late correction. Buchner et al. (1997) suggested that judgments in the process-dissociation procedure are the same as those involved in standard source-monitoring tasks. Consequently, I attempted to fit the data with a multinomial model akin to those used in the source-monitoring literature (Batchelder & Riefer, 1990; Buchner et al., 1997; Johnson et al., 1994; Mulligan & Hirshman, 1997). The portion of this model that describes exclusion performance is shown in the upper panel of Figure 1, and the complete model is described in the Appendix. The model describes successful exclusion of earlier-read words as reliant on source discrimination (d) that is conditional on the detection of old items (D), recognition memory. To interpret the results of Experiments 1–3 using this model, it could be argued that, as compared with the familiarity that is used as a basis for detection of old items, information required to discriminate between sources (read vs. heard) becomes available later (Johnson et al., 1994), is more reliant on attention during study, and is more likely to reflect age-related differences in memory (Johnson et al., 1993). Use of source information is viewed as a late-selection mechanism that serves to reject items that would otherwise be accepted because of their high familiarity. The model is a redundancy model in that it is assumed that source information is recovered from a subset of items detected as "old."

The model is similar to Batchelder and Riefer's (1990) model 5b, which assumes that $a = g$.[2] In our model we have collapsed a and g into one parameter, g, which can differ for inclusion and exclusion tests. Fits of the model were conducted using the GPT (General Processing Tree) program made available through FTP (http://irvin.psyc.memphis.edu/gpt) by Hu (1997) and were analyzed using the chi-square statistic. Critical values for the chi-square tests were determined using the G-power program of Erdfelder, Faul, and Buchner (1996).

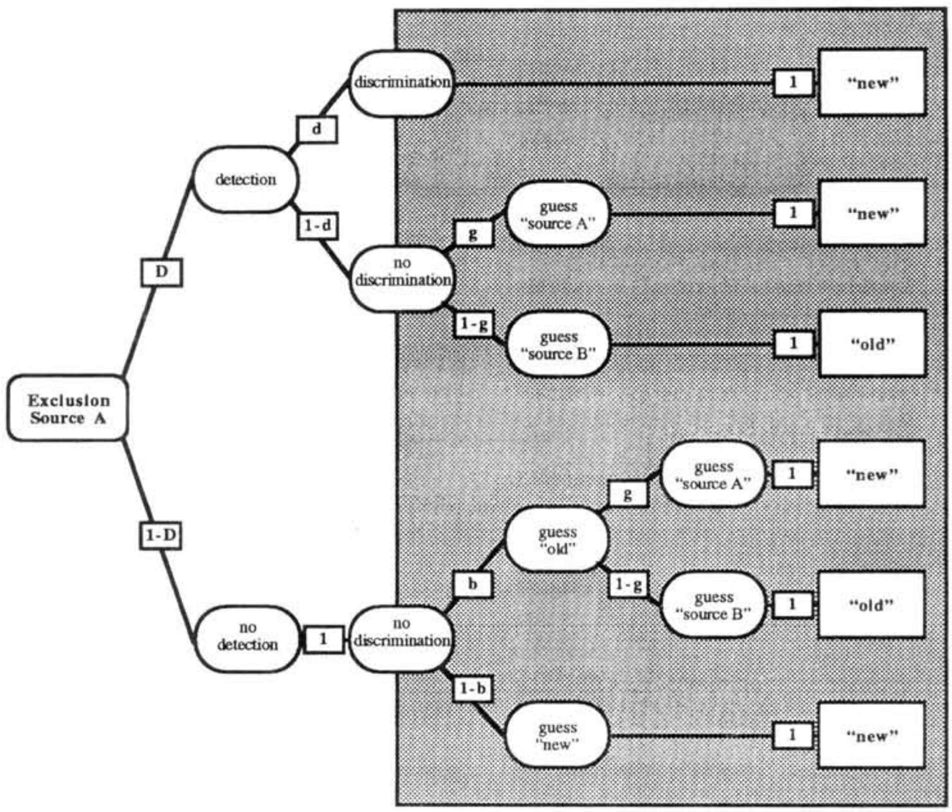
I started with the assumption that bias, b, was constant across tests (i.e., $b_{INC} = b_{EXC}$, where INC = inclusion and EXC = exclusion) and that the detection, D, and discrimination, d, parameters were constant across tests (i.e., $D_{INC} = D_{EXC}$ and $d_{INC} = d_{EXC}$).[3] Then, I tested the hypothesis that the guessing parameter, g, was equal across tests and groups (i.e., $g_{INCYNG} = g_{EXCYNG} = g_{INCELD} = g_{EXCELD} = g_{INCELDX} = g_{EXCELDX}$). With this restriction, the model fit the data very well, producing a $\chi^2(2, N = 6129) = 3.87$, which is well below the critical value of $\chi^2(2, N = 6129) = 10.60$.

Next, I tried to fit the data with a more restricted version of the model by testing the hypothesis that the bias parameters were the same across the three groups (i.e., $b_{YNG} = b_{ELD} = b_{ELDX}$). Once again, the model was found to fit the data quite well, as $\chi^2(4, N = 6129) = 4.12$, which is below the critical value of $\chi^2(4, N = 6129) = 14.86$. Parameter values for this model are shown in the Appendix.

With these restrictions in place, I tested the hypothesis that the detection parameters were the same for the three groups within each level of repetition (i.e., $D_{1_{YNG}} =$

[2] Batchelder and Riefer (1990) described a multinomial modeling approach for analyzing source-monitoring data. According to their model, the "a" parameter refers to the probability of guessing that a detected but nondiscriminated item belongs to a particular source (e.g., heard items) and the "g" parameter refers to the probability of guessing that a nondetected item belongs to the same source (e.g., heard items).

[3] The subscript INC was used to denote inclusion, and the subscript EXC was used to denote exclusion. The subscript YNG was used to denote young participants at long deadline, the subscript ELD was used to denote elderly participants at long deadline, and the subscript ELDX was used to denote elderly participants at an extra long deadline.



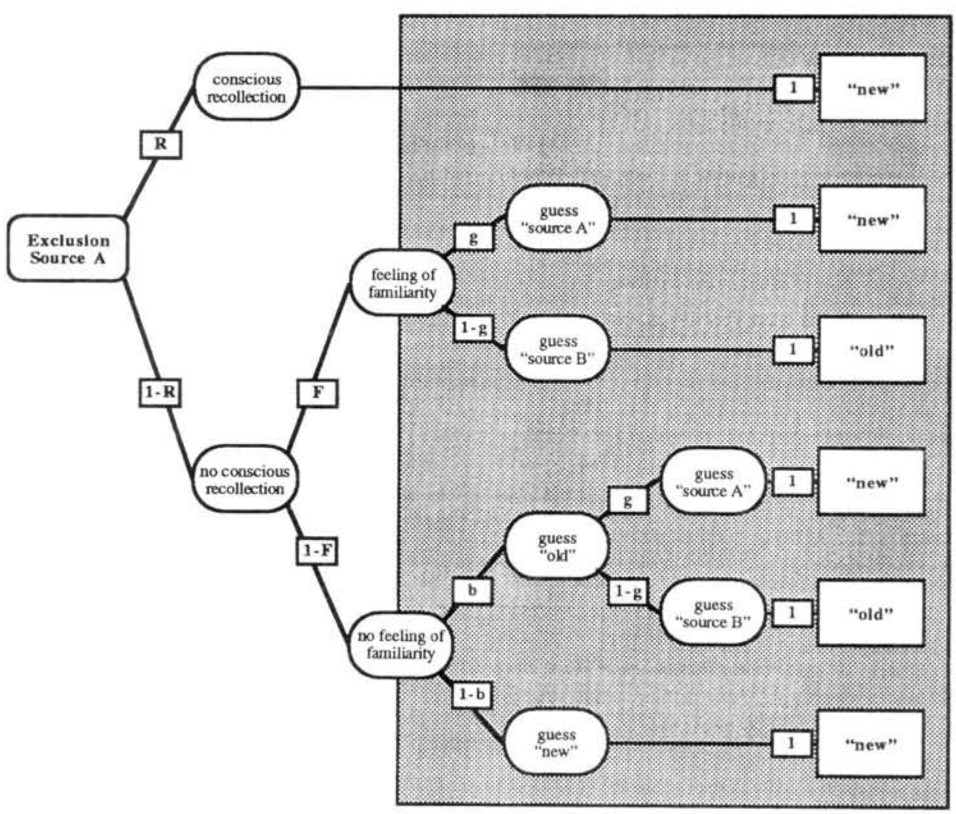


Figure 1. General processing tree diagram for exclusion items in a source-monitoring model (upper panel) and in an independence model (lower panel). D = detection; d = discrimination; g = probability of guessing that a nonrecollected, nonfamiliar item was a "read" word; b = bias to respond "old" to nonrecognized items; R = recollection; F = familiarity.

$D1_{\text{ELD}} = D\ 1_{\text{ELDX}}$ and $D3_{\text{YNG}} = D3_{\text{ELD}} = D\ 3_{\text{ELDX}}$). The hypothesis of equal detection parameters across the three groups was rejected, as $\chi^2(8, N = 6129) = 29.78$ surpasses the critical value of $\chi^2(8, N = 6129) = 21.96$.

Next, instead of restricting the detection parameters, I tested the hypothesis that the discrimination parameters were the same for the three groups within each level of repetition (i.e., $d1_{\text{YNG}} = d1_{\text{ELD}} = d1_{\text{ELDX}}$ and $d3_{\text{YNG}} = d3_{\text{ELD}} = d3_{\text{ELDX}}$). The hypothesis of equal discrimination parameters across the three groups was rejected as $\chi^2(8, N = 6129) = 68.76$ surpasses the critical value of $\chi^2(8, N = 6129) = 21.96$.

Finally, I tested the hypothesis that both the detection and discrimination parameters were the same for the three groups within each level of repetition. The hypothesis of equal detection and discrimination parameters across the three groups was rejected, as $\chi^2(12, N = 6129) = 80.76$ surpasses the critical value of $\chi^2(12, N = 6129) = 28.30$.

In summary, fitting the data with the source-monitoring model required separate parameters for each of the three groups of participants for both detection and discrimination. In contrast, the independence multinomial model, described in the next section, allowed the results to be fit with fewer parameters.

Independence multinomial model: Early selection. An independence multinomial model, based on those reported by Buchner et al. (1995) and Jacoby (1998), was fit to the data. The portion of that model that describes exclusion performance is shown in the lower panel of Figure 1, and the full model is described in the Appendix. The model includes a recollection parameter and a familiarity parameter (as opposed to discrimination and detection parameters). On the basis of that model, the account of results from Experiments 1–3 is the same as used when reporting those results. The model holds that recollection of source information circumvents, rather than follows, a source-invariant assessment of familiarity. Whereas the source-monitoring model treats the recovery of source information as a late filter, the independence model treats the recovery of source information as an early filter that allows rejection of items that were recollected as earlier read, without assessing their familiarity.

To allow a more direct comparison with the source-discrimination model, parameters that account for bias and guessing (b and g parameters) were included in the same manner as in that model. As is illustrated in Figure 1, the structure of the two models is very similar. This is important when attempting to discriminate between the models.

As before, I started with the assumption that b was constant across tests (i.e., $b_{\text{INC}} = b_{\text{EXC}}$), and that F and R were constant across tests (i.e., $F_{\text{INC}} = F_{\text{EXC}}$ and $R_{\text{INC}} = R_{\text{EXC}}$). I then tested the hypothesis that the g parameter was equal across tests and groups (i.e., $g_{\text{INCYNG}} = g_{\text{EXCYNG}} = g_{\text{INCELD}} = g_{\text{EXCELD}} = g_{\text{INCELDX}} = g_{\text{EXCELDX}}$). With this restriction, the model fit the data very well, producing a $\chi^2(2, N = 6129) = 3.87$ which is well below the critical value of $\chi^2(2, N = 6129) = 10.60$. Notice that the chi-square value here is identical to the chi-square from the source-monitoring model.

Next, I tested the hypothesis that the bias parameters were the same between the three groups: (i.e., $b_{\text{YNG}} = b_{\text{ELD}} = b_{\text{ELDX}}$). The model was found to fit the data quite well, as $\chi^2(4, N = 6129) = 4.12$ is below the critical value of $\chi^2(4, N = 6129) = 14.86$. Once again, the chi-square value here is exactly the same as that of the source-monitoring model.

With these restrictions in place, I tested the hypothesis that the familiarity parameters were the same for the three groups within each level of repetition (i.e., $F1_{\text{YNG}} = F1_{\text{ELD}} = F1_{\text{ELDX}}$ and $F3_{\text{YNG}} = F3_{\text{ELD}} = F3_{\text{ELDX}}$). The hypothesis of equal familiarity parameters across the three groups was not rejected, as $\chi^2(8, N = 6129) = 7.68$ is well under the critical value of $\chi^2(8, N = 6129) = 21.96$. As with the ANOVA reported in the previous section, I failed to reject the hypothesis that estimates of familiarity did not differ among the three groups.

Next, instead of restricting the familiarity parameters, I tested the hypothesis that the recollection parameters were the same for the three groups within each level of repetition (i.e., $R1_{\text{YNG}} = R1_{\text{ELD}} = R1_{\text{ELDX}}$ and $R3_{\text{YNG}} = R3_{\text{ELD}} = R3_{\text{ELDX}}$). The hypothesis of equal recollection parameters across the three groups was rejected, as $\chi^2(8, N = 6129) = 75.47$ surpasses the critical value of $\chi^2(8, N = 6129) = 21.96$. Again the multinomial results mirrored that of the ANOVA reported in the previous section, as I rejected the hypothesis that estimates of recollection did not differ among the three groups.

Finally, I tested the hypothesis that both the familiarity and recollection parameters were the same for the three groups within each level of repetition. The hypothesis of equal familiarity and recollection parameters across the three groups was rejected, as $\chi^2(12, N = 6129) = 80.76$ surpasses the critical value of $\chi^2(12, N = 6129) = 28.30$.

In summary, the multinomial analysis, using a model based on the assumption that recollection and familiarity independently contribute to performance, revealed results that are the same as those found using the process-dissociation estimation procedure. The estimate of recollection (R) is nearly identical to those gained using the original (Jacoby, 1991) process-dissociation estimation procedure. The absolute magnitude of estimates of familiarity (F) differ for the two means of computing estimates, but the pattern of results is the same. The differences in absolute magnitude of F are because the multinomial model includes separate parameters to take guessing into account, whereas the original estimation procedure combines any effects of guessing with those of automatic influences of memory in the estimate of F.

Choosing between models. If one obtains a fit to the source-monitoring multinomial model by allowing detection (D) and discrimination (d) parameters to vary freely, then the goodness of fit will be identical to the goodness of fit of the independence multinomial model with familiarity (F) and recollection (R) parameters being allowed to vary freely. This is because the only difference between the two models is in the parts of the models that contain those pairs of parameters. The remaining portions of the models (parts in shaded areas in Figure 1) are identical. Further, when memory parameters are allowed to vary freely, the values of parameters in one model can be computed given values of parameters from the other model. For example, the value of the parameter for recollection in the independence model can be computed by multiplying the detection and discrimination values from the source-monitoring model ($R = D \times d$).

Clearly, goodness of fit cannot be used to discriminate between the independence and source-monitoring models when memory parameters are allowed to vary freely. Rather, to choose between models, one must make an assumption about the relationship between processes (recollection and familiarity) or measures (recognition and source memory) and predict findings of dissociations. Only by constructing situations in which invariance is predicted, as done here, and by restricting parameters when fitting the data, can the models be pitted against each other (cf. Buchner et al., 1997).

Although the goodness of fit would be the same for the two models given that memory parameters are allowed to vary freely, interpreting the results (choosing which model to use) cannot be done in a neutral or assumption-free manner. Estimates of recollection in the independence model are not identical to estimates of source discrimination in the source-monitoring model, and without predicted invariance, there is no reason to prefer one set of parameters over the other. Differences between the parameters reflect different assumptions about the relation between detection of old items (familiarity) and source discrimination (recollection).

Results show that the distinction between recollection and familiarity in the independence model is more fundamental than is the distinction between recognition and source memory. By assuming independence, I was able to predict invariance in the familiarity parameter across conditions and show that age-related differences and requiring fast responding only influenced recollection. In contrast, reliance on the source-monitoring model required more parameters to fit the results. By the source-monitoring model, elderly differed from young participants in both detection of old items and source discrimination, as did conditions created by varying the amount of time allowed for responding. As is discussed later, the necessity of additional parameters to fit the source-monitoring model is expected if recollection contributes to both recognition and source memory.

General Discussion

Older adults are more susceptible to a variety of memory distortions and illusions than are younger adults (e.g., Cohen & Faulkner, 1989; Schacter, Norman, & Koutstaal, 1998).[4] Results of the present experiments add to this list by showing that older adults are also more prone to showing an ironic effect of repetition. Repeatedly reading a word decreased the probability of falsely accepting the word as earlier heard for young participants in a long-deadline condition but had an opposite effect for elderly participants tested in the same condition (Experiments 1 and 4). An ironic effect of repetition that was the same as found for elderly participants was also shown by young participants who were required to respond rapidly (Experiments 1 and 3) or who read words under conditions of divided attention (Experiment 3).

The finding that young adults at short deadlines show an ironic effect of repetition similar to older adults might be taken to imply that the effect in the elderly is due to general slowing of processing (e.g., Salthouse, 1996). If that were true, these effects should be reversible if more time to respond is given. However, results were inconsistent across experiments. Elderly participants in an extra-long-deadline condition showed every possible pattern of results: an ironic effect of repetition (Experiment 1); no effect of repetition (Experiment 2); and a reduction in errors with increased repetition (Experiment 4). This variability in results is, in part, likely due to differences in level of cognitive functioning of elderly participants in the particular samples, particularly those who were most elderly. For example, in Experiment 2, participants who were over 75 years old showed an ironic effect of repetition, whereas for those 75 years and under, repetition decreased the probability of mistakenly accepting earlier-read words.

Opposition procedures can provide a measure of age-related decline in ability to recollect that is more sensitive than is recognition-memory performance. The experiments reported here showed large age-related differences in exclusion performance, although older and younger participants did not differ in their ability to recognize words that were heard earlier (Experiments 1, 2, and 4). Jennings and Jacoby (1997) reported similar results and argued that recognition memory can be insensitive to age-related differences in recollection because those differences are masked by the use of familiarity as a basis for recognition memory. However, exclusion performance is not always a more sensitive measure of age-related differences in memory than is recognition-memory performance. In Experiment 4, older adults in the inclusion test condition were less able to recognize earlier-read words than were younger adults, and that difference was as large as the difference in exclusion performance. It is unclear to us why recognition memory of earlier-read words showed an effect of aging but recognition of earlier-heard words did not show such an effect.

Findings of opposite effects of repetition for younger and older adults clearly show that repetition has separate effects on recollection and familiarity. Repeatedly reading a word increases its familiarity, and that familiarity can be attributed to an incorrect source and result in the word's being mistakenly accepted as heard earlier. However, recollection is also enhanced by repeatedly reading a word, and recollection can successfully oppose familiarity and allow errors in exclusion performance to be avoided. The opposite effects of repetition for young and older adults provide strong evidence that repetition has these two effects; this allows one to conclude that the ability to recollect is more vulnerable to effects of aging than is familiarity.

[4] A similar susceptibility to memory distortions is reported to occur in children (Schacter, Kagan, & Leichtman, 1995; Bruck & Ceci, 1993, as cited in Bruck & Ceci, 1995).

Process Dissociations

The use of opposition procedures is sufficient to show the necessity of a dual-process model and is potentially useful for diagnosing age-related deficits in memory. However, an opposition condition alone does not provide a means of measuring the contributions of the different forms of processing. Differences in exclusion performance can reflect differences in recollection, differences in familiarity, or differences in both of the two forms of processing. The process-dissociation procedure (e.g., Jacoby, 1991, 1998) combines results from an exclusion condition with those from an inclusion condition to estimate the contributions of recollection and familiarity. Estimates gained using that procedure (Experiment 4) showed that both aging and fast responding resulted in a reduction in recollection in combination with unchanged automatic influences of memory (familiarity). The process dissociation produced by age-related differences in memory replicates results of earlier experiments (Jennings & Jacoby, 1993, 1997) as does the dissociation produced by varying response deadline (e.g., Yonelinas & Jacoby, 1994).

The process-dissociation procedure can be cast in the form of a multinomial model to take differences in guessing into account (e.g., Buchner et al., 1995). Doing so will not change the pattern of results so long as false-alarm rates for new words, used to estimate guessing, do not differ across type of test (inclusion vs. exclusion) or groups (Jacoby, 1998; Yonelinas & Jacoby, 1996). Such differences were not found in Experiment 4, and the pattern of results was the same for estimates gained using a multinomial model as for estimates computed using the original estimation procedure. Similarly, Jacoby (1998) analyzed recall cued with word stems using the same multinomial model as used here to estimate recollection and familiarity and found results that were equivalent in pattern to those found with the original estimation procedure.

Details of the results of Experiment 4, such as the lack of differences in false alarms to new words for inclusion and exclusion tests, suggest that assumptions of the process-dissociation procedure were met in that experiment. However, the experimental procedure is such that assumptions can easily be violated. Although most attention has been given to the independence assumption, an assumption of equal recollection across tests is also required to compute estimates and is potentially problematic. Participants might rely less on recollection for an inclusion than for an exclusion test (e.g., Graf & Komatsu, 1994). Violation of the equal-recollection assumption can produce artifactual dissociations that are the same as would be produced by violating the independence assumption (Jacoby, 1998). We have developed alternative procedures that are more likely to satisfy the equal-recollection assumption than is the inclusion–exclusion procedure. Yonelinas and Jacoby (1995) used a Sternberg memory-search task and created conditions that serve the same role as inclusion and exclusion tests by manipulating the relation of target items to prior training rather than by manipulating test instructions. Results gained using that procedure converge with those from the inclusion–exclusion procedure. As an example, manipulating response deadline influenced recollection but left estimated familiarity unchanged. Similarly, Hay and Jacoby (in press) varied prior training to create conditions equivalent to inclusion and exclusion tests and showed that elderly and young participants differed only in their ability to recollect. Effects of habit, an automatic influence of memory, were age invariant.

Yonelinas, Kroll, Dobbins, Lazzara, and Knight (1998) provided evidence to support the assumption that recollection and familiarity serve as independent bases for recognition memory and to show that, unlike the normal elderly, amnesiacs also suffer a deficit in familiarity. They analyzed the memory performance of amnesiacs by using a dual-process model that treats familiarity as reflecting a signal-detection process and, thereby, explicitly incorporates response bias. Reanalysis of results from published studies as well as results from a new experiment supported the conclusion that amnesia was associated with deficits in both recollection and familiarity—but with a much greater deficit in recollection. Further, they examined receiver-operating characteristics (ROCs) in amnesiacs and controls and showed that the difference in shape of ROCs supported predictions of their model along with its underlying independence assumption.

Advantages of Assuming Independence: Indifference Is Not a Virtue

Buchner et al. (1995) argued that an advantage of their multinomial approach is that it is largely indifferent to the relation between processes or tasks. Results of the present experiments show that goodness of fit of a source-monitoring multinomial model is identical to that of an independence model when memory parameters in the two models are allowed to vary freely. However, it is not a virtue for the goodness-of-fit to be indifferent. Although the fit of the model does not change with the relation between processes, the meaning of the measures gained from the model does change. The relation between processes dictates when dissociations will be found as well as the interpretation of measures. Findings of process dissociations do not prove that recollection and familiarity serve as independent bases for responding (Erdfelder & Buchner, 1998; Curran & Hintzman, 1997), but an assumption of independence gave reason to predict invariances in familiarity that were found. Findings of such invariances allow results to be fit with fewer parameters and are important for the diagnosis and treatment of age-related differences in memory. The results show that age-related differences in performance were because of differences in recollection—familiarity was age invariant.

Source-monitoring multinomial models have not been joined with a theory that provides guidelines for when invariances in recognition- or source-memory performance will be found. The importance of predicting invariance is obvious when one considers the goal of diagnosing age-related deficits in memory. A claim that the elderly suffer a deficit in source memory has little meaning if the measure of source memory shows that the deficit varies radically across different levels of recognition-memory performance. Indeed, without changes in source memory across invariant recognition, there is no reason to distinguish between the two measures of memory. Batchelder and Riefer (1990) motivated their multinomial approach to measuring memory for source by showing that alternative measures of source memory reflect differences in recognition-memory performance as well as differences in source memory (see also Murnane and Bayen, 1996). However, stability in the measure of source memory over levels of recognition memory cannot be expected if recollection contributes both to detection of old items (D) and implies memory for source, allowing discrimination among sources (d).

Hintzman and Curran (1994) suggested that recognition is mediated primarily by an assessment of familiarity and that recollection (source information) is used only in difficult cases. An implication of their view is that recall of source information serves as a late selection mechanism, in which participants "recall to reject" items of high familiarity (see Clark & Gronlund, 1996). The recall-to-reject view is implicit also in the source-monitoring model used to fit results of Experiment 4. For that model, the probability of retrieving source information is conditional on having recognized or detected an item as old, and has an impact on responding only for exclusion tests. Responses for the inclusion test are the same whether or not participants are able to discriminate among sources; that is, changes in (d) would not change performance on an inclusion test.

In contrast, the independence model holds that recollection contributes to inclusion performance, recognition memory, as well as exclusion performance; that is, the independence model incorporates a dual-process model of recognition memory (see Clark & Gronlund, 1996, for a discussion of evidence for a dual-process model of recognition), whereas results from the source-memory model are interpreted most easily if detection of old items depends only on familiarity. By the independence model, a consistent relationship between source memory and recognition memory should not be expected because both parameters can reflect mixtures of recollection and familiarity. However, the independence assumption does predict that recollection will remain invariant across levels of familiarity and vice versa, predicting process dissociations.

To measure source memory (recollection) adequately, one must take response biases into account and make an assumption about the relation between recognition and source memory, which, in turn, requires that one adopt a model of recognition-memory performance. McElree et al. (in press) provided evidence to show that, rather than always being preceded by familiarity, recollection can be sufficiently rapid to allow ironic effects of repetition to be avoided even when very fast responding is required. They describe how ironic effects of repetition can be accommodated in the framework of global-memory models (e.g., MINERVA 2, Hintzman, 1988; search of associative memory [SAM], Gillund & Shiffrin, 1984; theory of distributed associative memory [TODAM], Murdock, 1982). Ratcliff et al. (1995) showed that a two-process variant of SAM fit data reported by Jacoby (1991), and that same model would fit the dissociations reported here. McElree et al. noted that this difference in processing (familiarity vs. recollection) can be recast in terms of a difference in information content (e.g., item vs. source information) and outlined how global-memory frameworks could accommodate findings of ironic effects either by postulating a separate recall operation or by incorporating different types of information in a retrieval operation. Although important for theories of recognition memory, discriminating among accounts of dissociations in terms of processes versus content (Clark & Gronlund, 1996) may not be necessary for purposes of isolating memory deficits in special populations. As an example, for purposes of diagnosing age-related difference in memory, it may not matter whether the elderly are said to suffer a deficit in recollection or are said to not rely on source information when judging familiarity.

Older adults' greater susceptibility to false memory (e.g., Jacoby, in press; Schacter, Kagan, & Leichtman, 1995) results from failure to use recollection as a basis for excluding test items that are familiar for a wrong reason. People sometimes make exclusion errors even though, if directly asked, they could report the source information that would allow those errors to be avoided; this is likely to be particularly true for older adults (e.g., Dywan & Jacoby, 1990; Multhaup, 1995). Understanding exclusion errors requires that one consider differences in familiarity as well as differences in recollection. Recognition and source memory will not always be independent if recollection, which implies source memory, serves as a basis for recognition-memory performance. Focusing on processes underlying recognition- and source-memory measures reveals invariances that would otherwise not be observed. An assumption of independence gives reason to expect invariance in the measure of recollection across changes in familiarity and vice versa, producing stability of the sort required to make the measures potentially useful for diagnosing memory deficits. Deficits in recollection can be accompanied by preserved automatic influences of memory and, thereby, produce ironic effects of repetition.

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(Appendix follows on next page)

Appendix

Source-Monitoring Model

Formula	Response
Inclusion items	
Presented once at study	
$D_{1_{INC}} \cdot d_{1_{INC}}$	Yes
$D_{1_{INC}} \cdot (1 - d_{1_{INC}}) \cdot g_{INC}$	Yes
$D_{1_{INC}} \cdot (1 - d_{1_{INC}}) \cdot (1 - g_{INC})$	Yes
$(1 - D_{1_{INC}}) \cdot b_{INC} \cdot g_{INC}$	Yes
$(1 - D_{1_{INC}}) \cdot b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - D_{1_{INC}}) \cdot (1 - b_{INC})$	No
Presented 3 times at study	
$D_{3_{INC}} \cdot d_{3_{INC}}$	Yes
$D_{3_{INC}} \cdot (1 - d_{3_{INC}}) \cdot g_{INC}$	Yes
$D_{3_{INC}} \cdot (1 - d_{3_{INC}}) \cdot (1 - g_{INC})$	Yes
$(1 - D_{3_{INC}}) \cdot b_{INC} \cdot g_{INC}$	Yes
$(1 - D_{3_{INC}}) \cdot b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - D_{3_{INC}}) \cdot (1 - b_{INC})$	No
Not presented at study (new)	
$b_{INC} \cdot g_{INC}$	Yes
$b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - b_{INC})$	No
Exclusion items	
Presented once at study	
$D_{1_{EXC}} \cdot d_{1_{EXC}}$	No
$D_{1_{EXC}} \cdot (1 - d_{1_{EXC}}) \cdot g_{EXC}$	No
$D_{1_{EXC}} \cdot (1 - d_{1_{EXC}}) \cdot (1 - g_{EXC})$	Yes
$(1 - D_{1_{EXC}}) \cdot b_{EXC} \cdot g_{EXC}$	No
$(1 - D_{1_{EXC}}) \cdot b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - D_{1_{EXC}}) \cdot (1 - b_{EXC})$	No
Presented 3 times at study	
$D_{3_{EXC}} \cdot d_{3_{EXC}}$	No
$D_{3_{EXC}} \cdot (1 - d_{3_{EXC}}) \cdot g_{EXC}$	No
$D_{3_{EXC}} \cdot (1 - d_{3_{EXC}}) \cdot (1 - g_{EXC})$	Yes
$(1 - D_{3_{EXC}}) \cdot b_{EXC} \cdot g_{EXC}$	No
$(1 - D_{3_{EXC}}) \cdot b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - D_{3_{EXC}}) \cdot (1 - b_{EXC})$	No
Not presented at study (new)	
$b_{EXC} \cdot g_{EXC}$	No
$b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - b_{EXC})$	No

Note. The parameters shown in this model are generic. In fact, each of the three groups of participants from Experiment 3 had a separate set of parameters. "Yes" and "No" indicate the responses that would be given on an inclusion (INC) or exclusion (EXC) test.

Parameter estimates derived using the source-monitoring multinomial model shown above. With the assumption that g and b were constant across conditions, this model fit the data quite well, as $\chi^2(4, N = 6129) = 4.12$, which was well below the critical value of $\chi^2(4, N = 6129) = 14.86$.

Constraints:
$D_{1_{INC}} = D_{1_{EXC}}$ and $D_{3_{INC}} = D_{3_{EXC}}$ and $d_{1_{INC}} = d_{1_{EXC}}$ and $d_{3_{INC}} = d_{3_{EXC}}$
$g_{INCYNG} = g_{EXCYNG} = g_{INCELD} = g_{EXCELD} = g_{INCELDX} = g_{EXCELDX}$
$b_{INC} = b_{EXC}$
$b_{YNG} = b_{ELD} = b_{ELDX}$

Parameter	Young, long deadline	Elderly, long deadline	Elderly, extra-long deadline
D_1	.60	.45	.46
D_3	.84	.72	.76
d_1	.57	.36	.34
d_3	.77	.37	.59
b	.17	.17	.17
g	.10	.10	.10

Note. D = detection (separate parameter for 1x and 3x presented items); d = discrimination (separate parameter for 1x and 3x presented items); b = bias to respond "old" to nonrecognized items; g = probability of guessing that a nonrecollected, nonfamiliar item was a "read" word; INC = inclusion; EXC = exclusion; YNG = young, long deadline; ELD = elderly, long deadline; ELDX = elderly, extra-long deadline; INCYNG = inclusion, young, long-deadline; EXCYNG = exclusion, young, long deadline; INCELD = inclusion, elderly, long deadline; EXCELD = exclusion, elderly, long deadline; INCELDX = inclusion, elderly, extra-long deadline; EXCELDX = exclusion, elderly extra-long deadline.

Independence Model

Formula	Response
Inclusion items	
Presented once at study	
$R_{1_{INC}}$	Yes
$(1 - R_{1_{INC}}) \cdot F_{1_{INC}} \cdot g_{INC}$	Yes
$(1 - R_{1_{INC}}) \cdot F_{1_{INC}} \cdot (1 - g_{INC})$	Yes
$(1 - R_{1_{INC}}) \cdot (1 - F_{1_{INC}}) \cdot b_{INC} \cdot g_{INC}$	Yes
$(1 - R_{1_{INC}}) \cdot (1 - F_{1_{INC}}) \cdot b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - R_{1_{INC}}) \cdot (1 - F_{1_{INC}}) \cdot (1 - b_{INC})$	No
Presented 3 times at study	
$R_{3_{INC}}$	Yes
$(1 - R_{3_{INC}}) \cdot F_{3_{INC}} \cdot g_{INC}$	Yes
$(1 - R_{3_{INC}}) \cdot F_{3_{INC}} \cdot (1 - g_{INC})$	Yes
$(1 - R_{3_{INC}}) \cdot (1 - F_{3_{INC}}) \cdot b_{INC} \cdot g_{INC}$	Yes
$(1 - R_{3_{INC}}) \cdot (1 - F_{3_{INC}}) \cdot b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - R_{3_{INC}}) \cdot (1 - F_{3_{INC}}) \cdot (1 - b_{INC})$	No
Not presented at study (new)	
$b_{INC} \cdot g_{INC}$	Yes
$b_{INC} \cdot (1 - g_{INC})$	Yes
$(1 - b_{INC})$	No
Exclusion items	
Presented once at study	
$R_{1_{EXC}}$	No
$(1 - R_{1_{EXC}}) \cdot F_{1_{EXC}} \cdot g_{EXC}$	No
$(1 - R_{1_{EXC}}) \cdot F_{1_{EXC}} \cdot (1 - g_{EXC})$	Yes
$(1 - R_{1_{EXC}}) \cdot (1 - F_{1_{EXC}}) \cdot b_{EXC} \cdot g_{EXC}$	No
$(1 - R_{1_{EXC}}) \cdot (1 - F_{1_{EXC}}) \cdot b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - R_{1_{EXC}}) \cdot (1 - F_{1_{EXC}}) \cdot (1 - b_{EXC})$	No
Presented 3 times at study	
$R_{3_{EXC}}$	No
$(1 - R_{3_{EXC}}) \cdot F_{3_{EXC}} \cdot g_{EXC}$	No
$(1 - R_{3_{EXC}}) \cdot F_{3_{EXC}} \cdot (1 - g_{EXC})$	Yes
$(1 - R_{3_{EXC}}) \cdot (1 - F_{3_{EXC}}) \cdot b_{EXC} \cdot g_{EXC}$	No
$(1 - R_{3_{EXC}}) \cdot (1 - F_{3_{EXC}}) \cdot b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - R_{3_{EXC}}) \cdot (1 - F_{3_{EXC}}) \cdot (1 - b_{EXC})$	No
Not presented at study (new)	
$b_{EXC} \cdot g_{EXC}$	No
$b_{EXC} \cdot (1 - g_{EXC})$	Yes
$(1 - b_{EXC})$	No

Note. The parameters shown in this model are generic. In fact, each of the three groups of participants from Experiment 3 had a separate set of parameters. "Yes" and "No" indicate the responses that would be given on an inclusion (INC) or exclusion (EXC) test.

(Appendix continues on next page)

Appendix (*continued*)

Parameter estimates derived using the independence multinomial model shown on page 21. With the assumption that g and b were constant across conditions, and the assumption that F was constant across conditions, this model fit the data quite well, as $\chi^2(8, N = 6129) = 7.68$, which was well below the critical value of $\chi^2(8, N = 6129) = 21.96$.

Constraints:

$F_{1_{INC}} = F_{1_{EXC}}$ and $F_{3_{INC}} = F_{3_{EXC}}$ and $R_{1_{INC}} = R_{1_{EXC}}$ and $R_{3_{INC}} = R_{3_{EXC}}$

$g_{INCYNG} = g_{EXCYNG} = g_{INCELD} = g_{EXCELD} = g_{INCELDX} = g_{EXCELDX}$

$b_{INC} = b_{EXC}$

$b_{YNG} = b_{ELD} = b_{ELDX}$

$F_{1_{YNG}} = F_{1_{ELD}} = F_{1_{ELDX}}$ and $F_{3_{YNG}} = F_{3_{ELD}} = F_{3_{ELDX}}$

Parameter	Young, long deadline	Elderly, long deadline	Elderly, extra-long deadline
F_1	.36	.36	.36
F_3	.59	.59	.59
R_1	.35	.16	.16
R_3	.64	.26	.45
b	.17	.17	.17
g	.10	.10	.10

Note. F = familiarity (separate parameter for 1x and 3x presented items); R = recollection (separate parameter for 1x and 3x presented items); b = bias to respond "old" to nonrecognized items; g = probability of guessing that a nonrecollected, nonfamiliar item was a "read" word; INC = inclusion; EXC = exclusion; YNG = young, long deadline; ELD = elderly, long deadline; ELDX = elderly, extra-long deadline; INCYNG = inclusion, young, long-deadline; EXCYNG = exclusion, young, long deadline; INCELD = inclusion, elderly, long deadline; EXCELD = exclusion, elderly, long deadline; INCELDX = inclusion, elderly, extra-long deadline; EXCELDX = exclusion, elderly extra-long deadline.

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