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Proactive Interference, Accessibility Bias, and Process Dissociations: Valid Subjective Reports of Memory

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Proactive interference was assessed with a variant of the process-dissociation procedure, which separates effects of habit (accessibility bias) and recollection (discriminability). In three cued-recall experiments, proactive interference was shown to be an effect of bias rather than an effect on actual remembering. Divided attention, age, and study duration selectively influenced the recollection parameter, whereas training probability selectively influenced the habit parameter. Furthermore, in Experiments 2 and 3, subjective reports of remembering were highly correlated with, and nearly identical to, objective estimates of recollection gained from the process-dissociation procedure. The authors discuss the relevance of the results to theories of proactive interference and argue that older adults' greater susceptibility to interference effects is sometimes caused by an inability to recollect rather than by an inability to inhibit a preponderant response.

Proactive interference refers to the impaired ability to remember an item because of its similarity to other items that were stored earlier in memory. Anderson and Neely (1996) reviewed results and theorizing about interference effects (see also Crowder, 1976). They illustrated such effects with the example of remembering where one's car was last parked. Because of proactive interference, one might mistakenly return to yesterday's parking spot, which is more usual, rather than to today's spot. The standard procedure for investigating proactive interference has been paired-associate learning, in which the experimental condition conforms to an *A-B, A-D* paradigm: Two different responses, *B* and *D*, are learned in association with the same stimulus. After learning *A-B*, participants in the experimental group study and are tested on their memory for *A-D*. The control group "rests" rather than learning *A-B* and only studies *A-D*. Proactive interference is measured as the advantage of the control over the experimental group in recalling D when probed with A.

Warrington and Weiskrantz (e.g., 1970) examined the possibility that poor retention by persons with amnesia is due to difficulties in retrieval produced by greater susceptibility to interference effects. To limit interference, they developed tests of the sort that were later named "indirect" or "implicit" tests of memory. They tested memory by presenting fragmented versions of earlier-studied words as cues for retrieval and found that with these cues, memory performance of people with amnesia was nearly equal to that of people with normally functioning memory. They described their results by saying,

> It may not be too far-fetched to suggest that effective normal day-to-day memory demands that previous events be forgotten or suppressed and the inability to do so in the amnesic subject produces responses analogous to prior-list intrusions recorded in formal verbal learning experiments. (p. 630)

The notion is that people with amnesia are more vulnerable to interference because of a deficit in their ability to inhibit inappropriate responses. Providing fragments as cues for retrieval was said to limit interference in a way that allowed them to eliminate incorrect, alternative responses just as could people with normal memory. Similar to the account of amnesia proposed by Warrington and Weiskrantz, age-related differences in memory have been explained as resulting from older adults being less able to inhibit preponderant, inappropriate responses (Hasher & Zacks, 1988; Zacks & Hasher, 1997).

A great deal of evidence now shows that conscious recollection, or explicit memory, is dissociable from more automatic responding such as that produced by habit, or implicit memory (for a review,

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see Roediger & McDermott, 1993). Hay and Jacoby (1999) used a variant of the process-dissociation procedure (Jacoby, 1991) to examine the possibility that age-related decline in memory is restricted to recollection. They introduced their article with an anecdote about an elderly math professor who committed an "action slip" by flying back from a conference to which he had driven. Their anecdote parallels the example of mistakenly returning to yesterday's parking spot because it is more usual, and it could have been described as an example of proactive interference rather than as an action slip. However, the conditions used by Hay and Jacoby differ from those that have traditionally been used to investigate effects of proactive interference. Whereas the traditional approach compares performance in an interference condition (*A-B, A-D*) to that in a neutral, control condition (rest, *A-D*), the process-dissociation procedure combines results from an interference (opposition) condition with those from a facilitation (in-concert) condition to separate the contributions of recollection and habit.

Hay and Jacoby demonstrated the utility of their approach with findings of process dissociations. The disadvantage of older, as compared to younger, participants was shown to be restricted to a deficit in recollection (Hay & Jacoby, 1999). Both decreasing study time and requiring fast responding also decreased the probability of recollection but left estimated habit unchanged (Hay & Jacoby, 1996). In contrast, manipulating prior training influenced estimated habit but left recollection invariant (Hay & Jacoby, 1996, Exp. 1).

Results reported by Hay and Jacoby (1996) can be interpreted as showing that manipulating prior training (proactive interference) influenced bias (habit) but left discriminability (recollection) unchanged. Although the advantages of separating effects on discriminability from effects on bias might seem obvious, the two types of effect cannot be separated by using the standard design for investigating proactive interference. In contrast, Jacoby, Toth, and Yonelinas (1993, pp. 150–151) noted that the equations used by the process-dissociation procedure to separate the contributions of recollection from automatic influences of memory are the same as those used by other researchers to separate discriminability effects from bias effects (e.g., Snodgrass & Corwin, 1988). Recollection corresponds to a measure of discriminability, whereas automatic influences of memory, or habit, corresponds to a measure of bias.

Our goal here is to forward a dual-process theory of proactive interference that distinguishes between recollection (discriminability) and habit (bias). Experiment 1 of the current article replicated and extended results reported by Hay and Jacoby (1996). Study took place under conditions of full or divided attention. Effects of manipulating attention are of interest because prior research has shown that effects of dividing attention during study are the same as those of aging (e.g., Craik & Byrd, 1982). We expected that dividing attention would reduce recollection (discriminability) but leave estimated habit (bias) unchanged, just as found for effects of aging (Hay & Jacoby, 1999). The manipulation of attention was factorially combined with a manipulation of prior training that was the same as used by Hay and Jacoby (1996). Hay and Jacoby varied prior training alone, without simultaneously manipulating a factor that was meant to influence recollection. Combining the manipulations of attention and prior training allowed us to examine any interaction between the two factors. Are people more susceptible to interference effects after divided attention during study? If so, why? The training manipulation created conditions that can be construed as serving the same role as the "rest control" conditions in standard investigations of proactive interference. These conditions are used to show the advantage of the process-dissociation procedure over the traditional design for investigating proactive interference.

As will be discussed, results of Experiment 1 are better described by the recollection/habit model than by a model that treats proactive interference as the result of a failure to inhibit or suppress a preponderant response. That is, the greater susceptibility to interference effects found after divided attention was due to a deficit in recollection rather than a deficit in the ability to inhibit a preponderant response. The same is shown to be true for older participants' greater susceptibility to interference effects. Experiments 2 and 3 replicated the finding by Hay and Jacoby (1999) that age-related differences in memory performance are due to a deficit in recollection, and investigated the relation between our objective measure of recollection and a subjective measure of recollection. The subjective measure is similar to that used in the remember/know procedure (Gardiner & Richardson-Klavehn, 2000; Tulving, 1985).

Does proactive interference influence remembering? It should not do so if "remember" judgments reflect recollection and recollection is uninfluenced by proactive interference. Recent focus on illusions of memory (e.g., Jacoby, Kelley, & Dywan, 1989; Roediger & McDermott, 2000) might obscure the general validity of subjective reports of remembering. That is, although false remembering occurs under some conditions, "remember" judgments may often be quite accurate. The present methods permit a comparison between subjective reports ("remember" responses in the remember/know procedure) and objective estimates of recollection derived from the process-dissociation procedure. High correspondence between objective and subjective measures of recollection provides further converging evidence to support the assumptions underlying the process-dissociation procedure as well as providing external validation for the subjective reports of remembering. The process-dissociation model, with its distinction between qualitatively different bases for responding, provides reason to expect correspondence between subjective and objective measures. To date, no techniques for validating subjective reports of remembering have been published.

Experiment 1

The procedure in Experiment 1 was similar to that used by Hay and Jacoby (1996, 1999) and is illustrated in Figure 1. The probability of a particular response appearing with a stimulus word was varied in the initial training session (Phase 1). During that initial phase, participants were exposed to pairs of associatively related words, with each stimulus word being paired with two different responses. The probability of a particular pairing was varied. For a 75% condition, the response that was made "typical" by training (e.g., *bone*) appeared with the stimulus (e.g., *knee*) on 75% of the trials (15 out of 20 presentations), whereas the response that was made "atypical" (e.g., *bend*) appeared on only 25% of the trials (5 out of 20 presentations). For the 50% condition, the two responses paired with a stimulus were presented equally often (10 out of 20 presentations).

	Phase 1	Phase 2	
Condition	Training	Study	Test
75/25			
Congruent	ale-beer 75%; ale-brew 25%	ale-beer	ale-b_e_
Incongruent	knee-bone 75%; knee-bend 25%	knee-bend	knee-b_n_
Guessing	cherry-pit 75%; cherry-pie 25%	----------	cherry-pi_
50/50			
Congruent	bed-sheet 50%; bed-sleep 50%	bed-sheet	bed-s_ee_
Incongruent	barn-farm 50%; barn-yard 50%	barn-yard	barn-_ar_
Guessing	lake-pond 50%; lake-pool 50%	----------	lake-po__

Figure 1. Overview of basic experimental design for Experiment 1. A guessing condition similar to an indirect test was added for comparison with estimates of habit derived from the process-dissociation procedure. For the 50/50 condition, the responses were arbitrarily selected to be congruent or incongruent.

Short lists of pairs were presented in Phase 2 of the experiment. After each study list, participants were given a cued-recall test for pairs in the list just studied. Stimulus words were presented with a fragmented version of the response, and participants were instructed to complete the fragment with the response presented in the immediately preceding study list. The fragments were such that they could be completed with either of the two responses that had been paired with a stimulus during the training phase. For congruent test pairs, the response presented in the study list was the one presented most frequently during training, making recollection and habit congruent in dictating the same response. For incongruent test pairs, habit and recollection were placed in opposition—the response presented in the study list was the one presented least frequently during training. Responding with the word that was made "typical" by training counted as correct recall for congruent test pairs but counted as false recall for the incongruent test pairs. Participants were instructed to produce a response to each test item and to guess if necessary. Presentation of fragments as cues made it easy for participants to follow those instructions, and resulted in their responses being restricted to the alternatives presented during training.

For congruent test pairs, correct recall could result either from recollection of the studied word or, when recollection failed, from reliance on habit developed during training: $P(\text{correct recall}|\text{congruent}) = R + (1 - R)H$. For incongruent test pairs, false recall (e.g., saying *bone* when *bend* was in the study list) occurs when recollection fails in combination with reliance on habit: $P(\text{false recall}|\text{incongruent}) = (1 - R)H$. Using these equations, we can estimate recollection by subtracting the probability of false recall in the incongruent condition from the probability of a correct response in the congruent condition: $R = P(\text{correct recall}|\text{congruent}) - P(\text{false recall}|\text{incongruent})$. Given an estimate of recollection, an estimate of habit can be obtained by dividing the probability of false recall in the incongruent condition by the estimated probability of a failure of recollection: $H = P(\text{false recall}|\text{incongruent})/(1 - R)$.

Test items designed to measure guessing were included as a source of converging evidence for estimates of habit. These "guessing" items were not in the target study list but could be completed with either of the two responses that had been paired with the stimulus during training. Participants were warned that the test list would include guessing items and were instructed to respond to those items by producing the first completion word that came to mind. We expected performance on the guessing items to provide a relatively pure measure of habit that would agree with the estimate of habit gained by means of the process-dissociation procedure. Both should reflect the training probabilities used in Phase 1 of the experiment. Indeed, the procedure for constructing guessing items is similar to that used in experiments of probability learning; thus, one might expect performance on those items to show probability matching (Hay & Jacoby, 1996). For responses presented with a probability of .75 during training, the probability of producing the response to guessing items and estimated habit should be near .75, whereas estimated habit and the probability of guessing should be .50 for responses presented with a probability of .50 during training.

Experiment 1 also examined the effects of dividing attention during study on estimates of recollection and habit. Experiments using an inclusion/exclusion variant of the process-dissociation procedure have shown that dividing attention during study reduces recollection but leaves estimated automatic influences of memory unchanged (e.g., Jacoby, Toth, & Yonelinas, 1993), and we expected to find a similar dissociation using the "habit" procedure. That is, we expected the manipulation of full versus divided attention to selectively influence estimated recollection, whereas the manipulation of training was expected to selectively influence estimated habit.

Method

Participants. Thirty-two introductory psychology students from McMaster University participated for either a $10.00 stipend or two course credits. All participants were tested individually. The experiment was conducted in two sessions that took place on 2 successive days. Each session consisted of a training phase followed by a study–test phase.

Materials and design. A set of 18 stimulus words paired with two associatively related responses (e.g., *knee/bend, knee/bone*) was selected from the norms reported by Jacoby (1996). Both associatively related responses contained the same number of letters and could be used to complete the same word fragment (e.g., *knee/b—n—*). The list of 18 stimulus words and responses was divided into two sets of 9 stimulus words and responses to construct training pairs for the 75% and 50% conditions. The preexperimental probability of completing fragments with the various responses was equated across the sets of pairs used to construct conditions. Four formats were created, so that all response words occurred as the "typical" response equally often in both the 75% and 50% conditions. As an additional counterbalance to accommodate the attention manipulation at study, half of the study–test lists on each day were under either full- or divided-attention conditions. The manipulation of attention was blocked, with half of the participants receiving full attention first followed by divided attention and the other half of the participants receiving the opposite order.

The training phase on Day 1 consisted of five blocks of 72 trials. In each block, all stimulus word–word fragment pairs (e.g. knee/b—n—) were presented four times. For the 75% condition, the "typical" response was presented as the correct response on three trials, whereas the "atypical" response was presented as the correct response on the other trial. For the 50% condition, the "typical" and "atypical" responses (an arbitrary distinction) were presented as the correct response on two trials each. On Day 2, participants received only two blocks of training, which were the same as the first two blocks on Day 1. The trials were randomized within each block, with the restriction that no more than 3 trials with the same stimulus word appeared in a row.

On both days, the test phase consisted of 18 successive study–test lists divided into two blocks of 9 lists, one block each for the full- and divided-attention manipulation. Each study list contained word pairs that had been presented during training (e.g. *knee/bend*) and maintained the earlier proportion of "typical" and "atypical" items from training. Thus, each study list consisted of eight items: four 75% items (for which the "typical" response was shown on three of them) and four 50% items (for which the "typical" response was shown on two of them). Study trials that presented the "typical" response were called "congruent" trials because training was congruent with study, whereas study trials that presented the "atypical" response were called "incongruent" because training was incongruent with study. Within each set of 9 study lists, each "typical" item in the 75% condition was presented three times across different lists, whereas each "atypical" response was presented only once. For the 50% condition, each "typical" and "atypical" item was presented twice. For each test list of 10 items, test cues were presented for all eight study items and for two additional items that were not presented in the preceding study list. For these two additional "guessing" items, one stimulus was always selected from each of the two probability conditions (75% and 50%). Within each set of nine tests, all stimulus words and their respective fragments appeared as "guessing" items once. Thus across the 2 days of testing there were a total of 360 test trials, 180 in each attention condition and 90 in each probability condition. Within the 75% condition there were 54 congruent trials, 18 incongruent trials, and 18 guessing trials; in the 50% condition, there were 36 congruent trials, 36 incongruent trials, and 18 guessing trials. The presentation order for all items in the study and test lists was randomly determined and remained fixed across participants, with the constraint that no item was repeated within a list.

Procedure. The stimuli were presented by means of a PC-compatible computer with a VGA color monitor, and Schneider's (1990) Micro-Experimental Laboratory (MEL) software. Words were presented in lowercase white letters (approximately 3 mm × 5 mm in size), on a black background in the center of the screen.

In the training phase, word pairs were presented with the stimulus word intact and the second word missing some of its letters (e.g., *knee*/b—n—). Participants were instructed to guess the word that would complete the fragment and were told that the correct completion word would be associatively related to the intact word. The word and fragment remained on the screen for 2 s, during which time participants responded aloud with their predicted completion word. After the 2-s response window, the correct response word along with the stimulus word were presented for 1 s, two lines below the original stimulus word and fragment, followed by a 500-ms intertrial interval consisting of a blank screen. There were always two possible completions for each fragment, only one of which was presented as correct on any given trial. Participants were informed that each stimulus word would be paired with two completion words throughout the course of each block. They were instructed to try to predict the completion word that would appear as correct on each particular trial. Furthermore, they were told that some completion words would appear more often than others and that they should pay attention to the answers the computer was presenting because doing so would help guide their responding. Participants were encouraged to make a response on every trial before the correct answer appeared on the screen. Two examples were given: the first one to demonstrate the display and to encourage guessing; the second, to demonstrate the timing. A short break was taken between each block.

After training, participants received 18 successive study–test lists, divided into two blocks of 9 lists, one block for the full-attention condition and one block for the divided-attention condition. Order of the blocks was counterbalanced between participants. Each study pair was presented at a rate of 1 s per pair with a 500-ms interpair interval. Participants were instructed to read the word pairs silently and to remember them for the memory test that would follow the presentation of the study list. Each word pair was presented with a single digit on either side of it (e.g., 5 *knee/bend* 3). In the full-attention condition, the participants were told to ignore the numbers. In the divided-attention condition, they were told to say the sum of the two digits out loud before the next word pair was presented and to remember the word pairs for the following memory test. Between study and test, participants performed a short distracter task. A random number between 30 and 100 was presented on the computer screen immediately after each study list. Each number appeared for 1 s, followed by a blank screen for 6.5 s. During that time, participants counted backward by threes aloud, as quickly as possible, starting with the number that appeared on the screen. It was emphasized that the backward counting should continue until a message appeared that instructed them to begin the test. Different numbers were presented for the distracter task between each study–test list. The experimenter monitored the participants' performance in both math tasks (i.e., sum of the numbers, and backward counting).

After each study list, participants received a cued-recall test of memory for the word pairs just studied. For this test, stimulus words were presented with a fragmented version of the response with which they were paired in the study list (e.g., knee/b—n—)—the same cues presented during the training session in Phase 1. Participants were instructed to complete the fragments with the response words they had studied in the previous study list. They were told that if they could not recall the studied item, they were to guess with the first word that came to mind. In addition, participants were warned that some pairs would be tested although they had not appeared in the previous study list. For those test items, participants were told to complete the fragment with the first word that came to mind. Participants were given 3 s to respond at test. The experimenter using the keyboard coded the participants' responses, and the next trial was presented after a 500-ms intertrial interval. After completing nine study–test blocks, participants rested for a few minutes while the new instructions for the attention manipulation were given; they then proceeded to the next nine study–test blocks. The same procedure was followed on Day 2. Before the first study–test list on Day 1, a short practice was given with a three-item study list, followed by counting backward, and a four-item test list. The study list and test list were constructed from the two training examples plus two additional items that were not used elsewhere in the experiment.

Results and Discussion

Probabilities of correct responses on congruent test trials (hits) and incorrect responses on incongruent test trials (false recalls) are shown in Table 1 for each of the combinations of training (50/50 vs. 75/25) and attention (full vs. divided) conditions. The process-

Table 1
Hits and False Alarms (FAs) on Congruent and Incongruent Trials

Condition	Full/younger		Divided/older	
	Con (Hits)	Incon (FAs)	Con (Hits)	Incon (FAs)
Experiment 1				
50/50 items	.72	.32	.59	.43
75/25 items	.83	.40	.75	.57
Experiment 2				
67/33 items	.80	.35	.73	.44
Experiment 3				
67/33 items	.90	.18		

Note. Experiment 1 manipulated attention at study (full vs. divided). For Experiment 2, younger and older participants replaced the manipulation of attention from Experiment 1. For Experiment 3, only younger participants were tested. Con = congruent; Incon = incongruent.

dissociation equations described in the introduction were used to gain estimates of habit and recollection for each participant. Those estimates (Table 2) were then subjected to analyses of variance (ANOVAs) to examine their selectivity to effects of prior training and attention during study.

The analysis of estimates of recollection revealed a significant main effect of attention, $F(1, 31) = 136.23$, $MSE = .013$, with full attention during study yielding a higher estimate of recollection than did divided attention during study. The manipulation of training did not produce a significant main effect and did not interact with attention (both $Fs < 1.32$). The analysis of estimates of habit revealed a significant main effect of training condition, $F(1, 31) = 55.30$, $MSE = .018$. Habit was higher in the 75/25 than in the 50/50 training condition. Habit was not influenced by the manipulation of attention during study, and there was no significant interaction between prior training and attention during study ($Fs < 1.63$). Thus, dividing attention reduced estimates of recollection while leaving estimates of habit invariant. Conversely, manipulating training influenced estimates of habit but left estimates of recollection unchanged.

An analysis of the habit estimates and performance on guessing items showed only a main effect of training condition, $F(1, 31) = 100.16$, $MSE = .023$. Habit estimates were not significantly different from guessing scores ($F < 1$), and did not significantly interact with other factors ($Fs < 2.29$). Furthermore, habit estimates and guessing scores showed probability matching by being very close to the actual probabilities used during training. Collapsing across attention condition, estimates of habit and guessing for the 50/50 condition were .52 and .52, respectively; for the 75/25 conditions, they were .70 and .72, respectively. The convergence of guessing scores and estimates of habit provides support for assumptions underlying the estimation procedure. That convergence as well as the finding of probability matching replicates results reported by Hay and Jacoby (1996, 1999).

Comparison of Designs: Proactive Interference as Bias

The traditional design for investigating proactive interference compares performance in an experimental group (*A-B*, *A-D*) with that of a control group that did not engage in prior learning (rest, *A-D*). Proactive interference can also be measured by a similar comparison of performance on incongruent test trials for the 75/25 and 50/50 training conditions. The difference between those conditions is the amount of exposure to interfering material (*A-B*), just as is true for the traditional comparison. As shown in Table 1, the probability of false recall was higher in the 75/25 condition, revealing proactive interference. Furthermore, the increase in the probability of false recall in the 75/25, as compared with the 50/50, condition was nearly twice as large when divided rather than full attention was devoted to study (.14 vs. .08). Those results are all that one would have if the traditional design for investigating proactive effects had been used. They might be misinterpreted as showing that dividing attention and proactive interference reduced memory performance by the same means, and that dividing attention during study increased susceptibility to proactive interference.

However, combining results from the congruent test trials (correct recalls) with results from the incongruent test trials (false recalls) clearly shows that dividing attention had a very different effect from that produced by manipulating prior training. Dividing attention reduced discriminability (recollection). In contrast, proactive interference, produced by manipulating prior training, occurred solely because of an influence on bias (habit) that was the same regardless of whether attention was divided or not. Prior training had no effect on recollection. These differential effects could not have been detected with the traditional design for investigating proactive interference. Probably because of their reliance on results from the traditional design, theories of proactive interference have not distinguished between discriminability and bias effects.

Table 2
Estimates of Habit (H) and Recollection (R) and Probabilities of Responding With the "Typical" Response on Guessing Trials

Condition	Full/younger R	Full/younger H	Full/younger Guess	Divided/older R	Divided/older H	Divided/older Guess
Experiment 1						
50/50 items	.40	.54	.52	.17	.51	.52
75/25 items	.43	.69	.74	.18	.70	.70
Experiment 2						
67/33 items	.44	.63	.66	.29	.62	.64
Experiment 3						
67/33 items	.72	.62	.67			

Note. Experiment 1 manipulated attention at study (full vs. divided). For Experiment 2, younger and older participants replaced the manipulation of attention from Experiment 1. For Experiment 3, only younger participants were tested.

Experiment 2

Experiment 2 examined age-related differences in memory. Hay and Jacoby (1999) found that aging selectively affected recollection but left habit unchanged. Other variants of the process-dissociation procedure have shown similar dissociations (for a review, see Jacoby, Jennings, & Hay, 1996). On the basis of that prior research, we expected older adults to be less able than young adults to recollect an earlier event but not to differ in their reliance on habit. A second purpose of Experiment 2 was to compare objective measures of recollection with participants' subjective reports of their memory experience.

Are people aware that they are recollecting when they are doing so? The procedure used to answer that question was largely the same as in Experiment 1 except that, after completing the fragment at test, participants were asked to report when they had recollected the completion word. They were instructed to say "recall" only if they had remembered something specific about studying the word in the immediately preceding list, such as an association, an image, or something more personal that arose during its presentation. Participants were warned that familiarity was not sufficient for a "recall" response, as all the words would seem familiar because of their presentation in prior lists and training.

We compared the objective measure of recollection, computed by using the process-dissociation procedure, with the probability of recollection indexed by the probability of participants saying "recall." A low correlation between the two measures would be expected if subjective reports were often invalid, which would reflect an illusion of remembering. However, we expected the probability of saying "recall" to be very similar to the objective probability of recollection. Correspondence between objective and subjective measures would provide converging evidence of the validity of the estimation procedure used to objectively measure recollection as well as providing an objective check of the subjective reports. Of particular interest was whether the correspondence between objective and subjective measures would be as large for older as for younger participants. Perhaps the subjective experience of older participants is less well calibrated to their objective performance. A possibility of this sort has been entertained in investigations of age-related differences in metamemory (e.g., Lovelace, 1990).

Our "recall" instructions are similar to those for "remember" judgments in the remember/know procedure introduced by Tulving (1985) and further developed by Gardiner and his colleagues (e.g., Gardiner, 1988; Gardiner & Richardson-Klavehn, 2000; Richardson-Klavehn, Gardiner, & Java, 1996). For that procedure, participants report on experiential states while taking a test of memory. The procedure has most often been used with tests of recognition memory performance. Participants classify items as "old" or "new" and further classify items called "old" as "remember" or "know." Items are to be classified as "remember" only if a detail of the study presentation is recollected. A "know" response is to be given to items that seem familiar but whose study presentation cannot be recollected. The remember/know procedure is aimed at measuring qualitatively different states of awareness. Dissociations between "remember" and "know" responses are produced by a variety of manipulations (for a review, see Gardiner & Richardson-Klavehn, 2000; Richardson-Klavehn et al., 1996). Older, as compared to younger, participants are less likely to "remember" but more likely to "know" that an item is old on a test of recognition memory (e.g., Parkin & Walter, 1992).

An obvious difference between our procedure and the remember/know procedure is that we did not ask participants to make "know" judgments. Rather, they were instructed to say "recall" only if they produced their response by means of remembering and, otherwise, to produce their response without comment. More important, our procedure provides a means of examining the relationship between objective and subjective recollection, whereas the standard remember/know procedure does not provide an objective measure of recollection that is separate from the subjective report measure. If participants only classified items as "remember" or "new," a procedure that is comparable to that used in our Experiment 2, there would necessarily be a perfect correlation between subjective reports and recognition memory performance. This is because the subjective report measure would also serve as the measure of "hits" for recognition memory performance.

However, for our procedure, there is no necessary relation between subjective reports and the objective measure of recollection—the objective measure of recollection does not rely on subjective report. For example, participants might differ in their willingness to say "recall" without differing in their objective measure of recollection and vice versa, with this happening to an extent that produced a zero correlation between the two measures. A procedure that would parallel ours for recognition memory would be a forced-choice test of recognition, in which participants are asked to report when an alternative had been chosen because of remembering. Again, it would be possible for there to be a correlation of zero between subjective reports of "recall" or "remember" and the objective measure of discriminability (recollection). Whereas the remember/know procedure has been used to examine experiential states, the goal of our procedure was to examine the validity of subjective reports, as indexed by their agreement with an objective measure of recollection.

Method

Participants. Sixteen introductory psychology students from McMaster University participated for course credit. The older adult participants were 16 volunteers over the age of 60 (mean age of 72.0 years), who were alumni of McMaster University or the University of Toronto and lived in the surrounding community. They were compensated for traveling expenses.

Materials, design, and procedure. The materials, design, and procedure were basically the same as in Experiment 1, with the exceptions that only one training probability was used and there was no manipulation of attention. Because there were fewer manipulations, the experiment was conducted in one session consisting of three blocks of training followed by 18 study–test lists. "Typical" items were presented on 67% of trials in training instead of 75% or 50%. The change in probability resulted in three blocks of training of 108 trials each. Within each block, each stimulus was presented six times: four times with its "typical" response and two times with its "atypical" response. The test phase consisted of 18 study–test lists and maintained the proportion of "typical" and "atypical" responses from training. Each study list contained nine word pairs, six "typical" and three "atypical." Overall, each of the 18 "typical" responses was presented six times across lists, and each of the 18 "atypical" responses was presented three times. Test cues were presented for all nine study items and for two additional guessing items. This resulted in a total of 198 test trials with 108 congruent trials, 54 incongruent, and 36 guessing trials. There was no study–test practice.

The instructions for the training and the distracter task were exactly the same as in Experiment 1. In the study phase, there were no numbers presented along with the study pairs. Participants were instructed to read the word pairs silently and to remember them for the following memory test. Word pairs in the study lists were again presented at 1 s per pair with a 500-ms interpair interval.

In addition to the test instructions used in Experiment 1, participants were asked to subjectively report on their memory experience. After responding at test, participants were instructed to say "recall" if the item given as a response was retrieved by means of a particular association, image, or something more personal from the time of study on the immediately preceding study list. Participants were cautioned that items in the study list would be very familiar to them and it would therefore be easy to mistakenly believe that they recalled a certain response when in fact they did not. As before, participants had 3 s to give their test response, but they were allowed to take as much time as they needed to give a subjective report on their memory experience. Participants initiated the next trial by pressing the spacebar on the computer.

Results and Discussion

The probability of a "typical" response on the congruent and incongruent trials is shown in Table 1 for both younger and older participants. Table 2 shows the estimates of recollection and habit calculated from the congruent and incongruent trials as in Experiment 1. Estimates of recollection revealed a significant effect of age, $F(1, 30) = 9.52$, $MSE = .018$, with younger participants having an advantage over older participants. Estimates of habit, however, showed no difference between the groups ($F < 1$). The probability of a "typical" response on guessing trials also did not differ for the two age groups ($F < 1$). An analysis that compared guessing probability and estimates of habit for younger and older participants revealed no significant effects ($Fs < 2.97$). That is, as in the previous experiment, estimates of habit converged with performance on guessing trials. It should be noted that estimates of habit and guessing scores reflect the 67% probability for "typical" items used during training in this experiment and are lower than in the first experiment, in which the "typical" item appeared on 75% of the trials during training.

Subjective Reports of Memory

Participants' "recall" responses were used to compute subjective estimates of recollection. The proportion of congruent and incongruent trials on which participants correctly responded "recall" is shown in Table 3. Estimates of subjective recollection were compared across age group and trial type (congruent vs. incongruent). The ANOVA revealed a significant effect of age group, $F(1, 30) = 19.40$, $MSE = .031$, but neither the main effect of trial type nor the interaction was significant ($Fs < 1$). Consequently, for each participant, an average value of subjective recollection was calculated by collapsing across the congruent and incongruent trials. That measure of subjective recollection was then compared with the measure of objective recollection calculated from the process-dissociation procedure. The results revealed a main effect of age group, $F(1, 30) = 15.73$, $MSE = .030$, with younger participants exhibiting higher recollection regardless of the measure. Neither the main effect of recollection measure nor the interaction of measure with age group reached significance ($Fs < 3.89$). However, it should be noted that the probability of subjective recollection was lower than the objective probability of recollection, particularly for the older participants.

The correlation between subjective recollection and objective recollection was quite high for both age groups; $r = .71$ for the younger group and $r = .81$ for the older group. The correlations were significant at the .005 level. These correlations suggest that participants were indeed aware of when they were recollecting.

A concern in interpreting the subjective report data is the likelihood of false recollection, that is, participants incorrectly saying "recall" when reporting a word that was not presented in the prior study list. The probabilities of such false recollection for incongruent, congruent, and guessing trials were .06, .04, and .06, respectively, for the older participants and .08, .05, and .04 for the younger participants. Subjective estimates of recollection were corrected by taking the average probability of correct recall responses for congruent and incongruent items and subtracting the false recall rate for guessing trials. That difference was then divided by 1 minus the false recall rate for guessing trials. Doing so uses a single-high-threshold model to correct for guessing. For a description of the rationale underlying a single-high-threshold model, see Macmillan and Creelman (1991, pp. 89–90).

Table 3
Objective and Subjective Measures of Recollection (R)

Condition	Objective R	Subjective R
Experiment 2		
Younger	.44	.43 (.41)
Older	.29	.24 (.19)
Experiment 3		
Younger	.72	.76 (.72)

Note. Numbers in parentheses are subjective R corrected for false R on guessing trials.

Analysis of objective recollection and corrected subjective recollection revealed significant main effects of age group and measure, $F(1, 30) = 17.61$, $MSE = .031$ and $F(1, 30) = 17.18$, $MSE = .005$, respectively. Moreover, the interaction of age group and measure was also reliable, $F(1, 30) = 4.47$, $MSE = .005$ (see Table 3). Subjective recollection, when corrected, was lower than objective recollection, and the difference was larger for older, as compared with younger participants. The subjective reports of older participants were slightly less well calibrated to their objective performance than were the subjective reports of younger participants. However, the correlation between corrected subjective reports and objective performance was high; $r = .72$ for the younger group and $r = .74$ for the older group. Again, correlations were significant at the .005 level.

We further examined effects of aging on the probability of false recall. For the congruent and incongruent tests, probabilities of false recall underestimate false recollection because a false recall could only occur when the wrong word was given as a response (e.g., *bone* rather than *bend*). Consequently, we conditionalized false recollection for the congruent and incongruent items on incorrect responding. An ANOVA of those conditional probabilities revealed a significant main effect of age group, $F(1, 30) = 4.46$, $MSE = .037$, but neither the main effect of trial type (congruent vs. incongruent) nor the interaction of trial type and age group were significant ($Fs < 2.64$). Younger participants were more likely than older participants (.25 vs. .15) to incorrectly say "recall" when producing the wrong response on incongruent or congruent test trials.

Guessing trials did not need to be conditionalized. For guessing trials, responses could not be correct or incorrect because the tested pair was not represented in the study list. Next, we compared the false recall rate averaged across congruent and incongruent items to the false recall rate for the guessing items, again with age group as a factor. Results of that analysis showed a main effect of trial type, $F(1, 30) = 43.54$, $MSE = .008$, as well as an interaction of trial type and age group, $F(1, 30) = 7.22$, $MSE = .008$. There was little difference between age groups for false recalls on guessing items (.04 vs. .06) although there was a large difference between younger and older participants for false recalls on congruent and incongruent items (.25 vs. .15).

For incorrect responses produced on congruent and incongruent tests, false recollection can reflect correct recognition of the cue (e.g., *knee*) as presented in the prior study list. For guessing items, in contrast, the cue word was not presented in the study list. The higher false recollection for congruent and incongruent tests suggests that recognition of the cue was a basis for the subjective experience of recalling, more so for the younger than for the older participants. That is, younger participants had higher false recollection on congruent and incongruent tests because they were more likely to correctly recognize the cue as studied and use that recognition as a basis for saying "recall." Differences in reliance on recognition of the cue might also help explain why there was a greater discrepancy between objective and subjective recollection for older participants.

We further discuss the subjective report data after reporting the results of Experiment 3. The subjective report procedure was changed in that experiment to account for the possibility that participants sometimes recollected studied words but were unable to recollect any details surrounding prior study of the word. The procedure for subjective reports used in Experiment 2 underestimates subjective recollection if participants sometimes recollect without recollecting details. Only younger participants were tested in Experiment 3.

Experiment 3

Method

Participants. Twenty-four introductory psychology students from McMaster University participated for course credit.

Materials, design, and procedure. The materials, design, and procedure were largely the same as in Experiment 2. However, instructions for subjective report in the test phase were changed. Instead of the participants only saying "recall" for items that they recalled, they gave a subjective report on every trial by responding "remember," "know," or "guess." The instructions for responding "remember" were identical to the ones used in Experiment 2 for "recall." "Know" was to be used when the participants could recall the item but could not recall any supporting details. "Guess" was to be used when they couldn't recall the item as being presented on the prior study list. The study duration was slowed to a rate of 3 s per pair with a 500-ms interpair interval. In addition, buffer items were added to the beginning and end of each training block. The buffer items, along with one guessing trial (total of 4 trials), were presented as practice trials to ensure that participants understood the subjective report instructions. The test phase consisted of 12 study–test lists and maintained the 67% of "typical" responses from training. Each study list contained nine word pairs, six "typical" and three "atypical." Overall, each of the 18 "typical" responses was presented four times across lists, and each of the 18 "atypical" responses was presented twice. Test cues were presented for all nine study items and for three additional guessing items. This resulted in a total of 144 test trials with 72 congruent trials, 36 incongruent trials, and 36 guessing trials. In this experiment, participants were given 7 s to respond at test and, as in Experiment 2, were allowed to take as much time as they needed to give a subjective report. The experimenter entered both responses into the computer; after a 500-ms intertrial interval, the next trial was presented.

Results and Discussion

The probability of a "typical" response on the congruent, incongruent, and guessing trials is shown in Table 1. The congruent and incongruent trials were used to calculate estimates of recollection and habit as in the previous experiments (Table 2). Notice that the longer study duration used in this experiment produced a much higher probability of recollection (.72) than was observed for younger participants in Experiment 2 (.44) although the estimates of habit and performance on guessing trials were nearly identical to those in the earlier experiment. A comparison of the estimates of habit with "typical" responses on guessing trials revealed no significant difference ($F < 1.29$). Thus, once again, estimates of habit converged with "typical" responses on guessing trials, and both measures again showed probability matching.

Subjective Reports of Memory

The probability of correctly saying "remember" was higher on congruent than on incongruent trials (.63 vs. .59), $F(1, 23) = 6.47$, $MSE = .004$, whereas the probability of correctly saying "know" did not differ for the two trial types (.15 vs. .15; $F < 1$). The difference in correct remembering reflects a difference in the opportunity for such responses. The probability of a correct response was higher on congruent than on incongruent trials (Table 1). Conditionalizing the probability of remembering on correct responding showed that the conditional probability of remembering was higher on incongruent than on congruent trials (.71 vs. .69), a result that is opposite to that found for unconditionalized probabilities. Regardless, the effect of trial type was small.

We summed the probability of "remember" and "know" reports to produce a measure of subjective recollection. Doing so is justified because both "remember" and "know" reports were claims to have recollected the word given as a response. The difference in instructions was that participants were to say "know" if they recollected the studied word but could not recollect any supporting details. The probability of subjective recollection, averaged across congruent and incongruent items, was compared with that of objective recollection calculated with the process-dissociation procedure. The difference between the two measures of recollection (Table 3) approached significance, $F(1, 23) = 3.71$, $MSE = .004$, $p < .07$. The correlation between correct subjective recollection, averaged across congruent and incongruent items, and the objective measure of recollection derived from the process-dissociation procedure was very high, $r = .90$, which is significant at the .001 level.

Probabilities of "remember" and "know" reports were also summed to provide a measure of false subjective recollection—a claim to "remember" or "know" that a word had been studied when the word given as a response was not in the study list. The probability of false subjective recollection was .04, .08, and .11 for congruent, incongruent, and guessing items, respectively. As in Experiment 2, estimates of subjective recollection were corrected for general willingness to say "remember" or "know" as indexed by subjective reports on guessing trials. Adjusting correct subjective recollection by using the false subjective recollection for guessing items produced a value that did not significantly differ from the objective measure of recollection derived from the process-dissociation procedure (M = .72 and .72). Furthermore, the correlation between these two measures of recollection remained very high at $r = .89$, which was reliable at the .001 level.

False recall for congruent and incongruent items was conditionalized on incorrect responses. Comparing conditionalized false subjective recollection for congruent and incongruent tests showed no difference between the two types of test ($F < 1$). The conditionalized probability of false subjective recollection, collapsed across congruent and incongruent trials, was much higher than false subjective recollection for the guessing trials (.36 vs. .11), $F(1, 23) = 41.29$, $MSE = .019$. This difference replicates results found in Experiment 2 and can be interpreted as showing that correct recognition of the cue, which was possible on congruent and incongruent trials but not on guessing trials, served as a basis for subjective recollection.

Our finding in Experiment 2 that older participants were less likely to say "recall" when producing a studied word converges with their being less likely to say "remember" on tests of recognition memory (e.g., Parkin & Walter, 1992). However, our definition of a "know" judgment (Experiment 3) differs from the definition used in the remember/know procedure. In our procedure, "know" judgments cannot be identified with familiarity. All of the words produced as responses were familiar because of their presentation during prior training and in earlier study lists. Both "remember" and "know" judgments were meant to express recollection and were differentiated only with regard to the recollection of supporting details. Consequently, we did not expect dissociations involving "remember" and "know" judgments, and the pattern of findings for "remember" judgments was generally the same as that for "know" judgments.

The process-dissociation approach treats recollection and habit as being alternative bases for responding. Doing so gives reason to expect correspondence between objective and subjective measures of recollection. However, our measure of subjective recollection relies on a procedure that is similar to the remember/know procedure. Interpretation of results from the remember/know procedure has been challenged by advocates of signal-detection theory (Donaldson, 1996; Hirshman & Master, 1997; Inoue & Bellezza, 1998). Rather than treating remembering and knowing as qualitatively different states of awareness, the signal-detection theory account posits that the two types of judgment both rely on the strength of a unitary trace. A judgment of "remember" is said to reflect only the use of a criterion that is higher than that for a "know" judgment (for a summary of rebuttals to these arguments, see Gardiner & Richardson-Klavehn, 2000).

Similarly, it might be argued that in our experiments, participants' subjective reports of "remember" only reflected high trace strength. However, details of our results can be used to counter that suggestion. The probability of correctly producing a previously studied word was higher on congruent than on incongruent tests, and that difference can be described as reflecting a difference in strength produced by the larger number of presentations of the target word during prior training for congruent tests. However, the probability of subjective recollection was not significantly higher for congruent than for incongruent tests in Experiment 2. The advantage for congruent tests was small in Experiment 3, and it reversed when conditionalized probabilities were compared. Those findings suggest that subjective reports of recollection did not simply reflect overall strength or familiarity.

General Discussion

The results of the present experiments, which used a variant of the process-dissociation procedure, demonstrated that the contributions of habit and recollection can be dissociated. Manipulating prior training influenced estimates of habit but left estimated recollection unchanged (Experiment 1). Conversely, divided attention during study (Experiment 1), age (Experiment 2), and increased study time (Experiment 2 vs. Experiment 3) produced opposite dissociations by influencing recollection but leaving habit unchanged. Estimates of habit showed probability matching by being near the training probabilities, and their validity was supported by convergence with probabilities of guessing. Estimates of recollection were highly correlated with, and nearly identical to, subjective reports of recollection. The convergence between objective and subjective measures of recollection provided converging evidence of the validity of the estimation procedure used to objectively measure recollection, as well as providing external validation for the subjective reports of remembering.

Convergence of Subjective and Objective Measures of Recollection

How do people know that they are recollecting when they are doing so? There has recently been a great deal of interest in memory illusions. Participants report feelings of "pastness" or recollection even when memory for a particular event could not have been retrieved (e.g., Jacoby et al., 1989; Roediger, 1996). Against this backdrop of illusions of remembering, it is striking that subjective reports of memory had such high validity in the present experiments.

The possibility of illusions of memory motivated the attribution view of remembering forwarded by Jacoby et al. (1989; see also Johnson, Hashtroudi, & Lindsay, 1993; Koriat, 2000). An attribution account has also been used to explain the feeling-of-knowing phenomenon and the tip-of-the-tongue experience (for a review, see Schwartz, 1999). Recognition of a cue or question can be misattributed to memory for the to-be-remembered item and result in a false feeling of knowing (e.g., Connor, Balota, & Neely, 1992; Reder, 1987; Reder & Ritter, 1992). Cue recognition can also serve as the basis for the tip-of-the-tongue experience (Metcalfe, Schwartz, & Joaquim, 1993). Similarly, results of the present experiments show that cue recognition contributes to the subjective experience of recollection. False subjective recollection was more likely when the test cue was present during study (congruent and incongruent trials) than when it was not in the study list (guessing trials), and this was particularly true for younger, as compared with older, participants (Experiment 2).

We expect to find manipulations that will produce dissociations between objective and subjective measures of recollection, and doing so is important for specifying the bases for subjective experience (see Koriat & Goldsmith, 1996, for a general discussion of the relation between accuracy and confidence). However, if subjective experience often serves to guide subsequent behavior (e.g., Jacoby et al., 1989), high concordance between what people experience and how they behave should be common. Even demonstrations of false remembering, such as those using the Deese/Roediger and McDermott paradigm (Roediger & McDermott, 1995), can be interpreted as showing concordance between subjective and objective recollection. In this paradigm, participants who study a list of associatively related words show dramatic false memory for the highly related, nonpresented critical lure (e.g., *sleep*). As Roediger and McDermott pointed out, participants may in some sense be remembering the occurrence of the critical item. That is, if presentation of the list arouses or activates the item that is later falsely remembered, the failure is akin to a failure of reality monitoring. Participants may correctly remember the occurrence of the item but fail to attribute it to their private thoughts. Similarly, in our experiments, participants' false remembering might reflect their remembering the prior study presentation of a pair but misattribute its occurrence to the immediately preceding list rather than to an earlier list.

Findings of false subjective experience help to identify factors that often contribute to valid subjective experience. The importance of cue recognition as a basis for valid subjective experience of recollection is revealed by its influence on false recollection. Stated more generally, the strategy of using false memory to diagnose bases for remembering is the same as using visual illusions to diagnose bases for perceiving (Jacoby et al., 1989; Roediger, 1996). Although findings of false memory are useful, we suspect that, in general, subjective experience (remembering) will accord with past events.

Habit as Accessibility Bias: The Relation Between Process-Dissociation and a Two-High-Threshold Model

The equations used by the process-dissociation procedure to estimate habit and recollection are the same as those used by a two-high-threshold model to separate discriminability and bias effects. That model underlies the common use of hits minus false alarms as a measure of recognition memory performance, corrected for effects of bias. Snodgrass and Corwin (1988) described the theoretical characteristics of the two-high-threshold model and used the model to examine discriminability and bias effects on recognition memory performance (also see Macmillan & Creelman, 1991).

Although the equations are the same, the process-dissociation approach rests on a two-process model and uses the equations to separate the contributions of automatic and controlled processes (Jacoby, Toth, & Yonelinas, 1993, pp. 150–151). In contrast, it is common to use hits minus false alarms as an easily computed measure of "true" memory, corrected for guessing, without reporting a bias index. This is done because interest has typically been focused on differences in discrimination, not bias. For us, "guessing" reflects automatic influences of memory such as those of habit and is at least as interesting as differences in discrimination. Guessing or bias reflects memory, as does discriminability, but the two reflect qualitatively different forms or uses of memory.

For our "habit" experiments, both words that were falsely recalled (false alarms) and words that were correctly recalled (hits) had been presented earlier. The difference is defined by whether they were presented in the study list that immediately preceded the test. Recollection (discriminability) measures memory for the particular presentation of a pair, whereas habit (bias) reflects memory for pairings in the experimental setting as a whole, particularly training. Returning again to the example of parked cars, recollection measures memory for where you parked your car today, whereas bias measures memory for where you usually park your car. We refer to habit as "accessibility bias" to distinguish between general willingness to respond and willingness to make a particular response. When recollection fails, people produce the response that is most accessible, and this accessibility bias reflects habit.

The parallel between the process-dissociation equations and the two-high-threshold model highlights the importance of findings of invariance. Snodgrass and Corwin (1988) demonstrated the utility of the two-high-threshold model by showing that its estimate of discriminability remained invariant across levels of a manipulation that produced a large effect on bias, and estimated bias remained invariant across levels of a manipulation that produced a large effect on discriminability. As they discussed, the measures would not be useful if, for example, estimated bias changed radically across levels of discriminability. Similarly, our results demonstrate the utility of the process-dissociation approach by showing invariance in estimated habit across manipulations that influenced estimated recollection and vice versa. When the results from Experiments 2 and 3 were combined (Table 2), estimated recollection ranged from .29 (older participants in Experiment 1) to .72 (younger participants in Experiment 2). Treating guessing trials as reflecting zero recollection extends that range to 0–.72. Across that wider range of estimated recollection, estimated habit ranged from .67 to .62; this range is reduced to .62 to .63 if guessing trials are ignored. Despite the wide use of hits minus false alarms as an index of discriminability, very few experiments have shown such selectivity of effects on the discriminability and bias parameters of the two-high-threshold model. To our knowledge, our experiments are the only ones that have done so for memory by manipulating training.

Ratcliff and McKoon (1995, 1997) argued that implicit memory effects are actually bias effects and proposed a counter model of such effects. We (e.g., Jacoby, Toth, & Yonelinas, 1993) agree that automatic influences of memory (implicit memory) can be expressed as bias. Jacoby, McElree, and Trainham (1999) provided a discussion of similarities and differences between the counter model and the process-dissociation approach, and showed that results reported as support for the counter model by Ratcliff and McKoon (1997), when reanalyzed, reveal striking dissociations that are similar to those found by Hay and Jacoby (1996).

Proactive Interference: A Comparison of Models

Proactive interference has traditionally been investigated separately from facilitative effects of prior experience, such as learning. Performance in the experimental group for proactive interference (*A-B, A-D*) and for learning (*A-D, A-D*) are each compared to performance in a neutral control condition (rest, *A-D*) to measure interference and facilitation, respectively. In contrast, we combined facilitation (in-concert) and interference (opposition) conditions to separate the contributions of recollection and habit, which is the same as distinguishing between discriminability and bias. Doing so allowed us to demonstrate that, in our experiments, proactive interference reflected only an influence of accessibility bias (habit) and had no effect on discriminability (recollection).

The Ratio Rule and Bases for Responding

M. C. Anderson and Neely (1996, p. 249) described the strength dependence of interference in terms of a ratio-rule equation (Luce, 1959), illustrating its use with an example: "p(recall *rock*, given *dog*) = Strength(*dog-rock*)/Strength(*dog-rock*) + Strength(*dog-sky*) . . . Strength(*dog*-Nth item)." They point out that the ratio-rule equation can be found in the relative strength retrieval assumptions that are adopted by theories meant to explain interference effects (e.g., J. R. Anderson, 1983; Raaijmakers & Shiffrin, 1981). By that equation, as the associations between competing items becomes stronger, the probability of recalling the target word from a particular associative pair decreases because of the increase in the denominator.

The ratio-rule equation provides an excellent description of the effects of habit (bias). Both the present experiments and those done by Hay and Jacoby (1996, 1999) showed that estimates of habit and performance on guessing trials closely approximated training probabilities, just as would be expected if the ratio-rule equation held. However, effects of bias were independent of the ability to recollect the target word. Bias (i.e., the ratio-rule equation) was important only when recollection failed. Recollection can be described as relying on cues that differ from those that are relied on by habit. Recollection relies on retrieval cues that are *not* shared by competitors. For recollection, retrieval is highly constrained by the participant's focus on the immediately preceding study list along with other cues used to access memory for presentation of the target item in that list (e.g., "knee" *contextualized* in the particular study list). If that attempt at highly constrained retrieval is unsuccessful, more general cues that are shared by another response are used to produce a response (e.g., "knee" contextualized in the experiment as a whole), and the ratio rule holds.

The process-dissociation approach separates the contributions of habit and recollection at a macroscopic level but is consistent with more micro-level theorizing. McClelland, McNaughton, and O'Reilly (1995) distinguished between a fast-learning, hippocampal memory system that is responsible for recollection and a slow-learning, neocortical system. Norman, O'Reilly, and Huber (2000) provided a model of hippocampal and neocortical memory systems and showed that results from their model were consistent with dissociations between recollection and automatic influences of memory. That is, hippocampal (recollection) and neocortical (habit) memory systems can serve as independent bases for responding. Responding on the basis of habit (accessibility bias) is akin to categorization or classification in reflecting a more general basis for responding. Nosofsky (1988) showed that independence of classification performance and recognition memory performance could result even if both relied on the same memory for exemplars. The key assumption in his model is that the two types of task differ both in computation of similarity between a probe and exemplars as well as in the decision rule used. Similarly, recollection and habit could be described as relying on the same memory for exemplars but as reflecting the use of different cues and/or decision rules.

Is Responding on the Basis of Habit "Automatic"? Recollection Deficit Versus Inhibition Deficit as a Cause of Proactive Interference

Automaticity has been defined as a form of responding that is fast, requires little effort, and does not require either awareness or intent (e.g., Posner & Snyder, 1975). However, these criteria are seldom all satisfied (Bargh, 1989; Jacoby, Ste-Marie, & Toth, 1993; Neumann, 1984). Responding on the basis of habit, manipulated by prior training, is fast. Requiring participants to respond rapidly reduces estimated recollection but leaves estimated habit unchanged (Hay & Jacoby, 1996). It is less certain that the criteria concerned with awareness and intent are satisfied in the current paradigm. Returning to the parked car example, one might sometimes be aware of one's failure to recollect where the car was most recently parked, remember where the car is usually parked, and intentionally go to that remembered, usual location. However, knowledge of a usual parking location is unlikely to always be used strategically—with awareness that a location is a usual one and intention to use memory for where one usually parks. Such strategic use of memory requires time and attention. When rushed or distracted, mistakenly returning to the usual location seems more likely to reflect habit, a use of memory that is unaware and unintentional. Because fast responding leaves estimated habit unchanged (Hay & Jacoby, 1996), we do not believe our participants strategically used memory gained from training as a basis for guessing.

However, the automaticity of habit in our experiments likely differs from the automaticity observed in Stroop tasks. For Stroop tasks (Stroop, 1935), participants are to name the color of the ink in which the names of colors are printed. Interference occurs when the ink color and color name are incongruent (e.g., *red* printed in green ink) as compared with a control condition in which color patches are presented. Note that the conditions parallel those traditionally used to examine proactive interference in that an interference condition is compared with a control condition. Responding to color patches serves the same role for measuring Stroop interference as does the rest control condition for measuring proactive interference. Experiments investigating Stroop interference usually also include a facilitation condition in which the ink color and color name are congruent (e.g., *red* printed in red ink). That is, the conditions are the same as used in our habit experiments to separate effects of discriminability and accessibility bias.

Although the conditions in our experiments are the same as those used in Stroop tasks, we doubt that our participants had to inhibit or suppress the habitual response, whereas the inhibition or suppression of word reading does seem necessary for Stroop tasks. In our experiments, habit served as a form of bias that was important only when recollection failed, rather than as a source of preponderant responses that had to be inhibited or suppressed. If so, then proactive interference does not always reflect a failure to inhibit or suppress a preponderant response. A difference of this sort between proactive interference and Stroop interference is important both for theories of proactive interference and for theorizing about inhibition.

Hasher and Zacks (1988) suggested that larger Stroop interference effects shown by older adults provide evidence of a deficit in inhibitory processes. However, even if older participants do show larger Stroop interference effects (for a meta-analysis, see Verhaeghen & De Meersman, 1998), Stroop interference may not have the same cause as proactive interference. Greater susceptibility to proactive interference may not always be caused by a deficit in the ability to inhibit or suppress a preponderant response (cf., Hasher & Zacks, 1988; Warrington & Weiskrantz, 1970).

We cast the inhibition-deficit account into a form that is the same as a model used by Lindsay and Jacoby (1994) to describe performance in Stroop tasks, and compared the fit of that model to the fit of the recollection–habit model. The Stroop model has been used to reveal dissociations that are analogous to those found for habit and recollection. As described above, the standard paradigm for investigating Stroop interference, when congruent as well as incongruent tests are included, is the same as used by the process-dissociation approach. However, investigations of Stroop interference have typically examined response times rather than accuracy and have measured interference by comparisons with a neutral control condition. In contrast, we (Jacoby et al., 1999; Lindsay & Jacoby, 1994) have analyzed accuracy of performance in Stroop tasks in a way that is analogous to separating effects of bias from those of discriminability. Lindsay and Jacoby showed that increasing the proportion of congruent trials selectively increased the contribution of word reading, leaving the contribution of color-naming processes unchanged. In contrast, manipulating the colors in which words were presented selectively influenced the contribution of color-naming processes. Those results can be interpreted as showing that the effect of word reading, an automatic process, was a bias effect just as was the effect of habit in the current experiments. However, the Stroop bias effect is very different from accessibility bias. In the Stroop model, the automatic process (word reading) has to be inhibited or suppressed to allow the controlled process (color naming) to contribute to performance. Additional description of the Stroop model along with its comparison to the recollection–habit model appears in the Appendix.

The recollection–habit model clearly fit the data from our experiments much better than the inhibition (Stroop) model. Consequently, we conclude that the automaticity of habit, as manipulated in our experiments, is different from the automaticity of word reading in Stroop tasks. Proactive interference in our experiments did not reflect a failure to inhibit or suppress a preponderant response as may occur in Stroop tasks (cf. Hasher & Zacks, 1988; Warrington & Weiskrantz, 1970). Rather, by the recollection–habit model, proactive interference reflected an influence of habit that occurred when recollection failed. The greater susceptibility of the older participants to interference effects (Experiment 2) was caused by a deficit in recollection rather than a deficit in the ability to inhibit a preponderant response. Estimates of habit did not differ between younger and older participants, but older participants were less likely to recollect.

Rather than accepting our conclusions, one could argue that our models do not adequately capture the inhibition involved in proactive interference and in Stroop interference. If so, the challenge is to devise a model that not only fits the data from our habit experiments as well as does our recollection–habit model but also fits data from Stroop experiments as well as does our Stroop model. Meeting that challenge will require further clarification of the notion of inhibition. Attempting to devise a formal model is a means of gaining such clarification (cf. Burke, 1997; McDowd, 1997; see also the response to those articles by Zacks & Hasher, 1997). Simple models that emphasize differences between deficits in inhibition and deficits in recollection, such as those described in this article, serve as a better starting point than more complex models that would obscure such differences.

The difference between the recollection-deficit and the inhibition-deficit view has practical as well as theoretical implications. The two views dictate different practices for rehabilitating memory performance. Steps to rehabilitate recollection would be very different from those that would be taken to rehabilitate the ability to inhibit a preponderant response. As an example, Hay and Jacoby (1999) showed that manipulations that are akin to encouraging elaboration or deeper processing (e.g., Craik & Lockhart, 1972) are effective for enhancing older participants' ability to recollect. The effectiveness of those manipulations is not likely due to enhanced ability to inhibit a preponderant response. As another example, it is important to determine whether providing environmental support has its facilitative effects by enhancing recollection (discriminability) or by means of automatic influences of memory such as accessibility bias (e.g., Jacoby, 1994).

Concluding Comments

Stroop interference, proactive interference, bias effects, and subjective reports of memory are typically treated in separate chapters in textbooks. The experiments reported in this article were aimed at integrating these supposedly disparate topics. Questions about proactive interference are closely related to questions about automaticity and bias effects. Although the standard proactive interference paradigm is not the same as procedures for separating effects on bias from those on discriminability, we believe that it should be. The advantage of the process-dissociation procedure is that it separates the contributions of controlled processes (discriminability) from automatic influences (accessibility bias). Comparisons of processes underlying proactive interference and Stroop interference helped to clarify notions of automaticity and inhibition. The automaticity that is responsible for interference in Stroop tasks can differ from the automaticity that is responsible for proactive interference.

The convergence of objective and subjective measures of recollection serves both to provide support for our objective measure of recollection and to validate subjective reports of remembering. Neither our objective measure of recollection nor subjective reports of recollection were influenced by training. That is, estimates of recollection were not influenced by proactive interference. It is striking that recollection was so robust. In contrast, demonstrations of false memory (e.g., Roediger & McDermott, 1995) might be interpreted as showing that memory is fragile. Findings of false memory are useful for diagnosing the bases for the subjective experience of remembering. However, we believe that, in general, remembering is valid—in accord with past events.

Proactive interference is *sometimes* caused by an effect of accessibility bias. We suspect that, at other times, proactive interference occurs because of an effect on remembering (recollection). Also proactive interference may sometimes occur because of an inability to inhibit a preponderant response, as in Stroop interference tasks. What is needed is specification of the factors that determine whether the recollection–habit model, the inhibition (Stroop) model, or some combination of the models applies in a particular situation. What are the important differences between the Stroop situation and the situation we used to investigate proactive interference?

Although we cannot yet answer questions about differences between Stroop and proactive interference, we believe such questions are best answered by examining facilitation effects in combination with interference effects. Doing so encourages comparisons of proactive and Stroop interference. Answering questions about the relation between the two types of interference is important for theories of proactive interference as well as for theories of inhibition, and is even more important for better specifying the nature of the memory deficits shown by some older adults.

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Appendix

Recollection Deficit Versus Inhibition Deficit as an Account of Proactive Interference

We reanalyzed the results from our experiments by casting the process-dissociation estimation procedure into the form of a recollection–habit, multinomial model whose fit was compared with that of a multinomial version of the Stroop model used by Lindsay and Jacoby (1994). All multinomial analyses were performed with the Microsoft Excel Solver function (Dodson, Prinzmetal, & Shimamura, 1998). The Solver function was used to gain values of goodness-of-fit, G^2, which were compared against the chi-square distribution. Because of the large number of observations, alpha was set to .001 for all multinomial analyses. This value of alpha was chosen to avoid rejecting a model because of slight deviations from the observed data. For all analyses, the power to detect even a "small effect" (Cohen, 1988) was greater than .95.

Recollection/Habit Model

Rather than gaining estimates of recollection (discriminability) and habit (bias) for each participant as done by the process-dissociation estimation procedure, a multinomial model, as typically used, provides estimates only for a group of participants (e.g., Batchelder & Riefer, 1999). This is disadvantageous for some purposes. As an example, use of a multinomial model to gain group estimates would not have allowed us to examine the correlation between objective and subjective measures of recollection at the level of individuals. However, use of multinomial models does allow one to choose between alternative models by comparing their respective fits.

The recollection–habit model is the same as used by the process-dissociation procedure (the top panel of Figure A1). To fit that model to results from Experiment 1, the habit parameter for the 50/50 items was set to a constant at .50 for the full- and divided-attention conditions and for guessing trials. This was done because we expected the equal number of prior exposures to the two alternatives to result in their being equally likely to be given as a response when participants were unable to recollect. Habit in the 75/25 condition was a free parameter but was restricted to be the same for full- and divided-attention conditions and for guessing trials. Recollection was allowed to differ for the full- and divided attention conditions but was restricted to be the same for the 75/25 and 50/50 conditions. Recollection was set at zero for guessing trials. This model,

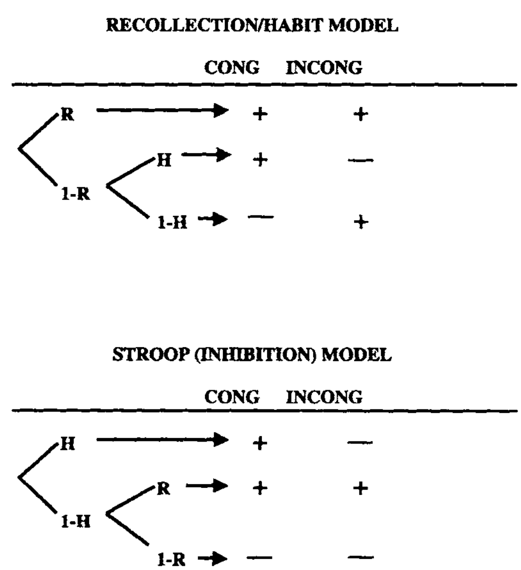

Figure A1. Models and correct (+) and incorrect (−) responses for congruent (CONG) and incongruent (INCONG) tests.

(*Appendix continues*)

using two recollections and one habit as parameters that were allowed to vary, fit the data extremely well, $G^2 = 10.80$ compared to a critical value of $\chi^2(9, N = 11{,}520) = 27.88$. That is, results from the multinomial analysis were the same as from the process-dissociation estimation procedure.

In a second analysis, the data from congruent, incongruent, and guessing trials in Experiment 2 and Experiment 3 were fit simultaneously using three recollection parameters: one for older participants; one for younger participants, short study duration (Experiment 2); and one for younger participants, long study duration (Experiment 3). Only one habit parameter was used. Habit was restricted to be the same across the three conditions (older and younger participants at two study durations) and guessing trials. Recollection was set at zero for guessing trials. These data were fit very well, $G^2 = 6.91$ compared to a critical value of $\chi^2(5, N = 9{,}792) = 20.51$. Again, the conclusions that can be drawn are the same as were drawn from results of the process-dissociation analysis: Age and study time influenced estimated recollection but left estimated habit unchanged, and estimates of habit converged with performance on guessing trials. Parameter estimates were nearly identical to those gained from the process-dissociation estimation procedure. Jacoby (1998, 1999) found that results from a multinomial analysis were in full agreement with those gained from the process-dissociation estimation procedure, just as reported in the present study.

Inhibition (Stroop) Model

The inhibition model is also a dual-process model, with habit and recollection as independent processes. However, by the inhibition model, habit is the dominant process: People respond by habit, but if habit is inhibited recollection is engaged. That is, the positions of habit and recollection are reversed for the inhibition model compared with the recollection–habit model (the bottom panel of Figure A1). This reversal of positions reflects the assumption that habit is "involuntary" and "uncontrolled" just as is word reading in Stroop tasks—serving as a source of preponderant responses. *H* refers to the probability of an uncontrolled or preponderant response based on habit. Successful inhibition reduces H. When a preponderant response is inhibited, (1 − H), responding is determined solely by recollection. Successful inhibition in combination with recollection, (1 − H)R, produces correct recall for both congruent and incongruent tests. In contrast, successful inhibition, in combination with a failure of recollection, (1 − H)(1 − R), produces an error for both types of test. H serves as a guessing parameter for the recollection/habit model but does not do so for the Stroop model. Again, the roles of R and H are reversed for the two models. Successful inhibition (1 − H) only gives an opportunity for recollection and, otherwise, does not influence responding.

The model is the same as used by Lindsay and Jacoby (1994; also see Jacoby et al., 1999; Trainham, Lindsay, & Jacoby, 1997) to fit performance in the Stroop task. For the Stroop task, the probability of a correct response in the congruent condition, in which the color name and word color are the same, is equal to the probability of mistakenly reading the word (W) plus the probability of successfully inhibiting word reading (1 − W) and correctly naming the ink color (C): W + (1 − W)C. On incongruent trials, the word color and color name are different (e.g., *red* printed in green ink) and, so, reading the word produces an error. To respond correctly, it is necessary to both inhibit reading the word and name the ink color: (1 − W)C. For the model used here, H has the same role as W in the Stroop model. Both refer to the probability of a preponderant response that reflects prior learning and has not been successfully inhibited. To respond correctly on incongruent trials, it is necessary to both inhibit production of the habitual response and recollect the word that was presented during study: (1 − H)R. *R* has the same role as does C and refers to a form of consciously controlled processing that is independent of that underlying H just as C is assumed to be independent of W.

To fit results from Experiment 1, the H parameter was set at a value of zero for the 50/50 training condition regardless of whether attention was full or divided during study. This was done because preponderant responses should not occur after the alternative responses were presented equally often, and parallels setting H at a value of .50 for the 50/50 condition when testing the recollection–habit model. When guessing trials were excluded from the analysis, restricting parameters in the inhibition model in the same manner as done for the recollection–habit model caused the fit for the inhibition model to be quite poor, $G^2 = 48.42$ compared to a critical value of $\chi^2(5, N = 9{,}216) = 20.51$. The fit was much poorer when guessing trials were included in the analysis. Even with six parameters (different Rs for each of the four combinations of full vs. divided attention and prior training, and different Hs for full and divided attention in the 75% conditions), the inhibition model failed to fit when the guessing trials were added into the analysis, $G^2 = 41.17$ compared with a critical value of $\chi^2(6, N = 11{,}520) = 22.46$. The inhibition model was fit to the congruent, incongruent, and guessing trials of Experiments 2 and 3. The model was restricted to have the same number of parameters as used for fitting the recollection–habit model to the same data. The inhibition model did not come close to fitting, $G^2 = 75.59$ compared with a critical value of $\chi^2(5, N = 9{,}792) = 20.51$.

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