Journal of Experimental Psychology: General
2001, Vol. 130, No. 3, 544–570

0096-3445/01/$5.00 DOI: 10.1037//0096-3445.130.3.544

Forgetting Our Facts: The Role of Inhibitory Processes in the Loss of Propositional Knowledge

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Seven experiments are reported that show that retrieving facts from long-term memory is accomplished, in part, by inhibitory processes that suppress interfering facts. When asked to repeatedly retrieve a recently learned proposition (e.g., recalling *The actor is looking at the tulip*, given cues such as *Actor looking t__*), subjects experienced a recall deficit for related facts (e.g., *The actor is looking at the violin*) on a recall test administered 15 min later. Importantly, this retrieval-induced forgetting was shown to generalize to other facts in which the inhibited concepts took part (e.g., *The teacher is lifting the violin*), replicating a finding observed by M. C. Anderson and B. A. Spellman (1995) with categorical stimuli. These findings suggest a critical role for suppression in models of propositional retrieval and implicate the mere retrieval of what we know as a source of forgetting of factual knowledge.

Memory is never more conspicuous than when it fails. One cannot help but wonder how the events of our past fade so completely; or how a once well-mastered idea deteriorates into confusion and misunderstanding; or how the name of a friend whom we have known for years eludes us, even if only momentarily. Such failures abound in daily experience, sometimes with great consequence. In this article, we examine the mechanisms of forgetting, with special concern for the forgetting of factual information from long-term memory. We show that people forget facts, in part, because of the retrieval process itself. When facts about a person, topic, or idea are recalled on a regular basis, people grow more likely to forget related but unused facts. On the basis of both previous research and seven new experiments, we argue that such forgetting is produced by an inhibitory process that serves to overcome the interference that related facts cause during retrieval.

Historically, theories of memory have used passive factors to explain forgetting. One such factor is *retrieval competition*. When one recalls something—an event, a fact, or an idea—a variety of related memories become activated. These compete with the target memory for access to conscious awareness. The more related memories there are, the less likely will be the retrieval of the target (Watkins, 1978). This idea dates back to the work of McGeoch (1942), who proposed competition as the primary mechanism of forgetting. Modern incarnations can be found in theories that posit relative strength rules to explain retrieval. These models state that the probability of recalling a memory is determined by its strength, relative to that of competing memories (e.g., J. R. Anderson, 1983; Mensink & Raaijmakers, 1988; Raaijmakers & Shiffrin, 1981; Rundus, 1973). Although the specific assumptions vary across models (see M. C. Anderson & Bjork, 1994, for a review), they each attribute forgetting in part to retrieval competition. Importantly, competition has featured prominently in accounts of forgetting facts (J. R. Anderson, 1983; J. R. Anderson & Reder, 1999a).

Over the last decade, we have questioned the adequacy of retrieval competition as a complete account of interference. Instead, we have emphasized the role of attentional control mechanisms associated with the retrieval process itself, in which competition from related memories creates internal distraction that impairs the selection of the target memory. To minimize this distraction, inhibitory processes suppress competitors (M. C. Anderson, Bjork, & Bjork, 1994; M. C. Anderson & Neely, 1996; M. C. Anderson & Spellman, 1995; for related proposals, see Blaxton & Neely, 1983; Conway & Engle, 1994; Dagenbach, Carr, & Barnhardt, 1990; Hasher & Zacks, 1988; Keele & Neill, 1978; Radvansky, 1999b). Although suppression helps retrieval, its lingering effects impair the subsequent retrieval of previous competitors. By this view, a connection exists between the effort to control cognition in the face of interference and some memory lapses. This emphasis on inhibition parallels proposals in a variety of cognitive domains ranging from visual selective attention (Tipper, 1985), language comprehension (Gernsbacher, 1990, 1991; Gernsbacher & Faust, 1991; Simpson & Kang, 1994), and executive control functions such as task stopping (Logan & Cowan, 1984) and task set switching (Mayr & Keele, 1999). Many studies support the inhibition view, showing negative effects of retrieval on the later recall of related items (e.g., M. C. Anderson et al., 1994; M. C. Anderson & Spellman, 1995; Bauml, 1998; Blaxton & Neely, 1983; Ciranni & Shimamura, 1999; Dagenbach et al., 1990; Roediger & Schmidt, 1980; Shaw, Bjork, & Handal, 1995; A. D. Smith, 1971). This phenomenon is referred to as *retrieval-induced forgetting* (M. C. Anderson et al., 1994).

Although retrieval-induced forgetting has been studied in a variety of settings, little work has focused on the retrieval of factual knowledge. The extension of this phenomenon to propositional memory is important for several reasons. First, if inhibition is a general cause of forgetting, it should affect both simple and complex knowledge. The very use of knowledge in some domain

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The research reported herein was supported in part by start-up funds from the University of Oregon. We would like to thank Bobbie Spellman and Doug Hintzman for comments on a draft of this article.

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should erode competence with related facts that remain unused. Second, theories of fact retrieval have emphasized competition as the sole process of interference by which we forget, positing no role for inhibition (J. R. Anderson, 1983; J. R. Anderson & Reder, 1999a). Demonstrating inhibitory processes in fact retrieval would show that these theories are incomplete. Finally, recent work has suggested that the inhibitory processes shown in retrieval-induced forgetting studies may not play a role in fact retrieval (J. R. Anderson & Reder, 1999a). If this is correct, it places limits on the role of inhibitory processes—limits that need to be better understood. We report experiments that seek to demonstrate long-term retrieval-induced forgetting in propositional memory and to determine whether this forgetting reflects the persisting effects of inhibition.

Inhibitory Processes and the Resolution of Interference

The present experiments build on previous research on retrieval-induced forgetting. In this section, we review this research to show that retrieval-related memory impairments are (a) long lasting and (b) caused by inhibitory processes that resolve interference. We then discuss recent attempts to find evidence for this in fact retrieval.

Inhibitory Processes and Retrieval-Induced Forgetting

The observation that retrieval might cause forgetting was first made in research on output interference (Dong, 1972; Roediger, 1973; Roediger & Schmidt, 1980; A. D. Smith, 1971, 1973; A. D. Smith, D'Agostino, & Reid, 1970; Tulving & Arbuckle, 1963, 1966). This work showed that the probability of recalling a studied item declined with its serial position in a testing sequence. Early research established that this decline was produced not by the loss of items from short-term memory, but by the prior "output" of other studied items (A. D. Smith, 1971). Until recently, however, neither the long-term effects of retrieval-induced impairments nor the role of inhibition had been established. Much of the evidence for these points has been found with the *retrieval practice paradigm* (M. C. Anderson et al., 1994). We discuss this paradigm in detail because it is the basis for the current studies.

In the retrieval practice paradigm, subjects study a set of category-exemplar word pairs (e.g., *Fruit Orange, Fruit Banana, Drinks Scotch*). After this initial study phase, there is a retrieval practice phase, in which subjects retrieve only some of the exemplars they studied, based on category–letter-stem cues (e.g., *Fruit Or__*). The aim of the retrieval practice phase is to have subjects repeatedly retrieve some exemplars, so that the long-term effects of this practice can be assessed for the unpracticed exemplars of the practiced categories. After retrieval practice, there is a 20-min delay, and then subjects are tested on their ability to recall all of the exemplars, given the category names as cues. People are, of course, better at recalling the practiced items (e.g., *Fruit Orange*). More interesting is the fact that retrieval practice impairs the remaining unpracticed exemplars of practiced categories (e.g., *Fruit Banana*), relative to performance on items from baseline categories that were studied initially but not given retrieval practice (e.g., *Drinks Scotch*). Importantly, this impairment is long lasting; unpracticed items are recalled more poorly even when recall is assessed with a category–letter-stem cuing procedure (e.g., *Fruit B__*) to ensure that unpracticed items are tested before their practiced category mates. M. C. Anderson et al. (1994) argued that this impairment reflects the effects of an inhibitory process that suppressed competitors during retrieval practice 20 min earlier.

Although consistent with an inhibitory process, retrieval-induced forgetting can be explained in other ways (see M. C. Anderson & Bjork, 1994, for a review). For example, many models of memory predict that strengthening the association between a retrieval cue and a practiced item (e.g., between *Fruit* and *Orange*) should block later retrieval access to other associates (e.g., *Fruit Banana;* J. R. Anderson, 1983; Mensink & Raaijmakers, 1988; Raaijmakers & Shiffrin, 1981; Rundus, 1973). These models reflect a long research tradition on associative competition as a source of interference (McGeoch, 1942; see M. C. Anderson & Bjork, 1994; M. C. Anderson & Neely, 1996, for reviews). Associative blocking may be thought of in terms of "tip of the tongue" experiences in which we forget a word or a name, presumably due to persistent intrusions of a highly accessible similar name (Baddeley, 1982; Jones, 1989; Reason & Lucas, 1984; Woodworth, 1938). Such models need not posit inhibition to explain impairment of unpracticed items but only that the strengthened alternatives are hyperfacilitated and that their persistent (and covert) intrusion frustrates efforts to recall the unpracticed items. Such blocking would occur after a 20-min delay provided that practiced items remain facilitated.

Several properties of this phenomenon make this account unlikely and strongly favor the operation of inhibition. First, retrieval practice does not always produce retrieval-induced forgetting; rather, impairment occurs when competitors are likely to interfere during retrieval practice. In all three experiments reported by M. C. Anderson et al. (1994), high-taxonomic-frequency items (e.g., *Fruit Banana*) suffered retrieval-induced forgetting, whereas low-frequency exemplars typically suffered none. This occurred despite significant (and equivalent) strengthening of the practiced items in the two conditions. Thus, strengthening practiced items, by itself, does not cause retrieval-induced forgetting, as would be predicted by blocking theories; instead, impairment depends on the strength of the unpracticed items. When subjects perform retrieval practice, other exemplars become activated in proportion to how strongly they are related to the category cue (e.g., the competitor *Banana* becomes more activated than *Guava*). If interference from strong items slows retrieval, inhibitory processes suppress them (e.g., *Banana*), ultimately impairing the ability to recall them. Similar dependencies of retrieval-induced forgetting on competitor interference have been found in other paradigms (e.g., output interference; see Bauml, 1998) and with other materials (e.g., with homographs; see M. C. Anderson & Shivde, 2000).

An inhibitory interpretation is favored also by studies contrasting different types of practice. For instance, Ciranni and Shimamura (1999) adapted the retrieval practice procedure to novel visuospatial representations. They found that retrieval practice on some attributes of an object (e.g., its color or shape) impaired the recall of attributes of related objects, such as their color, shape, or location. However, when retrieval practice was replaced by repeated study trials, the later recall of related objects was unimpaired. This occurred even though both methods of extra practice strengthened items to the same degree. Ciranni and Shimamura concluded that retrieval-induced forgetting is caused by a recall-specific mechanism and that recall places greater demands on the need to resolve retrieval interference (see also M. C. Anderson,

Bjork, & Bjork, 2000; Blaxton & Neely, 1983, for evidence for recall-specific retrieval-induced forgetting in semantic memory). Strengthening also has failed to impair related competitors in other paradigms, including retroactive interference (Bauml, 1996), proactive interference (DaPolito, 1966), and the list-strength paradigm (Bauml, 1997), provided that sources of retrieval-induced forgetting are eliminated from the strengthening and testing procedures (see M. C. Anderson, Bjork, & Bjork, 1994, 2000, for discussion). Thus, strengthening practiced items by itself does not cause these inhibitory phenomena.

In the preceding studies, blocking theories predicted forgetting when none was found. One might object that the extra-study manipulations were not strong enough to cause competing items to be blocked, despite the (apparently) successful efforts to match the degree of strengthening across retrieval practice and extra-study conditions. A different approach to distinguishing these theories is to seek evidence for forgetting where associative competition theories predict there should be none. For example, these theories predict that strengthening category-exemplar associations (e.g., *Red Blood*) through retrieval practice should make it more difficult to recall related exemplars (e.g., *Red Tomato*) when their shared category (*Red*) label is given as a test cue. But if the related exemplar (*Tomato*) is tested with an independent cue (e.g., *Food*), associative interference from practicing *Red Blood* should be circumvented, and no impairment observed. However, if retrieval practice suppresses competitors, related items such as *Tomato* should be less accessible, regardless of whether they are tested with the retrieval practice cue (*Red*) or with an independent cue (*Food*). Thus, retrieval-induced forgetting should be cue independent. M. C. Anderson and Spellman (1995) confirmed this across a series of five experiments (see M. C. Anderson, Green, & McCulloch, 2000, for a related finding). Furthermore, when retrieval practice is replaced with extra-study trials, competitors tested from an independent test cue are no longer inhibited (M. C. Anderson & Shivde, 1999, 2000). No inhibition is evident, even after 20 extra-study trials on practiced items (M. C. Anderson & Shivde, 2000) and even though practiced items are strengthened to the same degree as items in a retrieval practice condition that shows cue-independent forgetting.

The preceding studies suggest that retrieval interference is overcome by inhibitory mechanisms that suppress competing items, causing a persisting deficit in the recall of the inhibited material. These deficits have now been observed with a variety of materials, ranging from taxonomic categories (M. C. Anderson et al., 1994; M. C. Anderson et al., 2000; M. C. Anderson & McCulloch, 1999; Blaxton & Neely, 1983; Hartinger & Bauml, in press; R. E. Smith & Hunt, 2000), ambiguous words (M. C. Anderson & Shivde, 2000), visuospatial representations (Ciranni & Shimamura, 1999), and even complex eyewitness events (Koutstaal, Schacter, Johnson, & Galluccio, 1999; Shaw et al., 1995). These recall deficits persist well beyond the retrieval events by which they are induced and generalize to a variety of cues used to test the affected memories. If inhibition is a general process of retrieval and if it produces persisting deficits in recall, retrieval might also cause forgetting of factual knowledge.

Inhibitory Processes in Propositional Memory

Given the variety of materials for which retrieval-induced forgetting has been found, it seems likely that inhibition plays a role in fact retrieval as well. Interference must be overcome in fact retrieval just as in other retrieval situations. For instance, to verify that we have encountered "Napoleon ruled France," we must retrieve the fact from long-term memory amidst competition from other facts we know about each of the concepts—facts about Napoleon, France, and perhaps even about "ruling." As the number of facts known about each concept increases, people grow slower at verifying the truth of a particular one: a finding known as the *fan effect* (J. R. Anderson, 1974). Given the role of inhibition in the retrieval of other materials types, the extension to fact retrieval seems straightforward.

Although this seems reasonable, some models of propositional memory have not posited a role for inhibition in fact retrieval. The tendency for related facts to interfere with one another has been explained as a competition for limited activational resources at the time a retrieval cue is presented (e.g., J. R. Anderson, 1983; J. R. Anderson & Reder, 1999a). For example, when the fact "Napoleon ruled France" is presented for verification, the concepts in the probe are each assumed to receive a fixed amount of activation. This activation is then divided among all of the facts associated with the concept. If the concept "Napoleon" is in two facts, its activation is divided between them according to their relative strengths of association to that concept. As more Napoleon facts are learned, the limited activation given to that concept during later test probes would be divided among an even larger set. Thus, learning additional facts about a concept impedes retrieval because the probe cues' activation is spread to associated facts more thinly. No inhibitory process resolves interference among competitors. Because this model can explain many findings about fact retrieval, the case for inhibition must be established.

Although there have been many studies of fan interference, only three experiments have addressed the role of inhibition in fact retrieval. J. R. Anderson and Reder (1999a) recently examined whether inhibition contributes to the fan effect. Subjects studied sentences of the form *The [person] is in the [place]* such as *The banker is in the park, The banker is in the office,* and *The fireman is in the office.* The facts were structured to assess whether repeatedly retrieving a fact impairs the recognition of competing facts and whether this impairment was cue independent. For example, would repeatedly recognizing *The banker is in the park* impair recognition of direct competitors that shared a concept like *The banker is in the office* (an *interference* fact) as well as facts that used concepts of a direct competitor, but not those used in practiced facts (e.g., *The fireman is in the office;* referred to as a suppression fact). J. R. Anderson and Reder argued that according to the inhibition view, retrieving *The banker is in the park* would suppress concepts in the interference fact *The banker is in the office,* impairing subjects' recognition of *The fireman is in the office;* however, if inhibition was not involved, as suggested by the ACT models (e.g., J. R. Anderson & Reder, 1999a), only interference items would be impaired. To test this, subjects studied 48 sentences and then received test-feedback trials until they could recall all the facts. They were then given a speeded *yes–no* recognition test in which they saw either studied facts or distractors made by recombining parts of studied facts, such as *The fireman is in the park.* Subjects saw certain to-be-practiced facts repeatedly in this test phase (five times in each block). To determine whether the repeated recognition tests *(The banker is in the park)* impaired memory for both interference *(The banker is in the office)* and suppression facts *(The fireman is in the office),* response times for

the latter items were compared with those observed for control facts that did not share concepts with practiced or interference facts but were about the same topic as the suppression facts (e.g., *The fireman is in the lobby*). Consistent with the noninhibitory view, repeating practiced items impaired recognition for interference facts (1,580 ms) relative to control items (1,479 ms), but did not impair suppression facts (1,471 ms).

J. R. Anderson and Reder's (1999a) findings suggest that inhibition may not play a role in fact retrieval. However, as they noted, there are many differences between their procedure and the methods used to study retrieval-induced forgetting. Many of these differences could have reduced the ability to find inhibition. In the fan effect paradigm, subjects were highly trained on the study facts, whereas in the retrieval practice paradigm, subjects are typically presented items once, for 5 s. Overtraining is likely to encourage integration of the facts, a process known to eliminate retrieval-induced forgetting with other materials (see, e.g., M. C. Anderson & McCulloch, 1999; R. E. Smith & Hunt, 2000; for similar findings in fact retrieval studies, see, e.g., Myers, O'Brien, Balota, & Toyofuku, 1984; Radvansky & Zacks, 1991). Also in the fan effect paradigm, subjects received recognition practice, whereas the retrieval practice paradigm uses recall practice. Using recognition practice may have reduced impairment by reducing the need to resolve competition during retrieval. For instance, prior studies contrasting the effects of recall practice and repeated study trials on the later recall of competitors (e.g., M. C. Anderson et al., 2000; Ciranni & Shimamura, 1999) have found impairment to be selectively induced by recall practice, which requires greater effort to resolve interference. J. R. Anderson and Reder's (1999a) procedure randomly intermixed test trials on the to-be-practiced items with test trials on test items (and with test trials on distractors that reexposed to-be-inhibited concepts) rather than doing all of the practice first and assessing inhibition afterwards. Inhibition may be released by interleaved reexposure to inhibited items. Any of these differences may have reduced the possibility to observe inhibition.

A more sensitive procedure for detecting inhibition was introduced in a fan effect experiment by Radvansky (1999a). Like J. R. Anderson and Reder (1999a), Radvansky (1999a) examined whether the retrieval of a fact such as *The potted palm is in the hotel* would impair not only direct competitors such as *The potted palm is in the library* but also related facts that used concepts of the competitor (e.g., *The welcome mat is in the library*—a suppression item). Radvansky's (1999a) procedure was similar to J. R. Anderson and Reder's (1999a), except during the recognition test when inhibition was both induced and measured. Radvansky (1999a) suggested that J. R. Anderson and Reder's (1999a) test procedure may have allowed too much time between the recognition trials intended to induce and then measure inhibition and also that inhibition may have been released by mixing suppression and practice trials with distractors that included concepts shared with the suppression facts.

To address this, Radvansky (1999a) altered the test sequence so that recognition trials intended to induce inhibition (e.g., a trial on an item such as *The potted palm is in the hotel;* referred to as a *prime* trial) was followed immediately by a test of the related fact thought to be inhibited (e.g., an item such as *The welcome mat is in the library;* referred to as a *probe* trial), analogous to procedures often used in studies of negative priming (Tipper, 1985). On control trials, probe items were unrelated to either the prime item or its competitors (e.g., the unrelated prime might have been *The ashtray is in the airport*). Radvansky (1999a) found cue-independent impairment: Subjects were slower to recognize probe facts in the suppression condition (1,595 ms) than in the control condition (1,470 ms). Importantly, the degree of slowing on probe trials increased with the amount of fan interference a given subject showed during the prime trials. On the basis of these findings, Radvansky (1999a) argued not only that inhibition occurs in the fan procedure but also that it is part of the fan effect itself: Adding new facts should increase the number of facts inhibiting one another, slowing retrieval of any one item.

Radvansky's (1999a) findings strongly support a role for inhibition in fact retrieval. However, if Radvansky (1999a) is correct about the crucial differences between his and J. R. Anderson and Reder's (1999a) procedures, inhibitory effects on facts may be short-lived. This conclusion contrasts with evidence showing that retrieval-induced forgetting can be long lasting. The discrepancy between Radvansky's (1999a) suggestion and previous work on retrieval-induced forgetting may arise from procedural differences and not from the nature of the stimuli. A study by Macrae and MacLeod (1999) is consistent with this possibility. Subjects studied 10 geography facts about each of two fictitious islands ("Tok" and "Bilu"; e.g., *The official language of Tok is French* or *Bilu's only major export is copper*). After studying each fact once, subjects went on to the remaining phases of the retrieval practice procedure. For one island, subjects practiced retrieving 5 of its 10 facts. On each practice trial, subjects received cues such as *Bilu's only major export is c__* and were asked to recall the relevant fact. A final test followed 5 min after retrieval practice, cued by the name of each island (e.g., *Tok* or *Bilu*). Macrae and MacLeod found that practice facilitated recall of the practiced facts (M = 70%) over baseline facts about the unpracticed island (M = 38%) but impaired the related but unpracticed facts (M = 23%). Thus, using the retrieval practice paradigm, Macrae and MacLeod found retrieval-induced forgetting for facts that appeared to last 5 min. Like Radvansky (1999a), these authors argued that inhibition plays a role in fact retrieval.

Inhibition may have caused Macrae and MacLeod's (1999) findings, but they can be explained by noninhibitory theories as well. Because their procedure did not test the impaired facts from an independent probe, we cannot be sure of the role of inhibition. The stronger practiced facts may have blocked access to the related facts or diverted activational resources from unpracticed facts when their shared cue (e.g., *Tok*) was presented. If inhibition did cause the impairment, it is unclear when it took place. Inhibition may have occurred during practice and persisted over the 5-min delay, or the inhibition induced during practice may have dissipated, consistent with Radvansky's (1999a) suggestion, but was then reinstated during the final test by the retrieval of practiced facts. Thus, Macrae and MacLeod's finding does not specifically favor inhibition, nor does it clearly resolve how enduring inhibition might be.

The Present Studies

The preceding experiments suggest that the inhibitory processes at work in studies of retrieval-induced forgetting may also contribute to propositional retrieval. However, it is unclear whether inhibition causes enduring deficits in the recall of facts. An additional concern is that J. R. Anderson and Reder (1999a) did not find cue-independent impairment using a procedure similar to that

of Radvansky (1999a). Although Radvansky offered several explanations for this discrepancy, one might have concern on general grounds about the strength of the evidence favoring inhibition.

In the present studies, our aim was to show that retrieving facts from long-term memory leads to persisting deficits in the recall of related facts and to examine whether these deficits are caused by inhibition. Our strategy was to use a different paradigm from the ones used by J. R. Anderson and Reder and Radvansky, to permit comparisons with work on retrieval-induced forgetting. First, we report studies that adapt the retrieval practice procedure of M. C. Anderson et al. (1994) for use with facts. In these experiments, we sought to (a) replicate propositional retrieval-induced forgetting, (b) show that impairment lasts well beyond the practice phase during which it is induced, and (c) rule out biases in output interference during the delayed test. We also explored the scope of impairment: What types of materials it affects, as well as its boundary conditions. These experiments thus examine the importance of retrieval processes to long-term forgetting but do not specify the mechanism involved.

Next (Experiments 4a, 4b, and 5), we examine whether the impairment is caused by inhibition. These studies adapted the independent-probe method of M. C. Anderson and Spellman (1995) for use with facts to test for cue-independent forgetting. If our findings converge with those of Radvansky (1999a), the case for inhibitory processes in fact retrieval is strengthened. If inhibition is found at long retention intervals, it would highlight the importance of inhibition in the long-term forgetting of factual knowledge.

General Method: Experiments 1–3

In the current experiments, subjects encoded six facts about each of eight topics. The topics served a role analogous to that of the categories in previous retrieval practice designs. For example, subjects encoded six sentences about the topic "the actor" such as *The actor is playing the guitar* and *The actor is playing the oboe.* In the next phase, subjects practiced retrieving some of the facts for some of the topics. For each practiced fact (e.g., *The actor is playing the guitar*), subjects were given the topic, the relation, and a letter stem as cues (e.g., *actor playing gu__*) and asked to recall the sentence. Similar to prior work, subjects did retrieval practice on three facts about each practiced topic, three times each. After retrieval practice and a 15-min delay, subjects received a sentence frame for each topic (e.g., *The actor is playing the* __) and were asked to recall the sentences they remembered about that topic, in any order. (In later experiments, they also received the first letter of each sentence ending, to control output order.) If practice on some facts about a topic (hereinafter known as *practiced* items) causes retrieval-induced forgetting, long-term recall of unpracticed facts about that same topic (hereinafter referred to as *shared-topic* items) should be impaired.

Retrieval-induced forgetting may not be limited to facts that share a topic with the practiced items. This is because retrieving a fact often entails using both a topic and a relation as cues. For instance, when subjects practice retrieving items such as *actor playing gu__*, they are likely to use both the topic (*actor*) and the relation (*playing*) as cues. If so, other facts that contain the same relation might compete, even if they do not share a topic with the target fact. Does retrieval-induced forgetting occur for such facts? For instance, would practicing the retrieval of *The actor is playing the guitar* impair facts such as *The teacher is playing the saxophone* because the two share *playing?* This question is interesting because knowledge retrieval is often driven by both a concept and a relation. To look for relation-based impairment, we made pairs of topics for which all facts used the same relation. For example, if *the actor* and *the teacher* were topics in a related topic pair, subjects would study not only facts about the actor, such as *The actor is playing the guitar* and *The actor is playing the oboe* but also facts about the teacher, such as *The teacher is playing the saxophone.* Constructing topic pairs allowed us to study the effects of retrieval practice on facts that shared a relation with practiced items; hereinafter referred to as *shared-relation* items. Impairment of shared-topic and shared-relation items was measured by comparing performance on those items with the recall of baseline facts in identically structured topic pairs that were not practiced (and that used a different relation; see Figure 1 for a detailed explanation).

Although our main aim was to study long-term propositional retrieval-induced forgetting, we were also interested in characterizing the boundary conditions of this phenomenon. Prior work using the retrieval practice procedure has shown that when subjects integrated the exemplars of a category, retrieval practice no longer impaired related items (M. C. Anderson & McCulloch, 1999; R. E. Smith & Hunt, 2000; see also M. C. Anderson, Green, & McCulloch, 2000). These effects occur both when subjects are asked to integrate items and also when they choose to do so on their own as a study strategy (M. C. Anderson & McCulloch, 1999). Similar integration benefits have been found in studies using the fan effect procedure (McCloskey & Bigler, 1980; Myers et al., 1984; Radvansky, Spieler, & Zacks, 1993; Radvansky & Zacks, 1991; E. E. Smith, Adams, & Schorr, 1978; see Radvansky, 1999b, for a recent review, and J. R. Anderson & Reder, 1999a, 1999b, for another perspective). For example, Radvansky (1999b) found that when subjects could integrate facts about a topic into a situation model, interference was greatly reduced. Moreover, when facts were integrated, the inhibition of related facts disappeared, as measured by the independent-probe method.

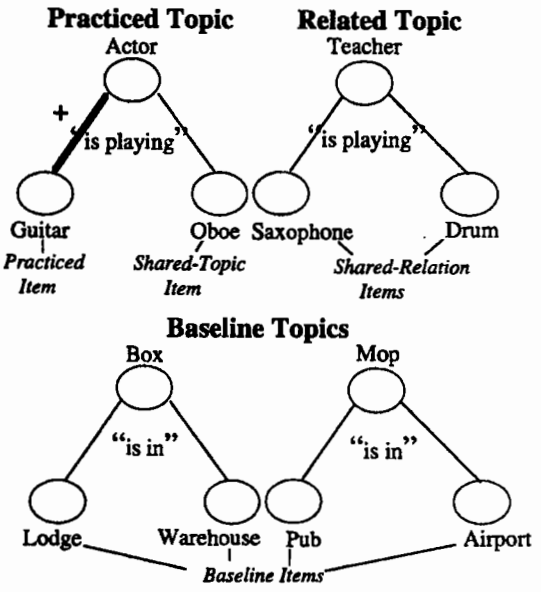

Figure 1. Design and example materials for Experiments 1 and 3. The top half of the figure depicts two topics, *Actor* and *Teacher,* for which the propositions use the same verb, "is playing," making them a related pair of topics. Of interest in this design is the effect of doing retrieval practice on half of the items from a practiced topic (e.g., *actor is playing guitar;* a practiced item, as indicated by the darkened line) on the remaining unpracticed facts about that topic (e.g., *oboe,* or shared-topic item) and on the unpracticed facts about the related topic that shares the same relation (*teacher*). Performance on each of these item sets is compared against performance on facts from a baseline pair of topics (*Box* and *Mop*) that are themselves related, but by a different semantic relation. Using a different semantic relation in the baseline topics removes contamination of recall performance by the repeated practice on the relation "is playing." Experiment 2 uses the same design but different materials.

Radvansky's (1999a) findings suggest that integration benefits will be observed in the current studies. To test this idea, all of our experiments included a postexperimental questionnaire that asked subjects how often they integrated the propositions concerning each topic. If integration reduces the need for inhibition, as suggested by M. C. Anderson and McCulloch's (1999) and Radvansky's (1999b) findings, subjects who report a higher degree of integration should show less retrieval-induced forgetting than subjects reporting less integration. If integration reduces impairment, it would suggest that representations composed of richly interconnected concepts might be especially resistant to retrieval-induced forgetting.

Experiment 1

Experiment 1 had three main goals. First, we sought to replicate the finding of propositional retrieval-induced forgetting using new materials and procedures. Unlike Macrae and MacLeod's (1999) study, the facts about a topic all shared a relation (e.g., *playing*), a practice common in fan effect studies. If we find retrieval-induced forgetting, shared-topic items should be more poorly recalled than baseline items.

Our second goal was to explore whether retrieval-practice impairs the recall of facts that share a semantic relation with the practiced fact and not a topic. If so, shared-relation items should be recalled more poorly than baseline facts on our final test. Finally, we examined whether subject-initiated integration might reduce retrieval-induced forgetting. To study this, we asked subjects about integration-based study strategies, using their responses to identify high and low integrators. If integration reduces propositional retrieval-induced forgetting, less impairment should be found for high- than for low-integration subjects.

Experiments 1a and 1b differed only in how the facts were presented. In Experiment 1a, the facts were presented once, for 8 s; whereas in Experiment 1b, there were two passes through the materials, with 5 s given on each pass. This difference was exploratory. We were concerned that a single pass might not allow enough time to encode the facts effectively, but multiple exposures might increase integration, thereby reducing retrieval-induced forgetting. The two experiments identified the procedure that would yield the best combination of performance level and impairment.

Method

Subjects

In Experiments 1a and 1b, 37 and 69 University of Oregon undergraduates participated to fulfill a course requirement, respectively. All subjects were native English speakers or had spoken English for at least 10 years. In each experiment, 5 subjects were excluded because they had failed to recall a minimum of one proposition for each topic during retrieval practice. In Experiment 1b, five were replaced for the same reason.

Design

The retrieval practice status of an item was manipulated within-subjects. Propositions were either (a) given retrieval practice (practiced items), (b) unpracticed but shared the same topic and relation as practiced items (shared-topic items), (c) unpracticed but shared only the same relation as practiced items (shared-relation items), or (d) unpracticed and unrelated to any practiced propositions (baseline items). For example, if *The actor is playing the guitar* were a practiced item, example items from other conditions would be *The actor is playing the oboe* (shared topic), *The teacher is playing the saxophone* (shared relation), *The box is in the lodge* (baseline), and *The mop is in the airport* (baseline). The dependent measure was the percentage of facts recalled in each condition on a sentence-stem cued-recall test.

Materials

Experiments 1a and 1b used the same materials and procedure, except as noted.

Construction of propositions. Forty-eight facts were used that had the form *The noun is [verb] or [preposition] the object* (e.g., *The actor is playing the guitar* or *The box is in the lodge*). These facts were grouped into topic sets, related topic pairs, and replications, as described next (see Appendix A for a complete listing of facts).

Topic sets. The materials were constructed in sets of six facts sharing a topic noun (e.g., *the actor*), which we refer to as a *topic set.* All of the sentences in a topic set also shared the same relation (e.g., *is playing*) but had a different object ending the sentence. The objects for a topic were from the same category (this was changed in Experiment 2; the categories were from Battig & Montague, 1969, and McEvoy & Nelson, 1982), and each object began with a two-letter stem that was unique with respect to all experimental words. The objects were all less than nine letters long. An effort was made to minimize preexisting associations among (a) the objects within each topic set, (b) the objects in one topic and those in others, (c) the relation for each topic and the objects of other topics (except for the related condition; see next section), and (d) the topics and the objects from other topics. This was achieved using association norms (Nelson, McEvoy, & Schreiber, 1994) and experimenter judgment. To minimize guessing during recall tasks, facts were made to be not highly predictable (e.g., we didn't use items such as "The bird ate the worm"). Four topic sets were made. Each set had a different topic, a different relation (e.g., *is playing, is crawling on, is in,* and *is made of*), and a different category (e.g., musical instruments, furniture, buildings, and metal) of objects. Within each six-item topic set, facts were randomly assigned to two subsets of three, Subsets a and b. These subsets allowed us to counterbalance which facts were practiced but were otherwise indistinguishable. Finally, filler topics with filler facts were also made. These had no associations with experimental facts, according to association norms.

Related pairs. For each topic set, a second topic set was made to share both the relation and the category with the original. For instance, for the topic set *The actor is playing,* we made the related topic set *The teacher is playing* with new instruments that did not occur for the topic *actor.* We refer to such topic pairs and their respective facts as a *related* topic pair. Because there were initially four topic sets, this yielded four related topic pairs. The construction of related topic pairs allowed us to measure whether practicing the retrieval of facts from one topic might impair the recall of facts studied under another topic, if they shared the same relation.

Replications. The four related topic pairs were assigned to two subsets of two related topic pairs. Each subset implemented all of the conditions in the experiment and was called a *replication.* Within each replication, one of the related topic pairs would receive retrieval practice, with the second serving as a baseline. Within the related pair that was practiced, only one topic set would be practiced, and only three of its six facts, with the remaining unpracticed facts serving in the shared-topic and shared-relation conditions (see Figure 1).

Study order. The study order consisted of the 48 experimental and 12 filler facts, presented 1 at a time. Facts from the same topic or from related pairs of topics did not appear successively, and the first and last 3 facts were fillers to control for primacy and recency effects. Regular sequences of topics were avoided. To control for serial position effects, the average study position of every subset of 3 items from a topic (the aforementioned Sets a and b) was equated. Filler facts were interspersed to help achieve these constraints. Two versions of this study order were created, with different facts assigned to positions in each version.

Retrieval practice books. Each retrieval practice book contained 44 trials. Each trial displayed the topic, the relation, and the first two letters of the object from one of the facts, followed by a dash (e.g., *actor playing*

gu__ for *The actor is playing the guitar*). The trials for a given booklet were on facts from two experimental topics that were drawn from two different replications. For instance, if the topics *Actor, Teacher, Box,* and *Mop* were in one replication and *Ant, Spider, Wire,* and *Sculpture* were in the other, participants might practice facts from the *Actor* and *Ant* sets. For each practiced topic, three of its six facts were practiced three times each.

The order of practice trials for each fact conformed to an expanding schedule in which the first and second test pages for an item had an average of 3.2 intervening tests on other items and the second and third test pages had an average of 7.3 intervening tests. To reduce the formation of associations among facts, facts from the same topic were not tested successively, using filler items to satisfy these constraints. Recurring sequences of facts were prohibited.

To ensure that all facts were practiced equally often across subjects, three counterbalancing measures were implemented. First, for a given topic (e.g., *Actor*), the three facts from each topic subset were each practiced by half of the subjects. Second, each topic in a related pair (e.g., *Actor, Teacher*) was practiced by half the subjects. Finally, in each replication, half the subjects practiced a topic from one related pair (e.g., *Actor, Teacher*), and the other half practiced a topic from the other (e.g., *Box, Mop*). These measures yielded eight retrieval practice groups.

Final test books. Each test booklet had nine pages. The first page tested a filler topic, to familiarize subjects with the procedure. Each remaining page presented a cue for one of the eight experimental topics. Cues for each topic consisted of the first part of a fact for that topic followed by a blank (e.g., *The actor is playing the __*). Topics from related sets were never tested adjacently. In addition, the practiced topic was tested before its related topic for one of the two related pairs that were practiced and after it for the other. Topics were ordered so that the average positions of the practiced, related, and baseline topic conditions were equated. Using these constraints, two test orders were constructed. These test orders counterbalanced which specific topic in a related pair appeared first in the test sequence, allowing us to equate the serial position of specific topics across subjects.

Questionnaire. Subjects were asked to rate how often they intentionally practiced sentences or sentence endings together to interrelate them during study. Each topic was rated on a 5-point Likert scale, ranging from 1 (*none of the time*) to 5 (*all of the time*).

Distractor task. This consisted of a packet with visual-pattern-completion problems.

Procedure

Experiment 1 was done in four phases: study, retrieval practice, distractor, and cued recall. During study, subjects were randomly assigned to one of the two study versions. Subjects in Experiment 1a saw the sentences for 8 s each. They were asked to study the sentences so that they could recall the ending, if provided with the topic and the verb. In Experiment 1b, subjects were told that they would have a second chance to study the sentences. During each pass, the pages were studied for 5 s, for a total of 10 s per item.

During retrieval practice, subjects were randomly assigned to one of the eight practice orders. They were given two words from each of the to-be-tested sentences plus the first two letters from another word as cues. Subjects were given 16 s to recall and write down each sentence and were warned that some items would be tested more than once. After practice, subjects performed the distractor task for 15 min.

For the final test, subjects were randomly assigned to one of the two test books. They were told that each page contained the first part of a sentence they had studied and that they would have 30 s to recall and write down as many of the endings as they could remember. After the test phase, the experimenter passed out the questionnaire and read each question aloud. Subjects were allowed 1 min to give their ratings.

Results

All analyses for all experiments included retrieval practice counterbalancing and the degree-of-reported integration as between-group factors.

Assignment of Subjects to Low- and High-Spontaneous-Integration Groups

We made an integration score by averaging each subject's integration ratings across the eight topics (see M. C. Anderson & McCulloch, 1999). Subjects were then rank ordered by their score within each counterbalancing group, with the bottom halves constituting the low-integration group and the top halves constituting the high-integration group. This ensures that the groups are matched on our counterbalancing measures. Appendix B reports the overall ratings as well as the ratings broken out by integration subgroup for all experiments.

Retrieval Practice Performance

The percentage of items correctly recalled during retrieval practice (hereinafter the *practice-success rate*) was in the moderate range (M = 78%). There were no reliable differences in practice-success rate as a function of integration group (except in Experiment 4b). Practice success rates are summarized in Appendix B for all experiments.

Retrieval-induced forgetting. As shown in Table 1, we found the overall facilitation and impairment of final recall performance often observed for practiced items and their direct competitors respectively: Retrieval practice facilitated the long-term recall of the practiced facts, relative to facts in baseline topics, $F(1, 16) = 41.0$, $p < .001$, $MSE = 0.03$, and impaired shared-topic items, $F(1, 16) = 8.59$, $p < .01$, $MSE = 0.020$. These findings replicate the retrieval practice pattern found with other verbal stimuli such as categorized words (M. C. Anderson et al., 1994; M. C. Anderson & McCulloch, 1999; M. C. Anderson & Spellman, 1995; Hartinger & Bauml, in press; R. E. Smith & Hunt,

Table 1
Mean Percentage of Facts Correctly Recalled as a Function of the Retrieval Practice Status of a Fact and the Degree of Reported Integration in Experiment 1

Degree of reported integration	Retrieval practice status of a proposition: Practiced	Shared topic	Shared relation	Baseline
Experiment 1a (n = 32)				
Low integrators	61	22	28	36
High integrators	59	35	34	38
M	60	29	31	37
Experiment 1b (n = 64)				
Low integrators	58	26	32	37
High integrators	65	40	37	40
M	61	33	35	38

Note. Experiments 1a and 1b differed only in that Experiment 1b presented items twice instead of once in the learning phase, for a total exposure of 10 s instead of 8 s. Degree of reported integration = the extent to which participants reported interrelating propositions about a topic in the learning phase.

2000) and visuospatial stimuli (Ciranni & Shimamura, 1999) and confirms the idea that retrieval-induced forgetting extends to factual materials (Macrae & MacLeod, 1999). Experiment 1b also exhibited the predicted facilitation of practiced items, $F(1, 48) = 80.15$, $p < .001$, $MSE = 0.035$, and impairment of shared-topic items, although the latter effect was marginal, $F(1, 48) = 3.72$, $p = .057$, $MSE = 0.033$.

A second aim of this experiment was to assess whether retrieval-induced forgetting would extend to facts that shared only a relation with the practiced fact. Shared-relation items were impaired by retrieval practice, $F(1, 16) = 5.42$, $p < .05$, $MSE = 0.028$, and to a similar degree as items that shared the same topic. Shared-relation items were also impaired in Experiment 1b, $F(1, 48) = 4.52$, $p < .05$, $MSE = 0.028$, although the effect was smaller.

Integration effects. A final question concerned whether integration benefits would be found. Replicating and extending M. C. Anderson & McCulloch (1999), the amount of retrieval-induced forgetting was greater for subjects reporting low integration than for those reporting high integration. Final recall performance was worse for shared-topic than for baseline items for low-integration subjects, $F(1, 16) = 12.32$, $p < .01$, $MSE = 0.02$, but not significantly so for high-integration subjects ($F < 1$). This interaction was marginally significant, $F(1, 16) = 4.13$, $p = .056$. Importantly, facilitation of the practiced items did not differ reliably across the low- and high-integrator groups ($F < 1$). This finding argues that the reduced impairment for high integrators was not caused by diminished strengthening of the practiced items for that group. Similar effects were observed in Experiment 1b: Retrieval-induced forgetting significantly interacted with integration, $F(1, 48) = 5.02$, $p < .05$, $MSE = 0.033$. Final recall performance for low integrators was worse for shared-topic than for baseline items, $F(1, 48) = 8.69$, $p < .01$, $MSE = 0.033$, whereas there was no impairment for high integrators ($F < 1$). As in Experiment 1a, facilitation for the practiced items did not differ reliably between low and high integrators ($F < 1$).

Interestingly, low and high integrators did not differ in the impairment observed for shared-relation items in either Experiment 1a or 1b (both $F < 1$). However, in keeping with the absolute amount of impairment evident in Table 1, shared-relation impairment was reliable for the low-integration group of Experiment 1a, $F(1, 16) = 4.57$, $p < .05$, $MSE = 0.028$, but not for the high-integration group, $F(1, 16) = 1.33$, $p = .26$, $MSE = 0.028$. Similarly, in Experiment 1b, the shared-relation effect was marginally significant for low integrators, $F(1, 48) = 3.42$, $p = .067$, $MSE = 0.028$, but not for high integrators, $F(1, 48) = 1.34$, $p > .25$, $MSE = 0.028$. The small size of the shared-relation effect, with these apparent numerical differences in the size of the effect, suggests that it may be prudent to entertain the hypothesis of a weak integration effect on shared-relation items.

Discussion

Experiments 1a and 1b show that retrieval-induced forgetting extends to propositional information, as suggested by Macrae and MacLeod (1999). As predicted, retrieval practice on a fact impairs the delayed recall of other facts sharing the topic of the practiced fact. This finding is consistent with the idea that the inhibition contributes to forgetting of factual material, although the operation of inhibition cannot be isolated in the present design.

Experiments 1a and 1b converge on two additional findings. First, both show that retrieval practice impairs retrieval of facts sharing a relation with the practiced item, regardless of whether they share a topic. This suggests that retrieval-induced forgetting can be subtle: Retrieving a fact from one topic may lead one to forget about another, ostensibly unrelated topic, if the respective facts share a relation. Whether this shared-relation impairment extends to unrelated sentence objects is unclear, however. This possibility is explored in Experiment 2.

Second, both experiments provide evidence that integration reduces long-term retrieval-induced forgetting for facts, particularly for shared-topic items. This replicates findings observed with categories and ambiguous words and is similar to findings in work on fan interference. The overall smaller amount of impairment in Experiment 1b also suggests that integration plays a greater role when more study time is given, although the reduction may also have been produced by adding study repetitions and not time per se. Integration appears to have a smaller effect in the shared-relation condition, although a small reduction in impairment cannot be ruled out. The existence of these effects is consistent with the notion that integration reduces inhibitory effects.

Relation-based impairment and integration effects highlight important constraints on when inhibition will be found with facts. To see inhibition, the materials cannot be highly integrated, nor should every fact use the same relation. If items are integrated, as is likely with extensively trained sets of facts, inhibition may be reduced, if not eliminated. Even if integration does not eliminate inhibition, using same-relation facts will obscure impairment. If baseline and practiced facts share a relation, little evidence for inhibition will be found. Exactly these conditions (overlearning and same-relation baselines) are found in the fan effect procedure. This may explain why J. R. Anderson and Reder (1999a) failed to find inhibition. This point is considered further in the General Discussion.

Experiment 2

Prior work has shown retrieval-induced forgetting with episodic materials (i.e., no preexperimental associations between the retrieval cues and the memory targets; Ciranni & Shimamura, 1999). The impairment of the novel facts learned in Experiments 1a and 1b is consistent with this. However, all of the facts about a topic had objects that shared a category. Perhaps the observed impairment depends on the preexisting categorical relationships among response items. To test this, we replicated Experiment 1a with new sentence endings that were categorically dissimilar. If retrieval-induced forgetting is tied to categorical materials, this should eliminate the impairment; however, if retrieval-induced forgetting can occur for novel episodic materials, as argued by Ciranni and Shimamura (1999), the impairment should remain.

Using unrelated objects also allowed us to discriminate between two views of relation-based impairment. According to the *relational overlap* view, shared-relation effects arise because the facts of the practiced and related topics share a semantic relation. However, it may be that object similarity between the practiced and shared-relation facts made the latter items vulnerable to impairment. Practicing items like *The actor is playing the guitar* could have impaired *The teacher is playing the saxophone* either because *saxophone* and *guitar* were similar, or because *saxophone* was similar to the other unpracticed objects studied with *actor*

(e.g., *oboe*). If object similarity causes shared-relation impairment, reducing similarity should reduce impairment; if relational overlap causes the impairment, making objects dissimilar should not affect impairment.

Method

Subjects

Seventy-three University of Oregon undergraduates were recruited in the manner described previously. Nine subjects were excluded because they failed to recall at least one fact per topic during retrieval practice.

Design and Materials

The design and materials were the same as those in Experiment 1a, except that the topics, relations, and objects differed (see Appendix C). All of the objects in the experiment belonged to different categories. To maximize diversity, we selected object categories from Battig and Montague (1969) and McEvoy and Nelson (1982). We eliminated categories that were strongly related, such as those sharing a superordinate, and we chose one item from each category. Each item had to begin with a unique two-letter stem and not be associated with other items on the list. Items were assigned to topics with the constraint that resulting facts not be highly distinctive, predictable, or bizarre.

Procedure

The procedure was identical to Experiment 1a except that subjects studied each proposition for 10 s instead of 8 s.

Results

Retrieval-Induced Forgetting

Retrieval practice facilitated the final recall of practiced facts over baseline facts, $F(1, 48) = 45.49, p < .001, MSE = 0.040$, and impaired shared topic items, $F(1, 48) = 9.47, p < .01, MSE = 0.026$. Unlike in Experiments 1a and 1b, shared-relation items were unimpaired ($F < 1$). The latter result suggests that eliminating object similarity reduced shared-relation impairment. To look at this, we contrasted the amount of shared-relation impairment observed in Experiment 2 (unrelated items) to the amount observed in the combined Experiments 1a and 1b (similar items). This interaction was marginally significant, $F(1, 112) = 2.71, p = .09$.

Integration Effects

Low and high integrators differed in the amount of retrieval-induced forgetting they exhibited, as evident in the interaction of impairment (shared topic versus baseline) with integration group, $F(1, 48) = 6.13, p < .05, MSE = 0.021$. As shown in Table 2, low integrators recalled fewer shared-topic than baseline items, $F(1, 48) = 15.42, p < .001, MSE = 0.021$, whereas no such impairment was found for high integrators ($F < 1$). Low and high integrators did not differ reliably in the degree to which practiced items were facilitated ($F < 1$), again showing that differences in shared-topic impairment across these groups were not caused by differential strengthening of practiced items.

Although no shared-relation impairment was found in the overall analysis, integration may have masked it. However, as can be seen in Table 2, the amount of shared-relation impairment was similar for low and high integrators ($F < 1$). Thus, the absence of shared-relation impairment might reflect a genuine reduction in this effect.

Table 2
Mean Percentage of Facts Correctly Recalled as a Function of the Retrieval Practice Status of a Fact and the Degree of Reported Integration in Experiment 2

Degree of reported integration	Retrieval practice status of a proposition			
	Practiced	Shared topic	Shared relation	Baseline
Low integrators	59	30	40	42
High integrators	55	34	34	35
M	57	32	37	38

Note. $n = 64$. Degree of reported integration = the extent to which subjects reported interrelating propositions about a topic in the learning phase.

Discussion

Experiment 2 shows that the retrieval-induced forgetting found in Experiments 1a and 1b is not limited to categorically related stimuli, reinforcing the idea that impairment can occur for purely episodic material (Ciranni & Shimamura, 1999). As in Experiment 1, the effect was modulated by subjects' integration strategies.

Reducing object similarity appears to have eliminated shared-relation impairment, however. When shared-relation items were dissimilar to both the practiced and shared-topic items, impairment was nonsignificant, even though the semantic relations in the current facts overlapped, as they did in Experiments 1a and 1b. This finding suggests that object similarity and not relational overlap produced the shared-relation impairment observed in those experiments. However, caution is warranted because the reduction in the amount of shared-relation impairment relative to Experiments 1 and 2 was only marginally significant. Before accepting this potential reduction in shared-relation impairment, it would be prudent to replicate the shared-relation effect itself, to be confident that it is consistently observed when objects are similar. If the shared-relation effect is genuine, its dependency on object similarity suggests an account in terms of second-order inhibition (M. C. Anderson & Spellman, 1995), as we discuss in the General Discussion.

Experiment 3: Durability of Retrieval-Induced Forgetting

In prior work, retrieval-induced forgetting has been shown to last at least 20 min. Experiments 1 and 2 also appear to show that propositional retrieval-induced forgetting is durable. However, they do not permit conclusions about how long the impairment lasts. As in Macrae and MacLeod's (1999) study, retrieval-induced forgetting may have occurred during the final test. Much work has shown that stronger associates of a cue are recalled before weaker ones (e.g., M. C. Anderson et al., 1994; see also Wixted, Ghadisha, & Vera, 1997). If subjects recalled the practiced items first, shared-topic impairment might reflect increased output interference, relative to baseline items for which recall order was not biased.

To test the output interference hypothesis, we replicated Experiment 1 but altered the final recall test to control for output order.

On the new test, subjects received a sentence stem, with the first letter of the object. For example, for *The actor is playing the guitar,* the cue would be *The actor is playing the g__*. This allowed us to test subjects' recall on all three shared-topic items before their corresponding practiced items, eliminating any output interference. If output interference caused the impairment in Experiments 1 and 2, testing shared-topic items in the first test positions should eliminate retrieval-induced forgetting. Final recall on shared-topic items when tested in the first three positions of a topic should be no worse than recall of baseline items when tested in the same positions. However, if retrieval-induced forgetting reflects the persisting effects of retrieval practice, impairment should remain even though practiced items are tested last.

Our second aim was to explore the effects of different types of retrieval practice. Experiments 1 and 2 used a procedure modeled after M. C. Anderson et al. (1994). Might impairment be tied to this type of practice? Concern over this question is fueled in the present experiment by the change in the final test format. Introducing letter-stem cued recall during the final test (e.g., *The actor is playing the o__*) makes the practice and test recall contexts similar. This increase in contextual similarity might lead subjects to search the practice context for shared-topic and shared-relation items during the final test. Because these items were not presented during practice, their recall might be impaired, lending the appearance of long-lasting inhibition when subjects are actually using an inappropriate contextual cue. To control for this, a second group of subjects (the mismatch condition) received a modified form of practice in which they were given the topic, the relation, and the object (e.g., *Actor playing guitar*) and asked to recollect the precise wording of the sentence they read. This modified task required subjects to recollect their experience but does not rely on stem cued recall. Thus, practice test context match should be reduced. If retrieval-induced forgetting is tied to matching contexts, impairment should occur for the match but not for the mismatch condition.

A third goal of Experiment 3 was to reduce subjects' integration tendencies to improve our measurement of retrieval-induced forgetting. We reduced integration by using an incidental encoding task. Subjects were told that they were participating in an experiment on imagery and judgment and that they would be rating the imagability of the situations described by sentences. They were encouraged to form novel images for each sentence, to prevent them from incidentally integrating facts through interactive imagery. As in previous experiments, we included a questionnaire to assess integration, although the wording was changed to ask about incidental imagery-based integration. Finally, we sought to replicate the shared-relation effect observed in Experiments 1a and 1b, to strengthen the evidence for it.

Method

Subjects

Sixty-six University of Oregon undergraduates were recruited in the manner described previously. Two subjects were excluded for failure to meet a minimum of one fact recalled during the retrieval practice phase per topic practiced.

Design

Two factors were manipulated: the type of practice and the practice status of an item. Half the subjects were given the standard practice method, which matched the letter-stem cued-recall format of the final test (the match condition), and half were given modified practice that did not (the mismatch condition). The practice status of an item was manipulated as in previous experiments.

Materials and Procedure

Materials and procedure were as in Experiment 1a with the following changes.

Construction of facts. The objects (in a related topic pair) were constrained to begin with a unique first letter to ensure that cues on the stem cued-recall test were unambiguous (see Appendix D for a complete listing of facts).

Incidental encoding phase. Subjects were told that they were participating in an experiment on imagery and judgment. They were asked to form an image of the situation described in each sentence and to rate the vividness of that image on a scale ranging from 1 (*not vivid*) to 5 (*extremely vivid*). Subjects were given 8 s for each rating.

Retrieval practice phase. In the mismatch condition, subjects were told that on each trial they would see three words from one of the sentences they had rated and that their task was to recall and write the entire sentence. Each page presented the topic, the relation, and the sentence object (e.g., *Actor Playing Guitar*). The instructions strongly emphasized that subjects recollect the exact wording of the original sentence, to encourage them to recall seeing the sentence. Trials were 16 s long, as in the match condition.

Final test. Each new final test booklet consisted of 54 trials. The first 6 were fillers. Each trial presented a studied fact with the first letter of the object (e.g., *The actor is playing the v__*). Subjects were told that they would have 7 s to recall and write down the sentence ending that fit the letter cue. Items were tested one at a time, and trials were blocked by topic. Shared-topic items (as well as shared-relation and corresponding baseline items) were always tested first within a topic test block to control for output interference. The average position of the practiced, related, and baseline sets was equated. Four separate test orders were created to achieve this counterbalancing.

Results

We first discuss overall recall collapsed over type of retrieval practice and testing position of categories. We then discuss the data broken out by these factors.

Overall Effects of Retrieval-Induced Forgetting

As shown in Table 3, retrieval practice impaired shared-topic items that were tested first, relative to the recall of corresponding baseline facts, $F(1, 48) = 10.27$, $p < .01$, $MSE = 0.060$, and it impaired shared-relation items, $F(1, 48) = 9.33$, $p < .01$, $MSE = 0.075$, relative to that same baseline. These findings suggest that the impairment in Experiments 1a and 1b was not caused solely by the prior output of practiced facts.

As expected, the facilitation of practiced facts over baseline items was significant, $F(1, 48) = 10.28$, $p < .01$, $MSE = 0.097$. One unexpected finding was reduced shared-relation impairment for items tested last relative to baseline items tested last (see Table 3). Shared-relation impairment for items tested in the second position was reliably less than for items in the first position, $F(1, 48) = 4.49$, $p < .05$, $MSE = 0.07$. The reasons for this effect are unclear. The effect appears to arise from greater output interference for baseline items (7%), $F(1, 48) = 11.69$, $p < .01$, $MSE = 0.054$, than for shared-relation items (1% facilitation; $F < 1$). Nevertheless, the shared-relation effect collapsed over test position was reliable, $F(1, 48) = 3.91$, $p = .05$, $MSE = 0.098$.

Table 3
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact, the Practice–Test Context Match, and the Within-Topic Testing Position in Experiment 3

Practice Context/ Test Context Match	Retrieval practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared relation	Baseline
Match	50	49	60
Mismatch	52	52	59
M	51	51	60
	Second three items tested within a topic		
	Practiced	Shared relation	Baseline
Match	66	58	53
Mismatch	61	46	52
M	64	52	53

Note. $n = 64$. The top half of this table reports items that were tested in the first three positions on the final recall test for a topic. Because only unpracticed items were tested in the first positions, only shared-topic, shared-relation, and baseline items were measured. The bottom half of this table reports the items that were tested in the last three positions on the final recall test for a topic. The practice status of an item differs in the top and bottom halves.

Recall Performance by Topic Testing Position

Retrieval-induced forgetting did not interact reliably with topic output position for either the shared-topic condition, $F(1, 48) = 1.56$, $p > .20$, $MSE = 0.059$, or the shared-relation condition ($F < 1$). As suggested by Table 4, shared-topic impairment, $F(1, 48) = 12.13$, $p < .001$, $MSE = 0.049$, and shared-relation impairment, $F(1, 48) = 4.90$, $p < .05$, $MSE = 0.066$, were reliable for the first topic to be tested in a related topic pair. Because these items and their corresponding baselines were tested before every other related item, their impairment rules out cross-topic output interference as a source of retrieval-induced forgetting.

Recall Performance by Type of Retrieval Practice

As Table 3 suggests, neither shared-topic ($F < 1$), nor shared-relation ($F < 1$) impairment interacted with the type of retrieval practice ($F < 1$). Shared-topic impairment was reliable for the match condition, $F(1, 48) = 7.04$, $p = .01$, $MSE = 0.060$, and marginal for the mismatch condition, $F(1, 48) = 3.53$, $p = .06$, $MSE = 0.060$. Shared-relation impairment also was reliable for the match condition, $F(1, 48) = 6.27$, $p < .05$, $MSE = 0.075$, and marginal for the mismatch condition, $F(1, 48) = 3.30$, $p = .07$, $MSE = 0.075$. Practiced item facilitation did not interact reliably with the type of practice ($F < 1$). This suggests that the standard and modified practice produced comparable amounts of retrieval-induced forgetting.

The similarities between these two forms of practice also could be seen in the independence of impairment and topic-testing order. Neither shared-topic nor shared-relation impairment interacted reliably with topic-testing position for either the match or the mismatch condition (all Fs < 1.8). However, one difference between these forms of practice suggested by Table 3 was in the unusual variation in shared-relation impairment with within-topic testing position noted in the section on overall effects of retrieval-induced forgetting. This variation appeared in the match condition, $F(1, 48) = 7.18$, $p = .01$, $MSE = 0.07$, but did not replicate in the mismatch condition ($F < 1$). This difference suggests that the interaction of shared-relation impairment with within-topic test position might not be a consistent feature of this phenomenon. On the whole, these findings argue that both the match and mismatch conditions cause impairment.

Final Recall by Integration Reports

Neither shared-topic nor shared-relation impairment varied with the amount of reported integration (all Fs < 1).

Discussion

Experiment 3 provides evidence that propositional retrieval-induced forgetting is long lasting. Retrieval-induced forgetting was observed even when shared-topic items were tested before practiced items, and even when we restricted the analysis to shared-topic, shared-relation, and baseline items that were tested in the first of the two related topics. This indicates that impairment reflects the lingering effects of the practice phase, and not final test dynamics. This also replicates the shared-relation effects observed in Experiments 1a and 1b (and in 2 between-subjects conditions).

One may be concerned that similarities between the practice and test phases introduced by the new stem-cued-recall test might have led subjects "astray" on the final test. This hypothesis seems unlikely. First, similar amounts of impairment were found for the match and the mismatch conditions, even though the practice format for the latter did not match the final test format. If search

Table 4
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact, the Output Position of a Topic, and the Within-Topic Testing Position in Experiment 3

Output position of a topic	Retrieval practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared relation	Baseline
First topic	50	53	62
Second topic	52	48	57
M	51	51	60
	Second three items tested within a topic		
	Practiced	Shared relation	Baseline
First topic	68	50	53
Second topic	59	53	52
M	64	52	53

Note. $n = 64$. Output position of a topic = whether a topic in a related pair (e.g., actor, teacher) was tested first or second; within-topic testing position refers to whether a proposition was one of the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports data from those items tested in the last half. The practice status of an item differs in the top and bottom halves.

bias were a major factor, there should have been more impairment in the match condition. Second, one may expect more shared-topic than shared-relation impairment according to the search-bias view, because the former items are tested with more cues that were used in the practice phase. Because the amount of impairment was similar for these types of items, the biased search hypothesis seems unlikely.[1]

Experiment 3 yields little evidence that integration affects retrieval-induced forgetting. This may have occurred for several reasons. First, incidental learning may have successfully reduced intentional integration. Also differences between low and high integrators may not have been strong enough to reveal integration effects. In keeping with this, the overall reported incidence of integration (2.04) is lower than in earlier studies (for Experiments 1 and 2, 2.90 and 3.21, respectively). The high-integration group's average ratings (2.60) did not reach 3 (*some of the time*). It is difficult to compare ratings across these studies, however, because the questionnaire for Experiment 3 was about imagery-based integration, whereas the others were not. Second, even if imagery-based integration had been common in this study, it might not have had the same effect as the kind of integration that took place in Experiments 1 and 2. This possibility is addressed in Experiments 4 and 5.

Experiment 4: Inhibitory Processes in Fact Retrieval

The aim of Experiment 4 was to isolate the mechanisms underlying propositional retrieval-induced forgetting. Although inhibition can explain the impairment found in Experiments 1–3, associative interference processes (e.g., J. R. Anderson & Reder, 1999a; Mensink & Raaijmakers, 1988) can as well. Consider the finding that practicing *The actor is playing the guitar* impairs shared-topic items such as *The actor is playing the oboe.* Rather than being inhibited, *oboe* may be recalled less well because the cue *actor* reminds subjects of the more practiced fact *The actor is playing the guitar.* Perhaps the persisting (and covert) intrusion of the stronger item led subjects to give up their search for *oboe,* lending the appearance of inhibition. Alternatively, subjects may be less effective at spreading activation from *actor* to *oboe.* Activation may spread less effectively if (a) the activation available is limited and (b) the greater strength of the practiced facts (*The actor is playing the guitar*) diverts resources from shared-topic items (*The actor is playing the oboe*) when the shared-topic cue is presented (*The actor is playing*). These mechanisms can explain shared-relation impairment as well. When recalling a shared-relation item such as *The teacher is playing the saxophone,* the presence of *playing* may have reminded subjects of the practiced item *guitar,* leading that item to block (or rob resources from) the target.

To determine whether inhibition contributes to propositional retrieval-induced forgetting, we adapted Experiment 1 for use of the independent-probe method (M. C. Anderson & Spellman, 1995). If practicing *The actor is looking at the tulip* suppresses shared-topic items such as *The actor is looking at the violin,* inhibition should also be observable if the object *violin* is tested through an independent fact such as *The teacher is lifting the violin. Violin* should be impaired because suppression affects the competing fact directly, making its concepts less accessible from any cue. This impairment should occur even though the cues used to test *violin* (e.g., *The teacher is lifting the v__*) share neither a topic nor a relation with the strengthened practiced items and so should circumvent associative interference from those items. Propositional retrieval-induced forgetting should be cue-independent.

To test for cue independence, we had subjects encode the object in each shared-topic fact twice: once in the context of the shared-topic fact (e.g., *The actor is looking at the violin*) and once under the related topic with a different relation (e.g., *The teacher is lifting the violin*). We refer to this new type of item as a *shared-object* item, to highlight the fact that it shares a sentence ending with shared-topic items. To measure impairment, performance on shared-topic and shared-object items was contrasted with baseline facts that were also studied twice (shared-object baseline items) but which were studied as part of a related topic pair that did not receive retrieval practice (see Figure 2 for an illustration). If associative interference causes propositional retrieval-induced forgetting (e.g., J. R. Anderson & Reder, 1999a; Mensink & Raaijmakers, 1988), retrieval practice should impair shared-topic but not shared-object items, because the latter were tested with cues that circumvent associative interference. However, if inhibitory processes are at work, retrieval-induced forgetting should be cue independent, that is, both shared-topic and shared-object items should be worse than corresponding baseline facts.

Figure 2 illustrates another change: We made the practiced and shared-topic items less similar to one another. In all the experiments thus far (except Experiment 2), all of the objects associated to a topic were in the same category. In Experiment 4a, we used practiced items that were not from the same category as shared-topic items. For instance, subjects received *The actor is looking at the tulip* instead of *The actor is looking at the guitar* as a to-be-practiced item, making the final object that was practiced dissimilar to the one used in the shared-topic item *The actor is looking at the violin.* Similarly, in the related topic (e.g., *teacher*), the three objects not in the shared-object condition were categorically unrelated to the shared-object items (see Figure 2). This was to determine whether cue-independent forgetting for shared-object items would occur even when those items had no similarity to the practiced items. Experiment 4b went one step further and had every object dissimilar to every other object. If impairment is found, it would show cue-independent forgetting for items that compete with practice targets purely on the basis of episodically formed associations.

Experiment 4a

Method

Subjects

Forty-four University of Oregon undergraduates were recruited as previously described. Four subjects were excluded because they failed to recall a minimum of one fact per topic during retrieval practice.

[1] Although impairment was found in both the match and mismatch conditions, one might argue that the numerically smaller effect for the mismatch condition argues for some role of context match in producing impairment. Note, however, that the retrieval task in our mismatch condition might be somewhat easier than the task in our match condition because the former provided more restrictive retrieval cues. To the extent that there is any reduction in the size of the inhibition effects, it may be due simply to a reduction in the need to resolve interference during the retrieval task.

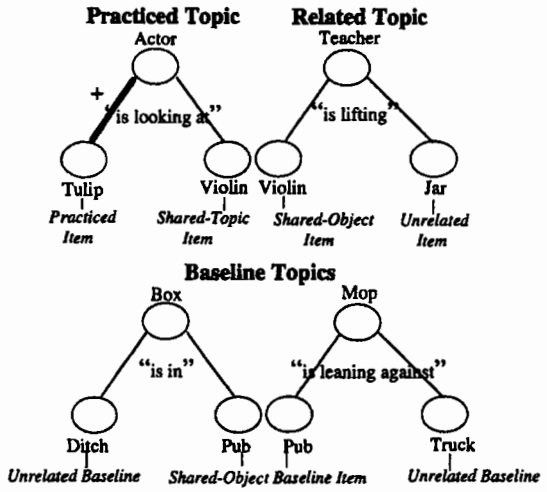

Figure 2. Design and example materials for Experiments 4a and 5. The top half of the figure depicts two topics, *Actor* and *Teacher*, for which the propositions use different relations, "looking" and "lifting." However, half of the sentences in one topic share sentence endings with half of the sentences in the other topic (e.g., *violin*), and half do not (e.g., *tulip* and *jar*). Of interest in this design is how doing retrieval practice on half of the facts about a topic (e.g., *The actor is looking at the tulip;* a practiced item, as indicated by the darkened line) affects recall of the sentence endings from the remaining half of the facts, both when they are tested using the same topic used to do retrieval practice (e.g., *The actor is looking at the v—* for *violin;* the shared-topic condition) and when they are tested with a different topic and relation (e.g., *The teacher is lifting the v—*; the shared-object condition). The bottom half of the figure contains a baseline pair of topics the facts of which are structured identically to those in the top half of the figure, but none of which receive retrieval practice. For each item in the top half of the figure, the corresponding baseline item is positioned directly underneath it. Experiment 4b uses this same design but different materials.

Design

The retrieval practice status of an item was manipulated within-subjects and had six levels. In each practiced topic pair (see Figure 2), there were practiced, shared-topic, shared-object, and unrelated items; in each baseline topic pair, there were shared-object baseline and unrelated baseline items. To illustrate, if *The actor is looking at the tulip* were to be a practiced item, (a) *The actor is looking at the violin* would be a shared-topic item, (b) *The teacher is lifting the violin* would be a shared-object item, (c) *The teacher is lifting the jar* would be an unrelated item, (d) *The box is in the pub* and *The mop is leaning against the pub* would be shared-object baseline items, and (e) *The box is in the ditch* and *The mop is leaning against the truck* would be unrelated baseline items (see Figure 2).

Materials and Procedure

The procedure was the same as in the match condition of Experiment 3, including the use of the incidental learning task. The materials of Experiment 1 were used, except for the following changes.

Construction of facts. As before, four related pairs of topics were used. For each topic, three facts contained objects from the same category, and the remaining three had objects from different categories (see Appendix E). For instance, *Actor* contained three facts that ended with musical instruments and three that did not. The relation used in each fact was the same within a topic, but every topic had a unique relation. In addition, three facts from each topic shared objects with three facts from the related topic. For example, subjects would see *The actor is looking at the violin* as well as *The teacher is lifting the violin.*

Retrieval practice booklets. Only four practice books were made for counterbalancing the particular topics and facts that were practiced. Because the particular set of three items within a topic that was practiced was not counterbalanced, the facts that shared objects with the related topic were never practiced.

Test booklets. Two different test orders were constructed to counterbalance average test position of each pair of related topics, following the format of Experiment 3.

Results

Overall Effects of Retrieval Practice

As can be seen in Table 5, retrieval practice impaired the final recall of shared-topic items relative to the shared-object baseline, $F(1, 44) = 4.05$, $p = .05$, $MSE = 0.114$. Retrieval practice also facilitated the practiced items, relative to their baseline items, $F(1, 44) = 54.58$, $p < .001$, $MSE = 0.074$.

Of main concern was whether the impairment for objects in shared-topic items might generalize to other facts in which those objects took part. As predicted, generalization occurred: Retrieval practice impaired the recall of shared-object items, relative to the shared-object baseline, $F(1, 44) = 4.11$, $p < .05$, $MSE = 0.126$. The amount of shared-topic (8%) and shared-object (9%) impairment was similar, even though the latter facts were tested with a topic and a relation unrelated to the practiced item. This provided strong evidence that retrieval practice inhibited the competing objects.

Effects of Repetition, Testing Position, and Relatedness

A counterintuitive aspect of the findings in Table 5 is that practiced items ($M = 62\%$) are recalled more poorly than the shared-topic ($M = 69\%$) and shared-object ($M = 68\%$) items. One

Table 5
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact, Degree of Reported Integration, and Within-Topic Testing Position in Experiment 4a

Degree of reported integration	Retrieval practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared object	Shared-object baseline
Low integration	64	67	79
High integration	75	70	76
M	69	68	77
	Second three items tested within a topic		
	Practiced	Unrelated	Unrelated baseline
Low integration	61	35	33
High integration	63	37	39
M	62	36	36

Note. $n = 40$. Degree of reported integration = the extent to which subjects reported interrelating propositions about a topic in the study phase; within-topic testing position = whether a proposition was in the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports data from those items tested in the last half. The practice status of an item differs in the top and bottom halves.

expects to see low recall for inhibited items and high recall for practiced items. This puzzle arises from three factors that boosted recall of shared-topic, shared-object, and shared-object baseline items (top half of Table 5) over the practiced, unrelated, and unrelated baseline items (bottom half of Table 5). First, objects in the top half were presented twice at encoding because they had to appear in the shared-topic *(The actor is looking at the violin)* and shared-object *(The teacher is lifting the violin)* conditions. This was not true for bottom-half items (see Figure 2). Second, items in the top half were tested in the first three trials for a topic, to eliminate an output interference account. Bottom-half items were tested in the second three trials, subjecting them to greater output interference. Finally, within each top-half condition, objects were categorically related, a property maintained from Experiment 3 to minimize the number of changes from that procedure. (Note that this relatedness was eliminated in Experiment 4b). This was not true for items in the bottom half. These factors confound comparisons of the top half of Table 5 with the bottom half and have little to do with the hypothesis of interest. For these reasons, the correct comparisons lie within the top and bottom halves in Table 5 (likewise for Experiments 4b and 5).

Recall Performance by Topic Testing Position

As illustrated in Table 6, retrieval-induced forgetting did not interact reliably with topic-output position for either shared-topic ($F < 1$) or shared-object ($F < 1$) conditions. These data replicate Experiment 3 and extend them to shared-object items.

Final Recall by Integration Reports

As shown in Table 5, subjects who did not integrate facts concerning a topic into a single image showed reliable shared-topic impairment compared to baseline items, $F(1, 40) = 6.40$, $p < .05$, $MSE = 0.115$, whereas subjects who did integrate showed little impairment ($F < 1$). However, the interaction of shared-topic impairment and integration level did not reach significance, $F(1, 40) = 2.75$, $p = .11$, $MSE = 0.115$. Nevertheless, this finding is compatible with the integration effects in Experiments 1a, 1b, and 2, and the trend suggested by this result might arise from a lack of statistical power. If real, this effect would suggest that lack of integration benefits in Experiment 3 might have been driven by the lower integration rate in that study (2.04) compared with the current one (2.61). This is addressed in the discussion of Experiment 5.

Integration effects for shared-object items were less clear. Numerically more shared-object impairment was found for low- (12%) than for high-integration subjects (6%). Indeed, shared-object impairment was marginally significant for low integrators, $F(1, 40) = 3.65$, $p = .06$, $MSE = 0.135$, but not for high integrators ($F < 1$). The interaction of shared-object inhibition with integration status was not significant ($F < 1$).

Table 6
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact, the Testing Position of a Topic, and the Within-Topic Testing Position in Experiment 4a

Testing position of a topic	Retrieval practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared object	Shared-object baseline
First topic	66	68	76
Second topic	73	69	79
M	69	68	77
	Second three items tested within a topic		
	Practiced	Unrelated	Unrelated baseline
First topic	62	39	39
Second topic	61	33	34
M	62	36	36

Note. $n = 40$. Testing position of a topic = whether a topic in a related pair (e.g., *Actor, Teacher*) was tested first or second; within-topic testing position = whether a proposition was one of the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports the data from those items tested in the last half. The practice status of an item differs in the top and bottom halves.

Experiment 4b

Experiment 4b replicated Experiment 4a, except that none of the objects were categorically related to ensure that cue-independent forgetting was not dependent on having categorical relationships among the inhibited items. If shared-object impairment depends on categorical relationships, these items should be unimpaired.

Method

Subjects

Forty-two University of Oregon students were recruited as described previously. Two subjects were replaced because they failed to recall a minimum of one fact per topic during retrieval practice.

Design, Materials, and Procedure

These were identical to those in Experiment 4a, except that the items in the shared-topic, shared-object, and baseline conditions that were categorically related in Experiment 4a were replaced with unrelated items. To achieve this, we combined the object sets from Experiments 2 and 4a, in which all the objects were unrelated (see Appendix F).

Results

Retrieval Practice Performance

The practice success rate was 70%. Unlike in prior experiments, practice success rates differed for the low-integration groups (M = 78%) and high-integration groups (M = 62%), $F(1, 32) = 10.66$, $p < .01$, $MSE = 0.023$, for reasons that were unclear. Because of this, comparisons between low- and high-integration groups should be interpreted with concern for differences in these populations.

Final Recall Performance

Experiment 4b replicated Experiment 4a in most respects. As can be seen in Table 7, retrieval practice impaired shared-topic items relative to shared-object baseline items, $F(1, 36) = 10.70$, $p < .01$, $MSE = 0.12$. Crucially, impairment of the objects in

Table 7
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact and the Within-Topic Testing Position of an Item in Experiment 4b

Within-topic testing position of an item	Retrieval practice status of a proposition		
	Shared topic	Shared object	Shared-object baseline
First items in a topic	44	47	59
	Practiced	Unrelated	Unrelated baseline
Second items in a topic	61	33	37

Note. $n = 40$. Within-topic testing position = whether a proposition was in the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. Experiment 4b differed from Experiment 4a only in that the critical to-be-inhibited items were not categorically related to one another or to any other item in the experiment.

shared-topic items generalized to other facts, as can be seen by comparing the recall of shared-object items to the shared-object baseline, $F(1, 36) = 7.69, p < .01, MSE = 0.066$. This shows that the cue-independent forgetting in Experiment 4a does not depend on categorical relationships among the inhibited items. This experiment also yielded further evidence that between-topic output interference is not essential in causing impairment. Retrieval-induced forgetting did not reliably interact with between-topic output position (i.e., whether the practiced or related topic was tested first) for either shared-topic ($F < 1$) or shared-object ($F < 1$) conditions.

The current findings ease any concerns about the fact that practiced items were recalled more poorly than shared-topic and shared-object items in Experiment 4a. As can be seen by comparing Tables 5 and 7 (top halves), eliminating categorical relationships among the items reduced recall in Experiment 4b so that it was again less than that of practiced items. Despite this reduction, shared-topic and shared-object impairment occurred, suggesting that impairment is independent of within-set similarity.[2]

Reported integration again had little effect on the amount of retrieval-induced forgetting. Shared-topic impairment was similar for low (baseline − shared topic = 62 − 45 = 17%) and high integrators (baseline − shared topic = 56 − 43 = 13%), with the interaction of inhibition and integration being nonsignificant ($F < 1$). Shared-object impairment was also similar for the low and high integrators (baseline − shared object = 14% and 10%, respectively), with the difference being nonsignificant ($F < 1$). These patterns resembled those in previous studies, with numerically more impairment for low integrators, but the effects were not robust. As in Experiment 3 (which also did not find integration effects), the integration rate was low ($M = 2.36$).

Discussion

Experiments 4a and 4b replicate most of the major findings of Experiment 3. As in Experiment 3, retrieval practice impaired the recall of shared-topic items. This occurred even though shared-topic items were tested before the practiced items on the final test and even when the related topic was tested after the practiced topic, eliminating all sources of output interference. This confirms that propositional retrieval-induced forgetting reflects persisting effects of retrieval practice.

More crucial is the finding that propositional retrieval-induced forgetting affects shared-object items. Performing retrieval practice on items such as *The actor is looking at the tulip* impaired not only shared-topic items such as *The actor is looking at the violin,* but also shared-object items like *The teacher is lifting the violin,* even though the latter fact tested the affected object with a different topic and relation than were used in retrieval practice. This suggests that propositional retrieval-induced forgetting is cue independent. Because competing items are impaired when associative interference is circumvented, these findings favor the view that competing facts are suppressed. The existence of such a process suggests that the cue-dependent-forgetting accounts (e.g., J. R. Anderson & Reder, 1999a; Mensink & Raaijmakers, 1988) are incomplete as accounts of forgetting.

Two additional features are worth noting. First, unlike in many prior studies, the inhibited items had no semantic similarity to the practiced items. The only basis on which these items (e.g., *Violin*) competed with practiced items (e.g., *Tulip*) was their association to a shared topic and relation (e.g., *Actor*). Although there were semantic relationships within the shared-topic and shared-object conditions in Experiment 4a, even these were eliminated in Experiment 4b, with no change to cue-independent impairment. Thus, the generalization of shared-topic impairment to a second cue may be the first evidence that long-term cue-independent forgetting occurs for materials related on a purely episodic basis. Second, because shared-object inhibition occurred when the topic for those items (e.g., *Teacher*) was tested before the related practiced topic (e.g., *Actor*), our findings argue that cue-independent forgetting lasts at least 15 min. This is particularly strong evidence because our independent test cue eliminates any covert output interference from practiced items that may have affected impairment in the shared-topic condition, despite our efforts to control output order.

Unlike Experiment 3, the present study suggests that imagery-based integration may affect retrieval-induced forgetting in the same way as the integration reported in Experiments 1–2. In Experiment 4a, low integrators showed substantial shared-topic impairment, and high integrators showed virtually none. Shared-object impairment showed a similar but less reliable pattern. In Experiment 4b, integration reduced neither shared-topic nor shared-object impairment, despite a numerical trend toward a reduction. However, the integration rate in Experiment 4b was

[2] The failure of within-set similarity to modulate retrieval-induced forgetting (across Experiments 4a and 4b) is inconsistent with recent findings by M. C. Anderson, Green, and McCulloch (2000). These authors reported that intraset similarity (similarity within the inhibited set) actually increased impairment, an effect that was not found here. However, M. C. Anderson, Green, and McCulloch (2000) manipulated within-set similarity by variations in encoding instructions (similarity-based encoding vs. distinctiveness encoding) and not by manipulating preexperimental similarity of the items. It is unclear why the present manipulation in preexperimental similarity did not yield similar findings. However, comparisons in the absolute amount of inhibition exhibited by different item sets may be influenced by item-specific factors other than within-set similarity. For present purposes, the main point is that shared-object impairment occurred despite an overall drop in recall (due to the elimination of categorical relations), easing any concerns about the unusually high levels of recall observed for critical items in Experiment 4a.

low, perhaps because we used unrelated objects (with incidental learning). Together, these experiments suggest that the integration effect in Experiment 3 was absent not because imagery-based integration has different properties, but because of the lack of integration in that study (see Appendix B). With a low overall integration rate, post hoc divisions into low and high groups may yield high integrators who are not especially high. Thus, the integration effect in Experiment 4a may have arisen because subjects integrated more often, allowing a better high group than was possible in Experiment 3. Before we conclude that imagery-based integration reduces impairment, however, it would be prudent to replicate Experiment 4a. We address this and other goals in Experiment 5.

Experiment 5

As discussed in the introduction, several studies suggest that the inhibition that underlies retrieval-induced forgetting may be engaged by the act of recall. In Experiment 5, we sought to determine whether propositional retrieval-induced forgetting also would exhibit this property or whether impairment might occur for any kind of repeated exposure to the practiced items. To study this, we replicated Experiment 4 with one change. During practice, some of the facts were given retrieval practice, as in the previous experiments. This was the retrieval-practice condition. Mixed with these trials were other facts (about a different topic) that were presented in their entirety (e.g., *The ant is crawling on the rock*). Subjects were told that whenever they saw an intact sentence, they were to study the item for a later test. This was the extra-study condition. If any kind of extra exposure inhibits competing items, impairment should occur regardless of whether subjects perform retrieval practice or extra study. However, if inhibition is tied to recall, retrieval-practice should cause impairment, but extra-study exposures should not.

Whether retrieval practice and extra-study exposures of to-be-practiced items behave differently may depend, of course, on whether subjects engage in some variety of retrieval during the extra-study exposures. To the extent that subjects recognize the item on an extra-study trial as one they have seen before, it is unclear whether retrieval is ever truly eliminated. However, prior studies suggest that there is a large enough difference in the difficulty of retrieval practice and extra study to reveal a difference in impairment. Nevertheless, we were concerned that subjects might engage in additional retrievals, beyond recognizing the re-presented sentence. If these covert retrievals were of other extra-study facts about the same topic, the extra-study condition would cause inhibition similar to that caused by retrieval practice. Thus, measuring the frequency of covert practice may be important to interpreting the relation between retrieval practice and extra study.

To measure covert retrieval practice, we included an item on our questionnaire asking subjects whether they engaged in "extra retrieval" during the extra-study trials. If covert practice influences the inhibition effect, we should expect different amounts of impairment for the high- and the low-covert-practice groups, with the precise nature of this interaction depending on uncontrolled differences in the items to which subjects gave extra practice. If covert practice is common, the analysis should focus on subjects who claimed to be doing little covert retrieval practice, so that the effects of reexposures and retrieval practice on inhibition can be isolated. Finally, we asked subjects about their integration strategies, to further test for the imagery-based integration effects found in Experiment 4.

Method

Subjects

Thirty-three University of Oregon undergraduates were recruited as described previously. One subject was excluded because he failed to recall a minimum of one fact per topic during practice.

Design

The design was similar to that of Experiment 4a except that type of practice was manipulated within subjects. In the extra-study condition, we presented subjects with the previously encoded sentence and asked them to study it. One topic from one replication received retrieval practice and one topic from the other replication received extra study.

Materials and Procedure

The materials (see Appendix E for a listing) and procedure were similar to those of Experiment 4a, except that the practice orders intermixed retrieval practice and extra-study trials. For the extra-study trials, subjects received the entire sentence (e.g., *The actor is looking at the tulip*) instead of a partial cue (e.g., *The actor is looking at the tu__*). For the extra-study trials, subjects were asked to study the sentence while writing it out underneath the printed item. Subjects were warned that their memory for these items might be tested later in the same test (although it never was), to encourage them to study. The final questionnaire also included a question concerning whether subjects engaged in covert retrieval practice during the extra-study trials of the retrieval practice phase. Subjects made a rating on a 5-point scale ranging from 1 (*none of the time*) to 5 (*all of the time*).

Results

Overall Effects of Practice

As shown in Table 8, practice impaired the final recall of shared-topic items relative to items in the shared-object baseline, $F(1, 24) = 5.93$, $p < .05$, $MSE = 303.31$. This replicates Experiment 3, in which shared-topic impairment was found when the output order of facts was controlled. Practice also facilitated the practiced items, relative to their corresponding baseline, $F(1, 24) = 23.66$, $p < .001$, $MSE = 573.72$.

Importantly, the shared-object impairment found in Experiments 4a and 4b was also replicated: Practice impaired shared-object items relative to the shared-object baseline, $F(1, 24) = 8.36$, $p < .01$, $MSE = 433.61$, supporting the conclusion that propositional retrieval-induced forgetting is cue-independent.

Effects of Integration Strategies

Impairment was significant for low integrators in both the shared-topic condition, $F(1, 16) = 8.67$, $p < .01$, $MSE = 291.20$, and the shared-object conditions, $F(1, 16) = 10.87$, $p < .01$, $MSE = 362.13$, but not for high integrators in either the shared-topic condition ($F < 1$) or the shared-object condition, $F(1, 16) = 1.38$, $p > .25$, $MSE = 540.51$, in keeping with prior integration effects. However, although suggestive of a trend, the interaction of shared-topic impairment with integration status did not reach significance, $F(1, 16) = 2.82$, $p = .11$, $MSE = 291.20$,

Table 8
Mean Percentage of Facts Recalled as a Function of the Retrieval Practice Status of a Fact, the Degree of Reported Integration, and Within-Topic Testing Position in Experiment 5

Degree of reported integration	Retrieval practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared object	Shared-object baseline
Low integration	64	60	76
High integration	72	69	74
M	68	65	75
	Second three items tested within a topic		
	Practiced	Unrelated	Unrelated baseline
Low integration	53	34	34
High integration	63	43	40
M	58	39	37

Note. $n = 32$. Degree of reported integration = the extent to which subjects reported interrelating propositions about a topic in the study phase; within-topic testing position = whether a proposition was one of the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports the data from those items tested in the last half. The practice status of an item differs in the top and bottom halves.

nor did the analogous interaction for shared-object impairment, $F(1, 16) = 2.25$, $p = .15$, $MSE = 362.13$. None of these effects varied with the type of practice. This pattern generally supports the view that integration reduces impairment in the shared-topic and shared-object conditions, as found in Experiment 4a.

The replication of the integration effects found in Experiment 4a suggests that the weak interactions of inhibition and integration in these two experiments reflect low statistical power. To test this, we combined the retrieval practice condition of Experiment 5 with Experiment 4a. The combined shared-topic inhibition interacted reliably with integration, $F(1, 48) = 6.51$, $p < .05$, $MSE = 0.060$, but shared-object inhibition did not, $F(1, 48) = 1.44$, $p = .23$, $MSE = 0.06$. Nevertheless, shared-object inhibition was reliable for the low-integration group, $F(1, 48) = 7.87$, $p < .01$, $MSE = 0.064$, but not the high-integration group, $F(1, 48) = 1.23$, $p > .25$, $MSE = 0.064$. These findings suggest that integration modulates shared-topic impairment but has a weaker effect on shared-object impairment.

Experiment 5, like Experiment 4a, contrasts with both Experiments 3 and 4b in the effects of integration on inhibition. The overall integration rate in Experiment 5 ($M = 2.83$), like in Experiment 4a ($M = 2.61$) was higher than in Experiment 3 ($M = 2.05$) or 4b ($M = 2.36$), all of which were lower still than the rates in Experiments 1a ($M = 2.91$), 1b ($M = 2.87$), and 2 ($M = 3.21$; see Appendix B). This fits the idea that the absolute level of integration affects how different the two post hoc groups are. Similar results have been found in work with categories (M. C. Anderson & McCulloch, 1999). It is unclear why integration varies, although factors that are likely to contribute may include variations in (a) exposure to memory techniques, (b) experimental group size, which may lead subjects to place greater or lesser effort into using special strategies (presumably with small groups being more motivated), and (c) the ease with which materials can be integrated, such as the degree of prior relatedness. Despite these variations, incidental encoding did appear, on average, to reduce integration (compare Experiments 1a, 1b, and 2, $n = 160$, $M = 3.0$, with Experiments 3, 4a, 4b, and 5, $n = 176$, $M = 2.5$).

Effects of the Type of Practice

As can be seen in Table 9, neither shared-topic ($F < 1$) nor shared-object ($F < 1$) impairment interacted reliably with the type of practice. In the retrieval practice condition, shared-topic impairment (baseline − shared topic = 9%) was marginally significant, $F(1, 24) = 3.98$, $p = .05$, $MSE = 389.47$, and shared-object impairment (baseline − shared object = 10%) was significant, $F(1, 24) = 4.57$, $p < .05$, $MSE = 418.54$. In the extra-study condition, shared-topic impairment (baseline − shared topic = 5%) was not significant ($F < 1$), but shared-object impairment (baseline − shared object = 10%) was, $F(1, 24) = 4.39$, $p < .05$, $MSE = 389.81$. Although shared-topic impairment was numerically weaker for the extra-study condition, the pattern of impairment for the shared-topic and shared-object conditions did not differ in the manner expected. However, we should consider whether the pattern for these practice types was caused by covert practice.

Effects of Covert Retrieval Practice

Subjects were divided into low- and high-covert practice groups, on the basis of their postexperimental questionnaire ratings of covert practice for extra-study items. Subjects were assigned to groups using a procedure similar to that described in Experiment 1 to construct integration subgroups (mean ratings for low- and

Table 9
Mean Percentage of Facts Recalled as a Function of the Practice Status of a Fact, the Type of Extra Practice, and Within-Topic Testing Position in Experiment 5

Type of extra practice	Practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared object	Shared-object baseline
Retrieval practice	66	65	75
Extra study	70	65	75
M	68	65	75
	Second three items tested within a topic		
	Practiced	Unrelated	Unrelated baseline
Retrieval practice	60	38	37
Extra study	55	40	37
M	58	39	37

Note. $n = 32$. Type of extra practice = whether subjects performed retrieval practice or received extra-study trials during practice phase; within-topic testing position = whether a proposition was one of the first three propositions tested within a topic (e.g., *Ant*) or was one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports data from those items tested in the last half. The practice status of an item differs in the top and bottom halves.

high-covert-practice groups = 2.31 and 3.56, respectively). Table 10 contains the results of this analysis. Low-covert-practice subjects showed neither shared topic impairment ($F < 1$) nor shared-object impairment ($F < 1$), whereas high-covert-practice subjects showed a trend toward shared-topic impairment, $F(1, 16) = 2.25$, $p = .15$, $MSE = 0.075$, and reliable shared-object impairment, $F(1, 16) = 6.75$, $p < .05$, $MSE = 0.046$. This suggests that the similarity between extra-study and retrieval practice conditions results primarily from spontaneous retrieval practice during extra-study trials.

It is possible that the low-extra-practice group differs in such a way that no impairment would be found even in the retrieval practice condition. To test this, we looked at impairment for the retrieval practice condition in the same low- and high-covert-practice groups. Contrary to this, low-covert-practice subjects showed both shared-topic impairment (baseline − shared topic = 75% − 56% = 19%), $F(1, 16) = 10.12$, $p < .01$, $MSE = 0.052$, and shared-object impairment (baseline − shared object = 17%), $F(1, 16) = 6.18$, $p < .05$, $MSE = 0.069$. Conversely, high-covert-practice subjects, showed little shared-topic ($F < 1$) or shared-object ($F < 1$) impairment. This contrasts with the greater impairment found with high covert practice in the extra-study condition. This difference for high-covert-practice subjects shows that high covert practice does not have a consistent effect on retrieval-induced forgetting. This finding is to be expected because covert practice, by its nature, is uncontrolled and may be distributed across practiced items and competitors in haphazard fashion. By this view, finding impairment for high-covert-practice subjects in the extra-study condition was an accident of subjects distributing covert practice on practiced items in that condition at the expense of shared-topic items. For these reasons, the low-covert-practice group yields a better picture of the effects of retrieval and extra study on inhibition.

Table 10
Mean Percentage of Facts Recalled as a Function of the Practice Status of a Fact, the Degree of Reported Covert Retrieval Practice, and the Within-Topic Testing Position for Items in the Extra-Study Condition in Experiment 5

Degree of reported covert retrieval practice	Practice status of a proposition		
	First three items tested within a topic		
	Shared topic	Shared object	Shared-object baseline
Low	77	71	77
High	63	58	74
M	70	65	75
	Second three items tested within a topic		
	Practiced	Unrelated	Unrelated baseline
Low	50	38	36
High	60	42	38
M	55	40	37

Note. $n = 32$. Within-topic testing position = whether a proposition was one of the first three propositions tested within a topic (e.g., *Ant*) or one of the last three. The top half of the table reports data from those items tested in the first half of a topic-testing sequence; the bottom half reports data from those items tested in the last half. The table only includes data from the extra-study condition.

Discussion

Experiment 5 replicates all of the major results of Experiments 4a and 4b, including both the shared-topic and shared-object impairment. This indicates that propositional retrieval-induced forgetting is not caused by output interference and is cue independent, as predicted by the inhibition view. As in Experiment 4, shared-object impairment shows that cue-independent forgetting may be found for items that compete with practice targets on the basis of episodic relationships to the practice cue.

Experiment 5 was concerned with whether propositional retrieval-induced forgetting was induced by mechanisms uniquely associated with retrieval practice. Unexpectedly, both the retrieval practice and extra-study conditions showed impairment. This contrasts with studies that have consistently reported inhibitory effects with retrieval practice but not with extra study. The basis for this discrepancy is unclear. One possibility is that propositional materials differ in an important respect from the materials used in prior studies. This seems unlikely because recall-specific effects have been found with diverse materials, including categories, ambiguous words, and visuospatial representations. A more likely explanation is that subjects in our extra-study condition engaged in covert retrieval practice (of the other to-be-practiced items) during the extra-study trials, making them more like retrieval practice. This idea is supported by questionnaire reports and by post hoc analyses showing that subjects reporting the use of this extra-practice strategy produced most of the inhibition in the extra-study condition. Nevertheless, further work controlling for this strategy should be done.

Experiment 5 replicated the integration effects found in Experiment 4a. When these data were combined, imagery-based integration virtually eliminated shared-topic impairment. This supports the idea that the lack of integration in Experiments 3 and 4b stems from a lower integration rate rather than special properties of imagery-based integration. Interestingly, integration effects on shared-object impairment are less clear, although these items show trends toward reduced inhibition. That integration benefits are less reliable for these items may indicate that they are at least partially cue dependent. However, because there did appear to be at least a modest reduction in impairment for shared-object items, it seems prudent to reserve judgment. At the very least, the benefits of integration are more clearly positive when subjects are tested with the same cue around which the integrated representation was formed.

General Discussion

Previous work has shown that recalling an event from long-term memory impairs the retention of related events (e.g., M. C. Anderson et al., 1994; M. C. Anderson & Spellman, 1995). The present experiments show that the same retrieval-based inhibition contributes to the forgetting of experimentally learned facts. All seven experiments found that retrieval practice (e.g., recalling *The actor is playing the guitar,* given *Actor playing gu—*) impairs recall of facts sharing a topic with the practiced items (i.e., shared-topic facts, e.g., *The actor is playing the oboe*). This impairment was long lasting: Shared-topic items were more likely to be forgotten even when they were tested before the stronger practiced facts on a cued-recall test (e.g., *The actor is playing the o—*) 15 min after retrieval practice, replicating findings with other materials (M. C.

Anderson et al., 1994; M. C. Anderson & McCulloch, 1999; M. C. Anderson, Bjork, & Bjork, 2000; M. C. Anderson & Spellman, 1995). Long-lasting impairment occurred regardless of whether the shared-topic facts were semantically related (Experiments 1a, 1b, and 3) or unrelated (Experiments 2, 4a, 4b, and 5) to the practiced facts. This outcome, like previous work (Ciranni & Shimamura, 1999), shows that retrieval-induced forgetting is not restricted to materials with preexisting relationships. Taken together, these studies suggest retrieval as an important force shaping long-term retention of facts.

The current findings confirm that inhibition underlies propositional retrieval-induced forgetting. The most direct evidence comes from Experiments 4a, 4b, and 5, which show that the impairment is cue independent. When subjects practiced retrieving some facts about a topic (e.g., *The actor is looking at the tulip*), it impaired not only other facts that directly competed with practiced items (e.g., *The actor is looking at the violin*) but also facts about another topic that used concepts involved in those competitors (i.e., shared-object items; e.g., *The teacher is lifting the violin*). This suggests that when a fact directly competes with a practiced item, its concepts are suppressed, rendering them less accessible within any fact in which they participate. Importantly, shared-object impairment occurred even when their objects (e.g., *Violin*) were not similar to those in practiced facts (e.g., *Tulip;* see Experiments 4a and 5). This finding extends prior work on cue-independent forgetting (M. C. Anderson et al., 2000; M. C. Anderson & Shivde, 2000; M. C. Anderson & Spellman, 1995) to a case in which competition is driven by purely episodic relationships. As in prior work, cue-independent forgetting was long lasting: Impairment occurred even when shared-object items were tested early in the final testing sequence, before practiced items.

The current studies reveal several features of propositional retrieval-induced forgetting that help delineate the conditions under which it occurs. First, three experiments (Experiments 1a, 1b, and 3) found that retrieving a fact (e.g., *The actor is playing the guitar*) impaired not only other facts sharing the topic (e.g., *The actor is playing the oboe*) but also other facts sharing the same relation (i.e., shared-relation items; e.g., *The teacher is playing the saxophone*). This suggests that retrieving facts may cause subtle patterns of forgetting in otherwise unrelated collections of facts, on the basis of the sharing of a semantic relation. This shared-relation inhibition may not be completely general, however. In Experiment 2, which used unrelated sentence endings, retrieval practice did not impair shared-relation items. Thus, shared-relation impairment may be limited to facts that both share a relation with practiced items and are similar to them or their direct competitors. However, because relations are often constrained regarding the objects they take as arguments (e.g., the "eat" relation will usually require food to be the thing eaten), shared-relation impairment may be common.

Second, most of the experiments suggest that retrieval-induced forgetting is modulated by how facts about a topic are represented. When people learn facts about a topic in isolation, they suffer more retrieval-induced forgetting than when they form interconnections among them. In Experiments 1a, 1b, and 2, subjects who reported integrating facts showed less impairment than those who reported less integration. This reduced impairment occurred even though practiced items were strengthened to the same degree for low- and high-integration groups (see M. C. Anderson & McCulloch, 1999, for a similar result). We attempted to minimize integration as a conscious strategy in Experiments 3–5 with an incidental encoding task in which subjects formed a mental image of each sentence. Although the degree of integration decreased, subjects still reported some. Interestingly, this incidental integration also tended to reduce retrieval-induced forgetting (Experiments 4a and 5), although differences between high- and low-integrators were largest when the overall integration rate was high. Overall, these results parallel findings from previous work on integration in retrieval-induced forgetting as well as work on fan interference (e.g., Myers et al., 1984; Radvansky & Zacks, 1991).

Although integration regularly affected shared-topic impairment, it did not consistently affect either shared-relation or shared-object impairment. In Experiments 1a, 1b, and 3, integration did not reliably modulate the shared-relation effect, although differences in the expected direction were observed. Experiments 4a and 5 found integration effects for shared-topic items but only weak integration effects for shared-object items. This suggests that integration benefits may be partially cue dependent. However, given that these integration effects lean in the expected direction, excluding a cue-independent factor to integration benefits seems premature (see Radvansky, 1999a, for evidence for this possibility).

Third, the present studies show that long-lasting retrieval-induced forgetting does not hinge on the use of stem-cued recall in the retrieval practice phase. One may argue that introducing a letter-stem-cued-recall task on the final test to control for output interference biased subjects to focus on the practice context in their memory search because letter-stem-cued recall also was used in the practice phase. Although such a search bias might cause deficits for shared-topic items that would mimic inhibition, retrieval-induced forgetting also was found when we eliminated stem-cued recall in the practice phase. Obtained shared-relation and shared-object impairment also argue that the match between practice and test cues is not central to retrieval-induced forgetting, because these items used few or no cues from the practice phase (see Ciranni & Shimamura, 1999, for a related finding). Rather, the factor underlying impairment is the effort to recall the practice targets.

Finally, the current studies yielded preliminary evidence that propositional retrieval-induced forgetting is initiated by processes uniquely associated with recall. Experiment 5 manipulated whether subjects received retrieval practice or extra-study trials. Work has found retrieval-induced forgetting only when the practice targets are actively recalled (M. C. Anderson, Bjork, & Bjork, 2000; M. C. Anderson & Shivde, 1999, 2000; Blaxton & Neely, 1983; Ciranni & Shimamura, 1999). Unlike in those studies, extra-study trials in the present experiments, although they did not impair shared-topic items, impaired shared-object items. This suggests that propositional retrieval-induced forgetting may not require active recall to be induced, but analyses suggest that the overall results are misleading. Inhibition in the extra-study-trials condition was caused mainly by subjects who covertly recalled practiced sentences other than the one they were currently studying. When the analysis focused on subjects who reported little covert practice, neither shared-topic nor shared-object impairment was found. Thus, the current findings are compatible with prior evidence that impairment is recall specific, although further work must be conducted before accepting this conclusion with confidence.

Taken together, these findings suggest a novel perspective on the way in which one forgets facts. Merely recalling facts that we know about a concept can cause long-lasting memory impairment for related facts. The properties of this impairment suggest that associative interference approaches to propositional forgetting are incomplete. In the next section, we consider these implications and whether complex adaptations of noninhibitory frameworks may accommodate the current data.

Noninhibitory Approaches Revisited

Forgetting in propositional memory has typically been explained by associative interference processes such as those incorporated into the ACT family of models (J. R. Anderson, 1983; J. R. Anderson & Reder, 1999a). These models assume that the activation of a retrieval cue is a limited resource that is divided among the associative pathways emanating from the cue. The more facts associated to a cue, the less activation any particular target memory will receive, reducing the speed and probability of retrieval. The stronger the pathway from a cue to a memory, the less activation will flow to other facts, again impeding retrieval. In this framework, suppression need not be posited to explain retrieval-induced forgetting.

This associative interference approach attributes retrieval-induced forgetting to the failure of a particular cue to activate a memory. As such, retrieval-induced forgetting should be cue specific. This renders models such as ACT–R problematic as accounts of retrieval-induced forgetting. Consider Figure 3, which depicts what subjects are likely to encode in the learning and practice phases of Experiment 4b. In this example, subjects encode associations between the topic and its relation, the relation and its object, and a direct association between the topic and the object of a sentence. The two related topics are assumed to make reference to the same representations in memory, although through different relations (e.g., to *Violin,* through *Is looking* and *Is lifting*). Retrieval practice on facts such as *The actor is looking at the tulip* should strengthen the association between the practiced topic and the object, and perhaps also the associations between the topic and the relation and the relation and the object, as depicted by darkened lines. Performance should be impaired on the delayed test when shared-topic items like *The actor is looking at the violin,* are cued with *Actor,* because a greater proportion of the limited activational resources should flow from *Actor* to *Tulip* (compared with when *Tulip* is unpracticed). Thus, the associative interference approach easily accommodates shared-topic impairment.

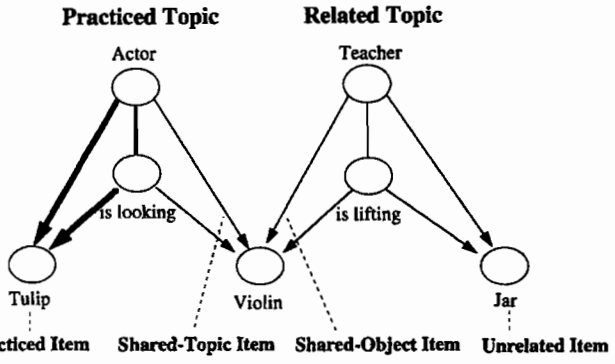

Figure 3. One hypothesis about how subjects might represent the sentences presented to them in Experiments 4a and 5 for a related pair of topics. This hypothesis assumes that when subjects encode a sentence, they associate the subject of each sentence (e.g., *Actor*) to its relation (e.g., *is looking*), its relation to its sentence ending (e.g., *Tulip*), and the subject to the sentence ending (e.g., *Tulip*). The sentences that make reference to the same word (e.g., *Violin*) are assumed to make use of a common representation in memory, which may either be the general concept for the word or an episode-specific representation of the referent of the word. Retrieval practice is presumed to strengthen the associative connection between the practiced topic and the practiced object, the practiced topic and the practiced relation, and the practiced relation and the practiced object, as depicted by the darkened lines for each of the foregoing associative connections.

Recall of the same concept, "violin" from the retrieval cue *The teacher is lifting the v—,* however, should be unimpaired because the diversion of resources caused by the stronger *Actor* to *Tulip* or *Looking* to *Tulip* associations should not occur when subjects are tested with a cue that is not associated with those strengthened concepts. Neither *Teacher* nor *Lifting* is associated to strongly competing concepts, so there is no difference between the practiced and baseline condition. With this set of assumptions, cue-dependent forgetting models do not explain shared-object impairment.

Associative interference models can explain shared-object impairment if more elaborate assumptions are made about subjects' strategies during final recall. For instance, suppose that when they were cued to recall shared-object items such as *The teacher is lifting the v—,* subjects augmented their search with additional cues. In addition to *Teacher,* suppose subjects retrieved the related *Actor* topic because (a) it was associated with "teacher," by virtue of the co-occurrence of these topics with shared-object items and (b) subjects believed that recalling this additional topic would help them recall the sentence endings shared by the topics. If subjects used the practiced topic as a second cue, they might have suffered interference from practiced items while recalling shared-object items. We refer to this as the *covert-cuing hypothesis.*

The covert-cuing hypothesis rejects the claim that cuing shared-topic items with a different topic and relation constitutes use of an independent probe. It asserts that subjects deliberately retrieve additional cues that systematically impair their recall. It seems implausible that subjects would use such a self-defeating strategy. Moreover, this hypothesis explains shared-object impairment only at the cost of generating additional predictions that are incorrect. In particular, it predicts the impairment of unrelated facts. For example, suppose that when presented with *The teacher is lifting the v—,* on the final test, subjects covertly recalled *Actor* to help them retrieve *Violin.* According to the covert-cuing hypothesis, this strategy triggers interference from practiced facts associated with *Actor,* and the recall of *Violin* is impaired. In addition to impairing *Violin,* however, this strategy should impair unrelated facts like *The teacher is lifting the jar* (see Figure 3). From the subjects' viewpoint, the sentence cues for shared-object and unrelated items are indistinguishable; subjects should use the strategy in each case, rendering unrelated items susceptible to interference. Indeed, unrelated items should be more impaired than shared-object items because they would not receive activation when the practiced topic was used as a second cue. That unrelated items were unimpaired in Experiments 4a, 4b, and 5 thus argues that either (a) subjects do not use covert cuing or (b) using covert cues does little to disrupt

unrelated or shared-object retrieval. In either case, the selectivity of impairment to shared-object items remains unexplained.

The foregoing findings suggest that purely associative approaches are unlikely to provide a satisfactory account of propositional retrieval-induced forgetting. Even when relatively complex assumptions are made about subjects' search strategies or knowledge representations, these approaches fail to correctly predict the current findings.

The Inhibitory Approach

According to the inhibitory approach, isolating an event or fact in memory poses computational problems isomorphic to those confronted when we must selectively attend to objects in our perceptual world. Just as focusing on a visual object often requires us to gate distraction from competing percepts, bringing a fact or event to consciousness often requires that we overcome internal distraction generated by retrieval cues. A key assumption is that distraction from competing memories is overcome by attentional inhibition that suppresses those items. Inhibition is adaptive both because it speeds access to target memories and because it prevents irrelevant representations from being incorporated into ongoing cognition. Thus, the inhibitory approach posits an active role of cognitive control in long-term retrieval (see Postman, Stark, & Fraser, 1968, for a related view).

Like associative interference, inhibition explains shared-topic and shared-relation impairment. Cuing subjects with a topic and relation during practice activates the to-be-practiced, shared-topic, and shared-relation items matching those cues. Because a letter stem also is given, the entire set of cues draws search toward the practiced item. However, highly active competitors slow access and may even intrude during retrieval. To enhance retrieval speed and to prevent competitors' inadvertent selection, inhibitory control mechanisms are recruited to suppress shared-topic and shared-relation items. To the extent that inhibition lingers, shared-topic and shared-relation impairment should be found on the delayed-recall test, even when output interference from practiced facts is eliminated.

The inhibitory approach also has advantages over associative interference models in explaining the present data. Unlike the associative interference approach, inhibition predicts shared-object impairment. According to the inhibition view, retrieval practice suppresses those components of competing facts that are not also part of the retrieval practice target. For example, retrieval practice should suppress the objects of shared-topic items, but not their topic and relation, because both of these are strengthened by their presentation in practiced facts. Because practice suppresses the competitors' objects, other facts using those objects should also be impaired, even if they use different topics and relations. Unrelated facts that share a topic with the shared-object items should not be impaired because (a) they contain no concepts used in shared-topic items and (b) they are not similar to shared-topic items. Thus, inhibition predicts cue-independent impairment without recourse to complex assumptions about search strategies on the final test and without incorrectly predicting impairment of unrelated items. Finally, the inhibition approach predicts that neither shared-topic nor shared-object items should be impaired if subjects do not have to resolve interference during retrieval practice. Thus, it should be possible to eliminate impairment by replacing retrieval practice with extra study, provided that subjects do not engage in covert retrieval practice during extra study. This pattern was observed in Experiment 5.

One difficulty with the inhibition view is that without additional assumptions, it cannot explain the apparent lack of shared-relation impairment in Experiment 2. There, subjects received the relation as a practice cue, which should have led shared-relation items to compete, but shared-relation impairment was not found. (This finding is also problematic for associative interference views.) Another interpretation of shared-relation effects can predict the findings of Experiment 2, however. Prior work has shown that retrieval practice on practiced targets (e.g., *Green Emerald*) impairs not only direct competitors (e.g., *Green Lettuce*), but also items that are semantically similar to the competitor, even if they are unrelated to the practiced item (e.g., *Soups Mushroom;* see M. C. Anderson, Green, & McCulloch, 2000, for a similar finding). A similar process may cause shared-relation effects: Perhaps shared-relation items (e.g., *The teacher is playing the saxophone*) are suppressed not because they are activated by the shared relation (e.g., *The actor is playing the gu—*) during practice, but because they are similar to suppressed shared-topic items (e.g., *The actor is playing the oboe*). By this account, shared-relation effects were not observed in Experiment 2 because shared-relation items were semantically unrelated to shared-topic items, so that impairment did not generalize. However, the reduction in the shared-relation effect with object dissimilarity should be replicated before any such alternative accounts are explored.

Although the current findings favor a role of inhibition in fact retrieval, important features of this effect remain to be characterized. Research remains to be done to specify both the locus and the nature of the impairment. Does episodic practice of the sort used here and in previous studies suppress episodic memory representations, semantic representations, or both? One may argue that impairment must reflect inhibition of general concepts for cue-independent forgetting to occur. Although this is reasonable, cue-independent forgetting may also be caused by suppression of episode-specific representations. For example, when subjects form an image or mental model of *The teacher is lifting the violin,* they may have imagined the same violin as they imagined for *The actor is looking at the violin.* If so, suppressing the image of "violin" should affect shared-object items, even though general concepts are not involved. No data currently discriminate this episodic view from the possibility that general semantic concepts also are affected. If correct, the episodic view would suggest limits on the generality of cue-independent impairment in that tasks that do not tap episodic representations may not reveal impairment. Implicit memory measures, for instance, may not yield retrieval-induced forgetting. Regardless of whether inhibition affects episodic or semantic representations, the demonstration of retrieval-induced forgetting with test cues not involved in practiced facts remains problematic for cue-dependent forgetting theories.

How inhibition affects competing representations also is unclear. Inhibition may reduce activation levels of competing items, with persisting suppression underlying impairment. Alternatively, inhibition may temporarily suppress competing items, which may in turn cause a more enduring structural change to the trace (M. C. Anderson & Spellman, 1995). Understanding the nature of this effect will have implications for how impairment changes under various conditions. If persisting suppression underlies the effect, retrieval-induced forgetting will dissipate over time and will be easily reversed. On the other hand, if inhibition causes a structural

change, spontaneous recovery may not occur. Although little is known about the temporal parameters of inhibition, the current results show that propositional retrieval-induced forgetting lasts at least 15 min.

The long-term forgetting shown here suggests that J. R. Anderson and Reder's (1999a) failure to find cue-independent impairment was not due to the rapid dissipation of inhibition, as suggested by Radvansky (1999b). However, Radvansky's hypothesis may still be correct for at least two reasons. First, the retrieval practice and fan effect procedures differ in the degree to which facts are trained initially. Inhibitory effects may be more enduring and disruptive when they are applied to weakly learned episodic traces (M. C. Anderson & Spellman, 1995) that are not highly integrated. Second, as Radvansky has noted, reexposure to inhibited concepts throughout J. R. Anderson and Reder's test phase (e.g., as distractors) may have released inhibition. Beyond such temporal factors, the present results suggest that their finding was produced in part by (a) the greater integration effects induced by their training procedure, (b) their use of repeated recognition trials on practiced items instead of recall practice, which may have weakened effects, and (c) the use of a same relation baseline against which to measure inhibition. Regardless, the current findings converge with Radvansky's (1999b) in supporting cue-independent forgetting in propositional memory. They further show that suppression can have a long-term impact on unused facts. In the next section, we consider the implications of this for how the use of what we know shapes our knowledge of the world.

Selective Retrieval and the Shaping of Knowledge

Many factors encourage the selective recall of certain facts about a topic or person at the expense of others. Selectivity may arise because some facts are more useful than others, because retrieval strategies favor recovery of some facts over others, or because certain facts are biased by environmental context, mood state, or motivation. Whatever the reasons, the present work suggests that being consistently selective can erode competence with related facts that remain unused. Such erosion should cause failures to retrieve knowledge when it is needed and encourage selective memory for facts conforming to our beliefs. In this section, we illustrate situations in which selective retrieval may shape what remains accessible in memory.

Selective Retrieval and the Schematization of Autobiographical Memories

The retrieval of factual knowledge often is guided by schemas. When recalling a birthday party at a restaurant, one may search memory not only with the identity of the birthday person and the spatiotemporal context but also with general knowledge about birthday parties and restaurants. Many studies have shown that long-term memory is better for schema-consistent than schema-irrelevant facts (e.g., Bower, Black, & Turner, 1979; Brewer & Treyens, 1981; Graesser & Nakamura, 1982; Hastie, 1981; Rothbart, Evans, & Fulero, 1979; D. A. Smith & Graesser, 1981; see Alba & Hasher, 1983; Stangor & McMillan, 1992, for reviews). Over time, people come to rely more heavily on schemas when retrieving specific facts about events, as evidenced by an increased tendency to recall schema-consistent information that never has been experienced, and to not recall schema-irrelevant information that has been encoded (e.g., Bartlett, 1932; Kintsch & van Dijk, 1978; D. A. Smith & Graesser, 1981). Most investigators attribute the relevance advantage to either selective encoding of schema-relevant facts or to the failure of the schema to provide an effective retrieval route for schema-irrelevant facts.

Retrieval-induced forgetting also may contribute to the recall advantage for schema-relevant information. According to this view, all varieties of information (schema consistent or not) may be adequately encoded (see Alba & Hasher, 1983, for evidence on this point). As time passes, people increasingly rely on schemas to facilitate recall. Schema-driven retrieval may lead schema-consistent knowledge to be recalled earlier than irrelevant facts because schematic cues favor schema-consistent facts. If schema-consistent knowledge routinely has a retrieval advantage, irrelevant knowledge associated with the cues guiding retrieval will be impaired. Furthermore, knowledge relevant to but inconsistent with the schema (e.g., events that contradict expectations), although often remembered better than schema-consistent information (see Stangor & McMillan, 1992), will be forgotten over time. Finally, if a general schema guides retrieval of different instances of an event type, retrieving facts about one instance ought to impair memory for unique details of others. Thus, retrieval-induced forgetting predicts that with time and reflection, events will become less distinct and more schema consistent.

Selective Retrieval and Biases in Person and Group Perception

When we meet someone, our perception is often guided by schemas that classify them on the basis of dimensions such as occupation, age, gender, and ethnicity. The same process that makes episodic memories less distinct may also cause shifts in our person perceptions toward a greater consistency with our schemas. This prediction is supported by research on *bolstering*. For example, Wyer and Martin (1986) asked subjects to form impressions of fictitious people. For each one, subjects read facts about the behavior of the person (e.g., *Joe shouted and honked the horn at slow drivers*) that were either consistent or inconsistent with traits initially learned. After reading these facts, subjects spent 5 min either thinking about the person (by writing a description) or performing a distractor activity. Afterwards, both groups recalled all of the behaviors they could remember, regardless of whether they had taken the behaviors into account in forming their impressions. In keeping with prior work, Wyer and Martin found that after 5 min of distraction, schema-inconsistent behaviors were recalled better than schema-consistent ones. However, an intervening period of thought about the person reversed this effect, by improving recall of schema-consistent facts and frequently by impairing recall of inconsistent knowledge. This fits well with the current argument, assuming that writing a description of the person favors selective retrieval of schema-consistent facts.

Retrieval-induced forgetting may contribute to other well-known phenomena in social cognition. For example, people often regard members of their own social group as being more varied and distinctive than members of out-groups (E. E. Jones, Wood, & Quattrone, 1981; Judd & Park, 1988; Linville, Fischer, & Salovey, 1989; Linville, Salovey, & Fischer, 1986; Park & Rothbart, 1982). It is likely that people know more about members of their own groups, and that this allows for encoding in terms of a more refined collection of social categories than is available for out-group

members. However, even when distinctive facts are encoded about out-group members, retrieval-induced forgetting may make it less likely that such distinctive knowledge is used to learn finer distinctions. This may arise because information about particular out-group members will tend to be first retrieved through schematic knowledge about their general category membership (Bond & Brockett, 1987; Bond, Jones, & Weintraub, 1985; Bond & Sedikides, 1988), leading to impairment of schema-irrelevant facts. As distinctive knowledge is forgotten, the ability to use that knowledge to encode distinctions among out-group members will be reduced, reinforcing the impression of out-group homogeneity.

Selective Retrieval and Selective Memory for Facts

It is worth mentioning briefly several situations in which retrieval-induced forgetting may play a role, although there is little direct evidence for these conjectures. There are a variety of situations in which people must communicate complex bodies of factual material on a recurring basis. When confronted with such situations, people structure their discourse into an organized communication that highlights only the most relevant facts. Teachers develop lessons for communicating complex concepts, lessons that omit facts that may be confusing. Opinions, once formed by someone, often follow a clear pattern as they are relayed to others. Comprehenders must develop explanations for the facts they confront. In each of these situations, people develop mental structures that guide which facts they retrieve and how they retrieve them. As with other cases of schema-driven retrieval, these situations may lend themselves to biases and oversimplifications arising in part from retrieval-induced forgetting. Facts may be forgotten if other facts related to them are recalled very frequently: Teachers may forget facts omitted for simplicity while recalling related facts during a lecture; opinion makers may forget observations inconsistent with or irrelevant to their views (even if relevant to a contrary one), as they recall and report that which is consistent; and explainers may forget facts that do not fit the story they have developed to convey understanding of a situation. Thus, selective memory for facts that fit one's beliefs may derive not only from biased encoding but also from propositional retrieval-induced forgetting.

Concluding Remarks

Previous work on propositional memory has emphasized the role of learning additional facts in the forgetting of ones already known. Learning new facts about a topic adds new associative pathways from it, and these new pathways are believed to weaken the ability to recall related facts. By this view, forgetting arises from the inability of limited attentional resources to activate facts in memory when too many associative pathways emanate from a cue. Although we agree that learning new facts, as a behavioral situation, is linked to the forgetting of related facts, the current work emphasizes a different perspective on how people forget. The current work suggests that many instances of propositional forgetting are caused by the retrieval process itself, particularly from the negative effects of inhibitory mechanisms invoked to resolve competition from related facts. Thus, forgetting often arises from the effort to control retrieval in the face of internal distraction. The consequence of retrieval-based suppression is an enduring impairment for the interfering facts, which generalizes also to other facts in which the interfering concepts participate. Although the present findings do not rule out the role of limited resources as a factor in forgetting, at a minimum, they suggest that retrieval-based inhibitory processes contribute to forgetting from long-term memory.

Regardless of the mechanisms that underlie the present phenomenon, our findings highlight how retrieval processes shape what people retain of the knowledge they acquire. When people regularly use some of the facts about a topic selectively—because those facts are the most relevant to their goals, because they are compatible with their expectations or views, or because they are trying to communicate more efficiently—they risk more rapidly forgetting related facts that go unused. Such forgetting is more pronounced when related facts interfere during retrieval of target facts and when they are not well integrated with target facts. Thus, patterns in how people use their knowledge will play a powerful role in shaping which facts ultimately influence cognition and action.

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Appendix A

Propositions Used in Experiment 1

Group	Proposition
Pair 1	The box is in the [lodge, warehouse, restaurant, pub, airport, hotel].
	The mop is in the [library, shed, office, jail, garage, mall].
Pair 2	The ant is crawling on the [television, fan, bookcase, lamp, cabinet, rug].
	The spider is crawling on the [chair, footstool, sofa, radio, bed, table].
Pair 3	The actor is playing the [violin, banjo, guitar, oboe, harp, cello].
	The teacher is playing the [keyboard, clarinet, drum, saxophone, trumpet, bugle].
Pair 4	The wire is made of [platinum, silver, copper, nickel, tin, iron].
	The sculpture is made of [pewter, lead, gold, aluminum, bronze, steel].
Fillers	The monkey is eating the [melon, blueberry, apple, pineapple, orange, grape].
	The wind is blowing through the [elm, willow, poplar, hickory, spruce, birch].

Appendix B

Retrieval Practice Success Rates and Postexperimental Integration Ratings for Experiments 1–5

	Retrieval-practice-success rate			Integration rating		
Experiment	Overall	Low	High	Overall	Low	High
1a	78	78	78	2.91	2.37	3.44
1b	80	80	80	2.85	2.25	3.44
2	65	63	67	3.21	2.62	3.80
3	76	75	76	2.05	1.49	2.60
4a	68	70	66	2.61	1.96	3.25
4b	70	78	62	2.36	1.73	2.98
5	76	79	73	2.83	2.43	3.22
M	73	75	72	2.69	2.12	3.25

Note. *Low* and *High* refer to the low- and high-integration subgroups that were created to examine the effects of integration on retrieval-induced forgetting.

Appendix C

Propositions Used in Experiment 2

Group	Proposition
Pair 1	The radio is in the [tent, church, box, store, bag, luggage].
	The key is in the [ditch, elevator, kitchen, desk, trailer, sailboat].
Pair 2	The ant is crawling on the [potato, bench, window, fence, cup, cane].
	The spider is crawling on the [lamp, wheel, bush, rake, rope, dryer].
Pair 3	The actor is looking at the [hammer, tulip, napkin, lion, mirror, vodka].
	The teacher is looking at the [sparrow, shoe, sun, river, football, swing].
Pair 4	The toy is made of [copper, wood, plastic, enamel, fiberglass, crystal, glass].
	The sculpture is made of [wax, marble, paper, clay, fabric, rubber].
Fillers	The carpet is [blue, purple, yellow, green, red, orange].
	The shark is eating the [abalone, eel, oyster, seahorse, squid, urchin].

(*Appendixes continue*)

Appendix D

Propositions Used in Experiment 3

Group	Proposition
Pair 1	The box is in the [lodge, warehouse, restaurant, pub, airport, hospital].
	The mop is in the [bookstore, shed, office, jail, garage, motel].
Pair 2	The ant is crawling on the [television, desk, mirror, lamp, footstool, rug].
	The spider is crawling on the [armchair, cabinet, sofa, vase, bed, endtable].
Pair 3	The actor is playing the [violin, banjo, guitar, oboe, harp, flute].
	The teacher is playing the [keyboard, clarinet, drum, saxophone, trumpet, accordion].
Pair 4	The wire is made of [platinum, magnesium, copper, nickel, tin, iron].
	The sculpture is made of [zinc, lead, gold, aluminum, bronze, steel].
Fillers	The monkey is eating the [melon, blueberry, apple, pineapple, orange, grape].
	The wind is blowing through the [elm, willow, poplar, hickory, spruce, birch].

Appendix E

Propositions Used in Experiments 4a and 5

Group	Proposition
Pair 1	The box is in the [bag, ditch, alley, *restaurant, pub, office*].
	The mop is leaning against the [truck, ladder, hedge, *restaurant, pub, office*].
Pair 2	The ant is crawling on the [potato, fence, wheel, *desk, chair, bed*].
	The spider is under the [rake, grass, hammer, *desk, chair, bed*].
Pair 3	The actor is looking at the [tulip, napkin, lion, *violin, guitar, flute*].
	The teacher is lifting the [mirror, jar, ribbon, *violin, guitar, flute*].
Pair 4	The toy is made of [fabric, enamel, stone, *copper, brass, tin*].
	The machine is cutting the [paper, wax, rubber, *copper, brass, tin*].
Fillers	The monkey is eating the [melon, blueberry, apple, pineapple, orange, kiwi].
	The wind is blowing through the [oak, willow, fir, hickory, spruce, birch].

Note. Italicized items are shared between topics from a given pair.

Appendix F

Propositions Used in Experiment 4b

Group	Proposition
Pair 1	The box is in the [bag, ditch, alley, *restaurant, shed, cart*].
	The mop is leaning against the [truck, ladder, hedge, *restaurant, shed, cart*].
Pair 2	The ant is crawling on the [potato, fence, wheel, *desk, beam, vent*].
	The spider is under the [rake, hammer, grass, *desk, beam, vent*].
Pair 3	The actor is looking at the [tulip, napkin, lion, *violin, football, boot*].
	The teacher is lifting the [mirror, jar, ribbon, *violin, football, boot*].
Pair 4	The toy is made of [clay, enamel, stone, *fabric, glass, tin*].
	The machine is cutting the [paper, wax, rubber, *fabric, glass, tin*].
Fillers	The monkey is eating the [melon, blueberry, apple, pineapple, orange, kiwi].
	The wind is blowing through the [elm, willow, poplar, hickory, spruce, birch].

Note. Italicized items are shared between topics from a given pair.

Received April 10, 2000
Revision received August 28, 2000
Accepted August 28, 2000 ■