Recollection Rejection: False-Memory Editing in Children and Adults

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Mechanisms for editing false events out of memory reports have fundamental implications for theories of false memory and for best practice in applied domains in which false reports must be minimized (e.g., forensic psychological interviews, sworn testimony). A mechanism posited in fuzzy-trace theory, recollection rejection, is considered. A process analysis of false-memory editing is presented, which assumes that false-but-gist-consistent events (e.g., the word SOFA, when the word COUCH was experienced) sometimes cue the retrieval of verbatim traces of the corresponding true events (COUCH), generating mismatches that counteract the high familiarity of false-but-gist-consistent events. Empirical support comes from 2 qualitative phenomena: recollective suppression of semantic false memory and inverted-U relations between retrieval time and semantic false memory. Further support comes from 2 quantitative methodologies: conjoint recognition and receiver operating characteristics. The analysis also predicts a novel false-memory phenomenon (erroneous recollection rejection), in which true events are inappropriately edited out of memory reports.

Although early interest in memory falsification may be found in Binet’s (1900) and Bartlett’s (1932) work and in gestalt psychology (Wulf, 1922), the appearance of true before memory would have seemed redundant until a few years ago. Since then, there has been a remarkable flowering of false-memory research. Old paradigms have been revived that induce false recognition and false recall of semantic associates of word lists (Deese, 1959; Underwood, 1965) and that induce false recognition of inferences from sentences (Bransford & Franks, 1971), the Loftus (1979; Loftus, Miller, & Burns, 1978) misinformation procedure, which was devised to investigate memory errors in testimony, has been adapted to explore false memories of physical trauma (e.g., Bruck, Ceci, Francouer, & Barr, 1995; Peters, 1997) and of behaviors that are reported as part of sexual abuse (e.g., Goodman & Quas, 1997; Poole & Lindsay, 1995). Further techniques have been developed to study false memories for unseen pictures (Koutstaal & Schacter, 1997), absent objects (Lampinen, Copeland, & Neuschatz, 2001), fictitious personal experiences (Goff & Roediger, 1998), unspoken sentences (Ackerman, 1994), fictitious narrative events (Howe, 1991), and unspoken metaphors (Reyna & Kieran, 1995). New theories have emerged that attempt to bring order to the data base that is being generated by these paradigms (e.g., Reyna & Brainerd, 1995; Roediger & McDermott, 2000).

An appealing aspect of the false-memory literature is that it cuts across traditional research boundaries. Two boundaries that are routinely crossed are those between basic and applied research and between developmental and adult research. In the former case, procedures and findings that have been used to test theoretical proposals about false memory have also been used to construct best-practice guidelines (recommended procedures for interviewing individual people) of criminal investigations (e.g., Ceci & Bruck, 1995; Poole & Lamb, 1998) and in psychotherapy (e.g., Hogarty & Flesher, 1999; Lindsay & Read, 1994). Conversely, programs of basic research have been motivated by false-memory phenomena in criminal investigations, such as false confession (e.g., Kassin & Kiechel, 1996), and by false-memory phenomena in psychotherapy, such as guided-imagery confabulation (e.g., Hyman & Pentland, 1996). Concerning the boundary between developmental and adult research, paradigms that were devised in laboratory research with adults have become staple features of more naturalistic developmental studies (e.g., Bjorklund, Bjorklund, Brown, & Cassel, 1998; Holliday, Douglas, & Hayes, 1999; Howe, 2000; Ornstein, Shapiro, Clubb, Follmer, & Baker-Ward, 1997; Pipe, Gee, Wilson, & Egerton, 1999; Powell, Roberts, Ceci, & Hembrouke, 1999; Roberts & Blades, 1999). Conversely, theoretical principles that evolved from developmental work now figure prominently in false-memory research with adults (e.g., Koriat, Goldsmith, & Pansky, 2000; McEvoy, Nelson, & Komatsu, 1999; Payne, Elie, Blackwell, & Neuschatz, 1996; Schacter, Israel, & Racine, 1999; Schacter, Verfaellie, & Pradere, 1996; Tussing & Greene, 1999).
The profusion of methodologies notwithstanding, the modal false-memory situation is one in which false reports are consistent with the gist of participants’ experience (Brainerd & Poole, 1997; Bruck & Ceci, 1997; Ceci & Bruck, 1993; Reyna & Lloyd, 1997). For example, a child abuse victim falsely reports having been touched on the left breast, rather than correctly reporting having been touched on the right breast; a participant in a free-recall experiment falsely recalls hearing the word DOCTOR after hearing the words NURSE, SICK, MEDICINE, HEALTH, HOSPITAL, and ILL; a witness to an armed robbery falsely reports that the suspect was carrying a rifle, rather than correctly reporting that the suspect was carrying a pistol; a participant in a recognition experiment falsely claims to have heard the sentence “The coffee is hotter than the tea” after hearing the sentences “The coffee is hotter than the cocoa” and “The tea is hotter than the cocoa.” The reported items in these examples, though not experienced, nevertheless seem very familiar because they preserve the gist of experience.

As real-life and laboratory demonstrations of false memory have multiplied, so too has interest in identifying cognitive mechanisms that allow humans to avoid false reports that concur with the gist of their experience. It is widely acknowledged that experimental verification of such false-memory editing operations would have important theoretical and practical consequences (Brainerd, Reyna, & Poole, 2000; Ceci & Bruck, 1995; Poole & Lamb, 1998). On the theoretical side, these operations have been predicted in certain accounts of the relation between true and false memories (Reyna, 1996; Reyna, & Brainerd, 1995), so that the validity of those accounts turns in some measure on the existence of specific editing operations. On the practical side, once an editing operation is identified, procedures can be instituted that support its use in naturalistic situations in which a premium is placed on avoidance of false reports, such as forensic psychological interviews, police interrogations, and sworn testimony. Indeed, a core objective of best-practice guidelines for such situations is to build in features that mitigate the influence of false memories (Ceci & Bruck, 1995; Poole & Lamb, 1998). Owing to this fact, it is essential that the level of scientific confirmation of putative editing operations should be high.

Ideally, a confirmed editing operation will satisfy two main criteria. First, the process details of the operation and the conditions under which it occurs will follow from well-established theoretical principles, which makes it possible to design research that is organized around theoretically motivated manipulations. Second, a confirmed editing operation will be detectable in multiple paradigms using various materials. Because cognitive processes are not directly observable, converging evidence is required (i.e., a series of distinct empirical effects, each of which is congruent with the posited operation).

The purpose of the present article is to consider a false-memory editing operation that satisfies both of these criteria. With respect to the first criterion, the candidate operation, recollection rejection, follows from dual-retrieval models of memory. We summarize such models and provide a process theory of the candidate operation in the first section, below. With respect to the second criterion, there are four classes of phenomena that provide converging support for recollection rejection. Two of them, recollective suppression of semantic false memory and nonmonotonic (inverted-U-shaped) relations between retrieval time and semantic false memory, are qualitative, in that they involve comparisons of false-memory rates across different experimental conditions. Relevant findings are reviewed in the second section. The other two classes of phenomena, recollection rejection in conjoint recognition and in receiver operating characteristics (ROCs), are quantitative, in that they involve estimation of actual amounts of recollection rejection, using mathematical models. Results for these estimation procedures, including findings from some new experiments, are reviewed in the third section. In the fourth section, we show that the same theoretical principles predict a complementary but inappropriate editing operation, erroneous recollection rejection, in which true memories are rejected.

A Process Theory of False-Memory Editing

There is introspective evidence that adults sometimes edit false-but-gist-consistent information out of memory reports (e.g., Koriat et al., 2000). A key source of such evidence is memory self-monitoring studies, which give participants the option of withholding or reporting items of information (Koriat & Goldsmith, 1994, 1996). Participants in such studies report that they screen candidate items of information, weeding out those that are believed to be false. Although such data are compelling as an existence proof of false-memory editing, they have three limitations when it comes to the identification of specific editing operations. First, these reports refer to deliberate, strategic control of memory output via the conscious phenomenology that accompanies remembering. However, editing could occur automatically, outside conscious awareness, and such activities would not be susceptible to verbal description by participants who are contemplating the phenomenology of remembering. For instance, participants in memory self-monitoring studies do not always report suppressing the output of candidate items, and they rarely report such suppression in standard free-recall conditions (e.g., Brainerd, Reyna, Howe, & Kevershan, 1990). Second, such evidence is difficult to obtain with children, whose performance on introspective self-report tasks is notoriously poor (e.g., Bjorklund, 1999). Yet, even young children seem to be capable of false-memory editing (Brainerd & Reyna, 2002; Brainerd, Stein, & Reyna, 1998; Koriat, Goldsmith, Schneider, & Nakash-Dura, 2001). Third, and most important, reports of deliberate control of output are not, in themselves, a process theory of false-memory editing; they do not supply a detailed account of the representational, retrieval, and output processes that suppress false-but-gist-consistent information. Such an account is essential in order to design theory-driven experiments that will further understanding of false-memory editing in sufficient depth to control it in applied contexts in which it is crucial to avoid false reporting.

Fortunately, a process theory of a specific false-memory editing operation, recollection rejection, can be derived from dual-retrieval models of memory. Two attractive features of this operation are that it could occur outside as well as inside conscious awareness and that it is likely to be available to young children, although it would surely undergo development. Before describing the operation, it is necessary to sketch the core assumptions of dual-retrieval theories. We then present the candidate editing operation and recount some puzzling findings that it is able to explain.
Dual-Retrieval Models

In recognition experiments, participants are exposed to a series of items (targets) and then respond to a series of memory probes, consisting of mixtures of targets and unpresented items (distractors). The classical account of recognition held that target acceptances (hits) are due to a single retrieval process, often called familiarity, that accesses a continuous memory-strength scale, with a probe being accepted (as “old”) if its strength falls above some subjective criterion. In contrast, Atkinson and associates (Atkinson & Juola, 1973, 1974; Atkinson & Wescourt, 1975; Juola, Fischler, Wood, & Atkinson, 1971) proposed that hits are the result of two retrieval processes. They argued that participants first evaluate a target’s global familiarity, with the probe being accepted (as “old”) or rejected (as “new”), accordingly, as its scale value exceeds some high subjective criterion or falls below some low criterion. When familiarity delivers an indeterminate value, a slower back-up operation, usually called recollection, searches participants’ stored verbatim list of study items, producing acceptance if the probe matches an item on the verbatim list or rejection if it does not.

Mandler (1980) extended this account by postulating that recollection is not contingent on familiarity failure, but rather, “the two separate processes occur conjointly; recognition involves the additive effects of familiarity and retrieval [recollection]” (p. 253). He implemented that assumption in the expression

\[
p(H) = R + (1 - R)F, \tag{1}
\]

where \(p(H)\) is the probability of a hit, \(F\) is the probability of familiarity-based acceptance, and \(R\) is the probability of recollection-based acceptance.\(^1\) He rewrote this equation as

\[
p(H) = R + F - RF, \tag{2}
\]

to highlight the additivity of the two processes. Mandler argued that predictions of dissociation between the two retrieval operations—circumstances in which manipulations only affect familiarity or only affect recollection—are hallmarks of this model.

The dual-retrieval perspective has been elaborated in various ways since Mandler’s (1980) seminal article. Four elaborations are of special significance with respect to our analysis of false-memory editing. The first is the introduction of the remember–know distinction (Tulving, 1985), according to which probes that are accepted via recollection generate a different phenomenology than probes that are accepted via familiarity. The specific idea was that recollection-based acceptance provokes “remember” phenomenology (a target’s prior presentation echoes in the mind’s ear or flashes in the mind’s eye), so that a probe is felt to exactly match a studied target, whereas familiarity produces “know” phenomenology (nonspecific feelings of resemblance between a probe and studied material), so that the probe is felt to be similar to studied material in some respects. This, in turn, suggested a simple approach to measuring the contributions of recollection and familiarity to recognition: Require participants to monitor and report phenomenology, with the mental states that are aroused by each accepted probe being classified as either “remember” (conscious awareness of specific concomitants of that probe’s prior presentation) or “know” (nonspecific feelings of resemblance between that probe and previously studied material).

The second elaboration is the accumulation of a large literature that was concerned with how such remember–know judgments react to selected manipulations, particularly manipulations that embody different process assumptions about recollection and familiarity (for reviews, see Donaldson, 1996; Gardiner & Java, 1991). If remember–know judgments supply direct estimates of the corresponding phenomenologies (and therefore of recollection and familiarity), a simple pattern of dissociations would be expected: Manipulations that affect processes that are unique to recollection ought to affect remember judgments but not know judgments, and manipulations that affect processes that are unique to familiarity ought to affect know judgments but not remember judgments. Instead, a complex pattern emerged in which (a) several manipulations affected remember judgments without affecting know judgments, (b) relatively few manipulations affected know judgments, (c) the first group of manipulations included variables that should influence the processing of surface content (e.g., full vs. divided attention) and variables that should influence the processing of semantic content (e.g., deep vs. shallow encoding instructions), and (d) the second group of manipulations consisted of variables that should influence semantic processing (e.g., words vs. nonwords, younger vs. older adults, masked priming).\(^2\)

The third elaboration is that more precise measurement of the two operations was achieved with modeling techniques, first the process-dissociation model (Jacoby, 1991) and later the conjoint-

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1 If Equation 1 were actually fit to data, it would be necessary to add a response-bias term, \((1 - R)(1 - F)\beta\), to Mandler’s (1980) basic expression, where \(\beta\) is the probability that bias produces acceptance when both recollection and familiarity fail to do so (also, see the section Quantitative Phenomena: Direct Measurement of Recollection Rejection), because response bias contributes to hit rates in most recognition situations. We assume this to be a baseline property of recognition that does not require repeated discussion. Therefore, in explicating the contributions of dual-retrieval operations to recognition, that response bias also contributes is understood but is only mentioned when there is reason to suppose that it is a confounding variable (e.g., see the section Inverted-U Relations Between Retrieval Time and Semantic False Recognition).

2 Following Mandler’s (1980) proposal that familiarity arises from a perceptual learning process that integrates targets’ surface features, it was sometimes proposed in early remember–know studies that know judgments respond to manipulations that affect the tendency to process surface information. However, our own review of this literature indicates that the common thread of these manipulations is that they affect the processing of meaning content. This conclusion is self-evident for words versus nonwords. It also seems obvious for younger versus older adults, considering the large corollary literature showing that the memory performance of older adults relies more on targets’ meaning content and less on their surface form than that of younger adults (e.g., Schacter et al., 1999; Tun, Wingfield, Rosen, & Blanchard, 1998). Concerning masked priming, it is true that this manipulation was once generally interpreted as inducing “unconscious perception” of targets (e.g., Jacoby & Whitehouse, 1989). More recently, however, it has been found that the processing of targets’ meaning content begins as early as 20–30 ms after stimulus onset (Seamons, Luo, & Gallo, 1998), so that masked priming produces unconscious semantic activation (Draine & Greenwald, 1998), and that words’ meanings are identified more rapidly than their surface forms (Stenberg, Lingren, Johansson, Olson, & Rosen, 2002; Wallace, Stewart, Shaffer, & Berry, 1998), so that the processing of surface content is selectively impaired, relative to the processing of meaning content, as presentation duration is shortened (McDermott & Watson, 2001).
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recognition model (Brainerd et al., 1998) and the dual-recall model
(Brainerd, Wright, Reyna, & Payne, 2002). In applications of the
first two models, it has usually been found that hits are chiefly
recollection based (for a review, see Brainerd, Reyna, & Mojardin,
1999), whereas Atkinson and associates (e.g., Atkinson & Juola,
1973) and Mandler (1980) had assumed that familiarity was the
predominant basis for hits. The principal motivation for develop-
ing these models, however, was that they are necessary to deal
with the fact that task-based methods of measuring recollection
and familiarity, such as remember–know judgments, may not be
process-pure (Jacoby, 1991). Findings such as those in the preced-
ing paragraph are surprising only if remember and know judg-
ments can be assumed to be uncontaminated measures of the

1. On standard immediate recognition tests, acceptances of
semantically related distractors were stochastically inde-
pendent of acceptances of their corresponding targets,
notwithstanding the marked meaning overlap.

2. On the same tests, there were positive dependencies
between acceptances of semantically related distractors
that shared meaning with a common target (e.g., PHOE-
NIX and DALLAS when HOUSTON was a target).

3. On immediate meaning-recognition tests, however, in
which the task was to ignore whether probes had actually
been studied and to accept any probe that preserved the
meaning of studied material, positive dependencies were
obtained between acceptances of semantically related
distractors and their corresponding targets (as well as
between semantically related distractors that shared
meaning with a common target).

4. When standard recognition tests were delayed until a few
days after study, an interval during which memory for the
surface form of experience degrades rapidly but memory
for meaning content remains reasonably intact (e.g.,
Gernsbacher, 1985; Murphy & Shapiro, 1994), the same
positive dependencies as with meaning-recognition tests
were obtained.

A dual-process interpretation that reconciles all of these findings
consists of two assumptions. First, target acceptances on immedi-
ate standard tests are dominated by a recollection operation that
processes representations of the surface form of studied material
(verbatim traces), but acceptances on meaning tests and on delayed
standard tests are dominated by a familiarity operation that pro-
cesses representations of meaning content (gist traces). Second, on
gist traces. Alternative interpretations that make contrasting as-
sumptions about the representational content that is processed by
recollection and familiarity—for instance, that recollection pro-
cesses semantic representations and familiarity processes surface
representations (Mandler, 1980), or that both types of representa-
tions are processed by both operations (Jacoby, 1996)—leave
some of these findings unexplained (Brainerd et al., 1999).

Recollection Rejection

Consider the typical semantic false-recognition design in which
participants study a list of unrelated words and respond to a
recognition test on which the test list contains some semantically
related distractors (e.g., PHOENIX and SOFA when HOUSTON
and COUCH were targets), as well as targets and unrelated dis-
tractors. Participants are instructed to accept only targets and are
told that some distractors will have meanings that are very similar
to those of targets. Although PHOENIX and SOFA should be very
likely to induce global familiarity reactions (“Yes, there was a
southwestern city on the list” and “Yes, there was some furniture
on the list”), these probes might sometimes access verbatim traces
of the corresponding targets, inducing recollection of those targets
(“No, it wasn’t PHOENIX, it was HOUSTON I heard” and “I
definitely remember hearing COUCH, not SOFA”). In such situ-
ations—either logically, on the basis of knowledge that the study
list did not contain subsets of semantically related words, or
intuitively, on the basis of a sense that the probe’s familiarity is
satisfactorily accounted for by the recollected target—recollection
of a related target can cause the probe to be rejected. It is this
process that we call recollection rejection.
It is important here to distinguish the differing effects of recollection and familiarity on responses to target probes versus distractors. Familiarity still supports acceptance of related distractors, which is a false-memory report. However, Brainerd, Reyna, and Kneer (1995) pointed out that target recollection can support rejection of distractors (as "new"), which is a true-memory report, by generating mismatches between the surface forms of distractor probes and recollected targets. Specifically, the simultaneous processing of two semantically related items, one of which is known to have been studied (the recollected target), can generate mismatches at the level of verbatim detail: COUCH (recollected target) and SOFA (distractor) strongly overlap in meaning, but they differ demonstrably in orthography and phonology; HOUSTON (recollected target) and PHOENIX (distractor) are both southwestern cities, but they differ demonstrably in orthography, phonology, and geographical location. Cognitively, mismatches in verbatim details provide a compelling argument of how probes whose meanings are very familiar (SOFA, PHOENIX) could nevertheless not have been presented (Brainerd, Reyna, Wright, & Mojardin, 2001). Indeed, for study lists composed of unrelated targets, a distractor probe that provokes such a verbatim mismatch supplies very strong evidence that it was not presented: Because participants are confident that the recollected target was presented and because the study list did not contain subsets of semantically related items, logically, the distractor probe could not have been on the list.3

Unlike lists of unrelated words, the people, objects, and actions that humans experience in everyday life are not unrelated to each other. On the contrary, the elements of people’s experience are often meaningfully connected, usually because they are features of common situations: The edibles that people ingest at a meal are meaningfully connected, sometimes intended to complement one another; many of the actions that people perform while driving to work are meaningfully connected in that they are necessary to operate motor vehicles; and so on. To emulate the relatedness of everyday experience experimentally, researchers can modify false-recognition designs so that study materials include groups of targets that are related in salient ways (e.g., ALBUQUERQUE, HOUSTON, and TUCSON might all appear on the study list). Reasoning as before, if a study list contains semantically related targets, a related distractor probe (PHOENIX) may induce recollection of one or more related targets. Except in the unusual circumstance that the exact number of related targets is known and all of them are recollected, recollection of semantically related targets is no longer indubitable evidence that a probe was not presented. Nevertheless, the joint fact that the presentation of the distractor cannot be recollected and the presentation of related targets can be increases the likelihood that the distractor was not presented and therefore provides a plausible, if not ineluctable, basis for rejecting it. Of course, the strength of such evidence decreases as the number of semantically related targets increases. Still, if a probe does not provoke recollection of itself but does provoke recollection of one or more related items, the latter are more likely to be targets than the probe.4 In short, even when participants have experienced multiple events that are semantically related to false events, recollections of true events still supply evidence against the false events (Reyna & Lloyd, 1997).

The three components of recollection rejection—the availability of verbatim traces of targets, the tendency of semantically related probes to provoke retrieval of verbatim traces of corresponding targets, and the perception that recollected targets differ in verbatim details from semantically related probes—provide a potential mechanism for editing false-but-gist-consistent information out of memory reports. We do not assume that this mechanism requires conscious awareness or deliberate strategic control, although both may sometimes be involved, especially in adults (see the section Measuring Recollection With ROCs). Because only minimal assumptions are made about representation (i.e., that verbatim and gist traces are available) and retrieval (i.e., that distractors sometimes cue retrieval of verbatim traces of semantically related targets), recollection rejection could operate early in life (Brainerd & Reyna, 2002), although known age improvements in retention of verbatim memories (Brainerd, Reyna, & Kneer, 1995; Reyna & Brainerd, 1998) and in the effectiveness of retrieval cues (Bjorklund & Muir, 1988) would surely increase the efficiency of this operation.

It is important to note that all three components of recollection rejection must be present in order for false-memory editing to occur via this mechanism. For example, it is not sufficient that a probe provokes recollection of some target: If HOUSTON and BOOK were studied and the probe PHOENIX provoked recollection of the unrelated target BOOK, this would not provide evidence against PHOENIX per se because it does not rule out PHOENIX on the basis of meaning similarity. Although it will not be explored in the present article, a second situation in which recollection may fail to edit out a false memory seems at first glance to be impossible: A distractor probe provokes recollection of the presentation of related targets, but simultaneously provokes remember phenomenology with respect to its own “presentation.” Although this seems impossible, recent research has suggested that in special circumstances, distractors can induce such phantom recollective experiences—vivid but illusory conscious remembrance of their appearance on the study list (see Footnote 3). Here, participants

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3 Surface mismatches between recollected targets and semantically related distractors would not supply a compelling basis for rejection if distractor “presentations” were also recollected. Such phantom “remem-ber” phenomenology has been found to be rare in most false-memory paradigms (Reyna & Brainerd, 1998). However, the possibility that this illusory phenomenology may sometimes impair recollection rejection cannot be dismissed because it is predicted to occur at relatively high levels under specific conditions—namely, when study materials repeatedly cue the same meanings and semantically related distractors are good examples of those meanings (Holliday, Reyna, & Hayes, 2002; Reyna, 1996, 2000; Reyna & Lloyd, 1997). Moreover, when those conditions are met, high levels of phantom “remember” phenomenology have sometimes been observed (see Payne et al., 1996; Roediger & McDermott, 1995). The procedure that has produced this result most consistently is the Deese-Roediger-McDermott paradigm (DRM; Deese, 1959; Roediger & McDermott, 1995). Although this paradigm provides an existence proof of high levels of phantom “remember” phenomenology under special conditions, the baseline situation, in everyday life as well as the laboratory, is one in which false reports are predominantly accompanied by nonspecific feelings of familiarity (for a review, see Reyna & Lloyd, 1997).

4 For a distractor, one cannot have a true recollection of its presentation, naturally. In some circumstances, however, participants respond as if they did, in fact, recollect the presentation of certain unpresented probes (see Footnote 3).
would presumably decide that both recollected targets and phantom-recollected distractors were presented.

In summary, recollection rejection suppresses the reporting of false-but-gist-consistent information when that information prompts recollection of semantically similar true information. The existence of such an editing operation would account for two findings about the semantic false-recognition effect that, as reviewers (e.g., Tussing & Greene, 1999) have noted, are not readily accommodated by traditional one-process familiarity explanations of this effect. The first finding is that the difference in false-alarm rates between related and unrelated distractors is ordinarily small when related distractors’ familiarity levels are high. For instance, suppose that the targets ALBUQUERQUE, HOUSTON, and TUCSON appear on the list. The familiarity level of PHOENIX will therefore be high, relative to unrelated distractors, and it may be close to the familiarity levels of the individual targets (because each target has been presented only once but the “southwestern city” meaning of PHOENIX has been instantiated three times). Nevertheless, the false-alarm rates for related and unrelated distractors ordinarily differ by only 5%–10%, and the difference is sometimes unreliable (e.g., Tussing & Greene, 1999). Such modest differences are understandable if increased distractor familiarity (which increases false-alarm rates) is neutralized to a considerable degree by increased availability of verbatim traces of corresponding targets (which supports higher recollection rejection rates).

The other finding concerns the effects of repetition during the study phase. Target repetition should increase the familiarity of related distractors (PHOENIX will be more familiar than RUBY if HOUSTON is presented five times and DIAMOND is presented once). However, the semantic false-recognition effect has not been found to increase as a function of repetition in normal child and adult populations (Braiderd, Reyna, & Kneer, 1995; Hall & Kozloff, 1970; Shiffrin, Huber, & Marinelli, 1995; Tussing & Greene, 1999, Experiments 1–4), and sometimes it has been found to decrease (Braiderd & Reyna, 1996; Seamon et al., 2002; Tussing & Greene, 1999, Experiment 5). Such findings are remarkable if distractor responses are based solely on familiarity, but they make sense if repetition of targets simultaneously increases the availability of verbatim traces. With added assumptions, of course, it is possible to formulate a one-process familiarity explanation of these results, as well as the finding of modest semantic false recognition when distractor familiarity is high. Indeed, Tussing and Greene (1999) pointed out that Shiffrin, Ratcliff, and Clark’s (1990) expansion of Gillund and Shiffrin’s (1984) search of associative memory (SAM) model can handle such data.

Qualitative Phenomena: Indirect Measurement of Recollection Rejection

Recollection rejection is anticipated on theoretical grounds, and its existence explains some perplexing findings about a classical laboratory demonstration of false memory. However, does it predict new effects that can be confirmed in data? Although many effects would be consistent with recollection rejection, the most instructive ones follow from key components of the process theory. Because recollection rejection cannot occur unless verbatim traces are available in memory and unless related distractors sometimes cue the retrieval of such traces, effects that follow from these components of the process theory are especially informative. In the present section, we consider evidence that bears on two classes of such theory-specific effects: recollective suppression of semantic false recognition and nonmonotonic relations between retrieval time and semantic false recognition. Although these classes of effects differ greatly in their empirical details, both are expected to occur for the types of memory tests that afford the greatest degree of experimental control over the content of false memories, namely, recognition tests.

Recollective Suppression of Semantic False Memory

Distractors that preserve the meaning of experience produce higher false-alarm rates than unrelated distractors. The process theory of recollection rejection forecasts that increased recollection rejection (and decreased false-alarm rates) should result from (a) manipulations that increase the tendency of meaning-preserving distractors to provoke retrieval of verbatim traces of related targets while leaving the retrievability of gist unchanged, and (b) manipulations that enhance the retrieval of verbatim traces more than the retrieval of gist traces (Reyna & Lloyd, 1997). We summarize evidence generated by two groups of manipulations that should have one or the other of these effects: test-phase cuing of verbatim traces and study-phase strengthening of verbatim traces.

Augmenting the Accessibility of Verbatim Traces at Test

Experiments using test-phase cuing of verbatim traces have focused both on false recognition of meaning-preserving sentences (Braiderd & Mojardin, 1998; Lim, 1993; Reyna & Kieran, 1994, 1995; e.g., "The table is below the light" when "The light is above the table" was a target) and on false recognition of meaning-preserving words (Braiderd, Reyna, & Kneer, 1995; Brainerd et al., 1999; Seamon, Luo, Schlegel, Greene, & Goldenberg, 2000). Across these articles, two manipulations were studied that should affect the ability of meaning-preserving distractors to access verbatim traces of their corresponding targets: (a) target priming of related distractors (testing a distractor’s instantiating target [e.g., COUCH] just before the distractor itself is tested [e.g., SOFA]) and (b) varying the delay between study and the presentation of retrieval cues. The rationale for the first manipulation is that targets themselves are the best retrieval cues for their verbatim traces—so that target probes make their verbatim traces more accessible for a time, increasing recollection rejection rates if related distractors are tested immediately thereafter (Braiderd, Reyna, & Kneer, 1995). The rationale for the second manipulation is the aforementioned notion that, as time passes, memory for the surface details of experience becomes inaccessible more rapidly than memory for the gist of experience, so that a test probe’s effectiveness as a verbatim retrieval cue is enhanced, relative to its effectiveness as a gist retrieval cue, as the interval between study and test is decreased (Reyna & Kieran, 1994).

5 Because false-alarm rates for unrelated distractors are usually low and because verbatim traces are less durable and more subject to interference than gist traces (Braiderd & Reyna, 1993), it is particularly important, in an experimental test of this prediction, that a manipulation provoke high levels of verbatim retrieval. Otherwise, the prediction may fail for the uninteresting reason that the test was not sensitive enough.
Both manipulations have been found to reduce semantic false-recognition effects, providing convergent evidence that such effects do decline as targets’ verbatim traces become more retrievable. Concerning the target-priming manipulation, Brainerd, Reyna, and Kneer (1995) and Brainerd et al. (1999) found that priming reduced semantic false recognition when related distractors were antonyms, category labels, same-category exemplars, or synonyms of targets. Further, in some conditions, target priming produced slightly lower false-alarm rates for related distractors than for unrelated distractors (i.e., a reversal of the false-recognition effect). The latter result is highly diagnostic of recollection rejection because the present theory specifically allows for such reversals if related distractors provoke verbatim retrieval much more often than gist retrieval (Reyna & Brainerd, 1995; Reyna & Lloyd, 1997), whereas one-process familiarity theories predict that the false-alarm rate for related distractors approaches the rate for unrelated distractors as a lower limit (Brainerd et al., 2001; see Footnote 5, present study).

Concerning the delay manipulation, Reyna and Kiernan (1994, 1995) reported some experiments in which participants studied a series of sentences, followed by immediate recognition tests (targets, meaning-preserving distractors, meaning-violating distractors) and 1-week delayed tests. Across their experiments, spreads between false-alarm rates for meaning-preserving versus meaning-violating distractors were consistently greater on delayed tests than on immediate tests. Brainerd and Mojardin (1998) obtained similar results in a sentence-recognition experiment in which there were immediate recognition tests, 1-week delayed tests, and 1-month delayed tests; and Lim (1993) obtained similar results in a sentence-recognition experiment in which there were immediate and 1-week delayed tests. Brainerd, Reyna, and Kneer (1995); Brainerd and Reyna (1996); and Seamon et al. (2000) also found, in word recognition experiments, that spreads between false-alarm rates for semantically related distractors versus unrelated distractors were greater on delayed tests when delayed tests were administered from 3 days (Seamon et al., 2000) to 2 weeks (Brainerd & Reyna, 1996) following list presentation.

**Strengthening Verbatim Traces During Study**

The accessibility of verbatim traces could also be increased by strengthening them via encoding manipulations, which has been done in two ways: by varying the rate of presentation of target materials and by varying the content of such materials. We summarize findings for presentation rate before summarizing findings for content manipulations.

**Presentation rate variations.** This manipulation is derived from research on the time course of surface and semantic processing during target presentation. That research suggests that when targets are familiar words, memory processing during presentation involves two stages: a recognition stage that focuses on meaning and a postrecognition stage that focuses on surface form (see Wallace, Stewart, & Malone, 1995; Wallace, Stewart, Shaffer, & Barry, 1998). During the first stage, processing focuses on identifying a presented item by determining whether its meaning is known. Once an item is identified, processing focuses on its surface structure, leading to the formation of verbatim traces that serve to distinguish the presented item (e.g., DRESS) from similar unpresented items (e.g., GOWN, SKIRT). The key consideration, from the standpoint of recollection rejection, is that the gist traces that are responsible for semantic false recognition are stored earlier than the verbatim traces that support recollection rejection. Hence, rapid presentation rates should selectively impair verbatim traces, reducing recollection rejection and increasing false-recognition effects (assuming that presentation rates are long enough for the recognition stage to be completed).

An early experiment on presentation rate involved false recall rather than false recognition and was reported by Toglia and Neuschatz (1996). Participants studied a series of Deese/Roediger/McDermott (DRM) lists, a type of list in which all of the targets are semantically related (e.g., NURSE, SICK, MEDICINE, HEALTH, HOSPITAL, DENTIST, PHYSICIAN, ILL, PATIENT, OFFICE, STETHOSCOPE, SURGEON, CLINIC, CURE) and are forward associates of a critical unpresented word (DOCTOR). These lists produce strong semantic false-recognition effects in which false-alarm rates for critical unpresented words approach or even exceed hit rates. In Toglia and Neuschatz’s experiment, the presentation rate was both the lists and for the other half.

Consistent with prediction, the 4-s rate produced less false recall of semantic associates than the 1-s rate.

The effect of presentation rate on false recognition was studied, again using DRM lists, by Seamon, Luo, and Gallo (1998) and by Buchanan, Brown, and Westbury (1999). Seamon et al. exposed targets on individual DRM lists for 20 ms, 250 ms, or 2,000 ms per word. False-alarm rates varied nonmonotonically, first increasing from .77 (20 ms) to .89 (250 ms) and then decreasing to .76 (2,000 ms). This inverted-U relation is consistent with the dual processes posited in the present theory: Twenty milliseconds is presumably too short for recognition-stage semantic processing to occur, so that false alarms initially increase as semantic processing is completed but then decrease as slower postrecognition verbatim processing is completed. Buchanan et al.’s methodology was different in that each DRM list was presented three or six times and was followed by a recognition test for that list. Lists were presented at a 72-ms rate for half the participants and a 2,000-ms rate for the other half. Like Seamon et al.’s results for 250 ms versus 2,000 ms, Buchanan et al. found that false-alarm rates for critical unpresented words were lower following 2,000-ms presentation than following 72-ms presentation.

Unfortunately, neither Seamon et al.’s (1998) nor Buchanan et al.’s (1999) data provide unambiguous evidence, because variations in presentation rate did not alter the size of the semantic false-recognition effect (the spread between the false-alarm rates for related and unrelated distractors). Although the false-alarm rate for related distractors decreased when presentation rate increased from 72 ms to 250 ms or from 250 ms to 2,000 ms, the false-alarm rate for unrelated distractors decreased by comparable amounts. Thus, the decrease for related distractors might be due to decreases in response bias, rather than to increases in recollection rejection. However, the response bias interpretation does not apply to recall data, and as noted, Toglia and Neuschatz (1996) found that false recall of critical unpresented words decreased as presentation duration was lengthened. This finding was replicated and extended by McDermott and Watson (2001), who presented DRM lists at rates of 20, 250, 1,000, 3,000, or 5,000 ms per word. False recall of critical unpresented words decreased as presentation duration increased from 250 ms (.31) to 1,000 ms (.22) to 3,000 or 5,000 ms (.14). The inverted-U relation between duration and semantic false
memory that was observed by Seamon et al. (and that is suggestive of dual processes) was also observed by McDermott and Watson. Although false recall decreased as duration was lengthened beyond 250 ms, it increased (from .14 to .31) as duration was lengthened from 20 ms to 250 ms. In addition to the two-process account of this effect that we have proposed, McDermott and Watson argued that, for DRM lists at least, inverted-U relations are consistent with an alternative two-process account in which the first stage involves spreading activation within an associative network and the second stage involves monitoring the origin of memories (presentation vs. associative activation).

Target content variations. This type of manipulation has been used by Reyna and Kiernan (1994, 1995), Israel and Schacter (1997), and Schacter et al. (1999). In Reyna and Kiernan’s experiments, participants were presented with a series of three-sentence narratives (e.g., The flowers are on the table. The table is above the light. The flowers are in a green pot.), followed by recognition tests for targets, meaning-preserving distractors (e.g., The light is above the table.), and meaning-violating distractors (e.g., The flowers are under the table.). These authors reasoned, on the basis of findings of earlier research, that narratives that specified linear magnitude relations between objects (e.g., The coffee is hotter than the tea. The tea is hotter than the cocoa. The cocoa is sweet.) would yield stronger, more accessible verbatim traces than sentences that specified spatial relations between objects (e.g., previous example). In fact, Reyna and Kiernan found that linear target sentences were recognized more accurately than spatial target sentences. Consistent with the notion that better verbatim accessibility enhances recollection rejection, the spread between the false-alarm rates for distractors that preserved the meaning of narratives and distractors that did not was smaller for linear narratives (.27 overall) than for spatial narratives (.44 overall).

Similarly, Reyna and Kiernan (1995) concluded, following a review of the literature on metaphor comprehension, that available data showed that when narratives contain a metaphorical sentence, participants store especially robust verbatim traces of that sentence (e.g., The woman was shopping in the grocery store. The woman saw the lost boy near the door. The woman was an aspirin, kneeling by the lost boy.) Thus, recollection rejection rates should be particularly high, and the semantic false-recognition effect should be particularly low, for metaphorical distractors that are related to metaphorical targets (e.g., The woman was medicine, kneeling by the lost boy) or literal distractors that are related to metaphorical targets (e.g., The woman made him feel better, kneeling by the lost boy). To test this prediction, Reyna and Kiernan presented several narratives that contained a single metaphorical statement and two literal statements. Participants responded to recognition tests containing metaphorical targets, metaphorical and literal distractors that preserved the meanings of metaphorical targets, and metaphorical and literal distractors that violated these meanings. Across experimental conditions, false-alarm rates for meaning-preserving distractors were very low (.08 overall), and the spread between the false-alarm rates for meaning-preserving and meaning-violating distractors was very small (.04 overall). The semantic false-recognition effect was not statistically reliable in most conditions on immediate tests.

A more recent content manipulation was introduced by Israel and Schacter (1997). They argued on the basis of prior studies of presentation formatting (e.g., Paivio, 1971) that verbatim traces should be stronger if targets are encoded in a visually distinctive format—specifically, as pictures rather than as printed words. They presented spoken DRM lists accompanied by visual presentations of targets, either pictures of the named objects (picture plus word condition) or printed versions of the words (letters plus word condition). On recognition tests, all probe words were again spoken aloud, with half being only spoken (context-mismatch probes) and half being accompanied by whatever visual support (pictures or letters) had accompanied targets at study (context-match probes). If pictures produce stronger verbatim traces than printed words, recollection rejection rates should be higher (and semantic false recognition should be lower) when critical unpresented words are tested following picture plus word presentation than following letter plus word presentation. Averaging over Israel and Schacter’s two experiments, the mean false-alarm rate for critical unpresented words was .26 following picture plus word presentation and .37 following letter plus word presentation, and the spread between the false-alarm rates for these distractors and unrelated distractors was smaller following picture plus word presentation than following letter plus word presentation (.20 vs. .34 overall). The latter finding indicates that the first is not an artifact of response bias. This basic pattern was replicated by Schacter et al. (1999), and further, the reduction in semantic false-recognition following picture plus word presentation was found to be larger in elderly adults than in young adults.

Schacter et al. (1999) interpreted their data as supporting a metacognitive false-memory editing strategy that they called the distinctiveness heuristic, rather than item-level suppression (i.e., recollection rejection). Although they acknowledged that item-level suppression may have contributed to their results, they proposed that in this particular paradigm, pictorial targets produce such vivid memories of their presentation that participants in the picture plus word condition expect that target probes will provoke recollections of distinctive visual details of their presentation. Hence, participants adopt the metacognitive strategy of rejecting probes that are not accompanied by such vivid visualizations, which disproportionately affects probes for which such recollections are not possible (distractors). Participants in the letter plus word condition are less likely to adopt this strategy because they do not anticipate that target probes will provoke realistic visualizations. The empirical basis for this proposal was the finding that the suppressive effect of picture plus word presentation was not reliable when presentation mode was varied within, rather than between, participants (see Dodson & Schacter, 2002, for similar findings). Although the distinctiveness heuristic may be the correct explanation of this particular suppression effect, it is not incompatible with a recollection rejection explanation of other types of suppression effects, including pictorial suppression in other paradigms and suppression from other visual distinctiveness manipulations in the DRM paradigm. In the latter connection, Arndt and Reder (2003) recently reported a series of experiments that resembled those of Schacter and associates, except that the visual distinctiveness manipulation consisted of presenting DRM lists in unusual fonts (e.g., Curlz, Matisse), whose surface forms should have attracted close attention (thereby producing stronger verbatim traces of targets). Consistent with recollection rejection, this manipulation consistently suppressed false recognition of critical unpresented words when it was varied within participants. Further, the distinctiveness heuristic cannot account for suppression effects.
that result from other types of within-participant manipulations that should yield stronger verbatim traces, (e.g., metaphorical targets, linear vs. spatial content, leisurely presentation rates, immediate memory tests). Thus, rather than challenging recollection rejection, a more likely interpretation is that the vivid memories that were induced by picture plus word presentation caused the distinctiveness heuristic to be adopted when it was inappropriate (within-participant manipulation), as well as when it was appropriate (between-participant manipulation). Consistent with this interpretation, Dodson and Schacter (2002) found that, indeed, participants adopt this strategy when it is not fully diagnostic of whether probes were presented.

A further consideration that bears on the compatibility of the distinctiveness heuristic with recollection rejection is the likely ontogenetic relation between them. In the developmental literature, the emergence of metacognitive knowledge about factors that influence remembering is viewed as a by-product of specific, item-level experiences with the effects of those factors on memory performance (e.g., Wellman, Ritter, & Flavell, 1975). Because the components of the process definition of recollection rejection are well within the known capabilities of young children, a straightforward hypothesis would be that early experience with recollection rejection contributes to later metacognitive understanding that vivid sensory phenomenologies can supply a reliable basis for distinguishing true memories from false ones. Recent developmental studies of false-memory editing have provided support for this hypothesis (Brainerd & Reyna, 2002).

Summary
Accumulated findings on recollective suppression of semantic false recognition can be summarized in three broad statements. First, there is evidence that semantic false recognition can be suppressed by manipulations that enhance the accessibility of verbatim traces of presented material, relative to gist traces. Second, two types of test-phase cuing manipulations that should selectively enhance verbatim retrieval have produced the predicted suppression effect: target priming of related distractors just before distractors are tested and varying the delay between study and test. Third, two types of study-phase manipulations that should selectively enhance verbatim accessibility on later memory tests have produced the predicted suppression effect: increasing the exposure duration of targets and presenting targets that yield especially robust verbatim traces (metaphorical sentences, pictures).

Inverted-U Relations Between Retrieval Time and Semantic False Recognition
The effects that were reviewed in the previous section follow from the principle that making verbatim traces more accessible will increase recollection rejection. Another qualitative phenomenon, inverted-U relations between retrieval time and semantic false recognition, follows from the principle that recollection is a slower process than familiarity. In Atkinson and associates’ (e.g., Atkinson & Juola, 1973, 1974) original formulation, global familiarity evidence is retrieved more rapidly than item-specific recollective evidence (for related proposals about reasoning, see Reder & Ritter, 1988, 1992). As both types of evidence favor hits, memory support for hits should increase monotonically as a function of retrieval time (Gillund & Shiffrin, 1994; Mulligan & Hirshman, 1995). In contrast, as the two types of evidence favor opposing decisions about related distractors, memory support for false alarms should vary as an inverted-U function of retrieval time, increasing at first (as familiarity evidence accumulates) and then decreasing (as recollective evidence begins to accumulate). Fuzzy-trace theory’s assumptions about the representational content of the two types of evidence provide an explanation of the initially more rapid accumulation of familiarity evidence. As previously noted, related distractors are assumed to be poorer retrieval cues for verbatim traces of their corresponding targets than for gist traces (Brainerd, Reyna, & Kneer, 1995). Thus, more time should be required, on average, for related distractors to gain access to verbatim traces than to gain access to gist traces, leading to the prediction that false-alarm rates will initially increase, as retrieval time lengthens, but will eventually decrease.

Dodson’s (1984a, 1984b; Dodson & Rosedale, 1991) response-signal paradigm has been used to investigate this prediction. In this paradigm, participants make recognition decisions within some short interval (e.g., 300 ms) following a signal to respond, with the signal being presented at various lags following probe onset (e.g., 100, 300, and 500 ms). In two of these studies (Dodson, 1984b; Dodson & Rosedale, 1991), participants studied lists of word pairs in which either (a) all pairs were composed of unrelated words (e.g., OPEN–VEGETABLE) or (b) some pairs were unrelated but others were semantically related (e.g., CHANCE–OPPORTUNITY). On recognition tests, some distractors were unrelated pairs (e.g., HIRED–SUPPRESS) and others were related pairs (e.g., PURSUE–FOLLOW). Dodson and Rosedale obtained the predicted inverted-U relation for a type of distractor in which words were semantically related to each other and one of the words had been part of a studied pair (e.g., JOY–SORROW, when the pair JOY–SHELL had been studied). For such pairs, false-alarm probability increased as retrieval time was lengthened from 100 to 300 ms but decreased as retrieval time was lengthened from 300 to 3500 ms.

The predicted inverted-U relation has been detected in other experiments involving pair recognition (Gronlund & Ratcliff, 1989; Rotello & Heit, 2000) and in experiments involving item recognition (Hintzman & Curran, 1994). In Gronlund and Ratcliff’s experiments, participants studied mixed lists of word pairs (AB) and singletons (C). Test lists were composed of both types of targets: of unrelated distractor pairs (XY) and unrelated singletons (Z); and of three types of related distractor pairs: AB’ (both words presented, but not in the same pair), AX (first word presented, second unordered), and XB (second word presented, first unordered). Response-signal lags in the 350–3,000-ms range were used. In one experiment, participants were instructed that, following the signal, they should accept studied singletons and pairs in which both words had been studied (regardless of whether they had been studied together). In another experiment, participants were instructed that they should accept studied singletons and pairs in which both words had been studied together. Thus, AB, AB’, and C were targets in the first experiment, and AX and XB were related distractors. In the second experiment, AB and C were targets, and AB’, AX, and XB were related distractors. The relation between false-alarm rates for related distractors and total retrieval time (signal lag + response latency) is shown in Figure 1, where it can be seen that the inverted-U relation was present in
both experiments. Rotello and Heit (2000) replicated the inverted-U relation in two experiments, using slightly different designs. In one experiment, participants studied word pairs and responded to recognition tests composed of targets (AB); distractors in which both words had been presented, but not in the same pair (AB'); and distractors in which neither word had been presented (XY), using signal lags of 100–2,000 ms. In a second experiment, the recognition test was composed of six types of probes: target pairs (AB); distractor pairs in which both words had been presented, but not in the same study pair (AB'); distractor pairs in which neither word had been presented (XY); single words that had been presented as part of a pair (A); unpresented single words that were synonyms of presented words (A'); and unpresented words that were unrelated to presented words (Z).

Hintzman and Curran (1994) investigated the relation between false-alarm rates and signal lag for individual words, rather than pairs. In two experiments, one with signal lags of 100–1,200 ms and another with signal lags of 100–2,000 ms, participants studied lists on which target words were presented either once or twice. On recognition tests, related distractors resembled targets in both surface form and meaning; they were generated by pluralization (e.g., COMPUTERS was a distractor corresponding to the target COMPUTER) or singularization (e.g., PAINTING was a distractor corresponding to the target PAINTINGS). When false-alarm probabilities were plotted against total retrieval time, distractors that were related to twice-presented targets displayed the predicted inverted-U relation in both experiments, and distractors that were related to once-presented targets displayed this relation in one experiment.

Although the predicted inverted-U relation has been obtained for both pair and item recognition, Rotello and Heit (1999) argued that variations in response-criterion stringency (as opposed to variations in recollection rejection) might be responsible. The inverted-U relation could be explained by familiarity retrieval alone, if more stringent response criteria are adopted at longer signal lags. Specifically, if participants adopt a liberal acceptance criterion for signal lags that fall below some threshold value (e.g., 500 ms) and adopt a more conservative one for longer lags, inverted-U relations could result without the operation of a slower recollective process: False-alarm rates would increase up to the threshold value (as familiarity evidence accumulates and the liberal response criterion is used) but would eventually decrease (as familiarity reaches asymptote and the conservative criterion is implemented). Rotello and Heit (1999) reexamined relations between signal lag and false-alarm rates using signal detection statistics to determine whether criterion shifts could account for inverted-U relations. They found that criterion shifts could account for the inverted-U relations that Hintzman and Curran obtained for item recognition, and the inverted-U relations that Rotello and Heit themselves obtained in two further item-recognition experiments using Hintzman and Curran’s lists. However, they found that inverted-U relations for pair recognition (e.g., Gronlund & Ratcliff, 1989) could not be explained by response criterion shifts; these relations were preserved when criterion shifts were controlled with signal-detection statistics.

In summary, published data on inverted-U relations between retrieval time and false-alarm rates are clear-cut for word pairs. The predicted pattern has been observed in multiple experiments, and response-criterion shift explanations have been ruled out. The picture for item recognition is less clear. Inverted-U relations have been observed for raw false-alarm rates, but those relations were not preserved with signal detection statistics. However, the null results reported by Rotello and Heit (1999) may simply mean that response-signal methodology is not sensitive enough to detect recollection rejection in item recognition. This interpretation is supported by the fact that the two quantitative methodologies that we introduce in the next section (conjoint recognition and ROC analysis) have both produced evidence of recollection rejection in item recognition.

Quantitative Phenomena: Direct Measurement of Recollection Rejection

The lines of evidence that have been reviewed thus far are examples of indirect measurement of a memory process. The dependent variables in all studies (false reports of unpresented material) do not yield direct measurements of the levels of recollective retrieval that were present in different experimental conditions. Instead, the strongest measurement assumption that could be made is that levels of false reporting were monotonically related to underlying levels of recollective retrieval; there was an ordinal relation such that false reporting decreased across conditions as recollective retrieval increased (e.g., Dunn & Kirsner, 1988; Howe, Rabinowitz, & Grant, 1993). It is obvious, however, that even this assumption must be qualified because the effects of target recollection were consistently confounded with those of familiarity. Dual-retrieval models imply that both processes affect false-reporting rates. Consequently, a monotonic measurement assumption about recollection is only justified if levels of familiarity are invariant across those conditions. Otherwise, failures to detect predicted variations in false reporting may be Type II errors caused by familiarity confounds (i.e., predicted effects are masked by uncontrolled influences of familiarity).

That predicted effects may be masked by familiarity confounds is easy to see for both recollective suppression of false recognition and retrieval time/false-alarm relations. For recollective suppression, manipulations that make verbatim traces more accessible on memory tests might also increase the accessibility of gist traces of
targets’ meaning content. Even when a manipulation has disproportionate effects on recollection, as would be expected for the manipulations discussed above, this does not guarantee reductions in false-alarm rates unless the response scales for recollection and familiarity are reasonably similar. If not, a larger change in recollection may have a smaller effect on false-alarm rates than a smaller change in familiarity (see Howe et al., 1993, for examples of how differences in response scales affect comparisons of performance variables that are controlled by multiple memory processes). Much the same concern arises with retrieval time/false-alarm relations. For instance, Rotello (2001) proposed that the recollective experiences that are provoked by related distractors may be confusing to participants (because target recollection is normally a basis for target acceptance) and sometimes cause them to adopt irrelevant response strategies. If so, increases in recollection would be less effective at decreasing false-alarm rates than increases in familiarity would be at increasing them, making nonmonotonic retrieval time/false-alarm relations difficult to observe, notwithstanding that theoretical assumptions about the two processes are correct.

These confounds cannot be eliminated, in principle, as long as predictions about recollection rejection are tested with dependent variables that are contaminated by familiarity influences. To eliminate such confounds, one must measure the respective levels of recollection and familiarity in different experimental conditions, thereby separating and quantifying their contributions to false reports. There are currently two methodologies that deliver such measurements: the conjoint-recognition procedure and ROCs for related distractors. We consider each methodology, presenting key findings on recollection rejection that each has produced.

Measuring Recollection Rejection With Conjoint-Recognition Methodology

Separating the Contributions of Target Recollection and Familiarity to False Reports

In conjoint-recognition methodology, separate estimates of recollection rejection and familiarity are obtained for semantically related distractors by requiring participants to respond to recognition tests under instructional conditions in which recollection rejection should lead to different responses. The specific instructional conditions are: verbatim (accept only targets), gist (accept only semantically related distractors), and verbatim plus gist (accept both targets and semantically related distractors). As part of each instruction, participants are informed that test probes will include distractors that are related to the studied material in specific ways (e.g., they are synonyms of targets), as well as targets and unrelated distractors. Therefore, being able to recollect the presentation of a distractor’s corresponding target supports rejection of related distractors in the first condition, but acceptance in the other two.

The initial step in using this methodology to measure recollection rejection and familiarity is the observation (Brainerd et al., 1998) that Mandler’s (1980) expression for targets can be adapted for semantically related distractors as

\[ 1 - p_{rd} = T_{rd} + (1 - T_{rd})(1 - F_{rd}), \]

or equivalently,

\[ p_{rd} = (1 - T_{rd})F_{rd}, \]  \hspace{1cm} (4)

where \( 1 - p_{rd} \) is the empirical probability that a related distractor will be correctly rejected; \( p_{rd} \) is the probability that a related distractor will be falsely accepted; \( T_{rd} \) is the probability that a related distractor provokes retrieval of the verbatim trace of a semantically related target, leading to rejection of the probe; and \( F_{rd} \) is the probability that evaluation of the familiarity of a related distractor produces acceptance. Thus, \( T_{rd} \) is the probability of correct rejection on the basis of recollection of corresponding target presentations, and \((1 - T_{rd})(1 - F_{rd})\) is the probability of correct rejection on the basis of lack of confirmatory evidence (i.e., familiarity fails) and lack of disconfirmatory evidence (i.e., target recollection fails).

Next, numerical estimates of recollection rejection and familiarity are obtained in conjoint-recognition designs by adapting Mandler’s (1980) expression for targets for each instructional condition:

\[ p_{rd,V} = (1 - T_{rd})F_{rd} + (1 - T_{rd})(1 - F_{rd})\beta_{V}, \]  \hspace{1cm} (5)

\[ p_{rd,G} = T_{rd} + (1 - T_{rd})F_{rd} + (1 - T_{rd})(1 - F_{rd})\beta_{G}, \]  \hspace{1cm} (6)

and

\[ p_{rd,VG} = T_{rd} + (1 - T_{rd})F_{rd} + (1 - T_{rd})(1 - F_{rd})\beta_{VG}, \]  \hspace{1cm} (7)

where \( p_{rd,V}, p_{rd,G}, \) and \( p_{rd,VG} \) are the distractor acceptance probabilities for the respective instructional conditions and \( \beta_{V}, \beta_{G}, \) and \( \beta_{VG} \) are the response-bias rates for these conditions. These equations preserve two assumptions: (a) Responses supported by recollection take precedence over responses supported by familiarity (when the two operations support different responses), and (b) recollection rejection and familiarity are independent processes (and therefore the parameters \( T_{rd} \) and \( F_{rd} \) are independent). Concerning the term \((1 - T_{rd})F_{rd}\), which appears in all three expressions, the model assumes that when a probe provokes familiarity without recollection, participants treat the probe as an example of whatever type of item is specified for acceptance in the instructions (see Brainerd et al., 1999). The response bias parameters are defined as

\[ p_{rd,V} = \beta_{V}, \]  \hspace{1cm} (8)

\[ p_{rd,G} = \beta_{G}, \]  \hspace{1cm} (9)

and

\[ p_{rd,VG} = \beta_{VG}, \]  \hspace{1cm} (10)

where \( p_{rd,V}, p_{rd,G}, \) and \( p_{rd,VG} \) are the false-alarm probabilities for unrelated distractors in the respective conditions. A tree diagram for these expressions appears in Figure 2. When the expressions are implemented in a likelihood function, \( T_{rd} \) and \( F_{rd} \) can be independently estimated and statistical hypotheses about the values of these parameters can be evaluated.

Two features of this model should be noted that affect the interpretation of estimates of \( T_{rd} \) and \( F_{rd} \). First, the model assumes that the values of \( T_{rd} \) and \( F_{rd} \) are the same in each instructional condition. Brainerd et al. (1999, Equations 22 and 23) showed that this assumption implies the testable constraint \( p_{rd,G} = 1 - (1 - p_{rd,V})(1 - p_{rd,VG})(1 - p_{rd,VG}) \). Goodness-of-fit tests for the model
implement this assumption, and Brainerd et al. (1998, 1999) reported a series of experiments in which those tests produced acceptable fits. The same tests produced acceptable fits in the experiments that we consider below, although, in the interests of brevity, we do not report these goodness-of-fit analyses. Second, the model does not build in any constraints on the estimates of \( T_{rd} \) and \( F_{rd} \) that are obtained from experimental data. In this connection, Brainerd et al. (1999) showed that Equations 5–7 have the property of identifiability, which means that in sample data, estimates that are obtained for one parameter are mathematically independent of the estimates that are obtained for other parameters. Thus, for instance, when a manipulation increases (or decreases) the value of \( T_{rd} \), the model does not force the value of \( F_{rd} \) to decrease (or increase); instead, \( F_{rd} \) may increase, decrease, or remain invariant, depending on the manipulation’s actual effects on familiarity.

Because numerical estimates of \( T_{rd} \) can be secured with conjoint-recognition methodology, theory-driven predictions about recollection rejection can be tested. The most straightforward one is that if recollection rejection occurs at reliable levels for the types of related distractors that produce false-memory reports, the null hypothesis \( H_0: T_{rd} = 0 \) should be rejected. If it is, it can then be determined whether estimates of \( T_{rd} \) are affected by manipulations that ought to increase distractors’ tendency to provoke recollection of target study events.

Results From Prior Experiments

Before presenting new data, we summarize findings from three experiments by Brainerd et al. (1999), an experiment by Brainerd et al. (1998), an experiment by Rotello (2001), and two experiments by Brainerd and Reyna (2002). The Brainerd et al. (1999) experiments focused on three types of semantically related distractors: synonyms (Experiment 1), antonyms (Experiment 2), and category names (Experiment 3). Adults studied lists of familiar words and then responded to recognition tests under verbatim, gist, or verbatim plus gist instructions that did not inform them that a recollection rejection strategy could be used to reject related distractors (in the verbatim condition) or accept them (in the other conditions). In Experiments 1 and 2, half of the words were presented once and half were presented twice. Rates of recollection rejection and familiarity were estimated with Equations 5–7. The featured manipulation, from the perspective of recollection rejection, was repetition. Rates of recollection rejection (i.e., values of parameter \( T_{rd} \)) should be greater for synonyms and antonyms.

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**Figure 2.** Tree diagram for Equations 5 (verbatim instructions; top), 6 (gist instructions; middle), and 7 (verbatim plus gist instructions; bottom). The tree diagram for Equation 6 is obtained from that for Equation 5 by replacing \( \beta_v \) with \( \beta_g \) and by replacing the first response outcome (Reject) with Accept. The tree diagram for Equation 7 is obtained from that for Equation 5 by replacing \( \beta_v \) with \( \beta_{vg} \) and by replacing the first response outcome (Reject) with Accept. \( T_{rd} \) = the probability that a related distractor provokes retrieval of the verbatim trace of a semantically related target, leading to rejection of the probe; \( F_{rd} \) = the probability that evaluation of the familiarity of a related distractor produces acceptance; \( \beta_v \), \( \beta_g \), and \( \beta_{vg} \) = the response-bias rates for the verbatim, gist, and verbatim plus gist conditions, respectively.
whose corresponding targets were studied twice than for synonyms and antonyms whose corresponding targets were studied once, because repetition should strengthen the labile verbatim traces that support recollection rejection more than it strengthens the gist memories that support familiarity (Brainerd, Reyna, & Kneer, 1995; Kensinger & Schacter, 1999).

Estimates of recollection and familiarity for the related distractors in these experiments are shown in Table 1. All estimates of recollection rejection exceeded zero, consistent with the hypothesis that related distractors sometimes induce principled rejection based on target recollection (the mean value of $T_{rd}$ was .31). Therefore, nearly one third of the time overall, related distractors produced recollection rejection. A second important finding is that repetition had the predicted effect on recollection rejection. The mean value of $T_{rd}$ was .22 for distractors whose corresponding targets had been studied once, but it was .41 when corresponding targets had been studied twice. A third finding, which is expected on the ground that target recollection overridges familiarity, was that repetition decreased $F_{rd}$ as it increased $T_{rd}$.

The design of Brainerd et al.’s (1999) third experiment was somewhat different in that related distractors’ (category names) corresponding targets (category exemplars) were presented twice on the study list. Brainerd, Reyna, and Kneer’s (1995) verbatim priming manipulation was used on the recognition test: Half of the related distractors were preceded by their corresponding targets (e.g., DIAMOND was tested immediately before JEWEL) and half were preceded by unrelated targets. Estimates of recollection rejection are shown in Table 1. As in Experiments 1 and 2, roughly one third of the related distractors produced recollection rejection. Also, target priming had the predicted effect. The level of recollection rejection for primed category names ($T_{rd} = .49$) was nearly four times the level for unprimed category names ($T_{rd} = .13$).

The fourth experiment was reported by Rotello (2001). Participants studied three lists of familiar words. Following each list, they responded to a recognition test on which related distractors were generated from targets by pluralization (e.g., TRUCKS was a distractor corresponding to the target TRUCK) and singularization (e.g., FROG was a distractor corresponding to the target FROGS). Thus, as in the response-signal experiments of Hintzman and Curran (1994) and Rotello and Heit (1999), related distractors were very similar to targets in both surface form and meaning. Participants responded to recognition tests under the instructional conditions of conjoint recognition. Levels of recollection rejection and familiarity for related distractors were estimated with Equations 5–7, and the results appear in Table 1. It can be seen that, in contrast to response-signal results for these same distractors, the estimate of recollection rejection, .25, was well above zero.

Brainerd and Reyna (2002) showed that estimates of $T_{rd}$ and $F_{rd}$ can be obtained in partial conjoint-recognition designs, which contain only the verbatim and verbatim plus gist conditions, because Equations 5, 7, 8, and 10 form an identifiable subsystem for the parameters $T_{rd}$, $F_{rd}$, $\beta_v$, and $\beta_G$. They reported estimates of $T_{rd}$ and $F_{rd}$ for two prior developmental studies of this sort that used the same study and test materials. One was the aforementioned study of Reyna and Kiernan (1994), which is labeled Experiment 5 in Table 1, and the other was a study by Kiernan (1993), which is labeled Experiment 6 in Table 1. The key difference between these experiments is that Reyna and Kiernan’s participants were younger (6-year-olds) and older (9-year-olds) children from the normal ability range, whereas Kiernan’s were 9-year-olds who had specific language disabilities involving surface discrimination (errors in differentiating words with similar phonology or orthography) and a sample of same-age control children. As can be seen in Table 1, estimates of $T_{rd}$ were again well above zero. However, there are some instructive new findings that bear on the memorial basis for recollection rejection. Concerning Experiment 5, other developmental studies (Brainerd, Reyna, & Kneer, 1995) indicate that when children study materials that instantiate meanings that are well known to them, memory for the verbatim form of study materials improves much more with age than memory for meaning content. Consistent with the notion that recollection rejection is dependent on verbatim memory, the age increase in $T_{rd}$ was more than three times the age increase in $F_{rd}$, and the latter age increase was not reliable. Consistent with the same notion, in Experiment 6 the estimate of $T_{rd}$ for language-disabled children was less than half the estimate for control children, but the estimates of $F_{rd}$ for the two groups did not differ reliably.

Table 1

<table>
<thead>
<tr>
<th>Experiment and condition</th>
<th>$T_{rd}$</th>
<th>$F_{rd}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Experiment 1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 presentation</td>
<td>.13 (.06, .21)</td>
<td>.43 (.38, .48)</td>
</tr>
<tr>
<td>2 presentations</td>
<td>.31 (.23, .38)</td>
<td>.36 (.30, .42)</td>
</tr>
<tr>
<td>Experiment 2</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 presentation</td>
<td>.31 (.24, .39)</td>
<td>.49 (.43, .54)</td>
</tr>
<tr>
<td>2 presentations</td>
<td>.50 (.44, .56)</td>
<td>.27 (.20, .35)</td>
</tr>
<tr>
<td>Experiment 3</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No priming</td>
<td>.13 (.08, .18)</td>
<td>.50 (.46, .54)</td>
</tr>
<tr>
<td>Priming</td>
<td>.49 (.45, .54)</td>
<td>.11 (.05, .17)</td>
</tr>
<tr>
<td>Experiment 4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Younger</td>
<td>.25 (.22, .28)</td>
<td>.39 (.37, .42)</td>
</tr>
<tr>
<td>Older</td>
<td>.15 (.11, .19)</td>
<td>.25 (.22, .28)</td>
</tr>
<tr>
<td>.38 (.35, .41)</td>
<td>.31 (.28, .34)</td>
<td></td>
</tr>
<tr>
<td>Experiment 6</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Control</td>
<td>.54 (.51, .57)</td>
<td>.33 (.29, .37)</td>
</tr>
<tr>
<td>Language disabled</td>
<td>.26 (.22, .30)</td>
<td>.27 (.24, .30)</td>
</tr>
</tbody>
</table>

Note. Confidence intervals for parameter estimates are in parentheses. Experiments 1–3 are reported in Brainerd, Reyna, and Mojardin (1999). Experiment 4 is reported in Rotello (2001), and Experiments 5 and 6 are reported in Brainerd and Reyna (2002).

6 The significance testing machinery that is used to determine whether parameter estimates are reliably above zero and whether experimental manipulations have reliable effects on parameter estimates is described in Brainerd et al. (1999). Because we wanted to avoid repeated presentation of significance tests, in the conjoint-recognition experiments reported in this section (a) estimates of $T_{rd}$ and $F_{rd}$ were reliably greater than zero (.05 level of confidence) unless otherwise indicated and (b) only reliable between-condition differences in parameter estimates (.05 level of confidence) are discussed. In the case of previously published research (Experiments 1–6), detailed results for significance tests have already been reported.
New Experiments

We now summarize results from three further conjoint-recognition experiments that supply additional tests of the predictions that (a) levels of recollection rejection should be above zero for the types of related distractors that produce false-memory reports and (b) the parameter $T_{rd}$ should react appropriately to manipulations that ought to increase distractors’ tendency to provoke retrieval of targets’ verbatim traces. Two of the experiments, which we designate as Experiments 7 and 8, were identical in design, save for the fact that different types of related distractors were administered on the test lists. A total of 240 undergraduates participated, with 120 being assigned to Experiment 7 and 120 being assigned to Experiment 8. Within each experiment, 40 participants were assigned to each of the three instructional conditions. During the study phase, each participant listened to a list of 120 common nouns presented at a 3-s rate, 40 of which had common synonyms and 40 of which had common antonyms. Participants then read instructions that were appropriate to their condition. Finally, the test list was presented, which consisted of 80 words, 40 targets, 20 related distractors (synonyms in Experiment 7, antonyms in Experiment 8), and 20 unrelated distractors. Participants also returned 1 week later for a long-term retention test. After rereading the instructions from the previous week, they responded to another test list consisting of 40 targets, 20 related distractors (synonyms in Experiment 7, antonyms in Experiment 8), and 20 unrelated distractors. None of these items had appeared on the immediate test.

As in Experiments 1 and 2, $T_{rd}$ should be greater than zero on the immediate tests. A new prediction concerns declines in recollection rejection and familiarity over the 1-week forgetting interval. If the ability to access verbatim traces of targets’ surface content declines more rapidly than the ability to access gist traces of their meaning content, recollection rejection ought to be more susceptible to forgetting than familiarity, and the decline in $T_{rd}$ between immediate and delayed tests should be greater than the decline in $F_{rd}$. Parameter estimates for the respective conditions of these experiments are reported in Table 2, where it can be seen that the results were congruent with predictions. There was evidence of recollection rejection in both experiments; the value of $T_{rd}$ on immediate recognition tests was greater than zero in each instance. Note, too, that the values of $T_{rd}$ for synonyms and antonyms were very close to the values in Experiments 1 and 2 for distractors whose corresponding targets had been studied once. Finally, recollection rejection declined far more than familiarity over the 1-week retention interval. The difference in forgetting rates was dramatic: The value of $T_{rd}$ dropped to zero in both experiments, but the value of $F_{rd}$ did not decline significantly. Here, it is important to reiterate that the conjoint-recognition model does not force estimates of $F_{rd}$ to behave in any particular way as estimates of $T_{rd}$ decrease. As estimates of $T_{rd}$ decrease, estimates of $F_{rd}$ are completely free to vary between 0 and 1 (see earlier comments about model identifiability). Finally, on the immediate tests in these experiments, as in Experiments 1 and 2, estimates of $T_{rd}$ were always higher for antonyms than for synonyms, suggesting that antonyms are better retrieval cues for verbatim traces of their corresponding targets (which is consistent with the fact that antonyms are stronger associates of each other than synonyms are).

Experiment 9 was similar in design to Experiment 7, except that a control or a misinformation task was interpolated between the study and test phases of Session 1. Half of the participants ($n = 60$) were assigned to a control condition, and half ($n = 60$) were assigned to a misinformation condition. After listening to a list of

| Parameter |
|-----------|---|---|
| $T_{rd}$   |   |   |
| $F_{rd}$   |   |   |

Note. Confidence intervals for parameter estimates are in parentheses.
common words (3-s presentation rate), all participants were given a page with 60 printed words, under the pretext that it would help them to consolidate their memory for the study list. Participants read each word silently and crossed it out. For the control condition, the interpolated list was simply the 60 targets that participants had just listened to. For the misinformation condition, the interpolated list was composed of 30 targets and 30 distractors that were synonyms of targets on the earlier study list (e.g., HILL, DIRT, and CRIMSON were synonym distractors for the targets MOUNTAIN, SOIL, and RED). These targets and distractors were further subdivided into two sets: (a) Set 1 consisted of 15 targets and their synonyms, in which both target and synonym appeared on the misinformation list (e.g., the target GOWN and the synonym DRESS might both appear); (b) Set 2 consisted of 15 targets and 15 synonyms of other targets that were not present on the misinformation list (e.g., JUGGLER might be a target on the misinformation list and HILL might be an illustrative synonym on the misinformation list whose corresponding target, MOUNTAIN, did not appear on the misinformation list).

Participants were randomly assigned to the three instructional conditions (verbatim, gist, and verbatim plus gist) following the interpolated phase. The three sets of instructions were to accept only words that had appeared on the oral study list (verbatim), to accept only words that were synonyms of words on the oral study list but had not appeared on the misinformation list (gist), and to accept words that appeared on the oral study list or that were synonyms of those words (verbatim plus gist). Participants responded to two recognition tests, immediate and 1-week delayed. The immediate test consisted of 30 targets, 15 related distractors (synonyms), and 15 unrelated distractors. For participants in the misinformation condition, all of the related distractors had appeared on the misinformation list, 8 from Set 1 and 7 from Set 2, and 15 targets had appeared on the misinformation list, 8 from Set 1 and 7 from Set 2. The 1-week delayed test consisted of: (a) the 60 probes that had been administered on the immediate test; (b) 60 probes that had not been previously tested, 30 targets, 15 related distractors (synonyms), and 15 unrelated distractors. Concerning the previously untested probes, in the misinformation condition, all of the related distractors had appeared on the misinformation list, 7 from Set 1 and 8 from Set 2, and 15 of the targets had appeared on the misinformation list, 7 from Set 1 and 8 from Set 2.

Principal interest attaches to predictions about the ordering of estimates of $T_{rd}$ for different conditions. Estimates should be highest in the control condition because targets were presented twice (providing highly accessible verbatim traces) and synonyms were never presented (providing no competing verbatim traces that would interfere with target recollection). Estimates should be next highest for Set 1 items in the misinformation condition because, although targets were again presented twice, synonyms had appeared on the misinformation list (providing competing verbatim traces that could interfere with target recollection). Estimates should be lowest for Set 2 items because targets were presented only once (providing less accessible verbatim traces). The relevant estimates appear in the bottom half of Table 2. On the immediate test, the ordering of $T_{rd}$ values was as predicted. On the delayed test, the predicted ordering was obtained for the set of probes that had been previously tested, although estimates of $T_{rd}$ were smaller, naturally. For items that had not been previously tested, the $T_{rd}$ value for misinformation Set 2 items was again reliably smaller than either the value for misinformation Set 1 items or the value for the control condition, but the latter pair of values did not differ.

Thus, the ordering of estimates of $T_{rd}$ across conditions was as predicted. In addition, a prediction that figured in Experiments 7 and 8 can be evaluated—namely, that $T_{rd}$ declines more than $F_{rd}$ over long-term retention intervals. Support for this prediction depended on condition. The prediction was confirmed in the control condition, which provided the purest test. In the misinformation condition, the prediction was also confirmed for Set 1 data and for Set 2 data of synonyms that had not been previously tested, but it was not confirmed for previously tested synonyms because estimates of $F_{rd}$ were near floor levels. It is not obvious why the prior recognition test produced such marked decrements in familiarity in the misinformation condition. Although this result could simply be a statistical anomaly, it may be a metacognitive suppression effect. When standard recognition tests (i.e., the verbatim condition) follow interpolated misinformation, participants often spontaneously report that they are aware that some of the test probes were presented as misinformation rather than as study material (Cassel & Bjorklund, 1995; Reyna & Titcomb, 1997). On later tests, such awareness may lead participants to suppress the use of familiarity as a basis for acceptance because it does not discriminate between studied material and misinformation.

Summing up the theoretical yield of the new experiments, measurement of rates of recollection rejection via parameter $T_{rd}$ showed that significant proportions of related distractors were rejected through target recollection. Further, $T_{rd}$ reacted appropriately to experimental manipulations that should affect the accessibility of targets’ verbatim traces: $T_{rd}$ increased as a function of repetition of targets (either during the study phase or on an interpolated task) and immediacy of testing, and it decreased as a function of the presentation of misinformation.

**Measuring Recollection Rejection With ROCs**

**Elements of ROC Analysis**

The memory tests that figure in most false-memory research involve simply recognizing or recalling presented material. In recognition experiments, in which participants make binary responses to all test probes, the certainty with which those responses are made may vary. This aspect of performance can be captured by requiring participants to make confidence judgments, rather than accept–reject responses, using a scale that ranges from sure-old at one end to sure-new at the other. Depending on the experiment, the number of categories that have been represented in these confidence scales has ranged from 6 to 10.

When participants make such confidence judgments, the data are often analyzed as ROCs. Such analyses provide information about the parameters of signal detection theory, particularly the memory-discrimination parameter $d'$ (Macmillan & Creelman, 1991; Norman & Wickelgren, 1969). ROC analysis involves plotting the hit rate for targets and the false-alarm rate for unrelated distractors as a joint function of confidence level. Each point on an ROC plot gives the cumulative probability of assigning targets a particular confidence value (and all values to the left of that value) and the cumulative probability of assigning unrelated distractors that same confidence value. The left-to-right arrangement of confidence values is from sure-old to sure-new. Hence, the leftmost
point is the probability of classifying targets as sure-old and the probability of classifying distractors as sure-old, whereas the rightmost point is the cumulative probability of classifying targets as sure-new (and as any of the prior confidence designations) and the cumulative probability of classifying distractors as sure-new (and as any of the prior confidence designations). Because probabilities sum to one, the rightmost point of an ROC plot must be one. ROCs can be plotted either on probability coordinates, which produce curves, or on \( z \) coordinates (i.e., transformations of cumulative probabilities to \( z \) scores), which produce straight lines. Plots on \( z \) coordinates provide measures of \( d' \), which is the \( y \) intercept of a \( z \) ROC.

Another feature of ROCs, which ultimately yields estimates of recollection rejection, is their symmetry. Symmetry refers to the shapes of ROCs that are plotted on probability coordinates and to the slopes of plots on \( z \) coordinates. In probability plots, if cumulative probability varies at a constant rate for targets and distractors, ROCs will be symmetrical about the main diagonal (lower curve, Figure 3). Symmetrical ROCs will result if a single familiarity process is responsible for recognition and if the distributions of familiarity values for targets and distractors are normal with equal variances (Macmillan & Creelman, 1991). However, when cumulative probabilities do not vary at constant rates (i.e., when there are disproportionate increases in some parts of the plot), ROCs will be asymmetrical. For example, the upper curve in Figure 3 is an asymmetrical ROC in which the sure-old category produces a disproportionate jump in the cumulative probability for targets. This yields a curve that is skewed toward the “new” end of the confidence scale, rather than a curve that is evenly bowed toward the “old” and “new” ends. With \( z \) plots, a symmetrical ROC has a slope of 1.0, whereas skewed ROCs like the upper curve in Figure 3 have slopes of less than 1.0.

Yonelinas (1994) made three key observations about ROC symmetry. First, under signal detection theory’s one-process analysis, ROCs will be symmetrical if familiarity values are distributed normally with equal variances for targets and distractors (the usual signal detection assumptions). Second, contrary to this analysis, it has generally been found that probability plots have skewed shapes that resemble the upper curve in Figure 3, with the slopes of the corresponding \( z \) ROCs usually being in the .7 to .8 range (e.g., Donaldson & Murdock, 1968; Murdock & Dufty, 1972). Third, although skewed ROCs violate signal detection theory’s one-process assumptions, they are predicted by the dual-retrieval model in Equation 1. Here, Yonelinas noted that if an additional (recollection) process contributes to hits, disproportionate increases in target confidence will occur at the start of an ROC plot (sure-old), which would yield the characteristic skewed ROCs. Recollection would have this effect because there should be no variability in confidence judgments for targets whose presentation can be explicitly recollected; all will be judged sure-old. Although skewed ROCs are inconsistent with the traditional one-process account of recognition, they are not inconsistent with more recent one-process accounts in which the assumptions of normality and equal variance are relaxed, such as Shiffrin and Steyvers’ (1997) REM model.

**ROC Results**

Rotello (2001; Rotello, Macmillan, & Van Tassel, 2000) developed a procedure for measuring amounts of recollection rejection (which she termed recall-to-reject) by extending ROC analysis. The y-axis is still the cumulative probability of assigning particular confidence values to targets, but the x-axis becomes the cumulative probability of assigning those values to related distractors. Rotello et al. referred to the latter plots as *old-similar ROCs* and to traditional plots as *old-new ROCs*. We saw that a recollection rejection process for targets destroys old–new symmetry because recollected targets will be judged sure-old, producing disproportionate increases at the start of ROCs. A recollection rejection process for related distractors will produce an effect at the end of old-similar ROCs. Specifically, if target recollection generates mismatches between targets’ verbatim traces and related distractors (“I definitely heard HOUSTON, not PHOENIX.”), there will be no variability in confidence judgments for such distractors; all will be judged sure-new, which will yield disproportionate increases in cumulative probability at the end of the plot.

Rotello et al. (2000) showed that recollection rejection produces old-similar \( z \)-ROCs that have a slight upward curvature (rather than plotting as straight lines). She showed that recollection rejection produces two further features in old-similar probability plots. First, such ROCs will plot approximately as lines, rather than as bowed curves. Second, old-similar ROCs will have an upward curvature, so that the point at which the curve bisects the upper x-axis, the upper-x-intercept, will be a point where the cumulative probability value on the x-axis has not yet reached 1.0. It is this second feature that delivers estimates of recollection rejection. Rotello et al. demonstrated that a specific relation exists between the upper-x-intercept and recollection rejection: The difference between the upper-x-intercept and unity is the lower limit of the proportion of related distractors for which recollection rejection occurred. That is, the amount of recollection rejection will be at least as great as the difference between the upper-x-intercept and unity, and it will be greater if sure-old judgments are sometimes made on a basis other than recollection rejection.

Rotello et al. (2000) reported four experiments in which this method was used to measure recollection rejection. As in the earlier conjoint-recognition studies, an important aim was to establish whether recollection rejection varied appropriately as a function of manipulations that should affect target recollection. In
an initial experiment, participants studied lists of unrelated words and made confidence judgments using a 6-point scale. Related distractors were pluralizations of targets (e.g., COMPUTERS if COMPUTER was studied) or singularizations (e.g., PAINTING if PAINTINGS was studied). Unlike conjoint-recognition instructions, Rotello’s instructions alerted participants to the recollection rejection strategy; that is, that they could be certain that a probe had not been presented if they could remember studying its plural form (for singular targets) or its singular form (for plural distractors). The results appear in Figure 4, where it can be seen that the old-similar ROC was nearly linear and the upper-x-intercept was .706, yielding .294 as the estimate of recollection of the strategy.

The results of this experiment, which appeared in Rotello et al. (2000) second experiment, was a replication, except that participants were not informed that they could be certain that a probe was a distractor if they could remember studying its corresponding plural or singular form. This modification was imposed to test the hypothesis that recollection rejection is a purely strategic process that requires metacognitive awareness of the strategy’s usefulness. The results, which appear in Figure 4, disconfirmed the hypothesis and are consistent with conjoint-recognition studies in which participants also are not informed that they can be certain that a probe was not presented if they can recollect its corresponding target). The upper-x-intercept, .865, was considerably below unity, although the estimate of recollection rejection, .135, was roughly half that of the first experiment. Hence, recollection rejection seems to occur in the absence of metacognitive awareness of its usefulness, but such awareness increases the frequency with which it is used.

In conclusion, Rotello et al.’s (2000) third experiment was an associative recognition study resembling that of Gronlund and Ratcliff (1989). Participants studied lists of word pairs (AB) and then responded to test lists composed of targets, related distractors, and unrelated distractors. The related distractors were AB pairs (i.e., both words had been presented, but the target word had been paired with a different cue word) and the unrelated distractors were XY pairs (i.e., neither word had been presented). Participants were not informed that they could be certain that a pair was a distractor if they could remember studying a pair containing one of the words but not the other. Rotello et al. reasoned that levels of recollection rejection should be higher in this experiment because AB probes should be better retrieval cues for their corresponding targets than the related distractors in the second experiment. This prediction was confirmed: The estimate of recollection rejection more than doubled, from .135 to .349.

In their last experiment, Rotello et al. (2000) used response-signal methodology to influence recollection rejection. The design was the same as the third experiment, except that response signals were used to control retrieval time at test. For half of the test probes (AB, AB’, and XY), probes were exposed for 450 ms, after which participants had to supply a confidence rating within 350 ms. The other half of the test probes were exposed for 2,500 ms, after which participants had to supply a confidence rating within 350 ms. The logic of this manipulation is that if recollection-based retrieval is slower than familiarity-based retrieval, levels of recollection rejection should be higher for AB’ probes with 2,500 ms of retrieval time. Consistent with this prediction, the upper-x-intercept for AB’ probes was .768 with 2,500 ms of retrieval time, yielding .232 as the estimate of recollection rejection, but it was 1.0 with 450 ms of retrieval time, indicating no recollection rejection.

Rotello (2001) reported another experiment, described in our earlier precis of conjoint-recognition studies, which is especially interesting because it combined ROC and conjoint-recognition measures of recollection rejection. As mentioned earlier, participants studied lists of familiar words and made recognition decisions about test lists composed of targets, unrelated distractors, and related distractors that were pluralizations or singularizations of targets. However, those decisions were 6-point confidence judgments that were made under the three instructional conditions of conjoint recognition. The instructions for the verbatim condition were the same as those of Experiment 1 of Rotello et al. (2000). In the gist condition, participants identified related distractors using the 6-point scale: 1 = sure changed, 2 = probably changed, 3 = maybe changed, 4 = maybe old or new, 5 = probably old or new, and 6 = sure old or new. In the verbatim plus gist condition, participants identified both targets and related distractors using the 6-point scale: 1 = sure old or changed, 2 = probably old or changed, 3 = maybe old or changed, 4 = maybe new, 5 = probably new, and 6 = sure new. When old-similar ROCs were plotted, the mean estimate of recollection rejection, .27, was in the neighborhood of the estimate in Experiment 1 of Rotello et al. When recollection rejection was estimated with the conjoint-recognition model, the mean estimate, .25, was virtually the same as the one from ROC plots. Thus, two very different procedures for estimating recollection rejection produced nearly the same values.

In conclusion, Rotello’s (2001; Rotello et al., 2000) ROC studies produced results that concur with conjoint-recognition findings on two key points. First, ROC estimates of rates of recollection rejection showed that significant proportions of related distractors are rejected through recollection of the presentation of their corresponding targets. In all of the ROC data that we have reviewed, only a single condition in one experiment (Rotello et al., 2000, Experiment 4) delivered a zero estimate, that condition being one in which retrieval time was presumably too short for target recollection to occur. Second, the manner in which ROC estimates reacted to experimental manipulations was consistent with the theoretical definition of recollection rejection. In particular, estimates of recollection rejection increased when related distractors were better retrieval cues for targets’ verbatim traces, when there

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**Figure 4.** Old-similar receiver operating characteristics for Experiment 1 (EXP 1) and Experiment 2 (EXP 2) in Rotello, Macmillan, and Van Tassel (2000).
was more time to access such traces, and when participants were informed that they could be certain that a test item had not been presented if they could recollect the presentation of its corresponding target.

A Complementary Operation: Erroneous Recollection Rejection

The process model of recollection rejection implies a complementary operation that generates a new class of false-memory phenomena for experienced material, phenomena that we refer to collectively as erroneous recollection rejection rejection. The minimum experimental preconditions for this editing operation are that the presented materials include items that are related to each other in salient ways and that those items appear on the memory tests. This leaves a wide range of possible implementations, including memory for situations that are relevant to forensic interviews, such as memory for the objects in a room (e.g., Lampinen et al., 2001) or memory for people’s descriptions of events (e.g., Bartlett, 1932).

For simplicity, however, we return to a previous example and assume that participants study a word list on which ALBUQUERQUE, HOUSTON, and TUCSON all appear. The research reviewed thus far suggests that true events—say, PHOENIX—will sometimes be rejected pursuant to recollection of semantically related true events (“No, I didn’t have heard PHOENIX because I definitely remember hearing HOUSTON instead.”). If distractors sometimes produce recollection of related targets, target probes themselves may sometimes produce recollection of related targets (e.g., HOUSTON produces recollection of ALBUQUERQUE), while not producing recollection of their own presentations. Such retrieval events should not occur with high frequency, as targets will be far better retrieval cues for their own verbatim traces than for verbatim traces of other targets, but when they occur, joint processing of target probes and verbatim traces of semantically related targets could generate verbatim mismatches that yield probe rejections. In other words, participants would treat related targets as though they were related distractors.

Although erroneous recollection rejection is not expected to occur with high frequency, it might be present at sufficient levels to be detectable. As mentioned, the minimum experimental preconditions for this editing operation leave a wide range of possible implementations. At one extreme, the presented materials might contain a small number of related items embedded within a large number of unrelated items, as in the example of a long list that contains ALBUQUERQUE, HOUSTON, and TUCSON. At the other extreme, the presented materials might consist entirely of related items, with DRM lists being examples. If erroneous recollection rejection occurs at detectable levels with such materials, Mandler’s (1980) basic equation for hit probability (Equation 1) must be expanded to

\[
p(H) = T_i + (1 - T_i)(1 - E_i)F_i + (1 - T_i)(1 - E_i)(1 - F_i)\beta_i \tag{11}
\]

where, as before, \(p(H)\) is the probability of a hit, \(F_i\) is the probability of familiarity-based acceptance, \(T_i\) is the probability of recollection-based acceptance, \(\beta_i\) is the usual bias parameter, and in addition, \(E_i\) is the probability of erroneous recollection rejection.

This equation predicts a novel qualitative effect that can be used to diagnose the presence of erroneous recollection rejection: unequal hit rates for standard instructions (accept only targets) versus meaning-recognition instructions (accept targets and accept distractors that have some specific meaning relationship to targets).7

To show how this prediction falls out, consider a design that satisfies the minimum preconditions for erroneous recollection rejection. If \(p(H')\) is the hit probability under standard instructions and \(p(H_m)\) is the hit probability for targets under meaning-recognition instructions, the expressions for these conditions that follow from Equation 11 are

\[
p(H') = T_i + (1 - T_i)(1 - E_i)F_i + (1 - T_i)(1 - E_i)(1 - F_i)\beta_i \tag{12}
\]

and

\[
p(H_m) = T_i + (1 - T_i)E_i + (1 - T_i)(1 - E_i)F_i + (1 - T_i)(1 - E_i)(1 - F_i)\beta_m \tag{13}
\]

As long as the response bias parameters are equal, erroneous recollection rejection \((E_i > 0)\) implies the directional inequality \(p(H') < p(H_m)\). However, equality of response bias cannot be assumed, the normal expectation being that bias will increase as the acceptance criterion becomes more inclusive (i.e., \(\beta < \beta_m\); see Buchner, Erdfelder, & Vaterrodt-Pluncke, 1995). Therefore, the predicted inequality must be evaluated with statistics that control for differences in response bias, such as \(d', d_i, \text{ or } A'\).

The aforementioned studies by Reyna and Kiernan (1994) supply relevant data. Participants were presented with related target sentences describing everyday objects and responded to recognition tests under either standard or meaning-recognition instructions. We reanalyzed Reyna and Kiernan’s data, computing memory-discrimination measures for their recognition tests. The mean value of \(A’\) for targets was .84 for standard recognition and .88 for meaning recognition, as expected. The same pattern (i.e., an \(A’\) difference favoring meaning recognition) was obtained when we reanalyzed data from experiments by Lim (1993) and Kiernan (1993). These experiments used the same sentence materials and testing procedures as Reyna and Kiernan, but the subject samples were different.

Some of the conjoint-recognition experiments that we reported earlier, especially Experiments 1–3, are also relevant because participants responded to test lists under both standard and meaning instructions. None of these experiments satisfies the minimum preconditions for erroneous recollection rejection because study lists did not contain related targets. Therefore, the prediction would be that, unlike Reyna and Kiernan’s (1994), Lim’s (1993), and Kiernan’s (1993) studies, \(A’\) values will not favor meaning recognition over standard recognition. This prediction was confirmed (see Brainerd et al., 1999).

Three further experiments that satisfy the minimum preconditions for erroneous recollection rejection were reported by Brainerd et al. (2001), which we designate as Experiments 10, 11, and

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7 Such instructions are analogous to the verbatim plus gist condition of conjoint-recognition methodology, but our concern here lies with hit rates, rather than false-alarm rates for related distractors.
12. Participants responded to recognition tests under meaning instructions, as well as standard instructions. In Experiment 10, study lists contained subsets of same-category exemplars embedded within a large number of unrelated targets. Half of the categories were represented by one exemplar (no meaning repetition), and half were represented by four exemplars (meaning repetition). In Experiment 11, participants studied 12 DRM lists before taking a recognition test. Experiment 12 resembled Experiment 11 in that participants studied DRM lists before taking a recognition test, but a 1-week delayed test was also administered, with half of the probes being previously tested and half being untested.

For the conditions of these experiments, we computed $A'$ values for individual participants under standard and meaning instructions. The mean values appear in Table 3. Four of the paired values are for immediate tests, and two are for delayed tests. There was evidence of erroneous recollection rejection in Experiments 10 and 12, but not in Experiment 11. In Experiment 10, the mean $A'$ value was significantly larger for meaning recognition than for standard recognition for categories that had been exemplified once and for categories that had been exemplified four times. In Experiment 12, the mean $A'$ value was significantly larger for meaning recognition than for standard recognition on immediate tests but not on delayed tests. In Experiment 11 (and on the delayed tests of Experiment 12), $A'$ did not differ reliably for standard versus meaning recognition.

Clearly, further experimentation that is narrowly focused on detecting erroneous recollection rejection will be required to provide convincing evidence of this predicted phenomenon and to isolate the precise conditions under which it occurs. Nevertheless, given its implications for false-memory reports in applied contexts, as well as its relevance to dual-retrieval theories, it is worth noting that available data on standard versus meaning recognition are at least suggestive of erroneous recollection rejection.

**Summary and Discussion**

The flourishing literature on false memory in natural and laboratory contexts has stimulated interest in mechanisms for editing false events out of memory reports. Anecdotal and introspective evidence suggest that people sometimes weed false-but-gist-consistent events out of their memory reports. To take advantage of that capability in contexts in which a premium is placed on minimizing false reports, one must understand the factors that control it, which requires a process model so that hypotheses about those factors can be formulated and tested.

A process model of a potential false-memory editing mechanism, recollection rejection, follows from dual-retrieval theories. According to this model, one retrieval operation (familiarity) accesses gist memories of the meaning of experience and favors acceptance of false-but-gist-consistent events, whereas the other (recollection) accesses verbatim traces of the surface form of experience and favors rejection. A preliminary advantage of this model is that it explains two puzzling findings about semantic false recognition: (a) modest semantic false-recognition effects when distractors’ meaning familiarity is high and (b) lack of increase in false-alarm rates for meaning-preserving distractors following repeated presentation of a single related target or presentation of several related targets. The model also generates new predictions, which follow from the principle that, other things being equal, manipulations that affect the ability of related distractors to access verbatim traces of their corresponding targets will affect semantic false recognition. These predictions include both qualitative and quantitative phenomena.

Qualitative predictions concern raw false reporting rates for gist-consistent information. We examined predictions about factors that should suppress semantic false recognition and predictions about the relation between retrieval time and false recognition. Factors that should suppress semantic false recognition center on enhancing test-phase accessibility of verbatim traces and on storage of more robust verbatim traces. Test-phase accessibility has been manipulated by priming the surface content of targets just before related distractors are tested and by decreasing the delay between study and test. Such manipulations have been successful in suppressing semantic false recognition. The strength of stored verbatim memories has been manipulated by increasing presentation duration and by presenting material that should yield robust verbatim traces. Such manipulations have also suppressed semantic false recognition. Concerning the relation between retrieval time and false recognition, inverted-U relations follow from the notion that recollective retrieval is slower than familiarity retrieval. Response-signal data provide confirmation of inverted-U-shaped relations in pair recognition but not in item recognition, but null findings for item recognition may be due to insensitivity of response-signal methodology, a view that is supported by evidence of recollection rejection in item recognition using more sensitive quantitative methodologies.

Turning to quantitative predictions, two methodologies provide estimates of amounts of recollection rejection: (a) conjoint recognition and (b) ROCs for related distractors. With the first methodology, considerable evidence has accumulated that, as predicted, significant proportions of related-distractor rejections are due to recollection rejection. Also, the recollection rejection parameter was influenced by manipulations that ought to affect the accessibility of verbatim traces (e.g., study–test delay, verbatim priming at test, target repetition at study). ROC methodology has produced convergent findings. Estimates of the upper-$x$-intercept of ROC curves have shown that recollection rejection is present in false-recognition designs, that it reacts appropriately to theoretically

<table>
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<th>Experiment and condition</th>
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<tr>
<td>Experiment 9</td>
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</tr>
<tr>
<td>1 exemplar</td>
<td>.82</td>
<td>.87</td>
</tr>
<tr>
<td>4 exemplars</td>
<td>.86</td>
<td>.90</td>
</tr>
<tr>
<td>Experiment 10</td>
<td>.83</td>
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<td>Delayed test</td>
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<tr>
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<td>.76</td>
<td>.76</td>
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<tr>
<td>Tested</td>
<td>.75</td>
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*Note.* Target $A'$ values were computed in the usual way, using the false-alarm rate for unrelated distractors as the baseline.
derived manipulations, and even that ROC estimates are similar to conjoint-recognition estimates.

Our general conclusion is that a false-memory editing operation has been isolated that satisfies both of the criteria that we specified at the outset. With respect to the first criterion, recollection rejection is theoretically well defined, both because it follows from dual-retrieval theories and because it involves explicit process assumptions about representation, retrieval, and output. With respect to the second criterion, supportive evidence has been obtained under diverse conditions, using manipulations that implement assumptions of the process model. Thus, the level of scientific support for this editing operation is high.

Recollection rejection has broad theoretical and applied ramifications. From a theoretical perspective, the existence of this editing operation provides support for contemporary dual-process theories of false memory rather than for older one-process theories, such as constructivism (e.g., Bransford & Franks, 1971). From an applied perspective, the implications of dual-process theories for situations in which it is important to minimize false reporting are quite different than those of one-process theories. According to dual-process theories, the existence of robust false reporting and of accurate editing of false information out of memory reports must both be acknowledged and be incorporated into best-practice recommendations. In that connection, recollection rejection adds an important dimension to best-practice recommendations. Traditionally, such recommendations have emphasized the avoidance of practices that encourage the formation of false memories (e.g., Ceci & Bruck, 1995; Fisher & Geiselman, 1992; Poole & Lamb, 1998; Poole, Lindsay, Memon, & Bull, 1995). Recollection rejection adds a second dimension—namely, the use of practices that can help people suppress the reporting of false memories, once they are formed, via the retrieval of verbatim traces that neutralize the familiarity of false-but-gist-consistent events (Brainerd & Reyna, 2002).

Finally, it should be stressed that there is no incompatibility between recollection rejection and potential metamemorial methods of editing false information out of memory reports, such as the distinctiveness heuristic of Schacter et al. (1999) or the metacognitive monitoring of Koriat and Goldsmith (1996; Koriat et al., 2003). The key difference between these approaches is that recollection rejection operates at the item level, screening out specific false events (e.g., PHOENIX is rejected because of a verbatim mismatch with HOUSTON), whereas metacognitive editing operates above the item level, screening out groups of false events that lack some generic property (e.g., that a visual representation of an event should come to mind, that only names of European capitol cities were studied). In adults at least, both item-level and metamemorial editing presumably occur, and in line with this possibility, Odegaard et al. (2001) have recently developed a procedure that estimates the respective contributions of the two types of editing. Our reasons for focusing on an item-level editing operation are largely theory-driven. Because a specific operation can be derived from dual-retrieval theories, it is possible to take advantage of our accumulated knowledge of dual-retrieval processes, which is considerable, to gain leverage on false-memory editing. Moreover, it may be that, developmentally, metacognitive editing evolves from prior experience with recollection rejection. In this connection, it is instructive to recall Rotello et al.’s (2000) finding that adults display significant levels of recollection rejection when they lack metacognitive awareness of its usefulness but they display increased levels when they possess such awareness.

References


