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Retroactive interference in recognition memory: The effects of mental effort and similarity on recollection and familiarity

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RETROACTIVE INTERFERENCE IN RECOGNITION MEMORY: THE EFFECTS

OF MENTAL EFFORT AND SIMILARITY ON RECOLLECTION AND

FAMILIARITY

By

Caleb Jordan Picker

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ABSTRACT

RETROACTIVE INTERFERENCE IN RECOGNITION MEMORY: THE EFFECTS
MENTAL EFFORT AND SIMILARITY ON RECOLLECTION AND FAMILIARITY

By

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Learning new material may retroactively interfere with memory for older material.

Retroactive interference research has typically focused on how similarity between old

and new material affects recall of old material, which predicts greatest interference when

similar material is presented just before test. However, mental effort may be another

source of retroactive interference that could disrupt consolidation: Mental effort could

cause the most retroactive interference when presented just after study. In Experiment 1,

participants engaged in tasks designed to induce mental effort (e.g., solving easy or

difficult math problems) at various times between the study and test of an associative

recognition task. Although familiarity estimates were unaffected, the timing of mental

effort affected recollection estimates. In Experiment 2, participants engaged in a different

set of tasks designed to induce mental effort (e.g., solving easy or difficult anagrams) and

increase similarity. Again, familiarity estimates were unaffected; however, mental effort

marginally affected recollection estimates, but in a way that was inconsistent with

expectations. The results showed inconsistent mental effort effects overall, consistent

with some past research showing that mental effort may not always cause retroactive

interference. The results also highlight the importance of a deeper investigation of

retroactive interference effects in recognition memory.

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Finally, I thank my friends and my family, including my parents, my brothers, and my grandparents, who have held steadfast in encouragement and support, wondering, "Are you almost done with your thesis?" This thesis is an answer to that question and proof that I am on my way to success in my career.

DEDICATION

This thesis is dedicated in loving memory of my grandfather, Solomon Henner, who died March 2012. Although he never made it to my wedding in June 2013 or saw me to my Ph.D. program's end, he died knowing he had a grandson who would never quit. This is for you, Grandpa, I am on my way.

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CHAPTER 1

INTRODUCTION

Retroactive interference occurs when new material learned during the retention interval of a memory test decreases memory for old material. Retroactive interference can be item-specific or nonspecific. Item-specific RI occurs when there is a strong correspondence between old and new material—for example, learning 'apple' interferes with learning 'pear'. Item-specific retroactive interference increases with the similarity between old and new materials (for reviews, see M. C. Anderson et al., 1994 and M. C. Anderson, 2003; cf. McGeoch, 1942). Non-specific retroactive interference occurs when there is little to no correspondence between old and new material. This nonspecific retroactive interference is thought to increase with the mental effort associated with learning new material or with decreasing the delay between learning old and new material.(Dewar, Cowan, & Della Sala, 2007; Lechner, Squire, & Byrne, 1999; Müller & Pilzecker, 1900; Skaggs, 1925, 1933; Wixted, 2004a, 2004b, 2005, 2010). Past research found an inverted-U relationship between recall of old material and the timing of learning new material, supporting the view that mental effort and similarity cause retroactive interference in different ways (Wixted, 2004b). However, retroactive interference usually has been measured with recall tasks: Few experiments have investigated retroactive interference in recognition memory (but see Heine, 1914, as cited in McKinney, 1935). The current study investigated the effects of mental effort and similarity on recognition memory performance.

The current literature review will focus on mental effort and similarity. The first section will introduce early research on mental effort. The second section will discuss

recent neurological research on mental effort. The third section will discuss early and recent research on similarity. The fourth section will discuss recognition memory research, linking mental effort and similarity effects to recognition memory processes.

Mental Effort as Retroactive Interference

Mental effort is the subjective state of mental demand that occurs while learning new material or while engaged in nonspecific tasks (Dewar et al., 2007; Müller & Pilzecker, 1900). In general, mental effort decreases memory performance: Early research found worse recall after a "filled" period of mental effort (e.g. solving multiplication problems) than after a relatively "unfilled" period of rest (Baldwin & Shaw, 1895; Calkins, 1896; DeCamp, 1915; Lechner et al., 1999; Lewy, 1895; Müller & Pilzecker, 1900; Münsterberg & Campbell, 1894; Skaggs, 1925; Warren & Shaw, 1895), suggesting that mental effort causes RI.

. Early research also found a nonlinear relationship between recall and the timing of learning new material (see Figure 1)—the temporal point of interpolation (TPI): Participants had worse memory when new material was learned just after study and just before test, but they had better recall when new material was learned midway between study and test (Archer & Underwood, 1951; Newton & Wickens, 1956; Postman & Alper, 1946; Sisson, 1939; Wixted, 2004b). This temporal gradient of retroactive interference is consistent with the view that there are two sources of retroactive interference, mental effort that disrupts consolidation and similarity that affects retrieval (for discussion of similarity, see section titled "Similarity and Response Competition").

One idea that has been closely tied to retroactive interference is consolidation. Consolidation refers to a process that stabilizes memories. Evidence for consolidation

comes from Ebbinghaus's forgetting curve (1885/1913), which plots amount of forgotten material over time. Mathematical descriptions of this forgetting curve suggest that that the proportion of material forgotten continually decreases over time (Wixted & Ebbesen, 1991; Wixted, 2004a). This is consistent with the view that consolidation increasingly protects memories from retroactive interference over time. Thus, more consolidated memories should be less susceptible to RI than less consolidated memories. Similarly, retroactive interference should affect memory more when presented just after study (less consolidation) compared to a delay (more consolidation).

Two laws of forgetting may reflect two sides of the same 'consolidation' coin (Wixted, 2004b): First, Jost's Law (the second tenet) holds that, given two memories of equal strength, the newer memory will decay more rapidly than the older memory. Second, Ribot's law holds that hippocampal damage causes temporally graded retrograde amnesia, that is, worse memory for newer than older memories (e.g., Nadel & Moscovitch, 1997, 2001). Thus, the hippocampus may consolidate memories: Newer memories may have consolidated less than older memories. Therefore, disrupting consolidation—by retroactive interference or by hippocampus damage —should affect newer memories more than older memories.

Müller and Pilzecker (1900, for review, see Lechner et al., 1999), in their original theory of RI, proposed that mental effort disrupts consolidation. First, they noticed that memories come to mind as afterimages after participants learn material, especially within the first 5 minutes after study. These uncued and spontaneous remindings were thought to consolidate memories and strengthen associations between them. Second, they found that mental effort made memory worse compared to rest (Müller & Pilzecker, 1900, as cited

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in Lechner et al., 1999). Third, and more importantly, mental effort made memory worse when presented shortly after study than when delayed, demonstrating a temporal gradient of nonspecific retroactive interference (i.e., mental effort). As noted earlier, this temporal gradient was replicated in other studies (Wixted, 2004b). Therefore, mental effort was thought to disrupt the occurrence of uncued remindings that facilitated consolidation (Lechner et al., 1999; Wixted, 2004a, 2004b)

Some early research did not support theory about mental effort (McGeoch & McDonald, 1931; Robinson, 1920, 1927). In some cases, mental effort did not decrease recall relative to rest: Only learning similar materials decreased recall (e.g. Robinson, 1920). In other cases, the temporal gradient of retroactive interference was not found (Wixted, 2004b). Unfortunately, these early mental effort experiments were flawed or had confounds (Skaggs, 1933; Wixted, 2004b): Participants read newspapers or spoke with the experimenter during unfilled conditions, likely inducing some degree of mental effort (McGeoch & McDonald, 1931; Robinson, 1920), and participants could have rehearsed old material more in unfilled than in filled conditions. Additionally, some studies only used two points of TPI when at least three points are necessary to find temporal effects (Wixted, 2004b). When considered with the experiments reviewed earlier, these limitations warrant further investigation of mental effort as a source of retroactive interference that disrupts consolidation (Wixted, 2004b).

Is Mental Effort New Learning?

One way the hippocampus could consolidate memory is through long-term potentiation, a resource-dependent (e.g. protein synthesis) synaptic plasticity mechanism that enhances neuronal communication (Dudai, 2004; Hebb, 1949). Over several hours,

long-term potentiation transitions between early and late phases, which could help convert memory from short-term to long-term storage (Dudai, 2004). Wixted (2004b) proposed that consolidation is a constant force that increasingly hardens memories from retroactive interference over time and can thus account for the shape of forgetting curves over longer timescales (Bahrick, 1984, as cited in Wixted, 2010). Thus, the temporal transitions in long-term potentiation are consistent with the temporal gradient of retroactive interference, suggesting that older memories are more likely to be consolidated (old long-term potentiation) and stored in long-term memory than newer memories (new long-term potentiation).

Wixted's (2004b) theory of retroactive interference states that old and new long-term potentiation compete for limited physiological resources in the hippocampus: He hypothesized that mental effort is new learning that preferentially leads to new long-term potentiation over old long-term potentiation in the hippocampus. It should be noted that long-term potentiation happens in other brain regions. However, long-term potentiation in the hippocampus could be especially important for consolidation of declarative memory (see section "Retroactive Interference in Recognition Memory"). Indeed, there is a long history of research demonstrating the link between the hippocampus and declarative memory (Scoville & Milner, 1957; Warrington, 1968; Yonelinas, 2002). For example, performance in recall tasks or tasks that increase demands on recall-like processes depends on hippocampal integrity. Three lines of evidence support Wixted's theory of retroactive interference.

First, there is some evidence to support the idea that mental effort disrupts consolidation in the hippocampus (Wixted, 2004a). If the hippocampus is critical for

consolidation, then mental effort should disrupt consolidation more in patients with damage to the hippocampus than in healthy controls. One group of patients who have damage including the hippocampus are amnestic mild cognitive impaired patients. Indeed, compared to controls, these patients show larger mental effort effects that are also temporally graded (Dewar, Della Sala, Beschin, & Cowan, 2010; Dewar, Garcia, Cowan, & Della Sala, 2009), consistent with the hypothesis that mental effort disrupts consolidation in the hippocampus—at least for the recall memory tested in these experiments. However, this conclusion is limited because amnestic mild cognitive impaired patients could have damage that extends to extra-hippocampal regions (Dewar, Pesallaccia, Cowan, Provinciali, & Della Sala, 2012).

Second, there is some evidence to support the idea that decreasing new long-term potentiation in the hippocampus retroactively facilitates memory (Wixted, 2004b). When a group of participants received midazolam—a benzodiazepine that inhibits new long-term potentiation but not old long-term potentiation in the hippocampus—before learning new material, their memory for new material was inhibited. More importantly, inhibiting the learning of new material retroactively facilitated memory for old material compared to a saline group (Reder et al., 2007). In a similar design, alcohol retroactively facilitated prose recall compared to placebo (Moulton et al., 2005). Thus, giving participants a drug to reduce new long-term potentiation in the hippocampus retroactively facilitates memory, suggesting that new long-term potentiation can disrupt old long-term potentiation in the hippocampus.

Third, there is some evidence to support the idea that increasing new long-term potentiation in the hippocampus retroactively interferes with memory. When rats were

given a tetanus (electrical stimulation to select neurons) or when rats explored a novel environment—manipulations that increase new but not old long-term potentiation in the hippocampus—their memory for hidden platform locations (Morris Water Maze task) or memory for shock environments (inhibitory avoidance learning) decreased (Brun, Ytterbo, Morris, Moser, & Moser, 2001; Izquierdo, Schröder, Netto, & Medina, 1999). Additionally, in the Morris Water Maze task, more frequent tetanuses decreased memory for hidden platform locations more than less frequent tetanuses (Brun et al., 2001), consistent with that idea that more new long-term potentiation caused more interference. Moreover, in the inhibitory avoidance learning task, the novel environment exposure decreased memory if exposure occurred one hour after learning but not six hours after learning (Izquierdo et al., 1999), consistent with the idea that new long-term potentiation disrupts consolidation of older memories. One intuitive implication from these findings is that if mental effort is new learning that increases new long-term potentiation and if increasing new long-term potentiation increases retroactive interference, then increasing mental effort should increase retroactive interference effects. One way to test this is to increase the degree of learning new material, which should increase retroactive interference. Indeed, a greater degree of learning new material caused more retroactive interference compared to a lesser degree of new learning (Archer & Underwood, 1951; Barnes & Underwood, 1959; Bäuml, 1996; Briggs, 1957), even when other factors that cause retroactive interference (e.g. retrieval practice and output interference) were controlled (Delprato, 2005). Thus, one possible explanation for why a greater degree of learning new material caused more retroactive interference is that better learning increased mental effort that disrupted consolidation. However, this possibility—that a

greater degree of mental effort increases retroactive interference—has not been directly addressed in recent research.

One possible difference between early mental effort theories and Wixted's (2004b) mental effort theory is related to new learning: Although Wixted's (2004b) hypothesis suggests that mental effort is new learning that increases new long-term potentiation, early mental effort research suggested that new learning may not be required to cause retroactive interference. That is, tasks that did not necessarily tax new learning but that may have increased subjective mental effort caused retroactive interference. For example, some tasks were naming pictures, reading poetry, and solving logic or math problems. Thus, subjective mental effort could indicate when retroactive interference occurs. This view does not describe how mental effort could disrupt consolidation (cf. Müller & Pilzecker, 1900). Nevertheless, it is possible that these mental effort tasks could have caused retroactive interference by increasing new long-term potentiation in the hippocampus (Wixted, 2004b).

Similarity and Response Competition

Early similarity research typically showed that increasing similarity between old and new material decreased memory (McGeoch & McDonald, 1931; McGeoch, 1942; Robinson, 1920, 1927; Skaggs, 1925; Wixted, 2004b). For example, increasing the semantic relatedness between old and new material decreased word recall (McGeoch & McDonald, 1931). This and other similar findings prompted development of the cue-overload paradigm to test an interference theory of similarity.

The cue overload paradigm tests memory for word pairs. First, participants study a list of A-B word pairs: A is the stimulus or cue word, and B is the response or target

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word. Then participants study a second list of A-D word pairs (the cue does not change between lists) or a second list of C-D word pairs (the cue changes between lists). Finally, participants are shown an ambiguous cue word at test (A) and must provide the target. If the task tests retroactive interference, memory for first-list targets is tested (B); if the task tests proactive interference, memory for second-list targets is tested (D). Interference occurs when participants produce more second-list intrusions (retroactive interference) or first-list intrusions (proactive interference) after learning an A-D list compared to learning a C-D list. This is commonly found in RI tasks (Gladis & Braun, 1958; Keppel & Zavortink, 1969; McGeoch, McKinney, & Peters, 1937; McGeoch, 1942), especially when the second list is learned just before recall (rather than earlier, e.g. Chandler & Gargano, 1998). Thus, the ambiguous cue (A) activates two competing responses (B and D), and this is called response competition: Response competition increases the number of second-list intrusions because the more recently presented word pairs (A-D) are more accessible than less recently presented word pairs (A-B).

Response competition theory and its variants are the standard definition of interference today (J. R. Anderson & Bower, 1980; J. R. Anderson, 1974; M. C. Anderson et al., 1994; M. C. Anderson, 2003; Bäuml, 1996; Delprato, 2005; Dosher, 1981; Dyne, Humphreys, Bain, & Pike, 1990; Radvansky & Copeland, 2006; Radvansky, 1999, 2005; Tendolkar, Doyle, & Rugg, 1997; Verde, 2004). For example, building on the cue overload paradigm, the fan effect is the finding that increasing the number of alternative responses (e.g. A-B, A-C, A-D, A-E) increases retrieval time from memory (J. R. Anderson, 1974, but see Radvansky & Copeland, 2006b; Radvansky, 1999, 2005). In situations of response competition, the cue is not uniquely diagnostic of one memory for

a single target; instead, the cue is common across multiple memories of multiple targets, decreasing the effectiveness of the cue in retrieving the paired associate (Nairne, 2002). Thus, one interpretation is that similarity causes interference; that is, memories are similar to the extent that they share a cue. Consistent with this, when testing memory for single words, increasing semantic similarity decreased recall (McGeoch & McDonald, 1931), and cue-overload increases the number of second-list intrusions (McGeoch, 1942). Inconsistent with this, when testing cued recall, increasing synonymy between second-list and first-list targets (i.e., increasing similarity) increases recall (Gladis & Braun, 1958). Therefore, a theory of similarity in which increasing similarity 'always' causes interference or 'always' causes facilitation can only explain a limited set of findings (Nairne, 2002; Osgood, 1949; Robinson, 1927; Skaggs, 1925).

Mental Effort and Similarity

Some theories of retroactive interference have accounted for both mental effort and similarity. For example, as shown in Figure 2, Skaggs (1925) hypothesized that varying similarity can retroactively facilitate or interfere with recall. The far left x-axis shows that repeating material can increase memory (Harden, 1929; Robinson, 1927; Wahlheim & Jacoby, 2012). The middle of the x-axis shows that for different stimuli, increasing similarity can decrease memory (McGeoch & McDonald, 1931; Robinson, 1920). However, there are exceptions to this: Learning different material, such as in the cue-overload paradigm (A-B vs. A-D), can increase and decrease memory (e.g. Wahlheim & Jacoby, 2012) depending on the detection of change between old and new material, at least in a proactive interference version of the cue-overload task (Jacoby, Wahlheim, & Yonelinas, 2013; Jacoby & Wahlheim, 2013; Wahlheim & Jacoby, 2012).

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Finally, the far right x-axis shows that dissimilar material can decrease memory because of mental effort (Müller & Pilzecker, 1900; Skaggs, 1925, 1933).

A more recent model—adapted and shown in Figure 3—treats mental effort and similarity separately rather than as falling along a single similarity dimension (Dewar et al., 2007, fig 2). According to the model, mental effort disrupts hippocampal consolidation, and learning similar stimuli causes response competition (M. C. Anderson, 2003; McGeoch, 1942; Skaggs, 1933). Like Skaggs (1925), this model suggests that learning new material, regardless of similarity, should cause mental effort (see also, Wixted, 2004b). Thus, similar and dissimilar material could disrupt hippocampal consolidation, and similar material, in addition, could cause response competition.

If similar material can disrupt consolidation and induce response competition, then similarity could enhance retroactive interference due to mental effort. For example, in a cue-overload paradigm, studying an A-D list could increase second-list intrusions (D) and disrupt consolidation of A-B items. If consolidation is disrupted, then the A-B memories memory might not move to long-term storage (e.g., Dudai, 2004) or might simply become less discriminable from other memories. If so, then similarity could enhance retroactive interference due to mental effort by increasing A-D accessibility while reducing discriminability among competing memories. Therefore, mental effort and similarity could interact to exacerbate retroactive interference effects.

Retroactive Interference in Recognition Memory

Recognition memory is the ability to tell if something was encountered previously. Recognition memory usually is tested by having participants study a list of items, usually

words or word pairs. At test, participants are shown old and new items, and participants decide whether each item is old or new.

Recognition memory decisions are usually described with signal detection theory (also see Appendix, Macmillan & Creelman, 2005). Signal detection theory separates response criteria from discrimination. Response criteria represents the willingness to respond a certain way (e.g., willingness to respond "old"), and discrimination is the ability to tell two categories apart (e.g. old vs. new). In recognition memory, each old and new item has one memory strength value, and, on average, old items have greater memory strength values than new items. If these memory strength values were known and could be plotted, they would form separate "old" and "new" distributions that vary and overlap. When the old and new memory strength distributions overlap, discrimination is worse compared to when these distributions are relatively non-overlapping. In addition, a response criterion separates both distributions into "old" and "new" decisions: If a memory strength value exceeds the criterion, the item is called "old"; otherwise, the item is called "new". Correct "old" decisions are called hits, and incorrect "old" decisions are called false alarms. Hits and false alarms can be used to calculate discrimination and response criteria (see Appendix for more details).

Recognition memory performance can be explained by two retrieval processes, familiarity and recollection (Atkinson & Juola, 1974; Jacoby & Dallas, 1981; Jacoby, 1991; Mandler, 1980; Parks & Yonelinas, 2007; Wixted, 2007; Yonelinas & Jacoby, 1994; Yonelinas, 2002). Familiarity is the feeling that something occurred, without recollecting when or where it occurred, and recollection is the retrieval of episodic details associated with a prior event. For example, upon seeing a news story, one might feel that

the story is familiar without remembering where or when it occurred. This is familiarity. Upon further thought, one might remember that they read that same story online a day earlier. This is recollection. Thus, on the basis of either familiarity or recollection, one would conclude that one read that news story before.

The dual-process signal-detection model suggests that familiarity occurs to some degree for each item but that recollection occurs for some items but not others (Yonelinas, 1994). Successful recollection leads to high-confidence responses: If a participant recollects episodic details, then they can be confident the item was studied. When recollection is unsuccessful, participants may vary in their confidence because decisions rely on familiarity. The dual-process signal-detection model can be used to estimate the relative contributions of recollection and familiarity to recognition memory performance (see Appendix; Yonelinas & Parks, 2007; Yonelinas, 1994).

The associative recognition task is driven primarily by recollection (Yonelinas, 1997). In this task, participants study a list of word pairs (i.e., A-B, C-D: *daisy-carnival*, *wound-teeth*), and at test, participants discriminate between intact (i.e., A-B, C-D: *daisy-carnival*, *wound-teeth*) and rearranged (i.e., A-D, C-B: *daisy-teeth*, *wound-carnival*) word pairs. On average, each word pair should be equally familiar because they consist of studied items, so familiarity should be relatively non-diagnostic of the pairing. Task performance, therefore, should rely more heavily on recollection of specific word pairings than familiarity. Successful recollection would lead to high-confidence hits for intact word pairs (i.e., correct "intact" decisions) and correct rejections for rearranged word pairs (i.e., correct "rearranged" decisions, also called recollect-reject; Rotello & Heit, 2000; Rotello, Macmillan, & Van Tassel, 2000; Yonelinas, 1997). When

recollection fails, decisions would rely on familiarity, which would lead to hits and to false alarms (i.e., incorrect "intact" decisions) at all levels of confidence.

Recollection and familiarity are considered independent processes mediated by different brain regions. Indeed, damage to the hippocampus preferentially impairs associative recognition memory (an indicator of recollection) over familiarity (Yonelinas, Kroll, Dobbins, Lazzara, & Knight, 1998), and damage to the perirhinal cortex preferentially impairs familiarity over recollection (Bowles et al., 2007, 2010; Yonelinas, 2002). Thus, the evidence suggests that the hippocampus is necessary for recollection, and the perirhinal cortex is necessary for familiarity. Research generally supports this double dissociation, both behaviorally and neurologically (for reviews see Parks & Yonelinas, 2007; Yonelinas & Parks, 2007; Yonelinas, 2002; but also see Wixted, 2007).

As the neurological double dissociation suggests, the hippocampus preferentially drives recollection-driven memory, such as associative memory, whereas the perirhinal cortex preferentially drives familiarity-driven memory, such as item memory (Yonelinas, 2002). Therefore, factors that affect hippocampal consolidation should affect recollection rather than familiarity. Consistent with this hypothesis, one rat study found that blocking hippocampal consolidation decreased associative memory but not item memory (Balderas et al., 2008). By contrast blocking perirhinal cortical consolidation decreased item memory but not associative memory. This double dissociation suggests that brain region-specific consolidation is necessary for brain region-specific memory. Therefore, mental effort that disrupts consolidation in the hippocampus should primarily affect hippocampally-driven memory or memory processes, like recollection. There are two lines of evidence to support this relationship.

First, there is some evidence to suggest that decreasing new long-term potentiation in the hippocampus retroactively facilitates hippocampal-driven memory, but this does not occur for non-hippocampal memory (e.g. item memory). One way to decrease new long-term potentiation in the hippocampus is by manipulating sleep, though this might depend on different sleep stages: Slow-wave sleep may prioritize old long-term potentiation over new long-term potentiation in the hippocampus, whereas REM sleep may prioritize new long-term potentiation over old long-term potentiation in the hippocampus (Mednick, Cai, Shuman, Anagnostaras, & Wixted, 2011). Thus, slow-wave sleep rather than REM sleep could selectively enhance hippocampally-driven memory. Indeed, sleep selectively benefits recollection-driven and hippocampus-driven memory in humans and rats (Inostroza, Binder, & Born, 2013; van der Helm, Gujar, Nishida, & Walker, 2011). This hypothesis is directly supported with recollection and familiarity estimates derived from introspective judgments: Compared to REM sleep or remaining awake, slow-wave sleep increased recollection estimates but not familiarity estimates (Daurat, Terrier, Foret, & Tiberge, 2007). These data are consistent with the hypothesis that sleep selectively enhances consolidation necessary for hippocampally-driven memory and memory processes.

Second, there is some evidence to suggest that consolidation in the hippocampus is temporally graded and is selectively important for hippocampally-driven memory, such as memory for locations or contexts (Yonelinas, 2002). In support of this hypothesis, blocking consolidation in the hippocampus made rats less likely to notice objects displaced from their original locations but did not affect object recognition memory (Oliveira, Hawk, Abel, & Havekes, 2010). This effect only occurred if consolidation was

blocked soon after study compared to a later delay. Thus, hippocampal consolidation may be temporally graded. Therefore, if, as Wixted (2004b) suggests, mental effort associated with new learning increases new long-term potentiation in the hippocampus, then mental effort should selectively affect memory processes thought to depend on the hippocampus. Moreover, as earlier evidence suggested, increasing mental effort might also increase new long-term potentiation. If so, then increased mental effort should affect recollection rather than familiarity. This hypothesis was explored in the current study.

Response competition and similarity effects in recognition memory are mixed. When testing item memory for full-page advertisements, learning similar new advertisements did not affect recognition memory (Heine, 1914, as cited in McKinney, 1935). Also, although cue-overload typically reduces recall, when cue overload was manipulated in associative recognition memory, cue overload decreased hits, decreased false alarms, or did not affect hits and false alarms (Dyne et al., 1990). To explain these mixed results, Verde (2004) proposed that cue overload manipulations affect recollection and familiarity differently and that the pattern of results depend on recollection and familiarity contributions to performance. Thus, cue overload should make recollection less diagnostic of pairings because recollection of an associate would not necessarily produce the correct answer. This should decrease recollection-driven hits. Cue overload also increases the number of items in memory, so this should increase the number of matches between a probe and old memories. Given that familiarity is likely determined, in part, by this type of global matching mechanism (e.g. Ratcliff & McKoon, 2000), this should increase the number of hits and false alarms due to familiarity. This pattern of results is what Verde (2004) found in a fan effect study: Increasing the number of

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associates decreased recollection hits but increased familiarity hits and false alarms. These data suggest that response competition should reduce recollection but that it might affect familiarity-related response criteria or discrimination (i.e., changes in hits and false alarms could signal changes in response criteria or discrimination). For the same reasons, increasing the similarity of the alternative responses (i.e., second-list targets) could reduce recollection and may or may not affect familiarity.

Overall, the literature review suggests that there are two sources of retroactive interference, mental effort and similarity. Wixted (2004b) hypothesized that mental effort associated with new learning increases new long-term potentiation in the hippocampus, disrupting consolidation of old memories (old long-term potentiation). By contrast, depending on the degree of similarity, similarity should promote response competition and thus interference. Therefore, mental effort should selectively disrupt recognition memory processes thought to depend on the hippocampus (recollection, not familiarity), whereas similarity should affect recollection and familiarity in different ways. Research, so far, has focused on free recall and cued-recall tasks with few studies investigating mental effort in recognition memory and no studies considering mental effort and similarity within the same experiment. Mental effort and similarity should affect recall and recognition memory similarly, at least to the extent that they rely on similar recall-like processes (Dyne et al., 1990; Okada, Vilberg, & Rugg, 2012; Yonelinas, 2002)

The Current Study

The current study asked whether mental effort and similarity cause retroactive interference in associative recognition memory. Experiment 1 asked whether the degree of mental effort affects recollection and familiarity estimates differently at different TPIs.

Experiment 2 asked whether the degree of mental effort affects recollection and familiarity estimates differently for higher versus lower similarity.

Experiment 1 asked if mental effort affects recollection. Like past research testing main effects of mental effort (mental effort vs. rest) and main effects of TPI (Lechner et al., 1999; Müller & Pilzecker, 1900; Skaggs, 1925; Wixted, 2004b), Experiment 1 compared associative recognition memory performance after rest and after mental effort presented at different TPIs. The current experiments extended this by inducing different degrees of mental effort at different TPIs (e.g. Archer & Underwood, 1951, as cited in Wixted, 2004b). This design addressed several shortcomings of previous experiments. In the past, rest periods were filled (e.g. reading); compared to mental effort, rehearsal may have been more likely during rest; and different tasks could have induced different degrees of mental effort. In the current experiment, participants rested in silence with the experimenter present; potential differences in rehearsal were reduced by manipulating degree of mental effort; and only one mental effort task was used (math problems). As a manipulation check for rehearsal, participants rated the frequency of spontaneous remindings; as a manipulation check for degree of mental effort, participants rated their subjective experience of mental effort.

Experiment 2 asked whether mental effort and similarity affect recollection and familiarity estimates. The mental effort predictions were the same as in Experiment 1. For similarity, past research showed that cue overload (fan effect) reduced recollection hits and increased familiarity hits and false alarms (Verde 2004). Recollection was reduced because recollection of a response would not necessarily lead to the correct answer. Thus, if the similarity among competing responses is increased, as in Experiment

2, then recollection estimates should decrease. For familiarity, increases in hits and false alarms could indicate a change in response criteria or in discrimination. Therefore, it is unclear if and how similarity would affect familiarity estimates. Finally, an interaction might be expected if learning the A-D list increases both mental effort and similarity effects.

CHAPTER 2

EXPERIMENT 1

The goal of Experiment 1 was to examine whether mental effort and the temporal point of interpolation (TPI) interact to affect recollection and familiarity estimates in an associative recognition task. As Figure 3 shows, participants were randomly assigned to be in a control group, or they were randomly assigned to groups engaged in a low or high mental effort task presented 0-, 10-, or 20-minutes after study. Mental effort was manipulated by requiring participants to mentally solve either easy addition problems (e.g., $2 + 2 = ?$) or difficult subtraction problems (e.g., $83 - 9 = ?$). Solving easy math problems should require less mental effort than solving difficult math problems. Moreover, math problems are highly dissimilar from word pairs, which should minimize response competition effects.

It was hypothesized that solving difficult rather than easy math problems would decrease recollection estimates. Similarly, solving either problem type at earlier rather than later TPIs should decrease recollection estimates. Finally, mental effort and TPI should interact such that more mental effort decreases recollection estimates more at earlier rather than later TPIs. This interaction would support the hypothesis that mental effort causes retroactive interference by disrupting consolidation. Also, recollection estimates should be smaller after any experimental condition than after the control condition, which would replicate early mental effort research. Because evidence suggests that familiarity does not depend on the hippocampus and because there is no evidence that new long-term potentiation in the hippocampus disrupts non-hippocampal memory, mental effort and TPI should not affect familiarity estimates, but only to the extent that

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mental effort selectively disrupts hippocampally-driven memory.

Method

Participants

Participants ($N = 140$, Male = 52) with ages ranging from 18 to 56 years old ($M = 20.92$ years, $SD = 5.45$ years) were recruited from university courses and were randomly assigned to test conditions ($n = 20$ per condition). They were tested individually with the experimenter present and given course credit for participation.

Participants were screened based on their responses to a pre-experimental questionnaire: medical conditions (e.g., memory-related neurological disorders), medications taken (e.g., benzodiazepines), loss of consciousness for more than five minutes, and length of time speaking English (e.g., must be native English speaker or speaking English longer than 20 years). All participants met criteria. However, some participants were excluded due to experimenter error ($n = 6$) and later re-run to equate group sizes.

Materials

Word stimuli. Two lists of 240 nouns with middle Kucera-Francis word frequency values (e.g., *50-150*), four to eight letters in length, were randomly selected from the MRC Psycholinguistic database (Kucera & Francis, 1967). Each list was matched for word frequency, and within each list, words were randomly paired and then scanned for preexisting semantic or orthographic relatedness that might give the word pair a mnemonic advantage or disadvantage over other word pairs (e.g., *chair-table* or *chair-fair*). Each word pair served as intact or rearranged word pairs across conditions, and each word served as a cue or target word across conditions.

Math stimuli. Addition problems and subtraction problems ($n = 35$; randomly selected from larger pool, $N = 1943$) were recorded into 2.0 s audio files by a female research assistant. Analyzed math accuracy scores were the proportion of correctly answered math problems.

Abbreviated Math Anxiety Scale. To control for individual differences in math anxiety, participants responded to the Abbreviated Math Anxiety Scale about how anxious they would feel in scenarios involving math (Hopko, Mahadevan, Bare, & Hunt, 2003). Analyzed Abbreviate Math Anxiety Scale scores were individual averages across all ratings for each scenario.

Post-experimental questionnaire. A questionnaire given after the final test assessed subjective mental effort, rehearsal frequency, and sleep intensity during the retention interval. Participants rated how much mental effort they experienced during the break (*How mentally demanding was the break?*) using a scale numbered from 1 (*minimum mental effort*) to 7 (*maximum mental effort*); participants rated how frequently studied materials came to mind during the break (*How frequently did the words you studied come to mind during the break?*) using a scale numbered from 1 (*not frequently*) to 7 (*very frequently*); and participants rated how intense their sleep was (*Please rate the intensity of your sleep during the break.*) on a scale numbered from 1 (*did not sleep at all*) to 7 (*slept for most or all of the break*).

Procedure

After filling out informed consent forms and pre-experimental questionnaires, participants were tested individually on computers running E-Prime 1.2 (Schneider, Eschman, & Zuccolotto, 2002). Sound was delivered through the monitor's built-in

speakers.

As shown in Figure 3, under incidental learning instructions, participants studied 120 word pairs (*daisy-carnival*, *wound-teeth*), and each word pair was presented one at a time in the middle of the screen for 3.5 s with a .5 s ISI. The order of word pairs was randomized for each participant. While the word pair was on the screen, participants rated how pleasant each word was together using on-screen response options 1 (*pleasant*) and 3 (*not pleasant*). Regardless of whether participants responded within 3.5 s, the computer automatically advanced to the next word pair after 3.5 s. This study section lasted eight minutes.

During the 30-minute retention interval, participants were given different instructions depending on their randomly assigned condition. Participants in the control condition were told to relax and get as comfortable as they could. They could not talk to the experimenter, play on their phones, or surf the internet; they sat quietly with their eyes open. Participants in the interference conditions mentally solved math problems presented over headphones, responded using the keyboard, and received immediate feedback about their accuracy. The order of math problems was randomized for each participant. Once a participant heard a math problem (2.0 s), a response box appeared on the screen for up to 10.2 s. If the participant responded before 10.2 s, then the screen cleared and the correct answer appeared on screen for 1.5 s. After the correct answer appeared, participants saw a fixation cross that cleared after the remainder of the total trial time (12.2 s). If they did not respond after 10.2 s, participants saw the correct answer with the message, "Please respond more quickly". Participants in the low mental effort condition solved easy addition problems, and participants in the high mental effort

condition solved difficult subtraction problems. Although an 85% accuracy cut-off was initially intended to exclude participants who put forth too little or too much effort, all participants were included despite having math accuracy scores less than 85%. This is because the math problems chosen were more difficult than anticipated (see Table 1). Participants solved math problems for eight minutes.

Participants also solved easy or difficult math problems at different TPIs. Participants in the 0-minute condition solved addition or subtraction problems immediately after study and then rested for the remaining 20 minutes. Participants in the 10-minute (or 20-minute) condition first rested for 10 minutes (or 20 minutes), and then solved addition or subtraction problems. Then participants rested for the remaining 12 minutes (or 2 minutes). A sound signaled math problem onset for the 10- and 20-minute conditions, and the same sound signaled test onset after the final break. Instructions for each break period and math problem period were given before participants began each break or solved math problems. The entire retention interval was 30 minutes.

During the associative recognition test, participants rated their confidence in their decisions about whether word pairs were intact or rearranged. Intact word pairs were word pairs that were the same at test as they were at study (*daisy-carnival*, *wound-teeth*), and rearranged word pairs were recombined from original word pairs (*daisy-teeth*, *wound-carnival*). Each word pair appeared one at a time in the middle of the screen, randomized for each participant, with the confidence scale displayed on-screen throughout the test. Ratings *4-6* corresponded to word pairs participants thought were intact, and ratings *1-3* corresponded to word pairs participants thought were rearranged. *Six* meant that participants were *absolutely sure* the word pair was *intact*, 5 meant *less*

sure, and *4* meant *guessing*. It was the same for the other ratings: *1* meant that participants were *absolutely sure* the word pair was *rearranged*, *2* meant *less sure*, and *3* meant *guessing*. Testing was self-paced, and participants were encouraged to spread their responses out across the scale so that over the course of the testing session, all responses were used. After the test, participants filled out the post-experimental questionnaire, were debriefed, and then given course credit.

Results

Experiment 1 examined whether mental effort and the TPI interacted to affect recollection and familiarity estimates in an associative recognition test. All signal-detection and model estimation procedures are briefly outlined in the Appendix. The dependent measures examined were d_a (discrimination between intact and rearranged word pairs), recollection estimates, recollect-reject estimates, familiarity estimates, and post-experimental questionnaire scores. As a reminder of the predictions, increased math problem difficulty (mental effort) should decrease recollection estimates more at earlier (0-minute) than later (10- or 20-minute) TPIs. If mental effort does not affect consolidation outside of the hippocampus (Balderas et al., 2008; Wixted, 2004b) and if familiarity depends on the perirhinal cortex rather than the hippocampus (Parks & Yonelinas, 2007; Yonelinas, 2002), then mental effort should not affect familiarity (*d'*).

Data screening and analysis

Within each condition, data were screened for univariate outliers (|*z*-scores| > 3.29, *p* < .001) and normality assumptions (Shapiro-Wilks test *p* < .05, visual inspection of histograms, and |skewness| and |kurtosis| values > 1). Most data did not meet normality assumptions, so non-parametric analyses are reported for all statistics, unless otherwise

noted. In most cases, no differences in the patterns of results emerged for parametric and non-parametric tests. Differences are footnoted. Alpha was set to .05 for all analyses unless otherwise noted. Marginal significance was considered if a *p*-value was between .05 and .10.

If the data met parametric assumptions, the main effect of mental effort—including the control condition—was assessed using a one-way ANOVA. The main effect of TPI and the mental effort by TPI interaction were assessed as part of a 2 x 3 ANOVA. In addition, planned *t*-tests were conducted on marginal means to assess the mental effort and TPI main effects. Also, planned *t*-tests were conducted on means in the mental effort by TPI conditions to assess the interaction.

If the data did not meet parametric assumptions, the main effects of mental effort (including the control condition) and TPI (excluding the control condition) were assessed using the Kruskall-Wallis test. In addition, planned comparisons using Mann Whitney *U*-tests were conducted on marginal means to assess main effects. The non-parametric interaction was assessed using the adjusted rank transformation test, which analyzes individual interaction scores predicted by the general linear model (Leys & Schumann, 2010). Each observed score is a linear combination of the grand mean ($\mu_{...}$), factor effects, interactions, and sampling error. Given that the grand mean is constant and sampling error varies around 0, Leys and Schumann (2010) suggested that only the marginal factor means need to be removed to reveal the interaction scores (see also Rosnow & Rosenthal, 1995). To do this, each row and column marginal mean was subtracted from each individual score. Technically, the remaining score includes the grand mean (baseline), the predicted interaction score, and sampling error. Then, like other non-parametric

techniques, the remaining individual scores are rank averaged disregarding condition. Finally, an ANOVA is performed on the averages of the predicted interaction rankings. Only the interaction term is interpreted. In addition to this analysis, planned t-tests were conducted on the interaction rankings from the mental effort by TPI conditions to assess the non-parametric interaction. Reported effect size for non-parametric main effects are based on χ^2, called Cramer's V, which can be considered as the proportion of maximum possible variation accounted for by a factor.

In the non-parametric interaction analyses, the marginal mean from the control condition was not subtracted from each individual score and was not included in rankings. This is because the primary interest of the interaction was between mental effort and TPI, disregarding the control condition. This resulted in a 2 x 3 between-subjects ANOVA where only the interaction term was interpreted (see previous paragraph). Disregarding the control condition, however, limited direct comparisons between rankings in these conditions and raw control condition scores.

Another set of planned comparisons used t-tests (parametric) or Mann Whitney U-tests (non-parametric) to assess differences between the control condition and each of the other conditions. This analysis was conducted as a replication of early mental effort research comparing memory after rest and after a mental effort task.

A final set of planned comparisons used t-tests to compare each group score to the lowest value possible. For example, for scale items, the lowest score was a '1'. For primary measures, this score was often '0'. This analysis was conducted to assess whether the mean scores in each condition were greater than the lowest value.

Descriptive and inferential statistics are presented in two Tables: Table 1 contains

post-experimental questionnaire measures, and Table 2 contains the primary measures. Although mostly non-parametric tests were conducted, means and standard deviations are presented in the tables to facilitate understanding of the patterns in the data. In addition, the results of each statistical test is presented in these tables, including the main effects, interactions, and planned comparisons. For significant ($p < .05$) and marginally significant effects ($.05 < p < .10$), results and average non-parametric rankings (or means, if applicable) are presented in the main text. Otherwise, the main text presents only high-level summaries of the results.

Counterbalances

To determine that counterbalancing did not affect any outcome, a series of 2 x 2 between-subjects parametric ANOVAs were conducted for each dependent variable. No statistically significant counterbalance effects emerged, although some marginally significant effects did occur (all ps > .05). These analyses are not reported further.

Post-experimental questionnaire data and math measures

Presented next are results for post-experimental ratings of subjective mental effort, rehearsal, and sleep intensity. Also included are analyses for Abbreviated Math Anxiety Scale scores and math accuracy. Significance and effect sizes of omnibus tests along with means and standard deviations are presented in Table 1.

How mentally demanding was the break? As Table 1 shows, the main effect of mental effort was significant, $\chi^2(2) = 34.125$, $p < .0001$, Cramer's $V = .349$. Planned Mann Whitney U-tests on marginal means revealed that participants experienced less mental effort between the easy and difficult conditions, $M_{Easy} = 41.83$ and $M_{Difficult} = 79.17$, $p < .0001$; and marginally less in the easy than in the unfilled conditions, $M_{Easy} =$

37.91 and $M_{Unfilled} = 48.28$, $p = .064$. There were no differences in mean non-parametric rankings between the difficult and unfilled conditions. The main effect of TPI and the interaction were not significant.

Planned Mann Whitney U-tests comparing each condition to the control condition revealed that participants experienced more mental effort in the unfilled condition than in the Easy-10-minute condition (non-parametric rankings: $M_{Unfilled} = 23.95 > M_{Easy,\ 10\ Minutes} = 17.05$, $p = .043$). This comparison was marginal for the Easy-20-minute condition (non-parametric rankings: $M_{Unfilled} = 23.63$ and $M_{Easy,\ 20\ Minutes} = 17.18$, $p = .054$). Planned t-tests comparing each group to the lowest scale value revealed that each group experienced more mental effort than 1 (ts(19) > 2.666, ps < .015), suggesting that participants reported some level of mental effort relative to none at all. Overall, the mental effort manipulation changed subjective mental effort ratings, although not always in the manner predicted.

How frequently did the words you studied come to mind during the break? As Table 1 shows, the main effects of mental effort, of TPI, and the interaction were not significant, suggesting that participants rated their degree of rehearsal similarly across conditions. Planned Mann Whitney U-tests comparing each condition to the control condition confirmed these findings. Planned t-tests comparing each group to the lowest scale value revealed that participants indicated that studied words frequently came to mind during the break (ts(19) > 2.666, ps < .015).

Please rate the intensity of your sleep during the break. As Table 1 shows, the main effect of mental effort, of TPI, and the interaction were not significant, suggesting that participants rated their sleep intensity similarly across conditions. Planned Mann

Whitney *U*-tests comparing each condition to the control condition confirmed these findings. Planned *t*-tests comparing each group to the lowest scale value revealed that participants indicated that sleep intensity was greater than 1 (ts(19) > 2.643, ps < .016), suggesting that participants' sleep intensity was greater than no sleep.

Average Abbreviated Math Anxiety Scale scores. As Table 1 shows, the main effect of mental effort, of TPI, and the interaction were not significant, suggesting that participants had similar Abbreviated Math Anxiety Scale scores across conditions. Thus, math anxiety did not confound any effect on the primary measures. Planned Mann Whitney *U*-tests comparing each condition to the control condition confirmed these findings. Planned *t*-tests compared each group to the lowest scale value, and this analysis revealed that participants had Abbreviated Math Anxiety Scale scores greater than 1 (ts(19) > 3.199, ps < .005), suggesting that participants in each group had at least some math anxiety relative to none at all.

Math Accuracy. As Table 1 shows, a 2 x 3 parametric ANOVA (no control condition) revealed a significant main effect of mental effort, $F(1, 114) = 157.616$, $p < .0001$, $\eta_p^2 = .580$, with larger math accuracy scores in the easy condition ($M = 99.4\%$) than in the difficult condition ($M = 69.23\%$), suggesting that participants did better on the easy than difficult math problems. The main effect of the TPI and the interaction were not significant. These results confirm that participants performed more poorly on the difficult than on the easy math problems.

Signal-detection and model estimates

Presented next are the signal-detection measures and model estimates, and the model is described in more detail in the **Error! Reference source not found.**.

Significance and effect sizes of omnibus tests along with means and standard deviations are presented in Table 2.

d$_a$. As Table 2 shows, the main effect of mental effort, of TPI, and the interaction were not significant, suggesting that participants discriminated intact from rearranged word pairs to a similar degree across conditions. Planned Mann Whitney *U*-tests comparing each condition to the control condition confirmed these findings. Planned *t*-tests comparing each group to the lowest theoretical value revealed that participants had *d$_a$* scores greater than 0 (ts(19) > 7.910, ps < .0001), suggesting that participants discriminated intact from rearranged word pairs in each group more than what would be expected by chance.

Dual-process signal-detection model fit. As indicated in the Appendix, recollection, recollect-reject, and familiarity estimates were derived using the dual-process signal-detection model (Yonelinas, 1994). Before conducting statistical analyses on process estimates, it was important to determine how well the model fit individual subjects' confidence response data. Model fit indices were calculated based on χ^2 with 2 *df*. If an estimated χ^2 was less than the critical χ^2 value (3.84), then the model fit individual subject data adequately ($p > .05$).

Within each condition, the percentage of participants for which the dual-process signal-detection model did not fit the data ($p < .05$) was calculated. This descriptive analysis revealed that the model fit the data for most participants across conditions. The largest number of participants for which the model did not fit was four participants in the Difficult-20 minute condition (80% had good model fit). The smallest number of participants was one participant in the unfilled condition and the Difficult-0 minute

condition (95% had good model fit). Although the estimates for these participants may not have been estimated properly, it was still decided to retain these participants for analysis given that the goal was to generalize any effects to the population. Nevertheless, inclusion and exclusion of these participants did not change the pattern of results, so statistics with all participants included are reported.

Recollection. As shown in Table 2, the main effect of mental effort was not significant, but the main effect of TPI was significant, $\chi^2(2) = 7.94$, $p = .019$, Cramer's $V = .168$. As Figure 5 shows, planned Mann Whitney U-tests on marginal means revealed larger recollection estimates after 20 minutes compared to 0- or 10-minutes (U-tests: M_0 $_{Minutes} = 40.93 = M_{10\ Minutes} = 40.08$, $p = .870$; $M_{0\ Minutes} = 33.88 < M_{20\ Minutes} = 47.13$, $p = .011$; $M_{10\ Minutes} = 34.48 < M_{20\ Minutes} = 46.53$, $p = .020$). The interaction was not significant. Planned Mann Whitney U-tests testing the interaction revealed partial support for the TPI main effect (U-tests: $M_{Easy,\ 0\ Minutes} = 16.73 < M_{Easy,\ 20\ Minutes} = 24.28$, $p = .041$; $M_{Easy,\ 10\ Minutes} = 17.15 < M_{Easy,\ 20\ Minutes} = 23.85$, $p = .070$). Planned Mann Whitney U-tests comparing each condition to the control condition were not significant. Planned t-tests comparing each group to 0 revealed that all recollection estimates were greater than 0 (ts(19) > 6.216, ps < .0001), suggesting that recollection did occur at least to some degree within each condition.

Recollect-reject. As Table 2 and Figure 6 show, the main effect of mental effort was marginally significant, $\chi^2(2) = 5.542$, $p = .063$, Cramer's $V = .141$. Planned Mann Whitney U-tests assessing the mental effort marginal means revealed that recollect-reject estimates were smaller after some mental effort than after rest: $M_{Unfilled} = 49.65 > M_{Easy} = 37.45$, $p = .023$; $M_{Unfilled} = 48.70 > M_{Difficult} = 37.77$, $p = .044$. The main effect of TPI was

not significant. Planned Mann-Whitney U-tests assessing the TPI marginal means revealed a marginally significant difference between the 10-minute and 20-minute conditions, $M_{10\ Mins} = 44.75 > M_{20\ Mins} = 36.25$, $p = .057$.

The non-parametric interaction was significant, $F(2, 114) = 5.60$, $p = .005$, $\eta_p^2 = .09$. Planned t-tests on interaction rankings suggested that mental effort mattered for the 0-minute and 10-minute conditions, but the direction was opposite of what was predicted for the 10-minute condition (interaction rankings: $M_{Easy,\ 0\ Mins} = 71.50$, $M_{Difficult,\ 0\ Mins} = 48.00$, $M_{Easy,\ 10\ Mins} = 37.05$, $M_{Difficult,\ 10\ Mins} = 58.35$, $M_{Easy,\ 20\ Mins} = 82.50$, and $M_{Difficult,\ 20\ Mins} = 66.05$; results: $M_{Easy,\ 0\ Mins} > M_{Difficult,\ 0\ Mins}$, $p = .024$; $M_{Easy,\ 10\ Mins} < M_{Difficult,\ 10\ Mins}$, $p = .037$; $M_{Easy,\ 20\ Mins} = M_{Difficult,\ 20\ Mins}$, $p = .106$). Additionally, recollect-reject estimates were smallest in the 10-minute condition compared to the other TPI conditions for easy math problems (interaction rankings: see above; results: $M_{Easy,\ 10\ Mins} < M_{Easy,\ 0\ Mins}$, $p = .003$; $M_{Easy,\ 10\ Minutes} < M_{Easy,\ 20\ Mins}$, $p < .0001$). There were no differences among TPI conditions for difficult math problems[1].

Planned Mann Whitney U-tests comparing each condition to the control condition were significant for the following conditions: Easy-10 Minute ($M_{Unfilled} = 24.70 > M_{Easy,\ 10\ Mins} = 16.30$, $p = .013$); Difficult-0 Minute ($M_{Unfilled} = 24.25 > M_{0\ Minutes} = 16.75$, $p = .024$); Difficult-20 Minute ($M_{Unfilled} = 25.05 > M_{20\ Minutes} = 15.95$, $p = .006$); and marginally significant for Easy-20 Minute ($M_{Unfilled} = 23.60 > M_{Easy,\ 20\ Mins} = 17.40$, $p = .062$). In each case, recollect-reject estimates were larger in the control condition than in the other

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[1] The parametric tests revealed a different pattern of results for the interaction. This different pattern of results likely occurred because of low marginal mean values that potentially reversed the pattern. The interaction was significant, $F(2, 114) = 5.463$, $p = .005$, $\eta_p^2 = .087$. Uncorrected follow-up t-tests revealed that, for the 10-minute condition, recollect-reject estimates were larger in the Difficult ($M = .135$) than Easy ($M = .033$) conditions, $p = .005$. In addition, for the Difficult condition, recollect-reject estimates were larger in the 10-minute condition ($M = .135$) than in the 0-minute condition ($M = .050$, $p = .018$) and in the 20-minute condition ($M = .025$, $p = .022$).

conditions, suggesting that solving easy or difficult math problems at various delays decreased recollect-reject estimates. Planned t-tests compared each group to 0 and revealed that recollect-reject estimates were greater than 0 (ts(19) > 2.108, ps < .049), except for the recollect-reject estimates in the Difficult-20 Minutes condition (t(19) = 2.044, p = .055). These comparisons suggest that participants in most groups relied on recollect-reject to some extent in the associative recognition task.

Familiarity. As Table 2 shows, the main effect of mental effort, of TPI, and the interaction were not significant, suggesting that familiarity estimates were similar across conditions (see also Figure 7). Planned Mann Whitney U-tests comparing each condition to the control condition confirmed these findings. Planned t-tests comparing each group to 0 revealed that all familiarity estimates were greater than 0 (ts(19) > 4.549, ps < .0001), suggesting that all groups discriminated intact from rearranged items on the basis of familiarity more than what would be expected by chance.

Discussion

Experiment 1 tested whether the degree of mental effort at different TPIs affected recollection and familiarity estimates in an associative recognition task. Experiment 1 showed a medium-sized main effect of TPI, such that solving math problems 0 or 10 minutes after study decreased recollection estimates relative to 20 minutes. This TPI effect was specific to recollection estimates: TPI did not affect familiarity estimates. In addition, mental effort and the mental effort by TPI interaction were not significant. These data are most consistent with the hypothesis that mental effort presented earlier rather than later after study disrupted consolidation necessary for recollection.

Mental effort and TPI inconsistently affected recollection and recollect-reject

estimates. First, mental effort decreased recollect-reject but not recollection estimates relative to the control condition. Second, TPI affected recollection but not recollect-reject estimates. Third, the interaction showed that recollect-reject estimates increased, decreased, or did not change with mental effort depending on the TPI condition. If recollection and recollect-reject depend on the hippocampus, mental effort and TPI should have affected them similarly. In addition, the facilitating mental effort effects are not predicted by any theory.

One reason for the inconsistent and null mental effort effects may be that mental effort was not manipulated appropriately. The current experiments assumed that increasing the mental demand of nonspecific tasks would cause retroactive interference, but this prediction was not consistently supported. Thus, these results join past research finding inconsistent mental effort effects: In some cases, mental effort decreased recall relative to rest (Baldwin & Shaw, 1895; Calkins, 1896; DeCamp, 1915; Lechner et al., 1999; Lewy, 1895; Müller & Pilzecker, 1900; Münsterberg & Campbell, 1894; Skaggs, 1925; Warren & Shaw, 1895), but, in other cases, mental effort did not affect recall in healthy controls (Dewar et al., 2010). Thus, the current results do not support the hypothesis that subjective mental effort, in and of itself, causes RI.

Additional lack of support for the subjective mental effort hypothesis comes from the finding that performance did not change consistently with subjective mental effort ratings. Consistent with predictions, participants reported more subjective mental effort after solving difficult rather than easy math problems. Inconsistent with predictions, participants reported less subjective mental effort in the control condition and high mental effort condition compared to the low mental effort condition, regardless of TPI.

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This latter result could have occurred because the question probed different information between the mental effort and control conditions. Perhaps control participants rated the content of their thoughts for mental effort, whereas participants in the math problem conditions rated the mental demand caused by solving the math problems. Thus, participants may have had different reference points for the scale (i.e., referencing different information and then basing their decisions on that referenced information).

Another possible reason for the inconsistent mental effort effects is that current and past experiments did not account for long-term potentiation in the hippocampus: Some manipulations of mental effort may increase or decrease new long-term potentiation in the hippocampus to different extents (e.g. novel environment exposure, Wixted, 2004b), which could cause different amounts of retroactive interference. If so, then the current results suggest that subjective mental effort and new long-term potentiation in the hippocampus are independent—at least in the current experiments. This independence would make it difficult to find behavioral manipulations that affect new long-term potentiation in the hippocampus.

Recollection estimates were similar in the high mental effort condition and the control condition, but they were smaller in the control condition than in the 20-minute condition. These results are problematic because they suggest that the control condition did not control mental effort. One reason for the lack of control is that participants could have been pre-occupied with their thoughts, and these thoughts could have been 'mentally effortful'. Often, participants indicated they thought about what they were going to do after the experiment or about an upcoming test. However, it is unclear why 30 minutes of this would somehow be worse for recollection than 22 minutes of planning

and eight minutes of solving easy or difficult math problems. Overall, while the TPI effect is consistent with a consolidation hypothesis, the other mental effort effects are difficult to interpret.

CHAPTER 3

EXPERIMENT 2

The goal of Experiment 2 was to examine how mental effort and similarity affect recollection and familiarity estimates in a cue-overload version of the associative recognition task. As Figure 8 shows, participants were randomly assigned to be in a low or high mental effort group. Mental effort was manipulated between-subjects by having participants solve easy or difficult anagrams of second-list targets in between study and test. Similarity was manipulated within-subjects by having participants learn a second list of word pairs where the targets were similar (synonyms) or dissimilar (random) to first-list targets (e.g., study: A-B; interference: A-B' or A-D).

If increased mental effort increases retroactive interference, then participants who solve difficult anagrams should have smaller recollection estimates than participants who solve easy anagrams. If similarity increases response competition, then studying similar word pairs should increase competition between responses (Gibson, 1940; McGeoch, 1942). Past research found that increasing the number of associated responses in the cue-overload paradigm decreased recollection hits and increased familiarity hits and false alarms (Verde, 2004). Thus, mental effort and similarity were expected to reduce recollection estimates. Like Experiment, mental effort should not affect familiarity estimates because familiarity does not seem to depend on the hippocampus (Eichenbaum, Yonelinas, & Ranganath, 2007). In contrast to mental effort, similarity could affect familiarity in two ways: It might affect familiarity estimates, a measure of discrimination, or it might affect response criteria (Verde, 2004). To measure familiarity in the current experiments, however, familiarity was estimated across different response criteria (see

Appendix). It is currently unknown if mental effort and similarity interact to affect memory performance. However, any observed interaction would help understanding of the roles of mental effort and similarity in RI (see General Discussion).

Method

Participants

Experiment 2 had 64 participants (Males = 9) with ages ranging from 18 to 64 years old (M = 20.84 years, SD = 6.85 years) were randomly assigned to conditions (n = 32 per condition). Participant selection and exclusionary criteria were the same as in Experiment 1. Participants were tested individually with the experimenter present for the procedure.

Materials

Word stimuli. Initially, the top 120 synonym pairs were selected from prior research because they had similarity ratings (Dey, 1969; Hilgard, 1951). However, for counterbalancing purposes, additional lists were needed, so a total of 960 randomly selected words (Kucera & Francis, 1967) were added to these words. These words were given to two independent groups (n = 10): One group rated pairs for similarity, and another group solved difficult anagrams of each word. These ratings served as the basis for word selection, where, ultimately, 16 lists of single words were constructed and used in the experiment. Each list contained 30 words. These lists were balanced for word frequency and anagram accuracy, with difficult anagram accuracy ranging from 58.0% to 66.7%. This resulted in two similar word pair lists (A-B') and two dissimilar word pair lists (A-D) that were rotated across participants. Similar lists (scale: *not similar* 0 to 3 *very similar*, see Hilgard, 1951) had average similarity ratings of 2.39 and 2.48, and

dissimilar lists had average similarity ratings of .32 and .37 (cf. Dey, 1969).

Counterbalancing. Four lists of words were created, which included cue words and target words for the study and interference phases. The cue words (A) were the same cue words for all participants, and the study targets (B) changed across participants. For the interference lists, similar targets (B') were necessarily yoked to the study targets (B). For ease, the dissimilar targets (D) also were yoked to the study targets (B). Although the study (B) and similar targets (B') did not change across participants, the study (B) and dissimilar targets (D) changed across participants such that these targets served as study or dissimilar targets equally often across participants. However, only 60 dissimilar and 60 similar word pairs were shown to a participant at a time. In addition, all A-B word pairs were interfered with using similar and dissimilar word pairs across participants. Each participant studied 120 A-B word pairs and then studied a second 120 word-pair list, with half consisting of similar A-B' word pairs and the other half consisting of dissimilar A-D word pairs .

Post-experimental questionnaire. Participants responded on a 10-point scale, with 10 being the highest value and 1 being the lowest value, to the following questions: *How hard did you try to solve the anagrams?*; *How often did you think about the word pairs from the pleasantness judgments task?*; *How difficult did you find the anagram task?*; *How often did you think about the word pairs from the anagram task?*; *During the break periods, how often did you rest or sleep?*; and an open-ended question assessed whether participants were aware of the similarity manipulation, *Did you notice anything about the word pairs in the anagram task? If so, what did you notice?*.

Procedure

Experiment 2 is briefly outlined in Figure 8. As an overview of the procedure, participants first were given two practice sessions designed to familiarize them with the anagram procedure. Then, participants began a pleasantness judgment task, which served as an incidental learning procedure for the A-B word pairs. Afterward, participants began the break section, completed the anagram procedure, and then had another break section. Similarity and mental effort were manipulated within the anagram task. Finally, participants took an associative recognition test and were given a post-experimental questionnaire. Then they were debriefed.

Because the anagram task was difficult to understand, the experimenter helped participants practice the anagram task in two different ways before starting the pleasantness judgments task. This allowed the experimenter to assess participant understanding and took approximately 10-15 minutes per participant. The first practice phase was untimed and allowed the participant to become familiar with applying an anagram rule (see below) for four trials. The second practice phase was timed and mimicked the later anagram task for 16 trials, 4 trials for each of the four word lengths (4-7 letters).

After participants finished practicing, participants were instructed on how to respond in the pleasantness judgment task. The pleasantness judgment task was the same as in Experiment 1, except participants read aloud each word pair while making the pleasantness judgments. This study section lasted eight minutes.

In the retroactive interference phase, using the same instructions as in Experiment 1, participants were told to rest for 10 minutes. Then they heard a sound that signaled the

start of the anagram task. The experimenter reviewed the anagram task instructions with the participants, and then pressed the spacebar to begin the anagram task when they were ready. The experimenter recorded all correct, incorrect, and missed answers, as well as when participants mispronounced words.

On any given trial in the anagram task, participants first saw and read aloud a cue word, which was the same as what they saw during study. After 1.5 s, the second-list target appeared next to the cue, but this target was "hidden" in an anagram. This second-list target was either similar to (synonym) or dissimilar from the first-list target. Each second-list target was scrambled according to an easy rule (*daisy-icrcle*: 213456, *wound-apper*: 21345) or a difficult rule (*daisy-iclecr* 245613, *wound-aerpp*: 24513), depending on the mental effort condition. The rule always appeared below the anagram. Once the anagram appeared, participants had 4.5 s to solve it and say the answer out loud. After 4.5 s, a neutral sound signaled the end of the trial, and then participants saw the correct answer on screen for 1.0 s. They read aloud this answer if they did not solve the anagram in time or if they solved the anagram incorrectly. The anagrams were presented in one of sixteen fixed random orders to allow the experimenter to record participant responses. The anagram order was restricted such that no one similarity word type (similar or dissimilar) appeared more than three times in a row. The anagram task lasted for 15 minutes, after which, participants took a 5-minute break. Then, the participant heard a sound to signal test onset. The entire retention interval lasted for 30 minutes.

Like Experiment 1, during the test phase, participants rated their confidence in their decisions about whether intact and rearranged pairs were intact or rearranged. After the test, participants completed the post-experimental questionnaire, were debriefed, and

then assigned course credit.

Results

Experiment 2 tested how mental effort and similarity affect recollection and familiarity estimates in a cue-overload version of the associative recognition task. The same signal detection measures and process estimates from Experiment 1EXPERIMENT 1 were used in Experiment 2 (see Appendix). To assess how well participants solved anagrams, an anagram accuracy measure was calculated as the proportion of correctly solved anagrams.

Data screening and analysis plan

In general, all dependent variables were analyzed in the same way as in Experiment 1, unless otherwise noted. Within conditions, each dependent variable was examined for univariate outliers ($|z$-scores$| > 3.29$, $p < .001$), normality assumptions (Kolmogorov-Smirnov tests, $p < .05$, visual inspection of histograms, and |skewness| and |kurtosis| values > 1), homogeneity of variance assumptions (Levene's test, $p < .05$), and sphericity assumptions (Mauchly's test of sphericity, $p < .05$).

If the data met parametric assumptions, a 2 x 2 between-within ANOVA was conducted to examine the interaction between mental effort (between-subjects) and similarity (within-subjects), and main effects were examined but interpreted in terms of statistically significant interactions. Planned t-tests were conducted on marginal means and cell means to assess main effects and interactions, respectively. If the data did not meet parametric assumptions, a non-parametric 2 x 2 between-within ANOVA was conducted on rankings of individual interaction scores (Leys & Schumann, 2010). Main effects were examined using Mann-Whitney U-tests (between-subjects: mental effort) and Wilcoxon Signed Rank Test (within-subjects: similarity). Alpha was set at .05 for all tests, and marginal significance was considered if a p-value was between .05 and .10. Statistics are reported with all participants unless otherwise noted. As before, descriptive and inferential statistics are presented in Table 3

Table 3 (post-experimental data) and Table 4 (primary measures).

Post-experimental questionnaire data

As Table 3 shows, the post-experimental questionnaire scores were, for the most part, unrelated to the manipulations. Non-parametric and parametric independent-samples comparisons revealed significant differences as a function of anagram difficulty for the following variables: non-parametric rankings: *How difficult did you find the anagram task?*, $M_{Difficult} = 43.09 > M_{Easy} = 21.91$, $p < .0001$; and marginally significant differences for *How hard did you try to solve the anagrams?*, $M_{Difficult} = 36.31$ and $M_{Easy} = 28.69$, $p = .090$. Thus, participants rated that the difficult anagrams were more difficult than the easy anagrams.

Anagram accuracy. A Table 3 shows, parametric analyses revealed that participants solved more easy than difficult anagrams, $F(1, 62) = 41.61$, $p < .0001$, $\eta_p^2 = .402$; more similar than dissimilar anagrams, $F(1, 62) = 10.41$, $p = .002$, $\eta_p^2 = .144$; and the interaction was also significant, $F(1, 62) = 12.95$, $p = .006$, $\eta_p^2 = .173$. Planned *t*-tests revealed significant differences: Similar ($M_{Easy} = .968 > M_{Difficult} = .744$, $t(33.12^2) = 6.23$, $p < .0001$); Dissimilar ($M_{Easy} = .970 > M_{Difficult} = .692$, $t(32.72^3) = 6.42$, $p < .0001$); and Difficult ($M_{Similar} = .744 > M_{Dissimilar} = .692$, $t(31) = 3.79$, $p = .001$), except for the difference between anagram accuracy for: Easy ($M_{Similar} = .968$ and $M_{Dissimilar} = .970$, $t(31) = -.43$, $p = .672$). This pattern of results suggests that participants solved more easy than difficult anagrams; however, for difficult anagrams, participants solved more similar than dissimilar targets, but the same difference was not true for easy anagrams.[4]

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[2] Levene's test was significant, $F(1, 62) = 57.9$, $p < .0001$, indicating different variances between groups.
[3] Levene's test was significant, $F(1, 62) = 60.6$, $p < .0001$, indicating different variances between groups.
[4] It should be noted that the non-parametric tests revealed a different pattern of results. This likely arose because of ceiling effects that may have affected the predicted interaction rankings for easy anagrams. The main effect of anagram difficulty was significant $U(128) = 325.00$, $p < .0001$, suggesting that participants

Signal-detection and model estimates

Presented next are the signal-detection measures and model estimates (see **Error! Reference source not found.**).

d_a. As Table 4 shows, the main effect of anagram difficulty, of similarity, and the interaction were not significant, suggesting that d_a was similar across conditions.

Dual-process signal-detection model fit. As in Experiment 1, descriptive analyses were conducted to determine whether the estimates derived using the dual-process signal-detection model were based on a good-fitting model (Yonelinas, 1994). Within each condition, the percentage of participants for which the dual-process signal-detection model fit the data ($p > .05$) was calculated. This descriptive analysis revealed that the model fit the data for all participants across conditions, except for one person in the Difficult-Similar condition. Statistics with all participants included are reported given that the results were the same including and excluding that one participant.

Recollection. As Figure 9 shows (see also Table 4), the main effect of anagram difficulty was marginally significant, $U(128) = 1.709$, $p = .087$, with numerically larger recollection estimates after solving difficult rather than easy anagrams ($M_{Difficult} = 70.09 > M_{Easy} = 58.91$). The main effect of similarity and the interaction were not significant.

Recollect-reject. As Figure 10 shows (see also Table 4), the main effect of anagram difficulty, of similarity, and the interaction were not significant, suggesting that recollect-reject estimates were similar across conditions.

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successfully solved more difficult than easy anagrams. The main effect of similarity was significant, $W(64) = 857.00$, $p < .01$, suggesting that participants solved more dissimilar anagrams than similar anagrams. The interaction was also significant, suggesting that the above results occurred because participants solved fewer easy, similar anagrams than any other target type ($ps = .0001$ to $.029$). No other significant differences emerged. These data suggest that average rankings of anagram hit rates were lowest in the easy similar anagram condition

Familiarity. As Figure 11 shows (see also Table 4), the main effect of anagram difficulty, of similarity, and the interaction were not significant, suggesting that familiarity estimates were similar across conditions. Listwise exclusion of one outlier and the participant for which the dual-process signal-model provided a poor fit did not change the results.

Discussion

The goal of Experiment 2 was to examine whether mental effort and similarity affect recollection and familiarity estimates in an associative recognition task. To this end, mental effort was controlled within each level of similarity. Participants solved either easy or difficult anagrams of cue-overloaded word pairs in which the second-list target was similar to (synonym) or dissimilar from the first-list target. The results showed marginal and non-significant effects: The main effect of mental effort on recollection estimates was marginally significant, but the effect was in the opposite direction of predictions

Experiment 2 failed to detect similarity effects on any memory measure. One possible reason why similarity did not cause retroactive interference is because the current study had participants learn A-D and A-B' word pairs 10 minutes after study rather than just before test. Another possible reason is that studying the second-list list could have produced an equal amount of facilitation and interference. This might be expected if, for some A-D and A-B' word pairs, participants noticed and later recollected the change at test, as was shown in a proactive interference version of the cue-overload task (Jacoby et al., 2013; Wahlheim & Jacoby, 2012; Wahlheim, 2014). Noticing and later recollecting change can produce proactive facilitation, whereas failing to recollect

that change can produce proactive interference. If these proactive interference effects generalize to similar retroactive interference paradigms, then studying a second list could produce null cue-overload effects. However, similarity should have made change detection more difficult, which should increase retroactive interference. It should be noted, though, that the retroactive interference experiments used orthographically similar materials, suggesting that change detection effects can occur for similar materials.

In addition to the null similarity effects, the null mental effort effects were replicated from Experiment 1. One possible reason for the null mental effort effects in Experiment 2 is that all participants solved anagrams 10 minutes after study rather than immediately after study. Mental effort effects should be greatest when presented immediately after study rather than after a delay. However, Experiment 1 showed null mental effort effects even when presented 0 minutes or 10 minutes after study, suggesting that a longer delay is necessary (i.e., 20 minutes).

Another possible reason for the null mental effort effects in Experiment 2 is that the recollection and familiarity estimates in Experiment 2 may have not been accurate because recollection and familiarity were estimated based on fewer responses than what is typically recommended (Yonelinas & Parks, 2007). This could have made these estimates less sensitive to retroactive interference effects. However, although the number of responses collected differed across experiments, the results were replicated. Thus, the current results join other studies finding weak or null mental effort effects (Dewar et al., 2010), suggesting that mental effort may not cause retroactive interference.

The results are consistent with the view that subjective mental effort is not sufficient to observe retroactive interference. This is supported by the finding across

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experiments that subjective mental effort was present and manipulated successfully, and retroactive interference was not found. In addition, the results with similarity are not consistent with past research demonstrating response competition effects. Future research should account for other factors that may be responsible for detecting retroactive interference in recognition memory (e.g., remindings and TPI).

CHAPTER 4

GENERAL DISCUSSION

The goal of this research was to examine some potential causes of retroactive interference and whether they affect associative recognition memory. Early researchers found evidence for two causes of retroactive interference, mental effort and similarity. Mental effort was thought to disrupt consolidation, and similarity was thought to promote response competition during retrieval. Recent neurological evidence suggests that new long-term potentiation disrupts old long-term potentiation in the hippocampus, consistent with the hypothesis that mental effort disrupts consolidation by increasing new long-term potentiation in the hippocampus. Moreover, similarity may promote response competition, which should reduce recollection estimates but may or may not affect familiarity estimates (e.g., Verde, 2004). Finally, learning new material that is similar to old material may cause retroactive interference because learning new material could have both specific (e.g., response competition) and nonspecific (e.g., mental effort) effects (Dewar et al., 2007; Wixted, 2004b), which could lead to an interaction between these two types of RI.

How Should Mental Effort Be Defined?

The current results do not support the hypothesis that mental effort causes retroactive interference by disrupting consolidation in the hippocampus. Participants indicated that they found the mental effort manipulations mentally demanding or difficult in both experiments. Despite this, mental effort in Experiment 1 decreased recollect-reject estimates in some conditions, increased recollect-reject estimates in one TPI condition, or did not affect recollect-reject estimates in another TPI condition. In addition,

mental effort did not affect recollection estimates in either experiment. There are several possibilities for these inconsistent mental effort effects.

Mental effort may not have affected memory in the manner predicted because the parameters of the experiment were not optimal for detecting those effects. First, the mental effort tasks used in the current experiments lasted eight minutes (Experiment 1) or 15 minutes (Experiment 2), whereas previous studies used shorter durations of mental effort (e.g., 3 minutes, Skaggs, 1925). Thus, mental effort may have been at its upper limit—though this seems unlikely given that overall memory performance was similar across conditions. Second, the retention interval length in the current experiments lasted 30 minutes, whereas previous studies used shorter retention intervals—six minutes (Dewar et al., 2007; Müller & Pilzecker, 1900; Skaggs, 1925). Memories would consolidate less within six minutes than within 30-minutes (the current study), making them more vulnerable to retroactive interference. Third, and finally, recognition tasks are usually easier than recall or cued-recall tasks, which may have reduced sensitivity of the current measures to retroactive interference effects. Nevertheless, recall and recognition may share a common set of underlying processes, which may be related to the degree to which recollection is diagnostic of task performance (Okada et al., 2012; Yonelinas, 2002). Thus, mental effort should have caused retroactive interference, at least in terms of overall performance or for recollection estimates, but this was not found. Understanding the boundary conditions of mental effort effects is important for constraining theorizing about mental effort as a cause of retroactive interference.

Another possibility is that mental effort may not cause retroactive interference. Although Wixted (2004b) treats mental effort as nonspecific retroactive interference that

disrupts consolidation in the hippocampus, mental effort—the subjective mental demand of a task—does not seem to cause retroactive interference, at least in the current experiments. Moreover, if mental effort effects have an upper limit or if mental effort causes more retroactive interference in shorter than in longer retention intervals, then these boundary conditions suggest that mental effort may be limited in how much everyday forgetting it can explain.

Temporal Point of Interpolation and Consolidation

In contrast to the inconsistent mental effort effects, TPI affected recollection estimates but not familiarity estimates. The TPI effect is consistent with past research demonstrating a temporal gradient of RI (Wixted, 2004b). This past research showed more forgetting when new material was learned immediately after study compared to a delay. Thus, this TPI effect is consistent with the hypothesis that nonspecific tasks can disrupt consolidation in the hippocampus (Wixted, 2004b). These findings extend past research showing weak TPI effects in controls (Dewar et al., 2010, 2009), which used a 10-minute retention interval. The current experiments found no differences between 0- and 10-minute retention intervals: a 20-minute retention interval increased recollection estimates. In addition, this TPI effect is consistent with research demonstrating that slow-wave sleep rather than REM sleep selectively benefited recollection estimates (Daurat et al., 2007). Thus, the TPI effect specific to recollection is consistent with the hypothesis that nonspecific tasks can disrupt consolidation in the hippocampus.

The TPI effect is also consistent with past research on forgetting curves. Ebbinghaus found that the proportion of forgotten information continually decreases over time (Ebbinghaus, 1885/1913). Moreover, the same mathematical function describes

forgetting curves using different retention intervals and materials (Wixted, 2004a; Wixted & Ebbesen, 1991). This suggests that memories consolidate over time, becoming increasingly less susceptible to retroactive interference (Ebbinghaus, 1885; Jenkins & Dallenbach, 1924; Wixted, 2004a, 2004b, 2005, 2010). Thus, the ever-decreasing forgetting rate suggests that consolidation increasingly protects memories from retroactive interference over time. If, as Experiment 1 suggests, consolidation is more related to recollection than familiarity, then forgetting curves for recollection and familiarity should differ. One study showed that recollection estimates derived from introspective reports decreased quickly from 0 to 7 days but did not change from 7 and 14 days (Tunney, 2010). By contrast, familiarity estimates were stable across these same time periods. These data suggest that the forgetting curve for recollection estimates might differ from the forgetting curve for familiarity estimates, at least for these three retention intervals. However, other research suggests that this pattern might not be typical (Gardiner & Java, 1991; Hockley & Consoli, 1999; Yonelinas, 2002). Limitations of introspective judgments notwithstanding (McCabe, Geraci, Boman, Sensenig, & Rhodes, 2011), forgetting curves for recollection and familiarity estimates may also be confounded with differences in memory strength, with recollection-driven memory generally being stronger than familiarity-driven memory (Yonelinas, 2002). Future research should establish the temporal gradients (e.g., forgetting curves) of recollection and familiarity estimates (e.g., derived from dual-process signal detection model) equated for overall memory strength (e.g., high-confidence recollection and familiarity) to determine the relative importance of consolidation (e.g., Wixted, 2004b), or RI, to recollection and familiarity.

One main limitation of the current experiments is that the TPI effect could have occurred because participants rehearsed new material more during the 20-minute condition than during other TPI conditions. While this cannot be completely ruled out, there are several reasons why this explanation seems unlikely. First, participants studied word pairs under incidental learning instructions. Second, the experimenter was with the participant throughout the procedure, and there was no evidence of overt rehearsal (though this does not rule out covert rehearsal). Third, rehearsal ratings were similar in all conditions. If these ratings indexed rehearsal, then there were little to no differences in amount of rehearsal across conditions. Fourth, recollection estimates were worse in the control condition than in the 20-minute condition. If participants were expected to rehearse more during a longer than a shorter break, then this condition should have been associated with increased recollection estimates (though this condition should have been associated with increased estimates for other reasons, too). Fifth, and finally, some participants spontaneously indicated that they were surprised that there was a test after the experiment, though this was not measured explicitly. Future studies might directly ask participants whether they knew a test was coming and if they prepared in any way for it. Therefore, the current results are most consistent with a consolidation hypothesis, such that a nonspecific task meant to induce retroactive interference disrupted consolidation necessary for recollection.

Similarity

In Experiment 2, similarity did not affect recollection or familiarity estimates. Similarity was manipulated by having participants study similar and dissimilar word pairs (A-B' vs. A-D) 10-minutes after studying a list of A-B word pairs in an associative

recognition task. Past research has shown that increasing the number of competing responses decreases recollection hits and increases familiarity hits and false alarms in an associative recognition task (Verde, 2004). Given this, it was expected that response competition effects would have been greater under similar than dissimilar conditions (A-B' vs. A-D). However, no evidence of retroactive interference was found. There are several possibilities for why no similarity effect was found.

One reason for the null similarity effect may be that the A-D and A-B' lists were not studied close enough to retrieval. Past research has shown that studying the second list closer to retrieval increases response competition effects (J. R. Anderson, 1974; McGeoch, 1942; Newton & Wickens, 1956; Postman & Alper, 1946; Wixted, 2004b). In Experiment 2, participants studied the interfering word pair list 10 minutes after study. The reason for this choice was to increase the ability to detect a potential mental effort by similarity interaction, at a point where mental effort or response competition did not dominate the other. Using a similar design, reducing the retention interval length from 30 minutes to 20 minutes could increase similarity effects because the second list would be learned closer to retrieval. In addition, the interaction might be more likely to be detected because the mental effort and similarity effects would likely be stronger. Reducing retention interval length could also reduce factors contributing to random error (e.g. participant thoughts).

Another possible reason for the null similarity effects is that similarity may have been at its upper limit (see Figure 2; Harden, 1929; Robinson, 1927). Indeed, increasing similarity by increasing synonymy of second-list targets reduced RI effects in a cued recall task, consistent with the far left x-axis of Skaggs similarity curve (see Figure 1;

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Gladis & Braun, 1958). However, Experiment 2 did not detect retroactive facilitation or retroactive interference, so it is unclear how similarity affected performance in the current experiments.

Another possible reason for the null similarity effects is that similarity could facilitate or interfere with memory in the cue-overload paradigm, depending on the detection of change, which could result in null overall effects (e.g., Wahlheim & Jacoby, 2011, 2013). In a proactive interference version of the cue-overload paradigm, participants who detected change (A-B vs A-D) and successfully recollected that change at test showed proactive facilitation. However, when change recollection was unsuccessful, participants showed typical proactive interference effects (Jacoby et al., 2013). The authors surmised that once change was detected, the memory of the A-B word pair became integrated with the memory for the A-D word pair, preserving temporal order, such that, when cued at test, participants later recollected that change at test. This was called a recursive reminding because the test pair reminded the participant of the previous reminding during the interference phase. Thus, although there were no overall effects after learning interfering a list of A-D and C-D word pairs, this null effect could have reflected a mix of both proactive facilitation and interference effects that depended on successful change recollection.

If the proactive effects that depend on change recollection generalize to similar retroactive interference paradigms, then performance in the current experiments also could reflect a mix of retroactive facilitation and interference. When presented A-D or A-B' word pairs in the current experiments, participants could have detected and later recollected the change between some items but not others. If change recollection

occurred in Experiment 2, increasing the similarity between first- and second-list targets should have decreased change detection, which would have decreased the probability of change recollection. This would have increased retroactive interference —but retroactive interference was not found in Experiment 2. It should be noted, however, that change recollection was demonstrated with orthographically similar materials (e.g., knee-bone vs. knee-bend). However, the effect of similarity on change recollection has not been systematically investigated.

Future research should help clarify the role of remindings in retroactive interference and proactive interference, especially because remindings may represent a large proportion of everyday memory (Hintzman, 2011). A remindings hypothesis dates back to Müller and Pilzecker's (1900) theory of retroactive interference. This theory claimed that spontaneous (and uncued) remindings retroactively facilitated memory by promoting consolidation (Müller & Pilzecker, 1900, as cited in Lechner et al., 1999). By contrast, the recursive remindings hypothesis claims that recursive remindings (cued) proactively facilitate memory by preserving the temporal order of old and new memories, as explained above. The role of spontaneous remindings in facilitating consolidation has not been tested, probably because they could be viewed as rehearsal (for example, Wixted, 2004b called spontaneous remindings "spontaneous rehearsal"). Nonetheless, remindings—whether cued or uncued—could facilitate memory in a way different than rehearsal. As the proactive interference research suggests, remindings could facilitate memory through memory integration processes. As the original theory of retroactive interference suggests, remindings could also facilitate consolidation of old material. These possibilities should be explored further in both retroactive interference and

proactive interference tasks and in recall and recognition tasks. Differences between recall and recognition tasks could help answer the question of whether learning new material affects the underlying memory or access to that memory.

Choice of Model

The current studies derived recollection and familiarity estimates using the dual-process signal-detection model (Yonelinas, 1994, 2002). It is possible that the assumptions of the dual-process signal-detection model constrained the sensitivity of recollection and familiarity estimates to retroactive interference. However, retroactive interference did not occur in d_a, a measure of overall discrimination that accounts for differences in the variances of the old and new memory strength distributions (Macmillan & Creelman, 2005). This suggests that the choice of model was not at issue because retroactive interference was not detected in overall performance.

A related point is that if the assumptions underlying the model were not valid, recollection and familiarity estimates might be inaccurate. As an illustration, if recollection was continuous (like familiarity)—that is recollected information varies in memory strength—then there would be variations in confidence (Ratcliff, Sheu, & Gronlund, 1992). If the dual-process signal detection model estimated recollection as a probability, the resulting sample estimate would underestimate true recollection because the estimate would only capture high-confidence responses (rather than also including the lower-confidence responses). However, this may not have been an issue because familiarity was associated with all confidence responses, and there was no effect of mental effort on familiarity. Thus, it seems unlikely that the specific assumptions of the model were related to the mental effort effects observed in the current experiments.

A final point to consider is model fit, which was good for most subjects in both experiments (see Results). If the model fit were poor, the parameter estimates might be less sensitive to retroactive interference. However, visual inspection of responses and the descriptive analyses reported earlier suggest that the dual-process signal-detection model fit the data well. This suggests that recollection and familiarity were estimated appropriately. Therefore, the current data do not suggest that the choice of model compromised power.

Concluding Remarks

In the beginning of his review article, Wixted (2004b) noted that the standard story of forgetting "has changed over the years from a theoretically coherent (and ultimately incorrect) interference-based account of forgetting to an atheoretical laundry list of factors that may or may not play a role" (p. 236). In the endeavor to better understand everyday forgetting, the current experiments examined mental effort and similarity effects in recognition memory: There was a temporal effect of learning new material on recollection estimates. This is consistent with the notion that nonspecific retroactive interference disrupts consolidation specifically for hippocampally-driven memory processes (e.g. Daurat et al., 2007). The current experiments also showed that subjective mental effort, in and of itself, probably does not cause retroactive interference, though further experimentation on the boundary conditions of mental effort is necessary to firmly support this conclusion.

As Wixted (2004b) supposed, it may be that nonspecific tasks cause RI only to the extent that they increase new long-term potentiation in the hippocampus (cf. Dewar et al., 2007). If true, it may be difficult to find manipulations that reliably increase new

long-term potentiation in the hippocampus, though, rat research suggests that exposure to novel environments increase new long-term potentiation in the hippocampus (Ballarini, Moncada, Martinez, Alen, & Viola, 2009; Martínez, Alen, Ballarini, Moncada, & Viola, 2012; Wixted, 2004b). Future research should establish manipulations that link nonspecific tasks to retroactive interference via hippocampal new long-term potentiation (e.g., exposure to virtual novel environments).

If remindings are important in proactive interference tasks, they may also be important in retroactive interference tasks. Thus, future research should investigate the roles of nonspecific retroactive interference, similarity, and remindings in recall and recognition to better understand when learning new material increases retroactive facilitation—via memory integration and consolidation—and increases retroactive interference—via disrupting memory access or the underlying memory representations. Over 100 years have passed since Müller and Pilzecker (1900) discovered retroactive interference, yet the basic processes underlying forgetting remain to be fully uncovered.

Table

Table 1

Inferential and Descriptive Statistics for Post-Experimental Questionnaire Ratings and Math Accuracy as a Function of Mental Effort and Temporal Point of Interpolation (Experiment 1)

	Omnibus $\chi^2(2)$			Unfilled	Easy Math Problems			Difficult Math Problems		
DV	ME	TPI	**ME x TPI	Control	0 Min	10 Min	20 Min	0 Min	10 Min	20 Min
Mental effort	34.13(.35)[b]	1.90(.09)	1.14(.02)	2.85(1.87)	2.30(1.45)	1.70(1.17)[a]	1.60(.94)[†]	3.70(1.84)	3.30(1.53)	3.75(1.68)
Rehearsal	0.46(.04)	1.35(.08)	0.48(.01)	2.15(1.53)	2.05(1.76)	1.95(1.28)	2.10(1.86)	2.10(1.86)	1.85(1.18)	2.70(1.98)
Sleep	2.13(.09)	0.17(.03)	0.42(.01)	2.40(1.93)	2.90(1.97)	3.05(2.14)	3.05(1.82)	2.95(1.86)	2.70(1.72)	2.40(1.96)
AMAS	*0.02(.00)	**0.04(.00)	0.08(.00)	2.80(.70)	2.82(.68)	2.81(1.01)	2.90(.67)	2.87(.58)	2.82(.72)	2.82(.73)
Math %	*157.62(.58)[b]	**0.43(.01)	0.60(.01)	--	0.99(.01)	0.99(.01)	0.99(.01)	0.73(.20)	0.68(.16)	0.67(.20)

Note. All inferential statistics were non-parametric, unless otherwise noted. The Omnibus section shows omnibus χ^2 tests (or *F*-tests). Parenthetical information shows measures of effect size (Cramer's *V* for χ^2 and η_p^2 for *F*-tests). The symbols for significance values in the Unfilled and math problems sections are based on planned comparisons to the unfilled condition. Means (standard deviations) are reported for ease of interpretation.

DV = Dependent variable; ME = Mental effort (main effect); TPI = Temporal point of interpolation (main effect); ME x TPI = Mental effort x temporal point of interpolation (interaction); Mental effort = "*How mentally demanding was the break?*"; Rehearsal = "*How frequently did the word pairs your studied come to mind during the break?*"; Sleep = "*Please rate your sleep intensity during the break.*"; AMAS = Abbreviate Math Anxiety Scale scores; Math % = Math accuracy.

*$F(2, 137)$ reported for parametric tests
**$F(2, 114)$ reported for parametric tests
[†]: $.05 < p < .10$
[a]: $p < .05$
[b]: $p < .0001$

Table 2

Inferential and Descriptive Statistics for Signal-Detection Measures and Model Estimates as a Function of Mental Effort and Temporal Point of Interpolation (Experiment 1)

DV	Omnibus $\chi^2(2)$			Unfilled	Easy Math Problems			Difficult Math Problems		
	ME	TPI	**ME x TPI	Control	0 Min	10 Min	20 Min	0 Min	10 Min	20 Min
d_a	*0.26(.00)	**0.97(.02)	0.46(.01)	1.35(.76)	1.51(.61)	1.34(.74)	1.57(.48)	1.35(.62)	1.44(.57)	1.59(.74)
R	0.08(.02)	7.94(.18)[a]	0.05(.00)	0.42(.26)	0.39(.22)	0.38(.24)	0.52(.21)	0.38(.25)	0.39(.28)	0.52(.24)
R_{Reject}	5.54(.14)[†]	3.62(.12)	5.60(.09)[b]	0.13(.14)	0.09(.12)	0.03(.12)[a]	0.07(.13)[†]	0.05(.11)[a]	0.14(.16)	0.03(.06)[b]
Fam (d')	1.79(.08)	0.28(.03)	0.15(.00)	0.64(.57)	0.91(.64)	0.80(.69)	0.75(.52)	0.75(.63)	0.70(.48)	0.78(.77)

Note. All inferential statistics were non-parametric, unless otherwise noted. The Omnibus section shows omnibus χ^2 tests (or *F*-tests). Parenthetical information shows measures of effect size (Cramer's *V* for χ^2 and η_p^2 for *F*-tests). The symbols for significance values in the unfilled and math problems sections are based on uncorrected multiple comparisons to the unfilled condition. Means (standard deviations) reported for ease of interpretation.

DV = Dependent variable; ME = mental effort; TPI = temporal point of interpolation; *R* = Recollection; R_{Reject} = Recollect-reject; Fam (d') = Familiarity estimate; Accuracy = Overall accuracy; d_a = Discrimination; HR = Overall hit rate; FAR = Overall false alarm rate.

*$F(2, 137)$

**$F(2, 114)$

[†]: $.05 < p < .10$

[a]: $p < .05$

[b]: $p < .01$

Table 3

Inferential and Descriptive Statistics for Post-Experimental Questionnaire Ratings as a Function of Mental Effort (Experiment 2)

DV	$U(64)$	Easy Anagrams	Difficult Anagrams
Sleep	413.5	5.81(3.14)	4.72(3.22)
Difficulty	851.0[a]	3.97(2.25)	6.88(1.93)
Trying	634.0[†]	7.06(3.23)	8.94(1.08)
AnagReh	503.5	5.03(2.65)	5.00(3.12)
StudyReh	452.0	6.44(2.59)	5.88(2.41)

Note. All inferential statistics were Mann-Whitney *U*-tests. Means (standard deviations) reported for ease of interpretation.

DV = Dependent variable; Sleep = "*During the break periods, how often did you rest or sleep?*"; Difficulty = "*How difficult did you find the anagram task?*"; Trying = "*How hard did you try to solve the anagrams?*"; AnagReh = "*How often did you think about the word pairs from the anagram task?*"; and StudyReh = "*How often did you think about the word pairs from the pleasantness judgments task?*".

[†] = $.05 < p < .10$
[a]: $p < .0001$

Table 4

Inferential and Descriptive Statistics for Anagram Hit Rates, Signal-Detection Measures, and Model Estimates as a Function of Anagram Difficulty and Synonymy (Experiment 2)

| DV | Omnibus U(128), W(64), or F(1,62) | | | Easy Anagrams | | Difficult Anagrams | |
	AD	SYN	*AD x SYN	Unrelated	Related	Unrelated	Related
*AnagHR	41.61(.40)[b]	10.41(.14)[a]	16.84(.21)[b]	0.97(.04)	0.97(.04)	0.69(.24)	0.74(.20)
Accuracy	2161.50	37.00	1.49(.02)	0.65(.10)	0.65(.10)	0.65(.10)	0.68(.10)
HR	2318.50	35.00	0.71(.01)	0.67(.17)	0.67(.17)	0.70(.12)	0.73(.11)
FAR	2125.00	32.00	0.91(.02)	0.37(.18)	0.37(.17)	0.40(.16)	0.37(.16)
d_a	2199.50	1237.50	1.29(.02)	0.87(.53)	0.86(.52)	0.90(.49)	1.04(.55)
R	2405.50[†]	904.00	0.50(.01)	0.26(.20)	0.25(.22)	0.31(.20)	0.32(.21)
R_{Reject}	2089.00	665.00	0.26(.00)	0.11(.15)	0.09(.13)	0.08(.11)	0.11(.14)
Fam (d')	1969.00	733.00	0.11(.00)	0.39(.43)	0.42(.45)	0.35(.45)	0.46(.55)

Note. All inferential statistics were non-parametric. Omnibus section shows values of omnibus Mann-Whitney U-tests (anagram difficulty), Wilcoxon Sign-Ranked Test (synonymy), or non-parametric interaction (F-tests). Parenthetical information shows effect sizes only for F-tests using η_p^2. anagrams sections shows means (standard deviations) for ease of interpretation.

AD = anagram difficulty; SYN = synonymy; DV = Dependent variable; AnagHR = Anagram hit rate; Accuracy = Overall accuracy; HR = Overall hit rate; FAR = Overall false alarm rate; d_a = discrimination; R = Recollection; R_{Reject} = Recollect-reject; Fam (d') = Familiarity estimate.

*F(1, 62)
[†]: $.05 < p < .10$
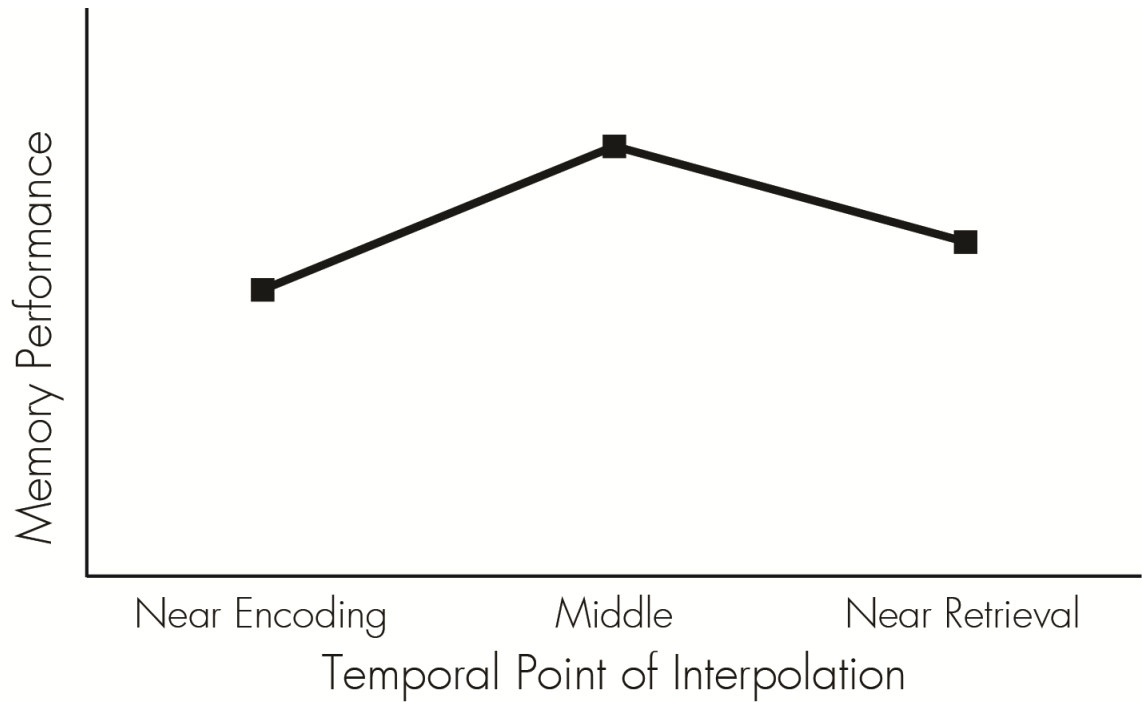[a]: $p < .01$
[b]: $p < .0001$

Figure 1. Theoretical inverted-U relationship between memory performance and the temporal point of interpolation (Wixted, 2004b). Memory performance is expected to be lower for when nonspecific retroactive interference occurs in the middle of a retention interval compared to when it occurs near encoding or near retrieval.
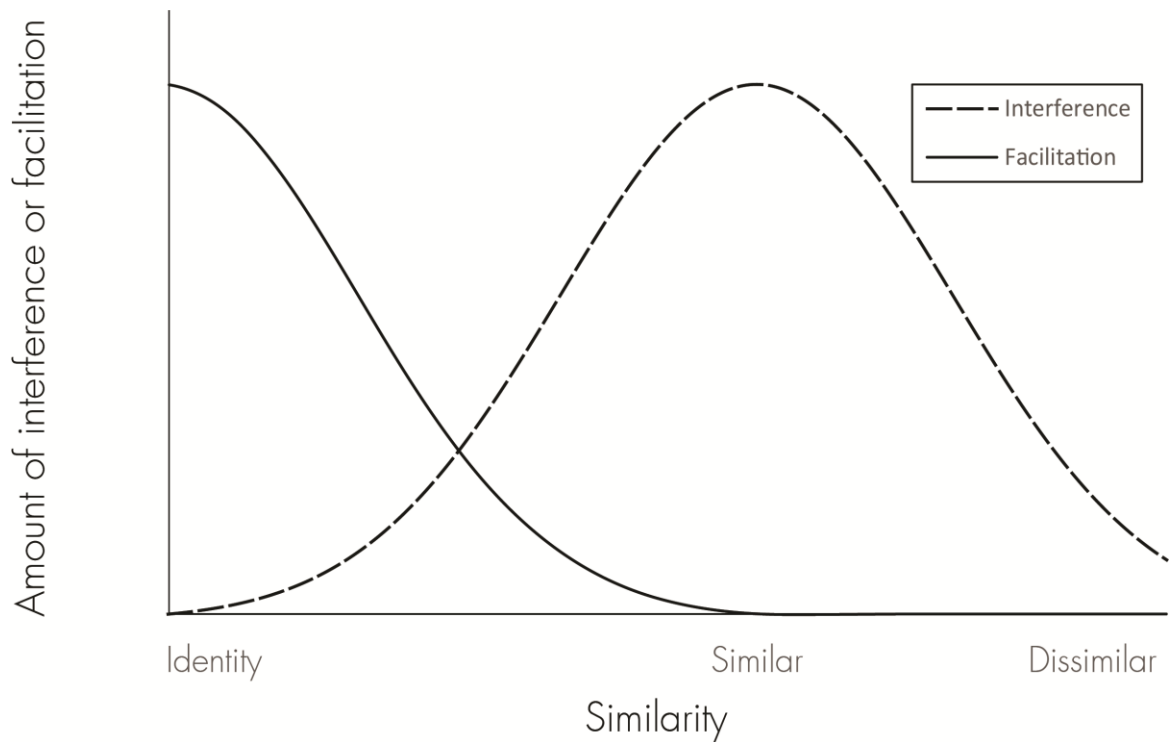
Figure 2. The Skaggs Hypothesis. This figure shows the relationships between retroactive facilitation and interference as a function of similarity between old and new materials (Skaggs, 1925; and later, Robinson, 1927). On the far-left x-axis, increasing similarity increases retroactive facilitation. In the middle of the x-axis, increasing similarity increases retroactive interference. Finally, on the far-right x-axis, even dissimilar stimuli can cause retroactive interference because of mental effort (Müller & Pilzecker, 1900; Skaggs, 1925; 1933; Wixted, 2004a).
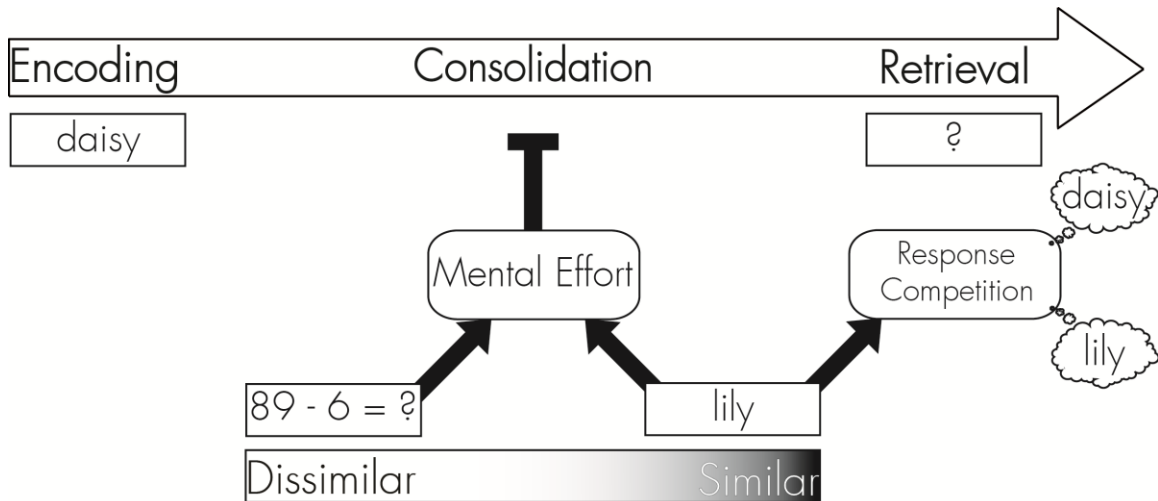
Figure 3. Retroactive Interference Model Adapted from Dewar, Cowan, and Della Sala (2007, Figure 15, p. 632). After encoding a stimulus (*daisy*), the memory consolidates and becomes increasingly less susceptible to retroactive interference. New material can vary in similarity, from highly dissimilar (*89 – 6 = ?*) to highly similar (*lily*). If similarity is high, the responses may compete when probed during retrieval, which is called response competition, In addition, learning similar or dissimilar material may be mentally effortful and may disrupt hippocampal consolidation.
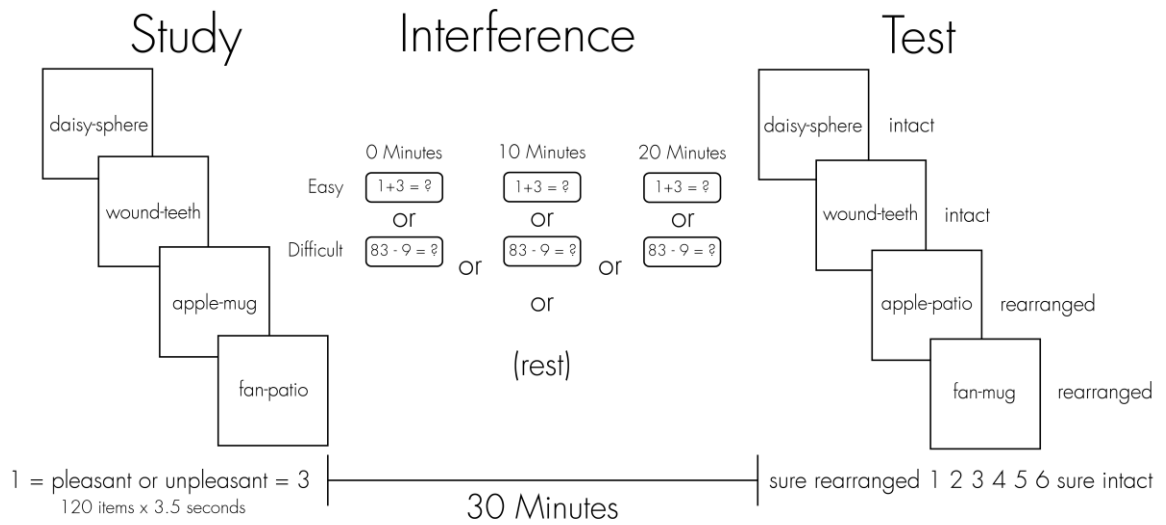
Figure 4. Experiment 1 Procedure. Participants judged the pleasantness of 120 word pairs. Mental effort was manipulated between-subjects by having participants solve either easy problems (e.g., *1 + 3 = ?*) or difficult problems (e.g., *83 – 9 = ?*). Additionally, the temporal point of interpolation was manipulated between-subjects by having participants solve math problems either 0, 10, or 20 minutes after study. Participants in the control (unfilled) condition rested for 30 minutes. Participants then rated their confidence in decisions about whether word pairs were intact or rearranged, they completed a post-experimental questionnaire (see Methods), and then they were debriefed.
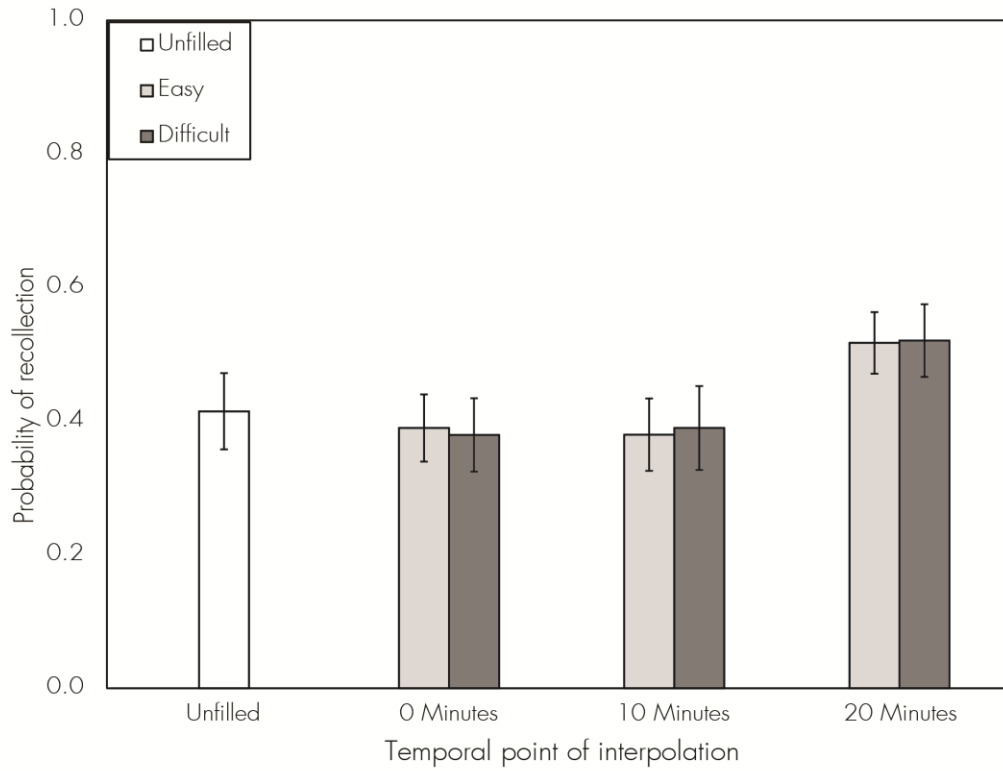
Figure 5. Recollection Estimates in Experiment 1. Estimates are plotted as a function of math problem difficulty, rest (unfilled), and the temporal point of interpolation. Bars are ± 1 standard error of the mean.
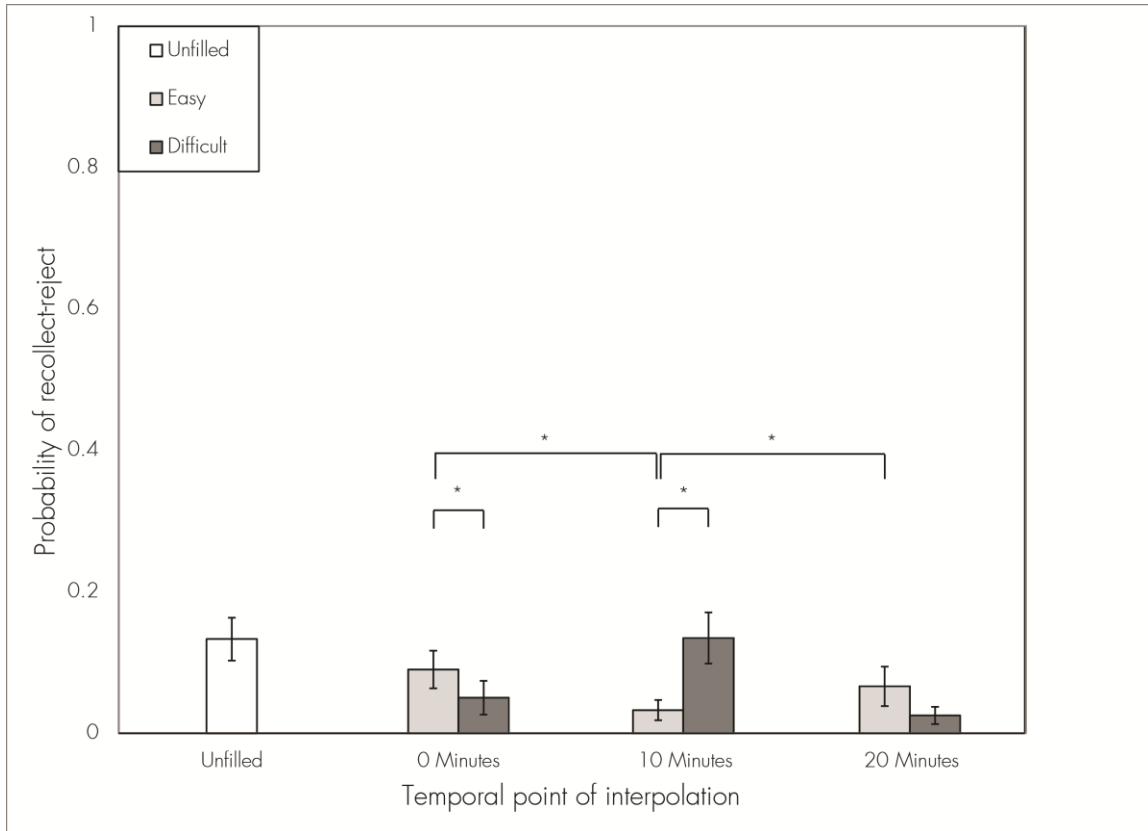
Figure 6. Recollect-Reject Estimates in Experiment 1. Estimates are plotted as a function of math problem difficulty, unfilled (control), and the temporal point of interpolation. Bars are $\pm$ 1 standard error of the mean.

*p < .05 for follow-up, Mann-Whitney *U*-tests (uncorrected)

Figure 7. Familiarity (*d'*) Estimates in Experiment 1. Estimates are plotted as a function of math problem difficulty, rest (unfilled), and the temporal point of interpolation. Bars are ± 1 standard error of the mean.
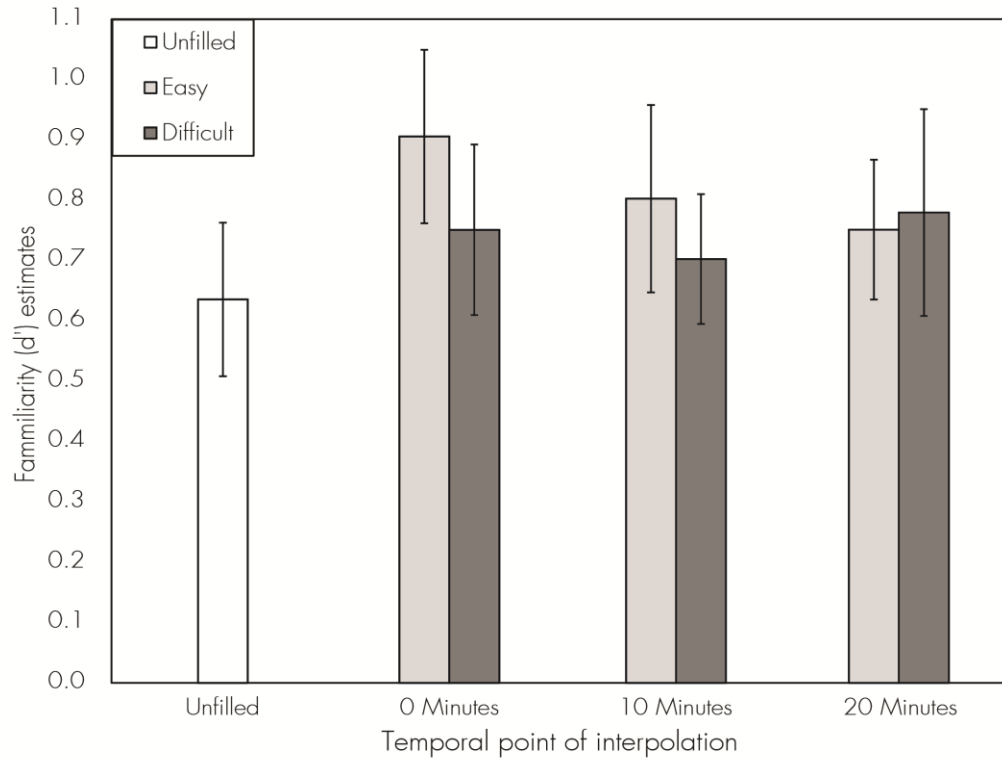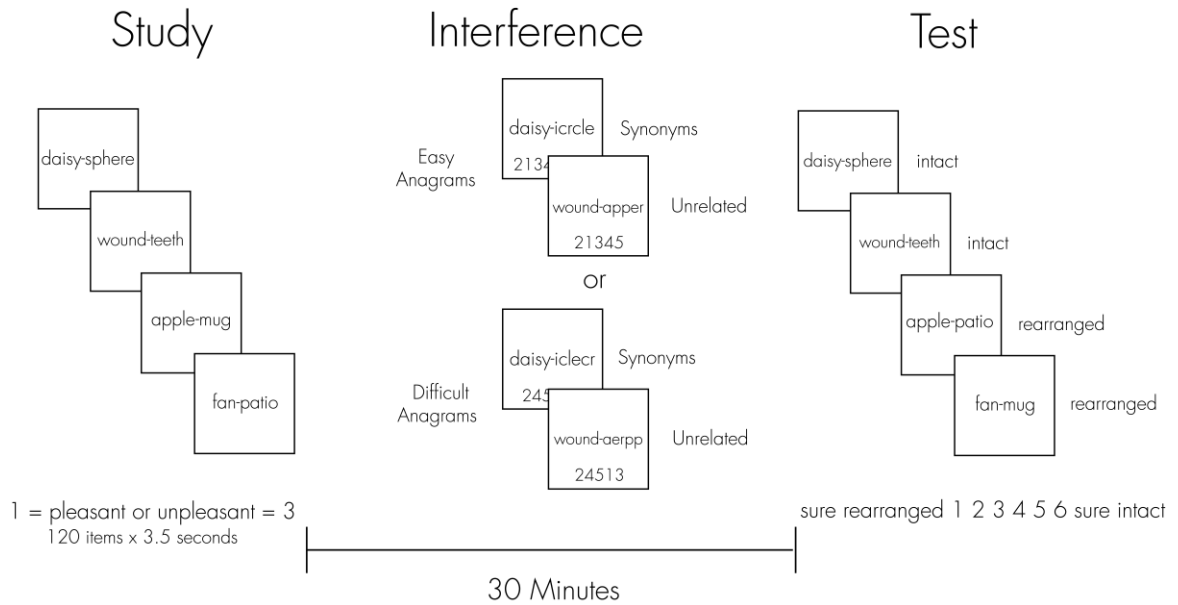
Study

daisy-sphere

wound-teeth

apple-mug

fan-patio

1 = pleasant or unpleasant = 3
120 items × 3.5 seconds

Interference

Easy
Anagrams

daisy-icrcle | Synonyms

2134

wound-apper | Unrelated

21345

or

Difficult
Anagrams

daisy-iclecr | Synonyms

245

wound-aerpp | Unrelated

24513

Test

daisy-sphere | intact

wound-teeth | intact

apple-patio | rearranged

fan-mug | rearranged

sure rearranged 1 2 3 4 5 6 sure intact

30 Minutes

Figure 8. Experiment 2 Procedure. Participants read aloud and judged the pleasantness of 120 word pairs (*daisy-sphere*, *wound-teeth*). Mental effort was manipulated between-subjects and required participants to solve either easy (*daisy-icrcle*) or difficult (*daisy-iclecr*) anagrams. The anagram targets were synonyms (*daisy-circle*) or unrelated words (*wound-paper*) to the studied targets. Participants then rated their confidence in decisions about whether word pairs were intact or rearranged, they completed a post-experimental questionnaire (see Methods), and then they were debriefed
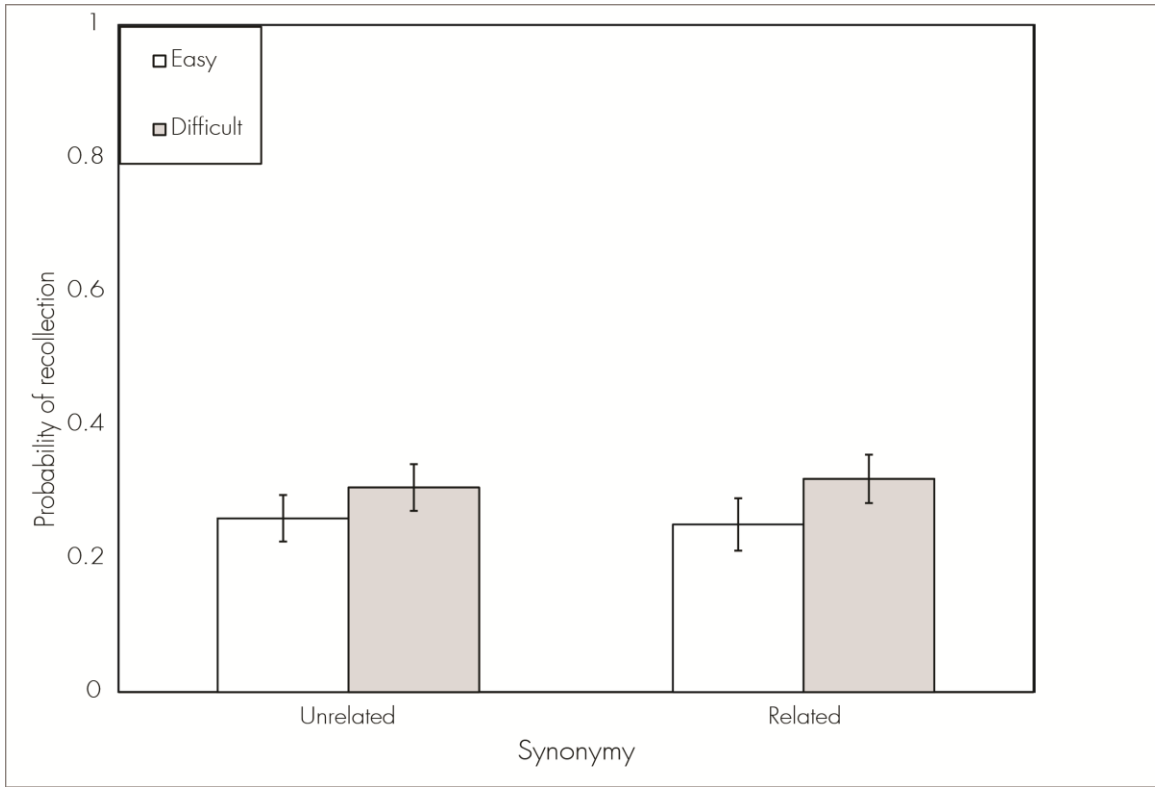
Figure 9. Recollection Estimates in Experiment 2. Estimates are plotted as a function of anagram difficulty and synonymy. Bars are $\pm$ 1 standard error of the mean.

Figure 10. Recollect-Reject Estimates in Experiment 2. Estimates are plotted as a function of anagram difficulty and synonymy. Bars are $\pm$ 1 standard error of the mean.

Figure 11. Familiarity (*d'*) Estimates in Experiment 2. Estimates are plotted as a function of anagram difficulty and synonymy. Bars are ± 1 standard error of the mean.

APPENDIX A
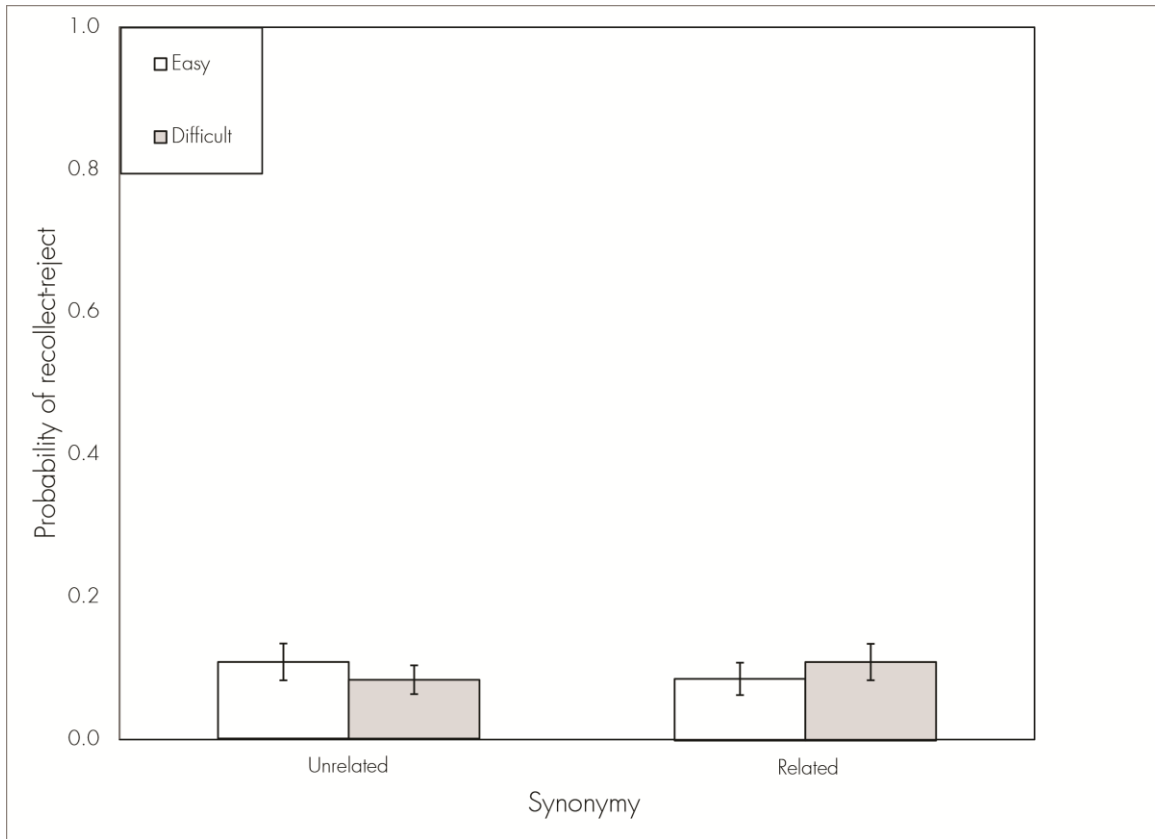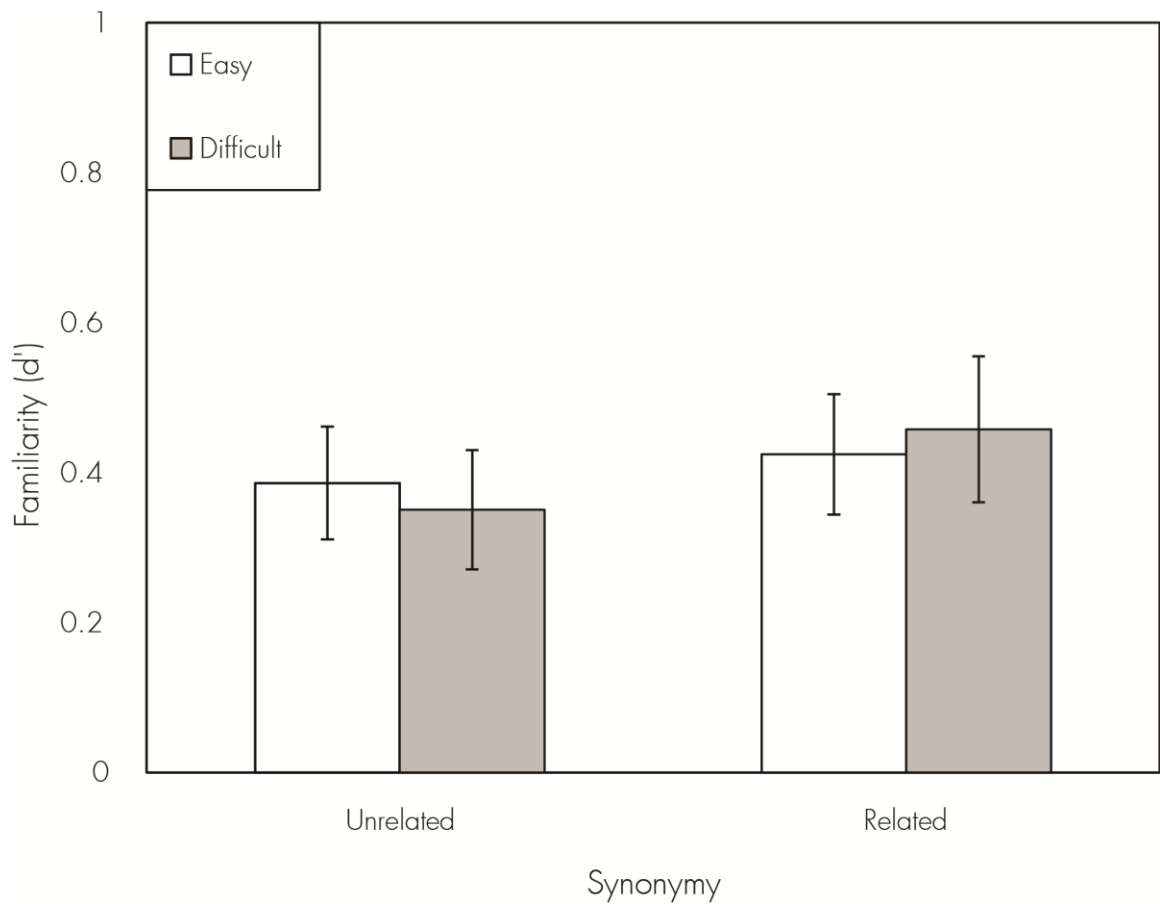
The following sections discuss, in brief a) the dependent variables derived from signal detection theory (Macmillan & Creelman, 2005) and b) how recollection and familiarity process estimates were derived from the dual-process signal-detection model in an associative recognition test.

Signal detection theory

Signal detection theory applied to recognition memory allows researchers to separate measures of discrimination from response criteria, which are derived from two classes of responses to two classes of items. Discrimination is the ability to tell apart two classes of items; response criterion is a subjective threshold that represents the willingness to respond a certain way. In associative recognition memory tasks, participants respond "intact" or "rearranged" to intact and rearranged items (see Method). Hit rates measure the probability of correct responses to intact word pairs, and false alarm rates measure the probability of incorrect responses to rearranged word pairs.

Hit rates and false alarm rates are related to discrimination (e.g., d_a) and response criterion (c) in the following manner. Discrimination is a standardized measure of the difference between hit rates and false alarms rates; response criterion is a standardized measure of the negative average of the hit rates and false alarm rates. Large positive standardized differences between hit rate and false alarm rate represent good discrimination; parallel changes in hit rates and false alarm rates reflect changes in response criteria (i.e., more or less willing to respond "intact"). Separating discrimination from response bias is important because standard performance measures, such as accuracy, confound them.

Dual-Process Signal-Detection Model Process Estimates

The dual-process signal-detection model assumes that recollection occurs probabilistically across items, and when successful, the subject retrieves associative information about the item with high confidence. For intact items, this increases hit rates; for rearranged items, this increases correct rejections (i.e., 1 – False Alarm Rate), resulting in two measures of recollection: recollect-accept (henceforth, called *recollection*) and recollect-reject. Familiarity, by contrast, is a continuous signal-detection process, and so familiarity is estimated as a discrimination measure, or *d'*. For intact items, familiarity increases hits; for rearranged items, familiarity increases false alarms. Based on these relationships between hit rates and false alarm rates, the model relies on variations in response confidence that represent variations in response bias to compute estimates of recollection, recollect-reject, and familiarity (*d'*).

To estimate the relative contributions of recollection and familiarity to associative recognition test performance, an iterative maximum likelihood estimation procedure was used. Estimates were derived from an unpublished Microsoft Excel spreadsheet, authored by Colleen M. Parks. This procedure works by calculating the maximum likelihood estimate of recollection and familiarity given observed hit rates and false alarm rates. The likelihood value is calculated using G^2 based on the differences between observed and predicted hit rates and false alarms. These likelihood values are summed and then maximized using the Solver Add-in to yield the most likely recollection (*R*), recollect-reject, and familiarity (*d'*) estimates given the data. To assess model fit, a χ^2 value is calculated (which is an approximation of G^2) and is tested against the critical $\chi^2(2)$ (= 3.84). If the observed χ^2 exceeds the critical $\chi^2(2)$, then the model fit is rejected at $p < .05$,

indicating poor model fit. It is important to note that the dual-process signal-detection model was fit to individual subject data rather than group averages. Past research has suggested that fitting to average rather than individual data inaccurately represents the true outcome (Malmberg & Xu, 2006; Yonelinas & Parks, 2007).

There are also certain constraints imposed on parameter estimates during the model-fitting procedures. Familiarity (d') was constrained to be no less than 0 (chance performance), and R and R_{Reject} were constrained to be between .000001 and .999999. These constraints were in place when estimating all parameters. For familiarity, negative d' is unlikely because it means the participant had below chance performance (i.e., practically, they may have reversed the scale). For recollection, probabilities of less than 0 or greater than 1 are not possible because they are probabilities.

An additional consideration, however, comes from Macmillan and Creelman (2005) who, while discussing whether one should include negative d' values in statistical analyses, suggested that only including d' values greater than 0 may inflate the true, or population, d' (p. 15). That is, negative d' values may arise from sampling error when calculated over a small number of trials. When averaged over subjects, the negative values would be incorporated into a less biased population estimate. Otherwise, the estimate will be inflated. However, this is not the accepted means anymore because it allows for model overfitting that may produce nonsensical estimates. From the former perspective, constraining parameter estimates may result in biased estimates, whereas from the latter perspective, constraints may produce more sensible estimates without overfitting sample data. The latter perspective was taken in the current experiments.

APPENDIX B

IRB APPROVAL

UNLV
UNIVERSITY OF NEVADA LAS VEGAS

Approved
OCT 09 2008
Expires

Social/Behavioral IRB – Exempt Review
Approved as Exempt

DATE: October 16, 2008

TO: **Ms. Colleen Parks,** Psychology

FROM: Office for the Protection of Research Subjects

RE: Notification of IRB Action by Dr. J. Michael Stitt, Chair
          Protocol Title: **Encoding and Retrieval Processes**
          OPRS# 0809-2850

This memorandum is notification that the project referenced above has been reviewed by the UNLV Social/Behavioral Institutional Review Board (IRB) as indicated in Federal regulatory statutes 45CFR46.

PLEASE NOTE:
Attached to this approval notice is the **official Informed Consent/Assent (IC/IA) Form** for this study. The IC/IA contains an official approval stamp. Only copies of this official IC/IA form may be used when obtaining consent. Please keep the original for your records.

The protocol has been reviewed and deemed exempt from IRB review. It is not in need of further review or approval by the IRB.

Any changes to the exempt protocol may cause this project to require a different level of IRB review. Should any changes need to be made, please submit a **Modification Form**.

If you have questions or require any assistance, please contact the Office for the Protection of Research Subjects at OPRSHumanSubjects@unlv.edu or call 895-2794.

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Education

Ph.D. (in progress)   2015 to present

Mentored by **Ned Silver**

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Research in human factors

Masters of Arts in Psychology   2010 to 2015

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Research in human recognition memory

Bachelor of Arts in Psychology   2006 to 2010

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Publications

Barchard, K. A., & **Picker, C. J.** (in preparation). Emotional awareness: Computer and hand scoring of an open-ended test.

Picker, C.J., Hannon, E.E., & Tichko, P. (submitted). Your favorite band says a lot about you: Musical compatibility influences social and physical attraction. *Personality and Social Psychology Bulletin.*

Silver, N. C., Ullman, J., & **Picker, C. J.** (submitted). COMPCOR: A computer program for comparing correlations using confidence intervals. *Psychology and Cognitive Sciences Open Journal.*

Posters and Presentations

Davis, L., **Picker, C.J.**, & Parks, C. M. (2014, May). *The spread and control of attention of action video game experts in a memory flanker task*. Poster presented at 2014 UNLV Psi Chi Poster Conference, Las Vegas, NV.

Lein, E., **Picker, C.J.**, & Parks, C. M. (2014, May). *Mental effort as retroactive interference: Comparing cued recall and pair recognition performance.* <u>Poster</u> presented at 2014 UNLV Psi Chi Poster Conference, Las Vegas, NV.

Picker, C.J. (2013, April). *Retroactive interference in recognition memory: The roles of mental effort, consolidation, and similarity.* <u>Talk</u> presented in partial fulfillment of program requirements for UNLV Psychology department, Las Vegas, NV.

Picker, C.J. (2013, March). *Retroactive interference in recognition memory: The roles of mental effort and consolidation.* <u>Talk</u> presented at the 2013 GPSA Research Forum, Las Vegas, NV.

Picker, C.J. & Parks, C.M. (2011, November). *Testing threshold and continuous accounts of recollection: Effects of response bias.* <u>Poster</u> session presented at the 52nd Annual Meeting of the Psychonomic Society, Seattle, WA.

Teaching

PSY 316 Instructor for two sections of **online** Cognitive Psychology	Fall 2014 to present
PSY 101 Instructor for **hybrid** course	Spring 2014
Helped develop course content, helped design course evaluation, collected data, and analyzed data with Wayne Weiten	
Culminated in co-authorship with Wayne Weiten on internal paper used to compare student performance in hybrid versus traditional courses	
PSY 101 Instructor for two sections per semester	Fall 2012 to Fall 2013

Consulting Work

Part-time statistical consultant for AAC&U grant-funded international project ($14/hr)	March 2014 to September 2014
Project: Transparency in Teaching and Learning in Higher Education	

Grants

$150 from Graduate and Professional Student Association (GPSA)	November 2012
$300 from GPSA	November 2011

Service

Mentor for Outreach for Undergraduate Mentoring Program	2012 to 2014
Vice President of Experimental Student Committee	Fall 2013 to Spring 2014

GPSA **Liberal Arts College Representative**	Summer 2013

Brain Awareness presentations

Cashman Middle School (Las Vegas, NV)	May 02, 2014
Laughlin Middle School (Laughlin, NV)	March 10, 2014
Paradise PDS Elementary School (Las Vegas, NV)	May 29, 2013
Cashman Middle School (Las Vegas, NV)	May 28, 2013
Cashman Middle School (Las Vegas, NV)	May 23, 2013
GPSA Psychology **Representative**	Fall 2012 to Spring 2013

Awards Committee

Professional Memberships

Association for Psychological Science (APS)	2012 to present
American Psychological Association (APA)	2011 to present
Psi Chi	2009 to present