

Research Article

Implicit Memory Influences on Metamemory During Verbal Learning After Traumatic Brain Injury

Pradeep Ramanathan,^a Mary R. T. Kennedy,^b and Chad J. Marsolek^b

Purpose: Prior research has shown that individuals with traumatic brain injury (TBI) may be overconfident in their *judgments of learning* (JOLs; online measures of self-monitoring of learning and memory). JOLs had been presumed to be driven by explicit processes, but recent research has also revealed implicit memory involvement. Given that implicit learning mechanisms are often intact in those with TBI, the purpose of this study was to investigate whether priming and antipriming of immediate and delayed JOLs in individuals with TBI might affect their overconfidence.

Method: A standard 3-field masked priming paradigm was combined with a paired-associate learning task with JOLs and administered to individuals with TBI and matched controls (18 per group). In each trial, a subliminal masked stimulus was immediately followed by supraliminal

presentation of a word pair for study; participants also made immediate and delayed JOLs, with cued-recall testing 10 min after study and judgment.

Results: Antipriming significantly lowered JOLs and overconfidence for both groups, whereas delaying JOLs significantly improved recall for both groups.

Conclusions: The results suggest that JOLs may be influenced by subliminal implicit memory. Clinical implications include the possible use of antipriming to reduce overconfidence after brain injury and delaying JOLs to improve recall.

Key Words: implicit memory, priming, antipriming, metamemory, judgments of learning, recognition memory, traumatic brain injury

Metamemory may be defined as “judgments, assessments, or commentaries that are made about memories or learning” (Metcalf & Dunlosky, 2008, p. 349). The two most intensively studied measures of metamemory are Feeling of Knowing judgments (FOKs) and Judgments of Learning (JOLs). FOKs are generally made after a failed attempt at recalling information; individuals are then asked to rate the likelihood that, if given a field of choices, they will be able to recognize the correct item (Koriat, 1995). In contrast, JOLs are usually made during or shortly after learning and generally address recall rather than recognition; individuals judge the likelihood that they will be able to recall the recently learned information at a later time (T. O. Nelson & Dunlosky, 1991). Skill in making accurate JOLs is particularly relevant in the daily

life of individuals with traumatic brain injury (TBI). Given the well-documented impairment to learning new information for those with TBI (e.g., Wiegner & Donders, 1999), the ability to make prospective judgments of how likely one is to later recall recently acquired information (e.g., a new medication schedule, a doctor appointment, etc.) is critical. Indeed, such prospective memory failures have been identified by individuals with TBI as more problematic than explicit memory failure (e.g., Mateer, Sohlberg, & Crinean, 1987; Roche, Fleming, & Shum, 2002).

During the initial decades of research into metamemory, investigators had presumed that metamemory was reliant on *explicit* memory, a form of memory in which participants deliberately retrieve information from the past. However, over the last 20 years, studies have begun to explore whether *implicit* memory, a form of memory that can be expressed without deliberate retrieval of past information plays a role in metamemory, with very interesting findings (Dunlosky & Metcalfe, 2009; Reder, 1996). One important finding has been that familiarity with nontarget cues (such as the context of the target, features of the question, etc.) plays a significant role in metamemory judgments (Koriat,

^aUniversity of Connecticut, Storrs

^bUniversity of Minnesota, Minneapolis

Correspondence to Pradeep Ramanathan: ramanathan@uconn.edu

Editor: Rhea Paul

Associate Editor: Robert Marshall

Received July 31, 2013

Revision received March 7, 2014

Accepted April 4, 2014

DOI: 10.1044/2014_JSLHR-L-13-0204

Disclosure: The authors have declared that no competing interests existed at the time of publication.

1997; Metcalfe, Schwartz, & Joaquim, 1993; Reder & Schunn, 1996). However, in such explorations investigators have generally used *supraliminal manipulations*. Supraliminal manipulations are those that are available to a participant's conscious awareness, such as presenting some items for study more often than others.

By contrast, masked priming is a type of implicit memory manipulation that minimizes the possibility that covert supraliminal (and therefore possibly explicit) processes may be engaged during metamemory judgment. In masked priming, a briefly presented prime stimulus (e.g., a word) occurs either before or after (or temporally sandwiched between) a row of symbols or nonsense characters (e.g., a row of Xs, ampersands, etc.). The brevity of stimulus presentation (typically <60 ms) combined with the preceding and/or succeeding row of symbols (called the mask and usually presented for some 500 ms), drastically reduce or eliminate individuals' conscious awareness of the prime stimulus. There has been a great deal of research on this phenomenon since the 1980s, fleshing out the conditions and types of manipulations by which individuals would have minimal to no awareness of the stimuli but would still show behavioral effects of the priming (for a comprehensive review, see Kinoshita & Lupker, 2003). However, despite the advantage of minimal to no conscious awareness of the prime, few studies have used masked priming methodology to investigate implicit memory involvement in FOK-type judgments (Jameson, Narens, Goldfarb, & Nelson, 1990; Kinoshita, 1997; Rajaram, 1993), and no prior study has investigated the effects of masked priming on JOLs.

Beyond such methodological tools, clinical populations may also help shed light on implicit memory contributions to metamemory. The TBI population is particularly illuminating in that prior research indicates that TBI often impairs explicit memory but leaves many aspects of implicit memory relatively intact (Vakil & Oded, 2003; Vakil & Sigal, 1997; Ward, Shum, Wallace, & Boon, 2002; or see the literature review in Vakil, 2005). It should be noted that there is some evidence of impairments to implicit memory in individuals with TBI in certain circumstances or conditions (e.g., in divided attention tasks, Schmitter-Edgecombe, 1996; Watt, Shores, & Kinoshita, 1999; or severe TBI in children, Lah, Epps, Levick, & Parry, 2011). However, given their significantly greater impairment of explicit memory than implicit memory, inclusion of the adult clinical population can help to minimize the aforementioned covert effects of explicit memory on the metamemory judgments. Additionally, there is some evidence that TBI may also impair metamemory (e.g., Kennedy & Yorkston, 2004; Krause & Kennedy, 2009); if this is the case, then perhaps relatively intact implicit memory processes can facilitate metamemory in individuals living with TBI. For these reasons, we investigated the effects of subliminal masked priming and antipriming on JOLs in individuals with and without TBI.

This study aims at answering two principal questions. The first question is, are metamemory judgments influenced by implicit memory processes? A difficult issue when measuring implicit memory effects is that involuntary explicit

memory for supraliminally presented items can "contaminate" putative measures of implicit memory for the items (e.g., Richardson-Klavehn, Gardiner, & Java, 1994). To avoid this, subliminal masked priming is used here to test effects of implicit memory on metamemory. In addition, a recent finding is that presentations of stimuli can cause both priming (increased accuracy and/or decreased response time) for those stimuli that are repeated, as well as antipriming (*reduced* accuracy and/or *increased* response time) for other nonrepeated stimuli (Marsolek, 2008; Marsolek et al., 2010; Marsolek, Schnyer, Deason, Ritchey, & Verfaellie, 2006). Thus, this first research question more specifically asks, will subliminal masked priming and antipriming manipulations affect measures related to JOLs in a paired-associate learning task? Here, the null hypothesis would be that such manipulations will not affect JOL ratings or affect either absolute or relative predictive accuracy. However, prior research investigating masked priming in FOKs or similar tasks has revealed some evidence for an effect of priming and/or antipriming (Kinoshita, 1997; Rajaram, 1993); therefore, we hypothesize that as with similar studies involving FOKs, masked priming/antipriming will affect one or more of these measures associated with JOLs.

The second research question investigates differences in implicit memory, explicit memory, or metamemory as a function of neurological status. This question is primarily aimed at the clinical goal of determining whether or not there are conditions under which individuals with TBI may demonstrate memory or metamemory performance that is closer to the normal population. For example, can individuals with TBI make use of relatively intact implicit memory to leverage learning, as suggested by Schmitter-Edgecombe (2006)? Similarly, Spellman and Bjork (1992) have suggested that delaying metamemory judgments provides an opportunity for spaced retrieval practice and ought to increase recall accuracy; if such an explicit memory benefit is observed in the TBI population, this might suggest a clinical strategy of interspersing delayed JOLs throughout learning to improve encoding and subsequent recall. Finally, Kennedy and colleagues (Kennedy, 2001; Kennedy & Yorkston, 2000, 2004) have found that the overconfidence in immediate JOLs among individuals with TBI, as compared to controls, normalized when JOLs were made after a delay. The null hypothesis would be that TBI has no effect on implicit memory, explicit memory, or metamemory (or on any relationship among these). However, given the evidence from the extant literature, alternative hypotheses might include improved explicit memory or metamemory in the TBI group because of manipulations of priming condition or manipulation of when the JOLs are solicited (immediately after study or after some delay).

Method

Participants were recruited from local brain injury support groups, state brain injury associations in both Minnesota and Connecticut, and from the campus communities. In the first of two sessions they provided informed

consent, confirmed their demographic information, and completed the experimental tasks. Mandatory 15-min breaks with filler conversation were provided approximately every 20–30 min during the experimental tasks. During the second session, participants completed all standardized neurocognitive tests with intermittent rest breaks. All procedures were approved by the institutional review boards at the University of Minnesota and the University of Connecticut.

Participants

Thirty-six adults completed the study: 18 with TBI (10 men, eight women) and 18 controls (10 men, eight women), matched by sex, age, education, and estimated verbal IQ. Participants with TBI averaged 45.97 ($SD = 11.90$) years of age, 14.83 ($SD = 2.05$) years of education, and 106.94 ($SD = 7.65$) estimated verbal IQ (National Adult Reading Test [NART]; H. E. Nelson & Willison, 1991). Control participants averaged 46.86 ($SD = 9.55$) years of age, 15.42 ($SD = 2.05$) years of education, and 110.33 ($SD = 7.98$) estimated verbal IQ. Groups did not differ significantly in any of these criteria, $t(34) = 0.81, 0.40,$ and $0.20,$ respectively, $p < .05$. For additional demographic details, see Table 1.

Inclusion criteria for all participants in the study were speaking with native fluency in English, being age 18–65 years, and having at least a 10th grade education. Exclusion criteria were having a history of neurological disease or stroke, learning disabilities, uncorrected auditory or visual impairments, any reading impairment, a history of alcohol or drug abuse, or psychiatric problems. Additionally, adults who received an aphasia quotient of ≤ 93.8 on the Western Aphasia Battery (WAB; Kertesz, 1982) were excluded as this is an indication of aphasia, an acquired language impairment that would confound task performance. For participants with TBI, the injury must have occurred after age 18. Medical records were reviewed to confirm the TBI diagnosis. Finally, individuals with TBI who were fewer than 6 months postinjury were excluded to minimize the impact of spontaneous neurological recovery.

Participants with TBI averaged 12.03 years since injury ($SD = 11.10$) and represented a range of injury severity. Initial Glasgow Coma Scale (GCS) scores, neuroimaging results, or medical reports were used to classify injury severity for 11 participants. For five other participants, neuroimaging information and self-report of posttraumatic amnesia (PTA) and/or loss of consciousness (LOC) was used to estimate injury severity. Self-report of PTA and/or LOC were used to estimate injury severity for the remaining two TBI participants. On the basis of this information and using Stein's (1996) classification of injury severity, it was determined that three participants had a mild TBI, five had at least a moderate TBI, and 10 had a severe TBI. It should also be noted that all participants with TBI were community dwelling who lived independently with minimal supervision from family or friends.

Psychometric Tests

Neurocognitive testing (described here and in Table 2) demonstrated differences in the cognitive profiles of the participant groups. Individuals with TBI showed the general pattern of impaired explicit memory and executive function typical of this population. In particular, our cohort demonstrated significant impairments to recent long-term explicit memory, verbal fluency, and sequencing. There are no standardized test batteries available to characterize implicit memory or metamemory; rather, tasks similar to those in our study are typically used to measure these.

Several standardized neuropsychological tests were administered to describe the samples of the populations. The WAB was used to exclude individuals with aphasia, and the NART (Bright, Jaldow, & Kopelman, 2002) was used to estimate premorbid verbal intelligence for both groups. The Digit Span subtest of the Wechsler Memory Scale—Third Edition (WMS-III; Wechsler, 1987) was used to characterize short term and working memory. The California Verbal Learning Test—Second Edition (CVLT-II; Delis, Kramer, Kaplan, & Ober, 2000) characterized verbal learning and memory. The Verbal Fluency, Trails, and Tower subtests of the Delis–Kaplan Executive Function System (D-KEFS; Delis, Kaplan, & Kramer, 2001) were used to assess aspects of executive functioning. See Table 2 for group means and standard deviations for these measures.

Two-tailed, unpaired-samples t tests revealed significant group differences in several measures of memory and executive function. As expected, individuals with TBI demonstrated lower scores in free recall after long delay in the CVLT-II ($p < .01$); short-delay free recall and short- and long-delay cued recall tended to be lower than controls ($p < .08, p < .08,$ and $p < .09,$ respectively). All measures of verbal fluency were significantly lower for those with TBI ($p < .01$). Individuals with TBI also demonstrated slower speed on the Trails subtest of the D-KEFs ($p < .01$) and greater errors in letter and number sequencing ($p < .05$). Thus, the TBI group had impairments that were anticipated— aspects of memory, learning, and executive functions consistent with the part of this population that has lasting chronic impairments.

Experimental Design

Implicit metamemory task details. The implicit metamemory task was based on Kennedy and Yorkston (2000) but modified to eliminate the prestudy phase and to include a standard three-field subliminal masked priming paradigm to add implicit processes (Forster & Davis, 1984). In summary, a preliminary instructional phase provided step-by-step task instructions to participants, including five sample word pairs, to demonstrate how the task was to be completed. This was followed by a training block identical in all respects to the ensuing two actual trial blocks; its purpose was to accustom participants to the nature and difficulty of the task and to provide an opportunity for participants to ask questions at the end of training. The training block was

Table 1. Detailed demographic information.

Participants	Group	Sex	Age at time of experiment (in years)	Time postinjury (years)	Education (in years)	Estimated (premorbid) verbal IQ ^a	Current or former occupation
1	TBI	M	37.40	5.53	16.0	99	Electronics technician
2	TBI	M	44.92	18.99	11.0	118	Factory worker
3	TBI	M	54.56	5.69	18.0	114	Engineer–business consultant
4	TBI	F	43.67	6.55	12.5	106	Medical secretary
5	TBI	F	55.26	35.09	16.0	117	Freelance writer
6	TBI	F	20.80	1.53	14.0	106	College junior (student)
7	TBI	F	56.38	37.96	13.5	98	Clerical (data entry)
8	TBI	F	56.78	15.46	15.0	110	Part-time interior decorator
9	TBI	F	56.64	13.21	16.0	113	Hardware store office mgr.
10	TBI	F	45.65	6.02	12.0	107	Unemployed (nanny–waitress)
11	TBI	M	54.96	0.51	18.0	108	Small business owner; musician (violinist)
12	TBI	M	36.26	14.11	16.0	101	Unemployed; volunteer
13	TBI	M	29.55	0.63	16.0	94	Unemployed
14	TBI	F	21.61	3.16	13.5	112	College student
15	TBI	M	49.68	2.87	12.5	92	Retired; was a mechanic
16	TBI	M	54.22	22.95	17.0	110	Unemployed; was electrical engineer
17	TBI	M	49.64	16.14	14.0	105	Owns–runs caulking company
18	TBI	M	52.93	10.16	16.0	110	Unemployed; was owner and CEO of window–door mfg.
<i>M</i>	TBI		45.97	12.03	14.83	106.94	
<i>SD</i>			11.90	11.10	2.05	7.65	
19	Control	M	39.08	—	15.0	116	Civil engineering intern
20	Control	M	51.31	—	13.0	106	Shipping manager
21	Control	M	56.69	—	15.5	116	Artist–carpenter
22	Control	F	52.33	—	13.0	97	Health unit coordinator
23	Control	F	51.91	—	16.5	114	ESL teacher
24	Control	F	21.41	—	15.0	105	College junior (student)
25	Control	F	56.80	—	16.0	121	Custodian
26	Control	F	43.51	—	13.0	94	Stage hand–truck driver
27	Control	M	53.00	—	16.0	110	Mfg. production planner
28	Control	F	43.54	—	14.0	115	Massage therapist, data entry
29	Control	F	52.43	—	18.0	111	Artistic director–musician
30	Control	M	32.87	—	16.0	103	Realtor (BS in biochemistry)
31	Control	M	38.13	—	12.0	101	Masonry
32	Control	F	47.22	—	14.0	111	Self-employed baker–decorator
33	Control	M	56.31	—	20	119	Former audiologist, graduate student
34	Control	M	51.89	—	18.0	117	Chemical engineer
35	Control	M	40.73	—	16.5	109	Title searcher
36	Control	M	54.35	—	16.0	121	BA in journalism–theater critic
<i>M</i>	Control		46.86		15.42	110.33	
<i>SD</i>			9.55		2.05	7.98	

Note. Means and standard deviations are in boldface type, provided after each participant group. TBI = traumatic brain injury; M = male; F = female; ESL = English as second language; Mfg. = manufacturing; BS = bachelor of science; BA = bachelor of arts.

^aNational Adult Reading Test (Bright, Jaldow, & Kopelman, 2002).

followed by the two trial blocks of the experiment. Finally, participants completed the recognition memory task.

More specifically, during each of the training or trial blocks, participants stared at a centrally located fixation point for 1,500 ms, followed by a 500-ms forward masking row of ampersands in the center of the screen. An item was then presented subliminally for 50 ms in the center of the screen in lower case. This item was either a row of xs (baseline condition), the ensuing target word of the cue–target word pair (priming condition), or a word unrelated either to the ensuing cue or target (antipriming condition). The

subliminal item was immediately followed by the cue–target word pair, presented side by side in the center of the screen in capital letters for several seconds (5 s for controls, 9 s for TBI participants). The difference in study time was done to prevent ceiling effects for controls and floor effects for TBI participants and is an accepted methodology that does not impact metamemory accuracy (Kennedy, 2001; Kennedy, Carney, & Peters, 2003; Kennedy & Yorkston, 2000, 2004). Subliminal items were pseudorandomly presented, in that no more than three consecutive trials were presented in the same priming condition (baseline, prime, antiprime).

Table 2. Means and standard deviations of neurocognitive performance for adults with TBI and for controls.

Neurocognitive test and measure	Controls: <i>M</i> ± <i>SD</i>	TBI: <i>M</i> ± <i>SD</i>
Language		
Aphasia quotient (WAB)	99.47 ± 1.14	98.89 ± 0.81
Memory		
Digit span–total (WMS–III)	11.56 ± 3.33	11.17 ± 3.07
Short-delay verbal recall (CVLT–II): Free	0.33 ± 0.89	–0.33 ± 1.30
Short-delay verbal recall (CVLT–II): Cued	0.22 ± 0.93	–0.39 ± 1.17
Long-delay verbal recall (CVLT–II): Free**	0.42 ± 0.81	–0.44 ± 1.12
Long-delay verbal recall (CVLT–II): Cued	0.19 ± 0.71	–0.36 ± 1.11
Executive functions (D-KEFS)		
Verbal fluency: Letters, total correct**	13.00 ± 4.14	9.94 ± 2.60
Verbal fluency: Category, total correct**	13.28 ± 2.61	10.78 ± 2.73
Verbal fluency: Category switching, total correct**	14.67 ± 3.22	10.83 ± 3.42
Tower test: Total achievement	11.50 ± 2.64	12.00 ± 2.85
Trails: Number–letter switching	11.94 ± 1.73	10.78 ± 2.60
Trails: Number–letter switching vs. motor speed	10.33 ± 1.78	10.61 ± 2.76
Simple trails speed: Number sequencing*	11.50 ± 1.50	9.61 ± 3.73
Simple trails speed: Letter sequencing*	12.00 ± 1.68	9.89 ± 3.63
Simple trails speed: Motor speed**	11.67 ± 1.03	10.17 ± 1.98

Note. Reported scores are all scaled scores. WAB = Western Aphasia Battery (Kertesz, 1982); WMS–III = Wechsler Memory Scale—Third Edition (Wechsler, 1987); CVLT–II = California Verbal Learning Test—Second Edition (Delis, Kramer, Kaplan, & Ober, 2000); D-KEFS = Delis–Kaplan Executive Function System (Delis, Kaplan, & Kramer, 2001).

p* < .05. *p* < .01.

A nonrestraining chin rest was used throughout to maintain a consistent eye-to-screen distance of 86 cm.

Forty-two word pairs were presented in each block, in two sets of 21 word-pair trials. For a random half of the trials, a JOL rating was made immediately after studying the word pair. For the remaining half of the items, the JOL was delayed until the end of the trial set (2–3 min after study). Thus, participants studied 21 word pairs and made judgments immediately after studying the respective word pair for a random 10 or 11 of them (immediate JOL condition). At the end of that 21 word-pair trial set, participants made judgments on the remaining 11 or 10 items for which they had not already made immediate judgments (delayed JOL condition). All JOL ratings (both immediate and delayed) were solicited using just the cue word and a question mark in place of the target word and the request “please rate your confidence now” with the rating choices (i.e., 0%, 20%, 40%, 60%, 80%, and 100%) depicted on the screen beneath the request. JOLs were self-paced and were made using a six-button serial response box with the buttons respectively labeled with the rating choices. Once all of the delayed judgments were completed for the first set, participants repeated this procedure for the second set of 21 word pairs in the 42 word-pair block. After completion of this second set, the study and judgment phase of the 42 word-pair block was complete. If the elapsed time was less than 10 min, participants were engaged in a filler task of 2 min of conversation before proceeding to the cued-recall test. Otherwise, participants went directly into the cued-recall test.

During the cued-recall test, to minimize recency effects, the cue words from the first trial set of the block were randomly presented first, followed by randomized presentation of the cue words from the second trial set. Participants spoke

their self-paced response to each item into the microphone. The spoken response triggered a dialog box to appear on the screen, and the experimenter typed in the participant’s verbal response. If the microphone was triggered either too soon or too late (e.g., because of a noise before the spoken response, or because the spoken response was too quiet to trigger the microphone), then an x was entered into the dialog box followed by the participant’s verbal response. Such trials were removed from response-time analysis but retained for recall accuracy and JOL analyses.

Once the training block was completed, the participant was encouraged to ask clarification questions about how to perform the task, and any questions were answered. No suggestions were made regarding memorization techniques or other strategies. Then the two trial blocks were each completed in the same manner as the training block but with no further discussion about the task. A 15-min break was provided after the training block and each trial block.

Validation that masked stimuli were subliminal—the recognition memory task. After completion of the implicit metamemory task, participants were given a recognition memory task to determine whether the subliminal masked items were consciously recognized. Single words were presented centrally one at a time. Participants were asked to make self-paced “old–new” judgments on the items by pressing a button labeled *old* or *new*. They were instructed to press the *old* button if they thought the word had been presented during the implicit metamemory task or the *new* button if not.

Forty-two single words were presented for the self-paced old–new judgments. Of these, 14 words consisted of the 14 target words corresponding to the positive priming condition of the second (most recent) trial block of the

implicit metamemory task. Not only had these words been seen supraliminally (for the 5- or 9-s study time for controls and those with TBI, respectively) but they had also been presented subliminally (i.e., primed) for 50 ms immediately before study of that item during the second block. Another 14 words were the antipriming words that were subliminally presented immediately before each of the word pairs that were in the antipriming condition. Antipriming items had only been presented subliminally and were never presented as part of the cue–target stimuli in any block of the experiment. The last set of 14 words consisted of *new* words—words that had not been used previously in the experiment at all. The list of new words was constructed according to the same criteria used for the lists of cue–target word pairs and antipriming stimuli (described below).

The order of presentation of the single words in this validation task was randomized. The statistical analyses of this validation check are presented in the “recognition memory task” subsection of the results section below. Finally, another manipulation check was done. On completion of the recognition memory task, participants were asked whether or not they thought they had seen any words during the presentation of the row of ampersands before studying each word pair.

Materials

List construction. The lists used for this task were constructed using words of three to seven characters in length, imageability values ranging between 510 and 690, and concreteness ranging between 600 and 700, as generated using the MRC Psycholinguistic Database (see Coltheart, 1981). The database derives imageability and concreteness values from Gilhooly and Logie (1980); Paivio, Yuille, and Madigan (1968); and Toglia and Battig (1978), and with values ranging between a minimum of 100 and a maximum of 700. The numerical ranges in our study were targeted to match previous research (Kennedy & Yorkston, 2000, 2004) and to maintain the narrowest range of imageability and concreteness values while still producing a sufficient number of words to construct lists of the required size. Words that were judged to be potentially disturbing (e.g., blood, gun, etc.) or which appeared in the psychometric tests (e.g., on the CVLT–II, NART, etc.) were removed.

Words were then assembled into pairs, using the pairwise comparison feature of the Latent Semantic Analysis (LSA) website (see Laham, 1998), to ensure that the two words in each of the word pairs were not similar. An LSA similarity rating of approximately 0.0 reflects minimal similarity between the words in the pair (e.g., tooth–valley), whereas a similarity rating of $>.70$ reflects a high degree of similarity (e.g., breathe–inhale). For our study, word pairs with a similarity rating greater than .14 (out of a maximum of 1.00) were randomly reshuffled until all word pairs had similarity ratings $\leq .14$. Examples of word pairs with similarity of .14 are clown–lunch, sister–palm, orange–flood. The global mean value of similarity for all of the word pairs was 0.061 ($SD = 0.048$).

Eighteen lists of seven word pairs each were created by successive random replacement of word pairs within a list until the mean values for similarity of the word pairs within the list and the mean values for the concreteness and imageability for the set of words in the list were within 0.5 SD s of the global mean, minimizing the chance that t tests would find significant between-list differences in similarity, concreteness, or imageability values. Two-tailed unpaired t tests showed that only 1.25% of the 765 t tests demonstrated any list differences at the $p < .05$ level.

To further minimize any chance that list differences may cause nuisance variables or artifacts in the analyses, these 18 lists were then permuted across all 18 participants within each group, to achieve full counterbalancing of all combinations of list conditions: 3 blocks (1 training, 2 trial) \times 3 priming conditions \times 2 JOL timing conditions. For each participant, the trial order was constructed using pseudo-randomization with replacement, such that from one trial to the next no more than three successive occurrences of any one experimental condition appeared. Thus, for example, no more than three trials with the same priming condition (baseline, prime, antiprime) or three with the same JOL timing condition (immediate, delayed) would occur in immediate succession. Also, for each participant the first four trials within each block were removed from analysis to minimize primacy effects. Recency effects were addressed as described in the Method section above.

Hardware–software. At the University of Minnesota, a Dell Optiplex GX260 personal desktop computer, running Windows XP, interfaced with a six-button serial response box and running E-Prime software version 1.1 (Psychology Software Tools, 2001; Schneider, Eschman, & Zuccolotto, 2002) controlled stimulus delivery and data acquisition for this task. The computer monitor was a Dell Ultrasharp 15-in. flat panel LCD monitor, with a refresh rate of 75 Hz, resulting in a refresh duty cycle of 13.3 ms. An Audio-Technica ATR-30 cardioid, low-impedance microphone was used to trigger participants’ verbal responses. A custom-made, adjustable, nonrestraining chin rest was fashioned for this experiment to ensure consistent vertical eye level and eye-screen distance. At the University of Connecticut, a nearly identical hardware–software platform was used. The only differences were the use of a Dell Optiplex 960 personal desktop computer, running E-Prime software version 2.0, with a Samsung Syncmaster 2233RZ 22-in. flat-panel LCD monitor with a refresh rate of 120 Hz, resulting in a refresh duty cycle of 8.33 ms and an Audio-Technica ATR-20 cardioid, low-impedance microphone.

Results

All of the sample distributions for the two tasks either met the statistical assumption of normality or only mildly violated it. As analysis of variance (ANOVA) is highly robust to mild violations of normality (Box & Anderson, 1955; Levy, 1980; Lindman, 1974); its use here was judged to be valid. Statistical assumptions for homogeneity and sphericity were either met or addressed through adjustment

of statistical tests.¹ Because the recognition memory task was crucial to establish the validity of the implicit metamemory task, it will be discussed first.

Recognition Memory Task

The purpose of this task was to confirm that participants were unaware of the subliminal masked priming in the implicit metamemory task. The dependent variable was percentage judged as old (“percent old”). The independent variables were group (TBI, control) and item-type (primed, antiprimed, new, described in the Method section). If indeed participants were unaware of the antiprime stimuli, then the prediction was that there should be a low percentage of antiprime stimuli judged as old. In fact, the hypothesis is that the percentage of old judgment for antiprime and new items should not be different from one another, but both should be significantly lower than the prime–target word stimuli.

In Table 3, we show descriptive statistics of the recognition memory task. A 2 × 3 repeated measures ANOVA was conducted, and this showed a highly significant main effect of item type with a large effect size, $F(2, 68) = 182.53$, $p < .001$, partial $\eta^2 = .843$. The main effect of group was nonsignificant, $F(1, 34) = 0.150$, $p = .701$. The interaction of group and item type was nonsignificant, $F(2, 33) = 0.322$, $p = .727$. Pairwise comparison for the effects of the different item types on percentage of old judgment showed that both antiprime and new items were judged as old significantly less often than primed items ($p < .001$). Pairwise comparison showed that the difference between antiprime and new items was nonsignificant ($p = .192$). Thus, the expected result that both antiprime and new stimuli would be significantly less likely to be judged as old, compared with prime–target items, was obtained for both groups.

One final source of evidence that participants were unaware of the subliminal masked presentation derives from participant responses on questioning. At the conclusion of the recognition memory task participants were asked whether they thought they had seen any words during the ampersand “flash” of the implicit metamemory task. If a participant answered “yes,” additional questions were to be asked to determine whether they really had some conscious awareness of the masked items (e.g., whether what they saw were words or strings of nonsense characters) and what percentage of the time this occurred. Thirty-five of the 36 participants denied seeing anything at all. Only one participant claimed to have seen anything; he claimed to have seen real words during presentation of the ampersands 1% of the time but could not recall any word.

¹There were no violations of the homogeneity assumption in the recognition memory task. In the implicit metamemory task, only four out of the 36 distributions mildly violated this assumption. However, the ANOVA procedure remains quite robust (Rasmussen, 1995; Refinetti, 1996) and in these cases, the epsilon-adjusted F test was used, and the Greenhouse–Geisser estimate of epsilon was reported.

Table 3. Recognition memory task: Means and standard deviations for percentage of items judged as old for each participant group ($n = 18$ per group).

Dependent variable and item type	Control: $M \pm SD$	TBI: $M \pm SD$
Old (%)		
Prime	87.84 ± 11.43	86.17 ± 10.69
New	24.16 ± 20.09	24.89 ± 18.14
Antiprime	16.98 ± 20.89	21.72 ± 16.79

Implicit Metamemory Task

In the implicit metamemory task, the independent measures were priming condition (baseline, prime, antiprime), JOL timing condition (immediate, delayed), and participant group (TBI, control). The dependent measures of the implicit metamemory task included JOL ratings, cued-recall accuracy, difference scores, and Goodman–Kruskal gamma correlation (G). Following the methodology of T. O. Nelson and Dunlosky (1991), the JOL ratings were obtained by asking participants to make item-by-item judgments of the likelihood that during the cued-recall test they would correctly recall the target word of the word pair if given just the cue word; ratings were made on a 6-point Likert scale, ranging from 0% *likely to recall* to 100% *likely to recall*, in increments of 20%. Difference scores were calculated by subtracting the mean cued-recall percentage accuracy from the mean JOL ratings, for each combination of conditions. Thus, for example, if a participant’s mean immediate JOL ratings for primed items averaged 75% and mean cued-recall accuracy was 35%, then the difference score for that combination of conditions would be +40%. Positive difference scores indicate the degree of overestimation by the participant (rating performance higher than actual performance), whereas negative difference scores indicate the degree of underestimation.

Difference scores are described as indicators of absolute predictive accuracy in that they compare a participant’s predictions of recall with actual cued-recall accuracy (Kennedy & Yorkston, 2000; T. O. Nelson & Dunlosky, 1991). In contrast, the widely used Goodman–Kruskal G correlation is considered a measure of relative predictive accuracy (Kennedy & Yorkston, 2000; T. O. Nelson, 1984; T. O. Nelson & Dunlosky, 1991). Gamma accounts for participants’ habitual overestimate or underestimate of their actual cued-recall performance. Even when participants demonstrate poor calibration of absolute predictive accuracy (e.g., with +40% overestimation as in the example above), relative to such a tendency to habitually overestimate or underestimate recall, they may still reliably demonstrate higher JOL ratings for items correctly recalled and lower JOL ratings for items incorrectly recalled. For example, a person with mean cued-recall accuracy of 35% may be overconfident and typically use only the 60%, 80%, and 100% ratings when making JOLs, with an average of 75% for JOL ratings; however, it may be that those items that receive JOLs of 100% are always correctly recalled, whereas those that receive

JOLs of 60% are always incorrectly recalled. Such a participant may be overconfident on the whole but may reliably move up or down in JOL ratings in a manner that tightly tracks actual cued-recall performance differences; that participant would then have a high degree of predictive accuracy relative to his or her typical use of the Likert scale even while demonstrating poor absolute predictive accuracy. Goodman–Kruskal G correlation is the most commonly used metric to capture this relative predictive accuracy.

Before analysis of the central research questions, two-tailed, paired-samples *t* tests were conducted to determine whether the trial block (trial block 1 vs. trial block 2) demonstrated any difference for either JOL ratings or cued-recall accuracy. All *t* tests were nonsignificant at the $p < .05$ level (*ps* ranged from $p = .113$ to $p = .989$), and therefore the data could be collapsed across the two trial blocks. A three-way (Priming \times JOL Timing \times Group) repeated-measures ANOVA was conducted for each of the dependent variables to answer the research questions. Dependent variables included JOL ratings, cued-recall accuracy (percentage recalled), absolute predictive accuracy (as measured by difference score), and relative predictive accuracy (as measured by Goodman–Kruskal G). See Table 4 for descriptive statistics of these variables.

Recall accuracy. There was no significant effect of priming condition on cued recall, $F(2, 33) = 0.885, p = .432$. There was a highly significant main effect of JOL timing, $F(1, 34) = 33.954, p < .001$, partial $\eta^2 = .500$, with items for which delayed JOLs were made demonstrating greater cued-recall accuracy than items for which immediate JOLs

were made (mean difference = 9.597, $SE = 1.647, p < .001$). The group variable approached significance, $F(1, 34) = 3.620, p = .066$, partial $\eta^2 = .096$, with cued recall for controls exceeding that for individuals with TBI (mean difference = 14.050, $SE = 7.384, p = .066$). No interactions were significant. The finding of increased recall for items receiving delayed JOLs is consistent with prior research (Rhodes & Tauber, 2011) and with the hypothesis that delayed JOLs provide an opportunity for spaced retrieval practice (Spellman & Bjork, 1992).

JOL ratings. There was a significant main effect of priming condition, $F(2, 68) = 4.216, p = .025$, partial $\eta^2 = .110$ (Figure 1). Pairwise comparison of priming conditions demonstrated that mean JOL for antiprime items was significantly lower than for baseline items (mean difference = 2.708, $SE = 0.755, p < .001$, while the decreased mean JOL for prime items relative to baseline items was not significant (mean difference = 1.917, $SE = 1.105, p < .092$). Mean JOL for the prime versus antiprime items was not significantly different (mean difference = 0.792, $SE = 0.985, p < .427$). Finally, there was no main effect on JOLs either of timing condition, $F(1, 34) = 2.519, p = .122$, or of group, $F(1, 34) = 0.144, p = .707$, nor was any interaction statistically significant. That antipriming significantly decreased JOLs is a novel finding and provides support for the alternative hypothesis that masked stimulus presentation should affect JOLs.

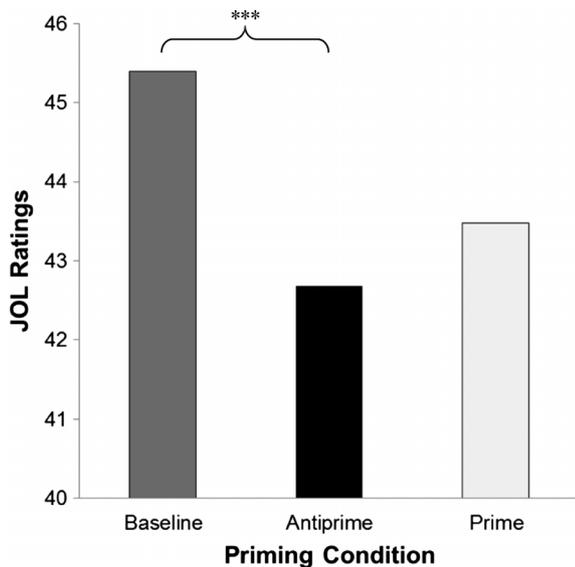
Difference scores. The priming condition was significant, $F(2, 33) = 3.357, p = .047$, partial $\eta^2 = .169$. Pairwise comparison showed that difference scores for antiprime

Table 4. Implicit metamemory task: Descriptive statistics for cued-recall accuracy, JOL ratings, difference score, and Goodman–Kruskal gamma correlation (G) for both participant groups ($n = 18$ per group), and data collapsed across both trial blocks.

Dependent variable	JOL timing condition	Priming condition	Control: $M \pm SD$	TBI: $M \pm SD$
Recall accuracy	Immediate	Baseline	36.98 \pm 22.38	25.53 \pm 23.61
		Prime	40.08 \pm 25.93	26.68 \pm 19.10
		Antiprime	43.12 \pm 25.98	26.67 \pm 20.68
	Delayed	Baseline	55.32 \pm 33.39	34.95 \pm 23.11
		Prime	46.71 \pm 24.00	33.12 \pm 23.13
		Antiprime	47.80 \pm 26.43	38.76 \pm 25.10
JOL ratings	Immediate	Baseline	39.84 \pm 21.38	43.43 \pm 25.90
		Prime	39.91 \pm 21.68	44.53 \pm 25.53
		Antiprime	39.98 \pm 20.60	40.42 \pm 27.29
	Delayed	Baseline	55.03 \pm 30.42	43.26 \pm 26.10
		Prime	48.08 \pm 25.43	41.39 \pm 23.05
		Antiprime	49.77 \pm 28.15	41.57 \pm 25.13
Difference score	Immediate	Baseline	2.87 \pm 24.36	17.89 \pm 28.54
		Prime	-0.17 \pm 25.31	17.86 \pm 23.16
		Antiprime	-4.14 \pm 25.76	13.74 \pm 22.67
	Delayed	Baseline	-0.28 \pm 20.54	8.32 \pm 13.13
		Prime	1.39 \pm 18.39	8.26 \pm 21.61
		Antiprime	1.98 \pm 19.42	2.81 \pm 10.65
G correlation	Immediate	Baseline	0.06 \pm 0.66	0.47 \pm 0.51
		Prime	0.35 \pm 0.57	0.62 \pm 0.39
		Antiprime	0.25 \pm 0.50	0.57 \pm 0.56
	Delayed	Baseline	0.72 \pm 0.65	0.84 \pm 0.20
		Prime	0.89 \pm 0.14	0.89 \pm 0.16
		Antiprime	0.86 \pm 0.20	0.83 \pm 0.18

Note. JOL = judgment of learning.

Figure 1. Main effect ($p = .025$) of priming on judgment of learning (JOL) ratings in the implicit metamemory task. Pairwise comparison reveals that antiprimed items received significantly lower JOL ratings than baseline items ($***p < .001$). Prime items were nonsignificantly lower than baseline ($p = .092$).



items were significantly lower than baseline (mean difference = -3.603 , $SE = 1.638$, $p = .035$) and borderline significant for antiprime versus prime (mean difference = -3.237 , $SE = 1.614$, $p = .053$), indicating the possibility that antipriming may be reducing participants' overconfidence. JOL timing condition was not significant, $F(1, 34) = 1.818$, $p = .186$, nor was participant group, $F(1, 34) = 3.639$, $p = .065$. No interactions approached statistical significance.

Gamma correlation. Priming condition was not significant, $F(2, 17) = 0.194$, $p = .826$. As expected, JOL timing condition was highly significant, reflecting the delayed JOL effect, $F(1, 18) = 68.355$, $p < .001$, partial $\eta^2 = .792$, with a large effect size; delayed JOLs demonstrated significantly greater G than immediate JOLs (mean difference = 0.426 , $SE = 0.052$, $p < .001$). Consistent with prior research, group differences were not significant, $F(1, 18) = 0.019$, $p = .891$. Finally, there were no significant interaction effects.

Discussion

Below, we summarize and interpret the findings in terms of the principal research questions. First, it is important to note that subliminally presented items were indeed not consciously perceived as demonstrated by judgments in the recognition memory task. The subliminally presented antiprime stimuli and the new items (not previously used in the experiment) were both judged as old significantly less often than target study items, with no group differences in this pattern. All but one participant denied seeing words during the masked presentation, and the one participant who claimed to have seen words stated that this occurred

less than 1% of the time. Thus, the subliminal masked items were not consciously perceived.

Summary of Principal Findings

The first research question asked whether subliminal masked priming and antipriming manipulations would affect measures related to JOL in a paired associate learning task. We found significant effects of masked priming/antipriming on JOL ratings and absolute predictive accuracy (difference score) and no effect on relative predictive accuracy (G). In particular, relative to baseline, antipriming significantly lowered JOL ratings and difference scores in both participant groups; however, priming did not significantly affect either measure.

Our second research question asked whether neurological status of TBI affected implicit memory, explicit memory, or metamemory, or any of the links found between or among these memory phenomena. With regard to implicit memory, the participant groups showed no differences and none of the effects of implicit memory manipulation on the metamemory measures differed for the two groups. Thus, TBI did not appear to compromise implicit memory; additionally, TBI did not affect the findings of a relationship between implicit memory and metamemory—there were no Group \times Priming interaction effects for any of the metamemory measures. As for explicit memory, individuals with TBI averaged about 14% lower in their cued-recall accuracy compared with controls, and this difference approached significance. One effect on explicit memory that was highly significant was the manipulation of the timing of when JOLs were requested (i.e., immediate vs. delayed JOL). In particular, delaying JOLs improved recall by almost 10% for both participant groups. It is noteworthy that neurological status did not interact with this effect. This finding is important because the approximate 10% absolute benefit to cued recall that occurred because of delaying the JOLs has a much greater relative impact on the individuals with TBI. Improving cued recall from 26.3% to 35.6% (TBI group) represents a larger proportional gain than improving cued recall from 40.0% to 49.9% (control group). Finally, as for the effects of TBI on metamemory, we found no significant main or interaction effects involving participant group for JOL ratings, difference scores, or for γ , relative predictive accuracy. However, it should be noted that there was a nonsignificant trend for the TBI group to overestimate their recall more than controls.

Interpretation of Findings and Limitations

Effects of masked priming and antipriming on metamemory measures. The finding of a significant main effect on JOL ratings and absolute predictive accuracy of masked stimulus presentation before word-pair study and judgment is novel and intriguing and addresses several questions. What is the mechanism by which antipriming affected JOLs? Did antipriming weaken the encoding strength of word pairs, which in turn lowered the JOL ratings, or was

there a more direct effect of antipriming on other aspects of the cognitive processes involved in making the JOLs? Although our research was not designed to compare the effects of masking stimuli immediately before encoding versus immediately before judgment, the absence of a priming or an antipriming effect on recall suggests that the masked stimuli affected the JOLs directly, rather than as a downstream byproduct of effects on the original encoding. Furthermore, immediate JOLs were made within seconds of antiprime stimulus presentation, and delayed JOLs were made within 2 min; by contrast, cued recall occurred about 10 min after the masked stimulus. Given the well-documented rapid decay of the effects of masked priming (Forster, Booker, Schacter, & Davis, 1990; Forster, Mohan, & Hector, 2003), it is not surprising that cued recall was not affected.

Only three prior studies have manipulated metacognitive judgments through presentation of masked stimuli, and all of these involved FOKs or remember-know judgments, not JOLs. Jameson et al. (1990) presented participants with a general knowledge question, followed by masked presentation of either a prime (the correct answer to the question) or a nonsense word, followed by the general knowledge question again. Participants then answered the questions and made FOKs on nonrecalled items. Recall was higher for primed items, FOK ratings remained unaffected, and relative FOK accuracy (γ) for the nonsense words was higher than for the primed answers. Counterintuitively, their findings mean that priming lowered the relative predictive accuracy of FOK judgments. Jameson et al. (1990) did not speculate as to the cause. Also, because real words were used as foils and there was no true baseline, Jameson et al.'s method and results are not directly comparable with our study. Nevertheless, this landmark study was the first to suggest that masked priming might affect relative predictive accuracy of a metamemory judgment.

Rajaram (1993) also found effects of masked stimulus presentation on metamemory judgments. Study of single words was followed by a recognition memory test (old-new judgments), during which a masked prime or antiprime was presented immediately before each test word; there was no neutral baseline. Participants then made *remember* versus *know* judgments, similar to FOKs. Rajaram found that priming significantly affected the judgments. However, in the absence of a true baseline condition against which to compare prime and antiprime items, we do not know whether this represented an increase for the primed items, a decrease for the antiprimed items, or some combination. Thus, it is possible that Rajaram's results demonstrated a similar phenomenon to what we have found here: namely, antipriming of a metamemory judgment.

Kinoshita (1997) replicated and extended Rajaram's findings by investigating masked priming of both remember-know judgments (Experiment 1) and FOKs (Experiments 2–5). Kinoshita obtained the same implicit effects for know judgments. For FOKs, there was no effect for failed recall attempts. However, for those items that participants

succeeded in recalling, FOK responses were higher for primed items (or lower for antiprimed items). Here again no neutral baseline was included; therefore, it is unclear whether the effects on the metamemory judgments reflected priming, antipriming, or some combination. What we can conclude from Kinoshita's work is that presentation of masked stimuli did affect metamemory judgments under certain conditions and that either priming enhanced FOKs or antipriming lowered them.

Taken together, the above three studies provide evidence that masked priming or antipriming can affect measures associated with remember-know and FOK-type metamemory judgments. Critically, however, a true baseline condition has been missing. Our results indicate that when such a condition is included, it is possible to disambiguate whether the effects on the metamemory judgment are because of priming or antipriming. We therefore recommend that future studies incorporate a neutral baseline (i.e., one which carries no phonological, lexical, or semantic information), as well as an antiprime condition.

Although the studies by Rajaram (1993) and Kinoshita (1997) provided possible support for the idea that masked antipriming may lower metamemory judgments, they do not shed light on why priming did not increase JOLs in this study or on why both immediate and delayed JOLs were equally lowered. Had antipriming affected one or more stages of memory (initial attention, encoding, storage, or retrieval), we might expect that cued recall would show a priming or antipriming effect, but this was not observed. This finding suggests that the masked stimulus presentation affected nonmnemonic aspects of the cognitive processes involved in making JOLs without a commensurate effect on recall. One possibility is that the masked stimulus affected attentional processes, which may have in turn affected the JOLs.

Kelley and Sahakyan (2003) examined the performance of young adults in full and divided attention conditions on a paired-associate learning task followed by cued recall and item-by-item retrospective confidence judgments (RCJs). They found that divided attention during encoding significantly lowered RCJs. Barnes and Dougherty (2007) investigated the effects of divided attention on global JOLs in a four-trial list learning task. They found that divided attention during encoding and retrieval significantly elevated participants' confidence scores during the first one or two of the four list learning trials, but it did not affect confidence during the third or fourth trials of any attention condition or any trials for divided attention during judgment. If manipulations of attentional processes during encoding can affect metamemory judgments, as in the above two studies, it may be suggested that the subliminal stimuli in the present study affected the JOLs here. This suggestion has the added merit that it might explain why we found an effect of antipriming but not priming; although the prime stimuli are congruent with the ensuing target word, they lack the attentional salience that the incongruent antiprime stimuli would have. This is an interesting speculation and bears follow-up investigation, particularly because we found no difference

in the antipriming effect for immediate versus delayed JOLs. Such a study would have to account for changes in attention between the immediate and delayed JOL conditions.

Prior research has shown that the effects of masked priming decay over some 8–9 s (Forster & Davis, 1984; Masson & Bodner, 2003). In our study, the elapsed time between masked stimulus onset and request for JOL was 5 s for controls and 9 s for individuals with TBI for immediate JOLs, and some 2 min for delayed JOLs. Thus, a plausible explanation for the antipriming effect on JOLs must account for the fact that there was no difference in the antipriming effect for immediate and delayed JOLs. In particular, how is it that the rapidly decaying masked stimulus was able to affect delayed JOLs, which are made some 2 min after stimulus presentation? One factor that may shed light on this is that in the present experiment participants in both groups were observed routinely to sit poised to make JOLs on every trial, irrespective of whether a JOL was requested. Despite being instructed that their main goal was to learn the word pairs, and their demonstrating clear understanding that button presses during study would not be recorded, it was a routine occurrence for participants to press a button to make a JOL during study while the word pair was still on the screen and no JOL request had yet been made. Perhaps, then, in the few seconds following masked stimulus presentation (i.e., during the 5 or 9 s of studying the word pair), two sets of cognitive processes were operating—the encoding processes to study and learn the word pairs and the JOL processes (occurring in anticipation that a JOL might be requested). The masked stimulus presentation would affect the JOL processes during this time. Then, when an actual JOL was later requested (whether immediately after study or after a 2-min delay), the appearance of the cue word in the JOL request would to some degree reinstantiate the previously generated JOL rating associated with that cue word, thus affecting the JOL rating irrespective of whether it is immediate or delayed. Such an explanation could also account for the lack of a priming effect. Because masked primes are congruent with the ensuing target word they may have no more effect on the JOL processing during word-pair study than essentially increasing target exposure time by 50 ms. Conversely, a masked antiprime stimulus, because of its incongruence with both the ensuing cue and target, might affect the JOL processing.

To our knowledge, our research is the first to find effects of masked stimulus presentation on JOLs. Furthermore, although these findings are compatible with existing models of JOLs, the evidence for a unique effect of antipriming and that it distributes to both immediate and delayed JOLs, may assist in refining the models. Finally, that the effect was similar in both healthy controls and individuals with TBI is encouraging in that it may suggest clinical application.

Effects of traumatic brain injury on implicit, explicit, and metamemory. The second research question asked whether there would be differences in implicit, explicit, or metamemory, or to any of the links we found between or among these memory phenomena, as a function of

neurological status. We found neither a main effect of group nor any Group \times Priming interaction for implicit memory effects on any of the dependent measures. This supports prior research showing little to no impairment in behavioral measures of implicit memory in those with TBI (Haut, Petros, Frank, & Haut, 1991; Swick, 1998; Vakil & Oded, 2003; Vakil & Sigal, 1997). As for explicit memory, given the consistent findings in the literature that individuals with TBI demonstrate impaired explicit recall (e.g., Levin, 1989; Lezak, 1979), we expected to find significant group differences. However, in the implicit metamemory task, group difference in cued recall did not reach statistical significance. That the TBI group's recall was impaired relative to the control group did manifest in the CVLT–II long-delay free-recall results. Very likely, several factors in the implicit metamemory task mitigated the effects of explicit memory impairment in the TBI group.

First, to avoid floor effects, we gave individuals with TBI 9 s to study word pairs, whereas controls were only given 5 s as done in prior research (e.g., Kennedy et al., 2003; Kennedy & Yorkston, 2000). The differential amount of study time likely closed some of the cued-recall performance gap between groups. Second, unlike the work by Kennedy and Yorkston (2000, 2004), which used a study–restudy paradigm (and did reveal a significant group difference in cued-recall accuracy), here we only allowed one study phase. It is likely that controls in the Kennedy and Yorkston investigations benefitted from restudy more than individuals with TBI; indeed this phenomenon is exploited by the repeated study and recall cycles of the CVLT as a means of detecting differences in rate of learning among individuals with no injury, mild TBI, and moderate–severe TBI (Delis et al., 2000). As further evidence of the reduction in cued recall because of having only a single study phase, recall accuracy by both groups in our study was lower than TBI and control groups in Kennedy and Yorkston (2000). Using similar differential study times but with a group of TBI participants who were less cognitively impaired than those in Kennedy and Yorkston (2000), recall accuracy between groups was similar.

Another independent variable was also found to affect recall performance in the JOL task. For both groups, items for which delayed JOLs were made were more accurately recalled than items for which immediate JOLs were made. This finding is in contrast to some prior studies (Kennedy et al., 2003; Kennedy & Yorkston, 2000, 2004; T. O. Nelson & Dunlosky, 1991), in which no difference in recall accuracy was observed between items for which immediate versus delayed JOLs were solicited. However, in their meta-analysis, Rhodes and Tauber (2011) found that across the literature, there was a modest but significant increase in recall for those items that had received delayed JOLs versus those receiving immediate JOLs. Spellman and Bjork (1992) provided a theoretical rationale for why this should be so. They argued that

one strategy for making a delayed JOL is to use the presented stimulus as a cue to try to recall the

response item . . . successful covert recall during the JOL task will in turn increase the likelihood that the subject will successfully recall that item on the later overt recall test. (p. 315)

Thus, for immediate JOLs, the target word is still in short-term memory because the word pair had been on the computer screen immediately before the JOL request; therefore, it can be argued that there was no reencoding or reinstating of the memory for the target word. However, delayed JOLs are made long enough after presentation of the word pair that short-term memory of the target will have decayed. When the delayed JOL is then solicited, a covert recall process may be made, as suggested by Spellman and Bjork.

However, this hypothesis does not explain why others have not found a delayed JOL effect on recall (e.g., Kennedy & Yorkston, 2000, 2004; T. O. Nelson & Dunlosky, 1991). One possibility is that the difference in recall accuracy between JOL timing conditions found here may be muted in a study–restudy paradigm sometimes used in earlier studies. Thus, it may be that in the previous studies the second opportunity to study the word pairs increased recall accuracy overall, to the point where the JOL effect on recall disappears. One final point in this context is that while the Rhodes and Tauber (2011) meta-analysis found a modest but reliable recall benefit to delayed JOLs, these studies only included neurologically normal individuals. The present research is the first to demonstrate this effect in individuals with TBI, and this has important clinical ramifications.

Consistent with prior studies, individuals with TBI demonstrated no real impairment in relative predictive accuracy (γ) as compared with controls (e.g., Kennedy, 2001; Kennedy & Yorkston, 2000; Schmitter-Edgecombe & Anderson, 2007). That is, individuals with TBI are able to make predictions of future recall using Likert scale ratings when the absolute values of the scale are ignored; meaning that higher values (e.g., 80% certain) are consistently assigned to items they recall whereas lower values (e.g., 60% certain) are consistently assigned to items they will not recall. Although we found no impairment in relative predictive accuracy, mean group differences in absolute accuracy (difference score) were significantly different. This is also consistent with prior research (Kennedy & Yorkston, 2004).

Limitations. There were a number of limitations to this research. As with similar studies of metamemory in the TBI population (e.g., Kennedy & Yorkston, 2000, 2004; Schmitter-Edgecombe & Anderson, 2007; Schmitter-Edgecombe & Wright, 2004), our research has a small sample size, limiting the power of the analysis and also precluding analysis of results by injury severity classification. Another limitation was the unavailability of complete and consistent medical information on all participants with TBI. As a result, classification of injury severity was made based on whichever one or more of several data were available for each participant with TBI (GCS scores, duration of posttraumatic amnesia, or duration of loss of consciousness).

Another limitation shared with other similar studies of JOLs in the TBI population is the challenge of avoiding recall ceiling effects in the control population while simultaneously avoiding floor effects in the TBI group. We addressed this by providing differential study time, as in prior research. Another approach might have been to give the TBI group an additional study opportunity as compared with controls; however, that might have washed out the antipriming effect in the TBI group.

A final limitation to the study was the complexity and length of the protocol. Although we were able to answer the questions posed, follow-up questions (such as what might happen if the masked prime were presented just before participants make the JOLs, rather than just before study) could not be investigated. Interesting to note, both Rajaram (1993) and Kinoshita (1997) took just such an approach in a nonclinical population making remember–know judgments or FOKs and found that masked stimuli presented immediately before judgment influenced the judgment. Therefore, a follow up to the present study comparing presentation of the masked prime before encoding versus before judgment would be important.

Clinical Implications and Future Directions

Schmitter-Edgecombe (2006) questioned which cognitive skills were spared after TBI and how to leverage those for rehabilitation. One important purpose of including a TBI population in the present study was to determine whether or not they could make use of the implicit memory manipulation to improve metamemory. A critical finding was that there was no difference in implicit memory performance between the groups. Intact implicit memory paves the way toward leveraging this ability for clinical purposes. More specifically, we found that masked antipriming significantly reduced JOL ratings and overconfidence in both participant groups. Because recall accuracy was not affected by priming condition, this may have effectively reduced the overestimation of the TBI group and warrants further investigation. If this finding bears out in a larger sample study, it may suggest the utility of antipriming in reducing the overconfidence (Kennedy & Yorkston, 2000) of individuals with TBI. In a clinical setting, masked stimulus presentation would not be practical. However, the phenomenon of long-term antipriming of memory has been documented by Marsolek and colleagues (Marsolek, 2008; Marsolek et al., 2006, 2010) both in controls and individuals with medial temporal lobe damage. Therefore, future directions for the current line of research may explore supraliminal long-term antipriming of metamemory in the TBI population to determine whether or not the antipriming benefit to metamemory accuracy can be combined with such clinical approaches as spaced retrieval, errorless learning, and so forth. For example, can supraliminally presented antiprime foils be used during tasks, while maintaining errorless learning, to promote more realistic predictions of performance? This could be combined with our findings of a recall benefit

produced by delayed JOLs, suggesting a clinical strategy of interspersing delayed JOLs throughout learning to improve encoding and subsequent recall, with the presentation of antiprime stimuli before each JOL to improve JOL calibration.

Acknowledgments

This research was made possible by generous funding from the University of Minnesota: a Doctoral Dissertation Fellowship, several block grants, a scholarship from the College of Liberal Arts, and the Bryng Bryngelson fund. We also thank the University of Connecticut for its generous support of this research through a Faculty Large Grant. We are grateful to the Minnesota Speech-Language-Hearing Association for its financial contribution. We acknowledge Edward Carney for copious technical assistance throughout the project and Krystal Baumgarten and Deborah Lanza for assistance in psychometric testing. We are grateful to the Brain Injury Associations of Minnesota and Connecticut for assistance in the recruitment of participants.

References

- Barnes, K. A., & Dougherty, M. R. (2007). The effect of divided attention on global judgment of learning accuracy. *American Journal of Psychology*, *120*, 347–359.
- Box, G., & Anderson, S. (1955). Permutation theory in the derivation of robust criteria and the study of departures from assumptions. *Journal of the Royal Statistical Society*, *17*, 1–34.
- Bright, P., Jaldow, E., & Kopelman, M. D. (2002). The National Adult Reading Test as a measure of premorbid intelligence: A comparison with estimates derived from demographic variables. *Journal of the International Neuropsychological Society*, *8*, 847–854.
- Coltheart, M. (1981). *MRC Psycholinguistic Database, version 2.00* [Web database and application]. Retrieved from http://www.psy.uwa.edu.au/MRCDataBase/uwa_mrc.htm
- Delis, D. C., Kaplan, E., & Kramer, J. H. (2001). *Delis-Kaplan Executive Function System*. San Antonio, TX: The Psychological Corporation.
- Delis, D. C., Kramer, J. H., Kaplan, E., & Ober, B. A. (2000). *California Verbal Learning Test: Second Edition*. San Antonio, TX: The Psychological Corporation.
- Dunlosky, J., & Metcalfe, J. (2009). *Metacognition*. Thousand Oaks, CA: Sage.
- Forster, K. I., & Davis, C. (1984). Repetition priming and frequency attenuation in lexical access. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *10*, 680–698.
- Forster, K. I., Booker, J., Schacter, D. L., & Davis, C. (1990). Masked repetition priming: Lexical activation or novel memory trace? *Bulletin of the Psychonomic Society*, *28*, 341–345.
- Forster, K. I., Mohan, K., & Hector, J. (2003). The mechanics of masked priming. In S. Kinoshita & S. J. Lupker (Eds.), *Masked priming: The state of the art* (pp. 3–37). New York, NY: Psychology Press.
- Gilhooly, K. J., & Logie, R. H. (1980). Age of acquisition, imagery, concreteness, familiarity and ambiguity measures for 1944 words. *Behavior Research Methods and Instrumentation*, *12*, 395–427.
- Haut, M. W., Petros, T. V., Frank, R. G., & Haut, J. S. (1991). Speed of processing within semantic memory following severe closed head injury. *Brain and Cognition*, *17*, 31–41.
- Jameson, K. A., Narens, L., Goldfarb, K., & Nelson, T. O. (1990). The influence of near-threshold priming on metamemory and recall. *Acta Psychologica*, *73*, 55–68.
- Kelley, C. M., & Sahakyan, L. (2003). Memory, monitoring, and control in the attainment of memory accuracy. *Journal of Memory and Language*, *48*, 704–721.
- Kennedy, M. R. (2001). Retrospective confidence judgments made by adults with traumatic brain injury: Relative and absolute accuracy. *Brain Injury*, *15*, 469–487.
- Kennedy, M. R., Carney, E., & Peters, S. M. (2003). Predictions of recall and study strategy decisions after diffuse brain injury. *Brain Injury*, *17*, 1043–1064.
- Kennedy, M. R., & Yorkston, K. M. (2000). Accuracy of meta-memory after traumatic brain injury: Predictions during verbal learning. *Journal of Speech, Language, and Hearing Research*, *43*, 1072–1086.
- Kennedy, M. R., & Yorkston, K. M. (2004). The effects of frontal injury on self-monitoring during verbal learning by adults with diffuse brain injury. *Neuropsychological Rehabilitation*, *14*, 449–465.
- Kertesz, A. (1982). *Western Aphasia Battery*. New York, NY: The Psychological Corporation.
- Kinoshita, S. (1997). Masked target priming effects on feeling-of-knowing and feeling-of-familiarity judgments. *Acta Psychologica*, *97*, 183–199.
- Kinoshita, S., & Lupker, S. J. (Eds.). (2003). *Masked priming: The state of the art*. New York, NY: Psychology Press.
- Koriat, A. (1995). Dissociating knowing and the feeling of knowing: Further evidence for the accessibility model. *Journal of Experimental Psychology: General*, *124*, 311–333.
- Koriat, A. (1997). Monitoring one's knowledge during study: A cue-utilization approach to judgments of learning. *Journal of Experimental Psychology: General*, *126*, 349–370.
- Krause, M. O., & Kennedy, M. R. T. (2009). Changes in meta-memory over time. *Brain Injury*, *23*, 965–972.
- Lah, S., Epps, A., Levick, W., & Parry, L. (2011). Implicit and explicit memory outcome in children who have sustained severe traumatic brain injury: Impact of age at injury (preliminary findings). *Brain Injury*, *25*, 44–52.
- Laham, D. (1998). *Latent Semantic Analysis, pairwise comparison feature* [Web application]. Retrieved from <http://lsa.colorado.edu/>
- Levin, H. S. (1989). Memory deficit after closed head injury. *Journal of Clinical and Experimental Neuropsychology*, *12*, 129–153.
- Levy, K. J. (1980). A Monte Carlo study of analysis of covariance under violations of the assumptions of normality and equal regression slopes. *Educational and Psychological Measurement*, *40*, 835–840.
- Lezak, M. D. (1979). Recovery of memory and learning functions following traumatic brain injury. *Cortex*, *15*, 63–72.
- Lindman, H. R. (1974). *Analysis of variance in complex experimental designs*. San Francisco, CA: W. H. Freeman.
- Marsolek, C. J. (2008). What antipriming reveals about priming. *Trends in Cognitive Sciences*, *12*, 176–181.
- Marsolek, C. J., Deason, R. G., Ketz, N. A., Ramanathan, P., Bernat, E. M., Steele, V. R., . . . Schnyer, D. M. (2010). Identifying objects impairs knowledge of other objects: A relearning explanation for the neural repetition effect. *NeuroImage*, *49*, 1919–1932.
- Marsolek, C. J., Schnyer, D. M., Deason, R. G., Ritchey, M., & Verfaellie, M. (2006). Visual antipriming: Evidence for ongoing adjustments of superimposed visual object representations. *Cognitive, Affective & Behavioral Neuroscience*, *6*, 163–174.
- Masson, M. E. J., & Bodner, G. E. (2003). A retrospective view of masked priming: Toward a unified account of masked and

- long term repetition priming. In S. Kinoshita & S. J. Lupker (Eds.), *Masked priming: The state of the art* (pp. 57–94). New York, NY: Psychology Press.
- Mateer, C. A., Sohlberg, M. M., & Crinean, J.** (1987). Focus on clinical research: Perceptions of memory function in individuals with closed-head injury. *Journal of Head Trauma Rehabilitation, 2*, 74–84.
- Metcalfe, J., & Dunlosky, J.** (2008). Metamemory. In H. L. Roediger, III (Ed.), *Learning and memory: A comprehensive reference* (pp. 349–362). Oxford, England: Elsevier.
- Metcalfe, J., Schwartz, B. L., & Joaquim, S. G.** (1993). The cue-familiarity heuristic in metacognition. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 19*, 851–861.
- Nelson, H. E., & Willison, J. R.** (1991). *The Revised National Adult Reading Test—Test manual*. Windsor, England: NFER-Nelson.
- Nelson, T. O.** (1984). A comparison of current measures of the accuracy of feeling-of-knowing predictions. *Psychological Bulletin, 95*, 109–133.
- Nelson, T. O., & Dunlosky, J.** (1991). When people's judgments of learning (JoLs) are extremely accurate at predicting subsequent recall: The "Delayed-JOL Effect." *Psychological Science, 2*, 267–270.
- Paivio, A., Yuille, J. C., & Madigan, S. A.** (1968). Concreteness, imagery and meaningfulness values for 925 words. *Journal of Experimental Psychology Monograph Supplement, 76*(3, Pt. 2), 1–25.
- Rajaram, S.** (1993). Remembering and knowing: Two means of access to the personal past. *Memory & Cognition, 21*, 89–102.
- Rasmussen, J. L.** (1995). Parametric and non-parametric analysis of groups by trials design under variance-covariance in homogeneity. *British Journal of Mathematical and Statistical Psychology, 42*, 91–102.
- Reder, L. M. (Ed.)** (1996). *Implicit memory and metacognition*. Mahwah, NJ: Erlbaum.
- Reder, L. M., & Schunn, C. D.** (1996). Metacognition does not imply awareness: Strategy choice is governed by implicit learning and memory. In L. Reder (Ed.), *Implicit memory and metacognition* (pp. 45–77). Mahwah, NJ: Erlbaum.
- Refinetti, R.** (1996). Demonstrating the consequences of violations of assumptions in between-subjects analysis of variance. *Teaching of Psychology, 23*, 51–54.
- Rhodes, M. G., & Tauber, S. K.** (2011). The influence of delaying Judgments of Learning (JOLs) on metacognitive accuracy: A meta-analytic review. *Psychological Bulletin, 137*, 131–148.
- Richardson-Klavehn, A., Gardiner, J. M., & Java, R. I.** (1994). Involuntary conscious memory and the method of opposition. *Memory, 2*, 1–29.
- Roche, N. L., Fleming, J. M., & Shum, D.** (2002). Self-awareness of prospective memory failure in adults with traumatic brain injury. *Brain Injury, 16*, 931–945.
- Schmitter-Edgecombe, M.** (1996). Effects of divided attention on implicit and explicit memory performance following severe closed head injury. *Neuropsychology, 10*, 155–167.
- Schmitter-Edgecombe, M.** (2006). Implications of basic science research for brain injury rehabilitation: A focus on intact learning mechanisms. *Journal of Head Trauma Rehabilitation, 21*, 131–141.
- Schmitter-Edgecombe, M., & Anderson, J. W.** (2007). Feeling of knowing in episodic memory following moderate to severe closed-head injury. *Neuropsychology, 21*, 224–234.
- Schmitter-Edgecombe, M., & Wright, M. J.** (2004). Event-based prospective memory following severe closed-head injury. *Neuropsychology, 18*, 353–361.
- Schneider, W., Eschman, A., & Zuccolotto, A.** (2002). *E-Prime user's guide*. Pittsburgh, PA: Psychology Software Tools.
- Spellman, B. A., & Bjork, R. A.** (1992). When predictions create reality: Judgments of learning may alter what they are intended to assess. *Psychological Science, 3*, 315–316.
- Stein, S. C.** (1996). Classification of head injury. In R. K. Narayan, J. E. Wilberger, Jr., & J. T. Povlishock (Eds.), *Neurotrauma* (pp. 31–41). New York, NY: McGraw-Hill.
- Swick, D.** (1998). Effects of prefrontal lesions on lexical processing and repetition priming: An ERP study. *Cognitive Brain Research, 7*, 143–157.
- Toglia, M. P., & Battig, W. F.** (1978). *Handbook of semantic word norms*. Hillsdale, NJ: Erlbaum.
- Vakil, E.** (2005). The effect of moderate to severe traumatic brain injury (TBI) on different aspects of memory: A selective review. *Journal of Clinical and Experimental Neuropsychology, 27*, 977–1021.
- Vakil, E., & Oded, Y.** (2003). Comparison between three memory tests: Cued recall, priming and saving closed-head injured patients and controls. *Journal of Clinical and Experimental Psychology, 25*, 274–282.
- Vakil, E., & Sigal, J.** (1997). The effect of level of processing on conceptual and perceptual priming: Control versus closed-head-injured patients. *Journal of the International Neuropsychological Society, 3*, 327–336.
- Ward, H., Shum, D., Wallace, G., & Boon, J.** (2002). Pediatric traumatic brain injury and procedural memory. *Journal of Clinical and Experimental Neuropsychology, 24*, 458–470.
- Watt, S., Shores, E. A., & Kinoshita, S.** (1999). Effects of reducing attentional resources on implicit and explicit memory after severe traumatic brain injury. *Neuropsychology, 13*, 338–349.
- Wechsler, D.** (1987). *Wechsler Memory Scale—Revised*. San Antonio, TX: The Psychological Corporation.
- Wiegner, S., & Donders, J.** (1999). Performance on the California Verbal Learning Test after traumatic brain injury. *Journal of Clinical and Experimental Neuropsychology, 21*, 159–170.