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Does survival processing enhance implicit memory?

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Recent research has shown that human memory may have evolved to remember information that has been processed for the purpose of survival, more so than information that has been processed for other purposes, such as home-moving. We investigated this survival-processing advantage using both explicit and implicit memory tests. In Experiment 1, participants rated words in one of three scenarios: survival, pleasantness, and moving, followed by a timed stem-cued recall/stem-cued completion task. Items were completed more quickly in the survival scenario, as compared with the other two for the explicit task, but no differences were found across conditions in the implicit task. In Experiment 2, the implicit task was changed to concreteness judgments to encourage more conceptual processing. Again, the survival-processing advantage occurred in the explicit task (speeded item recognition), but not in the implicit task. These results suggest that a survival-processing advantage may benefit participants' memory performance only during explicit retrieval.

Although the evolution of human memory has long been considered (see, e.g., Glenberg, 1997; Sherry & Schacter, 1987), it has not been empirically studied until very recently in cognitive psychology. As was pointed out by Klein, Cosmides, Tooby, and Chance (2002), structure follows function. To understand the characteristics of a memory system or process, one should specify the problems that the system or process has evolved to solve. Nairne and Pandeirada (2008b) proposed three characteristics of evolved memory mechanisms. First, since there is little adaptive value in reproducing the veridical past, memory should reconstruct previous episodic experience flexibly, rather than reproduce the past like a tape recorder. Second, memory should be geared especially to help us perform actions that enhance our reproductive fitness, such as remembering the location of food. Third, memory should be tuned to remember certain kinds of domain-specific information that is relevant to survival/fitness. To explain why memory can be boosted by a specific strategy, such as self-reference, one should seek to understand the proximate cause, such as elaboration, as well as the ultimate adaptive value of the corresponding memory "tuning." To study the characteristics of the evolved memory system, Nairne and his colleagues have used a functional approach (Nairne, 2005; Nairne & Pandeirada, 2008a): First identify the selection pressures that may have shaped the evolution of memory, generate a priori predictions, and then test these hypotheses empirically.

Several studies have used the functional approach to examine memory performance (e.g., Kang, McDermott, & Cohen, 2008; Nairne & Pandeirada, 2008a, 2010; Nairne, Pandeirada, Gregory, & Van Arsdall, 2009; Nairne, Pandeirada, & Thompson, 2008; Nairne, Thompson, & Pandeirada, 2007; Otgaar, Smeets, & van Bergen, 2010; Rudine, Craig, Overbeek, & Green, 2009; Weinstein, Bugg, & Roediger, 2008). These studies have examined whether memory might be tuned to remember information that has been processed for survival, perhaps as a result of fitness advantages accrued in the ancestral past (see, e.g., Lu & Chang, 2009, for studies related to the evolution of memory yet not directly related to survival). It has generally been predicted that items that have been processed for the purpose of survival should be better remembered than those that have been processed via means that are irrelevant for survival (e.g., pleasantness ratings).

Most of the studies have tested this hypothesis by using a design introduced by Nairne et al. (2007). Participants first rate, without anticipating a later memory test, lists of words on the basis of their relevance in one of these two scenarios: survival in the grasslands of a foreign land (*survival scenario*) or moving to a new home in a foreign land (*moving scenario*), and in an additional condition, participants are simply asked to rate the pleasantness of the words (*pleasantness scenario*). The latter two conditions serve as controls—whereas rating words on the basis of pleasantness induces meaning-driven, item-specific

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processing of those words, rating words on the basis of their relevance to a moving scenario induces schema activation, which is also likely the case when words are rated on the basis of their relevance to a survival scenario.

Supporting the idea that memory may have evolved, in part, to remember information that has been processed for survival, in previous studies, participants demonstrated superior memory for the words rated in a survival scenario, relative to those rated in moving and pleasantness scenarios. This finding suggests that the mnemonic advantage of survival processing cannot be merely attributed to any item-specific or schematic memory processing and suggests that the advantage reflects an adaptive bias that is activated when participants rate the words in a survival scenario. This *survival-processing advantage* occurs whether rating scenarios are manipulated within or between subjects (Nairne et al., 2007), whether relevance ratings refer to a character depicted in a video clip or to participants themselves (Weinstein et al., 2008), whether or not the memory test is anticipated (Nairne et al., 2007), and when performance in the survival scenario is compared with that in other encoding tasks (e.g., imagery, generation, and self-reference; see Nairne et al., 2008) and scenarios (e.g., planning a bank heist in Kang et al., 2008; enjoying a vacation at a fancy resort in Nairne et al., 2008; organizing a charity event with animals at a local zoo in Nairne & Pandeirada, 2007; and surviving in a city scenario in Weinstein et al., 2008).

So far, most of the published studies have manipulated encoding strategies and scenarios and then compared the degree to which survival processing enhances memory performance as other strategies/scenarios do in explicit memory tests, including free recall and recognition. To our knowledge, no study has examined whether the survival-processing advantage occurs in an implicit test of memory. In contrast to explicit memory, which reflects conscious recollection of prior episodes, implicit memory is an unintentional manifestation of the retention of previously acquired information. When taking an implicit memory test, participants perform a task apparently unrelated to a study phase, such as filling out a word stem or judging whether a word refers to a concrete concept, rather than remembering any items they have seen before. Implicit memory is reflected in repetition priming that refers to facilitation in processing speed (shorter response time [RT]) and/or higher accuracy for studied items, relative to nonstudied items. In the present experiments, we explored whether or not a survival-processing advantage would generalize to implicit memory. To satisfy the retrieval intentionality criterion (Schacter, Bowers, & Booker, 1989) as closely as possible, we used the analogous explicit memory tests, in which all stimuli, design, and procedures were the same as those in the implicit memory tests, except that we instructed participants to remember what they had seen in the rating phase. In Experiment 1, we used a stem-cued completion test and a stem-cued recall test, in which participants were instructed to fill out a word stem with the first word that came to mind (i.e., implicit memory) or with the studied items in the rating phase (i.e., explicit memory), respectively—tasks typically used in implicit memory studies (e.g., Roediger, Weldon, Stadler, & Riegler, 1992). Apart from testing the survival-processing advantage in implicit memory, we tried to generalize the advantage to the explicit stem-cued recall test, which, to our knowledge, has never been reported in the literature.

EXPERIMENT 1

Method

Participants. Two hundred forty English-speaking undergraduates with normal or corrected-to-normal vision participated for partial course credit. Sixty received an explicit memory test (*explicit* group), and 180 received an implicit one (*implicit* group). More participants were tested in the implicit group in order to ensure sufficient statistical power to detect a potential survival-processing advantage in implicit memory (see below).

Design and Materials. A 2 (group: explicit or implicit) × 4 (scenario: pleasantness, moving, survival, or nonstudied) mixed-factor design was used. Group was a between-subjects variable, whereas scenario was manipulated within subjects but between blocks for the first three rating scenarios. The participants were given three blocks of 20 words in the study phase, with a different rating scenario in each block. A total of 170 words were chosen as stimuli, 80 of which were critical items, 12 were primacy buffer items on the memory test, and the remaining 78 were filler items on the memory test. The lexical characteristics of the stimuli are summarized in Table 1. Because stem cues were used in the memory test, care was taken to ensure that the first three letters of each word were unique among all 170 words and that the baseline completion rate when the stem cues were given (estimated by Shaw, 1997, when the item has not been studied) was below .30 for critical items (see Table 1), so as not to mask the effect due to the rating scenarios in the implicit/explicit memory tests. Unlike in prior studies in which all the stimuli were concrete words (but see Rudine et al., 2009), half of our critical items were concrete words, and half were abstract. To ensure that items were not preexperimentally associated with the meaning of *survival* more than they were with the meaning of *moving*, the mean semantic similarity between critical items and *survival* was matched with the mean semantic similarity between critical items and *moving*. Semantic similarity, as reflected by cosines in latent semantic

Table 1
Mean Statistics for Lexical Characteristics of Stimuli in Experiment 1

Characteristic	Critical Items *M*	Critical Items *SD*	Primacy Buffer and Filler Items *M*	Primacy Buffer and Filler Items *SD*
Word length	6.04[a]	0.97	5.90[a]	0.92
Word valence	5.36[a]	1.81	5.55[a]	2.06
Word arousal	4.92[a]	0.91	5.06[a]	0.88
Word dominance	5.13[a]	0.93	5.13[a]	0.98
Word concreteness	4.67[a]	1.63	4.77[a]	1.54
Log HAL word frequency	9.27[a]	1.32	9.06[a]	1.28
Word connectivity	1.65[a]	0.69	1.79[a]	0.73
Word connection strength	2.87[a]	1.22	3.11[a]	1.29
Baseline completion rate	0.17[a]	0.05	0.26[b]	0.28
Word stem set size	10.74[a]	3.75	10.00[a]	4.86
“Survival”-LSA cosines	0.08[a]	0.07	0.08[a]	0.07
“Moving”-LSA cosines	0.08[a]	0.06	0.09[a]	0.06

Note—The values within each row with different superscripts are significantly different from each other ($p < .05$, two-tailed). Word valence, arousal, and dominance are from Bradley and Lang (1999). Word concreteness, connectivity, and connection strength are from Nelson, McEvoy, and Schreiber (2004). Baseline completion rates and word stem set sizes are from Shaw (1997). The survival-LSA cosines and moving-LSA cosines were estimated via http://lsa.colorado.edu.

analyses (LSA; Landauer & Dumais, 1997) was estimated using the "General Reading up to 1st year in college" database, since our participants were mostly first-year college students.

The 80 critical items were divided into four groups, three of which were assigned to be study items in the three rating scenarios, and the remaining was assigned to be nonstudied items, which were the baseline test items in the memory test. The four groups were rotated across participants, such that each served in one of four scenario conditions equally often. The order of the three rating scenarios was counterbalanced across participants. Equal numbers of participants in the explicit and implicit groups received each of the 12 counterbalancing lists.

Procedure. PC-compatible computers were used to display the stimuli and to collect data. The participants were tested in a quiet computer lab in groups of 2–8 and were seated 60 cm away from the screen. All stimuli were presented in Courier New Bold with a font size of 18 in white on a black background. There were three phases: rating phase, filler task phase, and surprise final memory test phase. In the rating phase, prior to each of the three blocks, the participants received one of the three scenarios (pleasantness, moving, or survival) and then rated the words on the basis of the scenario. On each trial of the rating task, a word appeared at the center of the screen, and participants were asked to rate it using a 5-point scale, where 1 = *totally irrelevant*/*unpleasant* and 5 = *extremely relevant*/*pleasant*. The rating scale appeared on the screen below each presented item, and the participants responded by pressing the appropriate key on the number pad. Neither the participants in the implicit group nor those in the explicit group anticipated that they would be given a memory test after the rating task. The participants were asked to respond within 5 sec on each trial, and response times (RTs) were recorded. If a response was not made within 5 sec, the next trial began automatically. Adapted from Nairne et al. (2007), the descriptions of the three scenarios were the following.

Survival scenario. "In this task we would like you to imagine that you are stranded in the grasslands of a foreign land, without any basic survival materials. Over the next few months, you'll need to find steady supplies of food and water and protect yourself from predators. We are going to show you a list of words, and we would like you to rate how relevant each of these words would be for you in this survival situation. Some of the words may be relevant and others may not—it's up to you to decide."

Moving scenario. "In this task we would like you to imagine that you are planning to move to a new home in a foreign land. Over the next few months, you'll need to locate and purchase a new home and transport your belongings. We are going to show you a list of words, and we would like you to rate how relevant each of these words would be for you in accomplishing this task. Some of the words may be relevant and others may not—it's up to you to decide."

Pleasantness scenario. "In this task, we are going to show you a list of words, and we would like you to rate the pleasantness of each word. Some of the words may be pleasant and others may not—it's up to you to decide."

The participants were given self-paced breaks in between the blocks of the rating tasks. Following completion of all three rating blocks, they performed a series of filler tasks to avoid the possibility of ceiling performance on the explicit memory test, as well as to mask the nature of the implicit memory test. In these tasks, they were instructed to type in (1) as many U.S. states as possible within 2.5 min, (2) as many U.S. presidents' last names as possible within 4 min, and (3) as many male and female names as possible within 2.5 min. Immediately after these filler tasks, the participants in the implicit group received a stem-cued completion test, and those in the explicit group received a stem-cued recall test. These procedures are in line with Schacter et al.'s (1989) retrieval intentionality criterion for implicit memory studies, since both explicit and implicit groups received the same set of procedures for the rating phase, filler task phase, and this surprise memory test phase, with the only difference being the memory test instructions.

In the explicit/implicit memory test, participants were presented with 170 word stems (e.g., TOA-), one at a time, at the center of the screen. These word stems consisted of 12 primacy buffer items (to familiarize the participants with the test procedure), which were presented at the beginning of the test, 60 studied items from the three rating tasks, 20 nonstudied items, which served as a baseline, and 78 filler items. The latter three groups of stimuli were randomly intermixed for each participant. Although including 12 nonstudied primacy buffer items might bias participants not to respond or to reject test items, this would affect memory performance equally across the three rating scenarios. None of the primacy buffer or filler items was presented in the rating phase, so the proportion of *studied* items in the memory test was .35 (i.e., 60/170). Upon presentation of the stem cue, the participants were instructed to type the first word that came to mind (for the implicit group) or an item that they had seen in the rating tasks (for the explicit group). They were asked to respond within 12 sec on each trial, and their RT and accuracy were recorded. At the beginning of the test, the participants in the explicit group were reminded that not all of the stem cues referred to the items they had seen in the rating phases. Both implicit and explicit groups could skip trials by typing XXX if they failed to come up with any answers.

Following Bowers and Schacter (1990), after the experiment, the participants in the implicit group were asked the following questions concerning their test awareness and intentionality to retrieve: (1) "What did you think was the purpose of the stem completion task that you just finished?" (2) "What was your general strategy in completing the word stems?" (3) "Did you notice any relation between the words I showed you earlier and the words produced on the stem-cued completion test?" and (4) "While doing the stem-cued completion test, did you notice whether you completed some of the stems with the words studied on the earlier list?" After completing all these questions, the participants were thanked and debriefed.[1]

Results

Unless otherwise specified, the significance level was set at .05, two-tailed. All analyses were planned in advance to test for a survival-processing advantage. Effect sizes η_p^2 and Cohen's *d* were reported for *F* and *t* statistics, respectively. In general, participants had little difficulty providing the relevant ratings within 5 sec. The mean proportions of unrated words were 2.2% (moving, 1.5%; pleasantness, 2.2%; survival, 2.8%) for the explicit group and 1.8% (moving, 1.6%; pleasantness, 1.8%; survival, 1.9%) for the implicit group. These proportions did not differ across the three rating scenarios (all $ps > .11$, $\eta_p^2\text{s} < .01$). Due to the small number of unrated words and to avoid item selection problems, we used the full set of data in the following analyses. A response on the memory test was scored as correct when it was correctly spelled or matched the grammatically derived forms of the answer (e.g., recalling games for game).

Rating phase. The mean ratings/RTs for moving, pleasantness, and survival scenarios were 2.85/1,742 msec, 3.04/1,802 msec, and 2.91/1,786 msec, respectively, for the explicit group, and 2.68/1,780 msec, 3.05/1,811 msec, and 2.88/1,805 msec, respectively, for the implicit group. There was no interaction associated with group for RTs or ratings [both $Fs(2,476) < 2.13$, $\eta_p^2\text{s} < .01$]. Collapsed across groups, all of the comparisons were significant in ratings (all $ps < .001$, $ds > 0.40$), but not in RTs (all $ps > .24$, $ds < 0.11$). The ratings were higher in the pleasantness scenario than in the survival scenario and were higher in the survival scenario than in the moving scenario.

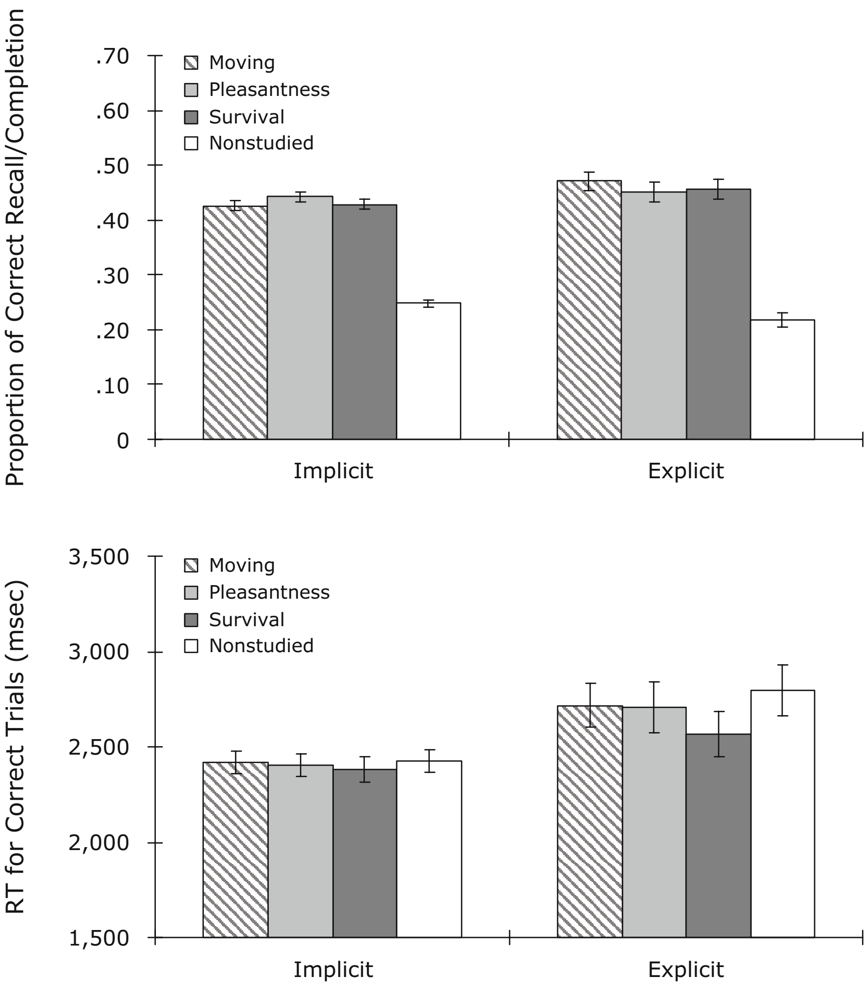

Figure 1. Mean proportions and median response times (RTs) for correct recall/completion for explicit and implicit groups as a function of scenario and group in Experiment 1. Error bars indicate standard errors of the means.

Memory phase. Figure 1 presents the mean proportion of correct recall/completion and median RT for correct trials as a function of scenario and group. The proportion of correct recall for nonstudied items in the explicit group was based on their mere guessing.

For the proportions of correct recall/completion, items rated in the moving, pleasantness, and survival scenarios were recalled or completed better than were nonstudied items in both explicit and implicit groups [all ts(59) or ts(179) $>$ 9.90, ds $>$ 1.80]. However, none of the comparisons among studied items rated in different scenarios approached significance in explicit or implicit groups [all ts(59) or ts(179) $<$ 1.48, ds $<$ 0.16]. For RT, only items rated in the survival scenario yielded shorter RTs than did nonstudied items for the explicit group [$t(59) = 2.12$, $d = 0.38$]. None of the other comparisons with nonstudied items in the explicit or implicit groups was significant [all ts(59) or ts(179) $<$ 1.09, ds $<$ 0.11]. The explicit group responded faster to items rated in the survival scenario than to those rated in the moving scenario [$t(59) = 2.59$, $d = 0.47$] and those rated in the pleasantness scenario [$t(59) = 2.01$, $d = 0.37$], but the RT difference between the latter two conditions was not significant [$t(59) = 0.10$, $d = 0.02$]. None of the RT comparisons was significant for the implicit group [all ts(179) $<$ 0.89, ds $<$ 0.09].

On the basis of Cohen's d (see Cohen, 1988, p. 48, for how it is computed) for the explicit group's within-subjects differences in RT between survival and pleasantness scenarios and between survival and moving scenarios (.37 and .48, respectively), the power to detect similar differences (with $p < .05$, two-tailed) for the implicit group was .92 and .99, respectively, with a sample size of 180. With a cautionary note that the effect size estimated from sample data might have overestimated the effect size observed in the population, the absence of the effects in the implicit group is unlikely to have been due to insufficient statistical power.

Test awareness/intentionality to retrieve in implicit memory. Using their responses to the four questions at the end of the experiment, the participants in the implicit group were divided into three groups on the basis of their test awareness (test aware vs. test unaware) and intentionality to retrieve (intend vs. not intend to retrieve studied items during the test): aware–intend ($n = 34$), aware–unintend ($n = 128$), and unaware–unintend ($n = 18$). Since individuals who were unaware of the nature of the memory test did not claim that they retrieved the studied items on the memory test, there was no unaware–intend participant. The test-aware participants were those who responded "yes" to Questions 3 and/or 4. The participants who were classified as *intending to retrieve* were those who answered on Questions 1 and/or 2 that they studied the items for subsequent tests, thought the purpose of the experiment was to test whether they could remember words presented before, and/or admitted that they did retrieve the studied items during the test. Only the main effect and the interaction associated with group are discussed below. The group (aware–intend, aware–unintend, or unaware–unintend) × scenario interaction was not significant for accuracy [$F(6,531) = 1.85$, $MS_e = 0.01$, $\eta_p^2 = .02$] or for RT [$F(6,531) = 0.33$, $MS_e = 154{,}687$, $\eta_p^2 = .004$]. The main effect of group was significant for accuracy [$F(2,177) = 5.53$, $MS_e = 0.01$, $\eta_p^2 = .06$] but not for RT [$F(2,177) = 0.28$, $MS_e = 543{,}162$, $\eta_p^2 = .003$]. Follow-up analyses on accuracy showed that the unaware–unintend group (.33) yielded lower accuracy than did the aware–unintend group (.39) and the aware–intend group (.40) (both ps $< .01$, ds > 0.36), but there was no difference between the latter two ($p = .45$, $d = 0.08$). Thus, although test awareness boosted overall accuracy, intentionality to retrieve (with the presence of test awareness) did not. Of course, one should interpret these results with caution, due to the small sample sizes in the aware–intend and unaware–unintend groups.

One could argue that the overall accuracy for studied items was quite low for the implicit group (~43%), such that any survival-processing advantage might have been masked. However, this range of accuracy was not unusual in previous studies (e.g., Roediger et al., 1992). Also, an analysis of individuals with above-median accuracy yielded results similar to those for the full set of participants, whether or not test awareness and intentionality were taken into account.

In summary, we replicated the survival-processing advantage in an explicit stem-cued recall test, although the effect was weak and occurred only in RTs. The reason why this effect occurred only in RTs was not clear but was perhaps due to participants' strategies of trading off accuracy at the expense of RT. The weak effect could also be due to the perceptual nature of the explicit memory test; that is, participants might rely on the stem–target orthographic association, rather than the meaning of the targets, in producing their recall responses. This could be tested in future studies by presenting a related cue stem (e.g., *happy–ple*), rather than only the stem (e.g., *ple*) with the study/test items, which would encourage more meaning-based encoding.

Regarding the implicit memory test, although we obtained robust overall priming effects (i.e., relative to nonstudied items), we did not find stronger priming effects for words rated in the survival scenario, relative to those rated in the moving or pleasantness scenarios. This pattern remained the same when test awareness and intentionality to retrieve were taken into account. However, this result could be complicated by the following reasons.

First, because in our explicit stem-cued recall test, the survival-processing advantage was weak, its analogous implicit stem-cued completion test might not have been sensitive enough to detect a survival-processing advantage.

Second, rating concepts (i.e., words) for their relevance in a survival scenario is a meaning-based encoding strategy, whereas completing the word stem with the first word that comes to mind, a perceptual implicit memory test, might not necessarily tap word meaning (see Roediger & McDermott, 1993, for more discussions regarding type of processing in the stem-cued completion test). Since previous studies showed that perceptual implicit memory tests were not sensitive to a levels-of-processing manipulation (e.g., Roediger et al., 1992), it may not be too surprising that relative to other meaning-based encoding strategies (e.g., pleasantness ratings), a survival-processing advantage could not be revealed in a perceptual implicit memory test.

Third, a few participants claimed that they did intentionally retrieve the studied items during the implicit memory test. These claims could be attributed to the long (12-sec) response deadline, as well as the involvement of word production in the stem-cued completion task. Even though the overall pattern of results remained the same after eliminating the 34 test-aware participants who intended to retrieve during the implicit memory test, it is important to use an implicit memory test that may further minimize opportunities to use explicit retrieval strategies.

Fourth, the dissociation between implicit and explicit memory could be attributed to the differential reliability of implicit and explicit memory measures (Buchner & Wippich, 2000). Since the participants could come up with any words, as long as they fit in the stems in the implicit stem-cued completion, the variability of their responses should be higher than that in the explicit stem-cued recall test, in which participants needed to fill out specific words, thus reducing the reliability of the measures. Buchner and Wippich (p. 248) noted that implicit memory tests that require participants to make rapid and restricted responses (e.g., word identification) were more reliable, since this procedure could limit the variety of processes involved in task performance. Hence, a speeded test with only *binary* responses may provide a more reliable implicit memory measure.

To address all these concerns, in Experiment 2, we used a speeded conceptual implicit memory test where participants made binary responses to judge whether or not the test item referred to a concrete concept. We chose this task because (1) we wanted to use the same set of study items as in Experiment 1, in which half of our study items were concrete words and half were abstract words, (2) this task taps the meaning of test items, (3) the speeded nature of this task could minimize opportunities for using explicit retrieval strategies (see, e.g., Hourihan & MacLeod, 2007, for a discussion), and (4) the binary responses (as well as its speeded nature) could boost the reliability of implicit memory measures. To follow as closely as possible the retrieval intentionality criterion (Schacter et al., 1989), we used another explicit memory test, an item recognition test, which was reported to demonstrate the survival-processing advantage in previous research (e.g., Nairne et al., 2007). To render the task demands of the implicit and explicit memory tests more comparable, we used a speeded item recognition test in which participants were instructed to respond as quickly and as accurately as possible.

EXPERIMENT 2

Method

Participants. Three hundred twelve English-speaking undergraduates with normal or corrected-to-normal vision participated in exchange for partial course credit. Ninety-six received an explicit memory test (*explicit* group), and 216 received an implicit memory test (*implicit* group). An equal number of participants in the explicit and implicit groups received each of the 24 counterbalancing lists (see the key assignment counterbalancing procedure, below).

Design, Materials, and Procedure. The design, materials, and procedure were identical to those used in Experiment 1, except that an item recognition task and a concreteness judgment task were used for explicit and implicit memory tests, respectively. The participants in the explicit and implicit groups received the same set of procedures for the rating phase, filler task phase, and the surprise memory test phase, with the only difference being the memory test instruction. In both the explicit and implicit memory tests, the participants were presented with 170 items intact, one at a time, at the center of the screen. Upon the presentation of the item, half of the participants in the implicit group and in the explicit group were instructed to respond by pressing the "l" key to indicate that the word was concrete (for the implicit group) or studied before (for the explicit group) *or* the "s" key to indicate that the word was abstract (for the implicit group) or not studied before (for the explicit group). For the remaining half, the "s" and "l" key assignment was reversed. The participants were asked to respond as quickly and as accurately as they could within 5 sec on each trial, and their RTs and accuracy were recorded. For the implicit group, the test-awareness/intentionality-to-retrieve questions became (1) "What did you think was the purpose of the concreteness judgment task that you just finished?" (2) "What was your general strategy in judging word concreteness?" (3) "Did you notice any relation between the words I showed you earlier and the words presented on the concreteness judgment test?" and (4) "While doing the concreteness judgment test, did you notice whether you saw some of the words studied in the earlier list?"

Results

The analytic procedure was the same as that in Experiment 1. The mean proportions of unrated words were 1.2% (moving, 0.9%; pleasantness, 1.2%; survival, 1.6%) for the explicit group and 1.2% (moving, 1.1%; pleasantness, 1.2%; survival, 1.4%) for the implicit group. The proportion of unrated words was very low and did not differ across the three rating scenarios (all ps $> .08$, ds $<$ 0.24). Hence, we used the full set of data in the following analyses.

Rating phase. The mean ratings/RTs for the moving, pleasantness, and survival scenarios were 2.76/1,939 msec, 3.08/1,908 msec, and 2.80/1,983 msec, respectively, for the explicit group, and 2.89/1,799 msec, 3.08/1,779 msec, and 2.96/1,852 msec, respectively, for the implicit group. There was no interaction associated with group for RT or ratings [both Fs(2,620) < 2.75, η_p^2s $< .01$]. Collapsed across groups, all of the comparisons were significant for ratings (all ps $< .05$, ds > 0.15), but not for RTs (all ps $> .10$, ds < 0.10). The ratings were higher for the pleasantness scenario than for the survival scenario and, in turn, the moving scenario, yielding a pattern similar to that in Experiment 1.

Memory phase. Figure 2 presents the mean proportion of correct judgment/*old* responses and median RT for corrected trials/*old* responses as a function of scenario and group. The *old* responses for nonstudied items in the explicit group, referred to as a *false alarm rate*, could not be compared with the *old* responses for studied items (i.e., hit rates) in the three rating scenarios. However, given that the participants' responses to the test items in the concreteness judgment task should be the same whether the items were studied or nonstudied, the proportion of correct judgment and correct trial RT of nonstudied items could be compared with those of studied items in the three rating scenarios and serve as a baseline to measure the priming effect.

For accuracy, items rated in moving, pleasantness, and survival scenarios were judged better than were nonstudied items in the implicit group, showing robust priming effects [all ts(215) > 2.20, ds > 0.21]. Regarding the comparisons among studied items rated in different scenarios, whereas for the implicit group none of the comparisons approached significance [all ts(215) $<$ 1.04, ds < 0.10], for the explicit group, items rated in the survival scenario yielded higher hit rates than did those rated in the pleasantness [$t(95) = 2.73$, $d = 0.39$] and moving [$t(95) = 3.04$, $d = 0.44$] scenarios, and the hit rate difference in the latter two conditions was not significant [$t(95) = 0.49$, $d = 0.07$]. (The signal detection measures such as d' that take into account the false alarm rates for nonstudied items could be computed. See Figure 2 for the proportion of *old* responses for nonstudied items. However, because the rating scenarios were manipulated within subjects, the analyses of d's are functionally identical to the analyses of hit rates, given that the same z-transformed false alarm rate was subtracted from the z-transformed hit rates for various rating scenarios.)

For RT, items rated in moving, pleasantness, and survival scenarios were judged faster than nonstudied items in the implicit group, again showing robust priming effects [all ts(215) > 2.56, ds > 0.25]. Regarding the comparisons among studied items rated in the different scenarios, whereas for the implicit group none of the comparisons approached significance [all ts(215) < 1.06, ds < 0.10], for the explicit group, items rated in the survival scenario yielded faster hit responses than did those rated in the pleasantness scenario [$t(95) = 4.71$, $d = 0.68$] and those rated in the moving scenario [$t(95) = 5.74$, $d = 0.83$], and the hit response RTs in the latter two conditions did not differ [$t(95) = 1.25$, $d = 0.18$]. Thus, the overall pattern duplicated that of the accuracy data.

On the basis of *smallest* Cohen's d among the explicit group's within-subjects differences in accuracy and in RT between the survival and pleasantness scenarios and between the survival and moving scenarios (i.e., .39), the power to detect similar differences (with $p < .05$, two-

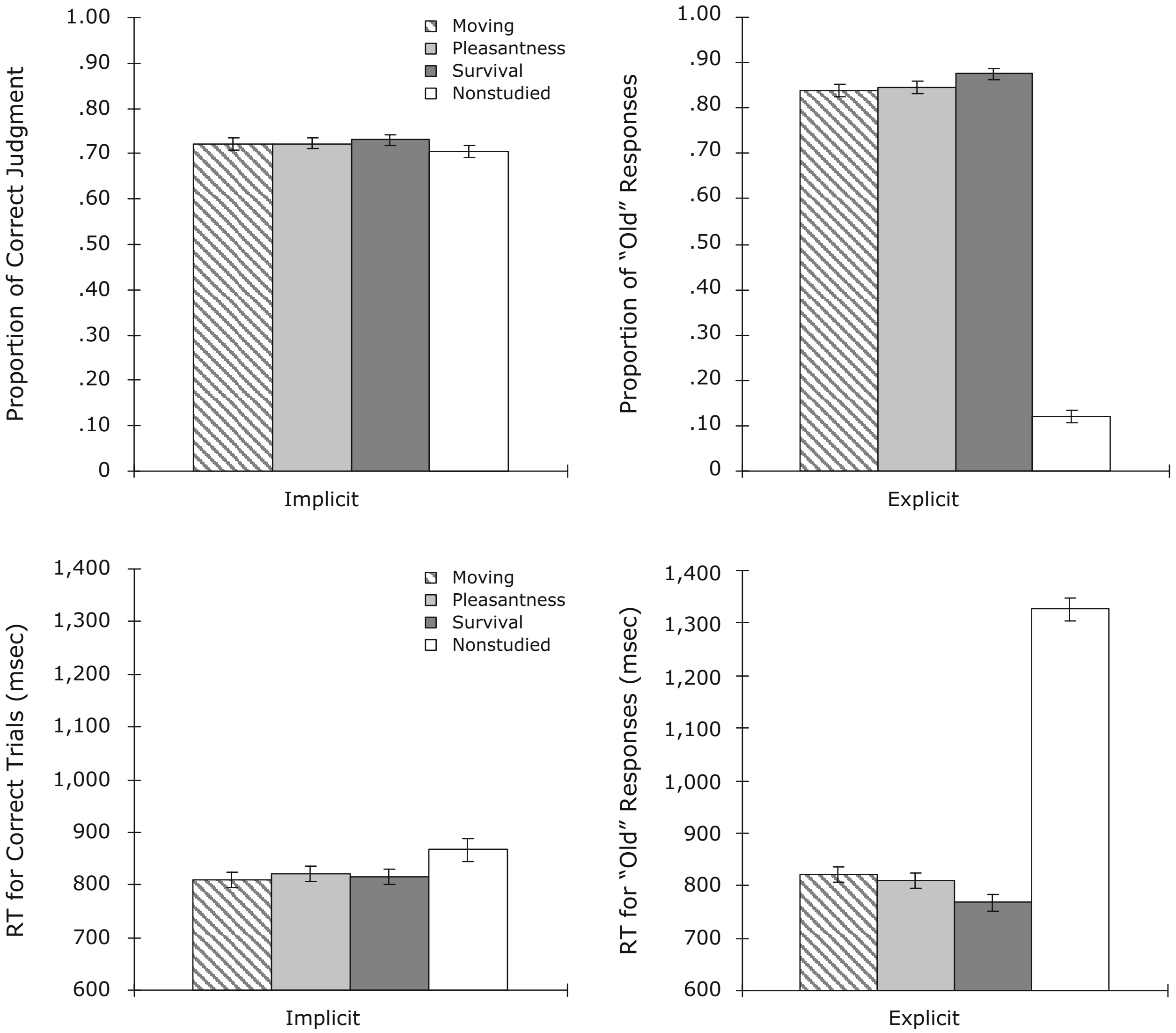

Figure 2. Mean proportions and median response times (RTs) for correct judgments for the implicit group and *old* responses for the explicit group as a function of scenario and group in Experiment 2. Error bars indicate standard errors of the means. Some participants did not produce any false alarms, so RTs for the *old* responses for nonstudied test items were based on 84 participants only.

tailed) for the implicit group was >.98, with a sample size of 216. Hence, the absence of the effects in the implicit group is unlikely to have been due to insufficient statistical power.

Test awareness/intentionality to retrieve in implicit memory. As in Experiment 1, we classified participants in the implicit group into three groups on the basis of their test awareness and intentionality to retrieve: aware–intend ($n = 3$), aware–unintend ($n = 201$), and unaware–unintend ($n = 12$). The speeded nature of the concreteness judgment task did discourage the participants from intentionally retrieving the studied items, relative to the stem-cued completion task, where the participants were given a 12-sec response deadline. In contrast to 34 out of 180 participants (18.9%) in Experiment 1's stem-cued completion task, only 3 out of 216 participants (1.4%) reported that they intentionally retrieved the studied items in the concreteness judgment task. The group (aware–intend, aware–unintend, or unaware–unintend) × scenario interaction was not significant for accuracy [$F(6,639) = 1.00$, $MS_e = 0.01$, $\eta_p^2 = .10$] or for RT [$F(6,639) = 0.48$, $MS_e = 22,806$, $\eta_p^2 = .004$], nor was the main effect of group for accuracy [$F(2,213) = 0.52$, $MS_e = 0.03$, $\eta_p^2 = .005$] or RT [$F(6,639) = 0.85$, $MS_e = 173,967$, $\eta_p^2 = .008$]. Thus, neither test awareness nor intentionality to retrieve enhanced participants' RT or accuracy in the concreteness judgment task, although these results should be interpreted with caution, due to the very small sample size in the aware–intend and the unaware–unintend groups. (The pattern of results remained the same in the overall analyses after taking out the 3 test-aware participants who intended to retrieve during the implicit memory test.)

In summary, using an explicit item recognition test, we now obtained a typical survival-processing advantage in both accuracy and RT. However, we again did not obtain any survival-processing advantages (relative to other conditions—e.g., rating words in the pleasantness or moving scenario) in the implicit memory test, despite the fact that we did find a robust priming effect (relative to the nonstudied items). Because the conceptual implicit memory test tapped the meaning of the test items, even in this optimal task condition survival processing still did not boost implicit memory performance, as opposed to its robust effect on explicit memory performance.[2]

GENERAL DISCUSSION

To our knowledge, the present study is the first to examine whether a survival-processing advantage, which has been reported in free recall and item recognition, would generalize to another explicit memory task (i.e., stem-cued recall), a stem-cued completion task that reflects perceptual implicit memory, and a concreteness judgment task that reflects conceptual implicit memory. We examined both RT and (typical) accuracy measures to test whether a survival-processing advantage could also occur in RT data. The findings are straightforward. Across two experiments, we did not find any survival-processing advantage in perceptual (stem-cued completion) or conceptual (concreteness judgment) implicit memory tests. This was so even when we found robust priming effects (relative to nonstudied items) in these two tests and replicated the survival-processing advantage in their analogous explicit memory tests (despite being quite weak in the stem-cued recall task) that were designed to follow Schacter et al.'s (1989) retrieval intentionality criterion.[3]

The Effect of Initial Ratings on the Survival-Processing Advantage

Butler, Kang, and Roediger (2009, Experiments 2 and 3) reported that the survival-processing advantage disappeared in an explicit free recall test when participants were asked to rate survival-related items in a robbery scenario and robbery-related items in a survival scenario. This result echoes the findings in memory research that participants remember items better if those items are congruent with the way in which they are processed (cf. Craik & Tulving, 1975) and suggests that the survival-processing advantage reported in prior studies could, in part, be mediated by the congruency of the study items with the survival-related concept. In the present experiments, to rule out the possibility that a survival-processing advantage could be explained solely by the items' being more relevant to the survival scenario, we controlled semantic similarity, as quantified by LSA cosines, between the study items and the concepts of survival/moving (see Table 1). The items were also rated as being more relevant in the pleasantness scenario than in the survival scenario, suggesting that the survival-processing advantage, if any, in explicit and implicit memory could not be attributed entirely to a congruity effect (Schulman, 1974) that would predict that memory performance would be highest for items rated in the pleasantness scenario. The congruity effect refers to the finding that words are remembered better when the answer to the initial encoding question is *yes* than when it is *no*. However, one could still ask whether our explicit memory data could replicate Butler et al.'s Experiment 1 effect of initial relevance ratings *within* each scenario. They showed that participants better recalled words that were judged to be more relevant (i.e., higher initial ratings), whether the scenario was survival, moving, or pleasantness. More important, in our experiments, implicit memory might have been enhanced by survival processing when the words were rated very high in relevance in the survival scenario, and thus, a genuine survival-processing advantage on implicit memory might have been masked. To address all these issues, we followed Butler et al.'s Experiment 1 procedure and recoded participants' accuracy and RT in the explicit and implicit memory tests as a function of their relevance ratings in each of the three scenarios. About 30%–40% of the participants in the explicit and implicit groups did not use the entire rating scale and might have had missing cells in at least one of the rating levels. To avoid excessive removal of data that could distort the pattern of our results, we randomly combined the raw data of 2 participants, within each counterbalancing list, into 1 superparticipant (i.e., $n = 30$ and 90 for implicit and explicit groups in Experiment 1 and $n = 48$ and 108 for implicit and explicit groups in Experiment 2) before we computed their conditionalized data.

The explicit and implicit memory data in Experiments 1 and 2 were separately submitted to 5 (initial rating: 1–5) × 3 (scenario: moving, pleasantness, or survival) repeated measures ANOVAs. Because the items in the three scenarios were different at each level of rating scale, this potential item selection problem complicated the comparison among the data in the three scenarios (see also Butler et al., 2009). Thus, we focus on the linear trend effects of initial ratings on memory performance overall and within each scenario to test whether we replicated previous results and then briefly discuss the comparison of cell means for the three scenarios (see Table 2).

For explicit memory RT data, the omnibus test of linear trends for initial rating was not significant in Experiment 1 [$F(1,29) = 3.09$, $MS_e = 603{,}881$, $\eta_p^2 = .10$] or Experiment 2 [$F(1,47) = 0.56$, $MS_e = 51{,}884$, $\eta_p^2 = .01$]. Despite nonsignificance in the omnibus test, a closer look at Experiment 1's data showed that only the effect of initial ratings on items rated in a pleasantness scenario was significant [$F(1,29) = 7.75$, $MS_e = 473{,}029$, $\eta_p^2 = .21$], but not the effect of those rated in the survival or moving scenario [both $Fs(1,29) < 2.50$, $\eta_p^2s < .08$]. This result indicates that the participants were faster to remember the words as a function of initial ratings in the pleasantness scenario, but not in the moving or survival scenarios.

The trends look clearer in stem-cued recall accuracy and recognition hit rates. The overall effect of initial ratings was significant in Experiments 1 [$F(1,29) = 15.64$, $MS_e = 0.07$, $\eta_p^2 = .35$] and 2 [$F(1,47) = 25.69$, $MS_e = 0.02$, $\eta_p^2 = .35$]. In Experiment 1, the effect of initial ratings was significant for items rated in the pleasantness [$F(1,29) = 5.67$, $MS_e = 0.06$, $\eta_p^2 = .16$] and moving [$F(1,29) = 8.95$, $MS_e = 0.07$, $\eta_p^2 = .24$] scenarios, but not in the survival

Table 2
Mean Proportions of Correct Recall/Completion/Recognition Hit Rates/ Concreteness Judgments and Median Response Times (RTs, in Milliseconds) for Corrected Trials As a Function of Initial Ratings, Scenario, and Group in Experiments 1 and 2

	Initial Rating				
	1	2	3	4	5
Experiment 1's Explicit Group					
Stem-Cued Recall Rate					
Pleasantness scenario	.37	.43	.45	.48	.50
Moving scenario	.40	.42	.52	.53	.57
Survival scenario	.41	.46	.47	.48	.52
Stem-Cued Recall RT					
Pleasantness scenario	3,045	3,087	2,753	2,701	2,685
Moving scenario	2,855	3,083	3,064	2,507	2,779
Survival scenario	2,429	2,691	2,586	2,596	2,710
Experiment 1's Implicit Group					
Stem-Cued Completion Rate					
Pleasantness scenario	.51	.53	.40	.43	.41
Moving scenario	.44	.46	.39	.39	.41
Survival scenario	.43	.50	.41	.40	.41
Stem-Cued Completion RT					
Pleasantness scenario	2,453	2,481	2,659	2,416	2,522
Moving scenario	2,528	2,497	2,563	2,540	2,433
Survival scenario	2,451	2,278	2,463	2,390	2,524
Experiment 2's Explicit Group					
Recognition Hit Rate					
Pleasantness scenario	.79	.85	.85	.87	.88
Moving scenario	.83	.84	.85	.85	.91
Survival scenario	.83	.85	.89	.89	.91
Recognition Hit RT					
Pleasantness scenario	837	797	841	807	766
Moving scenario	843	802	841	790	834
Survival scenario	737	814	737	787	763
Experiment 2's Implicit Group					
Concreteness Judgment Accuracy					
Pleasantness scenario	.74	.73	.74	.72	.66
Moving scenario	.73	.70	.75	.69	.76
Survival scenario	.75	.74	.70	.72	.73
Concreteness Judgment RT					
Pleasantness scenario	917	928	934	872	890
Moving scenario	863	942	868	898	846
Survival scenario	851	905	845	864	856

scenario [$F(1,29) = 1.72$, $MS_e = 0.10$, $\eta_p^2 = .06$]. Stem-cued recall accuracy increased as a function of initial ratings in the pleasantness and moving scenarios. Despite being nonsignificant, the trend did occur in the survival scenario (see Table 2). In Experiment 2, despite the near-ceiling performance, the effect of initial ratings was significant in all scenarios [pleasantness, $F(1,47) = 9.14$, $MS_e = 0.02$, $\eta_p^2 = .16$; moving, $F(1,47) = 4.68$, $MS_e = 0.03$, $\eta_p^2 = .09$; survival, $F(1,47) = 12.48$, $MS_e = 0.01$, $\eta_p^2 = .21$]. The recognition hit rates increased as a function of initial ratings in all three scenarios. Hence, despite using different tasks (stem-cued recall and item recognition vs. free recall), we replicated the effect of initial ratings on explicit memory in Butler et al.'s (2009) Experiment 1.

For the implicit memory data, in Experiment 1, none of the effects of initial rating was significant for RT [all Fs$(1,89) < 1.30$, η_p^2s $< .01$], whereas the overall effect of initial ratings was significant for word completion rates [$F(1,89) = 14.83$, $MS_e = 0.06$, $\eta_p^2 = .14$]. A closer inspection showed a significant effect in the pleasantness scenario [$F(1,89) = 18.07$, $MS_e = 0.04$, $\eta_p^2 = .17$], but not in the survival scenario [$F(1,89) = 3.42$, $MS_e = 0.04$, $\eta_p^2 = .04$] or the moving scenario [$F(1,89) = 1.48$, $MS_e = 0.09$, $\eta_p^2 = .02$]. However, the word-stem-cued completion rates in the pleasantness scenario actually *decreased* as a function of initial ratings, and a similar, albeit nonsignificant, trend was found for the word-stem-cued completion rates in the survival and moving scenarios. This was contrary to the explicit memory data, although the decrease was mostly between the ratings of 2 and 3. Nonetheless, this pattern was not replicated in Experiment 2, when a conceptual implicit memory test was used. The overall effect of initial ratings was not significant for accuracy [$F(1,107) = 2.57$, $MS_e = 0.04$, $\eta_p^2 = .02$] or for RT [$F(1,107) = 3.44$, $MS_e = 50{,}173$, $\eta_p^2 = .03$]. None of the effects within the individual scenarios was significant for accuracy or RT [all Fs$(1,107) < 1.79$, η_p^2s $< .02$], except the accuracy for items rated in the pleasantness scenario [$F(1,107) = 8.19$, $MS_e = 0.04$, $\eta_p^2 = .07$]. However, closer inspection of the data showed that the decrease in accuracy in the pleasantness scenario was mostly between the ratings of 4 and 5. Contrary to the clear trend in accuracy in explicit memory tests, the effects of initial ratings on implicit memory were unsystematic. We also analyzed the implicit memory data by combining participants on the basis of their test awareness and intentionality to retrieve, but the overall pattern did not interact with these variables.

Keeping in mind that there could be an item selection confound, in implicit memory tests we did not find a systematic superiority of items rated in the survival scenario, relative to those rated in the pleasantness and moving scenarios across five levels of initial ratings (see Table 2). Of the 40 pairwise comparisons for survival versus pleasantness/moving scenarios on all levels of initial ratings in all implicit memory measures in two experiments, we found 34 null, 5 positive, and 1 negative significant survival-processing advantages. The five positive effects did not lie on any particular level of initial rating: two at the rating of 3 and one each at the ratings of 2, 4, and 5. Thus, it is safe to conclude that it is unlikely that the absence of a survival-processing advantage in implicit memory might be due to the masking of higher initial relevance ratings. Future research needs to adapt Butler et al.'s (2009) Experiments 2 and 3 manipulations (i.e., rating survival-related items in a robbery scenario and robbery-related items in a survival scenario) in an implicit memory test to examine even further whether a survival-processing advantage would emerge when survival-related items were rated in a survival scenario.

Theoretical Implications of the Present Findings

One of the characteristics of evolved memory mechanisms (see Nairne, 2005; Nairne & Pandeirada, 2008b) was that memory should be tuned to remember certain kinds of domain-specific information that is relevant to survival/fitness. It could be argued that a flexible and adaptive memory system should not depend on retrieval intentionality, because many advantages that a memory system confers to an organism are more primitive in nature and less dependent on higher order processing. An organism can be benefited by the retrieval of previous survival-relevant episodes, independently of whether retrieval is explicit or not. For example, one may refuse to eat certain foods without intentionally remembering prior experiences of nausea related to them (e.g., stomachache). Hence, the strong view about memory evolution could be that a truly adaptive memory system should rely on prior episodes even in the absence of explicit retrieval. The finding that a survival-processing advantage occurred only in explicit memory, but not in implicit memory, seems to contradict, or at least call for modification of, this strong view. Although previous studies used, for instance, category verification (Mulligan & Peterson, 2008) and animacy decision (Zeelenberg & Pecher, 2003), but not concreteness judgment, as their conceptual implicit memory tests, the robust overall priming effect (i.e., relative to nonstudied items) observed in the concreteness judgment task clearly indicates its sensitivity. A similar point can also be made for the stem-cued completion task, due to the significant overall priming effects. In addition, the statistical powers for detecting potential priming differences between survival and moving/pleasantness scenarios were all higher than .90. Hence, the lack of a survival-processing advantage in implicit memory tests should not likely be due to the insensitivity of our tasks or insufficient statistical power. It could be that some selection pressure in our ancestral past may have led to the specificity of a survival-processing advantage in explicit memory, but obviously this idea should be tested further in future studies.

Although an absence of a survival-processing advantage in implicit memory may not be straightforwardly explained by the strong view of memory evolution, the null effect, particularly in the concreteness judgment task, might suggest that the survival-processing advantage that occurred in explicit memory could not be attributed merely to the deep processing that is induced via rating the study items in a survival scenario. However, this idea should be further validated in conceptual implicit memory tests that have been reported to yield the levels-of-processing effect (e.g., a modified free association task in Hourihan & MacLeod, 2007). If a survival-processing advantage still did not occur for those tests, one would be more confident that the survival-processing advantage in explicit memory was not just another levels-of-processing effect.

Before concluding the present study, it is important to consider two more alternative explanations for our findings. First, the lack of a survival-processing advantage in a conceptual implicit memory test could also be attributed to the nature of the task. Because participants only need to identify whether or not the test item refers to the concrete objects, the notion that their performance may not depend on the "availability" of the item in memory might make this task insensitive to detect any survival-processing advantage, despite its being conceptual in nature. However, given that *item availability*, or the productive nature of the memory test, is critical in detecting a survival-processing advantage, it is not clear why the advantage can be found in explicit item recognition, which also requires the participants to identify only the study status of test items. (Of course, some could postulate that recall-like processes are probably involved in the explicit item recognition test [e.g., Yonelinas, 2002].) Future researchers should use a conceptual implicit memory test that is *productive* in nature (e.g., category production task in Lee, 2008, and Srinivas & Roediger, 1990) and its analogous explicit memory test, in order to test whether a survival-processing advantage in implicit memory could be revealed.

Second, one could argue that the absence of a survival-processing advantage in implicit memory was due to a response mismatch between the study phase (rating the items in various scenarios) and the test phase (completing the word stem or judging word concreteness). On the basis of transfer-appropriate processing (Roediger, 1990), the reinstatement of prior cognitive operations should boost the priming effect, and the match between encoding and retrieval operations can be quite domain specific. For example, Pilotti, Gallo, and Roediger (2000) showed that change in voice in auditory presentation between study and test reduced priming. Future studies should test whether a survival-processing advantage could occur in implicit memory when the task demand is entirely identical at study and at test (e.g., decide whether items are relevant in a survival scenario *both* at study and at test; see Hughes & Whittlesea, 2003, for a similar design).

Conclusion

In two experiments, we found that a survival-processing advantage occurs when the tasks require the intentionality to retrieve (e.g., retrieving a studied item to complete a word stem or judging whether the item was studied before), but not when they did not require the intentionality to retrieve (e.g., filling out a stem cue with the first word that comes to mind or judging whether the word refers to a concrete object). These findings demonstrate that under some situations, survival processing may not necessarily boost memory performance, relative to other encoding strategies (e.g., pleasantness rating). The survival-processing advantage should be further tested using other perceptual and conceptual implicit memory tests in future studies in order to generalize the present findings and to examine whether the advantage in explicit memory could be attributed solely to the deep processing triggered by the relevance ratings in the survival scenario.

AUTHOR NOTE

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NOTES

1. We collected data regarding participants' experiences watching survival-related TV programs by asking them, "How often do you watch survival-related television programs (e.g., *Survivor*, *Lost*)?" at the end of the experiment. Their responses were categorized into four levels (never, sometimes, frequently, and always). However, this variable did not interact with scenario in RT or accuracy for explicit and implicit groups in either experiment.

2. Rudine et al. (2009) found that relative to self-referential and pleasantness ratings, survival ratings enhanced memory for concrete nouns, but not for items that describe personality traits, and argued that survival processing may be effective only for concrete words, but not for abstract words. However, post hoc analyses for the present data showed that word type (concrete vs. abstract) did not interact with any variables, including the type of memory test (explicit vs. implicit) in both experiments (all Fs < 1.04), although it should be noted that in our abstract word pool, very few items referred to personality traits (see the Appendix).

3. The present study is not intended to tease apart or test any one of the extant implicit memory theories (e.g., *process* vs. *system* view). According to the perspective of transfer-appropriate processing (Roediger, 1990), priming effects should be greater when the overlap in cognitive operations at study and at test becomes larger. Given that the rating tasks involved the processing of word meaning, one would expect a larger priming effect in a conceptual implicit memory test (Experiment 2) than in a perceptual implicit memory test (Experiment 1). However, these two implicit memory tests differed not only in the type of processing (perceptual vs. conceptual), but also in the response mode: Stem-cued completion was a production test, and concreteness judgment was an identification test. Therefore, the data from these two tests may not be directly comparable.

APPENDIX
Stimulus Lists in Experiments 1 and 2 and Their Stem Cues in Experiment 1

Critical Items

ambition–amb	circus–cir	garbage–gar	messy–mes	scream–scr
angry–ang	color–col	greed–gre	modest–mod	serious–ser
basket–bas	cottage–cot	guilty–gui	moment–mom	spouse–spo
beach–bea	cruel–cru	health–hea	moral–mor	stupid–stu
bench–ben	death–dea	holiday–hol	mosquito–mos	swamp–swa
black–bla	devil–dev	hospital–hos	mother–mot	talent–tal
bored–bor	diamond–dia	hotel–hot	movie–mov	tennis–ten
bouquet–bou	dirty–dir	hungry–hun	music–mus	trophy–tro
brave–bra	dress–dre	infant–inf	offend–off	trust–tru
breeze–bre	engine–eng	journal–jou	panic–pan	twilight–twi
bride–bri	excuse–exc	legend–leg	queen–que	vacation–vac
building–bui	fabric–fab	lonely–lon	quick–qui	vanity–van
cabinet–cab	failure–fai	machine–mac	reptile–rep	virtue–vir
candy–can	finger–fin	manner–man	ridicule–rid	weapon–wea
cellar–cel	flower–flo	material–mat	rigid–rig	whistle–whi
church–chu	friend–fri	medicine–med	rough–rou	window–win

Primacy Buffer Items and Filler Items in the Memory Test

abuse–abu	divorce–div	justice–jus	poverty–pov	sugar–sug
adult–adu	dollar–dol	kindness–kin	prairie–pra	sunset–sun
agony–ago	elbow–elb	knife–kni	prison–pri	swift–swi
alive–ali	fantasy–fan	letter–let	puppy–pup	taste–tas
alone–alo	fault–fau	lottery–lot	rabbit–rab	terrific–ter
answer–ans	field–fie	luxury–lux	rainbow–rai	thief–thi
autumn–aut	filth–fil	melody–mel	razor–raz	tobacco–tob
blossom–blo	fungus–fun	mountain–mou	reward–rew	travel–tra
bottle–bot	glory–glo	nasty–nas	river–riv	triumph–tri
bullet–bul	grief–gri	noisy–noi	robber–rob	unhappy–unh
butter–but	happy–hap	ocean–oce	sapphire–sap	vehicle–veh
cliff–cli	highway–hig	option–opt	secure–sec	violent–vio
coast–coa	honey–hon	patient–pat	skull–sku	warmth–war
corpse–cor	house–hou	pencil–pen	snake–sna	water–wat
crime–cri	illness–ill	perfume–per	sphere–sph	woman–wom
dagger–dag	immoral–imm	pillow–pil	spring–spr	world–wor
delight–del	injury–inj	pizza–piz	statue–sta	yacht–yac
dinner–din	jelly–jel	pleasure–ple	stink–sti	young–you

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