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# Stuck in the past? Rumination-related memory integration

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ABSTRACT

Memories connected to ruminative concerns repetitively capture attention, even in situations designed to alter them. However, recent research on memory updating suggests that memory for benign substitutes (e.g., reinterpretations) might be facilitated by integration with the ruminative memories. As a first approach, two experiments (*Ns* = 72) mimicked rumination-related memories with rumination-themed stimuli and an imagery task. College undergraduates screened for ruminative status first studied and imaged ruminative cue-target word pairs, and then in a second phase they studied the same cues re-paired with benign targets (along with new and repeated pairs). On the test of cued recall of benign targets, they judged whether each recalled word had been repeated or changed across the two phases (or was new in the second phase). When target changes were not remembered, recall of benign targets revealed proactive interference that was insensitive to ruminative status. However, when participants remembered change and the ruminative targets, their recall of benign targets was facilitated, particularly if they identified as ruminators (Experiment 1). When the test simply asked for recall of either or both targets (Experiment 2), ruminators recalled both targets more frequently than did others. These outcomes suggest that ruminative memories might provide bridges to remembering associated benign memories, such as reinterpretations, under conditions consistent with everyday ruminative retrieval.

People who ruminate mentally chew their unresolved concerns, repetitively and perseveratively, along with the negative feelings and memories associated with them (see Watkins & Roberts, 2020). These informal observations are supported by experimental research on rumination; people who ruminate cannot readily put aside such "sticky" thoughts and memories (Joormann et al., 2011). Indeed, negative life events that spawn rumination are better recalled subsequently (Connolly & Alloy, 2018). Moreover, memories associated with ruminative concerns seem to dominate retrieval and interfere with conceiving and remembering more helpful contextually similar events, such as counterfactual interpretations offered in therapy or benign episodes connected to the same contextual cue. This power to interfere clearly poses challenges to therapeutic attempts.[1] In the present experiments, we used rumination-themed materials and imagery instructions as stand-ins for participants' ruminative memories, to make it possible to experimentally investigate whether the sticky memories that pepper rumination are bound to interfere or whether their perhaps inevitable retrieval might be useful in establishing better recall of related benign events. Before explaining the rationale for the latter alternative, we consider evidence regarding the stickiness of ruminative memories.

First, ruminative memories[2] can initiate as negative biases during attention and working-memory tasks (e.g., Grafton et al., 2016; Joormann et al., 2011; Sanchez-Lopez et al., 2019). In addition, ruminative interpretation biases (major producers of memory biases; see Hertel & Brozovich, 2010) respond inflexibly to modification attempts (Everaert et al., 2018, 2020). In general, ruminative recall biases—experimental and autobiographical—are well documented, as are a few successful attempts at modification (reviewed by LeMoult & Gotlib, 2019; Watkins

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[1] The idea that activation of rumination-related memories interferes with therapeutic interventions (the perseverative cognition hypothesis, Brosschot et al., 2006) is supported empirically (see Watkins & Roberts, 2020).

[2] Throughout this report, we use the term ruminative memory to represent both actual memories connected to ruminative episodes and memory for the rumination-themed experimental targets. We rely on the context to identify the relevant meaning.

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& Roberts, 2020). However, other modification attempts have produced clear evidence of stickiness. For example, in retrieval-practice experiments designed to simulate or modify repetitive retrieval in rumination, ruminators recalled as many unpracticed words from negative contexts as the positive-context words that were practiced as simulation of an intervention (Hertel et al., 2017; cf. Visser et al., 2019). Also, delayed recall by ruminators benefited less well from the prior practice of positive autobiographical memories (Hertel et al., 2021). From a different view of sticky memories, ruminators have shown deficient suppression-induced forgetting on indirect tests (Hertel et al., 2018). Frameworks for understanding ruminative cognition, emphasizing impaired cognitive control, well strengthened habits, and motivational factors, are consistent with difficulties in memory modification (see Hertel, 2004; Koster et al., 2011; LeMoult and Gotlib, 2019; Watkins & Nolen-Hoeksema, 2014; Watkins & Roberts, 2020).

The process of changing sticky ruminative memories finds a useful analogy in the continued influence effect (CIE; Johnson & Seifert, 1994; see Chang et al., 2019). Just as corrections are intended to update misinformation, reinterpretations or benign substitutes are offered with the intent to compete with retrieval of sticky memories; in each of these instantiations of classic interference paradigms, the first-encountered experience often prevails.[3] In fact, however, there are small modification successes, several of which have been achieved via the provision of misinformation reminders at the point of correction (Ecker et al., 2017; Wahlheim et al., 2020). More generally, integrative-encoding frameworks incorporate the assumption that if existing memories are retrieved during the encoding of related events, their successful binding in memory will produce elaborated representations that facilitate rather than interfere with subsequent recollection of more recent events (Bauer, Cronin-Golomb, Porter, Jaganjac, & Miller, 2021; Ecker et al., 2022; Wahlheim et al., 2021). Based on this evidence, we envisioned integration as an adaptive use of ruminative memories. Their unintentional but often frequent recall might serve as a bridge to remembering more adaptive associates of the cuing situation. To implement this analogy, we developed versions of a paired-associate learning paradigm used to examine proactive effects of existing memories (often represented as A-B, A-D—vs. an A-D control—followed by cued-recall testing). We then performed two simulation experiments to test hypotheses about whether ruminative memories simply interfere with or possibly facilitate recall of subsequently experienced benign events that share the same cues.

## 1. Experiment 1

The analogy with the continued influence of unwanted memories highlights two possible patterns regarding the modification of ruminative memories. First, ruminators might not attend as well to benign changes because they focus more on the sticky past (and, more generally, rehearsing existing memories distracts attention from changes, Arkes & Lyons, 1979). In Experiment 1, this sticky-past hypothesis predicts that ruminators will show greater proactive interference in the form of poorer recall of the benign changes and more intrusions from (experimental) ruminative targets. Alternatively, according to the integration hypothesis, the benign changes might be especially noticeable to ruminators because the rumination-themed target is easily brought to mind by the same cue that is being learned with the new benign target. Ruminators may be more sensitive to connections between the alternative targets—connections that provide a basis for integration (see Chanales et al., 2019). To illustrate: The *team*-centered *argument* becomes a *team* focused on a *goal*; the *homework* that caused *stress* is now the *homework* that gains *approval*. In short, the first pattern that might set ruminators apart would be stickiness expressed as exaggerated proactive interference; the second pattern would be greater integration that proactively facilitates memory for the change.

### 1.1. Method

#### 1.1.1. Transparency

This experiment was preregistered on the Open Science Framework, with updates, for design and analyses: https://osf.io/u4da7/.[4] The materials, data, and code can be found at https://osf.io/vyzuf/. We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. Data collection was preapproved by the Trinity University Institutional Review Board.

#### 1.1.2. Materials and design

Thirty-six triplets comprised the primary materials. Each triplet consisted of one cue and two targets, one assigned to represent ruminative concerns in Phase-1 and the other a more benign concept in Phase-2 (e.g., *body-shame* and *body-comfort, family-collapse* and *family-reunion*). We created these triplets by trying to capture possible student concerns and experiences. Pairs assigned to Phase 1 were intended to invoke thoughts of emotion-laden problems possibly involved in ruminative episodes rather than merely negative concepts. However, to simplify, we label the targets in these pairs as negative.

To ensure that not all pairs in the first learning phase were emotionally negative (a situation that could invite confounding strategies), we selected an additional 36 triplets from materials used in previous experiments on episodic memory integration (e.g., *walnut-almond* and *walnut-squirrel, soup-bowl* and *soup-sandwich;* Wahlheim, 2015). The inclusion of these typically nonemotional items also served as a check on whether our method could closely replicate previously published evidence for proactive memory effects. We refer to the two different sets of materials as ruminative and nonemotional.

The role enacted by triplets with respect to the two learning phases served as a within-subjects factor in the design (and illustrated in Table 1). Either the Phase-1 cue-target pairs were repeated in Phase 2 (as a second means to prevent the use of valence as a retrieval strategy on the final test), the negative targets were changed in Phase 2 to their corresponding benign targets, or the cues appeared for the first time in Phase 2 with benign targets (to serve as the control condition for assessing interference and facilitation). Thus, within the ruminative triplets, Phase-1 pairs were negative and Phase-2 pairs were benign, except in the repeated condition. (In the case of nonemotional triplets, assignment of targets to both phases was also fixed.)

**Assignment to sets for counterbalancing.** To assign triplets to experimental roles within each triplet type, we first distributed them into three sets of 12 and balanced the sets on cue- and target-word frequencies and concreteness (Brysbaert et al., 2014), number of letters (ranging from 4 to 8), and both emotional valence and arousal

**Table 1**
Design illustration.

| Target role | Phase 1 | Phase 2 | Test |
|---|---|---|---|
| Repeated | body-shame (negative) | body-shame (negative) | body - ? |
| Changed | family-collapse (negative) | family-reunion (benign) | family - ? |
| New | – | team-goal (benign) | team - ? |

*Note.* Emotional status appears in parentheses.

[3] Consider the power of the first-learned event in clinically relevant contexts (Bouton, 2000).

[4] Through oversight, preregistration occurred after data collection began; however, data had not been examined.

(Warriner et al., 2013).[5] The balanced sets were fully counterbalanced with their experimental role as repeated, changed, and new pairs. (We also balanced the nonemotional sets on the forward and backward association strength of both cue-target pairs in each triplet, Nelson et al., 1998. The pairs from the ruminative triplets rarely appeared as either type of associate in those norms.)

**Presentation orders and buffers.** The pairs in both phases appeared in 12 fixed blocks, with order within the blocks randomized anew for each participant. In Phase 1, each block contained two pairs (one nonemotional and one ruminative) that would be repeated and two that would be changed in Phase 2. In Phase 2, two more pairs were added to each block from the sets to be presented for the first time (new). The order of cues on the test replicated the block assignment in Phase 2.

The ruminative pairs in Phase 1 were preceded and followed by three buffer pairs, taken from the same pool and held constants across all counterbalancing conditions. In Phase 2 we repeated one of the beginning buffers, changed the target in two buffers, and added a new pair; the ending buffers in Phase 2 contained one repeated from Phase 1, one changed, and one new. Cues from the beginning buffers in Phase 2 appeared at the start of the test and were used for practice. All materials (including normative data for the sets) and presentation orders are available on the Open Science Framework at https://osf.io/vyzuf/

#### 1.1.3. Procedure

Instructions and procedures were implemented with Qualtrics software (Qualtrics, Provo, UT, 2020) and shared on the Zoom platform (Zoom Video Communications, 2020). Experimenters instructed participants to find a quiet place with good wifi capabilities and where they would not be interrupted. Codes provided in advance insured anonymity. At the start of the session we asked them to close all other programs, mute notifications, share their screen, minimize the Zoom display, and start the Qualtrics program, while making sure that the experimenter's face was visible in the upper right-hand corner of the screen. In each phase, the experimenter read instructions on the monitor aloud and discussed them.

**Phase 1.** We instructed participants to read each word pair aloud and then imagine a scenario involving the pair to learn it for a later test. After the 5-s pair display, the participants rated the degree of difficulty in imagining, clicking one of five radio buttons ranging from "very difficult to imagine a scenario" to "very easy." The rating was self-paced. To encourage compliance with instructions, the experimenter asked the participants to describe their thinking aloud during the buffer trials.

**Phase 2.** After a short break, we instructed participants to study each upcoming pair in expectation for a test that would present the first member of each pair as a cue for recalling the second member (the target). We also asked them to notice the relation between the pairs in this phase and those in the first phase and explained that some pairs would be the same as in the previous phase, other pairs would have the same cues but different targets, and still others would be entirely new. (Given our interest in incidental noticing and integration as well as concerns about reactive effects of overt retrieval during Phase-2 learning. we chose not to assess change detection or retrieval of the Phase-1 target.) Again, we used a 5-s presentation rate but omitted the rating task.

**Test.** Test trials employed all the cues from Phase 2 as prompts for recalling the Phase-2 targets. Each trial consisted of 1–3 steps: The first step was to recall the target from Phase 2, guessing if necessary, and passing as a last resort. The next step was to decide whether the recalled word was repeated (presented as a target in both learning phases), changed (a Phase-2 target to a cue presented with a different target in Phase 1), or a target from a new cue-target pair in Phase 2; these options appeared as radio buttons that participants clicked. Last, if they clicked "changed," participants were instructed to recall and type the corresponding Phase-1 target. The experimenters explained each trial component by using examples shown on a diagram in the program. Participants were asked to describe the options, and errors were corrected. The first three buffers at the start of the test were used as practice trials to check and correct misunderstandings. The test was self-paced.

**Questionnaires**. After the test, we asked the participants to stop sharing their screen so that they could respond privately to the questions on the Ruminative Response Scale (RRS, Treynor et al., 2003) and the Beck Depression Inventory (BDI-II, omitting the suicide item; Beck et al., 1996). They were assured of anonymity. Participants also provided demographic information (gender, age, and race or ethnicity) before they were debriefed and awarded credit toward their class grades.

#### 1.1.4. Participants and design

We screened all students attending introductory-psychology classes at Trinity University by administering the RRS. Usually within a few days or (rarely) as long as four weeks after screening, we recruited the students whose scores fell in the first and fourth quartiles; 97 students participated, without any connection to the prior screening having been mentioned. While observing the constraint of equal cell size, we randomly assigned them to a counterbalancing condition for rotating triplet sets across their three experimental roles (repeated, changed, and new in Phase 2). Our stopping rule for data collection was determined by the size of the pool of screened students during two semesters. Strongly correlated with RRS scores, gender incurred an additional limitation on cell size because it was difficult to find ruminative male and nonruminative female students. (Only one person in the pool rejected those traditional gender labels.) We recruited the largest cell size possible while balancing gender within group and counterbalancing conditions.

Session RRS scores were inspected soon after participation. If the score deviated from the initial cutoffs (46 and 62) in the "wrong" direction by more than two points ($n = 13$), we replaced the participant without examining the recall data.[6] We also replaced the data from participants whose behavior during the Zoom session was inattentive or noncompliant with instructions ($n = 12$).

The final sample consisted of 36 students in each participant group, 24 female and 12 male, with complete counterbalancing preserved within group and gender. Their average age was 18.6 years (in each participant group). The students identified as White/Caucasian (54%), Hispanic/Latinx (15%), East or Southeast Asian (15%), Black/African-American (10%), and other (3%); one student identified as Middle Eastern and one as Pacific Islander. (We did not collect data concerning the cultural and economic backgrounds of these students.) The average end-of-session scores on the RRS was 70.6 for ruminators, *95% CI* [68.2, 73.0] and 34.9 for others, *95% CI* [32.5, 37.4]. Corresponding means on the BDI-II were 27.6, *95% CI* [23.8, 31.4] and 8.4, *95% CI* [6.4, 10.3].

### 1.2. Results

The results are organized according to the alternative hypotheses for outcomes associated with rumination: To address the sticky-past hypothesis (based on the prevalent findings that ruminators focus on problem-oriented, negative memories, at a cost of deficient processing of more recent similar, yet benign, experiences) we examined possible

[5] The overall mean valence rating (9-pt scale) was 2.9 for negative targets and 6.7 for benign targets.

[6] Four additional female students recruited for the rumination group produced session RRS scores that place them cleanly within the nonrumination group. The opposite happened for one male student. We switched their data accordingly, because there were no others who qualified from the larger pool. Inclusion is justified on the grounds that it works against our hypothesis. We also suggest that the RRS scores in the session were likely more trustworthy, given the time constraints during screening as well as their temporal proximity to the recall data.

group differences in (a) proactive interference in Phase-2 recall and (b) intrusions of Phase-1 negative targets. In contrast, the integration hypothesis would be supported by a ruminative benefit in proactive facilitation of Phase-2 targets, conditionalized on recall of Phase-1 negative targets. Regardless of the pattern, exaggerated effects in the rumination group would constitute evidence in support of the hypothesis.

Below, we report outcomes of analyses of variance that correspond to those hypotheses, conducted with the significance level set at .05. Participant group (ruminators and others) serves as a between-subjects factor and the experimental role of the triplets as a within-subjects factor. Significant main effects are not reported in detail if they are qualified by significant interactions. For the full design, the mean proportions of Phase-2 targets recalled are shown in the top three rows of Table 2. (We note that the repetition condition is the only condition in which Phase-1 targets were also correct responses in Phase 2. This condition is traditionally included to discourage strategies for recalling Phase-2 targets and providing options for the source decision on the test; it is not relevant to subsequent analyses.)

In addition to the ANOVA outcomes, we performed corresponding generalized linear mixed-effects analyses, with outcomes reported in Supplemental Materials (SM 1). SM 2 contains the ANOVA results concerning the recall of targets from the nonemotional triplets. (We informally describe those results parenthetically at the end of each section below.[7]) SM 3 reports the mixed-effects analyses corresponding to SM 2.

### 1.2.1. Outcomes regarding the sticky-past hypothesis

**Proactive interference.** The first test of proactive interference included factors for target role (changed or new in Phase 2) and participant group. Contrary to the sticky-past hypothesis, no effect associated with rumination status was significant, $p > .249$, $\eta_p^2 < 0.020$. More centrally, we found no evidence for overall proactive interference with these ruminative materials, $p = .557$, $\eta_p^2 = 0.005$. (A significant interference effect obtained with the nonemotional triplets; see SM 2.1.)

**Phase 2 recall conditionalized on faulty classification of changed targets.** The second test of proactive interference was based on prior evidence for clear interference when Phase-2 recall of changed targets was conditionalized on the failure to recollect Phase-1 targets and the fact that they changed (for a review, see Wahlheim et al., 2021). In this analysis, the factor for target role consisted of new targets versus changed targets recalled as a proportion of all words wrongly classified as either repeated or new on the changed trials. Only nonsignificant and small group-related differences were revealed, $p > .263$, $\eta_p^2 < 0.020$. Regardless of group, however, proactive interference under conditions of not remembering change was obtained, $F(1, 70) = 50.13$, $p < .001$, $\eta_p^2 = 0.417$. This outcome is illustrated in the left panel of Fig. 1. (SM 2.1 reports a similar outcome on trials with nonemotional triplets.)

**Table 2**
Mean proportions of targets recalled, Experiment 1.

| Target role | Ruminators | Others |
|---|---|---|
| Overall design | | |
| Repeated (negative) | .78 [.72, .84] | .71 [.64, .77] |
| Changed (benign) | .36 [.30, .43] | .31 [.25, .38] |
| New in Phase 2 (benign) | .38 [.31, .45] | .33 [.26, .40] |
| Additional comparisons | | |
| Phase 1 correct (negative) | .31 [.25, .37] | .31 [.25, .37] |
| Phase 1 intrusions (negative) | .19 [.15, .22] | .13 [.10, .17] |

**Note.** $n = 36$. Brackets contain 95% CIs. "Phase 1 correct" refers to. Phase-1 targets recalled following a correct classification of "changed."

**Intrusions of Phase-1 targets.** If ruminators were attending less well to the changed targets in Phase 2, Phase-1 targets might intrude more often during the test. In fact, ruminators did produce more negative intrusions, compared to other participants, $t(70) = 2.33$, $p = .023$, *95%* $CI_{diff}$ [0.01, 0.10]. Means are reported in the last row of Table 2. (This difference in intrusions from nonemotional Phase-1 targets was not significant; see SM 2.1.)

**Summary**. In short, the results provide minimal support for the hypothesis that ruminators are memorially stuck in the past and inattentive to change; no evidence for differential proactive interference obtained in Phase-2 target recall. However, ruminators did produce significantly more negative intrusions in response to cues for changed targets, a finding that likely reveals the retrieval strength of the ruminative targets on trials where the benign targets failed to come to mind.

### 1.2.2. Outcomes relevant to the integration hypothesis

As preparation for the conditional analyses (reported above and in this section) we examined possible differences in the accuracy of classifying the test responses as repeated, changed, or new in Phase 2. Then we examined the proportion of all 12 "changed" test trials on which participants correctly classified the role and then recalled the Phase-1 target. In this section we first report those results before the outcomes related to proactive facilitation.

**Classifications and subsequent recall of Phase-1 targets.** The proportions of correct classifications were examined in an analysis of variance with factors for participant group and the actual role (repeated, changed, and new). Only the main effect for actual role reached significance, $F(2, 140) = 94.56$, $p < .001$, $\eta_p^2 = 0.575$. (For other effects, $p > .432$, $\eta_p^2 < 0.013$.) As should be expected, participants were less often correct about changed pairs, compared to the other roles, regardless of ruminative status, $F(1, 70) = 192.23$, $p < .001$, $\eta_p^2 = 0.733$. Means were 0.47 (changed), 0.81 (repeated), and 0.79 (new); 95% CIs were, respectively: [0.42, 0.52], [0.77, 0.85], and [0.74, 0.83]. Classification accuracy on repeated and new trials did not significantly differ, $p = .409$, $\eta_p^2 = 0.010$. (Similar outcomes obtained in classifications of nonemotional triplets, but in addition repeated targets were classified more accurately than new targets; see SM 2.2.) Next, descriptive statistics suggested that the two groups "equally" recalled Phase-1 targets following correct classification on changed trials (Table 2, fourth row).

**Phase-2 recall conditionalized on recalling Phase-1 targets.** Although ruminators showed no advantage in accurate classification or in the proportion of trials in which they subsequently recalled Phase-1 targets, their advantage was revealed in proactive facilitation. The appropriate assessment of target integration during Phase-2 learning is one in which Phase-2 recall is conditionalized on the correct recall of Phase-1 targets made after the participants correctly classified the targets as changed. We submitted these proportions to a comparison with new-target recall, as a planned test of proactive facilitation and included a factor for participant group. In this analysis of variance, the main effects corresponding to an overall rumination advantage and to overall facilitation were both clearly significant. However, the interaction of these factors revealed that the facilitation effect was larger for ruminators, $F(1, 68) = 6.67$, $p = .012$, $\eta_p^2 = 0.089$. The mean facilitation advantage was 0.33 for ruminators and 0.15 for the other participants. This interaction is illustrated in the right panel of Fig. 1. (A similar pattern obtained in the recall of targets from nonemotional triplets, but the interaction failed to reach significance and its effect was smaller; see SM 2.2.)

## 1.3. Discussion

The results from Experiment 1 replicated previous evidence for proactive interference and facilitation when recall was conditionalized on recollecting change across phases (see the review by Wahlheim et al.,

[7] We do not report analyses that include the two types of materials as a factor. Interpretations of main effects or interactions with that factor would be confounded by their differential nature, degree of conceptual similarity within the type, word frequency, concreteness, imagery, and probably other characteristics.

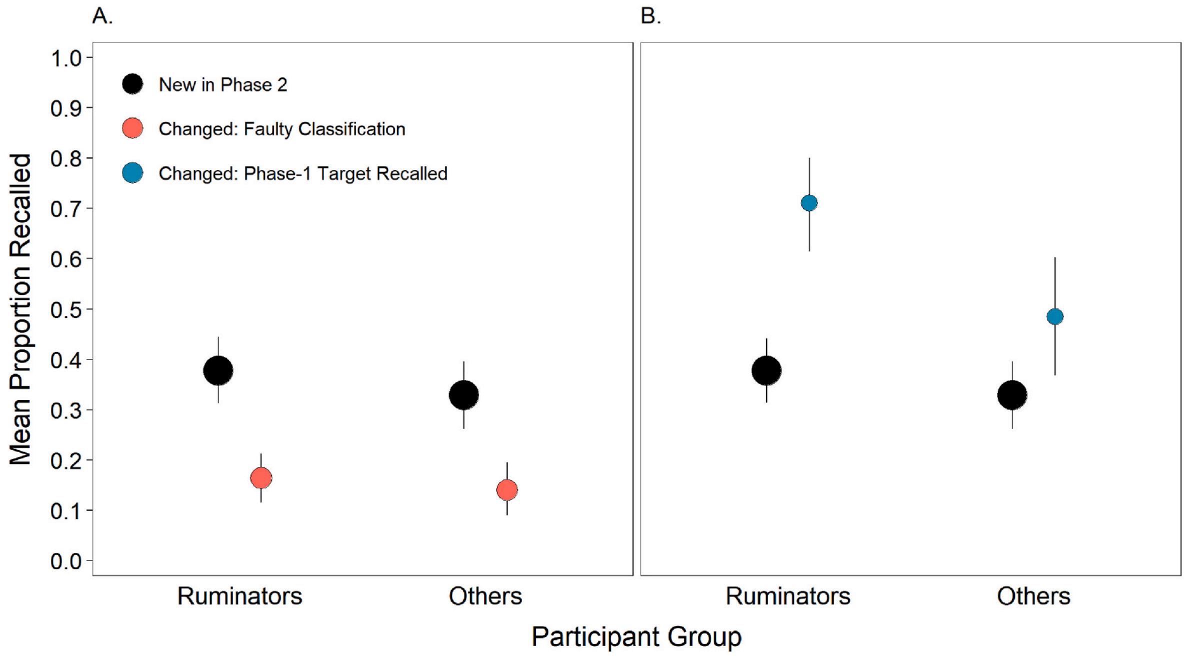


**Fig. 1.** Proactive Effects of Ruminative Memories on Phase-2 Target Recall
***Note.*** (A) Proactive interference effects associated with faulty classification of changed items. (B) Proactive facilitation effects associated with correct classification of changed items and correct recall of Phase-1 targets. Point size areas for conditional (colored) points indicate relative differences in the denominator of the measures. Error bars are 95% confidence intervals.

2021). Our main concern, however, was the conceptual utility of such findings for understanding ruminative patterns of remembering. In that regard, the results tip the scales in the direction of a ruminative advantage in integrative processing and later facilitation of recalling the changed target. Yet, another outcome was also informative: When ruminators failed to recall the benign Phase-2 targets, the Phase-1 negative targets intruded. Test instructions strongly cautioned against passing. Therefore, compliant participants might have more deliberately substituted a negative target when they could not recall the benign target correctly. Ruminators were possibly better at doing just that. It is also important to consider that integration might not have been encouraged by all triplets equally. That consideration implies that intrusions could reflect the high probability of retrieving the negative target on trials where integration has not occurred.

Regardless of evidence for retrieving negative memories at a cost or using them as bridges for recalling benign targets, knowing how well ruminators would be able to recall the Phase-1 targets that constituted the proactive role is important to a fuller understanding of differential remembering associated with rumination. Experiment 2 therefore was designed to replicate the procedures of Phases 1 and 2 and to follow them with a test that encouraged but did not require the recall of both targets.[8] Another benefit to this test was that it also examined the generality of the proactive-facilitation advantage in a test that does not specify source or request its monitoring. Real-life conditions inconsistently cue memory for source. Together with Experiment 1, this approach therefore more fully addresses modifications related to everyday ruminative remembering.

## 2. Experiment 2

### 2.1. Method

#### 2.1.1. Transparency and openness

This experiment was preregistered on the Open Science Framework, with updates, for designs and analyses: https://osf.io/eb2vr. The materials, script, data, and code can be found at https://osf.io/6ebym/files/. We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. Data collection was preapproved by the Trinity University Institutional Review Board.

#### 2.1.2. Materials, design, and procedure

We made a few changes in specific triplets to better represent ruminative concerns or situations (according to our conceptions). The nonemotional materials were again included, unchanged, to maintain a consistent learning context across experiments.

The design and procedure used in all phases replicated Experiment 1, with the following exceptions. First, instead of the built-in request on the test to recall Phase-2 responses followed by source information, two text boxes followed each cue display and signaled the recall of any targets that came to mind. Instructions mentioned that participants might recall just one word on some trials, because many cues were associated with only one word. In the case that two words were recalled, instructions asked participants to type the first word that came to mind in the first text box. We also instructed against the explicit use of an alternative strategy, such as typing the words in the order of their presentation in the two learning phases. Finally, when only one word came to mind, we asked participants to pay attention to whether they had a vague feeling that there had been a second word and, if so, to try to recall it; there was no time limit.

The second change from the procedure in Experiment 1 was to include a strategy questionnaire following the test (available at https://osf.io/6ebym/files/). We asked participants to judge the extent to which they used specific strategies or followed instructions. Because several participants indicated use of phase-related strategies, this report does not include analyses relevant to the order of typing the words. Moreover,

[8] This test, commonly called Modified Modified Free Recall (MMFR), has served an historically important role in investigations of interference and facilitation effects (e.g., Barnes & Underwood, 1959).

we did not examine the strategy reports because, upon reflection, we lost confidence in their accuracy due to their retrospective and holistic nature.

### 2.1.3. Participants

All recruitment and assignment procedures during the academic year were the same as in Experiment 1, except in these populations the cutoff scores on the RRS were 48 and 63. From the 89 students who participated in the experiment, 10 produced end-of-session RRS scores that fell within the interval between 50 and 61, so their data were set aside. An additional six participants were nonattentive during Zoom sessions, passed often on the test, or experienced interruptions. To work toward balancing gender across the counterbalancing conditions and participant groups, we also set aside the data from the four most-recent male non-ruminative participants. After the spring semester, we still needed two more non-ruminative female and one more ruminative male participant, and we sought them from the pool of students who were conducting research during the summer. Of those who took the RRS survey and were eligible (according to the same cutoffs as during the academic year) six students participated (but two were later disqualified by their end-of-session RRS scores). To balance the addition of summer participants (a different population) across the two participant groups, we set aside the data from a ruminative female student in the relevant counterbalancing condition who participated late in the spring semester.

The final sample contained data from 9 male and 27 female participants in what we term the "other" group, distributed equally across the three counterbalancing conditions.[9] The same proportion characterized the ruminators, except that one of the counterbalancing conditions contained data from 10 female and 2 male students. The students identified as White/Caucasian (51%), Hispanic/Latinx (22%), East or Southeast Asian (18%), Black/African-American (7%), and other (1%). Their average age was 18.8 years (in each group). The average end-of-session scores on the RRS was 68.3 for ruminators (*95% CI* [66.2, 70.4]) and 38.6 for the others (*95% CI* [36.5, 40.7]). Corresponding means on the BDI-II were 25.0 (*95% CI* [22.4, 27.5]) and 9.0 (*95% CI* [6.4, 11.5]).

## 2.2. Results

In the first two sections below, we report analyses that loosely correspond to the tests of interference and facilitation, performed in Experiment 1. It is important to keep in mind that, unlike Experiment 1, Experiment 2 procedures did not specify the phase from which the targets should be recalled, a change that muddies comparisons with Experiment 1. Next, we report tests that correspond more directly to the sticky-past and integration hypotheses when the source (phase) is not specified. All analyses were conducted with alpha set at 0.05. The corresponding results of mixed-effects analyses are reported in SM 4. The results of analyses performed on recall of the nonemotional materials are reported in SM 5 and SM 6. (Informal descriptions of the nonemotional results are provided parenthetically at the end of each section.)

Means for the full design are shown in Table 3. Significant group differences occurred for targets from changed trials and for the new targets from Phase 2, as revealed by the confidence intervals. (Corresponding differences in recalling nonemotional targets were nonsignificant, as shown in SM 5.)

### 2.2.1. Evaluating proactive interference

The clearest measure of proactive interference in this procedure is simply the comparison of Phase-2 targets recalled in the changed and new conditions. The statistics provided in Table 3 make it obvious that

**Table 3**
Mean proportions of targets recalled, Experiment 2.

| Target role | Ruminators | Others |
|---|---|---|
| Overall design | | |
| Repeated (negative) | .69 [.63, .76] | .66 [.60, .72] |
| Changed (negative) | .45 [.39, .52] | .36 [.30, .43] |
| Changed (benign) | .39 [.33, .46] | .30 [.24, .38] |
| New in Phase 2 (benign) | .40 [.34, .46] | .29 [.23, .36] |
| Additional comparisons | | |
| Changed (only negative) | .25 [.21, .29] | .25 [.19, .30] |
| Changed (both targets) | .20 [.15, .25] | .12 [.07, .16] |

**Note.** $n = 36$. Brackets contain 95% CIs.

such evidence for interference was not obtained. (In contrast, the nonemotional materials produced evidence of proactive interference, unqualified by group; see SM 5.)

We mimicked the conditionalized interference measure used in Experiment 1 with a comparison of new targets recalled to Phase-2 benign targets recalled alone, without recall of the corresponding negative targets from Phase 1. An analysis of variance that included participant group as a factor revealed a significant interaction, $F(1, 70) = 5.07$, $p = .027$, $\eta_p^2 = 0.068$. However, the interaction was determined entirely by the new-target recall advantage for ruminators (see Table 3 for those means), because the "conditionalized" recall of changed targets approached identity in the two groups (in each group, $M = .19$, 95% CI [0.14, 0.24]). The main effect of target role was significant, $F(1, 70) = 43.03$, $p < .001$, $\eta_p^2 = 0.381$. (Analysis of variance in recalling targets from the nonemotional materials revealed only the main effect of target role, representing evidence for proactive interference; see SM 5.)

### 2.2.2. Evaluating facilitation

To examine something akin to the evidence for proactive facilitation in Experiment 1, we calculated the proportion of Phase-2 targets recalled from changed trials, conditionalized on the recall of the corresponding Phase-1 target. In an analysis of variance with target role (conditional changed vs. new Phase-2 targets) and participant group as factors, only the main effect of group was significant, $F(1, 69) = 6.78$, $p < .011$, $\eta_p^2 = 0.089$. This outcome might also be influenced by the large group difference in the recall of new targets (Table 3). In neither group did the conditionalized recall of changed targets surpass the recall of the new targets (for ruminators, $M = .39$, 95% CI [0.30, 0.47]; for others, $M = 0.27$, 95% CI [0.18, 0.36]). This result might reflect output interference from Phase-1 recall on this unconstrained test. (The corresponding analysis of nonemotional materials revealed an interference [instead of facilitation] effect; see SM 5.)

### 2.2.3. The sticky past versus integration: Newly relevant measures

The primary reason for conducting Experiment 2 was to investigate the potential accessibility of unrepeated negative targets, in the context of a proactive design, because such accessibility is central to the integration hypothesis and Experiment 1 could not assess it directly. In Experiment 2, the recall of these negative targets can be understood as (a) their singular recall, unaccompanied by recall of the corresponding benign target, and (b) the joint recall of both targets. As obvious as that seems, however, these two separate measures have special meaning in the context of our two hypotheses.

**Negative-target recall.** On a test that cues recall without specifying source, the tendency for ruminators to more frequently recall only the negative Phase-1 targets from changed trials would support the sticky-past hypothesis and provide an analogy to the common tendency to bring only the unmodified problematic event to mind. In this experiment, however, this measure was not greater for ruminators than for others, $t(70) = 0.28$, $p = .778$, Cohen's d = 0.067 (descriptive statistics in Table 3, first row of additional comparisons). (A similar outcome obtained for the recall of nonemotional Phase-1 targets; see SM 5.)

**Recall of both targets in the changed condition.** To examine

[9] A power analysis based on the interaction for facilitation in Experiment 1 suggested an N of 82, however the important differences in Experiment 2 were simple two-group comparisons. An effect size of 0.60 required an N of 72.

evidence for the integration hypothesis on an unconstrained test, the joint recall of both targets seems better suited, compared to recall of benign targets conditionalized on the recall of negative targets. In the case of our ruminative materials, joint recall of both targets would suggest that the negative target was retrieved and connected meaningfully to the benign target during Phase 2. Consistent with the integration hypothesis, ruminators recalled both targets in the changed condition more often than did the other participants, $t(70) = 2.57$, $p = .012$, Cohen's d = 0.605 (descriptive statistics in Table 3, last row). (The difference in the recall of both nonemotional targets was in the same direction but nonsignificant and small.)

Another approach to understanding these two aspects of Phase-1 target recall is to examine their bivariate correlations with identifiable components of the RRS score. Treynor et al. (2003) identified three subscales of the RRS, one of which is highly correlated with measures of depression. However, the other two measures are more informative: one is a maladaptive measure called brooding and the other taps the more useful problem-oriented aspect of rumination called reflective pondering (reflection, for short). These latter two subscales were each correlated with the joint recall of both targets from ruminative pairs; for brooding, $r(70) = 0.36$, $p = .002$; for reflection, $r(70) = 0.27$, $p = .022$. The correlations between these subscales and recall of negative targets alone were both less than 0.09 ($p > .455$). And all such correlations with the corresponding measures of nonemotional recall were less than 0.15 ($p > .194$).

## 3. General discussion

Our conceptual goal for these experiments was to initiate research on the effects of memories associated with ruminative concerns on attempts to establish newer emotionally benign memories that would come to mind to the same contextual cues subsequently. In real-life, a therapeutic goal is to encourage retrieval of alternatives to the problematic memories contributing to rumination—to help disrupt the habit—because ruminative memories seem particularly sticky in therapeutic contexts. Indeed, experimental studies support the sticky-memory hypothesis. Yet recent research conducted within an integration framework led us to envision a bridging role for ruminative memories. According to the integration framework, opportunities for integration can result from the activation of ruminative memories when new memories or interpretations are later experienced with the same cues occur anew in. Subsequently, the resulting connections facilitate recall of the new experience when the ruminative memories emerge again, as they inevitably do. In therapeutic settings, for example, facilitation of meaningful connections between the ruminative memory and the new interpretation would promote memory for the latter and make good therapeutic use of the sticky memory.

Our experimental attempts to mimic these complex situations necessarily changed the nature of the memories involved. In place of participants' personal memories, we substituted target words and cuing contexts with ruminative themes and instructions to imagine related scenarios; benign targets related to the same contextual cues were later learned, and the cues provided for a final test. Our results from these initial attempts are consistent with the integration hypothesis and, with one interesting exception, we failed to find evidence for ruminative stickiness. (For all reported outcomes, the results from mixed-effects models converged closely with the results from analyses of variance; see Supplemental Materials.)

Sticky memories produce noticeable proactive interference in real-life settings (see Watkins & Roberts, 2020). On the guided recall test in Experiment 1, interference occurred when participants failed to remember that the targets had changed in Phase 2. On the unguided test in Experiment 2, interference was found when benign targets but not the corresponding negative targets were recalled. Across both experiments, however, rumination status did not play a role in these outcomes. A possible glimpse of stickiness emerged in the more frequent intrusions of the negative (but not the nonemotional) targets by ruminators in Experiment 1. Ruminative intrusions on trials where triplets had not encouraged sufficient integration suggest differential accessibility (seen again in the group difference in negative recall in Experiment 2). In summary of the stickiness issue, we have offered no support from the main interference measures (and Chang et al., 2019, reported similar outcomes concerning the CIE of rumination-relevant misinformation) but sticky possibilities expressed by intrusions. Importantly, those negative intrusions are not incompatible with our main findings of proactive facilitation and joint recall that implicate integration. The degree of each phenomenon likely varies across trials (episodes) that are differentially meaningful on an individual basis.

The slight evidence for stickiness in general does not rule out differential availability and accessibility of ruminative memories, a widely documented phenomenon (see Watkins & Roberts, 2020). According to the integration hypothesis, the retrieval of ruminative memories is central to the act of noticing changes connected to their cues. In our experiments, an important consideration regarding the detection of change is that our ruminative materials made possible relations between the Phase-1 and Phase-2 pairs meaningful, beyond the fact of a shared cue. That extra meaning likely is a central aspect of the Experiment-1 finding of the exaggerated proactive facilitation in recalling changed targets by ruminators, an interaction that did not obtain for the nonemotional materials. Additional research should include nonemotional materials in which the basis for the change between the targets is meaningful in the context of the cue (e.g., *church-bell, church-service*). We also predict that if the nature of that integrative meaning is related to the basis for participant selection (e.g., church membership), similar interactions could result. Lacking bases for integration, sticky interference might obtain for any selected participants with concerns related to the salient first experience. Such possibilities, we stress, should not be understood to undermine the integration hypothesis; instead, they would enrich our understanding of the conditions that promote integration and extend its generality in establishing proactive facilitation.

An unexpected but useful outcome relevant to the integration hypothesis is that the nature of its support shifted in Experiment 2. Clearly, the most straightforward evidence in that experiment was greater joint recall of the targets by ruminators. The test procedure in Experiment 1 could not as clearly reveal joint retrieval, because it first focused retrieval attempts on the second target, then required participants to remember that it had changed, before they had an opportunity to report the first target. Moreover, when we measured facilitation of benign recall in Experiment 2 as we had done in Experiment 1—by conditionalizing its recall on the recall of negative targets—benign recall was not greater than in the control condition, possibly due to output interference from the first-learned target. Upon reflection, conditional measures seem inappropriate when search is not constrained to a specific source that requires monitoring. In everyday terms, the difference between these two test experiences is the difference between trying to remember the changed event in particular (and being helped by integration, given memory for the problematic version) and simply reporting what comes to mind in relation to the cuing context. In real-life, compared to the lab or the therapist's office, the latter version seems more applicable; the goal is to have benign alternatives come to mind when controlled strategies are less available. Regardless, both test scenarios have offered support for the integration hypothesis and together provide a fuller characterization of the accessibility and use of rumination-themed memories under varying retrieval conditions. It is important to notice, however, that generalizing these results to ruminative recall under more natural conditions and by a more heterogeneous population is a stretch that must ultimately be achieved with empirical support; our student population is clearly a shortcoming of this work. Our experiments addressed the mechanisms, as "proof" of the principle that memory integration can ultimately help ruminators remember other contextually related events and interpretations, along *with* the ubiquitous fuel for rumination.

In closing, we call attention to the relatively small number of trials that contributed grist to integration. Clinical psychologists would join us in noting that people, perhaps ruminators in particular, have specific concerns and interests, and that experimental materials must somehow tap them. The student who found meaning when *essay failure* turned into *essay praise* is probably not the same student whose family collapsed and then reunited. Significantly, however, that student might now remember that the seemingly failing essay was ultimately praised.

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## CRediT authorship contribution statement

**Paula T. Hertel:** Conceptualization, Methodology, Software, Formal analysis, Resources, Data curation, Writing – original draft, Writing – review & editing. **Christopher N. Wahlheim:** Methodology, Formal analysis, Resources, Data curation, Visualization, Writing – review & editing. **William A. Price:** Conceptualization, Software, Investigation, Resources, Writing – review & editing. **Emily M. Crusius:** Investigation, Resources, Writing – review & editing. **Christina L. Patino:** Investigation, Writing – review & editing.

## Declaration of competing interest

The authors declare that there were no conflicts of interest with respect to the authorship or the publication of this article.

## Data availability

Open Science

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## Supplementary data

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