Reports

"If only I could stop generating counterfactual thoughts": When counterfactual thinking interferes with academic performance☆

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ABSTRACT

Previous research suggests that counterfactual thoughts in reaction to undesirable, academic performances can enhance subsequent performances by providing functional, behavioral prescriptions. However, on the basis of research distinguishing counterfactual content for the self and others, metacognitive findings suggesting that people are inaccurate in their self-appraisals, and the link between hindsight bias and counterfactual thinking, it was hypothesized that counterfactuals can inhibit improvements in academic performance by providing a false sense of competence. Study 1 showed that studying behavior and improvement on standardized exam items were inhibited by spontaneous counterfactual thought responses. Study 2 manipulated the salience of counterfactual thinking and showed that the negative relationship between counterfactual thought frequency and exam improvement was mediated by studying behavior. Furthermore, perceived skill mediated the link between counterfactual thinking and studying behavior. Implications of these results are discussed in light of functional and dysfunctional viewpoints of counterfactual thinking.

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Introduction

When people experience undesirable events, they often cannot help but think of how things might have been (see Hofstadter, 1979; Roese, 1997). Such mentally simulated alternatives to reality are known as counterfactual thoughts. Counterfactuals are often characterized by conditional statements (i.e., "If only X, then Y.") and can serve as standards of comparison for judgments (Kahneman & Tversky, 1982; Roese, 1997). Thus, counterfactuals can have an important influence on affective, cognitive, and behavioral reactions (Petrocelli & Sherman, 2010).

Our understanding of counterfactual thinking as an antecedent of performance is critical because people can use mental simulation to prepare for future cognitive tasks (e.g., Pham & Taylor, 1999). For instance, generating a counterfactual after missing a flight (e.g., "If only I had left for the airport five minutes earlier…"), may increase the likelihood of taking necessary steps in the future. Consistent with this notion, Roese (1994) showed that upward counterfactuals (i.e., mental simulations better than reality) and additive counterfactuals (i.e., addition of antecedents to reality), in response to anagram performance feedback, enhanced performance on subsequent trials. Markman, McMullen, Elizaga, and Mizoguchi (2006) also demonstrated that under particular

conditions of regulatory fit (i.e., promotion focus) upward counterfactuals can boost task persistence.

Functional views of counterfactual thinking (e.g., Epstude & Roese, 2008; Markman, Gavanski, Sherman, & McMullen, 1993; Roese, 1994) hold that counterfactuals provide behavioral prescriptions that aid in the avoidance of undesirable outcomes, or promote desirable outcomes, in the future. Indeed, existing evidence (Kray, Galinsky, & Markman, 2009; Markman et al., 2006; Morris & Moore, 2000; Nasco & Marsh, 1999; Reichert & Slate, 2000; Roese, 1994) supports this line of reasoning. The present research also adopts a functional perspective of counterfactual thinking, but concentrates on the dysfunctional consequences of generating counterfactuals on performance.

Dysfunctional possibilities of counterfactual thinking for academic performance

Despite evidence suggesting that counterfactuals should enhance performance, there are reasons to believe that counterfactuals can also have a deleterious effect on performance (see Markman, Karadogan, Lindberg, & Zell, 2009; Petrocelli & Harris, 2011; Sherman & McConnell, 1995). We propose that people can overestimate their competence by generating counterfactuals that "explain away" their failures. Such judgments should result in inhibited attention to related activities, leading to missed opportunities for improvement.

Our proposal is based on three lines of reasoning. First, research on metacognitive judgments of performance and learning suggests that people are often inaccurate about their performance (Kruger & Dunning, 1999) and their knowledge/skills (Dunlosky & Nelson, 1994; Nelson &

Dunlosky, 1991). Furthermore, studies conducted by Girotto, Ferrante, Pighin, and Gonzalez (2007) conclusively showed that people have a tendency to mentally undo aspects of the problem/undesirable situation rather than their own behaviors. When they do recognize that they have failed, this tendency would seem to reduce the likelihood of functional changes in behavior.

Second, although counterfactuals may have the potential to reduce hindsight bias (i.e., overestimation of the foreseeability of an outcome once the outcome is known; Fischhoff, 1975), some evidence suggests that counterfactuals can enhance this bias (Petrocelli & Sherman, 2010; Roese & Maniar, 1997; Roese & Olson, 1996). That is, counterfactual thinking often makes one's causal attributions for an outcome salient and prevalent in his/her mind—and when counterfactuals are easy to generate, people are more likely to misremember that they were aware of the causal links of an event even before knowing its outcome. In the context of negative feedback on an exam, hindsight bias may lend to a false sense of competence and reduce motivation to improve. In fact, others (Markman, Elizaga, Ratcliff, & McMullen, 2007) have shown that upward counterfactuals can lead to an inflated self-assessment of intelligence. Furthermore, Kim, Chiu, and Zou (2010) showed that inflated self-assessments of performance can increase the likelihood of practicing self-handicapping and result in relatively poor performance on subsequent tasks. To maintain their inflated self-assessments people often ignore the diagnosticity of negative feedback (Allison, Mackie, & Messick, 1996), further inhibiting accuracy.

Finally, the work of Metcalfe (1998, 2009) suggests that people's metacognitive judgments of their own learning are causally linked to their study behavior. Specifically, the belief that material has already been learned decreases study time on related material and increases focus on material that is not already well learned. Given these tendencies, subsequent learning will occur to the extent that one's metacognitions are accurate and he/she makes appropriate study choices.

Study 1

Study 1 was designed to explore how students respond to incorrect exam items. Also, we were interested in how counterfactual responses affect study preferences and subsequent exam performance. After completing an exam, we provided participants with veridical feedback for each item and asked them to list their thoughts. Consistent with Gilovich's (1983) findings regarding the processing of performance feedback, we expected participants to accept their success feedback at face value but to explain away incorrect exam items with counterfactuals (e.g., "I would/could have responded to that item correctly had I thought about it a little longer."). We then provided participants with study materials for each of the exam topics, and recorded the total time they spent attending to information on each topic. Participants then completed a final exam.

Because students tend to be motivated toward improvement until they reach a subjective sense of mastery (Metcalfe, 1998, 2009), we hypothesized that our participants would be less likely to study exam topics for which they had generated a counterfactual in response to incorrect exam items than exam topics for which they did not generate a counterfactual in response to any items. In other words, we contend that counterfactuals permit a subjective sense of skill or mastery, thereby inhibiting efforts toward improvement. Thus, we hypothesized that performance improvements on a subsequent exam would be associated only with those topics that were not characterized by counterfactual responses during initial exam feedback.

Method

Participants and design

Forty-one introductory psychology students participated in exchange for partial course credit. We employed a 2 (Practice Exam Sections: counterfactual vs. no counterfactual) × 2 (Exam Performance: practice vs. final) mixed quasi-experimental design, using Exam Performance as a within-subjects factor.

Procedure

Experimental materials were presented to participants in individual cubicles viewing personal computers using MediaLab v2010 research software (Jarvis, 2010). The instructions of the experiment were self-paced.

Practice exam. Participants first completed a 16-item, multiple-choice practice exam consisting of four items in each topic: English, Math, Reading, and Science. All items were taken from *Preparing for the ACT 2009/2010* (ACT, 2009a). Items were presented on a single HTML display such that participants could navigate as they wished.

Participants were given feedback following completion of the practice exam and were informed that they could review exam guides before completing a final exam of the same level of difficulty. To increase motivation, it was also noted that in our previous research we had found performance to be highly correlated with various forms of intelligence.

Feedback and thought-listing. After completing the practice exam, participants were reminded of each item and their response one at a time. Feedback was provided for each alternative response, briefly explaining why the correct answer was correct and why each incorrect alternative was incorrect. After each feedback item, participants were asked to type the first thought that came to mind.

Study period. Before continuing to the final exam, participants could review study materials (containing, for example, practice questions and tips, ACT, 2009b) that might aid them in the final exam. Participants were required to spend a minimum of five minutes reviewing this information. Participants were then prompted to either continue to the final exam or continue reviewing the study materials as long as they wished. Participants were permitted to select any topic for study at any time.

Final exam. Finally, participants completed a final exam (16 items) taken from *Preparing for the ACT 2009/2010* (ACT, 2009a). Pilot testing revealed that the practice and final exams were equivalent in difficulty and time needed for completion. Practice and final exam performances were calculated as the proportion of items correct.

Results and discussion

Counterfactual thoughts

Two independent coders categorized each thought-listing as a counterfactual or non-counterfactual. A thought was coded as a counterfactual when it clearly expressed the consideration of an alternative antecedent and either directly described or implied an alternative outcome. In the case of a counterfactual coding, coders were instructed to then designate the direction (i.e., upward or downward—mental simulations worse than reality) and structure (i.e., additive or subtractive—subtraction of antecedents to reality) of the thought. Initial agreement reached 83%, and a third judge settled disagreements. Examples of counterfactuals listed included: "The correct answer was my other option. Shoulda read the question more carefully." and "I should have read the answer choices more thoroughly because I would have picked up on the grammar mistake."

On average, the sample generated 1.41 counterfactuals ($SD = 1.26$). The average proportion of incorrectly answered practice items followed by a counterfactuals thought was .35 ($SD = .65$). A one-way repeated-measures analysis of variance (ANOVA) showed that this rate did not depend on the four topic sections, $F(3, 120) = .05$, *ns*. Furthermore, practice exam performance was not correlated with the counterfactual

frequency ($r = -.16$, *ns*). Thus, participants responded to several items incorrectly without generating a counterfactual. Also, counterfactual thought responses were dominated by an upward (95%) rather than a downward direction and an additive (85%) rather than a subtractive structure.[1]

Study time

Participants opted to spend an average of 155.54 s ($SD = 78.93$) in study time beyond the minimum of five minutes. The study-time totals were summed as either part of the sections for which participants had or had not generated one or more counterfactuals. Because time spent studying was marginally associated with practice performance ($r = .28$, $p < .08$), we used performance as a covariate in a one-way repeated-measures analysis of covariance comparing the study time for sections in which participants had and had not generated counterfactuals. Consistent with expectations, the time participants spent on sections in which they had generated counterfactuals was significantly less ($M = 38.46$, $SD = 53.70$) than the time they spent on sections in which they had not generated counterfactuals ($M = 117.09$, $SD = 79.35$), $F(1, 39) = 20.90$, $p < .01$, $\eta^2 = .34$; the covariate remained marginally associated with study time, $F(1, 39) = 3.29$, $p < .08$, $\eta^2 = .08$.

Exam performance

To assess whether generating counterfactuals on the practice exam was associated with overall performance on the final exam, we employed a 2 (Practice Exam: counterfactual vs. no counterfactual) × 2 (Exam: practice vs. final) repeated-measures ANOVA. A main effect for Practice Exam emerged, such that overall performance was significantly better for practice sections in which participants had not generated counterfactuals ($M = .68$, $SD = .22$) than those in which they had generated counterfactuals ($M = .52$, $SD = .26$), $F(1, 31) = 18.49$, $p < .001$, $\eta^2 = .37$. A main effect for Exam Performance also emerged, such that performance was significantly better on the final exam ($M = .66$, $SD = .22$) than the practice exam ($M = .54$, $SD = .26$), $F(1, 31) = 12.79$, $p < .001$, $\eta^2 = .29$.

These effects were qualified by the hypothesized interaction, $F(1, 31) = 15.13$, $p < .001$, $\eta^2 = .33$ (see Fig. 1). Although, no difference in performance was found between the practice sections in which participants did and did not generate counterfactuals, $t(31) = -1.15$, *ns*, a significant difference was obtained for the final exam, $t(31) = -6.66$, $p < .001$, $d = 2.39$, such that participants performed better on sections in which they did not generate counterfactuals. Importantly, participants improved on sections in which they did not generate counterfactuals, $t(31) = -5.54$, $p < .001$, $d = 1.99$, but did not improve on sections in which they did generate counterfactuals, $t(31) = -.03$, *ns*.

Although we obtained data suggesting that counterfactual thinking can inhibit studying, it is possible that counterfactual thinking simply covaries with academic topic areas that one is deficient in, and topic areas one is deficient in may not be ones in which people are particularly fond of studying. This possibility would place counterfactual thinking in a coincidental position and not an influential one as outlined in our theoretical stance. Because of this possibility, and other limitations of correlational studies, we manipulated the salience of counterfactual thinking in Study 2 to directly assess its impact on studying and performance.

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[1] Although we would expect the smallest performance improvements for exam sections in which participants generated upward counterfactuals and the greatest improvements for exam sections in which participants generated downward counterfactuals, we believe that our data did not contain enough downward counterfactuals to test these notions. Unfortunately, the same was true with regard to subtractive counterfactuals. We employed a liberal assessment of counterfactual frequency by counting all counterfactuals, regardless of direction and structure.
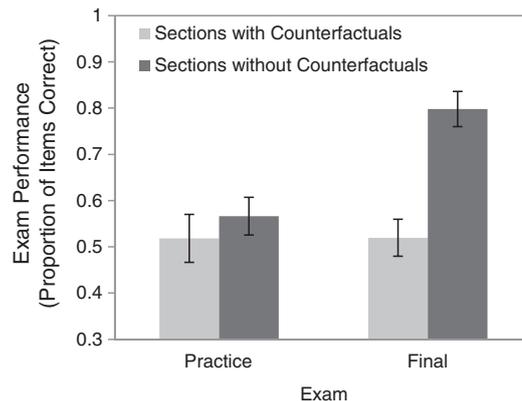

Fig. 1. Performance means by exam and exam sections characterized by counterfactual thought responses versus exam sections not characterized by counterfactual thought responses (Study 1).

Study 2

Study 1 demonstrated that counterfactual thinking was associated with less study-time and ultimately poorer performance. This counter-intuitive effect has both theoretical and applied implications. Therefore, Study 2 tested study time as a mediator of this effect and provided an opportunity to conceptual replicate this finding through manipulations of counterfactual salience. We also considered potential mediators of the link between counterfactual thinking and studying behavior such as expectations, perceived skill, and level of enjoyment of the subjects covered by the ACT.

Method

Participants and design

Fifty-seven introductory psychology students participated in exchange for partial course credit. We employed a 2 (Thought-Listing Instructions: counterfactual vs. open-ended) × 2 (Exam Performance: practice vs. final) mixed design, with Exam Performance being a within-subjects factor.

Procedure

The procedures were very similar to those of Study 1, with the exceptions of a thought-listing manipulation and presentations of additional questions during the study phase of the protocol.

Feedback and thought-listing instructions. Participants were randomly assigned to one of two thought-listing conditions: counterfactual or open-ended thought conditions. When participants in the *counterfactual thought-listing* condition responded incorrectly to a practice exam item, they were instructed to consider how they might have answered the question correctly (i.e., upward counterfactual) and type their thought. When these participants answered a question correctly, they were instructed to type the first thought that came to mind. Participants assigned to the *open-ended* condition were instructed to type the first thought that came to mind for correct and incorrect responses made during the practice exam.

Study period. Directly following the thought-listing task, participants reported how much they enjoyed, as well as how skilled they believed themselves to be in, each of the four topics covered by the ACT on nine-point scales with *not at all* (1) and *very much* (9) as the anchor labels. Then, using the same procedures and guidelines used in Study 1, participants were introduced to the study period. Finally, participants reported how many questions they expected to answer correctly for each of the four sections of the final exam.

Results and discussion

Counterfactual thoughts

As in Study 1, thought-listing was coded by two coders, blind to the hypotheses and conditions. Initial agreement between the coders was high (80%), and a third coder resolved disagreements.

On average, the sample generated 2.31 counterfactuals ($SD = 1.84$). The average proportion of incorrectly answered practice test items followed by a counterfactual was .49 ($SD = .34$). A one-way repeated-measures ANOVA showed that this rate did not depend on the four topic sections, $F(3, 168) = 2.03$, ns. As in Study 1, counterfactuals were dominated by an upward direction (93%) and an additive structure (78%).

Next, we conducted a manipulation check for our thought-listing manipulation. As expected, counterfactual condition participants listed more counterfactuals ($M = 3.21, SD = 2.06$) than open-ended condition participants ($M = 1.45, SD = 1.06$), $F(1, 55) = 16.75, p < .001, \eta^2 = .23$.

Study time

Participants opted to spend an average of 159.92 s ($SD = 86.28$) in additional study time beyond the required minimum of five minutes. Consistent with expectations, participants in the counterfactual thought-listing condition spent significantly less time (seconds) studying ($M = 135.85, SD = 86.50$) than participants in the open-ended condition ($M = 183.17, SD = 80.83$), $F(1, 55) = 4.56, p < .04, \eta^2 = .08$.[2]

Exam performance

Next, we employed a 2 (Thought-Listing Instructions: counterfactual vs. open-ended) × 2 (Exam Performance: practice vs. final) repeated-measures ANOVA. This analysis failed to reveal a main effect for Thought-Listing Instructions, $F(1, 31) = 1.17$, ns. However, a main effect for Exam Performance emerged, such that performance was significantly better on the final exam ($M = .76, SD = .16$) than the practice exam ($M = .69, SD = .13$), $F(1, 55) = 18.68, p < .001, \eta^2 = .25$.

As expected, the main effect was qualified by a significant Thought-Listing Instructions × Exam Performance interaction, $F(1, 55) = 6.62, p < .02, \eta^2 = .11$ (see Fig. 2). Although, the thought-listing conditions did not differ in their performance on the practice exam, $t(55) = .19$, ns, a significant difference was obtained for the final exam, $t(55) = -3.40$, $p < .01, d = .92$, such that participants assigned to the open-ended condition performed better than their counterfactual thought-listing counterparts. Importantly, participants asked to generate counterfactuals did not improve from the practice to the final exam, $t(55) = -1.21$, ns, but participants asked to generate open-ended thoughts did improve, $t(55) = -4.86, p < .001, d = 1.31$.[3]

Enjoyment, skill, and expectations

The two thought-listing conditions failed to differ in their reports of their enjoyment and skill for the four ACT sections, as well as their expectations about the same sections on the final exam (all 12 Fs < 2.00). Thus, we did not consider these individual variables as potential mediators.

However, we reasoned that participants might have developed an overall sense of their skills that would be observed in a composite skill measure, rather than a domain-specific measure. Consistent with our guiding theoretical perspective, it should be the case that one engaged in open-ended thought-listing activity would be more likely

[2] We computed the same repeated measures ANOVA among the open-ended condition ($n = 29$) alone. Participants spent marginally less time studying those sections in which they generated a counterfactual ($M = 74.26, SD = 79.70$) than sections in which they had not ($M = 108.91, SD = 76.64$), $F(1, 28) = 1.94, p < .18, \eta^2 = .07$.

[3] We tested the same repeated measures ANOVA among the open-ended condition ($n = 29$). Although the predicted interaction only reached marginal significance, $F(1, 28) = 2.79, p < .11, \eta^2 = .09$, the overall pattern of performance means was similar to the pattern of Study 1.
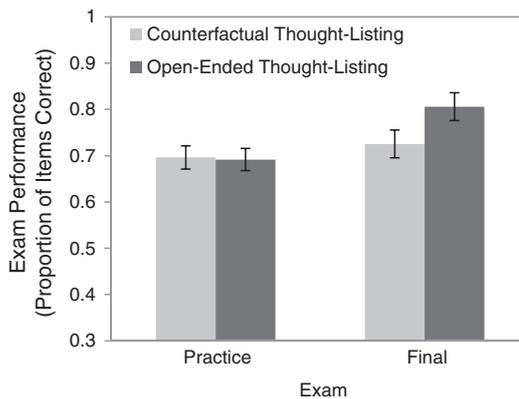
Fig. 2. Performance means by exam and thought-listing instructions (Study 2).

to take practice feedback as a cue for his/her overall skill level, in contrast to participants generating counterfactuals. Thus, we expected to find a positive correlation between performance and overall skill within the open-ended thought-listing condition, but not within the counterfactual condition. In other words, we expected participants to take the practice feedback as a cue to their competence. However, when participants generated alternative outcomes via counterfactual thinking, even relatively low performance would be associated with competence.

To assess whether thought-listing instructions and practice exam performance had the predicted interactive effect upon overall skill, we employed hierarchical regression analyses following the recommendations of Cohen, Cohen, West, and Aiken (2003), by regressing overall skill onto thought-listing instructions and performance (continuous, mean centered) and their interaction term. Main effect tests were examined in Step 1, and the two-way interaction term was examined in Step 2. This analysis revealed a marginal main effect of performance ($\beta = .22, t(53) = 1.69, p < .10, d = .46$), such that performance was positively associated with skill. A main effect of thought-listing instructions also emerged ($\beta = .30, t(53) = 2.50, p < .02, d = .69$), such that the counterfactual condition was associated with greater overall skill than the open-ended condition.

Most importantly, these main effects were qualified by the interaction between thought-listing condition and practice exam performance, $\beta = -.36, t(53) = -2.77, p < .01, d = .76$. To examine this interaction further, we calculated predicted means of perceived skill at one standard deviation above and below the mean of performance (see Fig. 3). As predicted, when participants were asked to write open-ended thoughts, perceived skill increased as performance increased, $\beta = .58, t(53) = 2.72, p < .01, d = .74$. However, when participants were asked to write counterfactuals, perceived skill was not related to performance,
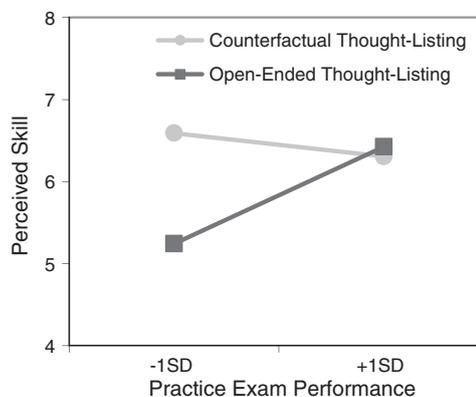

Fig. 3. Predicted perceived skill means by practice exam performance and thought-listing instructions (Study 2).

$\beta = -.13$, $t(53) = -.93$, ns. Viewed another way, when participants performed relatively well, the perceived skill of participants in the open-ended and counterfactual conditions did not differ in perceived skill, $\beta = -.06$, $t(53) = -.32$, ns. However, consistent with expectations, when participants performed relatively poorly, participants who wrote counterfactuals perceived themselves to be more skilled than their counterparts who wrote open-ended thoughts, $\beta = .66$, $t(53) = 3.72$, $p < .001$, $d = 1.02$.

Mediation analysis. To test our meditational hypotheses, we used a bootstrap procedure to construct bias-corrected confidence intervals based on 1000 random samples with replacement from the full sample, as recommended by methodologists and statisticians (Preacher & Hayes, 2004, 2008). This method tests whether or not the size of an indirect effect differs significantly from zero. First, we tested our hypothesis regarding perceived skill as a mediator of the relationship between thought-listing instructions and study time. For this, and subsequent meditational analyses, we dummy-coded open-ended and counterfactual thought-listing conditions as "0" and "1" respectively (see Fig. 4). As already discussed, thought-listing instructions significantly predicted study time, as well as our proposed mediator — perceived skill. A full model simultaneous regression revealed that perceived skill was a significant predictor, such that study time decreased with perceived skill. However, thought-listing instructions were no longer a significant predictor. The size of the indirect effect was −156.40 (SE = 114.86), and the 95% confidence interval excluded zero, 95% CI [−440.94, −7.08]. Thus, perceived skill significantly mediated the relationship between thought-listing instructions and study time.

To test study time as a mediator of the relationship between thought-listing instructions and exam performance improvement, we first calculated improvement by subtracting the practice exam correct proportion from the final exam correct proportion (see Fig. 4). As already discussed, thought-listing instructions significantly predicted exam performance improvement, as well as our proposed mediator — study time. A final simultaneous regression analysis revealed that study time was a significant predictor, such that exam performance improved with study time. However, thought-listing condition was no longer a significant predictor. The size of the indirect effect was −.04 (SE = .01), and the 95% confidence interval excluded zero, 95% CI [−.07, −.01]. Thus, study time significantly mediated the relationship between thought-listing instructions and exam performance improvement.

Taken as a whole, our results suggest that using counterfactuals to explain away incorrect exam items makes students feel more or less skilled in particular topic areas, but does not necessarily affect their expectations of subsequent performances. Subsequently, perceived skill in an academic domain does appear to affect the amount of time students are willing to study that domain. Thus, counterfactual thinking affects variables that are critical to subsequent performance. These results are consistent with Metcalfe's (1998, 2009) conclusions that students expend effort on academic activities until they believe they have reached the level of mastery. In our paradigm, counterfactual thinking apparently created a false sense of competence/mastery manifested through inhibition of direct efforts at improvement.
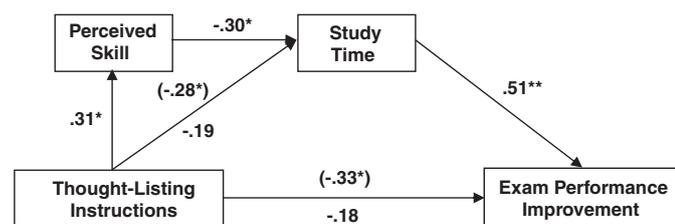

Fig. 4. Results of mediation analyses (Study 2). *Note.* Thought-listing instructions were dummy-coded using 0 for the open-ended condition and 1 for the counterfactual condition. *$p < .05$. **$p < .001$.

General discussion

Prior research has shown that counterfactual thinking can enhance task persistence and performance (e.g., Markman et al., 2006; Roese, 1994). However, we showed that counterfactuals in response to failed exam items inhibited studying relevant material—a behavior that otherwise improved subsequent performance. Study 1 showed that these effects can occur spontaneously. Study 2 confirmed the causal links between counterfactual thinking, academic behavior and performance. Participants who used counterfactual thinking to explain away their incorrect responses perceived themselves to be skilled regardless of their actual performance. Furthermore, the indirect effect of counterfactuals on academic performance appears to be partly explained by subjective estimates of skill, as this variable appears to influence the amount of time participants spent studying relevant material. Unfortunately, this tendency did not influence plans to work on solving their problems, but rather suggested that simple adjustments could be made in the future. Our data suggest that this approach is not effective.

From one perspective, our data seem to suggest that counterfactuals in reaction to failures can leave one with an optimistic view of competence in an academic domain. Interestingly, however, the work of Sanna (1996, 1998) indicates that optimism is associated with downward counterfactual thinking whereas pessimism is associated with upward counterfactual thinking. Thus, it is possible that when our participants received failure feedback they became somewhat pessimistic and subsequently put less effort into studying and the second exam (Chang, Chang, & Sanna, 2009). However, our perceived skill data suggest that counterfactuals maintained an overly optimistic assessment of their overall academic competence. Furthermore, our data are consistent with Metcalfe's (1998, 2009) findings that a subjective sense of mastery (real or not) will prevent studying behavior. In either case, counterfactual thinking in our paradigm served to impede studying and performance. Yet, future research is needed to examine the roles of state and/or trait optimism/pessimism as potential mediators or moderators of our effects.

Contrasts with prior research

Our findings clearly contrast with the conclusions of prior work that suggests counterfactuals provide global benefits for the individual. For instance, work by Morris and Moore (2000) found that enhanced learning in pilots was associated with self-critical upward counterfactuals that evoked responsible cognitive strategies for the future (i.e. specific plans to execute behavioral changes to avoid mistakes). However, given the ubiquity of egocentric biases (Girotto et al., 2007; Ross & Sicoly, 1979), we estimate that such counterfactuals are infrequently generated.

Sherman and McConnell (1995) argued that the causal ascriptions implied by one's counterfactual are not always correct. In our studies, many of our participants generated counterfactuals that seemed to reduce the size of the problem (e.g., "If only I spent more time thinking about the question…") rather than consider other possibilities (i.e., lack of knowledge or understanding). We suggest that participants in earlier research generated counterfactuals with clear prescriptions for change, whereas those we obtained were frequently characterized by hindsight bias. Our data suggest that counterfactuals may not only perpetuate "knew it all along" conclusions, but may also contribute to distorted judgments about what is necessary for improvements or promote solutions that may have no actual benefit. Consistent with the assertions of Petrocelli and Harris (2011), we contend that counterfactuals may be functional only to the extent that one's counterfactuals imply correct casual antecedents, one possesses the ability to change his/her behavior in the direction of their counterfactual prescription(s), and one is motivated to follow the prescription(s).

We suspect that the discrepancies between our findings and those of other studies (Kray et al., 2009; Roese, 1994) may also be due to differences in the correctness of the counterfactual-based causal ascriptions that tend to be generated in the different paradigms as well as the behavioral prescriptions the counterfactuals imply for the future. Specifically, we suspect that counterfactuals generated in prior studies were more correct than the typical counterfactual generated by our participants. For example, in Roese's (1994) Experiment 3, the counterfactuals of participants instructed to list upward/additive counterfactuals were most likely characterized by considerations of increasing persistence and focus. If adopted for subsequent anagrams, these approaches were likely to improve performance. In our paradigm, time and focus *during the exam* were only two of several possible inhibitors of performance, and thus, relatively less likely to be casual agents. To the extent that counterfactuals imply incorrect causal ascriptions, people may fail to alter their approaches—so critical to their subsequent performance (Petrocelli & Harris, 2011). Furthermore, incompetence in a domain appears to covary with the inability to distinguish between competence and incompetence in that domain (Kruger & Dunning, 1999). Thus, when people are inaccurate about the causes of their failures, failures are likely to persist. We suggest that one route to competence may be to first consider how one's counterfactuals, and the causal ascriptions they imply, are actually correct (see Petrocelli, Percy, Sherman, & Tormala, 2011).

Finally, given the similarities between our paradigms, our findings most strongly contrast with those reported by Nasco and Marsh (1999). Their participants listed counterfactuals immediately after receiving their grade on a psychology exam. One day prior to their next exam, participants were asked to recall their counterfactuals and report any changes they made from the first to the second exam. Upward counterfactual frequency enhanced performance on the second exam through perceived changes, prescribed by their counterfactuals, and perceived control of their exam performance.

In spite of the apparent similarities, there are important differences between our procedures. For instance Nasco and Marsh (1999) requested participants to recall their counterfactuals before a second exam; this request likely increased the salience of counterfactual prescriptions, motivating students to study more for the next day's exam. In conjunction with the present research, the work of Nasco and Marsh demonstrated that counterfactuals can either impede or facilitate academic performance — a topic that deserves further research, as discussed below.

Future directions

We do not contend that counterfactual thoughts are uniformly dysfunctional. We suggest that the functionality of counterfactuals may depend in part on their direction and structure. Counterfactuals are more likely to be generated in response to undesirable than desirable outcomes (Markman et al., 1993) and tend to be additive in response to failures (Roese & Olson, 1993). However, if we had requested thoughts to be listed following a potential study period, and after making our participants aware of the fact that our study materials were actually useful, they may have generated subtractive counterfactuals (e.g., "If only I hadn't neglected to examine the study materials…"). Such counterfactuals would seem to provide functional prescriptions for future performance.

Although our data did not afford an adequate test of the effects of downward counterfactuals, it seems possible that contemplating one's nearly incorrect exam items could signal a need for improvement and subsequently lead to increased studying behavior and performance. Indeed, Markman, McMullen, and Elizaga (2008) showed that reflective downward counterfactual thinking (i.e., focusing *only on the alternative worse than reality*) enhances persistence and performance relative to evaluative downward counterfactual thinking (i.e., comparing the

alternative to reality).[4] Furthermore, a counterfactual mind-set can benefit performance on tasks that involve the consideration of relationships and associations among a set of stimuli (Kray, Galinsky, & Wong, 2006). Markman, Lindberg, Kray, and Galinsky (2007) showed that the functionality of a counterfactual mind-set also depends on structure; additive counterfactuals appear to enhance creativity, but subtractive counterfactuals appear to enhance analytical problem solving.

Again, we believe that the experimental context is also critical to determining the functionality of counterfactuals. Future research would do well to test whether or not the contextual differences we have highlighted moderate the functionality of counterfactual thinking for learning and performance, as well as identify how these features may be further moderated by the content of counterfactuals (particularly their direction and structure).

Conclusion

The results of the current studies contribute to the ongoing debate emerging between functional and dysfunctional accounts of counterfactual thinking. We showed that counterfactual thinking can aid students in explaining away failures and maintaining positive evaluations and expectations. However, counterfactuals can also inhibit studying behaviors that would otherwise promote academic improvement. Although our results paint a dim picture for the link between counterfactual thinking and performance, our hope is that they promote greater attention to determining the task and contextual features that moderate the functionality of counterfactual thinking.

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[4] Our findings are consistent with prior theory and research suggesting that reflective, as opposed to evaluative, upward counterfactuals can induce positive evaluations, inhibit performance (see Markman & McMullen, 2003; Markman et al., 2008) and distort memory (see Petrocelli & Crysel, 2009). The evaluative and reflective modes of counterfactual thought warrant greater attention with respect to functional and dysfunctional accounts.

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