

**Depression, Control, and Counterfactual Potency: A Proposed Moderated Mediation
Model of Counterfactual Thinking and Performance**

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Depression, Control, and Counterfactual Potency: A Proposed Moderated Mediation Model of
Counterfactual Thinking and Performance

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Abstract

The functional theory of counterfactual thinking was created to explain the purpose and corresponding outcomes of counterfactual thoughts, thoughts in which individuals imagine how differences in past life events may have led to differences in their current circumstances. Though this theory predicts that the generation of upward counterfactual thoughts, in particular, where the imagined outcome is better than the actual outcome, leads to performance improvements between tasks due to its catalytic effect on behavior, evidence supporting this idea has been inconsistent. In light of this, two models were constructed and tested using an SPSS macro known as PROCESS. In these models it was hypothesized that upward counterfactual thinking would lead to performance improvements between two anagram tasks through increases in perceived control. Further, the magnitude of this enhancement effect was predicted to vary with the degree of plausibility perceived by the thinker, and would not occur for individuals experiencing depression. As both of these models were unsupported, the chosen methodology for this study was evaluated and the relationship between counterfactual thinking and performance was considered further.

Keywords: counterfactual thinking, performance, perceived control, depression

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“It’s never too late to be what you might have been.” –George Eliot¹

It is not uncommon for people to reminisce about how their life might have turned out differently. When engaging in such thoughts individuals may mentally travel back in time to undo a choice, change a behavior, or alter an event that happened, and then imagine how their life might be in the present as a result of such a change. These thoughts are known as counterfactual thoughts, thoughts that compare reality to an alternative, imagined life. Typically, these thoughts take the form of if-then statements, where the consequent is a result of some previous life event that has been mentally undone or altered (Roese, 1997). Just as George Eliot implies, researchers studying counterfactual thinking have proposed that through the process of imagining how our lives in the past and present could have been different, we discover what alternative behaviors we can engage in to achieve goals in the future (Roese, 1994).

This instructive potential has only been attributed to one kind of counterfactual thought, however. Previously, researchers categorized counterfactual thoughts according to their direction, either upward or downward (Markman, Gavanski, Sherman, & McMullen, 1993). In downward counterfactual thoughts, the imagined alternative is worse than reality. For example, following a fender-bender, someone might think, “If I had looked up at the road just a few seconds later, I might have been seriously injured in this accident.” These downward counterfactuals have been shown to trigger positive affective responses, such as a sense of relief (Roese, 1994), and have not been associated with behavioral changes.

On the other hand, in upward counterfactual thoughts an imagined alternative is better than the present reality. For example, following a fender-bender someone might think, “If only I

had not been playing with the radio, then I wouldn't have gotten in this accident.” In contrast to downward counterfactuals, these thoughts have been associated with negative emotions, often leading to dissatisfaction (Markman et al., 1993) or regret (Petrocelli, Percy, Sherman, & Tormala, 2011). If upward counterfactual thinking is so unpleasant, though, why do people continue to engage in such thoughts? In an effort to answer this question and understand the subsequent effects of these thoughts, Roese (1994, 1997) has proposed the functional theory of counterfactual thinking. Within this framework, upward counterfactual thoughts are instructive, suggesting alternative behaviors that can be used in the future to prevent one from re-experiencing negative events.

Though many aspects of the functional theory have been empirically supported, the relationship between upward counterfactual thinking and future behavior has not yet been undeniably established. For example, some researchers have recently criticized the foundational assumptions of this theory (Mercier et al., 2017). Others have proposed alternative models that extend the original functional theory beyond its focus on thought content (Myers, McCrea, & Tyser, 2014). Furthermore, though some experiments have suggested a link between upward counterfactual thinking and behavior (Roese, 1994), this link, along with the implied relationship between behavior and performance, has also not yet been firmly demonstrated.

Therefore, the aim of this study was to continue testing the functional theory of upward counterfactual thinking. Following Roese's (1994, Experiment 3) classic experiment, participants were prompted to generate upward counterfactual thoughts between two word puzzle tasks to determine the effect of upward counterfactual thinking on later performance. Similar to Myers et al. (2014), this investigation moved beyond the proposed relationship between thought content and behavior to determine if performance effects could be explained by another variable,

perceived control. Simultaneously, the generalizability of this effect was tested by assessing participants' experience of depression. In particular, those experiencing depression were expected to view their counterfactual thoughts as less plausible, minimizing gains in perceived control hypothesized to be found in others. Together, findings from this study were anticipated to not only contribute to the literature on counterfactual thinking, but also shed light on the interaction between cognitive processes and depression.

The Functional Theory of Counterfactual Thinking

To explain the evident differences in thought content and affective results of upward and downward counterfactual thoughts, Roese (1994) proposed the functional theory of counterfactual thinking. According to this theory, upward and downward counterfactual thoughts serve diverging purposes (Roese, 1994, 1997). Functionally speaking, downward counterfactuals can be used to regulate mood by boosting positive affect in a bad situation. Accordingly, research has demonstrated that downward counterfactual thoughts are frequently followed by feelings of relief (Roese, 1994), and are commonly deployed when individuals are motivated by goals of self-enhancement (White & Lehman, 2005). Conversely, Roese (1994, 1997) proposed that upward counterfactual thoughts, though they yield negative affect, illustrate for the individual what behaviors can be utilized to avoid bad situations in the future. Expanding on our earlier example, according to the functional theory, because an individual thinks he could have avoided a car accident by not playing with the radio, the next time he finds himself driving he knows not to touch the radio, thereby protecting himself from repeated harm.

Prompted by this functional understanding of counterfactual thinking, many researchers have accurately predicted when upward counterfactual thinking is likely to occur. For example, this functional explanation predicts that upward counterfactual thoughts will occur more

frequently in situations that are not only negative but are also likely to reoccur in the future. In keeping with these expectations, Markman et al. (1993) tested whether counterfactual direction would vary depending on the conditions of a blackjack task. They found that, in comparison to games that were framed as a win or neutral, when games were framed as a loss participants reported lower levels of satisfaction and generated more spontaneous upward counterfactual thoughts. Importantly, evidence also showed that upward counterfactuals were more frequent when individuals thought they were going to play blackjack twice in comparison to those who did not expect to repeat the game.

Similarly, Sanna et al. (1999) demonstrated that induction of negative moods increased individuals' likelihood of generating upward counterfactual thoughts. As suggested by these researchers, the relationship between negative affect and upward counterfactual thinking may follow a "feelings-as-information" view of emotions (e.g., Schwarz & Clore, 1983). This perspective holds that negative affect signals to the individual that there is a situation that needs to be addressed. Thus, counterfactual thinking may be one process that is triggered by negative emotions in an effort to resolve an undesirable situation. Together, these studies suggest, as implied by the functional theory, that upward counterfactual thinking may be a strategy employed in repeatable life events in order to reflect on and learn from negative outcomes and prevent their reoccurrence.

Some research has also supported a link between upward counterfactual thinking and performance. In one of the early tests of this functional theory, Roese (1994) found that upward counterfactual thinking led to greater performance improvements on an anagram task when compared to performance outcomes following the generation of downward counterfactuals. Further, upward counterfactual thoughts were found to lead to greater reported feelings of

control and intentions to change future behavior. Roese theorized that this effect on performance was a result of individuals' intentions and ability to change their behavior, as guided by the content of their upward counterfactual thoughts.

Explaining the Performance Effects of Upward Counterfactual Thoughts

Explaining how the functional theory of counterfactual thinking works, Roese (1997) has noted that thinking about what might have been can “suggest prescriptions that may facilitate success in the future” (p. 134). This explanation was applied to the performance effects previously reported by Roese (1994). In his frequently-referenced experiment (Experiment 3), participants completed two sets of ten anagrams. In order to elicit negative moods, after the first set of anagrams were completed all subjects were deceived into thinking their performance had been poorer than others. Then, before completing the second set of anagrams, individuals were prompted to think counterfactually, either in an upward or downward direction. As previously noted, a performance enhancement effect was found when participants generated upward counterfactual thoughts.

Within these anagram tasks, participants could choose to skip over words they found difficult or receive clues. However, these options cost the participant points so they could ultimately help or harm the subjects' final scores, depending on how and when they were used. Thus, individuals had to strategically decide when using these would be best. According to the functional theory, it was hypothesized that by thinking counterfactually, generating such thoughts as, “If only I had used more clues, then I would have gotten a higher score,” when participants repeated the task they would have known what strategies to use (i.e., use more clues) to improve their performance.

In this respect, Roese's hypothesis was only partially supported, however. While the analysis of participants' strategies showed that the mention of clue use in counterfactual thoughts was significantly correlated with the frequency of clues used when participants repeated the task, this pattern of results was not found in relation to skips (Roese, 1994). Furthermore, as has been noted by Myers et al. (2014), this variable was not experimentally manipulated, so a causal relationship cannot be determined. Unfortunately, few studies have been published that experimentally test the relationship between counterfactual thinking and behavior. Instead, performance is more commonly the target in research, assumed to reflect changes in behavior.

However, whether behavior changes or performance effects are tested, results of other studies have also not conclusively supported this aspect of the functional theory. Across three experiments conducted by Page and Colby (2003), participants were randomly assigned to think counterfactually about a health scare scenario. Contrary to hypotheses, it was found these participants were no more likely to schedule a medical test. Similarly, performance effects have not always supported this theory. For example, Chan, Caputi, Jayasuriya, and Browne (2013) prompted participants to think counterfactually between two Excel training tasks. These researchers reported that participants who generated upward counterfactual thoughts did not improve more between tasks than their peers, as had been predicted based on the functional theory.

In light of these inconclusive findings, alternative explanations regarding the effect of upward counterfactual thinking have been offered that move beyond the role of thought content and behavior. Specifically, Myers et al. (2014) have advocated for a content-neutral model to explain the possible relationship between counterfactual thoughts and performance. They have suggested that the comparison of current circumstances to better, imagined alternatives indicates

lack of progress toward a goal, which subsequently elicits negative affect. This, in turn, serves to mobilize effort toward resolving the task at hand. Though counterfactual thoughts may provide an individual with alternative behaviors to engage in in the future, these researchers claim that actual future behavior is not necessarily tied to the content of these thoughts. Rather, it is the comparative process between what is and what could be that provides motivation toward, and improvement on, a task.

To test this alternative explanation, Myers et al. (2014, Study 1) extended Roesse's (1994) foundational study by manipulating the structure of participants' thoughts between a series of anagram tasks. After completing the first set of anagrams and receiving false feedback about their performance, half of the study's participants were prompted to repeat a provided upward counterfactual thought three times ("If I had bought a clue after 60s for anagrams that were difficult to solve, then my total score on this test would have been better"). Alternatively, the other half of participants repeated a similar statement that took the form of an implementation intention ("Whenever I cannot solve an anagram within 60s, then I will buy a clue"). Implementation intentions are specific plans of action individuals create in an effort to overcome obstacles they expect to face in pursuit of a goal (Gollwitzer & Brandstätter, 1997).

According to Roesse's conceptualization of upward counterfactual thinking, repeating the provided counterfactual thought should have led individuals to alter their use of clues when completing the second set of anagrams. However, thought content predicted behavior only for participants who were instructed to form implementation intentions. No such relationship was found for individuals engaging in counterfactual thoughts (Myers et al., 2014).

Despite these findings, significant improvements in performance were found for participants who were prompted to repeat the upward counterfactual thoughts if they reported

negative moods. Results of a second study by Myers et al. (2014, Study 2) also indicated that participants who thought counterfactually and were of low mood persisted longer on a task than those who had not engaged in counterfactual thinking. However, once again, no link could be established between the content of participant's counterfactual thoughts and the behaviors that led to improvements in performance. Moving beyond Roese's functional theory, these results demonstrate one way counterfactual thinking may impact performance without directly affecting behavior.

One criticism of the model proposed by Myers et al. (2014), though, is that it does not explain why counterfactual thoughts are structured as two part, if-then statements. If individuals are motivated purely by the comparison between what is and what could be, then following a failure all one would have to think about is the alternative outcome they desired. Yet, counterfactual thoughts are more complex and individuals commonly explore more than one way an alternative outcome could have occurred. Therefore, while Myers et al. offered a compelling model, they did not provide a complete explanation of the function of upward counterfactual thoughts.

However, a compromise between these two models may be possible. Though upward counterfactual thoughts may not dictate future behavior, they may still act as functional indicators, particularly in the realm of perceived control. Numerous studies have already suggested that a link between counterfactual thinking and control exists. Roese and Olson (1995) found that in the face of controllable situations, individuals were more likely to generate upward counterfactual thoughts than downward counterfactuals. Similarly, Markman, Gavanski, Sherman, and McMullen (1995) found that individuals who were prompted to think counterfactually after playing a game of chance were more likely to focus their thoughts on the

elements of the game which were within their control. It has also been reported that engaging in upward counterfactual thinking leads to feelings of greater perceived control (McMullen & Markman, as cited in McMullen, Markman, & Gavanski, 1995).

While these results were previously believed to be linked with Roese's functional theory, in which counterfactuals highlight behaviors that can be modified to achieve an alternative outcome, a slightly different take on these findings can tie together this functional theory with the later content-neutral model of counterfactual thinking. This focus on control may suggest that upward counterfactual thoughts functionally indicate the degree to which an event is perceived to reside in one's personal control. In other words, in the face of failure an individual may mentally explore different sequences of events that could have led to an alternative outcome in an effort to determine how easily an outcome could have been avoided or changed. These feelings of control may, in turn, influence motivation in the face of a repeated event.

It is relatively well known that control is positively related to motivation toward a task. Rotter (1966) long ago reported on numerous studies indicating that when individuals felt they had personal or internal control over their environment, rather than being externally controlled, they were more inclined to be motivated to pursue a goal. Similarly, Bandura (1977, 1997) outlined the role self-efficacy, or the belief that one can achieve a desired goal, in motivation and behavior. Pintrich and DeGroot (1990) have since extended Bandura's concept by illustrating how students' self-efficacy positively contributed to their educational achievements.

Further, Eccles and Wigfield (2002) have combined these and other theories together under the expectancy-values model of motivation. According to this model, motivation is at its peak when individuals believe an outcome is desirable as well as achievable. Therefore, it is hypothesized that the content of an individual's counterfactual thoughts indicate to the individual

whether an outcome is possible, which further contributes to the regulation of motivation toward a desired outcome. This mediation effect would explain why individuals who have generated upward counterfactual thoughts have not only felt greater control, but have also demonstrated improvements in performance.

If this new understanding is indeed true, it is expected that individuals' feelings of control would vary depending on the degree to which their counterfactual thoughts suggest control over an event is possible. With each counterfactual thought, a new hypothetical scenario is explored, allowing the individual to determine if control is possible. In line with this reconceptualization, Petrocelli et al. (2011) have already demonstrated how the effects of counterfactual thoughts vary according to their plausibility. According to these researchers, a counterfactual's "potency", or magnitude of cognitive, emotional, and behavioral outcomes, depends on how much its content is determined to be plausible by the thinker. Petrocelli et al. suggest the thinker independently evaluates each of these two components of a counterfactual thought, the "if" and "then" portion, for its probability of occurring. Together, these two judgments serve a multiplicative effect. A counterfactual thought will most significantly impact affect, cognition, or behavior when both the "if likelihood" and the "then likelihood" are high.

To illustrate this, we can imagine some possible counterfactuals that may arise following a car crash. For example, one may think, "If only I had driven a tank, then I wouldn't be in this car accident." Though it may be highly likely that an accident would have been avoided in these circumstances, due to the implausibility of the initial portion of this causal statement, according to Petrocelli et al.'s work, this counterfactual will not be particularly potent. Likewise, one may think, "If only I had worn a different shirt today, I wouldn't have gotten into this accident." In this case the "if likelihood" is high; it is very possible that wearing a different shirt could have

occurred. However, even if this had happened, it is unlikely that this change would have prevented a car crash. Therefore, the “then likelihood” is low and once more the counterfactual would theoretically have a diminished impact. While these examples take an exaggerated form of the counterfactuals that individuals might typically generate, they model the way such thoughts can vary in plausibility and, thus, potency.

Across four separate studies, Petrocelli et al. (2011) consistently found a relationship between the perceived likelihood of counterfactual thoughts and their affective and cognitive outcomes. Most notably, after reflecting on a hypothetical scenario about a game show contestant, only when participants judged both the “if likelihood” and the “then likelihood” of their counterfactuals to be relatively high did they report that the character in the scenario was responsible for his loss. As participants in this case were not reflecting on their own experiences, these findings do not definitively demonstrate that the generation of plausible upward counterfactual thoughts will lead to increases in perceived control; however, they provide preliminary evidence for this hypothesized relationship. McMullen and Markman (as cited in McMullen et al., 1995) have also reported similar findings; these researchers described one study in which the plausibility of participants’ counterfactuals impacted feelings of perceived control.

While the characteristics of any one counterfactual thought may objectively appear to lend themselves to being more or less plausible, Petrocelli et al. (2011) are careful to point out that the likelihood of any alternative scenario of occurring is *subjectively* determined. Thus, what may be a functional process for many, leading to the performance enhancing effects found by Roese (1994), this also suggests that upward counterfactual thinking is not an inherently objective system and may be subject to dysfunction. Given the cognitive tendencies of individuals with depression, it is likely these individuals will not gain a sense of control through

upward counterfactual thinking as others do, hindering performance enhancement effects for this group.

Counterfactual Thinking in Individuals with Depression

Depression is most simply characterized by the presence of a persistently low mood and a significant loss of pleasure or interest in activities that were once enjoyable. Additionally, many other symptoms have been known to accompany this significant mood shift, including physiological changes, rumination, irritability, and feelings of worthlessness and hopelessness (American Psychiatric Association, 2013). In an attempt to understand the development of this disorder, researchers have explored the complex interplay between cognition, emotion, and behavior as they relate to chronic feelings of despair. Together, the results of these investigations illuminate some reasons why individuals experiencing symptoms of depression may regard their counterfactual thoughts as particularly implausible.

One of the initial theories postulated in an attempt to explain the development of depression was the theory of learned helplessness. Initially, this theory was proposed to explain why motivation in both humans and animals severely declined after being continuously exposed to an undesirable and uncontrollable stimulus (Maier & Seligman, 1976). Extending this idea, Abramson, Seligman, and Teasdale (1978) described the dimensions of causal attributions that lend themselves to feelings of helplessness. In particular, in the face of failure, causal attributions made by depressed individuals are commonly internal, global, and stable, suggesting to an individual that failure is not only inevitable but also likely across multiple domains. The result of these attributions is a state of helplessness that gives way to both a depressive affect and loss of self-esteem, which is also common to individuals with depressive disorders (Abramson et al., 1978).

Others have similarly suggested that individuals' attributional styles contribute to their development of depression. Beck and Alford (2009) described the "cognitive triad" of the depressed as a combination of negative views of the world, negative views of the self, and negative views of the future. These attributional styles have long since been associated with depression symptoms (Sweeny, Anderson, & Bailey, 1986), with more recent research supporting the causal relationship between attributions and depression (Ledrich & Gana, 2013).

Outside the realm of depression, researchers have demonstrated how attributional traits yield particular motivational outcomes. For example, Weiner, Heckhausen, Meyer, and Cook (1972) explored how attributions of stability significantly impacted the expectations of students. These researchers reported that in a sample of high school students, those who believed an academic failure was the result of ability or task difficulty, both stable attributions, were more likely to expect failure in the future, in comparison to individuals who attributed their failure to unstable factors. Therefore, the attributional biases of individuals with depression may affect the plausibility of their counterfactual thoughts, limiting the performance improvements found in individuals without depression.

To illustrate how the cognitive biases of individuals with depression may undermine the perceived plausibility of one's counterfactual thoughts, we can once again imagine a scenario involving a car accident. Following this accident, the driver may attempt to alleviate the discomfort of the undesirable outcome by imaging, "If only I wasn't playing with the radio, I wouldn't have gotten in this accident." Calling on Petrocelli et al.'s formula for determining plausibility, this thinker would have to believe that it was not only likely he would not play with the radio but also that avoiding a car accident was a possibility. However, as individuals with depression often characterize their world as stable, if this individual was suffering from

depression, he may be particularly uninclined to believe that an alternative outcome was plausible.

Furthermore, as noted, individuals with depression commonly make internal causal attributions (Sweeny et al., 1986). Therefore, if this driver were depressed he would more likely think, “If I wasn’t such a terrible person, I wouldn’t have gotten in this car accident.” Such a statement would also diminish the plausibility of the counterfactual statement. Although one may truly believe that he would have been able to avoid the accident if he had been a better person, this internal and stable attribute does not lend itself to an imagined scenario that is highly plausible. So, while upward counterfactual thinking may increase motivation and performance in many individuals, this outcome is less likely for those with depression.

The performance effects of counterfactual thinking for those with and without depression has yet to be empirically tested. However, other studies investigating the dynamics of cognition and performance provide further support for this hypothesis. In one study of the relationship between depression and performance, following research conducted by Dweck (1996) on the individual theories of intelligence, Da Fonesca et al. (2008) measured the degree to which participants felt intelligence was a malleable or fixed trait. It was found that individuals holding a fixed view of intelligence were more likely to show signs of depression. Furthermore, fixed views of intelligence predicted deficits in school performance.

Similarly, views of stability have been shown to affect the outcomes of counterfactual thought. Though they were not targeting depressed individuals, Dyczewski and Markman (2012) investigated the relationship between Dweck’s (1996) individual theories of intelligence and counterfactual thinking. Extending Roese’s classic anagram study, Dyczewski and Markman (2012) found that individuals ascribing to the malleable view of intelligence improved more

between two anagram tasks than those holding the fixed view. It was hypothesized that the individuals in this study who held malleable views of intelligence found their counterfactuals to be more plausible, allowing them to profit from their counterfactual thoughts where those holding a stable view of intelligence could not.

Lastly, providing preliminary evidence for this hypothesis, in a study of counterfactual thinking in individuals with depression, Markman and Miller (2006) rated the “reasonableness,” of participants’ thoughts. These researchers defined “reasonability” as the extent to which the content of each counterfactual “could have been controlled by the actor at that time” (p. 216). When compared with individuals showing signs of depression, it was found that non-depressed participants generated more reasonable counterfactual thoughts. Of course, though these studies suggest the outcomes of counterfactual thinking may be different for those with depression, research is needed to confirm if this is the case.

Research Goals and Hypotheses

Though much is known about the antecedents and consequents of counterfactual thinking, many aspects of this thinking process have yet to be fully understood. Roese’s (1994, 1997) functional theory of counterfactual thinking has provided a foundation for future research. In keeping with performance enhancements effects originally reported by Roese (1994), it was hypothesized that individuals in this experiment who engaged in upward counterfactual thinking would improve more between two anagram tasks than those who did not engage in such thoughts.

However, due to the lack of substantial evidence supporting Roese’s behavioral explanation for the link between upward counterfactual thinking and performance (e.g., Page & Colby, 2003) alternative explanations, such as those proposed by Myers et al. (2014), may be

more accurate. Thus, in this study it was postulated that performance effects found in the past were a result of control being re-established through upward counterfactual thoughts. Following this proposed explanation, and further supported by the established relationship between counterfactual thinking and control (e.g., Roese & Olsen, 1995), it was hypothesized that perceived control would mediate the relationship between upward counterfactual thinking and performance. Individuals generating upward counterfactual thoughts were expected to feel greater control over a task compared to those who did not think counterfactually. In turn, this increased sense of control was expected to lead to greater improvements in performance on the anagram task.

Additionally, this relationship between upward counterfactual thinking, control, and performance, was assumed to be conditional. While upward counterfactual thoughts functionally serve as a means of re-establishing control after a negative outcome, this is dependent on the degree of plausibility of the generated counterfactual thoughts. One group of individuals who were predicted to judge their upward counterfactual thoughts as less plausible were individuals experiencing symptoms of depression. Thus, it was hypothesized that upward counterfactual thoughts would only yield a sense of perceived control over the task for individuals not reporting symptoms of depression. Furthermore, individuals reporting symptoms of depression were not predicted to demonstrate performance improvements after counterfactual thinking that individuals without depression would. Overall, this final moderated mediation model (see Figure 1) was tested using regression-based analyses as outlined by Hayes (2013).

As noted, the lack of performance improvements for those exhibiting symptoms of depression was expected to result from the perceived implausibility of these thinkers' counterfactual thoughts. It was therefore hypothesized that counterfactual plausibility, as

measured using methods established by Petrocelli et al.'s (2011), would differ for those with and without depression symptoms. In particular, it was predicted that individuals reporting more symptoms of depression would rate their counterfactual thoughts as having either lower "if likelihood" or "then likelihood" or both. As the attributional style of individuals with depression could undermine either component of the counterfactual statement (i.e., the "if" or "then" component), no specific predictions were made about which part of the counterfactual would be believed to be less plausible.

While the plausibility of counterfactual thoughts was expected to be used as a tool to explore the counterfactual thinking process of individuals with depression, a similar relationship between counterfactual potency and performance was expected to hold for all individuals, regardless of reported depression symptoms. Specifically, it was hypothesized that the higher participants rated the plausibility of their counterfactual thoughts, the more potent these thoughts were expected to be, yielding greater improvements between two anagram tasks. Further, perceived control was once more expected to mediate this relationship. This secondary model was also tested using Hayes's (2013) method of testing mediator variables (see Figure 2).

Roese (1994, 1997) initially proposed the functional theory of counterfactual thinking. Since initial reports on this theory were published, this has been used as the foundation for many later studies on counterfactual thinking. Therefore, fully testing the elements of this theory is of the utmost importance. Further, though Petrocelli et al. (2011) have provided evidence for the influence of perceived likelihood of counterfactual thoughts on cognitive, emotional, and behavioral outcomes, few studies have considered this variable when exploring counterfactual thinking. However, if this is a key component in the counterfactual thinking process, it must be incorporated into research in a more consistent fashion.

Finally, as previously noted, little is known about the relationship between counterfactual thinking and depression, particularly as it relates to performance. Determining whether or not this process differs in individuals with depression may not only contribute to what we know about counterfactual thinking, but may also better identify why those with this disorder have deficits in motivation. As is often the case with any research on psychological disorders, knowing more about how this disorder functions has the potential to positively guide strategies for assisting afflicted individuals. Beyond this, understanding counterfactual thinking can provide larger clues toward understanding the very nature of interactions between affect, thinking, and behavior.

Method

Participants

Brief advertisements were placed on Facebook to recruit participants for this study. Simultaneously, students from a university in the North-Eastern United States were notified of this open study via an email listserv; psychology students attending this university could use research participation to fulfill undergraduate degree requirements. All individuals who joined the study were entered into two raffles to win a \$50 gift certificate and completed all research tasks online.

One hundred sixty individuals began the study; 28 of these individuals terminated their participation early, did not adequately complete the measures, or did not properly follow the directions and were subsequently removed. Therefore, a total of 132 participants (101 females, 27 males, and 4 individuals who chose not to disclose their gender) completed the study. These participants ranged between the ages of 18-79 ($M = 22.52$, $SD = 7.41$) and most (87.12%) had at least some college education. Over half of the participants (56.82%) were between the ages of

18-21 and had completed some college, suggesting that a large portion of the sample was recruited from the college. Additionally, nearly one in five participants (18.85%) showed evidence of severe depression symptoms.

Materials and Procedure

Each participant completed two trials of an anagram task based on the methodology described by Roeser (1994, Experiment 3). After giving their consent, individuals were informed that they would be partaking in a study of decision-making under time pressure (see Appendix A for full instructions). Instructions defined an anagram as “a string of scrambled letters” and informed participants that the goal of their task was to determine what real word these letters formed. As also noted in the instructions, all anagrams used in this study had only one solution. Finally, individuals were informed that they would complete two separate sets of anagrams, each comprising of ten words.

For each correctly solved anagram participants were awarded up to 120 points. These points corresponded to the 2 minutes (120 seconds) allotted to solving each anagram. One point was lost for each second spent figuring out the correct solution. Thus, as also described in the provided instructions, if a subject took 30 seconds to solve an anagram, 90 points would be awarded for that word. Points were also deducted if an anagram was skipped over without being solved; one point was lost for each second spent on an anagram that was not solved. Finally, participants had the option of “buying” one clue per anagram. If a participant chose to use a clue, the center letter of the solution would appear and 30 points would be deducted from the total score. To motivate all participants to do their best on the task, they were also told that the individual who received the highest score out of all participants would win a \$50 gift certificate.

After being provided with these instructions, participants solved three practice anagrams to confirm their understanding of the task. Following this practice period, all subjects were allowed to selectively alter three parameters of the first anagram trial. These parameters were included to allow participants to feel a degree of control over their game playing and were expected to provide choices to reflect on later when prompted to think counterfactually about their performance. First, participants selected how long of a time delay there would be between each anagram, with options ranging from 1 to 9 seconds. Next, they chose the topic of the list of anagrams, either “People and Events” or “Science and Nature.” Finally, participants selected the difficulty of their anagrams, either Difficult or Easy. They were told that they would be awarded an additional 10 points for each correctly solved anagram if they chose Difficult. This final parameter, however, was a dummy category; no extra points were awarded and all words were equally challenging regardless of the parameters chosen.

Once participants selected their parameters they began their first set of ten anagrams. Each anagram was displayed onscreen above a box in which subjects could type in their solutions. If an incorrect solution was entered, a feedback message appeared and instructed participants to “Please try again”; the next anagram would not appear unless subjects entered the correct solution, chose to skip the word, or ran out of time. “Skip” and “Clue” buttons also appeared on screen along with a timer that counted down from 120 by seconds and milliseconds.

Following Roese’s (1994, Experiment 3) initial study, four lists (two for each topic) of ten anagrams were chosen based on Gilhooly and Johnson’s (1978) research on anagram difficulty. Gilhooly and Johnson found that anagram difficulty was determined by pronounceability, anagram-word similarity, and bigram rank; therefore, each word list was equally matched on these variables (see Appendix B for complete anagram lists). Also, all word

solutions were five-letters in length, began with a consonant, and were not plural. In the first anagram trial, one of the two lists within the chosen topic was randomly assigned to the participant and anagrams were presented in random order.

After the first trial was completed all participants were presented with their final scores, the total number of correctly solved anagrams, the total amount of words skipped, and the total amount of clues used. Along with this information, false “failure feedback” was also displayed. Fake scores were calculated that were 53 points and 89 points higher than the subject’s score and were presented as the average score and highest score of all other participants, respectively. All participants, regardless of their final score, were also told that their rank was “8th (out of 19)” and that their score was “average.” This false feedback was meant to induce the sense of failure and negative affect that typically prompts counterfactual thought. Upon receiving this feedback, participants reported how satisfied they were with the outcome of this task using a 5 point scale that ranged from *Extremely dissatisfied* to *Extremely satisfied* (see Appendix C).

Next, participants were prompted to partake in a free-thought listing task for 1 minute. In this task they were asked to record any thoughts they had upon receiving their results (see Appendix D). The thoughts listed by each participant were later read over by two independent coders to determine the presence of counterfactual thoughts. Following this task subjects were randomly assigned to one of two conditions, either an upward counterfactual thinking or an empty control group. Therefore, this thought listing task was used to weed out individuals who generated spontaneous counterfactual thoughts and remove them from analyses, preventing them from being falsely included in the non-counterfactual control group.

After the thought-listing task, subjects not assigned to the control group were asked to complete three upward counterfactual thoughts relating to their performance on the anagram

task. Participants were provided with the statement, “If only . . ., I would have performed better on this anagram task” three times and asked to fill in the blank with three different responses (see Appendix E for full instructions). After completing these statements their level of “if likelihood” (IL) and “then likelihood” (TL) were assessed for each statement (see Appendix F for full instructions). To measure IL, participants were first shown the portion of the statement they filled in. Then, for each of the three counterfactuals they were asked to report how likely they thought this possibility was of happening. Next, to measure TL, for each of the three counterfactuals participants were asked to report how likely they would have performed better if the filled in portion of the statement had been true. Both IL and TL questions were answered using a 9-point scale (1 = *Extremely unlikely*, 9 = *Extremely likely*).

Before beginning the second set of anagrams, all participants were next asked how much they felt they had control over their performance on the task using a 9-point scale (1 = *No control*, 9 = *Total control*; see Appendix G). Then, participants once again had an opportunity to set the parameters for the second anagram trial. If the subject chose to select the same topic category as was chosen in the first trial, the other list of anagrams for that topic was be used. On the other hand, if the subject chose to switch topics, one of the two anagram lists of that topic was randomly selected for this second set. Following the completion of this second set of anagrams, subjects once again saw their score, number of correct solutions, number of words skipped, and number of clues used. However, no false feedback was provided at the conclusion of this repeated task. Otherwise, all particulars of Trial 1 and Trial 2 remained the same.

Once participants finished the second trial of anagrams they were asked to report on their experience of depression symptoms by completing the Beck Depression Inventory-II (BDI-II; Beck, Steer, & Brown, 1996). The BDI-II is comprised of 21 items corresponding with 21

different symptoms of depression. Subjects were asked to report, on a 4-point scale (0-3), the degree to which they have experienced each symptom over the previous two weeks. Final scores on the BDI-II could range from 0-63, with higher scores indicating greater presence of depressive symptoms. This measure has previously demonstrated high internal consistency ($\alpha = 0.93$) in a sample of college students (Beck et al., 1996).

Lastly, upon completion of the BDI-II, along with a brief demographic questionnaire, participants were asked to report the degree to which they believed their Trial 1 score and the reported average score of other participants in the study. Individuals were also asked how seriously they took their participation in the study. As participants were completing the experiment online, and could not be directly monitored by a researcher, responses to these items were used to confirm the success of the false failure feedback and assess each individual's investment in following the study protocol. Following completion of these final items, all participants were debriefed and dismissed.

Coding

To determine if participants were spontaneously generating counterfactual thoughts following the first anagram trial, their reported thoughts were coded for the presence of counterfactual thoughts. Each participant's spontaneous thoughts were coded by two independent coders who were blind to the group and depression level of the individuals. Coders were familiar with the concept of counterfactual thinking and rated each response dichotomously for the presence of a counterfactual thought. Coders agreed 92.42% of the time and demonstrated moderate agreement according to benchmarks outlined by Landis and Koch (1977), $k = .55$, $p < .001$. Discrepancies in coding were resolved by the lead researcher.

Ten individuals mentioned a counterfactual thought during the spontaneous thought listing task; these individuals were removed from subsequent analyses. Individuals that did and did not spontaneously mention a counterfactual thought did not significantly differ by age, gender, level of education, or presence of depression symptoms. Further, these two groups did not have significantly different Trial 1 scores or differ in their reported level of satisfaction with the outcome of the first anagram trial.

Results

Anagram Task Performance

During both anagram trials, participants from both the counterfactual thinking and control conditions chose similar settings for the task. As shown in Table 1, before each trial participants selected one of the two topic categories with similar frequency. Also, prior to both trials, slightly more than half of the individuals selected a difficulty level of “Easy.” Neither of these trial settings were found to significantly differ by condition. Furthermore, while engaging in the task, participants from both conditions utilized similar strategies, using roughly two clues and skipping roughly two words each trial.

Just as no differences were evident between conditions, typically, participants’ strategies did not vary by demographic characteristics. However, some significant relationships between variables were found. For example, results demonstrated that, in comparison to male participants, a significantly higher portion of female participants selected a difficulty of “Easy” in both Trial 1, $\chi^2(1) = 12.41, p < .001$, and Trial 2, $\chi^2(1) = 10.65, p < .01$. Additionally, correlations were tested between each of the quantitative task strategies and study outcomes, including participants’ use of skips and clues. As can be seen in Table 2, in both trials participants’ use of skips was inversely related to reported presence of depression symptoms. Aside from these

relationships, though, no other significant associations between task strategies and demographic variables were evident.

On average, in the first anagram trial participants correctly solved 7.55 ($SD = 1.95$) out of the 10 anagrams and scored a total of 630.62 ($SD = 354.97$) out of 1200 possible points. The presence of one outlier, whose score fell over three standard deviations below the mean, contributed to the non-normal distribution of participants' Trial 1 scores, $W(122) = 0.95$, $p < .001$. Though removing this outlier minimized issues of kurtosis and skew, the distribution maintained a mild negative skew as the result of a ceiling effect. However, as no evidence existed to support removal of this outlier, it was retained within the data. Given this distribution, a Mann-Whitney U test was used to compare Trial 1 scores between conditions. Results demonstrated that scores did not significantly differ between counterfactual thinking and control groups, $U = 1856.50$, $p = .99$; these results were consistent whether the outlier was removed or retained.

While scores were not found to vary by condition, participants' Trial 1 scores significantly differed between difficulties and education levels. Participants who selected a difficulty level of "Easy" (mean rank = 55.07, $Mdn = 579$) scored significantly lower than those who selected "Difficult" (mean rank = 70.45, $Mdn = 841$), $U = 1354.00$, $p < .05$. Also, results of a Kruskal-Wallis H test demonstrated that participants who had not completed any college (mean rank = 41.03, $Mdn = 390$) scored significantly lower than those who had completed some college (mean rank = 65.19, $Mdn = 733$), $\chi^2(2) = 6.67$, $p < .05$. Those who reportedly completed a college degree and/or higher degrees did not significantly differ from either of these groups, $p > .05$.

Reflecting on their performance, less than half (40.98%) of the participants reported that they were dissatisfied with the outcome of the first anagram trial. Though participants were shown “false feedback” to elicit dissatisfaction, nearly one third (31.97%) of participants reported that they were satisfied with the outcome of Trial 1. Satisfaction levels were found to significantly correlate with Trial 1 scores, $r_s(120) = .32, p < .001$, with those scoring higher also reporting greater levels of satisfaction.

Outcome satisfaction was also found to significantly correlate with participants’ perceived control over the second trial of the anagram task. Overall, participants reported a moderate level of control ($M = 4.52, SD = 1.85$) before completing the second set of anagrams, with those who were more satisfied with Trial 1 scores reporting significantly higher levels of perceived control, $r_s(120) = .34, p < .001$. Neither outcome satisfaction nor perceived control were found to significantly differ between counterfactual thinking and control groups.

Compared to scores from the first trial, participants’ scores in the second trial were significantly higher, $t(121) = 2.20, p < .05$, with participants’ scores increasing by an average of 63.75 ($SD = 319.69$) points. In the second trial, participants correctly solved an average of 7.84 ($SD = 2.05$) anagrams and scored a total of 694.38 ($SD = 296.70$) points. As in the first trial, scores in the Trial 2 were not normally distributed, $W(122) = 0.97, p < .01$. While no outliers were apparent in second trial scores, a ceiling effect was again evident. Scores in the second trial were also not found to significantly differ by condition, $U = 1740.50, p = .56$.

Once more, scores in the second trial were also found to vary by participant’s selected difficulty. Participants who selected “Easy” (mean rank = 54.36, $Mdn = 628$) scored significantly lower than those who selected “Difficult” (mean rank = 73.72, $Mdn = 863$), $U = 1182.50, p < .05$. Additionally, while scores in Trial 2 were not found to vary by education as they had in

the first, they did significantly relate to presence of depression symptoms. Those who scored higher on the BDI-II received higher scores in the second trial of anagrams, $r_s(120) = .20, p < .05$.

To preliminarily explore the cause of participants' improved scores, task settings and strategies used in Trial 1 and Trial 2 were compared. Between trials, 34.42% of participants switched topic categories and 14.76% changed their difficulty. While changes in difficulty did not appear to relate to Trial 1 outcomes, participants who chose a different topic in the second trial (mean rank = 48.30, $Mdn = 502.50$) scored significantly lower in Trial 1 than those who picked the same topic in both rounds (mean rank = 68.43, $Mdn = 750.50$), $U = 1125.50, p < .01$.

Furthermore, over 60% of the participants utilized different numbers of skips and clues in each round. For each participant, change in skip and clue usage was computed by subtracting Trial 1 total skips and clues from total skips and clues in Trial 2. Change in skip usage was found to significantly correlate with Trial 1 scores, $r_s(120) = .48, p < .001$; participants who scored higher in the first trial increased the amount of words they skipped in the second trial. A similar relationship was found for change in clue usage. Participants who scored higher in Trial 1 were also found to use significantly more clues in Trial 2, $r_s(120) = .38, p < .001$.

These changes in strategy, however, did not necessarily yield an increase in score. A change score was computed for each individual by subtracting the participant's first trial score from their second trial score. This change score was then compared for those who did and did not switch strategies between rounds. These analyses revealed that those who switched topics or difficulties did not have significantly different change scores than those who did not alter these settings. However, changes in skip and clue usage were found to inversely relate to change scores. Participants who skipped more words in the second trial were found to have significantly

lower change scores, $r_s(120) = -.85, p < .05$. Similarly, participants who increased their use of clues in Trial 2 had significantly lower change scores, $r_s(120) = -.53, p < .001$. Lastly, beyond these changes in strategy, change scores were also found to be significantly and inversely related to satisfaction with Trial 1 outcomes, $r_s(120) = -.22, p < .05$.

Counterfactual Thinking Task

Roughly half of the participants (51.64%) were randomly assigned to complete three counterfactual statements between anagram trials. Most participants (84.13%) completed all three counterfactuals, with the remaining completing only two (11.11%) or one (4.76%) of the statements. Therefore, in total, 176 counterfactual statements were completed by participants.

Most commonly, counterfactual statements described a hypothetical change in the conditions of the anagram task itself (39.20%; e.g., “the letters were not in all caps”), noting that such a change would have led to a better performance in Trial 1. Also common, counterfactual statements often illustrated a change in participants’ current physical, mental, or environmental state. Overall, 27.27% of the counterfactual statements were completed with a hypothetical change in state (e.g., “I wasn’t hungry”).

The remaining counterfactual statements related to participants’ aptitude toward the task or use of a specific task strategy. In 19.32% of the statements, participants either explicitly noted that they would have done better had they naturally been better at the task (e.g., “I was better at unscrambling words”) or otherwise implicitly commented on their aptitude by noting how regular engagement in other behaviors would have improved their skills in the task (e.g., “did more word puzzles”). Alternatively, counterfactual statements relating to participants’ strategies (14.20%) referred to a specific choice or behavior that had been used in the first anagram trial

that, had it been different, would have resulted in a better performance (e.g., “I had reset my hands on the keyboard before each anagram was shown”).

Following the procedure detailed by Petrocelli et al. (2011) to determine the potency of counterfactual statements, participants’ “if likelihood” and “then likelihood” scores for each counterfactual statement were multiplied. Then, the potency of each statement was averaged, based on the number of counterfactual statements that were completed, yielding a composite counterfactual potency score for each participant. Across all participants, the average of these counterfactual potency scores was 41.06 ($SD = 17.80$).

Through preliminary analyses, composite counterfactual potency scores were found to be significantly higher for participants who selected the science and nature category for Trial 1 ($M = 45.64$, $SD = 17.58$) than those who selected the people and events category ($M = 36.34$, $SD = 17.04$), $t(61) = -2.13$, $p < .05$. However, counterfactual potency was not found to relate to any of the other measured demographic variables or Trial 1 settings, strategies, or outcomes. Counter to expectations, evidence did not support the hypothesis that individuals exhibiting greater depression symptoms would report lower counterfactual potency scores, $r_s(61) = -.01$, $p = .95$.

Moderated Mediation and Mediation Models

BDI-II scores were analyzed to determine if the sample consisted of an adequate number of participants exhibiting depression symptoms to explore the impact of depression on counterfactual thinking and performance. Based on benchmarks described by Dozois, Dobson, and Ahnberg (1998), 18.85% of participants fell into the category of “dysphoric-depressed,” with an additional 19.67% exhibiting milder levels of depression (i.e., “dysphoric”). The remaining participants were determined to be “nondepressed.” When the distribution of participants among these three categories were compared with those of a previous study conducted by Whisman and

Richardson (2015), it was found that this sample contained a significantly higher proportion of depressed participants, $\chi^2(2) = 10.05, p < .01$. Therefore, it was concluded that the sample contained a sufficient number of participants experiencing symptoms of depression to statistically test the hypothesized model.

Based on the conceptual model depicted in Figure 1, a regression-based model including predictors of counterfactual thinking, perceived control, and depression symptoms, along with an interaction term, was constructed first (see Figure 3). Following procedural guidelines set forth by Hayes (2013), the validity of this model was subsequently tested using a macro known as PROCESS. Through 10,000 bootstrapped samples, this add-on to SPSS tested both the direct effect of counterfactual thinking on changes in performance, as well as the conditional indirect effect of counterfactual thinking on performance, as mediated by perceived control and moderated by presence of depression symptoms.

Breaking down this model into its component parts, the first relevant question that was explored was the direct effect of counterfactual thinking on change scores. This is represented by coefficient c' in Figure 3. Counter to previous findings in support of the functional theory of counterfactual thinking, no direct effect of counterfactual thinking on performance changes were found (see Table 3). In other words, participants who engaged in counterfactual thinking between trials did not improve significantly more than those randomly assigned to the control group.

Though traditional mediation analyses would necessitate a direct relationship between two variables as a precondition to the exploration of the mediating variable(s) between them, it has since been understood that conditions exist where an indirect relationship between variables may exist despite no known direct relationship (Hayes, 2013). Therefore, though counterfactual

thinking did not appear to directly affect performance, the conditional indirect effect of perceived control and depression symptoms on the relationship between these two variables was explored next.

Based on coefficients illustrated in Figure 3, the conditional indirect effect of counterfactual thinking on change scores was signified by the following equation: $(a_1 + a_3 \times \text{Depression symptoms})(b)$. Further deconstructed, this equation represented the conditional indirect effect of counterfactual thinking on performance as product of counterfactual's conditional effect on perceived control $(a_1 + a_3 \times \text{Depression symptoms})$, as moderated by depression symptoms, and perceived control's subsequent effect on performance (b) . As is evident in Table 3, none of the aforementioned variables were statistically significant, demonstrating that no conditional indirect effect of counterfactual thinking on performance changes was found. These findings were consistent when the outcome variable was changes in the amount of correctly solved anagrams between Trial 1 and Trial 2, instead of changes in scores. Therefore, results did not support the first hypothesized model.

Despite no known relationship between counterfactual thinking and performance, the second proposed model of counterfactual potency on performance was also tested. While counterfactual thinking may not have increased participants' performance above that of their peers, it is possible that counterfactual potency represents a necessary condition for the performance enhancing effects of counterfactual thinking. Therefore, as hypothesized, the effect of counterfactual thinking on performance was expected to work as a function of counterfactual potency, which was further predicted to be mediated by perceived control (see Figure 2).

This secondary mediation model was tested using the same procedure noted earlier; however, only the 63 participants assigned to complete the counterfactual thinking task between

anagram trials were included in this analysis. Results demonstrated that this model, statistically represented in Figure 4, also yielded no significant effects. A direct effect of counterfactual potency on changes in performance was not found (see Table 4). Furthermore, no significant indirect effect of counterfactual potency, as mediated by perceived control, was evident. When the model was re-tested, considering changes in correctly solved anagrams between Trial 1 and Trial 2 as the outcome variable instead of changes in scores, results were the same.

Discussion

Contrary to expectations, participants' performance in this study did not vary depending on their counterfactual thinking. Though this does not follow Roese's (1994, 1997) functional theory of counterfactual thinking, these results are also not entirely without precedence. In Roese's (1994, Experiment 3) initial test of his functional theory, participants who generated upward counterfactual thoughts only improved more between two tasks in comparison to those who generated downward counterfactuals. Results of analyses conducted to test performance differences between counterfactual thinking groups and those in an empty control group demonstrated no significant differences. Similarly, one of the few other studies that tested the performance enhancing effects of upward counterfactual thinking also initially provided no evidence difference between groups on an anagram task. However, significant effects of upward counterfactual thinking were evident when individuals' moods were accounted for in the model (Myers, McCrea, & Tyser, 2014).

Therefore, though no direct effect of counterfactual thinking on performance was noted, it was expected that, as with the study by Myers et al. (2014), this lack of an effect could be explained by the presence of some moderator variable. To confirm this hypothesis, the conditional indirect effect of depression and perceived control on performance was subsequently

tested. Once more, though, hypotheses were not supported. Results did not show that individuals' depression symptoms moderated a relationship between upward counterfactual thinking and performance. Additionally, no evidence supported the prediction that perceived control would mediate a relationship between counterfactual thinking and performance.

Similarly, the second model of counterfactual potency, control, and performance was not supported. No evidence suggested that individuals experiencing depression symptoms found their counterfactual thoughts to be any less plausible than others. Further, no significant relationship was found between counterfactual potency, perceived control, and performance. Thus, overall, this study found no support for either of the proposed models of counterfactual thinking and performance. As a lack of effects can be explained in a multitude of ways, from methodological issues to a general misjudgment of the constructs involved, there is no way to be certain why the proposed models were not supported; however, some of the more relevant possibilities are explored below.

Methodological Concerns

The procedures of this replication and extension study were heavily modeled off those described in Roese's (1994) previous work, as this has been a common methodological approach for many experiments investigating the functional effects of upward counterfactual thinking (e.g., Myers, McCrea, & Tyser, 2014). Despite this, some differences between the current experiment and these other studies did exist and may explain the differential findings. One such difference was that while both the current study and those it was modeled off administered the anagram task on a computer, in previous research participants engaged in the research tasks in a designated testing location (e.g., a research lab). Conversely, in the current study participants completed the study online in a setting of their choice.

It is possible that in such an uncontrolled setting, participants may have cheated on the anagram task or taken it less seriously. Though randomization is assumed to equally distribute these hypothetical participants across both groups, if enough were present the effects of counterfactual thinking may have been diluted. For example, if many participants, motivated by the opportunity to win a gift certificate, cheated to successfully complete the anagram task, this would have limited participants' room for growth between anagram trials and minimized the proposed effect of upward counterfactual thinking.

Some unexpected findings initially suggested this may have been occurring. Overall, participants in this study appeared to have successfully completed more anagrams in the two tasks, leading to higher scores, than previous studies employing similar procedures. The average score and total anagrams solved across all participants in this study were 691.83 and 7.12, respectively, nearly double the performance outcomes noted previously by Roese (1994). Also, in both trials a ceiling effect was evident, with 17.21% of participants in Trial 1 and 23.95% of participants in Trial 2 solving all ten anagrams. In an effort to determine if participants were cheating to achieve these higher scores, the task strategies used by participants were investigated further.

If participants were cheating, it was assumed that they would not be using any clues or skipping any words. However, only 10 individuals (5.5% of the final sample), used no clues or skips to complete both sets of anagrams. Further, it was assumed that the most common strategy to achieve a perfect score would be to rely on other technologies to generate the solutions for the anagrams. However, participants who did not use clues or skips in either set of anagrams took, on average, 13.32 seconds to solve each anagram. It is unlikely that participants could have viewed the anagram, selected another computer screen, typed in the anagram, received the

solution, returned to the study, and typed in the solution in this time consistently across multiple trials. Though this suggests that few, if any, participants were cheating by utilizing this strategy, this does not definitively prove that cheating was not an issue for this study. Participants may have relied on other strategies to cheat (e.g., asking friends) which could not be investigated through the measured variables.

Another possibility was that participants, removed from the formal setting of a science lab, were somehow influenced by their environment. Once more, though randomization would likely distribute these hypothetical setting effects equally across the groups, if settings affected enough participants in each group, this could once again limit the overall power of the study. Again, some preliminary evidence suggested that participants' environments influenced the outcomes of the study. When given the chance to provide qualitative feedback, be it in the thought listing task or in the generation of counterfactual thoughts, some participants noted the effect their setting had on their performance. For example, one participant noted, "I wish that I was somewhere more quiet so I could think more." However, despite these occasional remarks, participants' relatively high scores suggest that their performance was not severely impeded by these potentially problematic environments.

A larger concern was the potential that the experimental manipulation, intended to elicit negative affect through false failure feedback, was unsuccessful. Once again, environment may have played a role here. As is clear from the classic studies in behavioral obedience (e.g., Milgram, 1963) the presence of an apparent authority figure can profoundly influence the behavior of participants. When removed from the environmental indicators of a formal setting, was the experiment less credible, diminishing the power of the experimental manipulation? Judging from feedback provided by participants, however, this did not appear to be the case.

When asked to report how they viewed their role in the study, only two (1.10 %) of the final participants noted that they did not take their role “at all seriously.”

To further confirm that the manipulation was successful, participants were also asked to report how accurately they felt they the score of the first anagram task represented their performance and whether or not they doubted the validity of this score. Some participants did note a degree of doubt, with 11.54% of participants reporting that the score did not accurately describe their performance and 15.93% doubting the accuracy of this score. However, these responses did not vary by group, suggesting randomization, in regards to this factor, was successful. Further, results were no different when analyses were repeated after excluding all participants who suggested they did not take their role seriously.

It is possible, however, that the manipulation was unsuccessful for some other, currently unexplained reason. As proposed by Roese (1994, 1997), counterfactual thinking is typically elicited by negative outcomes. With this in mind, Roese included false performance feedback in his original study to purposefully manufacture this negativity in an effort to drive counterfactual thinking effects. Following this design, in the current study a manipulation check was included to confirm that participants were dissatisfied with their performance. Though almost no participants reported that they were “extremely satisfied” with their results, less than half (40.98%) reported some level of dissatisfaction, suggesting that this manipulation was only moderately successful.

It is similarly possible that the control analyses of Roese’s (1994, Experiment 3) original anagram study yielded no significant effects because the manipulation was not sufficiently powerful. As previously demonstrated by Myers et al. (2014), the effects of counterfactual thinking occur in the presence of negative emotions. Therefore, if the manipulation could not induce sufficient dissatisfaction, the effect of counterfactual thinking may have been minimized.

To explore this possibility in the current study, analyses were once again repeated after excluding participants who reported some degree of satisfaction with the results of the first anagram trail. However, this yielded no significant findings and the hypothesized models remained unsupported. Additionally, results demonstrated that no significant relationship was found between outcome satisfaction and counterfactual potency scores. Together, these findings suggest that the effectiveness of the manipulation cannot sufficiently explain the unsupported hypotheses.

Lastly, one area of remaining methodological concern was the study assessments, particularly those measuring counterfactual potency and perceived control. As noted, the method of assessing counterfactual potency was modeled off the procedure used by Petrocelli et al. (2011). These researchers describe three separate experiments that were conducted to test the construct of counterfactual potency. Across all three studies, no comments were made suggesting some degree of difficulty in reliably eliciting “if likelihood” and “then likelihood” scores from participants.

However, feedback received during piloting, conducted to prepare for the current study, suggested that items related to counterfactual potency were mildly confusing. Though these issues were assumed to have been resolved prior to data collection, in the current study a few of the participants (11.11%) assigned to the counterfactual thinking condition noted a degree of confusion surrounding these items (e.g., “Some of the questions are unclear, particularly the “If I only” scale”). While the total number of participants reporting confusion was low, with other participants explicitly noting that the study was clear (e.g., “Wasn’t confused at all”), the reliability of this assessment procedure remains in question.

One participant similarly noted that he/she did not understand how to respond to the single item used to assess perceived control (e.g., “I had no idea what the control options were for and it was not made clear”). Once again, this comment was not widely made and may represent the experience of only one participant. However, it is a possible that this variable, too, had not been reliably measured. Therefore, if future studies are conducted in attempt to continue exploring the relationship between counterfactual thinking and counterfactual potency or perceived control, it is necessary that reliable methods of assessing these variables be determined.

Alternative Explanations

Beyond methodological explanations which suggest the hypothesized results would have been found under different study conditions, it is also possible that the predicted models were not supported because the hypothesized relationship between counterfactual thinking, control, performance, and depression do not exist. Generally speaking, this model posited three linked effects. The first predicted effect was that upward counterfactual thoughts, particularly those with a high degree of potency, would lead to increases in performance between two tasks. Second, upward counterfactual thinking were predicted to lead to increased feelings of perceived control. Finally, it was hypothesized that the cognitive style of individuals with depression would impede on the relationship between counterfactual thinking and control. As all three of these effects were unsupported in this study, each of these were re-examined in an effort to better understand these variables and their relationship to the counterfactual thinking process.

Following Roese’s (1994, 1997) early work, many studies have continued to investigate the functional theory of counterfactual thinking, including the supposed performance enhancing effect of upward counterfactual thoughts. However, support for these performance enhancements

has been inconsistent. While Myers et al. (2014) found performance improvements following counterfactual thinking, Roese's (1994, Experiment 3) own study only did so in comparison to an alternative counterfactual thinking group, not in comparison to an empty control group. Further, others have been unable to reproduce this effect (e.g., Chan et al., 2013) or have demonstrated a deleterious, rather than beneficial, effect of the upward counterfactual thinking process (e.g., Petrocelli, Seta, & Seta, 2013). While this inconsistency may be the result of some moderator variable that must also be present in order for the performance effects of upward counterfactual thoughts to occur, as was proposed by Myers et al. (2014), these results may also indicate that the outcomes of the functional theory of counterfactual thinking have been misunderstood.

Roese (1994, 1997) originally postulated that performance improvements were a result of individuals, having reflected on their performance, imagining and then engaging in alternative behaviors. However, within this explanation there is no consideration of the imperfect nature of the learning process. Roese's theory depicts individuals' understanding of performance naively, assuming that individuals will correctly understand what has led to a poor outcome and be willing and able to subsequently modify their behavior to yield better outcomes. But following a negative outcome it is also highly likely that individuals will generate counterfactual thoughts that incorrectly identify the cause of their outcomes.

For example, in the current study it was consistently demonstrated that those who used more clues and skipped more words ultimately had lower scores. Thus, if individuals reflected on their performance and thought, "If only I had used more clues, I would have performed better on this anagram task," they would have been incorrect; this "prescriptive" change in behavior would have actually led to decreases in performance. Though the example noted here follows Roese's

description of the preparative function of upward counterfactual thinking, it does not suggest that this process would result in performance improvements. In light of this, it is clear that more research is needed to fully understand the relationship between upward counterfactual thinking, performance, and behavior.

Mercier et al. (2017) have also provided evidence that the basic premise of this functional theory of counterfactual thinking is unsupported. These researchers claimed that a foundational, untested prediction of the functional theory is that counterfactual thoughts will highlight controllable aspects of an experience. If these thoughts are to serve a preparative purpose, as proposed, they should target elements of an event that can be altered if the situation re-occurs in the future. However, though some studies have demonstrated that upward counterfactual thinking is more likely following controllable events (Markman et al., 1995; Roese & Olson, 1995), Mercier et al. (2017) reported that in three separate studies, participants' counterfactual thoughts were commonly uncontrollable in nature.

Similar to the method used in the current study, participants studied by Mercier et al. (2017) completed two word tasks and were prompted to generate thoughts about their experience in between. In comparison to individuals directed to generate future oriented statements considering ways to specifically alter their performance in the second task, individuals who were prompted to generate counterfactual thoughts about their previous performance often focused on characteristics of the task, stable traits, or contextual factors that remained outside the control of the individual. Though counterfactual thoughts in the current study were not coded by degree of controllability, it was also found that participants commonly generated counterfactuals that focused on these uncontrollable areas.

The results reported by Mercier et al. (2017) may shed light on the lack of relationship found between counterfactual thinking and perceived control. If participants do not commonly generate controllable counterfactual thoughts, as suggested by the functional theory, it is of little surprise that those in the counterfactual thinking group felt no more control over the task than other participants. Further, research from Nasco and Marsh (1999) also illustrated that perceived control may not inherently be a product of counterfactual thinking. Following a group of undergraduate students after receiving an exam grade, these researchers found that perceived control over a subsequent exam did not directly relate to counterfactual thinking, but rather indirectly related through behavior change. Only those who generated upward counterfactuals and also subsequently changed their behavior felt more control prior to their next exam. However, while Nasco and Marsh (1999) also reported that the counterfactuals generated by study participants were controllable in nature, other researchers have reported that increases in perceived control resulting from counterfactual thinking (McMullen & Markman, as cited in McMullen, Markman, & Gavanski, 1995), it is clear that more research in this area is needed.

In addition to no evident relationship between counterfactual thinking and control, in the current study no significant relationship was found between counterfactual potency and perceived control. Participants who judged their counterfactual thoughts as being more likely to occur and more likely to lead to performance improvements reported no greater feelings of control over the second anagram task. However, given the conceptualization of counterfactual potency as relating to the magnitude of the given outcome of counterfactual thought, this is once again no surprise. If perceived control is not a given outcome of upward counterfactual thinking, then there is nothing to vary according to the characteristics of the counterfactual thoughts.

The hypothesized relationship between counterfactual potency and depression was also not supported in this study. Similarly, participants who reported greater levels of depression did not also report lower levels of perceived control, as predicted. These hypotheses were built off the proposition that individuals with depression generate causal attributions that depict their world as less controllable, which subsequently impedes on the functional process of counterfactual thinking. Given the lack of support for this proposition, however, a new understanding of counterfactual thinking, depression, and control may be necessary.

Instead of individuals with depression being unable to generate feelings of control, other researchers have suggested that individuals with depression do experience feelings of control but are less inclined or able to alter their behavior in the face of negative events. As such, Markman and Weary (1998) posited that individuals with depression suffer from “breakdowns in the implementation of behavioral strategies,” resulting from debilitating rumination or an excessive caution in taking action (p. 382). If this alternative perspective is accurate, rather than depression impeding an effect of counterfactual thinking on control, which in turn hinders performance improvements, individuals with depression may be unable to transform their counterfactual thoughts into actual changes in behavior. Further, if perceived control is only a result of actual behavioral change that may or may not result from counterfactual thinking, as suggested by the research of Nasco and Marsh (1999), this may explain why individuals with depression chronically suffer from feelings of lack of control. Future studies are recommended in order to investigate this alternative explanation further.

Additional Limitations

Beyond the potential methodological and theoretical issues previously discussed, there were also a few other limitations of this study. First of all, as is the unfortunate case of many

psychology research studies today, the sample of participants was fairly homogeneous. Most participants were female and between the ages of 18 and 22 (80.22% of the final sample). Though all efforts were made to attract a diverse sample by placing ads on the internet and hosting the study online, the vast majority of individuals were recruited from a college psychology program. Though previous research has not indicated that gender or education level impacts the counterfactual thinking process, for the most generalizable findings future studies investigating the effect counterfactual thinking should continue to strive for heterogeneous samples.

Furthermore, this study only explored the counterfactual thinking process in individuals exhibiting various levels of depression symptoms, not clinically diagnosed depression. No mental health professional was recruited to screen participants, therefore these results cannot conclusively provide evidence for how the process of counterfactual thinking occurs in those with diagnosed depression. Future studies investigating this relationship would benefit from the inclusion of individuals who carry a formal and current diagnosis of major depressive disorder.

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Footnotes

¹ Though this well-known quote has frequently been attributed to George Eliot, no original source material has been found to corroborate this; therefore, some argue that this reference may be inaccurate (see Mead, 2011).

Table 1

Descriptive Statistics of Participant Strategies and Outcomes for Anagram Task Trial 1 and Trial 2

		Trial 1	Trial 2
		<i>n</i>	<i>n</i>
Topic	People and Events	55	65
	Science and Nature	67	57
Difficulty	Easy	71	77
	Difficult	51	45
		<i>M (SD)</i>	<i>M (SD)</i>
Intertrial interval		3.30 (2.35)	3.00 (2.24)
Skips		2.28 (1.98)	2.05 (2.09)
Clues		2.52 (2.84)	2.28 (2.42)
Total correct		7.55 (1.95)	7.84 (2.05)
Score		630.62 (354.97)	694.38 (296.70)

Table 2

Correlations Between Quantitative Variables

	Spearman's correlations								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) T1 Clues									
(2) T1 Skips	.33***								
(3) T1 Score	-.65***	-.73***							
(4) T2 Clues	.71***	.27**	-.45***						
(5) T2 Skips	.17	.54***	-.32***	.26**					
(6) T2 Score	-.37***	-.41***	.55***	-.54***	-.77***				
(7) Change score	.36***	.42***	-.58***	-.01	-.36***	.31**			
(8) Outcome satisfaction	-.23*	-.35***	.32***	-.14	-.23*	.14	-.22*		
(9) Perceived control	-.24*	-.16	.22*	.16	-.04	.14	-.17	.34***	
(10) Depression symptoms	.02	-.20*	.09	.02	-.21*	.20*	.08	-.17	-.10

Note. T1 = Trial 1; T2 = Trial 2. * $p < .05$. ** $p < .01$. *** $p < .001$.

Table 3

PROCESS Rendered Regression Coefficients for Model 1

Predictor	Outcome							
	Perceived control				Change score			
		Coefficient (SE)	<i>t</i>	<i>p</i>		Coefficient (SE)	<i>t</i>	<i>p</i>
Intercept		4.44 (.38)	11.58	<.001		129.20 (80.68)	1.60	.11
Counterfactual thinking	<i>a</i> ₁	.39 (.53)	.73	.47	<i>c'</i>	52.13 (57.96)	.90	.37
Depression symptoms	<i>a</i> ₂	-.004 (.02)	-.18	.86				
Counterfactual x Depression	<i>a</i> ₃	-.01 (.03)	-.35	.72				
Perceived control					<i>b</i>	-20.45 (15.75)	-1.30	.20

Note. All reported coefficients are unstandardized.

Table 4

PROCESS Rendered Regression Coefficients for Model 2

Predictor	Outcome							
	Perceived control			Change score				
		Coefficient (SE)	<i>t</i>	<i>p</i>		Coefficient (SE)	<i>t</i>	<i>p</i>
Intercept		3.82 (0.49)	7.72	< .001		334.54 (137.02)	2.44	<.05
Counterfactual potency	<i>a</i>	0.02 (0.01)	1.80	.08	<i>c'</i>	-2.37 (2.24)	-1.06	.29
Perceived control					<i>b</i>	-32.47 (25.23)	-1.29	.20

Note. All reported coefficients are unstandardized.

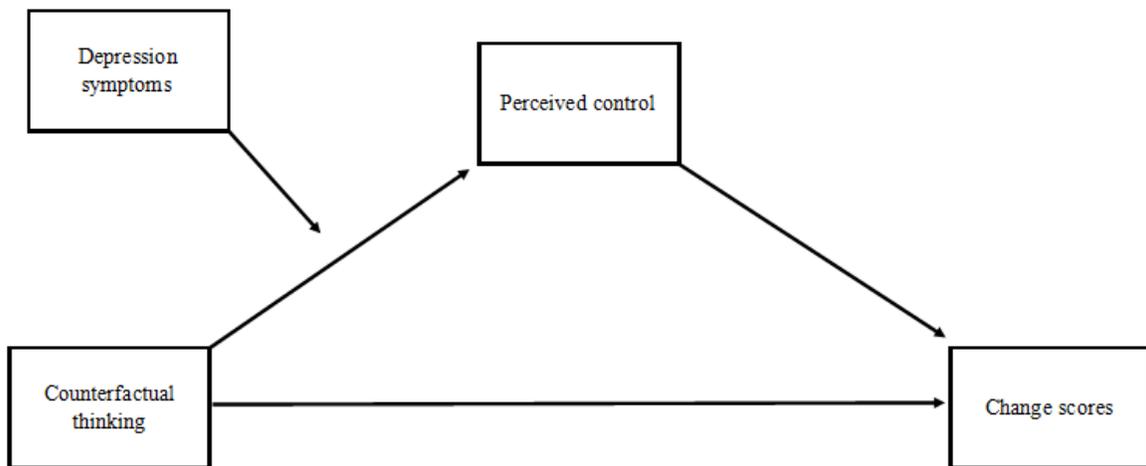


Figure 1. Conceptual diagram of the first proposed moderated mediation model of counterfactual thinking and performance.

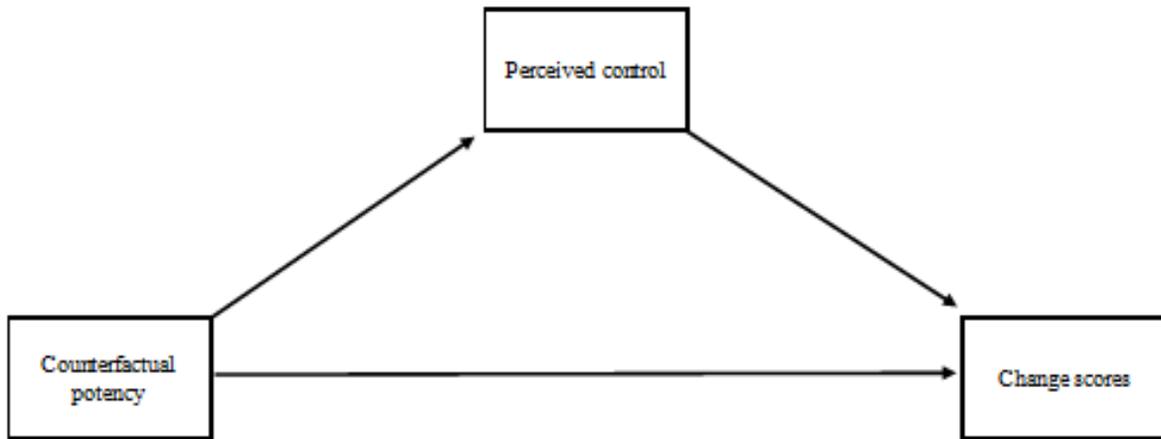


Figure 2. Conceptual diagram of the second proposed mediation model of counterfactual potency and performance.

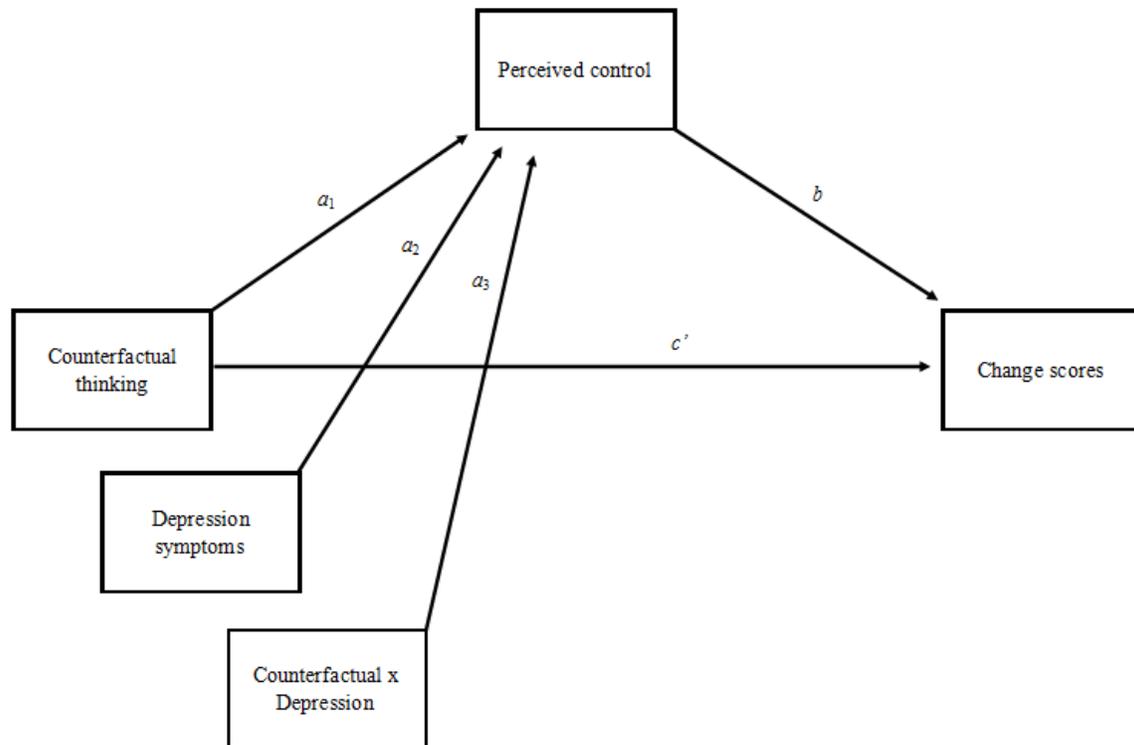


Figure 3. Statistical representation of the first proposed moderated mediation model, depicting both the direct effect (c') and the conditional indirect effect $((a_1 + a_3 \times \text{Depression symptoms})b)$ of counterfactual thinking on change scores.

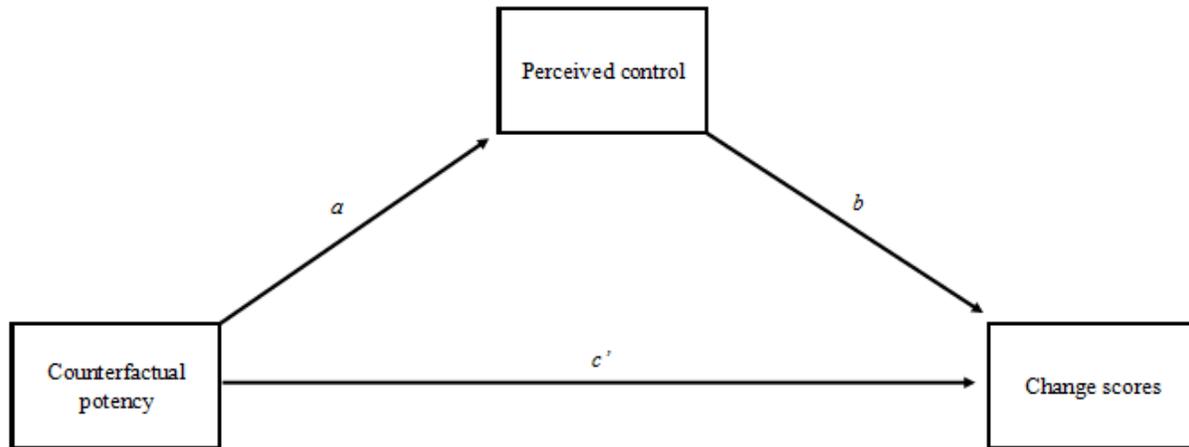


Figure 4. Statistical representation of the second proposed mediation model, depicting both the direct effect (c') and the indirect effect (ab) of counterfactual potency on change scores.

Appendix A

This is a study of decision-making processes under time pressure. Your task will be to solve two sets of 10 anagrams within a certain period of time. Anagrams are scrambled word combinations – solving them simply means unscrambling them into an actual word. For example, “YHAPP” is an anagram, and its solution is “HAPPY.” All anagrams in this study have only ONE solution.

To guess a solution, type it into the provided space and press enter on your keyboard or click the “ENTER” button. For each anagram solved you will be awarded a certain number of points. At the end of the anagram task, the computer will calculate a total score, and then it will tell you how good this score is compared to other participants in this study. At the conclusion of this study, the participant who had the highest score in either set of anagrams will be awarded a \$50 gift certificate, so it is important to try to get as many points as possible.

You will be awarded a maximum of 120 points for each anagram solved. However, the longer it takes for you to solve the anagram, the lower your score. One point will be deducted per second. So if you took 20 seconds to solve an anagram, you would get 100 points; if you took 60 seconds to solve it, you would get only 60 points. You have a total of 2 minutes (120 seconds) to solve each anagram. After 2 minutes have elapsed, the computer will give you the solution and present the next anagram.

But that’s not all. If you cannot solve the anagram within the 2 minute period, you will lose points. One point will be deducted from your TOTAL score for each second spent on an anagram you didn’t solve. This means it’s a good idea to SKIP OVER anagrams that seem especially

difficult; this allows you to minimize the points lost. Therefore, deciding which anagrams to skip over is one of the most important decisions you'll make in this task. You can skip ahead at any time simply by clicking the "SKIP" button on the bottom of the screen.

If you think you are near to solving the anagram but can't quite get it, you may "buy" a clue. This will cost you 30 points from your score. The clue will be the middle letter of the correct solution. So for the anagram "YHAPP," you would get the clue "P," which is the middle letter of the solution "HAPPY." You can request this clue at any time by simply clicking the "CLUE" button on the bottom of the screen.

Appendix B

People and Events (A)

CLOWN	OWNLC
CROWD	ORDCW
FIGHT	GIFTH
JUDGE	JEGUD
MATCH	HACTM
MONTH	HTMON
PITCH	CHITP
RUGBY	GBRUY
SCOUT	OUSTC
WOMAN	OWAMN

People and Events (B)

BIGOT	GBTIO
CLASH	HSLAC
CRUSH	HRCUS
DUNCE	EUNDC
FAIRY	FIYRA
FLIRT	FTLRI
GUARD	AUGRD
MAJOR	OAJRM
THIEF	FTEIH
WALTZ	ZLTWA

Science and Nature (A)

CHORD	ORHCD
DEPTH	PHTED
FINCH	CNIHF
FORCE	OFREC
FRONT	NRTOF
HONEY	NOEHY
LIGHT	GITHL
PERCH	HECPR
TULIP	IPTLU
WIDTH	HTIWD

Science and Nature (B)

BLADE	ADEBL
COUNT	TCUON
FLASH	HSAFL
FRUIT	ITRUF
HOUND	HNDUO
HYENA	HAENY
LOGIC	IOGLC
PLUCK	LCKUP
TREND	TERDN
VIRUS	SRIVU

Appendix C

On the following scale, please rate how satisfied you are with the outcome of this task.

Extremely dissatisfied	Somewhat dissatisfied	Neutral	Somewhat satisfied	Extremely satisfied
1	2	3	4	5

Appendix D

What thoughts occurred to you when you received your results of this task? For the next minute please record any thoughts you have had since the completion of this task in the space below.

Appendix E

In situations like these people often have thoughts like “if only...” Sometimes these thoughts can be about things that would have made a situation better, and they are about things that are better than what actually happened. In the spaces below, please complete each of the statements, listing three things that might have been different that would have made the situation better.

If only _____, I would have performed better on this anagram task.

If only _____, I would have performed better on this anagram task.

If only _____, I would have performed better on this anagram task.

Appendix F

[IL] Consider the statement above; to what degree do you think this could have occurred?

Extremely unlikely	Very unlikely	Somewhat unlikely	Slightly unlikely	Neither unlikely nor likely	Slightly likely	Somewhat likely	Very likely	Extremely likely
1	2	3	4	5	6	7	8	9

[TL] Now consider the whole statement you completed, above. If the first part of the statement was true, to what degree do you feel you would have performed better on this anagram task?

Extremely unlikely	Very unlikely	Somewhat unlikely	Slightly unlikely	Neither unlikely nor likely	Slightly likely	Somewhat likely	Very likely	Extremely likely
1	2	3	4	5	6	7	8	9

Appendix G

On the following scale, please indicate how much control you have over your upcoming performance on the next anagram task.

No control		Some control		Moderate control		Lots of control		Total control
1	2	3	4	5	6	7	8	9