The Role of Episodic Specificity in Future Thinking and Emotion Regulation in Young and Older Adults

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The Role of Episodic Specificity in Future Thinking and Emotion Regulation in Young and Older Adults

A dissertation presented

by

Helen G. Jing

to

The Department of Psychology

in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the subject of Psychology

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The Role of Episodic Specificity in Future Thinking and Emotion Regulation in Young and Older Adults

Abstract

Much research in the past decade has highlighted the importance of episodic simulation, the construction of a detailed representation of a possible personal future experience. Episodic simulation can be highly adaptive because it allows people to imagine different ways in which the future might play out without having to engage in actual behavior, which is beneficial across a variety of contexts, including problem solving and emotion regulation. The current dissertation aims to examine not only the mechanisms that support various types of episodic future simulation, but also the functions that future thinking may serve. Paper 1 (Jing, Madore, & Schacter, 2016) examines the impact of an episodic specificity induction (ESI), a brief training in recollecting details of a recent experience, on two positive simulation tasks: means-end problem solving (MEPS) and episodic reappraisal. We demonstrate that the ESI, relative to a control condition, boosts the steps and details people generate to solve or reframe a series of personally worrisome future problems. Further, this boost in details was linked to subsequent improvements in emotional well-being towards the target events. Paper 2 (Jing, Madore, & Schacter, 2017) also aims to investigate the impact of the ESI on emotional well-being using a novel alternative event generation task in young adults. Results show that the ESI increased the number of alternative positive outcomes that participants generated to a series of anticipated negative events, and that the boost in alternative outcomes was related to subsequent decreases in the perceived plausibility and negativity of the original events. Paper 3 (Jing, Madore, & Schacter, 2019) examined the effect of detailed problem solving on subsequent emotion regulation in older adults in two ways.
Experiment 1 contrasted problem-solving performance after administering the ESI relative to a control induction, and found that while the ESI boosted performance on a MEPS task, there were no observed differences in emotion regulation between the two induction conditions. In Experiment 2, we contrasted performance on a personal problem-solving task intended to draw on episodic retrieval with a novel advice task focused on semantic processing. Participants provided more concrete steps and details in the personal problem-solving task relative to the advice task, and boosts in detail were related to larger improvements in emotion regulation. The results of these papers support the idea that imagining constructive behaviors regarding worrisome events may be related to improved emotional well-being.
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**Introduction**

Human memory is prone to error, as individuals are often susceptible to misremembering important details of their pasts (Schacter, 1999, 2012). Importantly, such flaws demonstrate that memory is not just a carbon copy of the past, but rather is a constructive process. According to the *constructive episodic simulation hypothesis* (Schacter & Addis, 2007, in press), having a constructive, flexible episodic memory system allows individuals to imagine or simulate future scenarios by drawing on one’s past experiences. In recent years, there has been a surge in research focusing on *episodic simulation* or the construction of a detailed representation of a hypothetical personal future experience (Schacter, Addis, & Buckner, 2008), as well as other types of prospective, future-oriented thought (Seligman, Railton, Baumeister, & Sripada, 2013; Szpunar, Spreng, & Schacter, 2014).

The constructive episodic simulation hypothesis has been supported by extensive research highlighting the striking cognitive and neural similarities between episodic memory and episodic future simulation. For example, populations with deficits in recalling past events face similar challenges when asked to imagine future experiences. Tulving (1985, 2002) first reported that amnesic patient K.C. was not only unable to recollect specific episodes from his past, but was also unable to imagine future experiences, proclaiming that his mind was “blank.” Behavioral research has also noted similarities between past and future thinking by using the Autobiographical Interview (AI; Levine, Svoboda, Winocur, & Moscovitch, 2002), which segments event narratives into two major types of details: “internal” details (i.e., information about specific people, objects, and actions that constitute an event) and “external” or semantic details (i.e., factual information that is not specific to time and place, commentary, or references to other events). Researchers have observed not only reduced specificity of autobiographical memory (i.e., fewer reported internal,
event details), but also reduced specificity of future imaginations in normal aging (Addis, Musicaro, Pan, & Schacter, 2010; Addis, Wong, & Schacter, 2008; Cole, Morrison, & Conway, 2013; Rendell et al., 2012; Romero & Moscovitch, 2012), as well as in clinical populations characterized by such conditions as amnesia (Race, Keane, & Verfaellie, 2011), Alzheimer’s disease (Addis, Sacchetti, Ally, Budson, & Schacter, 2009), depression (Williams et al., 1996), posttraumatic stress disorder (Brown et al., 2014), and schizophrenia (D’Argembeau, Raffard, & Van der Linden, 2008). Further, recalling the past and simulating the future recruit similar regions within the core network, including medial prefrontal regions, medial temporal lobe, lateral temporal cortex, lateral parietal cortex, and retrosplenial cortex (for recent reviews, see Benoit & Schacter, 2015; Schacter et al., 2012; Szpunar, 2010).

Recent work has explored not only the mechanisms that support episodic future simulation, but also the functions that future thinking may serve (for review, see Schacter, Benoit, & Szpunar, 2017). Some researchers have argued that episodic simulation can be highly adaptive because it allows people to construct simulations of different ways in which the future might play out without having to engage in actual behavior (cf., Ingvar, 1979; Schacter, 2012; Suddendorf & Corballis, 1997, 2007). Consistent with this observation, previous research has shown that the process of simulating a future event can be beneficial across a variety of contexts, including planning and problem solving (e.g., Arnold, Iaria, & Ekstrom, 2016; Gerlach, Spreng, Gilmore, & Schacter, 2011; Spreng, Stevens, Chamberlain, Gilmore & Schacter, 2010), prospective memory (e.g., Terrett et al., 2016), decision-making (e.g., Benoit, Gilbert, & Burgess, 2011; Peters & Büchel, 2010), and emotion regulation (e.g., Taylor, Pham, Rivkin, & Armor, 1998; for review, see Schacter, 2012).
Emotional Future Simulation

While episodic simulation and prospection more generally serve adaptive functions, some forms of emotional future thought can be disruptive to psychological functioning and well-being. For example, in emotional disorders such as anxiety or depression, individuals may have a reduced capacity to imagine positive future experiences related to the self (e.g., MacLeod & Conway, 2007), show greater anticipation of negative future experiences (e.g., MacLeod & Byrne, 1996) and harbor excessive worry about the future (e.g., Borkovec, Ray, & Stöber, 1998; see also Bulley, Henry, & Suddendorf, 2017; Miloyan, Pachana, & Suddendorf, 2014). Negative thoughts towards the future can be maladaptive if one repeatedly engages in those thoughts, particularly given that repetitive future thinking has been linked to increased estimates in the perceived plausibility of an event’s occurrence (Szpunar & Schacter, 2013; Wu, Szpunar, Godovich, Schacter, & Hofmann, 2015) and increased accessibility of negative event outcomes (Byrne & MacLeod, 1997; MacLeod, Tata, Kentish, Carroll, & Hunter, 1997).

However, existing research suggests that constructing a positive mental simulation of a worrisome future event can be beneficial for emotional well-being. For example, Brown, MacLeod, Tata, and Goddard (2002) demonstrated that more detailed imaginings of a worrisome event (e.g., going into labor in a group of first-time pregnancy mothers) were correlated with reduced ratings of worry and increased subjective probability of a good outcome (e.g., successful delivery). Structured mental simulation of a controllable ongoing stressful event (e.g., preparing for an exam) has also been shown to increase ratings of positive affect and decrease negative emotions towards the event, as well as increase engagement in active coping strategies (e.g., facilitating studying behaviors, increasing planning) (Pham & Taylor, 1999; Rivkin & Taylor, 1999). Further, generating a number of positive alternative outcomes for a set of anticipated
negative events leads to reduced ratings of event plausibility of the original negative events (Bentz, Williamson, & Franks, 2004; Hirt, Kardes, & Markman, 2004; Raune, MacLeod, & Holmes, 2005). Taylor et al. (1998) have proposed a number of intrinsic characteristics of mental simulations that likely make them useful for self- and emotion-regulation, including that simulation increases the perceived plausibility of occurrence of an event, that simulations involve an organization of action that can yield a plan, and that simulations evoke emotional states and their potential control. While their hypotheses have largely been supported, there thus far exists little experimental evidence examining the specific processes that influence whether and how future event simulation may benefit emotional well-being.

**Episodic Specificity and Emotion Regulation**

In the three papers of this dissertation, we focus on how episodic specificity and the detail with which one simulates a negative future event might impact subjective well-being and emotion regulation towards that event. As briefly mentioned above, there have been reports of reduced specificity (i.e., reduced levels of internal, event detail) of episodic memory retrieval and future simulation in individuals with emotional disorders, including depression (Williams et al., 1996) and anxiety disorders such as post-traumatic stress disorder (Brown et al., 2014; McNally, Lasko, Macklin, & Pitman, 1995; McNally, Litz, Prassas, Shin, & Weathers, 1994). According to the functional avoidance hypothesis (Williams et al., 2007), recollection of more general descriptions (as opposed to more specific, concrete episodic memories) produces less affective arousal, reducing the impact of unpleasant emotional material. In a similar vein, there is evidence that worry in generalized anxiety disorder (GAD) involves predominately negative verbal and conceptual thought that lacks specific, concrete details typically contained in visual imagery and episodic simulations (Borkovec et al., 1998; Stöber & Borkovec, 2002). Worry elicits less
sympathetic arousal than visual imagery and suppresses somatic anxiety, and thus may serve a
cognitive avoidance function to threat so that individuals can disengage and avoid arousing
emotional processing towards the aversive or worrisome trigger (Borkovec et al., 1998; Williams,
2006). While reduced specificity and concreteness about an aversive, arousing event may serve as
temporary relief, adopting such an orientation can have adverse long-term consequences, such as
reducing the ability to cope with the problem at hand. It is thought that reduced specificity can
magnify existing features of emotional disorders (e.g., hopelessness, avoidance) to make it more
difficult for anxious and depressed individuals to imagine their future in a sufficiently concrete
fashion to generate specific plans and goals, thus exacerbating their symptomology (Borkovec et
al., 1998; Williams, 2006; Williams et al., 1996, 2007). Indeed, in addition to reduced specificity,
patients with emotional disorders also show poorer problem-solving performance relative to
healthy controls (Dickson & MacLeod, 2004; Goddard, Dritschel, & Burton, 1996; Raes et al.,
2005; Sutherland & Bryant, 2008).

The results of several studies suggest that manipulations aimed at increasing specificity
and detail of episodic retrieval can have beneficial consequences for subsequent performance of
various kinds of tasks, including problem solving. Madore, Gaesser, and Schacter (2014)
developed an episodic specificity induction (ESI), a brief training in recollecting details of a recent
experience that is based on the Cognitive Interview (CI; Fisher & Geiselman, 1992; Memon,
Meissner, & Fraser, 2010), a well-established procedure for increasing recall of episodic detail in
eyewitnesses. The ESI encourages participants to focus on specific details of a past experience
(e.g., details related to settings, people, and actions in a short video they just watched), which
selectively biases them to focus on specific event details of a similar nature during subsequent
tasks that are dependent on episodic retrieval (e.g., memory, imagination, problem solving, and
divergent thinking tasks that require constructing and filling in an event with specific episodic details). Critically, the ESI has no impact on the performance of subsequent tasks that are thought to rely on primarily semantic retrieval or non-episodic narrative processing (e.g., picture description, generating word definitions and object associations; Madore, Addis, & Schacter, 2015; Madore et al., 2014; Madore, Jing, & Schacter, 2016; Madore & Schacter, 2016; for review, see Schacter & Madore, 2016). Madore and Schacter (2014) demonstrated that the ESI boosts the number of relevant steps and episodic details that both young and older adults provide to solve a set of everyday social problems in a means-end problem solving task (MEPS; Platt & Spivack, 1975) relative to a control condition, confirming that episodic retrieval contributes to successful problem solving (see also Sheldon, McAndrews, & Moscovitch, 2011; Sheldon et al., 2015; Vandermorris, Sheldon, Winocur, & Moscovitch, 2013). Thus, the ESI shows promise as a tool that can be used to enhance episodic specificity and problem-solving performance. Additionally, recent work has shown that increasing the specificity of autobiographical memory using a Memory Specificity Training protocol (MEST; Raes, Williams, & Hermans, 2009) can be linked to improvements in depressive symptoms (Lang, Blackwell, Harmer, Davison, & Holmes, 2012; Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD symptoms (Moradi et al., 2014) with respect to negative and distressing past events (for review, see Hitchcock, Werner-Seidler, Blackwell, & Dalgleish, 2017). However, there is little existing evidence that increased specificity can be beneficial for processing worrisome future events that have not yet been experienced, and the evidence that does exist is correlational, such as the previously mentioned study by Brown et al. (2002) showing that more detailed imaginings of a worrisome event correlated with increased subjective probability of a positive outcome regarding the imagined event. Given that reduced episodic specificity can limit the ability to imagine the future and to engage in effective problem
solving towards potential future obstacles, it is thus possible that increasing the details with which people imagine the future might serve as a useful intervention to foster more constructive problem-solving behaviors that can promote active coping and decrease overall maladaptive functioning.

**Motivation for Papers of the Dissertation**

The main goals of the three papers in this dissertation are to examine: 1) the episodic memory mechanisms underlying our ability to engage in various forms of constructive and positive future thinking, and 2) adaptive ways in which simulating the future subsequently impacts our original expectations and perceptions of the future.

Paper 1 (Jing, Madore, & Schacter, 2016) examines the impact of detailed episodic future simulation (e.g., future-oriented problem solving and reappraisal) on subsequent emotional well-being towards impending events that young adults find to be worrisome. Prior work has shown that problem solving can engage episodic memory processes (Sheldon et al., 2011, 2015; Vandermorris et al., 2013), and further, that the ESI (Madore et al., 2014) can boost problem solving performance (Madore & Schacter, 2014). Thus, we implement the ESI prior to a means-end problem solving task (MEPS; Platt & Spivack, 1975) that encourages participants to generate steps to solve a worrisome future event, as well as a novel episodic reappraisal task that prompts participants to consider ways a negative outcome can be reframed. We hypothesize that the ESI should lead to subsequent boosts in the steps and episodic details that participants generate to achieve a positive goal, relative to a control condition. We also hypothesize that increased levels of episodic specificity during problem solving and reappraisal will lead to subsequent improvements in emotional well-being (e.g., reduced anxiety, changes in perceived plausibility of a positive and negative outcome) towards the target events.
Paper 2 (Jing, Madore, & Schacter, 2017) investigates the relationship between episodic specificity and emotional well-being by using a novel alternative event generation task in young adults. Existing literature has demonstrated that repeatedly dwelling on a negative future scenario can lead to maladaptive consequences by enhancing the perceived plausibility of the event’s occurrence (Szpunar & Schacter, 2013; Wu et al., 2015) and increasing the accessibility of negative event outcomes (Byrne & MacLeod, 1997; MacLeod et al., 1997). However, Bentz et al. (2004) showed that generating alternative positive outcomes can decrease the perceived likelihood that the original negative event will occur. Thus, we seek to use techniques to interrupt the cycle of repetitive thinking and reduce pessimistic likelihood judgments of the future. Since the generation and simulation of alternative event outcomes likely depends on episodic memory, we hypothesize that the ESI (Madore et al., 2014), which selectively impacts tasks reliant on episodic memory, should increase the number of positive alternative outcomes that participants generate towards anticipated negative events, relative to a control condition. In addition, considering alternative positive outcomes may grant access to additional information about situational and emotional factors that were previously ignored, which can encourage a subsequent adjustment of negative expectations. Thus, we also hypothesize that boosts in the number of alternative positive outcomes should lead to subsequent decreases in the perceived negativity and plausibility of the original negative events.

Paper 3 (Jing, Madore, & Schacter, 2019) aims to examine the relationship between the specificity of future-oriented problem solving and emotional well-being in older adults. Older adults show declines in a number of cognitive domains, including reductions in the specificity of past memories and future imaginations (for reviews, see Schacter, Gaesser, & Addis, 2013; Schacter, Devitt, & Addis, 2018), which may impact their ability to plan for and solve potential
stressors in their daily lives. Despite the observation that emotion regulation improves with age (Scheibe & Carstensen, 2010; Urry & Gross, 2010), older adults tend to experience a niche of health-related and interpersonal worries (Diefenbach, Stanley, & Beck, 2001; Hunt, Wisocki, & Wanko, 2003; Powers, Wisocki, & Whitbourne, 1992), and emotional disorders such as anxiety and depression are still prevalent in older adults (Djernes, 2006; Reynolds, Pietrzak, El-Gabalawy, Mackenzie, & Sareen, 2015). Thus, we aim to elucidate whether and how encouraging older adults to simulate the future in an episodically detailed manner (e.g., during problem solving) can improve emotion regulation towards worrisome happenings in their daily lives. In the first experiment, we administered the ESI (Madore et al., 2014) prior to asking older adults to generate steps to solve a series of worrisome personal problems in a MEPS task (Platt & Spivack, 1975), relative to a control condition. In the second experiment, we contrast levels of episodic specificity and concreteness during simulation on a personal MEPS task versus semantic reflection on a novel advice task towards worrisome future experiences. We hypothesize that in both experiments, increasing levels of episodic detail during problem solving should be positively related to improvements in emotional well-being.

Next, I present the three papers of the dissertation, followed by a concluding chapter that highlights the implications of each paper and potential steps for future research.
Paper 1:

Abstract

Previous research has demonstrated that an episodic specificity induction – brief training in recollecting details of a recent experience – enhances performance on various subsequent tasks thought to draw upon episodic memory processes. Existing work has also shown that mental simulation can be beneficial for emotion regulation and coping with stressors. Here we focus on understanding how episodic detail can affect problem solving, reappraisal, and psychological well-being regarding worrisome future events. In Experiment 1, an episodic specificity induction significantly improved participants’ performance on a subsequent means-end problem solving task (i.e., more relevant steps) and an episodic reappraisal task (i.e., more episodic details) involving personally worrisome future events compared with a control induction not focused on episodic specificity. Imagining constructive behaviors with increased episodic detail via the specificity induction was also related to significantly larger decreases in anxiety, perceived likelihood of a bad outcome, and perceived difficulty to cope with a bad outcome, as well as larger increases in perceived likelihood of a good outcome and indicated use of active coping behaviors compared with the control. In Experiment 2, we extended these findings using a more stringent control induction, and found preliminary evidence that the specificity induction was related to an increase in positive affect and decrease in negative affect compared with the control. Our findings support the idea that episodic memory processes are involved in means-end problem solving and episodic reappraisal, and that increasing the episodic specificity of imagining constructive behaviors regarding worrisome events may be related to improved psychological well-being.
Episodic simulation can be beneficial for emotion regulation by affecting levels of worry, affect, and the perceived plausibility of anticipated stressful events (e.g., Brown et al., 2002; Pham & Taylor, 1999; Rivkin & Taylor, 1999; Taylor et al., 1998). Additionally, the detail of one’s simulations (i.e., “internal” details containing specific episodic information about people, objects, and actions, versus “external” or semantic details containing factual information that is not specific to time and place, task commentary, etc.; Levine et al., 2002) may further impact subsequent emotional well-being. Suggestive evidence comes from populations with emotional disorders, where patients with depression (Williams et al., 1996) and anxiety disorders (Brown et al., 2014; McNally et al., 1994, 1995) report fewer episodic details when asked to recall past memories and imagine future events. Generalized anxiety disorder (GAD) patients also tend to worry about the future using verbal and conceptual loops of thought that lack specific, concrete details (Borkovec et al., 1998). Reduced specificity is thought to interfere with patients’ abilities to generate concrete plans and goals to cope with future stressors, ultimately exacerbating their symptoms (Borkovec et al., 1998; Williams, 2006; Williams et al., 1996, 2007).

Fortunately, recent interventions developed to increase memory specificity (e.g., Memory Specificity Training; Raes et al., 2009) have been linked to subsequent improvements in depressive symptoms (Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD symptoms (Moradi et al., 2014) with respect to negative and distressing past events. Furthermore, Madore et al. (2014) developed an episodic specificity induction (ESI; based on the Cognitive Interview; Fisher & Geiselman, 1992; Memon et al., 2010), a brief training in recollecting details of a recent experience that biases people to focus on specific event details and construct more detailed mental scenes or events only
during subsequent tasks that draw upon episodic retrieval (e.g., memory, imagination, problem solving, and divergent thinking tasks), but has no impact on the performance of subsequent tasks that are thought to rely on primarily semantic retrieval or non-episodic narrative processing (e.g., picture description, generating word definitions and object associations; Madore et al., 2014, 2015, 2016; Madore & Schacter, 2016; Schacter & Madore, 2016). Madore and Schacter (2014) demonstrated that using the ESI prior to a problem solving task can boost the number of steps and episodic details that young and older adults’ generate to solve everyday social problems, relative to a control condition. Given that reduced episodic specificity can hinder effective problem solving towards potential future obstacles, increasing the specificity with which people engage in constructive problem-solving behaviors using the ESI might serve as a useful intervention that can promote active coping and decrease overall maladaptive functioning.

In the current experiments we focus on two main avenues through which modulation of worrying about future events can be explored: (1) by taking steps to prevent a worrisome bad outcome and (2) by preparing to emotionally regulate or cope with a bad outcome after it occurs (Taylor & Schneider, 1989). The first avenue can be measured via the means-end problem solving paradigm (MEPS; Platt & Spivack, 1975), which involves a set of standardized problems which participants must generate steps (i.e., means) to solve. Patients with emotional disorders tend to show poorer performance on this task relative to healthy controls (Dickson & MacLeod, 2004; Goddard et al., 1996; Raes et al., 2005; Sidley, Whitaker, Calam, & Wells, 1997; Sutherland & Bryant, 2008), in part because the MEPS task is known to be reliant on episodic memory processes (Sheldon et al., 2011, 2015; Vandermorris et al., 2013) and reduced specificity of episodic memory has been documented in this population (McNally et al., 1994, 1995; Williams et al., 1996). As previously mentioned, Madore and Schacter (2014) showed that the ESI (Madore et al., 2014)
positively impacted performance on the MEPS task by increasing the number of relevant steps and
details generated for each problem. Given evidence from Pham and Taylor (1999) showing that
constructive simulations can benefit emotion regulation in response to stressful events, we
hypothesize that using the ESI before executing a MEPS task involving personally worrisome
events would increase the details participants generate to solve the worrisome events and further
improve emotional well-being concerning the problems used in the task.

The second avenue of examining modulation of worry about future events concerns
emotion regulation after a negative outcome takes place. Existing studies have primarily focused
on two emotion regulation strategies: cognitive reappraisal and expressive suppression, the former
of which has been demonstrated to be more effective (for review, see Gross, 1998). Cognitive
reappraisal is used to modulate responses to an affectively salient stimulus by reframing a negative
response to that stimulus or situation, and is effective in down-regulating emotional experience
and behavior (e.g., Goldin, Manber-Ball, Werner, Heimberg, & Gross, 2009; Goldin, McRae,
Ramel, & Gross, 2008; Hofmann, Heering, Sawyer, & Asnaani, 2009). In the present studies we
modified the traditional paradigm to involve reappraisal and reframing of a worrisome future
episode, where participants are asked to simulate a specific event in which they actively engage in
reappraisal regarding a negative outcome (for more details, see methods below). We will refer to
this process as episodic reappraisal. Suggestive evidence related to the potential usefulness of
episodic reappraisal comes from research on imaginal exposure treatment, during which PTSD
patients are asked to recall details of a traumatic event while focusing their attention on their
feelings, thoughts, and emotions (Arntz, Tiesema, & Kindt, 2007). This procedure has been found
to reduce the severity of PTSD symptoms, such as a reduction in fear, avoidance, and feelings of
helplessness (Arntz et al., 2007). While imaginal exposure treatment involves elaborating upon
negative details of a past experience and confronting that event, we hypothesize that elaborating upon negative details of a future outcome and reframing such a scenario (i.e., episodic reappraisal) could also be effective for emotion regulation. Critically, we suggest that utilizing a specificity induction to increase the amount of episodic detail in this reappraisal task would lead to even larger gains in subsequent measures of emotion regulation and well-being, compared with engaging in a reappraisal task with less specificity.

In summary, we tested the hypothesis that manipulating the level of specificity with which individuals imagine worrisome future events would influence subsequent measures of emotion regulation and well-being based on problem-solving and episodic reappraisal tasks. While the MEPS task primarily assesses participants’ ability to generate steps to prevent a bad outcome, the episodic reappraisal task targets their ability to cope with a negative outcome. In light of previous findings and theoretical ideas, we predicted that the episodic specificity induction, relative to a control induction, should (1) increase the number of relevant steps and internal details on the MEPS task (cf. Madore & Schacter, 2014), (2) increase the specificity with which participants perform the episodic reappraisal task, and (3) improve subjective measures of well-being and coping for a given problem.

**Experiment 1**

**Method**

**Participants.** A total of 35 healthy undergraduate students (ages 18 to 25, $M = 20.16$ years, 23 female) were recruited from Harvard College and Boston University. Participants were paid or received course credit for their participation. All participants had normal vision and no history of neurological or psychological impairment. A total of 10 participants were excluded due to experimenter error (2 participants), incompletion of the experiment (5 participants), or
noncompliance (3 participants), leaving 25 participants in the final sample. Before the study was run we performed a power analysis to determine that a sample size of at least 24 useable participants was necessary to observe a medium-sized effect of the induction (power > .80, α = .05, two-tailed, for a within-subjects design, \( d = 0.60 \)), which has also been the case in prior induction studies (e.g., Madore et al., 2014, 2015). Given scheduling constraints with multiple sessions, data collection was stopped once it was determined that approximately enough useable participants had been run to reach this number.

**Equipment.** All experimental sessions were executed using Qualtrics on an Apple desktop computer. During the induction phases, participants viewed the induction videos using Quicktime media player. Participants’ responses during the induction phases were recorded using an audio recorder.

**Experimental Procedure.** The experiment as a whole lasted approximately 6.5 hours across 3 separate sessions. The first session lasted 2.5 hours, during which participants provided 30 worrisome events. The second session took place 1 to 3 days after the first session (\( M = 1.72 \) days) and lasted 2 hours, and the third session took place 5 to 7 days after the second session (\( M = 5.88 \) days) and lasted 2 hours. In the second and third sessions, participants first completed an induction phase (specificity or control induction), and then completed two tasks (means-end problem solving and episodic reappraisal tasks) involving a subset of the worrisome events that they provided in the first session. See Figure 1.1 for a diagram of the experimental procedure.
**Session 1.** Participants provided 30 worrisome, anxiety-provoking problems or specific events that might take place in the near future (i.e., within the next 3-5 years). They were instructed to list specific, concrete, and highly familiar events or scenarios with tangible outcomes. Example categories of potential worries or problems included academics, health, career, relationships, and finances. Participants typically provided worries or problems from multiple categories, and were discouraged from listing events that involved the death of a loved one. Participants also generated a brief title and answered the following questions for each event they listed: (1) What exactly about this event worries you?; (2) What is the bad or negative outcome that you fear for this event?; and (3) What is the good or positive outcome that you hope will happen for this event? Responses to these questions were used to tailor the main experimental tasks to each participant.

Participants also rated each event on a 1 to 9 scale on the following dimensions (modified from Brown et al., 2002): (1) How anxious or worried are you about this problem or event?; (2) How likely is it that you will experience a good outcome for this event?; (3) How likely is it that you will experience a bad outcome for this event?; and (4) How difficult do you think it would be
to cope with a bad outcome for this event? Participants made these ratings in all three sessions, and changes in these ratings were used as subjective measures of well-being.

At the end of the first session, participants were asked to fill out the COPE Inventory (Carver, Scheier, & Weintraub, 1989), which can be divided into two separate composites for engagement coping (i.e., positive reinterpretation and growth, use of instrumental and emotional social support, active coping, planning) and disengagement coping (i.e., mental and behavioral disengagement, denial). We adapted the COPE Inventory to assess how participants judge that they will respond to the stressful events that they imagined in the experiment in the near future (i.e., the next week or month), instead of how they typically respond to stressors (e.g., Rivkin & Taylor, 1999). The COPE Inventory was administered after all three sessions to assess changes in indicated coping responses towards the worrisome events after the induction manipulation. Participants also completed the State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, & Lushene, 1970) at the end of the first session to examine whether trait anxiety might be associated with the quality of simulation or baseline levels of worry.

Sessions 2 and 3. The second and third experimental sessions consisted of multiple phases. First, participants completed an induction phase with either a specificity or control induction; only one induction was administered per session. Second, participants completed two tasks involving the worrisome events they provided in session 1: a means-end problem solving (MEPS) task and episodic reappraisal task.

Induction Phase. In the beginning of the second and third sessions, participants watched a short video of two adults performing routine activities in a kitchen; two different videos were used between induction conditions and the order of videos was counterbalanced across subjects. Following the videos, participants completed a math filler task (i.e., addition and subtraction
questions) for 2 minutes. Afterwards, participants either received questions about the video in the form of an episodic specificity induction or a control induction; only one induction was administered per session and the order of inductions was counterbalanced across subjects. In the episodic specificity induction, participants were given mental imagery probes asking them to recall specific details about the people, setting, and actions in the video, with follow-up probes to encourage them to elaborate more on the details they had mentioned. In the control induction, participants worked on a packet of math questions for the same amount of time (i.e., no episodic retrieval and elaboration required). The episodic specificity and control inductions use the same procedures that have produced significant effects on memory, imagination, and problem solving tasks in previous work (Madore et al., 2014; Madore & Schacter, 2014). Contrasting performance on the subsequent tasks between the two induction conditions allowed us to assess the effect of episodic detail (i.e., more episodic detail with the specificity induction versus baseline detail with the math control) on self-report subjective well-being measures concerning the events involved in the tasks. See Appendix for the specificity induction script.

**Experimental Tasks.** After the induction phase, participants completed two experimental tasks: the means-end problem solving (MEPS) task and the episodic reappraisal task. The events from session 1 were randomized and adapted into an appropriate format for each task using answers to questions from session 1 (e.g., “What is the bad or negative outcome that you fear for this event?” and “What is the good or positive outcome that you hope will happen for this event?”). Participants completed one practice trial and subsequently viewed 6 events in each task. The order of tasks was counterbalanced across subjects.

**Problem Solving.** In the means-end problem-solving task (MEPS; adapted from Madore & Schacter, 2014; Platt & Spivack, 1975), participants viewed 6 different problem stories relating to
the personal worrisome events they provided in session 1. Each story described the beginning of
the problem (e.g., worrying about the problem) and an ending solution (e.g., achieving the positive
outcome specified for the event in session 1). Participants were given 5 minutes to type out the
steps they would execute to reach the final solution in each problem in as much detail as possible.
They completed one practice trial with the experimenter before beginning the task to ensure that
they understood all instructions. MEPS trials in sessions 2 and 3 were created from 14 randomly
selected problems out of the 30 total problems that participants generated in session 1. See
Appendix for task instructions and a sample story.

Episodic Reappraisal. The episodic reappraisal task was adapted from traditional cognitive
reappraisal tasks that tap into primarily semantic knowledge of an emotional stimulus to aid in
reframing a negative response to that stimulus. For example, an experiment examining distorted
negative self-beliefs instructed participants to reinterpret the content of the belief; e.g., if the belief
is “No one likes me,” participants should tell themselves, “That is not always true, some people
like me,” or “This is only a thought, not a fact” (Goldin et al., 2009). These instructions are given
in order to bring attention to objective, factual information that detracts from more subjective,
emotional information about the stimulus or task. In the present study, we modified the typical
paradigm so that participants are required to focus on episodic details of a scenario in which they
are actively engaging in reappraisal of an imagined negative outcome, rather than focusing on
more semantic information about the situation.

Participants were presented with 6 negative-outcome scenarios for problems or events they
listed in session 1. For each event, participants were asked to (1) for 2 minutes, simulate a scenario
in which a negative outcome to the event took place, (2) rate how anxious or worried they felt
about the worrisome event, and (3) for 5 minutes, imagine themselves reinterpreting the situation
so that it becomes less negative to them and describe their thoughts, feelings, and actions as they are doing so in as much detail as possible. We included the 2-minute simulation component so that participants would be able to experience negative emotion towards the worrisome event prior to reappraising the event. Participants received one practice trial before the beginning of the task. Episodic reappraisal trials in sessions 2 and 3 were created from 14 randomly selected problems out of the 30 total problems that participants generated in session 1. See Appendix for task instructions and a sample scenario.

Participants received the same task instructions in sessions 2 and 3 regardless of induction condition, and focused on completing each task in as much detail as possible so that report criteria would be equated following the induction manipulation.

*Ratings.* After imagining each event during the MEPS and episodic reappraisal tasks, participants rated each event on a scale of 1 to 9 on the following: (1) How anxious or worried are you about this problem or event?; (2) How likely is it that you will experience a good outcome for this event?; (3) How likely is it that you will experience a bad outcome for this event?; and (4) How difficult do you think it would be to cope with a bad outcome for this event? We contrasted these ratings with the original ratings made in session 1 to examine changes in subjective measures of well-being and emotion regulation for the imagined events. For example, improved well-being could be marked by decreased ratings of anxiety, decreased plausibility for experiencing a bad outcome, increased plausibility for experiencing a good outcome, and decreased perceived difficulty in coping with a bad outcome.

*Questionnaires.* At the end of both sessions 2 and 3, participants once again completed the COPE Inventory questionnaire (Carver et al., 1989). Changes in responses to this questionnaire
measure shifts in indicated coping responses towards the worrisome events after the induction manipulation.

**Coding.** Three raters were trained to score responses from the 5-minute simulation components to both the problem-solving and episodic reappraisal tasks. Responses for the MEPS task were scored as a “relevant step”, “irrelevant step”, or “no step” using the step categories defined by Platt and Spivack (1975); for the analyses, irrelevant and no steps were collapsed into one “other steps” category (cf. Madore & Schacter, 2014; Sheldon et al., 2015). A relevant step is a step or event that leads towards the designated solution state or goal, an irrelevant step is a step or event that leads towards a different solution state not designated in the prompt, and a no step is information that does not fit the step framework (e.g., commentary about the task, repetitive or off-topic information). As in previous work, participants’ responses were also scored with the internal and external detail categories of the Autobiographical Interview (AI; see Levine et al., 2002; Madore et al., 2014; Sheldon et al., 2011). Internal details were segmented as any bits of episodic information contained in the responses (e.g., people, places, actions, objects, thoughts, feelings, etc. of the central event), and external details were segmented as any bits of other information contained in the responses (e.g., semantic facts and commentary, off-topic and repetitive information, etc.). In the MEPS task, internal details corresponded to episodic information (usually contained in relevant steps), whereas external details corresponded to semantic information (usually contained in other steps). Importantly, the MEPS task was scored for both steps and details because the two variables do not necessarily have a one-to-one correspondence. For example, individuals could provide more relevant steps with the specificity induction without much impact on detail, or they could provide more relevant steps and more
Responses for the episodic reappraisal task were also scored with the internal and external detail categories.

All raters were blind to the condition of the narratives (control, specificity). The three raters separately scored 20 participant practice trial responses (10 MEPS, 10 episodic reappraisal) to assess inter-rater reliability, and high inter-rater reliability was obtained for details (standardized Cronbach’s $\alpha = .977$ for internal details and $.982$ for external details) and steps (standardized Cronbach’s $\alpha = .973$ for relevant steps and $.926$ for other steps). The remainder of responses was scored by one of the three raters separately. Rater 1 scored 40% of participant responses, rater 2 scored 32% of participant responses, and rater 3 scored 28% of participant responses.

**Results**

We conducted a series of repeated-measures analyses of variance (ANOVAs) to test the hypotheses, which involved within-subjects factors of Induction (control vs. specificity), Task (MEPS vs. episodic reappraisal), Detail type (internal vs. external), Step type (relevant vs. other), and Time of Simulation (initial pre-simulation ratings during session 1 vs. post-simulation ratings during sessions 2 and 3). Both main effects and interactions were tested for each of the variables; we focus on the interactions to address the impact of induction on each of the variables. The counterbalanced order of induction and task did not have a significant effect on the analyses reported below.

**Event exclusion.** A total of 2.5% of event trials were excluded from the experiment (3.6% of reappraisal trials, 1.3% of MEPS trials) because the participant was unwilling or unable to perform the task, or because the participant actually experienced the event between session 1 and sessions 2 or 3.
**Induction Effects on Steps and Details.** We first examined how the specificity induction affected the steps generated in the MEPS task and details generated in both the MEPS and episodic reappraisal tasks (see Appendix Table 1.1 for mean steps and details) when equating for induction length. The duration of the control induction (i.e., math control task) was 4 min, $SD = 0$ min, and the mean duration of participants’ verbal responses during the specificity induction was 3.92 min, $SD = .98$ min.

For the MEPS task, we first conducted a 2 (Induction: control vs. specificity) x 2 (Step type: relevant vs. other) repeated-measures ANOVA. Critically, we found a significant interaction of Induction x Step Type, $F(1,24) = 71.63$, $p < .001$, $\eta^2_p = .75$. Two-tailed post hoc t-tests showed that participants generated significantly more relevant steps, $t(24) = -8.90$, $p < .001$, 95% CI = [-5.23, -3.26], $d = 1.78$, and significantly fewer other steps, $t(24) = 4.16$, $p < .001$, 95% CI = [.74, 2.20], $d = 0.83$, in the specificity condition compared to the control condition (Relevant steps: $M_{\text{difference}} = 4.25$, $SE = .48$; Other steps: $M_{\text{difference}} = -1.47$, $SE = .35$). Next, we conducted another 2 (Induction: control vs. specificity) x 2 (Detail type: internal vs. external) repeated-measures ANOVA, where we found a significant interaction of Induction x Detail Type, $F(1,24) = 51.88$, $p < .001$, $\eta^2_p = .68$. Participants generated significantly more internal details, $t(24) = -6.50$, $p < .001$, 95% CI = [-18.38, -9.52], $d = 1.30$, and significantly fewer external details, $t(24) = 4.32$, $p < .001$, 95% CI = [3.62, 10.25], $d = 0.86$, in the specificity condition compared to the control condition (Internal detail: $M_{\text{difference}} = 13.95$, $SE = 2.15$; External detail: $M_{\text{difference}} = -6.93$, $SE = 1.61$). Thus, the specificity induction effectively boosted the number of relevant steps (Figure 1.2A) and internal details (Figure 1.2B) that participants generated in the MEPS task. The number of relevant steps and internal details generated by participants were highly correlated, $r(23) = .94$, $p < .001$, 95% CI = [.87, .97].
In the episodic reappraisal task, we conducted another 2 (Induction: control vs. specificity) x 2 (Detail type: internal vs. external) repeated-measures ANOVA. Once again, we found a significant interaction of Induction x Detail Type, \( F(1,24) = 38.54, p < .001, \eta^2_p = .62 \), where participants generated significantly more internal details, \( t(24) = -4.78, p < .001, 95\% \text{ CI} = [-13.88, -5.50], d = 0.95 \), and fewer external details, \( t(24) = 3.72, p = .001, 95\% \text{ CI} = [1.76, 6.13], d = 0.74 \), in the specificity condition relative to the control condition (Internal: \( M_{\text{difference}} = 9.69, SE = 2.03 \); External: \( M_{\text{difference}} = -3.94, SE = 1.06 \)). Just as it did in the MEPS task, the specificity induction boosted the number of internal details that participants generated in the episodic reappraisal task (Figure 1.2C). Because the specificity induction boosted both the number of relevant steps and internal details in both tasks, we are able to relate this increase in specificity to changes in subjective well-being concerning the imagined worrisome events.

**Figure 1.2.** Experiment 1 mean induction effects on steps and details in control and specificity conditions: (A) Relevant and other steps in means-end problem solving (MEPS) task; (B) Internal and external details in MEPS task; and (C) Internal and external details in episodic reappraisal task. The y-axis represents the mean number of steps or details per trial, and error bars represent one \( SE \) of the mean.

**Induction Effects on Ratings of Subjective Well-being.** Next, we contrasted pre-simulation (session 1) to post-simulation (sessions 2 and 3) changes in ratings of anxiety, perceived likelihood of a good or bad outcome, and perceived difficulty to cope with a bad outcome between
the control and specificity conditions to assess effects of the induction on changes of subjective well-being towards the imagined events. Although most changes from pre- to post-simulation ratings were significant (see Appendix Table 1.2 for mean ratings in MEPS task and Appendix Table 1.3 for mean ratings in episodic reappraisal task), overall changes from session 1 to sessions 2 and 3 are of limited interest because they could reflect the influence of multiple factors. Accordingly, we focus on the contrast between rating changes in the control and specificity conditions through a series of 2 (Induction: control vs. specificity) x 2 (Time of Simulation: pre- vs. post-simulation) repeated-measures ANOVAs. In the following analyses we tested for both main effects and interactions, and focus on reporting the interactions to address the impact of induction on each of the variables. Correlations examining the relationship between the change in internal detail and the change in ratings between the specificity and control conditions are reported in Appendix Table 1.4. Trait anxiety was not significantly related to any changes in ratings regarding the imagined events in the observed sample of participants (see Appendix Table 1.5).

**Change in Anxiety.** For perceived anxiety concerning the imagined events, the Task (MEPS vs. reappraisal) x Induction x Time of Simulation interaction was not significant, $F(1,24) = .03, p = .87, \eta^2_p = .001$. However, below we separate the analyses by task due to the difference in the nature of the tasks, but note that the results are the same when collapsed across tasks.

In the MEPS task, we found a significant interaction of Induction x Time of Simulation, $F(1,24) = 5.96, p < .05, \eta^2_p = .20$. There was a significant decrease in ratings of anxiety for the imagined events from pre- to post-simulation in both the control and specificity conditions, but critically, there was a significantly larger decrease in anxiety ratings in the specificity condition than in the control condition, $t(24) = 2.44, p < .05, 95\% \text{ CI} = [.08, .95], d = 0.49$ (Figure 1.3A). In the episodic reappraisal task, we also found a significant interaction of Induction x Time of
Simulation, $F(1,24) = 9.56, p < .01, \eta_p^2 = .29$. There was a significant decrease in ratings of anxiety for the imagined events from pre- to post-simulation in the control condition and specificity condition, but we observed a larger decrease in anxiety ratings in the specificity condition than in the control condition, $t(24) = 3.09, p < .01, 95\% \text{ CI} = [.16, .79], d = 0.62$ (Figure 1.4A). However, we note that there was a small but significant difference between initial anxiety ratings for trials in the control condition and specificity condition for both the MEPS task [$M_{\text{difference}} = .34, SE = .11, t(24) = -3.21, p < .01, 95\% \text{ CI} = [-.55, -.12], d = 0.64$] and episodic reappraisal task [$M_{\text{difference}} = .45, SE = .09, t(24) = -4.95, p < .001, 95\% \text{ CI} = [-.64, -.26], d = 0.98$].

**Change in Perceived Likelihood of a Bad Outcome.** For perceived likelihood of a bad outcome to the imagined events, the Task x Induction x Time of Simulation interaction was not significant, $F(1,24) = 1.02, p = .32, \eta_p^2 = .04$. Once again, we separate the following analyses by task due to the difference in the nature of the tasks.

In the MEPS task, we found a significant interaction of Induction x Time of Simulation, $F(1,24) = 4.96, p < .05, \eta_p^2 = .17$. There was a significant decrease in ratings of perceived likelihood of a bad outcome for the imagined events from pre- to post-simulation in both the control condition and specificity condition, but we observed a larger decrease in ratings of perceived likelihood of a bad outcome in the specificity condition than in the control condition, $t(24) = 2.23, p < .05, 95\% \text{ CI} = [.03, .72], d = 0.45$ (Figure 1.3B). In the episodic reappraisal task, we also found a significant interaction of Induction x Time of Simulation, $F(1,24) = 13.72, p = .001, \eta_p^2 = .36$. There was a significant decrease in ratings of perceived likelihood of a bad outcome from pre- to post-simulation only in the specificity condition, but there was a larger decrease in ratings of perceived likelihood of a bad outcome in the specificity condition than in the control condition, $t(24) = 3.70, p = .001, 95\% \text{ CI} = [.27, .97], d = 0.74$ (Figure 1.4B). We once again note that there was a small
but significant difference between initial likelihood ratings for trials in the control condition and specificity condition in only the episodic reappraisal task \( [M_{\text{difference}} = .46, SE = .18, t(24) = -2.53, p < .05, 95\% \text{ CI} = [-.83, -.08], d = 0.50] \).

**Change in Perceived Likelihood of a Good Outcome.** For perceived likelihood of a good outcome to the imagined events, the Task x Induction x Time of Simulation interaction was not significant, \( F(1,24) = .07, p = .79, \eta_p^2 < .01 \). Below, we separate the analyses by task due to the difference in the nature of the tasks.

In the MEPS task, we found a significant interaction of Induction x Time of Simulation, \( F(1,24) = 5.42, p < .05, \eta_p^2 = .18 \). We observed a significant increase in ratings of perceived likelihood of a good outcome for the imagined events from pre- to post-simulation in both the specificity and control conditions, but there was a larger increase in ratings of perceived likelihood of a good outcome in the specificity condition than in the control condition, \( t(24) = -2.33, p < .05, 95\% \text{ CI} = [-.82, -.05], d = 0.47 \) (Figure 1.3C). We did not find a significant change in ratings of perceived likelihood of a good outcome in the episodic reappraisal task.

**Change in Perceived Difficulty to Cope with a Bad Outcome.** For perceived difficulty to cope with a bad outcome to the imagined events, there was a significant interaction of Task x Induction x Time of Simulation, \( F(1,24) = 6.01, p < .05, \eta_p^2 = .20 \).

In the episodic reappraisal task, we found a significant interaction of Induction x Time of Simulation, \( F(1,24) = 26.61, p < .001, \eta_p^2 = .53 \). There was a significant decrease in ratings of perceived difficulty to cope with a bad outcome for the imagined events from pre- to post-simulation in both the control condition and the specificity condition, but we observed a larger decrease in ratings of perceived difficulty to cope with a bad outcome in the specificity condition than in the control condition, \( t(24) = 5.16, p < .001, 95\% \text{ CI} = [.44, 1.02], d = 1.03 \) (Figure 1.4C).
For the MEPS task, the Induction x Time of Simulation interaction was not significant, $F(1,24) = .31, p = .58$, $\eta^2_p = .01$, although there was a significant decrease in ratings of perceived difficulty to cope with a bad outcome for the imagined events from pre- to post-simulation in both the control and specificity conditions.

Overall, these results suggest that greater detail of simulation via the specificity induction is related to 1) a larger reduction in anxiety towards the imagined events in both tasks, 2) a larger reduction in the perceived likelihood of a bad outcome for the imagined events for both tasks, 3) a larger increase in the perceived likelihood of a good outcome for the imagined events in the MEPS task, and 4) a larger reduction in the perceived difficulty to cope with a bad outcome for the imagined events in only the episodic reappraisal task, relative to the control induction (see Appendix Table 1.4 for correlations).

Figure 1.3. Experiment 1 mean initial and post-simulation ratings in the control and specificity conditions in the MEPS task of: (A) Anxiety; (B) Perceived likelihood of a bad outcome; and (C) Perceived likelihood of a good outcome. All ratings were made on a 1 to 9 scale. The y-axis represents the mean rating per trial, and error bars represent one standard error of the mean.
Figure 1.4. Experiment 1 mean initial and post-simulation ratings in the control and specificity conditions in the episodic reappraisal task of: (A) Anxiety; (B) Perceived likelihood of a bad outcome; and (C) Perceived difficulty to cope with a bad outcome. All ratings were made on a 1 to 9 scale. The y-axis represents the mean rating per trial, and error bars represent one standard error of the mean.

**COPE Inventory Questionnaire.** The COPE Inventory was administered at the end of all three experimental sessions. Responses to the COPE Inventory were split into two composite scores for engagement coping (i.e., scale items related to positive reinterpretation and growth, use of social support, active coping, and planning) and disengagement coping (i.e., mental and behavioral disengagement, denial). There was a significant increase in indicated use of engagement coping behaviors from the initial session to both the control condition session \([M_{\text{change}} = 3.92, SE = 1.41, t(24) = -2.77, p < .05, 95\% \text{ CI} = [-6.84, -1.00], d = 0.56]\), and the specificity condition session \([M_{\text{change}} = 5.20, SE = 1.24, t(24) = -4.20, p < .001, 95\% \text{ CI} = [-7.76, -2.64], d = 0.84]\). There was a slightly larger increase in indicated use of engagement coping in the specificity condition relative to the control condition, but the difference between the change scores from the initial session to the post-simulation sessions reached only trending significance, \(t(24) = -1.93, p = .066, 95\% \text{ CI} = [-2.65, .09], d = 0.38\) (Figure 1.5). There was no significant difference in indicated use of disengagement coping behaviors from the initial session to the control \([M_{\text{change}} = -.52, SE = .91, t(24) = .57, p = .57, 95\% \text{ CI} = [-1.35, 2.39], d = 0.11]\) or specificity sessions \([M_{\text{change}} = -1.04, SE = .87, t(24) = 1.20, p = .24, 95\% \text{ CI} = [-.75, 2.83], d = 0.24]\).
Figure 1.5. Experiment 1 mean engagement coping composite score from COPE Inventory scale items in the initial session (session 1), control and specificity sessions (sessions 2 and 3). The minimum composite score is 20 and the maximum composite score is 80. The y-axis represents the mean total score across questions, and error bars represent one standard error of the mean.

**Experiment 1 Discussion**

Overall, the results of Experiment 1 support the hypothesis that increasing episodic detail of simulation for constructive behaviors concerning worrisome events leads to improved subjective well-being towards those events. Using an episodic specificity induction increased the number of relevant steps and internal details that participants generated during a means-end problem-solving (MEPS) task and also increased the number of internal details generated during an episodic reappraisal task. Importantly, in the specificity condition relative to the control condition, we observed larger decreases in anxiety towards the worrisome events in both tasks, larger decreases in perceived likelihood of a bad outcome in both tasks, larger increases in perceived likelihood of a good outcome in the MEPS task (although the Task x Induction x Time of Simulation interaction was not significant), and larger decreases in perceived difficulty to cope with a bad outcome in only the episodic reappraisal task. There was also a trending increase in indicated use of engagement coping behaviors in the specificity condition relative to the control condition. These results suggest that episodic detail of simulation may be positively related to improved subjective well-being across a number of different measures.
As noted earlier, there were significant differences in the initial ratings of anxiety and perceived likelihood of a bad outcome between the control and specificity conditions, which limit our interpretation of the results. However, the direction of the difference (i.e., events in the specificity condition had higher initial anxiety and higher perceived likelihood of a bad outcome ratings) is opposite to the final pattern of anxiety ratings (i.e., events in the specificity condition had lower post-simulation anxiety and lower perceived likelihood of a bad outcome ratings), which makes it improbable that the difference in change between the specificity and control conditions is purely attributable to an initial difference in ratings. However, to account for this possibility, we aimed to more evenly match initial ratings for anxiety, perceived likelihood of a good or bad outcome, and perceived difficulty to cope with a bad outcome in Experiment 2.

**Experiment 2**

In Experiment 2, we aimed to extend the results of Experiment 1 after more evenly matching initial ratings of subjective well-being and using a different control induction than the math control used in Experiment 1. It is possible that the effects we attributed to specific episodic retrieval in Experiment 1 instead reflect other differences between the specificity induction and math control condition, such as the general requirement to think back to and talk about the video during the specificity induction. To address this issue, in Experiment 2 we used a more stringent impressions control induction that requires participants to reflect on general characteristics of the video, while not requiring them to retrieve specific episodic details. Thus, contrasting performance following the specificity and impressions control inductions will allow us to conclude with more certainty that effects of the specificity induction can be attributed to retrieving episodic details, rather than talking about the video more generally. Previous research has demonstrated similar effects of the episodic specificity induction on subsequent memory and imagination tasks.
compared with the math control and impressions control conditions (Madore et al., 2014), but it is critical for theoretical interpretation of our results to determine whether the same pattern holds for the key dependent measures in the present study. Overall, the methods used in Experiment 2 are very similar to those of Experiment 1, with differences highlighted below.

**Method**

**Participants.** A total of 32 healthy undergraduates were recruited from Harvard University and Boston University (ages 18 to 25, \( M = 20.84 \) years, 20 female). A total of 6 participants were excluded due to noncompliance (1 participant) or incompletion of the experiment (5 participants), leaving 26 participants in the final analysis. A power analysis based on the average effect sizes in Experiment 1 for changes in subjective well-being ratings in the specificity versus control condition revealed that a sample size of 22 would provide the ability to detect an overall effect with power of > .80 (two-tailed test, \( \alpha = .05, d = 0.63 \)). To keep the sample size in Experiment 2 comparable to that of Experiment 1, we stopped data collection after reaching the same approximate number of useable participants.

**Questionnaires.** In addition to the COPE Inventory and STAI questionnaires administered in Experiment 1, participants filled out the Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988) to measure changes in positive and negative affect before and after the simulation tasks. The PANAS was administered after all three sessions.

**Experimental Procedure.** On average, session 2 took place 1.46 days after session 1, and session 3 took place 5.74 days after session 2. See Figure 1.1 for a diagram of the experimental procedure.
**Session 1.** Session 1 remained the same in Experiment 2. In preparing event stimuli for sessions 2 and 3, we matched events more evenly on initial ratings of anxiety, perceived likelihood of a good or bad outcome, and perceived difficulty to cope with a bad outcome.

**Sessions 2 and 3.** Participants first completed the induction phase with either a specificity or control induction, and subsequently completed the problem solving and episodic reappraisal tasks. While the specificity induction procedures remained the same as in Experiment 1, we utilized a different control induction in Experiment 2, the impressions control induction. The order of inductions and tasks was counterbalanced across subjects.

*Impressions Control Induction.* After watching a short video and completing a math filler task, participants who received an impressions control induction were asked questions targeting general impressions, opinions, and thoughts about the video. The control induction did not require participants to retrieve specific episodic details about the video, while still allowing them to talk more generally about the video. See Appendix for the impressions control script.

*Problem Solving.* Participants viewed 6 problem stories related to their personal worrisome events and were asked to generate steps to reach a positive outcome. In contrast to Experiment 1, participants were also given 1 minute to imagine and describe a scenario in which they are worrying about the specified problem and to rate how anxious or worried they felt about the problem on a scale of 1 to 9, prior to generating steps to reach a positive outcome for 5 minutes. This format was adopted to match the time participants spent thinking about the worrisome event before the 5-minute simulation component in both the problem-solving and reappraisal tasks.

*Episodic Reappraisal.* The episodic reappraisal task consisted of 6 bad-outcome scenarios and was very similar to the version administered in Experiment 1. The only change from
Experiment 1 was that participants were asked to first simulate a scenario in which a bad outcome to the problem took place for only 1 minute (whereas they did so for 2 minutes in Experiment 1).

*Ratings and Questionnaires.* All participants were asked to answer the same ratings (i.e., anxiety, likelihood of good or bad outcome, difficulty to cope with a bad outcome) and questionnaires (i.e., COPE Inventory) that were administered in Experiment 1, with the addition of the PANAS questionnaire after all three sessions. Changes in responses to these ratings and questionnaires between session 1 and sessions 2 and 3 indicated shifts in subjective well-being towards the worrisome events after the induction manipulation.

*Coding.* The same three raters from Experiment 1 scored participant responses for Experiment 2 (i.e., inter-rater reliability coefficients for steps and details were high and the same in both experiments, all Cronbach’s αs ≥ .926). Rater 1 scored 38.5% of participant responses, rater 2 scored 23% of participant responses, and rater 3 scored 38.5% of participant responses.

**Results**

We tested our hypotheses by conducting a series of repeated-measures ANOVAs, which involved within-subjects factors of Induction (control vs. specificity), Task (MEPS vs. episodic reappraisal), Detail type (internal vs. external), Step type (relevant vs. other), and Time of Simulation (pre- vs. post-simulation). Below, we focus on the interactions to assess the effect of the inductions on each variable of interest. The counterbalanced order of induction and task did not have a significant effect on the following analyses.

*Event exclusion.* A total of 1.4% of event trials were excluded from the experiment (1.9% of reappraisal trials, 0.9% of MEPS trials) because the participant was unwilling or unable to perform the task, or because the participant actually experienced the event between session 1 and sessions 2 or 3.
**Induction Effects on Steps and Details.** Participants spent slightly longer discussing the video in the specificity induction ($M = 4.16$ min, $SD = 1.26$) than in the control induction ($M = 3.50$ min, $SD = .81$), $t(25) = -6.18$, $p < .001$, 95% CI = [-.89, -.44], $d = 1.23$. However, including the difference score for time as a covariate in the following repeated-measures ANOVAs did not significantly affect any results. See Appendix Table 1.6 for mean steps and details.

In the MEPS task, we first conducted a 2 (Induction: control vs. specificity) x 2 (Step type: relevant vs. other) repeated-measures ANOVA. Critically, we found a significant interaction of Induction (control vs. specificity) x Step Type (relevant vs. other), $F(1,25) = 39.60$, $p < .001$, $\eta_p^2 = .61$. Post hoc t-tests showed that participants generated significantly more relevant steps, $t(25) = -6.02$, $p < .001$, 95% CI = [-4.83, -2.37], $d = 1.18$, and significantly fewer other steps, $t(25) = 4.25$, $p < .001$, 95% CI = [.53, 1.53], $d = 0.83$, in the specificity condition compared to the control condition (Relevant steps: $M_{difference} = 3.60$, $SE = .59$; Other steps: $M_{difference} = -1.03$, $SE = .24$).

Next, we conducted another 2 (Induction: control vs. specificity) x 2 (Detail type: internal vs. external) repeated-measures ANOVA, where we also found a significant interaction of Induction x Detail Type (internal vs. external), $F(1,25) = 24.29$, $p < .001$, $\eta_p^2 = .49$. Participants generated significantly more internal details, $t(25) = -4.49$, $p < .001$, 95% CI = [-14.12, -5.24], $d = 0.88$, and significantly fewer external details, $t(25) = 2.87$, $p < .01$, 95% CI = [1.57, 9.53], $d = 0.56$, in the specificity condition compared to the control condition (Internal detail: $M_{difference} = 9.68$, $SE = 2.16$; External detail: $M_{difference} = -5.55$, $SE = 1.93$). The number of relevant steps and internal details generated by participants were highly correlated, $r(24) = .87$, $p < .001$, 95% CI = [.73, .94]. Thus, the specificity induction effectively boosted the number of relevant steps (Figure 1.6A) and the internal details (Figure 1.6B) that participants generated in the MEPS task, replicating and extending the effects of Experiment 1.
In the episodic reappraisal task, we conducted another 2 (Induction: control vs. specificity) x 2 (Detail type: internal vs. external) repeated-measures ANOVA. Once again, there was a significant interaction of Induction x Detail Type, $F(1,25) = 25.08, p < .001, \eta_p^2 = .50$. Participants generated significantly more internal details, $t(25) = -4.69, p < .001, 95\% \text{ CI} = [-12.34, -4.81], d = 0.92$, and fewer external details, $t(25) = 2.44, p < .05, 95\% \text{ CI} = [0.36, 4.22], d = 0.48$, in the specificity condition relative to the control condition (Internal: $M_{\text{difference}} = 8.57, SE = 1.83$; External: $M_{\text{difference}} = -2.29, SE = .94$). As in Experiment 1, the specificity induction boosted the number of internal details that participants generated in the episodic reappraisal task (Figure 1.6C).

![Figure 1.6](image.png)

**Figure 1.6.** Experiment 2 mean induction effects on steps and details in control and specificity conditions: (A) Relevant and other steps in means-end problem solving (MEPS) task; (B) Internal and external details in MEPS task; and (C) Internal and external details in episodic reappraisal task. The y-axis represents the mean number of steps or details per trial, and error bars represent one standard error of the mean.

**Induction Effects on Ratings of Subjective Well-being.** Next, we contrasted pre-simulation and post-simulation ratings of anxiety, perceived likelihood of a good or bad outcome, and perceived difficulty to cope with a bad outcome between the control and specificity conditions to assess effects of the specificity induction on changes in subjective well-being towards the imagined events. Unlike Experiment 1, there were no significant differences in baseline ratings for any of the variables. Although most overall changes from pre- to post-simulation ratings were significant (see Appendix Table 1.7 for mean ratings in MEPS task and Appendix Table 1.8 for
mean ratings in episodic reappraisal task), because it is not clear how to interpret these changes, as in Experiment 1 we focus on the *contrast* between rating changes in the control and specificity conditions through a series of 2 (Induction: control vs. specificity) x 2 (Time of Simulation: pre- vs. post-simulation) repeated-measures ANOVAs. In the following analyses we tested for both main effects and interactions, and focus on reporting the interactions to address the impact of induction on each of the variables. Correlations examining the relationship between the change in internal detail and the change in ratings between the specificity and control conditions are reported in Appendix Table 1.9. Trait anxiety was not significantly related to any changes in ratings concerning the imagined events in the observed sample of participants (see Appendix Table 1.10).

**Change in Anxiety.** For perceived anxiety concerning the imagined events, the Task (MEPS vs. reappraisal) x Induction x Time of Simulation interaction was not significant, $F(1,25) = .70, p = .41, \eta^2_p = .03$. Below, we separate the analyses by task due to the difference in the nature of the tasks, but note that the results are the same when collapsed across tasks.

In the MEPS task, we found a significant interaction of Induction x Time of Simulation (Pre vs. Post), $F(1,25) = 6.36, p < .05, \eta^2_p = .20$. There was a significant decrease in ratings of anxiety for the imagined events from pre- to post-simulation in both the control and specificity conditions, but importantly, we observed a larger decrease in anxiety ratings in the specificity condition than in the control condition, $t(25) = 2.52, p < .05, 95\% \text{ CI} = [.07, .72], d = 0.49$ (Figure 1.7A). In the episodic reappraisal task, there also was a significant interaction of Induction x Time of Simulation, $F(1,25) = 9.50, p < .01, \eta^2_p = .28$. There was a significant decrease in anxiety ratings for the imagined events in both the control and specificity conditions, but we observed a larger decrease in anxiety ratings in the specificity condition than in the control condition, $t(25) = 3.08, p < .01, 95\% \text{ CI} = [.21, 1.06], d = 0.60$ (Figure 1.8A).
**Change in Perceived Likelihood of a Bad Outcome.** For perceived likelihood of a bad outcome to the imagined events, the Task x Induction x Time of Simulation interaction was not significant, $F(1,25) = 2.50, p = .13, \eta^2_p = .09$. Once again, we separate the following analyses by task due to the difference in the nature of the tasks.

In the MEPS task, we found a trending interaction of Induction x Time of Simulation, $F(1,25) = 3.91, p = .059, \eta^2_p = .14$. There was a significant decrease in ratings of perceived likelihood of a bad outcome for the imagined events in both the control condition and the specificity conditions, but there was a larger decrease in ratings of perceived likelihood of a bad outcome in the specificity condition than in the control condition that showed only trending significance, $t(25) = 1.98, p = .059, 95\% \text{ CI} = [-.01, .69], d = 0.39$ (Figure 1.7B). In the episodic reappraisal task, we found a significant interaction of Induction x Time of Simulation, $F(1,25) = 17.68, p < .001, \eta^2_p = .41$. There was also a significant decrease in ratings of perceived likelihood of a bad outcome for the imagined events in both the control and the specificity conditions, but once again there was a larger decrease in ratings of perceived likelihood of a bad outcome in the specificity condition than in the control condition, $t(25) = 4.21, p < .001, 95\% \text{ CI} = [.33, .98], d = 0.82$ (Figure 1.8B).

**Change in Perceived Likelihood of a Good Outcome.** For perceived likelihood of a good outcome to the imagined events, there was a significant interaction of Task x Induction x Time of Simulation interaction, $F(1,25) = 8.52, p < .01, \eta^2_p = .25$.

In the MEPS task, we found a significant interaction of Induction x Time of Simulation, $F(1,25) = 7.53, p < .05, \eta^2_p = .23$. There was a significant increase in ratings of perceived likelihood of a good outcome for the imagined events in both the control and specificity conditions, but there was a larger increase in ratings of perceived likelihood of a good outcome in the specificity condition.
condition than in the control condition, \( t(25) = -2.75, p < .05, 95\% \text{ CI} = [-.60, -.09], d = 0.54 \) (Figure 1.7C). There were not significant changes in ratings of perceived likelihood of a good outcome in the episodic reappraisal task.

**Change in Perceived Difficulty to Cope with Bad Outcome.** For perceived difficulty to cope with a bad outcome to the imagined events, we found a significant interaction of Task x Induction x Time of Simulation, \( F(1,25) = 15.46, p = .001, \eta^2_p = .38 \).

In the episodic reappraisal task, there was a significant interaction of Induction x Time of Simulation, \( F(1,25) = 20.97, p < .001, \eta^2_p = .46 \). We observed a significant decrease in ratings of perceived difficulty to cope with a bad outcome for the imagined events in both the control and specificity conditions, but there was a larger decrease in ratings of perceived difficulty to cope with a bad outcome in the specificity condition than in the control condition, \( t(25) = 4.58, p < .001, 95\% \text{ CI} = [.32, .85], d = 0.90 \) (Figure 1.8C). For the MEPS task, the Induction x Time of Simulation interaction was not significant, \( F(1,25) = 1.85, p = .19, \eta^2_p = .07 \).

Consistent with the results of Experiment 1, these results suggest that greater detail of simulation via the specificity induction is related to 1) a larger reduction in anxiety towards the imagined events in both tasks, 2) a larger reduction in the perceived likelihood of a bad outcome for the imagined events for both tasks, 3) a larger increase in the perceived likelihood of a good outcome for the imagined events in only the MEPS task, and 4) a larger reduction in the perceived difficulty to cope with a bad outcome for the imagined events in only the episodic reappraisal task, relative to the control induction (see Appendix Table 1.9 for correlations).
Figure 1.7. Experiment 2 mean initial and post-simulation ratings in the control and specificity conditions in the MEPS task of: (A) Anxiety; (B) Perceived likelihood of a bad outcome; and (C) Perceived likelihood of a good outcome. The y-axis represents the mean rating per trial, and error bars represent one standard error of the mean.

Figure 1.8. Experiment 2 mean initial and post-simulation ratings in the control and specificity conditions in the episodic reappraisal task of: (A) Anxiety; (B) Perceived likelihood of a bad outcome; and (C) Perceived difficulty to cope with a bad outcome. The y-axis represents the mean rating per trial, and error bars represent one standard error of the mean.

**PANAS and COPE Inventory Questionnaires.** The PANAS and COPE Inventory questionnaires were administered at the end of all three sessions.

In examining the composite score for positive affect from the PANAS questionnaire, we found a significant increase in positive affect from the initial session to the specificity condition session \(M_{\text{change}} = 4.23, SE = 1.65, t(25) = -2.56, p < .05, 95\% \text{ CI} = [-7.64, -.82], d = 0.50\), but not in the control condition session \(M_{\text{change}} = 1.19, SE = 1.54, t(25) = -.78, p = .45, 95\% \text{ CI} = [-4.35,
While there was a slightly larger increase in positive affect in the specificity condition than in the control condition, the change scores from the initial session to the control and specificity conditions showed only trending significance, $t(25) = -1.89, p = .071, 95\% = [-6.36, .28], d = 0.37$ (Figure 1.9A).

There was a significant decrease in the composite score for negative affect from the initial session to both the control condition session [$M_{\text{change}} = -2.73, SE = 1.05, t(25) = 2.60, p < .05, 95\% \text{ CI} = [.57, 4.89], d = 0.51$] and specificity condition session [$M_{\text{change}} = -5.46, SE = .95, t(25) = 5.77, p < .001, 95\% \text{ CI} = [3.51, 7.41], d = 1.13$]. Overall, there was a larger decrease in negative affect in the specificity condition than in the control condition, $t(25) = 2.55, p < .05, 95\% \text{ CI} = [.52, 4.94], d = 0.50$ (Figure 1.9B).

**Figure 1.9.** Experiment 2 mean PANAS composite scores for (A) positive affect and (B) negative affect in the initial session (session 1), control and specificity sessions (sessions 2 and 3). The minimum composite score is 10 and the maximum composite score is 50 for both positive and negative affect. The y-axis represents the mean total score across scale items, and error bars represent one standard error of the mean.

For the engagement coping composite score from the COPE Inventory, there was a significant increase in the indicated use of engagement coping behaviors from the initial session to the specificity condition session [$M_{\text{change}} = 3.08, SE = 1.09, t(25) = -2.83, p < .01, 95\% \text{ CI} = [-5.32, -.84], d = 0.55$], but not in the control condition session [$M_{\text{change}} = .04, SE = 1.11, t(25) = -.04, p = .97, 95\% \text{ CI} = [-2.32, 2.24], d < 0.01$]. There was a significantly larger increase in the
indicated use of engagement coping behaviors in the specificity condition compared to the control condition, $t(25) = -2.71, p < .05, 95\% \text{ CI} = [-5.35, -0.73], d = 0.53$ (Figure 1.10). There was no significant difference in indicated use of disengagement coping behaviors from the initial session to the control [$M_{change} = .23, SE = .66, t(25) = -3.5, p = .73, 95\% \text{ CI} = [-1.59, 1.13], d = 0.07$] or specificity sessions [$M_{change} = -.58, SE = .72, t(25) = .8, p = .43, 95\% \text{ CI} = [-.91, 2.06], d = 0.16$].

**Figure 1.10.** Experiment 2 mean engagement coping composite score from COPE Inventory scale items in the initial session (session 1), control and specificity sessions (sessions 2 and 3). The minimum composite score is 20 and the maximum composite score is 80. The y-axis represents the mean total score across questions, and error bars represent one standard error of the mean.

**Experiment 2 Discussion**

The results of Experiment 2 extend the results of Experiment 1 using a tighter control condition (i.e., impressions control induction) and matching initial subjective well-being ratings. The episodic specificity induction increased the number of relevant steps and internal details that participants generated during the MEPS task and also increased the number of internal details generated during an episodic reappraisal task. Critically, in the specificity condition relative to the control condition, we observed larger decreases in anxiety towards the worrisome events in both tasks, larger decreases in the perceived likelihood of a bad outcome in both tasks, larger increases in the perceived likelihood of a good outcome in only the MEPS task, and larger decreases in the perceived difficulty to cope with a bad outcome in only the reappraisal task. There was also a
trending increase in overall positive affect, a larger decrease in negative affect, and a larger increase in indicated post-experimental use of engagement coping behaviors concerning the imagined events in the specificity condition relative to the control condition. These results confirm that the observed changes in ratings between the control and specificity conditions are not merely consequences of baseline differences in ratings. Similar to the results in Experiment 1, these data suggest that episodic detail of simulation may be positively related to improved subjective well-being across a number of different measures.

**General Discussion**

Overall, the data from both experiments support the hypothesis that increasing the level of episodic detail when imagining constructive behaviors regarding worrisome events is related to improved psychological well-being towards those events. We note three key findings to emerge from the two experiments reported here. First, using an episodic specificity induction that selectively targets episodic processes increased both the number of relevant steps and internal details that participants generated during a means-end problem-solving (MEPS) task involving real, personalized problems, thus replicating and extending previous results by Madore and Schacter (2014). Second, we demonstrated for the first time that the specificity induction boosts the internal details generated in an episodic reappraisal task. Traditional cognitive reappraisal tasks are used to down-regulate negative emotional responses to an affective stimulus, and task instructions are primarily semantic in nature, in the sense that they draw attention to factual information about the stimulus that detracts from the more arousing emotional information (e.g., Goldin et al., 2008, 2009). In the present study, we created a paradigm that required participants to imagine a specific, concrete event where they are actively engaging in reappraisal of a negative future outcome, presumably making the task more episodic in nature (episodic reappraisal). Given
that the specificity induction selectively boosted internal details in this episodic reappraisal task, this finding suggests that the modified paradigm indeed engages episodic memory processes.

Third, we provide novel evidence that increasing the specificity of simulated constructive behaviors for worrisome future events via the specificity induction can be positively related to psychological well-being towards those events. Although previous experiments have demonstrated that increasing the specificity of autobiographical memory can be linked to improvements in depressive and PTSD symptoms for distressing past events (Moradi et al., 2014; Neshat-Doost et al., 2012; Raes et al., 2009), existing literature on the impact of future simulation on subjective well-being has thus far relied on correlational evidence. For example, Brown et al. (2002) demonstrated that quality of future event simulation (e.g., temporal ordering, logic of sequential steps generated) is correlated with improved well-being; however, the authors did not document or even investigate the importance of episodic detail. Other studies have explicitly manipulated the level of specificity of future event simulation (Williams et al., 1996), but have not directly linked changes in specificity to measures of psychological well-being. In the present studies, we directly manipulated episodic detail by using the specificity induction and subsequently assessed changes in subjective well-being based on this selective increase in episodic detail. Notably, we found that an increase in relevant steps and internal details produced by the specificity induction is related to larger decreases in anxiety towards the worrisome events in both tasks, larger decreases in the perceived likelihood of a bad outcome in both tasks, larger increases in the perceived likelihood of a good outcome in only the MEPS task, and larger decreases in the perceived difficulty to cope with a bad outcome in only the episodic reappraisal task. We also report a trending increase in overall positive affect and a significant reduction in negative affect, as well as an increase in the indicated use of engagement coping behaviors concerning the
imagined events at a later time point in the specificity condition relative to the control condition. Thus, experimentally increasing episodic specificity of imagining constructive behaviors regarding worrisome future events may be related to improved subjective well-being towards the imagined events on a number of different measures.

How might an increase in episodic detail produced by the specificity induction relate to improvements in subjective well-being towards imagined worrisome future events? First, the specificity induction prompts individuals to retrieve episodic details related to people, objects, places, and actions, which leads them to focus on describing similar types of details when they later create mental events during the MEPS and episodic reappraisal tasks. We have argued previously (Schacter & Madore, 2016) that creating coherent mental events in part involves the construction of internal scenes (Hassabis & Maguire, 2007), and that the specificity induction increases the details associated with elements of a scene such as the people, setting, and objects, as well as the relation of these elements to one another within a mental scene (see Schacter & Madore, 2016, for further theoretical elaboration). Worry, as it is manifest in disorders such as GAD, is thought to be a primarily verbal and abstract process that reduces the concreteness of the visual imagery associated with simulations of a worrisome event and can minimize physiological response to a stressful trigger (Borkovec et al., 1998). By this logic, worry likely results in reduced concreteness of a mentally constructed event or scene. If the verbal, conceptual nature of worry serves to avoid the arousing emotional processing that comes with detailed visual imagery of an aversive event at the expense of generating concrete steps to resolve the worry (Borkovec et al., 1998), increasing the specificity of constructive mental simulations regarding these worrisome events might counter this effect by making the event more concrete and tangible.
Researchers have proposed that mental simulations possess a number of intrinsic properties that benefit emotion regulation (Taylor et al., 1998; Taylor & Schneider, 1989), and we argue that increasing episodic detail of simulation may augment multiple, and possibly all, steps in this process. First, mental simulation can make events “seem real or true” (Taylor & Schneider, 1989). That is, simulating a hypothetical event can make the scenario seem more realistic and concrete by providing more information about how the event might take place, and thus can enhance the subjective likelihood that the event will actually occur (Anderson, 1983; Carroll, 1978; Gregory, Cialdini, & Carpenter, 1982; Szpunar & Schacter, 2013). Increasing episodic detail of simulation via a specificity induction, relative to baseline levels of detail, can thus provide even more information about how the event might unfold, further contributing to an increase in plausibility that the event will take place. In support of this idea, we show that in the MEPS task, simulating more relevant steps (and internal, episodic details associated with those steps) to reach a positive outcome increased the perceived likelihood that the positive outcome would take place. Furthermore, increased episodic detail while simulating positive, constructive tasks (i.e., generating steps to solve a problem and reappraising a bad outcome into something less negative) decreased the perceived likelihood that a bad outcome would take place. Thus, manipulating the plausibility of an event may be one avenue through which episodic detail might affect subjective well-being.

Second, enhancing the likelihood of an event might pave the road for taking action. Taylor et al. (1998) proposed that simulations consist of a sequence of actions that tend to be causally linked, and this organization of action can help to yield a concrete plan. The concreteness of simulation can provide important information about the event that contributes to a more realistic representation of the constraints and requirements of the event or task (Hayes-Roth & Hayes-Roth,
In the present studies, generating more episodic detail while trying to reach a good outcome or reframing a bad outcome may have led participants to formulate more detailed sequences of action that produced a more concrete plan. It is also likely that individuals were able to gain access to relevant, more realistic features of an imagined scenario or plan that may not have been as negative as initially thought. Related to this point, our data showed that participants reported a larger decrease in the perceived difficulty to cope with a bad outcome after imagining themselves coping with a bad outcome in more episodic detail. The reported decrease in the perceived likelihood of a bad outcome may also speak to this point, such that participants might have accessed critical details about why a bad outcome was unlikely to occur after generating a more detailed plan to reach a positive outcome or after reframing a negative outcome. Thus, increasing the organization of action and the access to realistic details about constructive behaviors concerning a worrisome event may be another way in which episodic detail can improve subjective well-being.

Given that simulations can increase the subjective likelihood that an event might take place, that they contain an implicit organizational structure that can yield a plan, and that they facilitate access to more realistic representations of the event, mental simulations may thus provide links between thought and action, making it more likely for individuals to execute the plan at hand (Taylor et al., 1998; Taylor & Schneider, 1989). Indeed, we report suggestive evidence that there was a larger increase in the indicated post-experimental use of engagement coping strategies in the COPE Inventory questionnaire (e.g., positive reinterpretation and growth, use of instrumental and emotional social support, active coping, planning; Carver et al., 1989) towards the imagined events in the specificity condition relative to the control condition. While these data only speak to an increase in the intention of action and not the execution of action directly, we believe that the
demonstrated effects of episodic detail on self-reported psychological well-being take us a step closer to linking simulation and action. Overall, using an episodic specificity induction to increase episodic detail of mental simulation might serve as an upstream intervention that can augment all of these links, thus leading to positive downstream consequences such as a reduction in anxiety concerning a worrisome event and improving psychological well-being as a whole.

This research may have implications for clinical populations, and particularly for patients with anxiety disorders. It has often been shown that clinically anxious individuals report inflated subjective probabilities and greater anticipation that negative events will occur (Barlow, 2000; Butler & Mathews, 1983; MacLeod & Byrne, 1996; MacLeod et al., 1997), as well as increased vividness for negative events (Morina, Deeprose, Pusowski, Schmid, & Holmes, 2011; Stöber, 2000). These findings have been interpreted in the context of the simulation heuristic (Kahneman & Tversky, 1982), in that anxious individuals tend to have increased access to reasons for why negative events might occur, and reduced access to reasons for why they might not occur (Byrne & MacLeod, 1997; MacLeod, Williams, & Bekerian, 1991). However, Raune et al. (2005) reported that simulating reasons against why a negative event might happen lowered subjective probability estimates of the likelihood that a given negative event would take place. Along with our findings that the specificity induction results in simulating constructive behaviors in more episodic detail and increased likelihood estimates of a good outcome and decreased likelihood estimates that a bad outcome will take place, these results highlight the importance of positive and constructive mental future simulations for emotion regulation and psychological well-being.

It is also important to note that subjective well-being towards worrisome events may be modulated by other important aspects of the events. For example, in generating concrete plans and goals, qualitative features of implementation (e.g., ease, perceived likelihood of success) may also
modulate subjective well-being (for review, see Eccles & Wigfield, 2002). That is, generating steps that are more easily achievable and attainable may contribute more to subjective well-being than generating steps that are more difficult to achieve. Furthermore, personal significance and importance of a goal or worrisome event might also influence how beneficial simulation might be for a given event (Eccles & Wigfield, 2002; Emmons, 1986), such that a richer mental simulation of a worrisome event that holds more importance and weight might lead to larger gains in subjective well-being than simulation of a worrisome event of less importance. Thus, it is not only the quantity of rich, concrete details that individuals generate in mental simulations of constructive behaviors regarding worrisome future events that is important for psychological well-being; there are also other facets of worrisome events that can influence an individual’s subjective well-being towards the event. Although the present data cannot speak to this issue, we believe that further research is necessary to clarify how these different factors might influence psychological well-being and emotion regulation in relation to worrisome future events.

In summary, the results of our experiments extend the range of tasks on which a specificity induction selectively boosts episodic detail to means-end problem solving and episodic reappraisal of personally worrisome future events, and demonstrate that increased episodic detail of simulation can be positively related to improved subjective well-being across a number of different measures. While further research is needed to explore the exact mechanism behind how episodic detail might influence downstream factors such as plausibility, motivation, and taking action, this line of work could have important implications for understanding the regulation of future-oriented emotion in both healthy and clinical populations.
Paper 1 Acknowledgements

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Paper 2:

Abstract

A critical adaptive feature of future thinking involves the ability to generate alternative versions of possible future events. However, little is known about the nature of the processes that support this ability. Here we examined whether an episodic specificity induction – brief training in recollecting details of a recent experience that selectively impacts tasks that draw on episodic retrieval – 1) boosts alternative event generation and 2) changes one’s initial perceptions of negative future events. In Experiment 1, an episodic specificity induction significantly increased the number of alternative positive outcomes that participants generated to a series of standardized negative events, compared with a control induction not focused on episodic specificity. We also observed larger decreases in the perceived plausibility and negativity of the original events in the specificity condition, where participants generated more alternative outcomes, relative to the control condition. In Experiment 2, we replicated and extended these findings using a series of personalized negative events. Our findings support the idea that episodic memory processes are involved in generating alternative outcomes to anticipated future events, and that boosting the number of alternative outcomes is related to subsequent changes in the perceived plausibility and valence of the original events, which may have implications for psychological well-being.
According to the constructive episodic simulation hypothesis (Schacter & Addis, 2007), having a constructive, flexible episodic memory system allows individuals to imagine or simulate future scenarios by drawing on one’s past experiences. This ability to simulate future scenarios is thought to serve an adaptive preparatory function, enabling the consideration of different ways in which the future may unfold without actually engaging in the behavior in question. As David Ingvar (1979, p. 21) theorized in his pioneering article, “On the basis of previous experiences, represented in memories, the brain—one’s mind—is automatically busy with extrapolation of future events and, as it appears, constructing alternative hypothetical behavior patterns in order to be ready for what may happen” (see also Schacter, 2012; Suddendorf & Corballis, 1997, 2007).

However, despite Ingvar’s initial emphasis on the importance of constructing alternative behavior patterns, there remains very little investigation of the actual simulation of alternative versions of future events. While there is a large literature on counterfactual thinking, or generating alternative outcomes and consequences for autobiographical past events (for recent reviews, see Byrne, 2016; Epstude & Roese, 2008), most research on future event simulation focuses on the construction of complex scenes and action sequences within the specific context of a single event, without considering possible alternative outcomes for such an event. There does exist a smaller body of research on prefactual thinking, a type of conditional future thought concerning alternative future outcomes that may occur with some degree of certainty (Byrne & Egan, 2004; Petrocelli, Seta, & Seta, 2012; Epstude, Scholl, & Roese, 2016). Prefactuals often take the form of, “If I take action X, then it may lead to outcome Y”; for example, “If I study for 5 hours, then I may get a better grade on my exam, or “If I only study for 1 hour, then I may do poorly” (for related work...
on implementation intentions, see Gollwitzer, 1999; Gollwitzer & Sheeran, 2006). However, there are many instances when individuals tend to harbor expectations for an emotional future event outcome (e.g., “I will do poorly on the exam next week”), without necessarily considering conditional features of the event in a “what-if” or “if-then” manner (Norem & Cantor, 1986; Oettingen & Mayer, 2002; Wilson & Gilbert, 2005). These predicted emotional outcomes can subsequently color one’s actual experience of the event (Showers, 1992; Wilson, Lisle, Kraft, & Wetzel, 1989). Thus, there remains much to be explored regarding the consideration of alternative outcomes to an expected future experience.

One potential benefit of generating alternative future outcomes relates to emotion regulation. Engaging in constructive thoughts towards negative future events is known to decrease negative emotions and worry (Brown et al., 2002; Jing et al., 2016; MacLeod, 2017) and increase engagement in active strategies to cope with a stressor (Pham & Taylor, 1999; Rivkin & Taylor, 1999; for review, see Taylor et al., 1998). However, in emotional disorders such as anxiety or depression, individuals may show greater anticipation of negative future experiences (e.g., MacLeod & Byrne, 1996) and harbor excessive worry about the future (e.g., Borkovec et al., 1998; see also Bulley et al., 2017; Miloyan et al., 2014). Negative thoughts towards the future can be maladaptive if one repeatedly engages in those thoughts, particularly given that repetitive future thinking has been linked to increased estimates in the perceived plausibility of an event’s occurrence (Szpunar & Schacter, 2013; Wu et al., 2015). Indeed, clinically anxious individuals find imagining negative future scenarios to be easier with repetition (Wu et al., 2015), and more generally, they tend to inflate subjective probabilities of negative events because they find negative outcomes and reasons for their occurrence to be highly accessible (Byrne & MacLeod, 1997; MacLeod & Byrne, 1996; MacLeod et al., 1991, 1997). Thus, it is critical to interrupt the cycle of
repetitive thinking and reevaluate the perceived plausibility of anticipated negative future experiences, and one way to do so may be to consider alternative outcomes to negative future events.

Bentz and colleagues (2004) proposed that using techniques to generate alternative positive outcomes may serve as a “debiasing” strategy to reduce pessimistic likelihood judgments of the future. If anxious individuals operate under the availability heuristic (Tversky & Kahneman, 1973) and judge negative outcomes to be highly plausible because they are highly accessible, then activating positive outcomes should increase the ease by which those outcomes may come to mind, suggesting that the anticipated event may not be as predictable as previously believed. Thus, considering positive alternatives may subsequently decrease the perceived likelihood that the original negative event will occur. Bentz et al. (2004) used a Consider-An-Alternative debiasing strategy (cf. Hirt & Markman, 1995) and found that asking both highly trait-anxious and non-anxious individuals to generate three alternative positive outcomes for a variety of negative scenarios significantly reduced participants’ ratings of event plausibility (see also Hirt et al., 2004; MacLeod et al., 1991; Raune et al., 2005). However, Bentz et al. (2009) also demonstrated that generating more alternative outcomes (e.g., five positive outcomes) incurred no additional benefit in debiasing judgments when compared to generating fewer alternative outcomes (e.g., two positive outcomes). One possible explanation is that task difficulty increases when participants are asked to generate a larger number of alternative outcomes, presumably decreasing the realistic and positive quality of the generated alternatives.

While Bentz et al. (2004, 2009) demonstrated that considering positive alternative event outcomes, rather than fixating on a single future experience, is beneficial for debiasing pessimistic judgments of the future, little is known about the processes that support alternative event
simulation, and fundamental issues remain to be addressed. The main goal of the present studies is to enhance our basic understanding of how people generate and simulate alternative outcomes to future events by addressing two issues that emerge from the earlier work of Bentz et al. (2004, 2009).

First, utilizing a tool that can boost the number of positive alternative event outcomes as well as boost the ease of generating these outcomes may help to determine whether the ease of alternative outcome generation is truly a limiting factor in the demonstrated effects of the Consider-An-Alternative debiasing intervention. One such tool is an episodic specificity induction, a brief training in recollecting details of a recent experience (Madore, Gaesser, & Schacter, 2014). This procedure is based on the Cognitive Interview (CI; Fisher & Geiselman, 1992; Memon et al., 2010), a well-established method that increases recall of episodic detail in eyewitnesses. Prior work on the specificity induction has shown that encouraging participants to focus on specific details of a past experience (e.g., of a short video they just watched or of a past autobiographical memory) selectively biases participants to focus on specific event details during subsequent tasks that are dependent on episodic memory; consequently, individuals tend to construct more detailed mental scenes or events after a specificity induction than a control induction (for further discussion, see Schacter & Madore, 2016).

Apart from increasing the level of detail when recalling past and imagining future experiences (Madore et al., 2014), other known effects of the specificity induction on subsequent tasks include boosting the number of steps generated during problem solving (Jing et al., 2016; Madore & Schacter, 2014), increasing the number of creative solutions generated during divergent thinking tasks (Madore et al., 2015, 2016), and boosting the level of concreteness and detail during episodic reappraisal (Jing et al., 2016). Critically, whereas the specificity induction impacts
performance on subsequent tasks that are thought to be dependent on episodic memory, it has no detectable impact on the performance of tasks that are thought to rely on primarily semantic retrieval or non-episodic narrative processing, such as describing a picture (Madore et al., 2014), generating word definitions (Madore & Schacter, 2016), or generating object associations and semantic solution words (Madore et al., 2015). We suggest that the generation and simulation of alternative event outcomes depends critically upon episodic memory, in a similar manner as other types of future event simulation. Accordingly, we predict that the specificity induction will increase the number of positive alternatives that participants generate, relative to a control induction. In addition, the specificity induction should boost the ease with which individuals generate these positive alternative outcomes, allowing us to examine whether such a boost may be linked to further debiasing of pessimistic future judgments.

Second, while event plausibility is a construct that influences one’s psychological well-being towards the event in question (Bentz et al., 2004, 2009; MacLeod et al., 1997), perceived event valence is also important. Research on affective forecasting has shown that individuals are quite inaccurate at estimating their emotional reactions to future experiences, often displaying an impact bias, whereby they overestimate the intensity of their emotional reactions, particularly for negative events (Andrade & Van Boven, 2010; Gilbert & Wilson, 2009; Wilson & Gilbert, 2005). Some proposed causes of these errors include underestimating the influence that other events or other aspects of the event in question may have on one’s thoughts and feelings, as well as misjudging how well one might emotionally adapt to the situation (Gilbert & Wilson, 2009; Wilson & Gilbert, 2005). Here, we suggest that generating alternative positive outcomes for a negative event might reduce the perceived negativity of the original event. Considering alternative positive outcomes grants access to additional information about ways in which the event may unfold,
including previously ignored details about other situational and emotional factors (Gilbert & Wilson, 2009; Taylor et al., 1998). This additional information, in turn, may lead one to realize that the event might not be as negative as initially perceived, and thus encourage a subsequent adjustment of negative expectations.

In the current experiments, we tested the hypothesis that manipulating the number of alternative future outcomes that individuals generate would influence subsequent measures of perceived event valence and plausibility. Based on previous findings, we predicted that the episodic specificity induction, relative to a control induction, should (1) increase the number of relevant positive alternative scenarios that participants generate, (2) boost the perceived ease of generating alternative event scenarios, and (3) decrease the perceived negativity and plausibility of the original event.

**Experiment 1**

**Method**

**Participants.** A total of 33 healthy undergraduate students (ages 18 to 25, $M = 20.93$ years, 22 female) were recruited from Harvard University. All participants had normal vision and no history of neurological or psychological impairment, and were paid or received course credit for their participation. A total of 7 participants were excluded due to failure to complete the experiment (4 participants), noncompliance (2 participants), or inability to perform the experimental tasks (1 participant), leaving 26 participants in the final sample. Before the study was run, we performed a power analysis (G*Power 3; see Faul, Erdfelder, Lang, & Buchner, 2007) to determine that a sample size of at least 23 useable participants was necessary to observe a medium-sized effect of the induction (power $> .80$, $\alpha = .05$, two-tailed, for a within-subjects design, $d = 0.62$), which has also been the case in prior induction studies (e.g., Jing et al., 2016; Madore et al., 2014, 2015).
Given scheduling constraints with multiple sessions, data collection was stopped once approximately enough useable participants had been run to reach this number.

**Equipment.** All experimental sessions were executed using Qualtrics on an Apple desktop computer. Participants viewed the induction videos using Quicktime media player, and verbal responses during the induction questioning procedures were recorded using an audio recorder.

**Experimental Procedure.** The experiment as a whole lasted 4 hours across 2 separate sessions that took place 4 to 7 days apart ($M = 5.19$ days). Both experimental sessions lasted 2 hours and were very similar in structure, consisting of several different phases: event simulation, induction phase (specificity or control induction), alternative event generation, and finally an alternative event rating phase.

**Event stimuli.** Across both experimental sessions, participants viewed a total of 12 standardized negative events that were specific, concrete, and had tangible outcomes that could plausibly occur within the next several years. These 12 event stimuli were scenarios that 30 separate participants had most frequently generated during a pilot study, during which they were asked to list a series of plausible and familiar negative future events that spanned across a variety of domains such as academics, health, career, relationships, and finances. The 12 events were separated into two lists of 6 events each, and presentation of the two lists during the two experimental sessions was counterbalanced across participants (see Appendix for event lists).

**Event Simulation Phase.** In each session, participants were presented with 6 standardized negative events. For each separate event, participants were first provided with the event title (e.g., “I receive a bad grade on my exam”) and were instructed to imagine experiencing the concrete future scenario specified by the title for 2 minutes. They were told that each event should take place in a specific location within a time frame of several hours, and were encouraged to elaborate
upon their negative thoughts and feelings in as much detail as possible. Afterwards, participants rated each event for valence, the perceived plausibility of occurrence, and the similarity of the future event to something they had previously experienced on a scale of 1 to 9. Participants first completed a practice trial with the experimenter to ensure they understood all instructions.

**Induction Phase.** The specificity and control inductions were administered in the same manner as in our previous studies (e.g., Jing et al., 2016; Madore et al., 2014). Following event simulation, participants watched a short video of two adults performing routine activities in a kitchen; two different videos were used between induction conditions and the order of videos was counterbalanced across subjects. Following the video, participants completed a 2-minute math filler task comprised of addition and subtraction questions. Next, participants received questions about the video in the form of either an episodic specificity induction or an impressions control induction; only one induction was administered per session and the order of inductions was counterbalanced across subjects. In the episodic specificity induction, participants were given mental imagery probes asking them to recall specific details about the setting, people and actions in the video, with follow-up probes asking them to elaborate more on the details they had mentioned. In the impressions control induction, participants were asked questions targeting their general impressions and thoughts about the video, which allowed them to talk more generally about the video without requiring them to retrieve specific episodic details (see Appendix for induction scripts). Contrasting the two induction conditions allowed us to assess the effect of a more specific retrieval orientation (as induced by the specificity induction) on participants’ subsequent abilities to generate alternative scenarios, as well as potential effects of a boost in alternative event generation on the perceived plausibility and valence of the original events.
**Alternative Event Generation Phase.** After receiving one induction, participants viewed the titles of each of the 6 negative events they simulated in the first phase. For each event, participants were given 5 minutes to generate and type out as many alternative scenarios to the original event as possible. They were told that they could incorporate a variety of different changes to the original event, such as modifying the outcome of the original event (e.g., doing well on an exam instead of doing poorly), altering emotional aspects or content of the original event by reframing the situation or emotionally reinterpreting the outcome (e.g., despite doing poorly, still doing better than many other students in the course), altering the perceived consequences of the event (e.g., the exam is only worth a miniscule portion of the final grade), or changing the imagined interactions they had with others in the original event (e.g., finding out about the grade over email instead of directly hearing from the professor). Critically, participants were instructed to generate alternative scenarios that were more positive than the original negative event. In addition, they were instructed to keep constant the core elements of the original event; for example, if the original event concerned a specific academic class, in all alternative event scenarios they should continue thinking about the same class and refrain from thinking about alternative scenarios in other classes.

After generating as many alternative event scenarios as possible for 5 minutes, participants were then asked to make three ratings on a scale of 1 to 9: 1) the ease or difficulty of generating alternative scenarios for each event, 2) the perceived valence of the original event, and 3) the perceived plausibility of the original event. Once again, participants first completed a practice trial with the experimenter to ensure they understood all instructions.

**Alternative Event Rating Phase.** Next, participants were presented with each alternative scenario that they listed in the previous phase (i.e., the exact sentence or phrase they typed during alternative event generation), and were asked to rate each scenario on five different dimensions on
a scale of 1 to 9: 1) valence, 2) plausibility, 3) similarity of the alternative scenario to a past experience, 4) similarity of the alternative scenario to something they had previously thought about (without actual experience), and 5) how realistic they perceived the alternative scenario to be.

**Coding.** Three raters were trained to score responses from the 5-minute alternative event generation task. Responses were scored as either a relevant or irrelevant alternative event scenario. A relevant alternative event is one that keeps constant the core elements of the original negative scenario while changing other aspects of the event, such as the outcome, emotional content, perceived consequences, and other imagined interactions from the original event (for more detail, see the alternative event generation phase description). An irrelevant alternative event is one that either 1) changes a core component of the original event (e.g., describing an alternative scenario concerning a history exam when the original event concerned a math exam), 2) does not pertain to the original scenario in a coherent or logical manner (e.g., for an original event where one does poorly on an exam, generating an alternative scenario where one visits a museum), or 3) does not actually change elements of the original scenario. We drew the distinction between relevant and irrelevant alternative outcomes to match the scoring procedures in prior work examining the effect of the specificity induction on generating steps to solve a problem (e.g., “relevant steps” vs. “no steps”; Jing et al., 2016; Madore & Schacter, 2014) and on details in memory and imagination tasks (e.g., “internal”, episodic event details that are relevant to the task vs. “external”, semantic details or other details deemed irrelevant to the task; Madore et al., 2014).

All raters were blind to the condition (control, specificity) of participant responses. The three raters separately scored 20 sample participant responses to assess inter-rater reliability, and high inter-rater reliability was obtained for both types of alternatives (standardized Cronbach’s $\alpha = .99$ for relevant alternatives and .92 for irrelevant alternatives). The remainder of responses was
scored separately by one of the three raters. Rater 1 scored 38% of participant responses, rater 2 scored 31% of participant responses, and rater 3 scored 31% of participant responses.

Results

We conducted a series of repeated-measures analyses of variance (ANOVAs) to test the hypotheses, which involved within-subject factors of Induction condition (control vs. specificity), Alternative event type (relevant vs. irrelevant), and Time of Rating (initial ratings prior to generating alternative events vs. ratings after generating alternative events). Both main effects and interactions were tested for each of the variables, but the interactions trumped the main effects and explicitly addressed our hypotheses. The counterbalanced order of induction did not have a significant effect on the following analyses. We subsequently conducted a series of linear multilevel models that examine the relationship between the variables of interest on an event trial-by-trial basis, rather than aggregating data into participant means.

Initial Event Ratings. There were no significant differences in initial ratings of event valence between the two lists collapsed across both control and specificity conditions, \( M_{\text{difference}} = -0.08, SE = 0.20, t(24) = -0.43, p > .250, 95\% \text{ CI } = [-0.49, 0.32], d = 0.17 \). We also did not find significant differences in initial ratings of event plausibility between lists, \( M_{\text{difference}} = -0.09, SE = 0.39, t(24) = -0.23, p > .250, 95\% \text{ CI } = [-0.89, 0.71], d = 0.09 \).

Induction Effects on Alternative Event Generation. We first examined how the specificity induction affected the number of alternative event scenarios that participants generated. Participants spent slightly longer discussing the video in the specificity induction (\( M = 4.74 \text{ min, } SD = 0.87 \)) than in the control induction (\( M = 4.02 \text{ min, } SD = 0.60 \)), \( M_{\text{difference}} = 0.72, SE = 0.10, t(25) = 6.89, p < .001, 95\% \text{ CI } = [0.50, 0.93], d = 1.35 \). However, including the difference score for
induction duration as a covariate in the following repeated-measures ANOVAs did not significantly affect any results.

We conducted a 2 (Induction: control vs. specificity) x 2 (Alternative Event Type: relevant vs. irrelevant) repeated-measures ANOVA. Results revealed significant main effects of Induction, $F(1,25) = 7.15, p = .013$, $\eta_p^2 = .22$, and Alternative event type, $F(1,25) = 195.84, p < .001$, $\eta_p^2 = .89$, but critically, we found a significant interaction of Induction x Alternative event type, $F(1,25) = 37.47, p < .001$, $\eta_p^2 = .60$. Two-tailed post hoc t-tests showed that participants generated significantly more relevant alternative event scenarios in the specificity condition compared to the control condition, $M_{difference} = 1.05, SE = .20, t(25) = 5.20, p < .001$, 95% CI = [.63, 1.46], $d = 1.02$, whereas the induction had no effect on the number of irrelevant alternative event scenarios that were generated, $M_{difference} = -.13, SE = .19, t(25) = -.68, p > .250$, 95% CI = [-.52, .26], $d = 0.13$ (Figure 2.1). Thus, the specificity induction selectively boosted the number of relevant alternative event scenarios that participants generated (see Appendix Table 2.1 for mean values). For effects of the specificity induction on details contained within the generated alternative events (i.e., scored using the Autobiographical Interview; Levine et al., 2002), see Appendix Figure 2.1A.
Figure 2.1. Experiment 1 mean induction effects on the number of relevant and irrelevant alternative event scenarios generated in the control and specificity conditions, where the y-axis represents the mean number of alternative events per trial. Error bars represent one SE of the mean.

**Perceived Difficulty and Realistic Quality of Alternative Events.** Participants rated the process of generating alternative event scenarios in the specificity condition as less difficult than in the control condition, $M_{\text{difference}} = -0.59$, $SE = .21$, $t(25) = -2.77$, $p = .011$, $95\% \text{ CI} = [-1.03, -0.15]$, $d = 0.54$. Furthermore, there was no significant difference in the average ratings of how realistic participants perceived the alternative events to be in the control (i.e., fewer alternative events) and specificity (i.e., more alternative events) conditions, $M_{\text{difference}} = -0.18$, $SE = .16$, $t(25) = -1.14$, $p > .250$, $95\% \text{ CI} = [-0.51, 0.15]$, $d = 0.22$ (see Appendix Table 2.1 for mean values).

**Induction Effects on Valence and Plausibility Ratings.** Because we found that the specificity induction boosted the number of alternative events that participants generated, next we related this boost to changes in perceptions of the original negative events by comparing mean ratings of event valence and plausibility immediately before and after generating alternative events in both the control and specificity conditions. We first report analyses that examine the contrast between rating changes in the control and specificity conditions through a series of 2 (Induction: control vs. specificity) x 2 (Time of Rating: before vs. after generating alternatives) repeated-measures ANOVAs. We then report linear multilevel models that examine the relationship between the variables of interest on a trial-by-trial basis.

For ratings of perceived original event valence, there was no main effect of Induction, $F(1,25) = 1.52$, $p = .229$, $\eta^2_p = .06$, and a significant main effect of Time of rating, $F(1,25) = 53.70$, $p < .001$, $\eta^2_p = .68$. Most importantly, we found a significant interaction of Induction x Time of rating, $F(1,25) = 7.76$, $p = .010$, $\eta^2_p = .24$. There was a significant decrease in the perceived negativity of the imagined events from before to after alternative event generation in both the
control and specificity conditions (see Appendix Table 2.1 for mean values), but critically, there was a significantly larger decrease in ratings of negative valence in the specificity condition than in the control condition, \( M_{\text{difference}} = -0.45, SE = .16, t(25) = -2.79, p = .010, 95\% \text{ CI} = [-.79, -.12], d = 0.55 \) (Figure 2.2A). Furthermore, across all participants there was a trending negative relationship between the average boost in the number of alternative event scenarios that participants generated (i.e., the difference in the number of alternatives generated in the specificity condition vs. the control condition) and the average decrease in perceived negativity of the original events (i.e., the difference in the valence rating before vs. after generating alternative outcomes) between the specificity and control induction conditions, \( r_s(24) = -.36, p = .064 \) (Figure 2.2B).

Next, we ran a linear multilevel model to further examine the relationship between the number of alternatives generated and the perceived change in negativity of the original events by induction condition (control vs. specificity), with events as a level one predictor and participants as a level two predictor. This analysis allowed us to examine whether the number of alternative events generated predicted the change in negativity of the original event on a trial-by-trial basis. Both the induction condition and the number of alternative events were treated as fixed-effect predictor variables, and the interaction between the number of alternative events and participants was treated as a random effect. The outcome variable of interest was the change in perceived negativity of the original event. Induction condition was not a significant predictor, but the number of alternative events that participants generated significantly predicted the change in negativity of the original event, \( B = -0.19, t(197.43) = -3.57, p < .001 \). That is, the more alternatives that participants generated per event trial, the greater the observed decrease in perceived negativity towards the original event.
For ratings of perceived plausibility of the original negative events, there was no main effect of Induction, $F(1,25) = .03, p > .250, \eta_p^2 = .001$, and a significant main effect of Time of rating, $F(1,25) = 35.97, p < .001, \eta_p^2 = .59$. Critically, we found a significant interaction of Induction x Time of rating, $F(1,25) = 4.60, p = .042, \eta_p^2 = .16$. We found a significant decrease in the perceived plausibility of the imagined events from before to after alternative event generation in both the control and specificity conditions (see Appendix Table 2.1 for mean values), but there was a larger decrease in ratings of perceived event plausibility in the specificity condition than in the control condition, $M_{\text{difference}} = -0.53, SE = .25, t(25) = -2.15, p = .042, 95\% \text{ CI} = [-1.04, -0.02], d = 0.42$ (Figure 2.2C). We also observed a significant negative relationship between the average boost in the number of alternative event scenarios and the average decrease in perceived plausibility of the original events between the specificity and control induction conditions across all participants, $r_s(24) = -.420, p = .033$ (Figure 2.2D).

Once again, we ran a linear multilevel model to examine the relationship between the number of alternatives generated and the perceived change in plausibility of the original events by induction condition, with events as a level one predictor and participants as a level two predictor. Fixed-effect predictors included induction condition and the number of alternative events, the interaction between the number of alternative events and participants was treated as a random effect, and the outcome variable was the change in perceived plausibility of the original event. We found that the number of alternative events that participants generated significantly predicted the change in plausibility of the original event, $B = -0.30, t(133.39) = -5.05, p < .001$, such that the more alternatives that participants generated per event trial, the greater the reported decrease in perceived plausibility towards the original event.
Overall, these results suggest that the boost in the number of generated alternative event scenarios via the specificity induction is related to larger reductions in the perceived negativity and perceived plausibility of the original negative future events. On a trial-by-trial level, the number of alternative events was also negatively related to subsequent changes in perceived negativity and plausibility of the original events.

*Figure 2.2. Experiment 1 effects of generating alternative event scenarios on original event valence and plausibility: (A) mean induction effects on changes in valence ratings before and after alternative event generation, where the y-axis represents the mean rating per trial and error bars represent one SE of the mean, (B) the relationship between the average boost in alternative event generation and the average decrease in original event negativity between the control and specificity conditions across all participants, (C) mean induction effects on changes in plausibility ratings before and after alternative event generation, where the y-axis represents the mean rating per trial and error bars represent one SE of the mean, and (D) the relationship between the average boost in alternative event generation and the average decrease in original event plausibility between the control and specificity conditions across all participants.*
Experiment 1 Discussion

Overall, the results of Experiment 1 support the hypothesis that inducing a more specific and detailed retrieval orientation via an episodic specificity induction boosts the number of alternative event scenarios that participants generated. Furthermore, participants rated the process of generating alternative events as less difficult in the specificity condition than in the control condition, and the alternative events were rated as similarly plausible between conditions.

Importantly, we observed larger decreases in the perceived negativity and plausibility of the original events in the specificity condition, where participants generated more alternative outcomes, relative to the control condition. In contrast to the experiments by Bentz et al. (2004, 2009), by using the specificity induction we were able to further decrease the perceived negativity and plausibility of the original negative events without observing a reported increase in task difficulty. In accordance with our hypotheses, these results suggest that thinking about positive alternative outcomes to negative events may alter one’s initial perception of those events. In Experiment 2, we aimed to replicate and extend our findings from Experiment 1 to a series of personalized (rather than standardized) negative future experiences.

Experiment 2

In Experiment 2, we aimed to examine the effect of an episodic specificity induction on the generation of alternative outcomes for a series of personalized negative events. Rather than using a set of standardized event cues (as in Experiment 1), participants were directly asked to list a series of anticipated negative events that may occur in their own personal future. Overall, the methods used in Experiment 2 are very similar to those of Experiment 1, with differences highlighted below.
Method

Participants. A total of 36 healthy undergraduates were recruited from Harvard University and Boston University (ages 18 to 25, $M = 21.42$ years, 19 female). A total of 7 participants were excluded due to noncompliance (1 participant) or incompletion of the experiment (6 participants), leaving 29 participants in the final analysis. To keep the sample size in Experiment 2 comparable to that of Experiment 1, we stopped data collection after reaching a similar number of useable participants.

Experimental Procedure. On average, session 2 took place 5.86 days after session 1. As in Experiment 1, both experimental sessions lasted 2 hours and were very similar in structure, consisting of several different phases: event simulation (i.e., imagining 6 negative events for 2 minutes each), induction phase (specificity or control induction), alternative event generation (i.e., generating alternative event outcomes for 5 minutes), and finally an alternative event rating phase (i.e., rating each alternative event for valence, plausibility, and novelty). The only procedural change from Experiment 1 took place during the event simulation phase, which we describe below.

Event Generation and Simulation Phase. In each session, participants first provided 6 negative events that were specific, concrete, highly familiar, and had tangible outcomes that could plausibly occur within the next several years. Across both experimental sessions, participants generated a total of 12 negative events that concerned topics relating to academics, health, career, relationships, and finances. For each separate event, they were asked to provide a brief title and then imagine a concrete future scenario in which they were experiencing the event in as much detail as possible for 2 minutes. Afterwards, participants rated each original event for valence, plausibility, and similarity to previous experience on a 1 to 9 scale.
**Coding.** Two raters (raters 1 and 3 from Experiment 1) were trained to score responses from the 5-minute alternative event generation task as either relevant or irrelevant alternative event scenarios. Both raters were blind to the induction condition of participant responses. High inter-rater reliability was obtained for both types of alternatives (standardized Cronbach’s $\alpha = .99$ for relevant alternatives and .90 for irrelevant alternatives). The two raters scored 66% and 34% of participant responses, respectively.

**Results**

As in Experiment 1, we conducted repeated-measures ANOVAs to test the hypotheses, which involved within-subjects factors of Induction condition (control vs. specificity), Alternative event type (relevant vs. irrelevant), and Time of Rating (initial ratings prior to generating alternative events vs. ratings after generating alternative events). The counterbalanced order of induction did not have a significant effect on the analyses reported below. We also report a series of linear multilevel models that examine the relationship between the variables of interest on an event trial-by-trial basis.

**Induction Effects on Alternative Event Generation.** Participants spent slightly longer discussing the video in the specificity induction ($M = 4.73$ min, $SD = 1.24$) than in the control induction ($M = 3.95$ min, $SD = .68$), $M_{\text{difference}} = .78$, $SE = .20$, $t(28) = 3.90$, $p = .001$, 95% CI = [.37, 1.19], $d = 0.72$. However, including the difference score for induction duration as a covariate in the following repeated-measures ANOVAs did not significantly affect any results.

We first conducted a 2 (Induction: control vs. specificity) x 2 (Alternative Event Type: relevant vs. irrelevant) repeated-measures ANOVA, and found significant main effects of Induction, $F(1,28) = 5.13$, $p = .031$, $\eta_p^2 = .16$, and Alternative event type, $F(1,28) = 458.65$, $p < .001$, $\eta_p^2 = .94$. As in Experiment 1, there was a significant interaction of Induction x Alternative
event type, $F(1,28) = 22.66, p < .001, \eta^2_p = .45$. Participants generated significantly more relevant alternative event scenarios in the specificity condition compared to the control condition, $M_{\text{difference}} = 1.29, SE = .25, t(28) = 5.14, p < .001, 95\% \text{ CI} = [.78, 1.80], d = 0.95$. There was also a trend towards generating fewer irrelevant alternative event scenarios in the specificity condition than in the control condition, $M_{\text{difference}} = -.52, SE = .26, t(28) = -2.00, p = .056, 95\% \text{ CI} = [-1.05, .01], d = 0.37$, although this difference did not reach full significance (Figure 2.3). Thus, the specificity induction significantly boosted the number of relevant alternative event scenarios that participants generated (see Appendix Table 2.2 for mean values).

![Number of Alternatives by Induction Condition](image)

*Figure 2.3.* Experiment 2 mean induction effects on the number of relevant and irrelevant alternative event scenarios generated in the control and specificity conditions, where the y-axis represents the mean number of alternative events per trial. Error bars represent one $SE$ of the mean.

**Perceived Difficulty and Realistic Quality of Alternative Events.** As in Experiment 1, participants rated the process of generating alternative event scenarios in the specificity condition as less difficult than in the control condition, $M_{\text{difference}} = -.90, SE = .29, t(28) = -3.11, p = .004, 95\% \text{ CI} = [-1.49, -.31], d = 0.58$. Furthermore, there was no significant difference in the average ratings of how realistic participants perceived the alternative events to be in the control and
specificity conditions, $M_{difference} = -0.15, SE = .13, t(28) = -1.17, p > .250, 95\% CI = [-.41, .11], d = 0.22$ (see Appendix Table 2.2 for mean values).

**Induction Effects on Valence and Plausibility Ratings.** Next, we contrasted the mean ratings of event valence and perceived plausibility between the control and specificity conditions through a series of 2 (Induction: control vs. specificity) x 2 (Time of Rating: before vs. after generating alternatives) repeated-measures ANOVAs. For ratings of perceived original event valence, results revealed no main effect of Induction, $F(1,28) = 2.39, p = .133, \eta^2_p = .08$, and a significant main effect of Time of rating, $F(1,28) = 34.97, p < .001, \eta^2_p = .56$. Most importantly, we found a significant interaction of Induction x Time of rating, $F(1,28) = 7.20, p = .012, \eta^2_p = .21$. There was a significant decrease in the perceived negativity of the imagined events from before to after alternative event generation in both the control and specificity conditions (see Appendix Table 2.2 for mean values), but critically, there was a significantly larger decrease in ratings of negative valence in the specificity condition than in the control condition, $M_{difference} = -0.46, SE = .17, t(28) = -2.68, p = .012, 95\% CI = [-.80, -.11], d = .50$ (Figure 2.4A). We also observed a significant negative relationship between the boost in the number of alternative event scenarios (i.e., the difference in the number of alternatives generated in the specificity vs. control condition) and the decrease in perceived negativity of the original events (i.e., the difference in the valence rating before vs. after generating alternative outcomes) between the specificity and control induction conditions, $r_S(27) = -.381, p = .041$ (Figure 2.4B).

As in Experiment 1, we ran a linear multilevel model to further examine the relationship between the number of alternatives generated and the perceived change in negativity of the original events by induction condition (control vs. specificity), with events as a level one predictor and participants as a level two predictor. This analysis allowed us to examine whether the number of
alternative events generated predicted the change in negativity of the original event on a trial-by-trial basis. Induction condition and the number of alternative events were treated as fixed-effect predictor variables, and the interaction between the number of alternative events and participants was treated as a random effect. The outcome variable of interest was the change in perceived negativity of the original event. Induction condition was not a significant predictor, but the number of alternative events that participants generated significantly predicted the change in negativity of the original event, \( B = -0.21, t(284.19) = -3.95, p < .001 \). That is, the more alternatives that participants generated per event trial, the greater the observed decrease in perceived negativity towards the original event.

For ratings of perceived plausibility of the original negative events, we found no main effect of Induction, \( F(1,28) = 1.22, p > .250, \eta_p^2 = .04 \), and a significant main effect of Time of rating, \( F(1,28) = 60.16, p < .001, \eta_p^2 = .68 \). We observed a significant interaction of Induction x Time of rating, \( F(1,28) = 9.72, p = .004, \eta_p^2 = .26 \). There was a significant decrease in the perceived plausibility of the imagined events from before to after alternative event generation in both the control and specificity conditions (see Appendix Table 2.2 for mean values), but there was a larger decrease in ratings of perceived event plausibility in the specificity condition than in the control condition, \( M_{\text{difference}} = -0.44, SE = .14, t(28) = -3.12, p = .004, 95\% \text{ CI} = [-.73, -.15], d = 0.58 \) (Figure 2.4C). There was also a weak negative relationship between the boost in the number of alternative event scenarios and the decrease in perceived plausibility of the original events between the specificity and control induction conditions, \( r_s(27) = -.318, p = .093 \) (Figure 2.4D).

We also ran a linear multilevel model to examine the relationship between the number of alternatives generated and the perceived change in plausibility of the original events on a trial-by-trial basis. Induction condition and the number of alternative events were treated as fixed-effect
predictor variables, the interaction between the number of alternative events and participants was treated as a random effect, and the outcome variable of interest was the change in perceived plausibility of the original event. Induction condition was not a significant predictor, but the number of alternative events that participants generated significantly predicted the change in plausibility of the original events, $B = -0.12, t(254.44) = -2.32, p = .021$. Thus, the more alternatives that participants generated per event trial, the greater the observed decrease in perceived plausibility towards the original event.

As in Experiment 1, these results suggest that the boost in the number of generated alternative event scenarios via the specificity induction is related to larger reductions in the perceived negativity and plausibility of the original events.

Figure 2.4. Experiment 2 effects of generating alternative event scenarios on original event valence and plausibility: (A) mean induction effects on changes in valence ratings before and after alternative event generation, where the y-axis represents the mean rating per trial and error bars
represent one $SE$ of the mean, (B) the relationship between the average boost in alternative event generation and the average decrease in original event negativity between the control and specificity conditions across all participants, (C) mean induction effects on changes in plausibility ratings before and after alternative event generation, where the y-axis represents the mean rating per trial and error bars represent one $SE$ of the mean, and (D) the relationship between the average boost in alternative event generation and the average decrease in original event plausibility between the control and specificity conditions across all participants.

**Experiment 2 Discussion**

The results of Experiment 2 replicate the results of Experiment 1 even when using personalized rather than standardized negative future scenarios. These data once again support the idea that using an episodic specificity induction boosted the perceived ease of simulation and increased the number of alternative event scenarios that participants generated, while leading to larger decreases in the perceived negativity and plausibility of the original events in the specificity condition relative to the control condition. On a trial-by-trial level, the number of alternative events was also negatively related to subsequent changes in perceived negativity and plausibility of the original events. Thus, thinking about alternative positive outcomes to negative events can have beneficial effects on one’s perception of those events.

**General Discussion**

Overall, the data from two experiments support the hypothesis that increasing the number of different ways in which individuals consider how future events may unfold may subsequently alter one’s subjective perception of those events. First, using an episodic specificity induction increased the number of alternative events that participants generated. While the specificity induction has previously been shown to enhance performance on a variety of tasks thought to be dependent upon episodic memory (for review, see Schacter & Madore, 2016), we demonstrate for the first time that the induction also affects the process of generating alternative outcomes to a series of anticipated negative future events.
Second, we demonstrated that increasing the number of positive alternative event outcomes was related to larger decreases in the perceived plausibility of the original events. As previously shown in the experiments by Bentz et al. (2009), generating more positive alternative outcomes (long intervention) had no additional effect in decreasing probability judgments towards a series of negative events, compared with generating fewer alternative outcomes (short intervention), in part due to the more difficult nature of the long intervention which yielded less realistic alternative outcomes. Importantly, here we find that the specificity induction is a useful tool that boosts the perceived ease of simulating these alternative scenarios without decreasing the realistic quality of the imagined events. We were subsequently able to link this boost to larger decreases in the perceived plausibility of the original events when participants generated more alternatives in the specificity condition relative to the control condition. As formerly proposed by a number of researchers (Bentz et al., 2004, 2009; Hirt & Markman, 1995; Hirt et al., 2004), this reduction in pessimistic predictions may be explained via the availability heuristic (Tversky & Kahneman, 1973), whereby certain negative events are more readily available at the forefront of one’s mind, thus increasing likelihood judgments for pessimistic outcomes. Considering more positive outcomes increases the accessibility of and the ease with which one can think about these positive outcomes, relative to the original negative scenarios. In turn, this increased accessibility may reduce the likelihood that individuals will inflate estimates of uncertainty associated with anticipated negative experiences.

Third, we demonstrated for the first time that increasing the number of positive alternative event outcomes is related to larger decreases in the perceived negativity of the original events. These findings may have important implications, given that existing research on affective forecasting has found that individuals are frequently inaccurate when predicting their emotional
reactions to negative events (Wilson & Gilbert, 2005). For example, individuals are subject to an impact bias, where they overestimate the intensity and duration of an experience, which may arise from the tendency to focus too much on the event in question and discount the impact of other events or situational factors (Wilson & Gilbert, 2005; Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000). Gilbert and Wilson (2007, 2009) discuss possible reasons for these errors, including (but not limited to) the fact that the mental “previews” that we generate for anticipated future events: (1) tend to be based on the most available and salient information that comes to mind, which may not be representative of a “typical” experience, (2) may involve a different context than the actually experienced event, (3) generally focus on “essential” features while omitting “incidental” features that are deemed as less important but may actually have a large influence on our subjective emotional experience, and (4) tend to focus on the climax of the emotional experience while disregarding our ability to subsequently adapt to and temper our reactions to such an experience. Accordingly, the process of considering alternative positive outcomes grants access to additional information about ways in which the event may unfold that taps into each of these categories: it increases the availability of positive outcomes, which are most likely comparably plausible to their negative counterparts, it brings to light potential alternative or previously ignored contextual details about the situation, and may call attention to our ability to emotionally and behaviorally adapt to a variety of situations. This suite of additional details may subsequently lead one to realize that the event might not be as predictable or as negative as initially perceived, and thus may encourage a subsequent adjustment of negative expectations.

In the current experiments, we focused on the fluency of alternative event generation and encouraged participants to generate as many alternative event outcomes as possible. However, it is important to note that the concreteness and overall specificity of the alternative scenarios may
be another important dimension to examine. It has been reported that individuals with emotional disorders, such as depression (Anderson, Boland, & Garner, 2015; Williams et al., 1996) and anxiety (Brown et al., 2014; McNally et al., 1995), tend to show reduced detail and concreteness when retrieving episodic memories and imagining future events. Furthermore, the process of worry in the context of generalized anxiety disorder is thought to involve more verbal and conceptual thought that lacks specific and concrete details, which may elicit less unpleasant physical arousal than visual imagery and episodic simulations (Borkovec et al., 1998; McGowan et al., 2017; Stöber & Borkovec, 2002). It is hypothesized that the more abstract nature of worry allows individuals to disengage and avoid arousing emotional processing towards the anticipated threat, but doing so can have adverse long-term consequences, such as reducing the likelihood that these individuals will imagine their future in a sufficiently concrete fashion to actually cope with the problem (Borkovec et al., 1998; Williams, 2006; Williams et al., 1996). Indeed, recent evidence has shown that increasing the specificity of autobiographical memory and concreteness of mental imagery can be linked to improvements in depressive symptoms (Lang et al., 2012; Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD symptoms (Moradi et al., 2014) with respect to negative and distressing past events, and can also be beneficial for processing worrisome future events that have not yet been experienced (Jing et al., 2016; Skodzik, Leopold, & Ehring, 2017; for review, see Hitchcock et al., 2017). Thus, imagining a smaller number of alternative outcomes in more specific, concrete detail may be just as beneficial in reducing pessimistic future predictions as accessing more alternative outcomes overall, and more research should be conducted to shed light on this point.

It is also important to note that in these experiments we encouraged participants to generate a variety of alternative future scenarios, including changing the actual outcome, the perceived
consequences, and other emotional reinterpretations of the original event. However, it is possible that various types of changes may differentially impact one’s perceptions of the event. For example, changing the primary event outcome and the perceived consequences may have a larger influence on the perceived event plausibility and valence than mentally reframing emotional aspects of the original event, or vice versa. Furthermore, certain types of changes may be perceived as more controllable, hence increasing the likelihood of linking mere thought to the actual implementation of action (Epstude et al., 2016). Thus, separately manipulating different types of event changes may be another interesting avenue of future research.

In summary, in these experiments we have advanced our understanding of alternative future event generation by extending the impact of an episodic specificity induction to the simulation of alternative future event outcomes, and also demonstrate that boosting the number of alternative events is related to subsequent changes in the perceived plausibility and negativity of the original events. Further research is needed to characterize the mechanism behind how factors such as the types of alternatives that individuals generate may differentially impact the perceived plausibility and valence of anticipated emotional events, and how imagining alternative outcomes might impact decision-making and the implementation of action. Overall, this line of work may have important implications for understanding how cognitive processes underlying episodic simulation impact the regulation of future-oriented emotion in both healthy and clinical populations.
Paper 2 Acknowledgements

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Paper 3:
Abstract

Interventions that increase the specificity of episodic memory and future-oriented problem solving have been shown to help both young adults and clinical populations regulate their emotions towards potential stressors. However, little is known about how episodic specificity impacts anxiety levels in older adults, who show reduced specificity of episodic memory, future simulation, and problem-solving performance. Although emotion regulation generally improves with age, older adults still experience worries pertaining to their health and interpersonal relationships. The current studies test how episodic specificity affects emotion regulation in older adults. In Experiment 1, participants received an episodic specificity induction (ESI)—brief training in recollecting details of past experiences—prior to generating steps to solve worrisome problems. Older adults provided more relevant steps and episodic details after the specificity induction relative to a control induction, but we found no difference in emotion regulation ratings between induction conditions. In Experiment 2, we contrasted performance on a personal problem-solving task (i.e., generating steps to solve one’s own problems) intended to draw on episodic retrieval with an advice task focused on semantic processing (i.e., listing general advice for an acquaintance worried about similar problems). Participants provided more relevant steps and episodic details in the personal problem-solving task relative to the advice task, and boosts in detail were related to larger reductions in anxiety towards the target worrisome events. These results indicate that solving worrisome problems with greater levels of episodic detail can positively influence emotion regulation in older adults.
Existing research in young adults has shown that constructing a detailed, positive mental simulation of a worrisome future event (e.g., simulating studying behaviors prior to a difficult exam) can improve emotion regulation towards the anticipated event by reducing worry, increasing the subjective probability of a good outcome, decreasing the perceived probability of a bad outcome, boosting positive and minimizing negative affect, and increasing engagement in active coping strategies (Brown et al., 2002; Jing et al., 2016, 2017; Pham & Taylor, 1999; Taylor et al., 1998). Further, the detail of these simulations may have a direct influence on emotional well-being. Two major types of details are relevant: “internal” details involving episodic information about specific people, objects, and actions that constitute an event, and “external” details involving semantic, factual information that is not specific to time and place, commentary, or references to other events (Levine et al., 2002). Patients with emotional disorders, such as depression (Williams et al., 1996, 2007) and anxiety disorders (Brown et al., 2014; McNally et al., 1994, 1995), show reduced specificity (i.e., fewer internal details) and concreteness when asked to retrieve a memory or simulate a future event, which helps to avoid unpleasant, arousing emotional processing (Borkovec et al., 1998; Williams, 2006). However, reduced specificity can result in adverse long-term consequences by magnifying symptoms such as hopelessness and avoidance that hinder the process of imagining the future in a sufficiently concrete fashion to generate specific plans and goals (Borkovec et al., 1998; Williams, 2006; Williams et al., 1996, 2007). Indeed, in addition to reduced specificity, patients with emotional disorders also show poorer problem-solving performance relative to healthy controls (Dickson & MacLeod, 2004; Goddard et al., 1996; Raes et al., 2005; Sutherland & Bryant, 2008).
Recently, manipulations designed to increase the specificity and detail of episodic retrieval have been shown to have beneficial effects on the specificity of future thinking, problem solving, and subsequent emotion regulation. Madore et al. (2014) developed an episodic specificity induction (ESI), a brief training in recollecting details of a recent experience that biases participants to focus on specific event details during subsequent tasks that are dependent on episodic retrieval (e.g., memory, imagination, problem solving, and divergent thinking tasks), but has no impact on the performance of subsequent tasks that are thought to rely on primarily semantic retrieval or non-episodic narrative processing (e.g., picture description, generating word definitions and object associations; Madore et al., 2014, 2015, 2016; Madore & Schacter, 2016; for review, see Schacter & Madore, 2016). Madore and Schacter (2014) showed that the ESI boosts the number of relevant steps and details that both young and older adults generate in a means-end problem solving task (MEPS; Platt & Spivack, 1975) relative to a control condition, confirming that episodic retrieval contributes to successful problem solving (see also Sheldon et al., 2011, 2015; Vandermorris et al., 2013). Jing et al. (2016) replicated and extended these results in a group of young adults by showing that the ESI improved performance on a MEPS task where participants generated steps to solve problems concerning highly worrisome personal future experiences, and further, that this boost in steps and details was linked to improved affect and reduced anxiety towards the target worrisome events. Additionally, recent work has demonstrated that increasing the specificity of autobiographical memory using a Memory Specificity Training protocol (Raes et al., 2009) can be linked to improvements in depressive symptoms (Lang et al., 2012; Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD symptoms (Moradi et al., 2014) with respect to negative and distressing past events (for review, see Hitchcock et al., 2017). Thus, increasing the specificity with which people constructively imagine future experiences serves as a useful
intervention to foster productive problem-solving behaviors that can enhance emotion regulation in young adults and in clinical populations.

Despite well-documented effects in young adults, there currently exists very little experimental evidence examining the role of episodic specificity and its influence on emotion regulation in older adults. Older adults are a population of interest because they retrieve fewer episodic event details both when recalling past memories and imagining future events, relative to young adults (Addis et al., 2008, 2010; Cole et al., 2013; Gaesser, Sacchetti, Addis, & Schacter, 2011; Rendell et al., 2012; Romero & Moscovitch, 2012; for reviews, see Schacter et al., 2013, 2018). Furthermore, older adults show deficits in problem-solving performance, relative to young adults (Sheldon et al., 2011, 2015; VanderMorris et al., 2013). As previously mentioned, reduced specificity can hinder the generation of concrete plans and goals to solve a problem and may lead to increased avoidance and rumination, which negatively affects mental health. However, in spite of deficits observed in episodic memory and future thinking in older adults, emotion regulation generally seems to improve with age (for recent reviews, see Scheibe & Carstensen, 2010; Urry & Gross, 2010). Factors that might contribute to this upward emotional trend in older adults include the positivity effect (i.e., bias towards positive relative to negative information; Carstensen & Mikels, 2005; Mather & Carstensen, 2005; Reed, Chan, & Mikels, 2014), as well as observed differences in the experience, expression, and control of emotions, relative to young adults (Gross et al., 1997). Compared to young and middle-aged adults, older adults show preferential attention towards positive (vs. negative) stimuli (Isaacowitz, Toner, Goren, & Wilson, 2008; Phillips, Henry, Hosie, & Milne, 2008), show improvements in the selection of positive situations that minimize negative emotions (e.g., Carstensen, Fung, & Charles, 2003; Urry & Gross, 2010), and also are better at employing certain emotion regulation strategies such as positive reappraisal.
(Shiota & Levenson, 2009). Older adults are also reportedly more effective than younger counterparts at handling emotional situations, which likely results from accumulated knowledge and experience in dealing with such situations (Blanchard-Fields, 2007), amongst other theories.

Despite improvements in emotion regulation overall, emotional disorders such as anxiety and depression are still prevalent in older adults (Djernes, 2006; Reynolds et al., 2015; see also Miloyan, Pachana, & Suddendorf, 2016). Because the future is associated with a higher likelihood of negative outcomes such as disease and death, older adults tend to experience a niche of health-related and interpersonal worries (Diefenbach et al., 2001; Hunt et al., 2003; Powers et al., 1992). The Strength and Vulnerability Integration model (SAVI; Charles, 2010) proposes that while older adults effectively regulate lower levels of distress by utilizing the strengths derived with age (e.g., attentional strategies, appraisal, etc.), age-related advantages in emotion regulation tend to reverse in unavoidable negative situations, as in cases where they experience loss of a loved one, loss of social belonging, or encounter functional limitations in their daily lives. Further, when presented with negative events, older adults sometimes resort to more passive forms of emotion regulation (e.g., not directly dealing with issue, avoidance) when compared with middle-aged adults (Blanchard-Fields, Stein, & Watson, 2004), and have been reported to be less flexible in changing emotion regulation strategies across various situations (Eldesouky & English, 2018), which may limit their ability to adapt to and respond in different emotional contexts. These findings, in concert with the observation that older adults show reduced specificity of memory and imagination, suggest that examining older adults’ emotional responses towards worrisome future events that are relevant to their daily lives is an important research question. Thus, the current studies aim to elucidate whether and how encouraging older adults to simulate the future in an episodically
detailed manner—specifically during future-oriented problem solving—can improve emotion regulation towards worrisome happenings in their daily lives.

Recently, Jumentier, Barsics, and Van der Linden (2017) explored the impact of episodic specificity on emotion regulation by examining the relationship between different emotion regulation strategies and their influence on the subjective experience (including levels of episodic specificity) of past and future events in middle-aged and older adults. They asked participants to engage in a Specificity Thinking Task (modified from the Autobiographical Memory Test; Williams & Broadbent, 1986), in which they imagined plausible future events in response to word cues varying in temporal distance (near vs. distant future) and emotional valence (positive vs. negative). Participants also completed questionnaires that assessed their general tendencies to engage in experiential and cognitive avoidance (Acceptance and Action Questionnaire; Bond et al, 2011; Cognitive Avoidance Questionnaire; Gosselin et al., 2002) and maladaptive or adaptive emotion regulation strategies (Cognitive Evaluation Regulation Questionnaire; Garnefski, Kraaij, & Spinhoven, 2001). Jumentier et al. (2017) reported that relative to middle-aged adults, older adults provided less specific responses for both negative and distant-future word cues. Older adults who more frequently engaged in positive refocusing (i.e., using pleasant thoughts to replace negative ones, as assessed by the Cognitive Evaluation Regulation Questionnaire) reported higher ratings for the feeling of “pre-experiencing” the future, and marginally reported greater perceived vividness and visual detail of the imagined events. In contrast, in middle-aged adults, the level of experiential avoidance (as assessed by the Acceptance and Action Questionnaire) was negatively related to the specificity of their responses. Further, older adults who engaged in more cognitive avoidance (as assessed by the Cognitive Avoidance Questionnaire) showed higher rates of omission-type responses during the specificity thinking task, such that they were unable to
generate any event at all in response to a word cue. Thus, these results suggest a relationship among cognitive and behavioral avoidance, reduced specificity of event responses, and reduced vividness and quality of imagined future events in middle-aged and older adults.

Related work by Leahy, Ridout, Mushtaq, and Holland (2018a) has also examined the impact of autobiographical memory specificity on problem solving and mood in older adults (see also Leahy, Ridout, & Holland, 2018b). They measured autobiographical memory specificity by prompting older adults to recall specific memories in response to cue words on a standard Autobiographical Memory Test (Williams & Broadbent, 1986). The researchers also measured problem-solving ability as assessed by performance on the MEPS task (Platt & Spivack, 1975) and obtained measures of mood using the Hospital Anxiety and Depression Scale (HADS; Zigmond & Snaith, 1983). Participants then were assigned to one of three conditions: (1) Memory Specificity Training (MEST; Raes et al., 2009), (2) Life Review (Serrano et al., 2004), and (3) a control group where participants completed a workbook of cognitive stimulating activities such as crossword puzzles that were not tied to autobiographical memory. In the MEST intervention, participants were trained to recall specific event memories in response to emotional and neutral word cues over 4 weeks; this program has previously been shown to successfully boost memory specificity and improve symptomology in clinically depressed participants (Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD patients (Moradi et al., 2014). In contrast, Life Review (Serrano et al., 2004) targets and trains the recall of specific positive autobiographical memories, with the aim of constructing a positive life narrative to improve life satisfaction and reduce depressive symptoms. Leahy et al. (2018a) found significant improvements in both memory specificity and effectiveness of the steps generated in the MEPS task in both the MEST and Life Review intervention groups, relative to the control group, although these effects did not persist during a 3-
month follow-up and there were no observed changes in mood (i.e., anxiety and depressive symptoms as assessed by the HADS questionnaire).

Together, the two studies conducted by Jumentier et al. (2017) and Leahy et al. (2018a) successfully manipulated levels of specificity in autobiographical memory and future thinking in older adults, but neither group found reliable links between memory specificity and changes in mood. In both studies, specificity training was implemented on sets of neutral and emotional word cues, and the researchers used questionnaires for global assessments of mood and coping strategies. Given that older adults tend to experience a specific niche of concerns towards the future despite having overall higher levels of emotion regulation relative to young adults, and given that the perceived relevance of problems can affect problem-solving performance in older adults (Artistico, Cervone, & Pezzuti, 2003; Artistico, Orom, Cervone, Krauss, & Houston, 2010), we sought to use a more targeted approach to examine the link between episodic specificity and emotion regulation by implementing trial-level assessments of worry and verified age-relevant problems.

In two experiments, we manipulated levels of episodic specificity during a problem-solving task and examined its subsequent effects on emotion regulation towards future events that older adults find to be worrisome and relevant to their daily lives. We limited our sample to older adults because substantial differences in the types of worries experienced by young and older adults served as an obstacle to comparing and interpreting task performance between the two age groups. Moreover, our goal was not to directly explore variations in the types of worries between the two populations but rather to examine the effect of episodic specificity on worry within an older adult population (however, we note that previous studies have indeed documented differences in the kinds of worries expressed by young and older adults; e.g. Artistico et al., 2010; Powers et al.,
In Experiment 1, we used the ESI to boost problem-solving performance on a MEPS task in older adults (cf. Jing et al., 2016; Madore & Schacter, 2014) and assessed emotion regulation towards each individual worrisome event. In Experiment 2, we contrasted levels of episodic specificity and subjective well-being during simulation on a personal MEPS task versus semantic reflection on a novel advice task towards worrisome future experiences. We hypothesized that in both experiments, increasing levels of episodic detail during problem solving should be positively related to improvements in emotion regulation.

**Experiment 1**

**Method**

**Participants.** We performed a power analysis based on previous related work using the ESI on older adults (Madore et al., 2014) to determine that a sample size of at least 24 useable participants was necessary to observe a medium-sized effect of the ESI on episodic details (power > .80, $\alpha = .05$, two-tailed, for a within-subjects design, $d = 0.60$). We based our power analysis on the effect of the ESI on episodic details because there is no existing work that has successfully manipulated episodic specificity in a manner that also impacted subsequent emotion regulation in older adults. Given scheduling constraints with multiple sessions, data collection was stopped once it was determined that approximately enough useable participants had been run to reach this number. A total of 32 older adult participants (ages 65 to 85, $M = 74.03$ years, 20 female) were recruited from postings around the Greater Boston area and were paid for their participation. All participants had normal vision and no history of neurological or psychological impairment. They were screened with a neuropsychological battery prior to participating in the study and were considered cognitively healthy, with a mean Mini-Mental Status Examination (Folstein, Folstein,
& McHugh, 1975) score of 29.15 ($SD = .95$, range $= 27 – 30$). Participants were also screened for psychiatric illness and were excluded if they indicated a clinical diagnosis of depression or anxiety, or if they had taken antidepressants or anxiolytics in the past 5 years. Informed written consent was obtained from all participants prior to beginning the study, which was approved by the Harvard University Institutional Review Board. A total of 6 participants were excluded due to attrition (4 participants) or noncompliance (2 participants), leaving 26 participants in the final sample.

Experimental Procedure. The experiment lasted approximately 4.5 hours across two separate sessions. The durations of the first and second experimental sessions were approximately 2.5 and 2 hours respectively, each spaced 3 to 7 days apart ($M = 5.41$ days). We used a within-subjects design, and the two sessions were very similar in structure: participants first completed an induction phase (specificity vs. control induction), and then completed a problem-solving task involving a series of standardized worrisome future experiences (see Figure 3.1 for a diagram of the experimental procedure). Both sessions were executed using Qualtrics on an Apple desktop computer, and participants wrote down their responses in a paper packet provided by the experimenter.

![Figure 3.1. Schema of experimental design for Experiment 1.](image)

In the initial induction phase, participants watched a short video of two adults performing routine activities in a kitchen and then completed a math filler task for 2 minutes; a different video
was shown in each experimental session, and the order of the videos was counterbalanced across participants. Afterwards, they either received questions about the video in the form of the ESI or a control induction. In the ESI, participants were probed to recall mental imagery and specific details about the people, setting, and actions in the video, with follow-up questions that asked them to elaborate more on the details they had mentioned. In the other session, they received an impressions control induction, where they were asked questions targeting general impressions, opinions, and thoughts about the video, which allowed them to talk more generally about the video without requiring them to retrieve specific episodic details. Thus, differences in performance on the subsequent tasks should be attributable to changes in levels of episodic detail between the two induction conditions (i.e., more episodic detail with the ESI versus baseline detail with the control induction). The order of inductions was counterbalanced between subjects (see Appendix for full induction scripts).

After the induction phase, participants completed the MEPS task (adapted from Jing et al., 2016; Platt & Spivack, 1975). In each experimental session, participants viewed 7 standardized worrisome events that concerned their health, interpersonal relationships, and finances (1 event was used as a practice trial). These events were scenarios that 20 separate older adults had most frequently generated during a pilot study, where they were given 30 minutes to write down as many of their future worries and concerns as they could (e.g., overspending their savings for retirement, staying mentally active, growing distant with family, losing their ability to function independently; see Appendix for a full list of events). For each event, participants were first asked to simulate a scenario in the near future during which they are worrying about the event and were asked to write down their thoughts and concerns about the problem on a sheet of paper for 2 minutes. This 2-minute simulation component served to personalize and acquaint participants with
the negative emotions and worry they may feel towards the event, but was not the main focus of the experimental manipulation. Then, they rated each event on a 1 to 9 scale on four dimensions relating to emotion regulation: (1) perceived levels of anxiety, (2) likelihood of experiencing a good outcome, (3) likelihood of experiencing a bad outcome, and (4) difficulty to cope with a bad outcome. Next, participants were asked to directly solve the worrisome problem for 5 minutes. They were presented with a story that described the beginning of the problem (e.g., worrying about the problem) and an ending solution (e.g., achieving the positive outcome specified for the event), and were asked to write down steps they would execute to reach the final solution in each problem in as much detail as possible. They were encouraged to imagine themselves implementing the steps in their mind’s eye as they generated the solutions. Finally, participants concluded each event with the same four ratings of anxiety, likelihood of a good and bad outcome, and difficulty to cope with a bad outcome. At the end of the second session, participants completed the State-Trait Anxiety Inventory (STAI; Spielberger et al., 1970) to assess levels of trait anxiety.

**Coding.** Participant responses from the MEPS task were scored in two ways. Responses were first scored as a “relevant step” or “other step” (Jing et al., 2016; Madore & Schacter, 2014; Platt & Spivack, 1975). Relevant steps are steps or events that lead towards the designated solution state or goal, and other steps are those that lead towards a different solution state not specified in the prompt, or do not fit the step framework (e.g., commentary about the task, repetitive or off-topic information). Participant responses were also scored with the internal and external detail categories of the Autobiographical Interview (AI; see Gaesser et al., 2011; Levine et al., 2002). Internal details included bits of episodic information contained in the responses such as the people, places, actions, objects, thoughts, and feelings of the central event, whereas external details included bits of other information contained in the responses, such as semantic facts, task
commentary, and off-topic or repetitive information. Three raters who were blind to the induction condition of the narratives scored participant responses and had high inter-rater reliability for both steps and details (all Cronbach’s α > .93).

**Results**

We conducted a series of repeated-measures analyses of variance (ANOVAs) to test our hypotheses, which involved within-subjects factors of Induction (control vs. specificity), Detail type (internal vs. external), Step type (relevant vs. other), and Time of Rating (pre-MEPS ratings after 2-minute worry simulation vs. post-MEPS ratings for perceived levels of anxiety, likelihood of a good outcome, likelihood of a bad outcome, and difficulty to cope with a bad outcome). Both main effects and interactions were tested for each of the variables (see Appendix Table 3.1 for main effects), but below we focus on reporting the interactions to address the impact of the induction on each of the variables. All post-hoc t-tests were two-tailed and Bonferroni corrected at the \( p < .05 \) level. The counterbalanced order of induction and event list presentation did not have a significant effect on the reported analyses (\( ps > .19 \)).

**Changes in Steps and Details by Induction Condition.** To examine the impact of the ESI on the types of steps and details that participants produced, we first conducted a series of 2 (Induction condition: control vs. specificity) x 2 (Step type: relevant vs. other; Detail type: internal vs. external) repeated-measures ANOVAs. We found a significant interaction of Induction condition (control vs. specificity) x Step type (relevant vs. other), \( F(1,25) = 47.01, p < .001, \eta^2_p = .65 \) (Figure 3.2A). Two-tailed post hoc t-tests showed that participants generated significantly more relevant steps, \( t(25) = 6.51, p < .001, 95\% \text{ CI} = [1.29, 2.49], d = 1.28 \), and significantly fewer other steps, \( t(25) = -3.85, p = .001, 95\% \text{ CI} = [-1.14, -3.5], d = 0.75 \), in the specificity condition relative to the control condition (Relevant steps: \( M_{\text{difference}} = 1.89, SE = .29 \); Other steps: \( M_{\text{difference}} \)).
We also found a significant interaction of Induction condition x Detail type (internal vs. external), $F(1,25) = 33.28, p < .001, \eta^2_p = .57$ (Figure 3.2B). The specificity induction boosted the number of internal details contained in the solution steps, $t(25) = 5.94, p < .001$, 95% CI $= [3.03, 6.25]$, $d = 1.17$, and reduced the number of external details, $t(25) = -3.88, p = .001$, 95% CI $= [-5.47, -1.68]$, $d = 0.76$ (Internal details: $M_{\text{difference}} = 4.64, SE = .78$; External details: $M_{\text{difference}} = -3.58, SE = .92$). These results are consistent with prior work examining the impact of the ESI on the MEPS task in both young and older adults (Jing et al., 2016; Madore & Schacter, 2014).

Figure 3.2. Experiment 1 mean induction effects (control vs. specificity conditions) on (A) relevant and other steps, and (B) internal and external details. The y-axis represents the mean number of steps or details per trial, and error bars represent one $SE$ of the mean.

**Changes in Emotion Regulation Ratings by Induction Condition.** Next, we assessed the impact of the ESI on the 4 ratings pertaining to emotion regulation by conducting a series of 2 (Induction condition) x 2 (Time of Rating: pre-MEPS ratings vs. post-MEPS ratings) repeated-measures ANOVAs. There were no significant differences in initial ratings between induction conditions. Despite boosts in problem-solving performance, we did not observe differential changes in emotion regulation between the ESI and control induction conditions. Overall, we saw
decreases in levels of anxiety, perceived likelihood of a bad outcome taking place, and perceived
difficulty to cope with a bad outcome, and increases in the perceived likelihood of a good outcome
after participants engaged in the MEPS task (Figure 3.3; see Appendix Table 3.2 for mean rating
values), but these rating changes were similar after both inductions and did not statistically differ
(all $F$s < .69, $p$s > .41; see Appendix Table 3.1 for all main effects and interaction values). Trait
anxiety was not significantly related to any rating changes. Thus, the observed boosts in episodic
specificity and improved problem-solving performance are not related to differential
improvements in emotion regulation between the induction conditions.

Figure 3.3. Experiment 1 mean initial and post-MEPS task ratings in the control and specificity
conditions of: (A) anxiety; (B) perceived likelihood of a good outcome; (C) perceived likelihood of a bad outcome; and (D) perceived difficulty to cope with a bad outcome. The y-axis represents the mean rating per trial, and error bars represent one $SE$ of the mean.
**Experiment 1 Discussion**

Overall, the results of Experiment 1 show that the ESI increased the number of relevant steps and internal details that older adults generated while solving problems that they found to be worrisome and relevant to their daily lives, relative to a control induction. The magnitude of this observed increase in steps and details is similar to that previously reported by Madore and Schacter (2014) in older adults. However, contrary to our predictions, we did not observe larger improvements in emotion regulation when participants generated more details in the ESI, relative to the control condition.

Despite structural similarities in the MEPS task concerning worrisome events conducted in young adults by Jing et al. (2016) and in older adults in the current study, we do not think that task performance should be directly compared between the two age groups due to the different nature of worrisome experiences that are relevant to their daily lives (e.g., young adults worry about doing poorly on an exam or a job interview, whereas older adults worry about memory decline and their ability to stay mentally active; see Artistico et al., 2010, for experimental evidence). However, it is worth noting that whereas Jing et al. (2016) reported that the ESI boosted problem-solving performance in young adults by an average of 3.6 relevant steps and 9.68 internal details, it boosted performance in older adults in the current study by an average of only 1.89 steps and 4.64 details (consistent with Madore & Schacter, 2014). Thus, one possibility is that the boost in episodic specificity must reach a certain magnitude before it has perceptible influences on subjective anxiety between induction conditions. Whereas young adults showed more substantial increases in specificity with the ESI and thus reported larger subsequent changes in emotion regulation (Jing et al., 2016), the current boost observed in older adults in Experiment 1 may not have surpassed the necessary threshold. In order to observe changes in anxiety towards worrisome
future experiences in older adults, we may need to further amplify differences in episodic specificity and concreteness in problem solving.

Given that the boost in steps and details that is observed after the ESI in Experiment 1 is comparable to increases in episodic specificity in other ESI experiments in older adults (Madore et al., 2014; Madore & Schacter, 2014), it seems that effects of the ESI may have its limits in older adults. In Experiment 2, we aimed to use an alternative method to maximize differences in episodic specificity and concreteness in problem solving by comparing episodic future simulation in the MEPS task with a novel advice task that engaged more semantic reflection, as opposed to detailed episodic retrieval.

**Experiment 2**

In Experiment 2, we examined the effects of episodic detail during problem solving on subsequent changes in emotion regulation by comparing performance on two tasks: (1) a MEPS task identical to the task in Experiment 1, where participants concretely imagined and generated steps to solve a personal problem (hereafter we refer to this task as the “personal MEPS task”), and (2) a general advice task, which allowed participants to think about ways to solve the problem without retrieving as much concrete, episodic detail (see methods below for a more detailed task description). Whereas participants engaged in episodically detailed problem solving after both the ESI and control inductions (but produced even more episodic detail after ESI) in Experiment 1, participants should provide fewer concrete steps and details in the general advice task, relative to the personal MEPS task in Experiment 2. Thus, this task contrast should yield larger differences in episodic detail than that observed in Experiment 1.

Overall, we predicted that participants would generate more concrete, detailed steps in the personal MEPS task relative to the general advice task, and that we would observe larger
improvements in subsequent emotion regulation towards the worrisome future events in the personal MEPS condition relative to the general advice condition.

**Method**

**Participants.** A total of 40 older adult participants (ages 66 to 89, $M = 73.5$ years, 22 female) were recruited from postings around the Greater Boston area and were paid for their participation. All participants had normal vision and no history of neurological or psychological impairment. Participants were screened with a neuropsychological battery prior to participating in the study and were considered cognitively healthy, with a mean Mini-Mental Status Examination (Folstein et al., 1975) score of 29.16 ($SD = 1.35$, range = 25 – 30). Participants were also screened for psychiatric illness and were excluded if they indicated a clinical diagnosis of depression or anxiety. Informed written consent was obtained from all participants prior to beginning the study, which was approved by the Harvard University Institutional Review Board. A total of 8 participants were excluded due to attrition (5 participants) or noncompliance (3 participants), leaving 32 participants in the final sample. To keep the sample size in Experiment 2 comparable to that of Experiment 1, we stopped data collection after reaching the same approximate number of useable participants.

**Experimental Procedure.** Experiment 2 lasted approximately 4 hours across two separate sessions (2 hours each) that were spaced 3 to 7 days apart ($M = 5.43$ days). We employed a within-subjects design, and in each session, participants engaged in either a personal problem solving (MEPS) task or a general advice task involving the same set of standardized worrisome future experiences used in Experiment 1; task order was counterbalanced across participants (see Figure 3.4 for a diagram of the experimental procedure). Both sessions were executed using Qualtrics on
an Apple desktop computer, and participants wrote down their responses in a paper packet provided by the experimenter.

**Figure 3.4. Schema of experimental design for Experiment 2.**

In one session, participants completed a personal MEPS task (Platt & Spivack, 1975) that was identical in structure to the task in Experiment 1. Participants viewed 7 standardized worrisome events, 1 of which was used as a practice trial. For each event, participants were first asked to simulate a scenario in the near future during which they are worrying about the event for 2 minutes and rated each event on a 1 to 9 scale for perceived levels of anxiety, likelihood of experiencing a good and bad outcome, and difficulty to cope with a bad outcome. Next, they were presented with a story that described the beginning of the problem (e.g., worrying about the problem) and an ending solution (e.g., achieving the positive outcome specified for the event), and were asked to write down steps they would execute to reach the final solution for 5 minutes. Each event concluded with the same ratings pertaining to emotion regulation.

In the other experimental session, participants completed a general advice task. As in the MEPS task, participants viewed 7 standardized worrisome events, 1 of which used as a practice trial. They were first asked to simulate a personal scenario in which they are worrying about the event happening to themselves for 2 minutes, and rated each event for perceived levels of anxiety, likelihood of experiencing a good and bad outcome, and difficulty to cope with a bad outcome.
Next, they were presented with a story about an acquaintance experiencing a problem similar to the one they just described for themselves. Each story described the beginning of the problem (e.g., their acquaintance worrying about the problem) and an ending solution (e.g., their acquaintance achieving the positive outcome specified for the event), and they were given 5 minutes to write down as many pieces of general advice as they could think of to help their acquaintance reach the positive outcome. Each event trial concluded with personal ratings relating to emotion regulation (e.g., anxiety, perceived likelihood of good and bad outcome, and difficulty to cope with negative outcome), in addition to a rating of how similarly they would respond to solve the problem for themselves. Importantly, participants were instructed that the acquaintance they imagined should be a neighbor or a friend whom they know and have interacted with, but that they should avoid thinking about a very close friend or family member. These instructions were given to minimize the extent to which participants identified with their acquaintance, and to reduce the possibility that participants would “put themselves in the other’s shoes,” given existing literature showing that there is a larger overlap between knowledge and representations of the self and close others (such as a close friend or family member), relative to more distant others (e.g., Aron, Aron, Tudor, & Nelson, 1991; Aron & Fraley, 1999; Bower & Gilligan, 1979; Thornton, Weaverdyck, Mildner, & Tamir, 2018), and that we are more likely to utilize the episodic memory system when we think about more familiar others (Rabin & Rosenbaum, 2012). In contrast to simulating specific, episodically concrete steps to solve a problem in the personalized MEPS condition, the general advice task was designed to encourage participants to reflect upon and estimate ways of solving the problem in a more semantic manner without involving as much episodic detail. This logic is similar to that employed by Gaesser and Schacter (2014), where episodic simulation and concrete imagination of prosocial behaviors was contrasted with an “Estimate Helping” condition, where
participants engaged in more semantic retrieval to describe or estimate how someone could be helped. Thus, comparing performance on the personal MEPS and general advice tasks allowed us to isolate processes involved in retrieving specific episodic details. At the end of the second session, participants completed the STAI (Spielberger et al., 1970) to assess levels of trait anxiety.

**Coding.** Participant responses from the MEPS and advice tasks were scored in three ways. As in Experiment 1, responses were first scored as relevant steps (e.g., steps that lead the participant closer towards a positive solution) or other steps (e.g., steps that do not lead towards the solution; Platt & Spivack, 1975), and were subsequently scored using the traditional internal detail (e.g., episodic information about the people, places, and actions) and external detail (e.g., semantic facts and commentary) categories of the AI (Levine et al., 2002).

For Experiment 2, we also created a modified version of the AI scoring guidelines. Each internal detail (as defined by Levine et al., 2002) was further segregated by level of concreteness across six different categories: (1) references to specific individuals (e.g., family members, health professionals, etc.), (2) locations (e.g., stores, living- or hobby-related locations), (3) objects and other nouns (e.g., methods of contact, modes of transportation, physical aides, etc.), (4) actions (e.g., types of exercise, actions related to hobbies or health); (5) time descriptors (e.g., specific times, duration, and frequency), and (6) other references to sensory and perceptual detail (e.g., color, texture, etc.; see Appendix Table 3.3 for more information about categories and examples). In this framework, more general internal details (e.g., “family”, “store”) would be assigned one point as specified in the original AI protocol, whereas more detailed internal details (e.g., “daughter”, “Walmart”) would be assigned an additional point. We expected that the modified scoring method would provide more information and elicit greater differences in the levels of concreteness and detail contained within participant narratives for both the personal MEPS and
advice tasks, compared to traditional AI scoring. Two raters blind to task condition scored participant responses and had high inter-rater reliability for both steps and details (all Cronbach’s α > .91).

Results

We conducted a series of 2 (Task: MEPS vs. Advice) x 2 (Step type: relevant vs. other; Detail type: internal vs. external; Time of Rating: pre-task vs. post-task ratings for perceived anxiety, likelihood of a good outcome, likelihood of a bad outcome, and difficulty to cope with bad outcome) repeated-measures ANOVAs to test our hypotheses. Below, we focus on reporting the interactions to examine the impact of task on each of the variables of interest (see Appendix Table 3.4 for main effects). Post-hoc t-tests were two-tailed and Bonferroni corrected at the p < .05 level. The counterbalanced order of task and event list presentation did not have a significant effect on the reported analyses (ps > .12).

Changes in Steps and Details by Task. First, we found a significant interaction of Task (MEPS vs. Advice) x Step type (relevant vs. other), F(1,31) = 51.72, p < .001, ηp² = .63 (Figure 3.5A), such that participants generated more relevant steps, \( t(31) = 8.46, p < .001, 95\% \text{ CI} = [1.70, 2.77], d = 1.49 \), and fewer other steps, \( t(31) = -3.40, p < .01, 95\% \text{ CI} = [-.97, -.24], d = 0.60 \), in the personal MEPS task compared to the advice task (Relevant steps: \( M_{\text{difference}} = 2.23, SE = .26 \); Other steps: \( M_{\text{difference}} = -.61, SE = .18 \)). Next, we also found a significant interaction of Task x Detail type (internal vs. external), F(1,31) = 32.29, p < .001, ηp² = .51 (Figure 3.5B). Participants generated more internal details, \( t(31) = 6.79, p < .001, 95\% \text{ CI} = [4.08, 7.59], d = 1.20 \), and fewer external details, \( t(31) = -3.45, p < .01, 95\% \text{ CI} = [-4.47, -1.15], d = 0.61 \), in the personal MEPS task compared with the advice task (Internal details: \( M_{\text{difference}} = 5.84, SE = .86 \); External details:...
Thus, in comparing the two tasks, we found that participants indeed generated more episodically concrete solutions in the personal MEPS task.

However, it was clear upon reading participant responses that the traditional internal vs. external detail categories of the AI (Levine et al., 2002) did not fully capture differences in concreteness between the task conditions. For example, consider two steps provided by participants in the personal MEPS task and general advice task, respectively: (1) “I will call my daughter once a week using Facetime,” and (2) “I advise my neighbor to take advantage of technology more frequently to contact family.” According to traditional AI scoring, both “daughter” and “family” would be scored as one internal detail each, as would “once a week” and “more frequently”, or “Facetime” and “other technology.” However, “daughter” provides more specific information about the individual than does “family”, and “Facetime” is a very specific form of technology. Thus, we rescored all participant responses using a modified version of the AI (see Coding section above for more detail) to give more weight to more specific internal details. In doing so, we found that participants generated more internal details in the personal MEPS task as compared with the advice task, $t(31) = 9.05, p < .001, 95\% \text{ CI} = [6.77, 10.71], d = 1.60$ (Figure 3.5C), which revealed an even greater difference in levels of internal detail ($M_{\text{difference}} = 8.74$ details, $SE = .97$) than in Experiment 1 (for modified AI scoring results from Experiment 1, see Appendix). Thus, the modified AI scoring method captured more subtle nuances in the levels of detail contained in participant narratives between the two tasks. Importantly, we were able to relate this difference in specificity and concreteness to subsequent changes in emotion regulation concerning the imagined worrisome events.
Figure 3.5. Experiment 2 mean task effects (personal MEPS vs. general advice task) on (A) relevant and other steps, (B) internal and external details via traditional AI scoring, and (C) internal details via modified AI scoring. The y-axis represents the mean number of steps or details per trial, and error bars represent one SE of the mean.

Changes in Emotion Regulation Ratings by Task. To examine changes in measures of emotion regulation after each task, we conducted a series of 2 (Task: MEPS vs. Advice) x 2 (Time of Rating: before vs. after task) repeated-measures ANOVAs. There were no significant differences in initial ratings between task conditions. Overall, we found significant Task x Time of Rating interactions for changes in ratings of anxiety, $F(1,31) = 41.33, p < .001, \eta^2_p = .57$ (Figure 3.6A), perceived likelihood of a good outcome, $F(1,31) = 10.31, p < .01, \eta^2_p = .25$ (Figure 3.6B), perceived likelihood of a bad outcome, $F(1,31) = 10.70, p < .01, \eta^2_p = .26$ (Figure 3.6C), and perceived difficulty to cope with a bad outcome, $F(1,31) = 14.10, p = .001, \eta^2_p = .31$ (Figure 3.6D). In the personal MEPS condition, compared with the general advice condition, participants reported a larger reduction in anxiety, $M_{\text{difference}} = -.94, SE = .15, t(31) = -6.43, p < .001, 95\% \text{ CI} = [-1.23, - .64], d = 1.14$, a larger increase in the perceived likelihood of experiencing a good outcome, $M_{\text{difference}} = .58, SE = .18, t(31) = 3.21, p < .01, 95\% \text{ CI} = [.21, .94], d = 0.57$, a larger reduction in the perceived likelihood of experiencing a bad outcome, $M_{\text{difference}} = -.68, SE = .21, t(31) = -3.27, p < .01, 95\% \text{ CI} = [-1.10, -.26], d = 0.58$, and a larger decrease in the perceived difficulty to cope with a bad outcome, $M_{\text{difference}} = -.61, SE = .16, t(31) = -3.76, p = .001, 95\% \text{ CI} = [-.94, -.28], d = 0.66$ (see Appendix Table 3.5 for mean rating values).
Figure 3.6. Experiment 2 mean initial and post-task ratings in the personal MEPS and general advice tasks of: (A) anxiety; (B) perceived likelihood of a good outcome; (C) perceived likelihood of a bad outcome; and (D) perceived difficulty to cope with a bad outcome. The y-axis represents the mean rating per trial, and error bars represent one $SE$ of the mean.

Further, we found a significant negative relationship between the difference in the levels of internal detail using modified AI scoring (i.e., internal details in personal MEPS condition – internal details in general advice condition) and the difference in the perceived anxiety towards the worrisome events (i.e., change in anxiety rating in personal MEPS condition – change in anxiety rating in general advice condition), such that the greater the boost in detail between conditions, the larger the observed decrease in anxiety towards the worrisome events, $r_s(30) = -.399, p < .05$ (Figure 3.7A). There was also a significant positive relationship between the change in internal detail and the difference in ratings of the perceived likelihood of a good outcome, $r_s(30) = .374, p < .05$ (Figure 3.7B). We found trending negative relationships between changes in
internal detail versus difference in ratings of the perceived likelihood of a bad outcome, $r_s(30) = -.348$, $p = .051$ (Figure 3.7C), and versus differences in ratings of the perceived difficulty to cope with a bad outcome, $r_s(30) = -.328$, $p = .067$ (Figure 3.7D). Trait anxiety was not significantly related to any changes in ratings.

**Figure 3.7.** Experiment 2 correlations between the average boost in internal details via modified AI scoring between personal MEPS and general advice tasks (i.e., internal details in personal MEPS task – internal details in general advice task) and average decreases between personal MEPS and general advice tasks (i.e., rating after personal MEPS task – rating after general advice task) of: (A) anxiety; (B) perceived likelihood of a good outcome; (C) perceived likelihood of a bad outcome; and (D) perceived difficulty to cope with a bad outcome.

Although the general advice task encouraged more semantic reflection relative to the personal MEPS task, it is likely that participants still engaged in some degree of simulation while generating steps to help an acquaintance solve a worrisome problem, and that the advice they provided involved similar steps that they would engage in themselves to solve a personal problem.
As previously mentioned, after each event in the advice task, participants rated how similarly they would respond to solve the problem for themselves, which we included as a proxy for simulation during the advice task. We found a negative relationship between the similarity of one’s own response versus the change in anxiety towards the events in the advice task, \( r_s(30) = -.35, p = .05 \). Thus, the greater the similarity between the advice given to an acquaintance and one’s own problem-solving actions, the greater the observed improvements in emotion regulation.

**Experiment 2 Discussion**

In contrast with Experiment 1, data from Experiment 2 support the hypothesis that greater specificity of problem-solving simulations concerning worrisome events relevant to older adults leads to reduced anxiety and improved emotion regulation towards those events. First, we found that older adults generated more relevant steps and internal details (via both traditional and modified AI scoring) in the personal MEPS condition, relative to the general advice condition. It is worth noting that the advice condition did not induce a state of overgenerality; participant responses in the advice condition consisted of detailed, concrete and actionable steps, but these steps contained fewer descriptors of the highest specificity (e.g., naming individuals, stores, or specifying the exact technology used for communication) than responses in the personal MEPS task. Thus, we think it is unlikely that critical effects from Experiment 2 stem from overgeneral responding in the advice task. Second, participants reported larger improvements in emotion regulation in the personal MEPS condition compared to the advice condition, and these improvements were directly related to the difference in levels of internal detail generated between task conditions. Overall, these results suggest that episodic detail of simulation is positively related to improved emotion regulation across a number of different measures.
General Discussion

Across two experiments, we tested the hypothesis that increasing the level of episodic detail during problem solving in older adults would enhance emotion regulation towards worrisome future events that are relevant to their daily lives. In Experiment 1, we showed that an episodic specificity induction (ESI) successfully boosted the number of relevant steps and internal details that older adults generated during a means-end problem solving (MEPS) task, thus replicating and extending previous results by Jing et al. (2016) and Madore and Schacter (2014). Contrary to our predictions, we did not find greater reductions in anxiety in the ESI relative to the control condition, which we speculated may be related to the observed magnitude of the boost in episodic detail in Experiment 1, which was much smaller than the increase previously observed by Jing et al. (2016) in young adults but was similar to that observed by Madore and Schacter (2014) in older adults. Because evidence suggests that the ESI is limited in the degree to which it boosts episodic detail in older adults, in Experiment 2 we sought to amplify differences in levels of episodic detail by turning to a different task contrast—namely, by comparing older adults’ performance on a personal MEPS task (i.e., generating episodically concrete steps to solve one’s own problem) with a general advice task (i.e., providing general advice for an acquaintance experiencing a similar problem, which focused on more semantic processing). We found that participants indeed provided more relevant steps and internal details in the personal MEPS task compared with the general advice task, both using the traditional AI scoring framework (Levine et al., 2002) and a modified AI protocol that ascribed greater weight to more specific internal details. The effect sizes for the boost in relevant steps and internal details in Experiment 2 for the personal MEPS vs. general advice task contrast were also numerically larger than those observed in Experiment 1 for the ESI vs. control induction contrast. Further, we found that larger increases
in episodic detail in the personal MEPS task relative to the general advice task were linked to larger subsequent decreases in anxiety and improved emotion regulation towards the target worrisome events, which supported our initial hypotheses. In contrast to previous studies that successfully manipulated levels of episodic specificity in older adults but did not find subsequent changes in global measures of mood when using established questionnaires (e.g., Jumentier et al., 2017; Leahy et al., 2018a), in the current experiments we found for the first time that manipulating specificity and assessing changes in anxiety and emotion regulation on the level of individual events—particularly those that older adults found to be very worrisome in the context of their daily lives—revealed the relationship that we had predicted. Thus, although emotion regulation improves overall with age due to differential processing and experience of emotional information, and the use of different strategies in the face of emotional situations (Blanchard-Fields, 2007; Gross et al., 1997; Mather & Carstensen, 2005; Scheibe & Carstensen, 2010; Urry & Gross), older adults nonetheless experience a niche of worries primarily related to their health and interpersonal relationships (Hunt et al., 2003; Powers et al., 1992). Our data indicate that increasing the specificity of future-oriented problem solving towards these concerns can alleviate their worry.

Although we created the general advice task to encourage participants to engage in more semantic processing in contrast to concrete, detailed simulation, they likely still engaged in some degree of episodic simulation (e.g., “putting themselves in another’s shoes”). We included an indirect measure of simulation in the advice task by asking participants to rate how similarly they would respond to solve the problem for themselves, and found that the more similar the advice provided was to one’s own problem-solving actions, the greater the observed reduction in anxiety towards the worrisome event. This finding suggests that the degree to which participants engage in simulation during the advice task is related to the observed changes in subsequent emotion
regulation. We also note that differences in self-relevance between the two tasks could have contributed to the observed rating changes. That is, solving personal problems in the personal MEPS condition likely required a greater degree of self-relevant processing than solving others’ problems (similar to their own) in the advice task, which in turn may have influenced reported levels of anxiety between the two task conditions. Critically, however, participants engaged in similar processes in the two tasks because of the overlap in their own problems and their acquaintances’ problems (as instructed in the task), but generated more episodically detailed and concrete responses in the personal MEPS task relative to the general advice task, thus allowing us to compare this boost in detail to the observed changes in emotion regulation.

Although these data suggest an important role for episodic specificity in emotion regulation towards worrisome future experiences in older adults, we are not claiming that episodic memory is the only form of memory that is involved in future-oriented problem solving. Existing research has highlighted the role of semantic memory in organizing various kinds of future thinking (Demblon & D’Argembeau, 2014; D’Argembeau & Demblon, 2012; Irish & Piguet, 2013; Klein, 2013; Szpunar et al., 2014). Further, reports of general improvements in emotion regulation with age (Scheibe & Carstensen, 2010; Urry & Gross, 2010), despite known declines in the specificity of memory and imagination (Schacter et al., 2013, 2018), hint that there are other factors at play that likely influence emotional responses towards negative or stressful events. For example, compared with young adults, older adults can draw upon more years of accumulated experience when they encounter a negative situation to select the best plan of action (Blanchard-Fields, 2007; Urry & Gross, 2010), which likely taps into their semantic knowledge of which emotion regulation strategies tend to be more effective based on the resolution of similar past scenarios. Nonetheless, the current studies provide evidence for a role of episodic retrieval in problem solving and emotion
regulation. An important direction for future research will be to examine how semantic memory and other cognitive processes impact emotional simulations and appraisals of the future in aging and in young adulthood.

Increasing levels of episodic specificity may impact emotion regulation in a variety of ways. It is thought that in many emotional disorders, overgeneral memory and future thinking help patients to avoid the negative physiological arousal associated with visually imagining an aversive past or threatening future event (Borkovec et al., 1998; McGowan et al., 2017; Stöber & Borkovec, 2002; Williams, 2006), which hampers their ability to concretely plan for or solve the problem at hand. However, when participants solve these negative or worrisome problems with higher levels of specificity and concreteness (via the ESI versus control induction, or by contrasting personal MEPS versus general advice task performance), they retrieve more episodic details concerning the objects, location, actions, individuals, and other relations contained within the event, which makes the event more tangible (Schacter & Madore, 2016). Simulations are organized to contain sequences of causally-linked actions that help to yield concrete plans, and the details contained within simulations serve to make an event seem more realistic and plausible by providing more information about how the event might take place, thus enhancing the subjective likelihood that the event will actually occur (Anderson, 1983; Boland, Riggs, & Anderson, 2018; Carroll, 1978; Szpunar & Schacter, 2013). Indeed, in Experiment 2, we found that older adults reported a higher perceived likelihood that a positive outcome would take place and a lower perceived likelihood that a negative outcome would take place when they generated concrete details about a problem-solving plan in the personal MEPS task, relative to when they listed fewer concrete steps in the general advice task. Further, gaining access to relevant, more realistic features of the imagined event may help participants realize that the scenario is not as negative or as unmanageable as it
was initially perceived to be. For example, our data showed that participants reported a larger decrease in the perceived difficulty to cope with a bad outcome after thinking about various aspects of a worrisome problem in the personal MEPS condition. Thus, manipulations that boost episodic detail may improve emotion regulation by linking imagined actions that can yield a concrete plan and by favorably changing the perceived plausibility of those actions (Taylor et al., 1998; Taylor & Schneider, 1989). Given that older adults exhibit reduced specificity of autobiographical memory and future thinking (Schacter et al., 2013, 2018), tend to have futures that are increasingly associated with negative outcomes (e.g., disease, death), and are a vulnerable population with a relatively high prevalence of emotional disorders (Djernes, 2006; Reynolds et al., 2015), it is important to continue exploring how improving problem solving and the concreteness of future thinking may benefit their mental health and quality of life (for a related review, see Bahk & Choi, 2017).

This research may have implications not only for aging populations, but also for other clinical populations. The current studies are amongst a body of recent work that has demonstrated that increasing the specificity of autobiographical memory and future thinking can be linked to boosts in problem-solving behaviors (Leahy et al., 2018a; McFarland, Primosch, Maxson, & Stewart, 2017) and improvements in clinical symptoms in depressed and anxious patients (Lang et al., 2012; Moradi et al., 2014; Neshat-Doost et al., 2012; Raes et al. 2009; for review, see Hitchcock et al., 2017). Thus, increasing the specificity with which people imagine future experiences might serve as a useful intervention to foster more constructive problem-solving behaviors that can reduce anxiety in both aging and clinical populations, which highlights the importance of positive and constructive mental future simulations for emotion regulation.
Paper 3 Acknowledgements

This research was supported by National Institute on Aging grant AG08441 to DLS. KPM is supported by National Institute on Aging grant F32AG059341. We thank Jyotika Bindra and Laura Hatt for assistance with various aspects of the experiments.
Dissertation General Discussion

The three papers of the dissertation had two primary goals. First, we aimed to investigate the episodic memory mechanisms underlying our ability to engage in various forms of constructive and positive future thinking. To do so, we primarily used an episodic specificity induction (ESI; Madore et al., 2014) to dissociate the contributions of episodic from non-episodic processes (e.g., semantic memory and descriptive ability; for review, see Schacter & Madore, 2016) in various future-oriented imagination tasks, including problem solving, episodic reappraisal, and the generation of alternative future events. Critically, if the tasks recruit episodic processes, then the ESI should selectively boost levels of episodic detail in participant responses. Second, we aimed to examine adaptive ways in which simulating the future impacts our original expectations and perceptions of the future by relating the observed boosts in episodic specificity to subsequent changes in anxiety, valence, and plausibility in a series of negative, anxiety-provoking future events.

In Paper 1 (Jing, Madore, & Schacter, 2016), we show that the ESI, relative to a control induction, boosts the relevant steps and internal, episodic details that participants provide when solving a series of personally worrisome future problems (MEPS task; Platt & Spivack, 1975) and reframing negative outcomes (episodic reappraisal task). The effect of the ESI on both tasks confirms that problem solving in the context of the MEPS task draws on episodic memory, consistent with the findings of Madore and Schacter (2014), and demonstrates for the first time that the episodic reappraisal task, modified from traditional (and more semantic) cognitive reappraisal tasks, can also recruit episodic memory processes. In addition, we show that increasing the specificity of problem solving and reappraisal behaviors towards worrisome future events via the ESI is positively related to improved emotional well-being towards those events (e.g.,
decreases in anxiety and negative affect, changes in perceived likelihood of a positive or negative outcome, and increases in the indicated use of engagement coping behaviors).

Relatedly, Paper 2 (Jing, Madore, & Schacter, 2017) demonstrates that the ESI can also boost the number of positive alternative outcomes that participants generate towards anticipated negative events, relative to a control induction, confirming our hypothesis that alternative event generation relies on episodic memory. We also find that increasing the generation of positive alternative event outcomes is related to larger decreases in the perceived plausibility and negativity of the original events, enabling participants to adjust their original pessimistic predictions.

Building on the findings of Paper 1, Paper 3 (Jing, Madore, & Schacter, 2019) shows that the ESI increases the specificity with which older adults solve a series of worrisome future problems. However, contrary to our predictions, the observed boost in specificity is not related to subsequent changes in anxiety towards the worrisome future events, which we hypothesize may relate to the smaller overall increase in relevant steps and internal details that is observed in older adults, relative to the changes that were observed in Paper 1 (Jing et al., 2016) in young adults. We designed a new task contrast that compares levels of detail that participants provide when solving their own problems (personal MEPS task), relative to when they provide more general advice to help an acquaintance solve problems similar to the participants’ own concerns (general advice task). Results show that older adults provide more relevant steps and internal details in the personal MEPS condition compared to the general advice condition, and this difference is larger than that observed in Paper 1 when comparing the ESI and control induction conditions. Further, the difference in detail and concreteness between the two conditions is positively related to improvements in emotional well-being towards the worrisome events.
Theoretical and Functional Implications

On the whole, the findings from these three papers are consistent with the constructive episodic simulation hypothesis (Schacter & Addis, 2007, in press), which proposes that the flexible nature of our episodic memory system allows us to recombine elements of past experiences to imagine the future in novel ways, which can have important downstream consequences on preparatory actions and behaviors. The impact of the ESI on problem solving, episodic reappraisal, and alternative event generation is significant because it allows us to isolate contributions of episodic memory to these various tasks that require generative search and retrieval. Recently, Schacter and Madore (2016) have suggested that the retrieval of specific episodic details during the ESI biases participants to focus on details of a similar nature during subsequent tasks that involve event construction, the process of building a coherent mental event comprised of details relating to people, locations, and action sequences (Radvansky & Zacks, 2014; Romero & Moscovitch, 2012; see also Madore, Jing, & Schacter, 2018), all of which also contribute to the construction of a mental scene (Hassabis & Maguire, 2007; Schacter, Madore, & Jing, 2019). Thus, as people imagine the actions they might take to solve a worrisome problem, interactions they might have with others in specific places during these actions, as well as alternative ways in which those scenarios might play out, the ESI is impacting levels of episodic details contained in all of those elements. Importantly, however, we do not wish to imply that episodic memory is the only form of memory that is involved in these tasks. Previous research has highlighted the role of semantic memory in organizing various kinds of future thinking (D’Argembeau & Demblon, 2012; Demblon & D’Argembeau, 2014; Irish & Piquet, 2013; Klein, 2013; Szpunar et al., 2014), and an important direction for future research will be to examine how semantic memory impacts the
generation of alternative future outcomes. Nonetheless, the present research provides clear evidence of a role for episodic memory.

Mechanistically, increasing levels of episodic specificity during constructive, positive future thinking may impact emotional well-being in several ways. As previously discussed, overgeneral memory and future thinking help patients with emotional disorders to avoid the negative physiological arousal associated with visually imagining an aversive past or threatening future event (Borkovec et al., 1998; McGowan et al., 2017; Stöber & Borkovec, 2002; Williams, 2006). Reduced specificity, in turn, hampers their ability to concretely plan for or solve the problem. However, when people solve these negative or worrisome problems or consider alternative outcomes with higher levels of specificity and concreteness, they retrieve more episodic details concerning the objects, location, actions, individuals, and other relations contained within the event, which makes the event more tangible (Schacter & Madore, 2016).

Relatedly, Taylor et al. (1998; see also Taylor & Schneider, 1989) have proposed that mental simulations possess a number of intrinsic properties that benefit emotion regulation. First, imagining a hypothetical event provides more information about how it might take place, and thus can enhance the subjective likelihood that the event will actually occur (Anderson, 1983; Carroll, 1978; Gregory et al., 1982; Szpunar & Schacter, 2013). Second, enhancing the likelihood of an event might pave the road for taking action. Simulations typically consist of an organized sequence of actions that are causally linked, which can help to yield a concrete plan. The concreteness of simulation can provide important information about the event that contributes to a more realistic representation of the constraints and requirements of the event or task (Hayes-Roth & Hayes-Roth, 1979). Third, gaining access to relevant, more realistic features of the imagined event may help people adjust their frequently inaccurate predictions regarding their emotional reactions towards
anticipated negative events. For example, Gilbert and Wilson (2007, 2009) have discussed that the mental previews that we generate are prone to various biases, including that they are often based on the most available and salient information that comes to mind, which may not be representative of a “typical” experience, and that we tend to focus on “essential” features while omitting “incidental” features about the context of the event that are deemed as less important but may actually have a large influence on our subjective emotional experience. Further, we often overemphasize the climax of an emotional experience while disregarding our ability to subsequently adapt to and temper our reactions to such an experience. The process of simulating problem-solving behaviors or alternative positive outcomes can be beneficial in this context, because simulation increases the availability and plausibility of potential solutions, it brings to light previously ignored contextual details about the situation, and may call attention to our ability to emotionally and behaviorally adapt to a variety of situations, leading to a subsequent adjustment of negative expectations. Overall, using the ESI to increase the specificity of positive mental simulation might serve as an upstream intervention that can augment all of these links, thus leading to positive downstream consequences such as reductions in anxiety concerning worrisome future experiences and improving emotional well-being as a whole.

This research may have implications for clinical populations, particularly for patients with emotional disorders. Recent evidence has shown that increasing the specificity of autobiographical memory using the ESI can boost future-oriented problem solving in depressed patients (McFarland et al., 2017). Further, enhanced memory specificity and concreteness of mental imagery has been linked to improvements in depressive symptoms (Lang et al., 2012; Neshat-Doost et al., 2012; Raes et al., 2009) and PTSD symptoms (Moradi et al., 2014) with respect to negative and distressing past events, and can also be beneficial for processing worrisome future events that have
not yet been experienced (Jing et al., 2016; Skodzik et al., 2017; for review, see Hitchcock et al., 2017).

**Future Directions**

In the current papers, we primarily focus on the impact of episodic specificity on subsequent emotional well-being. However, it is important to note that emotional well-being may also be modulated by other important aspects of worrisome events, such as the subjective ease and perceived likelihood of success in solving a problem (Eccles & Wigfield, 2002). For example, generating steps that are more easily achievable and attainable may contribute more to emotion regulation success than generating steps that are more difficult to achieve. In a related vein, those prone to worrying (both non-clinical and clinically anxious) tend to show reduced problem-solving confidence (that was unrelated to their actual problem-solving ability) and reduced perceived control over the problem-solving process, which have been causally linked with higher levels of worry (Davey, 1994; Davey, Jubb, & Cameron, 1996; Dugas, Gagnon, Ladouceur, & Freeston, 1998). Thus, confidence and control are two other variables that might impact emotion regulation.

It is worth noting that in Papers 1 and 3, we measured participants’ perceived levels of control over the worrisome events before and after engaging in problem solving. While participants reported increases in perceived control after solving the target problems, there were similar increases in perceived control observed across both the ESI and control conditions. These data suggest that increasing the specificity of problem solving does not lead to differential increases in perceived control, and it is thus unlikely that the relationship between increased specificity and enhanced emotional well-being is mediated by perceived control in the context of these experiments. However, an important direction for future research will be to investigate how factors
such as perceived control, confidence, and the likelihood of success might further influence emotion regulation towards worrisome future events.

In addition, the tasks used in the current papers all require generative search and retrieval of possible solution steps or alternative outcomes. However, there exists no experimental evidence that examines the effect of non-generative processes on future thinking and subsequent well-being. One question that arises from the current experiments is whether the observed improvements in emotion regulation critically require people to generate solution steps themselves (i.e., involving generative search), or if it would be sufficient for people to merely read effective steps that others have produced (i.e., involving non-generative processes). An analogy drawn from a tangential literature concerns active versus passive learning strategies, whereby active retrieval of previously studied information in the form of brief tests or quizzes leads to better subsequent learning, relative to passive studying techniques, such as re-reading notes (e.g., Roediger & Karpicke, 2006; Szpunar, Khan, & Schacter, 2013). One hypothesis would be that self-generating steps to solve a problem (e.g., Jing et al., 2016, 2019) engages higher levels of concrete, detailed simulation than does reading steps provided by others, which should lead to larger improvements in emotional well-being. Alternatively, if people spontaneously engage in simulation or visualization as they read steps, the two conditions could result in similar improvements in emotion regulation. Thus, future studies can shed light on whether or not self-generation of solutions towards worrisome problems, relative to reading solutions generated by others, might have differential impacts on subsequent measures of interest, including emotional well-being.

While much of the dissertation has examined the impact of detailed episodic simulation on emotion regulation, another future avenue of research involves the effect of emotional future simulation on our “memories” for how anticipated future events may play out—that is, how the
event is remembered once it actually takes place. As previously discussed, the constructive nature of our episodic memory system allows us to retrieve and recombine elements of our past experiences to imagine hypothetical future scenarios (Schacter & Addis, 2007, in press). However, the same processes that contribute to this adaptive and flexible memory system may also lead to memory distortion (Schacter, 2012; Schacter, Guerin, & St. Jacques, 2011). One type of memory distortion, imagination inflation, occurs when simulating a novel experience leads individuals to believe that the imagined event actually occurred in the past (e.g., Garry, Manning, Loftus, & Sherman, 1996; Loftus, 2003). However, can simulation of novel future events also alter our memories for what we originally anticipated, once the event actually occurs? A recent experiment by Devitt and Schacter (2018) specifically examined whether the valence of imagined future events influenced subsequent emotional memory for those events. Participants were asked to simulate a series of positive and negative future events, and then read a neutral narrative (that contained both positive, negative, and neutral details) that described a hypothetical outcome for each event. During a subsequent recognition test, participants were presented with a series of details and were asked to indicate whether or not they saw that information in the original narrative: participants viewed true positive and negative details from the narrative, false details of the opposite valence to what was presented in the narrative (e.g., positive details that were originally negative, and vice versa), and false foil items that were not presented in the narrative. Devitt and Schacter (2018) found that participants were more likely to say they saw false positive details (when the original detail was actually negative) and less likely to say they saw false negative details (when the original detail was actually positive) in the narrative if the original simulation was positive, relative to if the original simulation was negative. These data indicate that positive future simulation biases participants to remember the event more positively, at the expense of forgetting some true
negative details. Thus, future research can harness the effects of imagination inflation in a beneficial way, and examine whether constructive, positive future simulation towards an anticipated negative event might also bias individuals to remember the negative event in a more positive light, which should also impact subsequent emotional well-being.

Future work should also investigate the neural correlates underlying how detailed episodic simulation affects subsequent emotion regulation. Neuroimaging studies over the past decade have revealed that remembering the past and imagining the future (including future-oriented problem solving tasks; Gerlach et al., 2011; Gerlach, Spreng, Madore, & Schacter, 2014) engage similar regions within a core network, including the hippocampus, medial prefrontal cortex, anterior cingulate cortex, inferior parietal lobule, and regions of the posterior cingulate cortex (for recent reviews, see Benoit & Schacter, 2015; Schacter et al., 2012; Szpunar, 2010). Specifically in relation to emotional future simulation, existing studies have shown that the ventromedial prefrontal cortex (vmPFC) is more active when people imagine future experiences associated with positive outcomes and rewards (Benoit, Szpunar, & Schacter, 2014; D’Argembeau, Xue, Lu, Vander Linden, & Bechara, 2008; Sharot, Riccardi, Raio, & Phelps, 2007). The vmPFC is also thought to mediate the transfer of affective value between disparate elements of a simulated event (e.g., transfer of positive value from a pleasant person to a neutral location), resulting in a different, emergent affective quality of the novel episode itself (Benoit et al., 2014). In addition, Gerlach et al. (2014) reported that when individuals were asked to imagine a positive outcome where they have achieved a goal (i.e., outcome simulation), relative to imagining steps to reach a goal (i.e., process simulation), activity in the mPFC and amygdala were functionally coupled. Further, the mPFC and amygdala also formed a functional network with core network and reward-processing regions, including the vmPFC. Despite these existing neuroimaging studies that have examined
problem solving and emotional simulation more broadly, none have investigated the impact of simulation on subsequent emotion regulation.

A separate literature has documented the neural signature of clinical anxiety and the effects of various emotion regulation strategies, which involve overlap with core network regions such as the mPFC and anterior cingulate cortex. Anxiety disorders occur in part due to a dysfunction of a frontal-limbic circuit, including regions such as the amygdala, PFC, and rostral anterior cingulate cortex (rACC), which are responsible for modulating a normal fear response and resolving emotional conflict. Existing studies in both animals and humans have shown that fear conditioning potentiates inputs from the various nuclei of the amygdala (Paré, Quirk, & LeDoux, 2004; Milad, Rauch, Pitman, & Quirk, 2006), which communicate with the brainstem and thalamus to subsequently regulate an autonomic fear response (LeDoux, Iwata, Cicchetti, & Reis, 1988). The PFC then exerts top-down control over the amygdala to dampen the fear response during extinction learning (Milad and Quirk, 2002; Phelps, Delgado, Nearing, & LeDoux, 2004; for review, see Hartley and Phelps, 2010). The rACC has also been reported to modulate and attenuate amygdala activity in order to resolve emotional conflict (Bush, Luu, & Posner, 2000; Etkin, Egner, Peraza, Kandel, & Hirsch, 2006). However, pathological anxiety involves hyperreactivity of the amygdala coupled with a general decrease in activation of prefrontal regions (for review, see Etkin, 2010; Shin & Liberzon, 2010; Taylor & Whalen, 2015). Functional alterations in the amygdala and prefrontal regions subsequently result in a heightened fear response and an inability to dampen that response in the face of a perceived threat that is not actually valid. This relationship has been supported by studies that reported that individuals with lower anxiety levels tend to have a greater functional coupling between the prefrontal cortex and the amygdala (Etkin, Prater, Schatzberg, Menon, & Greicius, 2009; Pezawas et al., 2005), and also, that the combined pattern of increased
rACC and reduced amygdala activity is related to reduced symptom severity and improved treatment response in GAD patients (Whalen et al., 2008).

Notably, common emotion regulation strategies have been shown to impact regions within this frontal-limbic circuit. For example, cognitive reappraisal tasks, which involve reinterpreting a negative stimulus to downregulate one’s emotional response, recruit lateral prefrontal and posterior parietal cognitive control regions to dampen amygdala activity (for review, see Buhle et al., 2013; Ochsner & Gross, 2005; Oschner, Silvers, & Buhle, 2012). One common hypothesis states that these lateral prefrontal regions attenuate amygdala activity via the vmPFC, a region that integrates affective valuations with other distributed knowledge, and may serve a more general negative emotion inhibitory function that extends beyond fear conditioning paradigms (Diekhof, Geier, Falkai, & Gruber, 2011; Etkin, Egner, & Kalisch, 2011). However, another competing hypothesis suggests that these cognitive control regions exert changes in lateral temporal regions associated with semantic representations, which in turn dampen the amygdala response (Buhle et al., 2013). The recruitment of these lateral temporal regions might reflect the semantic nature of the reappraisal task, which draws attention to factual information about the stimulus or task that detracts from the more subjective emotional information. For example, an experiment by Goldin et al. (2009) instructed participants to reinterpret the content of a negative belief, “No one likes me,” to include more objective information, such as, “That is not always true, some people like me,” or “This is only a thought, not a fact.” Although tasks such as cognitive reappraisal are quite different from the tasks that we used in the current dissertation, it is possible that positive simulation alters subsequent emotion well-being via a similar pathway. For example, positive future simulation (e.g., problem solving and episodic reappraisal) may recruit the vmPFC (e.g., D’Argembeau et al., 2008; Gerlach et al., 2014; Sharot et al., 2007), amongst other core network
regions, which may subsequently downregulate amygdala activity associated with an anticipated future threat. As we demonstrated behaviorally in Papers 1 and 3, the level of episodic detail with which people positively simulate the future further impacts emotion regulation. Recently, Madore, Szpunar, Addis, and Schacter (2016) have reported a neural signature of increased episodic specificity via the ESI. After the ESI (relative to a control induction), participants showed significantly more activity during the construction of novel future events in several core network regions, including the left anterior hippocampus (implicated in relational processing and the flexible recombination of learned elements into a novel representation; Giovanello, Schnyer, & Verfaellie, 2009; Preston, Shrager, Dudukovic, & Gabrieli, 2004) and the right inferior parietal lobule (supports imagination of specific, as opposed to general, events; Addis, Cheng, Roberts, & Schacter, 2011). Importantly, activity in these two regions also tracked with differences in levels of episodic detail between the induction conditions (i.e., ESI vs. control induction). Thus, it is possible that the coupling of reward-processing regions (such as the vmPFC) and other core network regions that track episodic detail and simulation more broadly might support changes in emotional well-being that are modulated by episodic specificity.

Conclusion

Across the three papers in this dissertation, we show that the ESI enhances performance on a variety of positive future simulation tasks, including problem solving, episodic reappraisal, and alternative event generation. Future work should continue to study different types of positive simulation tasks and the role that episodic memory may play in these tasks. We also demonstrate for the first time that increased episodic detail of simulation can enhance emotional well-being across a number of different measures. Overall, this line of work may have important implications.
for understanding how cognitive processes underlying episodic simulation impact the regulation of future-oriented emotion in both healthy and clinical populations.
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Appendix

Paper 1 (Jing, Madore, & Schacter, 2016)

Induction Scripts

See below for the episodic specificity induction script (Experiments 1 and 2), and impressions induction script (Experiment 2).

Episodic Specificity Induction Script

Introduction: So now I’m going to ask you a few questions about the video you watched. I haven’t seen the video myself, so you’re the expert on that. I’m also going to use the audio-recorder and write down what you say to keep track if that’s okay. How does that sound to you?

Mental imagery about the surroundings: Okay, so first I want you to close your eyes and get a picture in your head about the surroundings of the video you watched. I want you to think about what types of things were in the environment and how they were arranged and what they looked like. Once you have a really good picture in your head I want you to tell me everything you remember about the surroundings. Try to be as specific and detailed as you can.

General probing about the surroundings:

- Tell me more about… (details mentioned)
- Tell me more about how the kitchen was arranged.
- Tell me more about what was in the kitchen.
- Were there any other rooms?

Mental imagery about the people: Now I want you to close your eyes and get another picture in your head, this time about the people in the video you watched. I want you to think about what the people looked like and what they were wearing. Once you have a really good picture in your
head I want you to tell me everything you remember about the people in the video. Again, try to be as specific and detailed as you can.

**General probing about the people:**

- Tell me more about… (details mentioned)
- Tell me more about the man/woman’s outfit.
- Tell me more about the man/woman’s face.
- What color hair did the man/woman have?

**Mental imagery about the actions:** Now I want you to close your eyes and get a picture in your head about the actions in the video you watched. I want you to think about what the people were actually doing in the video and how they did these things. Once you have a really good picture in your head I want you to tell me everything you remember about the actions starting with the first one and ending with the last one. Try to be as specific and detailed as you can.

**General probing about the actions:**

- Tell me more about… (action mentioned)

**Follow-up and repeat for actions:** [only do this if participant doesn’t give sequence of actions first time around]

- What happened after that?
- What was the next thing?
- What was the last thing that happened?
**Impressions Induction Script**

**Introduction:** So now I’m going to ask you a few questions about the video you watched. I’m also going to use the audio-recorder and write down what you say to keep track if that’s okay. How does that sound to you? First I want you to tell me what you thought about the video. Just tell me what your thoughts and opinions of it were. What were your general impressions of the video?

**Question bank:**

- What adjectives would you use to describe the setting of the video? The people? The actions?
- Did you have any other opinions about the setting of the video? Did you have any other opinions about the people? The actions?
- Can you describe the whole video in one or two words? What one or two words would you use?
- Did you like the video?
- When do you think the video was made?
- How do you think it was made? (what equipment do you think they used?)
- Did the video remind you of anything? (from your own life)
- Can you guess how big the place was based on the video?
- Can you guess the people’s occupations based on the video?

**Concluding remarks:** Were there any other thoughts or opinions you had about the video? Is there anything else you wanted to say about it?
Means-End Problem Solving Materials

See below for MEPS problem story format, instructions, and a sample story. Italic text surrounded by brackets indicates material that is personalized to each participant. Note that the 1-minute instructions only apply to Experiment 2.

**Problem Story Format:**

**1-minute instructions:** Please imagine and tell a story about a scenario in which you are worrying about this problem or event *[specified by a title]*. Imagine and describe your thoughts and feelings as you are worrying about this event.

**5-minute story prompt:** You would like to *[achieve a goal concerning the worrisome problem]*. The story ends with you *[reaching the positive outcome]*. The story begins with you *[worrying about the problem]*. Provide steps to fill in the middle of the story.

**Sample 5-minute Story:**

You would like to do well on your psychology midterm exam. The story ends with you getting an A on your psychology midterm exam. The story begins with you worrying that you will not do well on your midterm exam. Provide steps to fill in the middle of the story.

**Additional Instructions to Participant:** Please generate as many steps as you can for the entire 5 minutes, and please provide as much detail as possible regarding your thoughts, feelings, and actions as you are executing the steps.
Episodic Reappraisal Materials

See below for episodic reappraisal task format, instructions, and a sample scenario. Italic text surrounded by brackets indicates material that is personalized to each participant. Note that in Experiment 1, the initial scenario prompt was presented for 2 minutes, rather than 1 minute as in Experiment 2.

Scenario Format:

1-minute scenario prompt: Please imagine and tell a story about a scenario in which you just found out [that a bad outcome had just taken place]. Imagine and describe how you might be feeling at that moment.

5-minute reappraisal instructions: Please now imagine and tell a story of how you are able to reinterpret the situation so that it feels less negative to you. Imagine and describe a scenario in which you are doing something to cope with those negative emotions.

Sample 1-minute scenario:

Please imagine and tell a story about a scenario in which you just found out that you did poorly on your psychology midterm exam. Imagine and describe how you might be feeling at that moment.

Additional Instructions to Participant: Please try to write out everything you imagine about the scenario, such as what you are thinking, how you are feeling, and what actions you will be performing in as much detail as possible.
Table 1.1. Experiment 1 means and differences across the specificity and control conditions in step type (relevant vs. other) in the MEPS task, and detail type (internal vs. external) in both the MEPS and episodic reappraisal tasks. All values represent mean steps or details.

<table>
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<tr>
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<th>Control Induction</th>
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<td>Relevant: 16.99 (1.32)</td>
<td>Relevant: 4.25 (.48) ***</td>
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<td>Other: 2.49 (.54)</td>
<td>Other: 1.02 (.36)</td>
<td>Other: -1.47 (.35) ***</td>
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<td>Internal: 65.12 (4.53)</td>
<td>Internal: 13.95 (2.15) ***</td>
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<td></td>
<td>External: 10.73 (2.26)</td>
<td>External: 3.80 (1.27)</td>
<td>External: -6.93 (1.61) ***</td>
</tr>
<tr>
<td><strong>Reappraisal</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Detail Type</td>
<td>Internal: 53.68 (3.58)</td>
<td>Internal: 63.37 (.85)</td>
<td>Internal: 9.69 (2.03) ***</td>
</tr>
<tr>
<td></td>
<td>External: 6.98 (1.31)</td>
<td>External: 9.69 (2.03)</td>
<td>External: -3.94 (1.06) ***</td>
</tr>
</tbody>
</table>

*Note. SE in parentheses. * p < .05. ** p < .01. *** p < .001.
Table 1.2. Experiment 1 mean initial (session 1), post-simulation, (session 2 or 3) and difference (session 2/3 – session 1) in ratings of anxiety, perceived likelihood of a bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of a good outcome for the imagined events in MEPS task. All ratings were made on a 1 to 9 scale. All values represent the mean rating for a given measure.

<table>
<thead>
<tr>
<th></th>
<th>Initial (Session 1)</th>
<th>Post-Simulation (Session 2 or 3)</th>
<th>Difference (Session 2/3 – Session 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Anxiety</strong></td>
<td>Specificity: 7.58 (.10)</td>
<td>Control: 7.25 (.13)</td>
<td>Specificity: 5.42 (.22)</td>
</tr>
<tr>
<td></td>
<td>Control: 5.42 (.22)</td>
<td>Control: 5.60 (.27)</td>
<td>Control: -1.65 (.25) ***</td>
</tr>
<tr>
<td><strong>Likelihood of Bad</strong></td>
<td>Specificity: 5.61 (.17)</td>
<td>Control: 5.40 (.19)</td>
<td>Specificity: 4.35 (.14)</td>
</tr>
<tr>
<td><strong>Outcome</strong></td>
<td>Control: 5.40 (.19)</td>
<td>Control: 4.52 (.21)</td>
<td>Control: -0.88 (.19) ***</td>
</tr>
<tr>
<td><strong>Difficulty to Cope</strong></td>
<td>Specificity: 7.05 (.15)</td>
<td>Control: 6.88 (.16)</td>
<td>Specificity: 5.71 (.20)</td>
</tr>
<tr>
<td></td>
<td>Control: 5.71 (.20)</td>
<td>Control: 5.66 (.20)</td>
<td>Control: -1.35 (.21) ***</td>
</tr>
<tr>
<td><strong>Likelihood of Good</strong></td>
<td>Specificity: 5.72 (.17)</td>
<td>Control: 5.88 (.14)</td>
<td>Specificity: 6.48 (.17)</td>
</tr>
<tr>
<td><strong>Outcome</strong></td>
<td>Control: 5.88 (.14)</td>
<td>Control: 6.20 (.19)</td>
<td>Control: 0.76 (.18) ***</td>
</tr>
</tbody>
</table>

*Note. SE in parentheses. * p < .05. ** p < .01. *** p < .001.*
Table 1.3. Experiment 1 mean initial (session 1), post-simulation (session 2 or 3), and difference (session 2/3 – session 1) in ratings of anxiety, perceived likelihood of a bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of a good outcome for the imagined events in episodic reappraisal task. All ratings were made on a 1 to 9 scale. All values represent the mean rating for a given measure.

<table>
<thead>
<tr>
<th></th>
<th>Initial (Session 1)</th>
<th>Post-Simulation (Session 2 or 3)</th>
<th>Difference (Session 2/3 – Session 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Likelihood of Bad Outcome</td>
<td>Specificity: 5.69 (.18)</td>
<td>Control: 5.23 (.21)</td>
<td>Specificity: 4.90 (.15) Control: 5.06 (.16)</td>
</tr>
<tr>
<td>Difficulty to Cope</td>
<td>Specificity: 6.95 (.14)</td>
<td>Control: 6.75 (.17)</td>
<td>Specificity: 5.76 (.19) Control: 6.29 (.20)</td>
</tr>
<tr>
<td>Likelihood of Good Outcome</td>
<td>Specificity: 5.60 (.15)</td>
<td>Control: 5.86 (.14)</td>
<td>Specificity: 5.68 (.13) Control: 5.66 (.14)</td>
</tr>
</tbody>
</table>

Note. SE in parentheses. * p < .05. ** p < .01. *** p < .001.
Table 1.4. Experiment 1 correlations (N = 25) between the difference (specificity condition – control condition) in the change in internal details and change in pre- and post-simulation ratings of anxiety, perceived likelihood of bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of good outcome.

<table>
<thead>
<tr>
<th></th>
<th>MEPS</th>
<th>95% CI</th>
<th>Episodic Reappraisal</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anxiety</td>
<td>( r(23) = -.346, p = .090 )</td>
<td>([-0.651, 0.056])</td>
<td>( r(23) = -.361, p = .076 )</td>
<td>([-0.661, 0.039])</td>
</tr>
<tr>
<td>Likelihood of Bad Outcome</td>
<td>( r(23) = -.315, p = .125 )</td>
<td>([-0.631, 0.091])</td>
<td>( r(23) = -.354, p = .083 )</td>
<td>([-0.657, 0.047])</td>
</tr>
<tr>
<td>Likelihood of Good Outcome</td>
<td>( r(23) = .298, p = .148 )</td>
<td>([-0.11, 0.62])</td>
<td>( r(23) = .095, p = .653 )</td>
<td>([-0.311, 0.472])</td>
</tr>
<tr>
<td>Difficulty to Cope</td>
<td>( r(23) = -.215, p = .301 )</td>
<td>([-0.562, 0.196])</td>
<td>( r(23) = -.416, p = .039 )</td>
<td>([-0.696, -0.025])</td>
</tr>
</tbody>
</table>
Table 1.5. Experiment 1 correlations (N = 25) between trait anxiety (STAI) and changes in subjective well-being measures. All correlations are collapsed across specificity and control conditions and separated by task.

<table>
<thead>
<tr>
<th></th>
<th>MEPS</th>
<th>95% CI</th>
<th>Episodic Reappraisal</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Anxiety</strong></td>
<td>$r(23) = -.08, p = .69$</td>
<td>[-.46, .33]</td>
<td>$r(23) = -.09, p = .68$</td>
<td>[-.47, .32]</td>
</tr>
<tr>
<td><strong>Likelihood of Bad Outcome</strong></td>
<td>$r(23) = -.12, p = .56$</td>
<td>[-.49, .29]</td>
<td>$r(23) = -.18, p = .38$</td>
<td>[-.54, .23]</td>
</tr>
<tr>
<td><strong>Likelihood of Good Outcome</strong></td>
<td>$r(23) = .07, p = .73$</td>
<td>[-.33, .45]</td>
<td>$r(23) = .11, p = .59$</td>
<td>[-.30, .48]</td>
</tr>
<tr>
<td><strong>Difficulty to Cope</strong></td>
<td>$r(23) = -.01, p = .96$</td>
<td>[-.40, .39]</td>
<td>$r(23) = -.13, p = .53$</td>
<td>[-.50, .28]</td>
</tr>
</tbody>
</table>
Table 1.6. Experiment 2 means and differences across the specificity and control conditions in step type (relevant vs. other) in the MEPS task, and detail type (internal vs. external) in both the MEPS and episodic reappraisal tasks. All values represent mean steps or details.

<table>
<thead>
<tr>
<th></th>
<th>Control Induction</th>
<th>Specificity Induction</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>MEPS</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Step Type</td>
<td>Relevant: 10.31 (.62)</td>
<td>Relevant: 13.91 (.60)</td>
<td>Relevant: 3.60 (.59) ***</td>
</tr>
<tr>
<td></td>
<td>Other: 1.80 (.36)</td>
<td>Other: .77 (.18)</td>
<td>Other: -1.03 (.24) ***</td>
</tr>
<tr>
<td>Detail Type</td>
<td>Internal: 44.34 (3.17)</td>
<td>Internal: 54.02 (3.24)</td>
<td>Internal: 9.68 (2.16) ***</td>
</tr>
<tr>
<td></td>
<td>External: 8.91 (2.31)</td>
<td>External: 3.36 (.81)</td>
<td>External: -5.55 (1.93) **</td>
</tr>
<tr>
<td><strong>Reappraisal</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Detail Type</td>
<td>Internal: 43.82 (2.78)</td>
<td>Internal: 52.39 (3.42)</td>
<td>Internal: 8.57 (1.83) ***</td>
</tr>
<tr>
<td></td>
<td>External: 4.84 (1.31)</td>
<td>External: 2.55 (.65)</td>
<td>External: -2.29 (.94) *</td>
</tr>
</tbody>
</table>

*Note. SE in parentheses. * $p < .05$. ** $p < .01$. *** $p < .001$. 
Table 1.7. Experiment 2 mean initial (session 1), post-simulation (session 2 or 3), and difference (session 2/3 – session 1) in ratings of anxiety, perceived likelihood of a bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of a good outcome for the imagined events in MEPS task. All ratings were made on a 1 to 9 scale. All values represent the mean rating for a given measure.

<table>
<thead>
<tr>
<th></th>
<th>Initial (Session 1)</th>
<th>Post-Simulation (Session 2 or 3)</th>
<th>Difference (Session 2/3 – Session 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Anxiety</strong></td>
<td>Specificity: 7.51 (.12) Control: 7.51 (.11)</td>
<td>Specificity: 5.32 (.23) Control: 5.72 (.27)</td>
<td>Specificity: -2.19 (.18) *** Control: -1.79 (.24) ***</td>
</tr>
<tr>
<td><strong>Likelihood of Bad Outcome</strong></td>
<td>Specificity: 5.58 (.17) Control: 5.60 (.23)</td>
<td>Specificity: 4.11 (.23) Control: 4.46 (.26)</td>
<td>Specificity: -1.47 (.17) *** Control: -1.14 (.15) ***</td>
</tr>
<tr>
<td><strong>Difficulty to Cope</strong></td>
<td>Specificity: 6.71 (.19) Control: 6.91 (.17)</td>
<td>Specificity: 5.26 (.21) Control: 5.21 (.19)</td>
<td>Specificity: -1.45 (.20) *** Control: -1.70 (.19) ***</td>
</tr>
<tr>
<td><strong>Likelihood of Good Outcome</strong></td>
<td>Specificity: 6.27 (.16) Control: 6.29 (.16)</td>
<td>Specificity: 7.08 (.16) Control: 6.76 (.17)</td>
<td>Specificity: 0.81 (.15) *** Control: 0.47 (.14) **</td>
</tr>
</tbody>
</table>

*Note.* SE in parentheses. * p < .05. ** p < .01. *** p < .001.
Table 1.8. Experiment 2 mean initial (session 1), post-simulation (session 2 or 3), and difference (session 2/3 – session 1) in ratings of anxiety, perceived likelihood of a bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of a good outcome for the imagined events in episodic reappraisal task. All ratings were made on a 1 to 9 scale. All values represent the mean rating for a given measure.

<table>
<thead>
<tr>
<th></th>
<th>Initial (Session 1)</th>
<th>Post-Simulation (Session 2 or 3)</th>
<th>Difference (Session 2/3 – Session 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anxiety</td>
<td>Specificity: 7.63 (.12)</td>
<td>Specificity: 5.40 (.23)</td>
<td>Specificity: -2.22 (.19) ***</td>
</tr>
<tr>
<td></td>
<td>Control: 7.68 (.11)</td>
<td>Control: 6.09 (.26)</td>
<td>Control: -1.59 (.22) ***</td>
</tr>
<tr>
<td>Likelihood of Bad Outcome</td>
<td>Specificity: 5.62 (.22)</td>
<td>Specificity: 4.16 (.23)</td>
<td>Specificity: -1.46 (.16) ***</td>
</tr>
<tr>
<td></td>
<td>Control: 5.59 (.24)</td>
<td>Control: 4.78 (.26)</td>
<td>Control: -0.81 (.14) ***</td>
</tr>
<tr>
<td>Difficulty to Cope</td>
<td>Specificity: 6.96 (.18)</td>
<td>Specificity: 5.28 (.23)</td>
<td>Specificity: -1.68 (.17) ***</td>
</tr>
<tr>
<td></td>
<td>Control: 7.14 (.15)</td>
<td>Control: 6.05 (.20)</td>
<td>Control: -1.10 (.17) ***</td>
</tr>
<tr>
<td>Likelihood of Good Outcome</td>
<td>Specificity: 6.41 (.14)</td>
<td>Specificity: 6.13 (.15)</td>
<td>Specificity: -0.28 (.17)</td>
</tr>
<tr>
<td></td>
<td>Control: 6.12 (.19)</td>
<td>Control: 5.94 (.15)</td>
<td>Control: -0.18 (.18)</td>
</tr>
</tbody>
</table>

Note. SE in parentheses. * p < .05. ** p < .01. *** p < .001.
Table 1.9. Experiment 1 correlations (N = 26) between the difference (specificity condition – control condition) in the change in internal details and change in pre- and post-simulation ratings of anxiety, perceived likelihood of bad outcome, perceived difficulty to cope with a bad outcome, and perceived likelihood of good outcome.

<table>
<thead>
<tr>
<th></th>
<th>MEPS</th>
<th>95% CI</th>
<th>Episodic Reappraisal</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anxiety</td>
<td>$r(24) = -0.329, p = .100$</td>
<td>[-0.635, 0.066]</td>
<td>$r(24) = -0.349, p = .081$</td>
<td>[-0.648, 0.044]</td>
</tr>
<tr>
<td>Likelihood of Bad Outcome</td>
<td>$r(24) = -0.285, p = .157$</td>
<td>[-0.605, 0.115]</td>
<td>$r(24) = -0.384, p = .053$</td>
<td>[-0.671, 0.003]</td>
</tr>
<tr>
<td>Likelihood of Good Outcome</td>
<td>$r(24) = 0.300, p = .136$</td>
<td>[-0.098, 0.615]</td>
<td>$r(24) = -0.082, p = .691$</td>
<td>[-0.454, 0.315]</td>
</tr>
<tr>
<td>Difficulty to Cope</td>
<td>$r(24) = -0.174, p = .394$</td>
<td>[-0.525, 0.346]</td>
<td>$r(24) = -0.397, p = .044$</td>
<td>[-0.679, -0.012]</td>
</tr>
</tbody>
</table>
*Table 1.10.* Experiment 2 correlations (N = 26) between trait anxiety (STAI) and changes in subjective well-being measures. All correlations are collapsed across specificity and control conditions, and separated by task.

<table>
<thead>
<tr>
<th></th>
<th>MEPS</th>
<th>95% CI</th>
<th>Episodic Reappraisal</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anxiety</td>
<td>$r(24) = -.11$, $p = .58$</td>
<td>[-.48, .29]</td>
<td>$r(24) = -.15$, $p = .48$</td>
<td>[-.51, .25]</td>
</tr>
<tr>
<td>Likelihood of Bad Outcome</td>
<td>$r(24) = -.12$, $p = .57$</td>
<td>[-.48, .28]</td>
<td>$r(24) = -.12$, $p = .55$</td>
<td>[-.48, .28]</td>
</tr>
<tr>
<td>Likelihood of Good Outcome</td>
<td>$r(24) = -.18$, $p = .38$</td>
<td>[-.53, .22]</td>
<td>$r(24) = -.04$, $p = .83$</td>
<td>[-.42, .35]</td>
</tr>
<tr>
<td>Difficulty to Cope</td>
<td>$r(24) = -.08$, $p = .71$</td>
<td>[-.45, .32]</td>
<td>$r(24) = -.07$, $p = .72$</td>
<td>[-.45, .33]</td>
</tr>
</tbody>
</table>
Paper 2 (Jing, Madore, & Schacter, 2017)

Experiment 1 Standardized Event List

See below for the full list of standardized negative event prompts for Experiment 1. As mentioned in section 2.3.1. of the manuscript (“Initial Event Ratings”), we did not find significant differences in initial ratings of event valence and plausibility across the specificity and control conditions between the two lists.

List 1

1. I gain weight or do not achieve my ideal fitness level.
2. I am fired from my job.
3. I receive a bad grade on my exam.
4. I do poorly at a graduate school interview.
5. My significant other breaks up with me.
6. I am unable to go on a trip or vacation that I planned.

List 2

1. I receive a negative recommendation letter.
2. I give a bad presentation.
3. I am rejected from a job application that I really want.
4. I have a bad fight with a good friend.
5. My laptop crashes and I lose all of my work.
6. I am criticized by my boss/adviser for poor performance.
Specificity of Alternative Events—Data and Analyses

In addition to scoring the data for the number of alternative events that participants generated, we also scored the data for alternative event detail and specificity using the Autobiographical Interview (AI; Levine, Svoboda, Hay, Winocur, & Moscovitch, 2002).

Within a given event simulation, individuals produce two major types of details: “internal” or episodic details and “external” or semantic details (see Levine et al., 2002; Madore et al., 2014). Internal details were segmented as any bits of episodic information contained in the responses (e.g., people, places, actions, objects, thoughts, and feelings about the central event), and external details were segmented as any bits of other information contained in the responses (e.g., semantic, factual information and commentary that is not specific to time and place, off-topic and repetitive information, etc.). In the alternative event generation task, all details contained within irrelevant alternative events were deemed external details, but relevant alternative events could contain both internal and external details.

Two raters who were blind to the condition (control, specificity) of the alternative event narratives separately scored 15 participant practice trial responses to assess inter-rater reliability. High inter-rater reliability was obtained for both internal and external details (standardized Cronbach’s α = .97 and α = .98, respectively).

In Experiment 1, we conducted a 2 (Induction: control vs. specificity) x 2 (Detail type: internal vs. external) repeated-measures ANOVA. We found significant main effects of Induction, $F(1,25) = 10.68, p = .003, \eta^2_p = .30$, and Detail type, $F(1,25) = 209.72, p < .001, \eta^2_p = .89$, but most importantly, we found a significant interaction of Induction x Detail Type, $F(1,25) = 17.32, p < .001, \eta^2_p = .41$. Two-tailed post hoc t-tests showed that participants generated significantly more internal details per event trial, $t(25) = 4.24, p < .001, 95\% \text{ CI} = [5.88, 17.002], d = 0.83$, in the
specificity condition compared to the control condition ($M_{control} = 49.62, SE = 3.32; M_{specificity} = 61.06, SE = 4.39$). There was no significant difference in the number of external details generated in the specificity and control conditions, $t(25) = -1.70, p = .101, 95\% \text{ CI} = [-5.40, .51], d = 0.33$ ($M_{control} = 4.56, SE = 1.25; M_{specificity} = 2.12, SE = .64$). See Supplemental Figure 1A below.

In Experiment 2, we similarly found significant main effects of Induction, $F(1,28) = 5.73, p = .024, \eta_p^2 = .17$, and Detail type, $F(1,28) = 121.84, p < .001, \eta_p^2 = .81$, but critically, there was a significant interaction of Induction x Detail Type, $F(1,28) = 17.31, p < .001, \eta_p^2 = .38$. Participants generated significantly more internal details per event trial, $t(28) = 4.51, p < .001, 95\% \text{ CI} = [4.91, 13.10], d = 0.84$, in the specificity condition compared to the control condition ($M_{control} = 35.50, SE = 3.38; M_{specificity} = 44.51, SE = 3.86$). There was also a significant decrease in external details generated in the specificity relative to the control condition, $t(28) = -2.35, p = .026, 95\% \text{ CI} = [-7.70, -.53], d = 0.44$ ($M_{control} = 5.32, SE = 1.55; M_{specificity} = 1.21, SE = .53$). See Supplemental Figure 1B below.

Thus, the induction increased the level of internal detail in participants’ descriptions of alternative future events, which replicates existing studies that examine the effect of the episodic specificity induction on other tasks thought to engage the episodic memory system, such as recalling past and imagining future experiences (Madore et al., 2014) and problem solving (Jing et al., 2016; Madore & Schacter, 2014).
Figure 2.1. Mean induction effects on the number of internal and external details generated per event trial in control and specificity conditions in (A) Experiment 1 and (B) Experiment 2. Error bars represent one standard error of the mean.
Perceived Plausibility of Alternative Events

Prior work (e.g., Hirt & Markman, 1995) has shown that the debiasing effects observed when participants consider alternative outcomes is influenced by the perceived plausibility of the generated outcomes. That is, the more plausible the alternative outcomes that are generated, the less plausible the original negative event.

In the current experiments, we observed a negative correlation between the perceived plausibility of the alternatives generated (i.e., mean difference score between the perceived plausibility of the alternative positive events vs. the original negative events) and changes in the perceived plausibility of the original event (i.e., mean difference score between plausibility ratings of the original event before and after alternative event generation). We found these negative correlations in both Experiment 1, $r(24) = -0.393, p = .047$, and in Experiment 2, $r(27) = -0.365, p = .052$. Although the latter was only trending, these data indeed seem to be consistent with the results of Hirt and Markman (1995), suggesting that the more plausible the generated alternative outcomes, the larger the reduction in the perceived plausibility of the original event.
Table 2.1. Experiment 1 mean values and standard errors in the control and specificity conditions for the number of alternatives that participants generated (relevant vs. irrelevant) and ratings of perceived difficulty to generate alternatives, realistic quality of the alternatives, event valence (before vs. after generating alternatives), and event plausibility (before vs. after generating alternatives).

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control Condition</th>
<th>Specificity Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SE</td>
</tr>
<tr>
<td># Relevant Alternatives</td>
<td>5.17</td>
<td>0.36</td>
</tr>
<tr>
<td># Irrelevant Alternatives</td>
<td>0.45</td>
<td>0.12</td>
</tr>
<tr>
<td>Difficulty Rating</td>
<td>5.10</td>
<td>0.24</td>
</tr>
<tr>
<td>Realistic Rating</td>
<td>5.96</td>
<td>0.17</td>
</tr>
<tr>
<td>Valence Rating (before generating alternatives)</td>
<td>2.42</td>
<td>0.10</td>
</tr>
<tr>
<td>Valence Rating (after generating alternatives)</td>
<td>3.26</td>
<td>0.14</td>
</tr>
<tr>
<td>Plausibility Rating (before generating alternatives)</td>
<td>4.88</td>
<td>0.27</td>
</tr>
<tr>
<td>Plausibility Rating (after generating alternatives)</td>
<td>4.24</td>
<td>0.30</td>
</tr>
</tbody>
</table>
Table 2.2. Experiment 2 mean values and standard errors in the control and specificity conditions for the number of alternatives that participants generated (relevant vs. irrelevant) and ratings of perceived difficulty to generate alternatives, realistic quality of the alternatives, event valence (before vs. after generating alternatives), and event plausibility (before vs. after generating alternatives).

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control Condition</th>
<th>Specificity Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SE</td>
</tr>
<tr>
<td># Relevant Alternatives</td>
<td>4.77</td>
<td>0.22</td>
</tr>
<tr>
<td># Irrelevant Alternatives</td>
<td>0.74</td>
<td>0.21</td>
</tr>
<tr>
<td>Difficulty Rating</td>
<td>5.32</td>
<td>0.24</td>
</tr>
<tr>
<td>Realistic Rating</td>
<td>5.90</td>
<td>0.15</td>
</tr>
<tr>
<td>Valence Rating (before generating alternatives)</td>
<td>2.18</td>
<td>0.11</td>
</tr>
<tr>
<td>Valence Rating (after generating alternatives)</td>
<td>3.17</td>
<td>0.24</td>
</tr>
<tr>
<td>Plausibility Rating (before generating alternatives)</td>
<td>5.50</td>
<td>0.16</td>
</tr>
<tr>
<td>Plausibility Rating (after generating alternatives)</td>
<td>4.86</td>
<td>0.21</td>
</tr>
</tbody>
</table>
Standardized Means-End Problem Solving Prompts

See below for the complete list of standardized worrisome event prompts used in Experiments 1 and 2. List order was counterbalanced both between induction/task condition and between participants.

List 1
Practice event:
Title: Bad winter weather makes it hard to get around
Prompt: You would like to make sure you are able to get around despite the bad weather this winter. The story begins with you worrying about how the bad winter weather is making it hard for you to get around. The story ends with you successfully finding ways to get around despite the bad weather.

Event 1:
Title: Doing household chores on your own without help
Prompt: You would like to find ways to still complete household chores on your own without the help of others. The story begins with you worrying about not being physically capable of doing household chores on your own without the help of others. The story ends with you successfully managing to do household chores independently.

Event 2:
Title: Meeting with friends
Prompt: You would like to continue to stay in touch and meet with your friends. The story begins with you worrying about not being capable of continuing to meet up with your friends and staying in touch with them. The story ends with you successfully managing to stay in touch with and meet up with your friends regularly.

Event 3:
Title: Keeping up your hobby
Prompt: You would like to continue engaging in your favorite hobby. The story begins with you worrying that you will no longer be able to continue engaging in your favorite
hobby. The story ends with you successfully finding a way to continually engage in your hobby.

Event 4:

Title: Conflict with a close friend
Prompt: You would like to successfully navigate a situation of conflict with a close friend. The story begins with you worrying about having an unpleasant conflict with your close friend. The story ends with you successfully resolving the conflict with your friend.

Event 5:

Title: Making sure to pay bills on time
Prompt: You would like to make sure that you remember to pay your bills on time. The story begins with you worrying that you will forget to pay your bills on time. The story ends with you successfully finding ways to remember to pay your bills on time.

Event 6:

Title: Staying mentally active
Prompt: You would like to continue to stay mentally active. The story begins with you worrying about your ability to continue to stay mentally active. The story ends with you successfully finding ways to stay mentally active.

List 2

Practice event:

Title: Being able to use transportation on your own
Prompt: You would like to continue to be able to use transportation on your own, without the help of others. The story begins with you worrying about no longer being physically capable of using transportation on your own. The story ends with you successfully continuing to use transportation on your own.

Event 1:

Title: Conflict with a neighbor
Prompt: You would like to be able to avoid or successfully navigate a situation of conflict with a neighbor. The story begins with you worrying about experiencing an unpleasant conflict with your neighbor. The story ends with you either successfully avoiding or resolving conflict with your neighbor.
Event 2:
Title: Budgeting money to prevent overspending
Prompt: You would like to budget your money wisely to prevent yourself from overspending the money you have saved for retirement. The story begins with you worrying that you are overspending the money you saved for retirement, and that it won't last as long as you need it to. The story ends with you successfully budgeting your money to prevent yourself from overspending.

Event 3:
Title: Not remembering to keep appointments
Prompt: You would like to make sure that you remember to keep your appointments. The story begins with you worrying that you will forget to keep your appointments. The story ends with you successfully keeping all of your appointments.

Event 4:
Title: Taking better care of your health
Prompt: You would like to take better care of your health. The story begins with you worrying that your health will decline and that you are not taking good enough care of yourself. The story ends with you successfully taking better care of your health.

Event 5:
Title: Staying in touch with or visiting family
Prompt: You would like to stay in touch with and visit your family more often. The story begins with you worrying about not being able to stay in touch with or visiting your family as frequently as you used to. The story ends with you successfully staying in touch with and visiting your family regularly.

Event 6:
Title: Attending senior social activities
Prompt: You would like to continue to attend senior social activities. The story begins with you worrying that you will no longer be able to attend senior social activities. The story ends with you successfully finding a way to regularly attend senior social activities.
Table 3.1. Experiment 1 main effects and interactions for the 2 (Induction condition: control vs. specificity) x 2 (Step type: relevant vs. other; Detail type: internal vs. external; Time of rating: Before vs. after MEPS task) repeated measures ANOVAs.

<table>
<thead>
<tr>
<th>Induction Condition</th>
<th>Step Type / Detail Type / Time of Rating Main Effect</th>
<th>Interaction with Induction Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Relevant vs. Other Steps</td>
<td>$F(1,25)=13.74, p=.001, \eta^2=.36$</td>
<td>$F(1,25)=289.70, p&lt;.001, \eta^2=.92$</td>
</tr>
<tr>
<td>Internal vs. External Details</td>
<td>$F(1,25)=1.26, p=.272, \eta^2=.05$</td>
<td>$F(1,25)=195.87, p&lt;.001, \eta^2=.89$</td>
</tr>
<tr>
<td>Anxiety Rating</td>
<td>$F(1,25)=.99, p=.33, \eta^2=.04$</td>
<td>$F(1,25)=66.26, p&lt;.001, \eta^2=.73$</td>
</tr>
<tr>
<td>Likelihood Good Outcome Rating</td>
<td>$F(1,25)=.37, p=.551, \eta^2=.01$</td>
<td>$F(1,25)=30.75, p&lt;.001, \eta^2=.55$</td>
</tr>
<tr>
<td>Likelihood Bad Outcome Rating</td>
<td>$F(1,25)=.18, p=.677, \eta^2=.01$</td>
<td>$F(1,25)=58.86, p&lt;.001, \eta^2=.70$</td>
</tr>
<tr>
<td>Difficulty to Cope Rating</td>
<td>$F(1,25)=.11, p=.748, \eta^2=.004$</td>
<td>$F(1,25)=51.12, p&lt;.001, \eta^2=.67$</td>
</tr>
</tbody>
</table>
Table 3.2. Experiment 1 mean values for step type (relevant vs. other), detail type (internal vs. external), and initial vs. post-MEPS ratings (on 1 to 9 scale) of anxiety, perceived likelihood of a good outcome, perceived likelihood of a bad outcome, and perceived difficulty to cope with a bad outcome.

<table>
<thead>
<tr>
<th></th>
<th>Control Condition</th>
<th></th>
<th>Specificity Condition</th>
<th></th>
<th>Induction Difference (Specificity - Control)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Before MEPS</td>
<td>After MEPS</td>
<td>Control Difference (After MEPS – Before MEPS)</td>
<td>Before MEPS</td>
<td>After MEPS</td>
</tr>
<tr>
<td># Relevant Steps</td>
<td>-</td>
<td>8.31 (.43)</td>
<td>-</td>
<td>-</td>
<td>10.21 (.44)</td>
</tr>
<tr>
<td># Other Steps</td>
<td>-</td>
<td>1.69 (.25)</td>
<td>-</td>
<td>-</td>
<td>0.94 (.20)</td>
</tr>
<tr>
<td># Internal Details</td>
<td>-</td>
<td>30.3 (1.41)</td>
<td>-</td>
<td>-</td>
<td>34.94 (1.52)</td>
</tr>
<tr>
<td># External Details</td>
<td>-</td>
<td>7.78 (1.18)</td>
<td>-</td>
<td>-</td>
<td>4.2 (.83)</td>
</tr>
<tr>
<td>Anxiety Rating</td>
<td>5.45 (.32)</td>
<td>3.82 (.23)</td>
<td>-1.63 (.23)***</td>
<td>5.33 (.32)</td>
<td>3.66 (.24)</td>
</tr>
<tr>
<td>Likelihood Good Outcome Rating</td>
<td>5.76 (.25)</td>
<td>6.92 (.18)</td>
<td>1.17 (.26)**</td>
<td>5.56 (.29)</td>
<td>6.84 (.25)</td>
</tr>
<tr>
<td>Likelihood Bad Outcome Rating</td>
<td>5.15 (.27)</td>
<td>3.71 (.25)</td>
<td>-1.44 (.22)**</td>
<td>5.14 (.31)</td>
<td>3.56 (.29)</td>
</tr>
<tr>
<td>Difficulty to Cope Rating</td>
<td>5.44 (.26)</td>
<td>4.25 (.21)</td>
<td>-1.19 (.17)**</td>
<td>5.37 (.29)</td>
<td>4.19 (.29)</td>
</tr>
</tbody>
</table>

Note. SE in parentheses. * p < .05. ** p < .01. *** p < .001.
Table 3.3. Experiment 2 modified Autobiographical Interview (AI) scoring, where internal details were segregated across 5 primary categories: (1) references to specific individuals, (2) locations, (3) objects/nouns, (4) actions, and (5) time descriptors. Specific examples received an additional point (i.e., worth 2 points instead of 1 point as an internal detail).

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>General category</strong></td>
<td><strong>Specific examples</strong></td>
<td><strong>General category</strong></td>
<td><strong>Specific examples</strong></td>
<td><strong>General category</strong></td>
</tr>
<tr>
<td>Professional counsel/assistance</td>
<td>Social worker, paralegal, health aide, housekeeper, building manager, gym trainer, nutritionist, etc.</td>
<td>Stores</td>
<td>Proper names of stores/services (Star Market, Walmart, Best Buy, Peapod, Instacart, CVS, Rite Aid), bank, post office, etc.</td>
<td>Method of contact/technology</td>
</tr>
<tr>
<td>Health professionals</td>
<td>Occupational therapist, cardiologist, dentist, vet, other medical specialists, etc.</td>
<td>Home-related or outdoors</td>
<td>Kitchen counter, dining table, bathtub, fireplace, deck/patio, driveway, bus stop, etc.</td>
<td>Mode of transportation</td>
</tr>
<tr>
<td>Family members</td>
<td>Husband/wife, daughter/son, grandchild, sister, cousin, etc.</td>
<td>Hobby- or Social-related</td>
<td>Senior center, YMCA, book club, luncheon, library, lectures, chess club, fitness club, church/mass, golf club, park, museum</td>
<td>Health-related tools</td>
</tr>
<tr>
<td>Friends</td>
<td>Best friend, next door neighbor, any proper names, etc.</td>
<td>Living/housing-related</td>
<td>Assisted living, nursing home, condo, hotel</td>
<td>Hobby-related objects</td>
</tr>
</tbody>
</table>
Table 3.4. Experiment 2 main effects and interactions for the 2 (Task: personal MEPS vs. advice) x 2 (Step type: relevant vs. other; Detail type: internal vs. external; Time of rating: Before vs. after task) repeated measures ANOVAs.

<table>
<thead>
<tr>
<th>Task Condition</th>
<th>Step Type/Detail Type/Time of Rating Main Effect</th>
<th>Interaction with Task Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Relevant vs. Other Steps</td>
<td>$F(1,31)=56.40, p&lt;.001, \eta^2=.65$</td>
<td>$F(1,31)=51.72, p&lt;.001, \eta^2=.63$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=123.53, p&lt;.001, \eta^2=.80$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=51.72, p&lt;.001, \eta^2=.63$</td>
<td></td>
</tr>
<tr>
<td>Internal vs. External Details</td>
<td>$F(1,31)=18.69, p&lt;.001, \eta^2=.38$</td>
<td>$F(1,31)=32.29, p&lt;.001, \eta^2=.51$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=174.95, p&lt;.001, \eta^2=.85$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=32.29, p&lt;.001, \eta^2=.51$</td>
<td></td>
</tr>
<tr>
<td>Anxiety Rating</td>
<td>$F(1,31)=2.79, p=.105, \eta^2=.08$</td>
<td>$F(1,31)=41.33, p&lt;.001, \eta^2=.57$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=80.33, p&lt;.001, \eta^2=.72$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=41.33, p&lt;.001, \eta^2=.57$</td>
<td></td>
</tr>
<tr>
<td>Likelihood Good Outcome Rating</td>
<td>$F(1,31)=.16, p=.695, \eta^2=.01$</td>
<td>$F(1,31)=10.31, p&lt;.01, \eta^2=.25$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=78.69, p&lt;.001, \eta^2=.72$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=10.31, p&lt;.01, \eta^2=.25$</td>
<td></td>
</tr>
<tr>
<td>Likelihood Bad Outcome Rating</td>
<td>$F(1,31)=.02, p=.886, \eta^2=.001$</td>
<td>$F(1,31)=10.70, p&lt;.01, \eta^2=.26$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=99.71, p&lt;.001, \eta^2=.76$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=10.70, p&lt;.01, \eta^2=.26$</td>
<td></td>
</tr>
<tr>
<td>Difficulty to Cope Rating</td>
<td>$F(1,31)=.24, p=.629, \eta^2=.01$</td>
<td>$F(1,31)=14.10, p=.001, \eta^2=.31$</td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=130.29, p&lt;.001, \eta^2=.81$</td>
<td></td>
</tr>
<tr>
<td></td>
<td>$F(1,31)=14.10, p=.001, \eta^2=.31$</td>
<td></td>
</tr>
</tbody>
</table>
Table 3.5. Experiment 2 mean values for step type (relevant vs. other), detail type (traditional internal, modified internal, and external), and initial vs. post-task ratings (on 1 to 9 scale) of anxiety, perceived likelihood of a good outcome, perceived likelihood of a bad outcome, and perceived difficulty to cope with a bad outcome.

<table>
<thead>
<tr>
<th></th>
<th>Personal MEPS Task</th>
<th>Advice Task</th>
<th>Task Difference (Personal MEPS - Advice)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Before Task</td>
<td>After Task</td>
<td>Personal MEPS Difference (After Task – Before Task)</td>
</tr>
<tr>
<td># Relevant Steps</td>
<td>-</td>
<td>8.60 (.56)</td>
<td>-</td>
</tr>
<tr>
<td># Other Steps</td>
<td>-</td>
<td>0.84 (.15)</td>
<td>-</td>
</tr>
<tr>
<td># Internal Details</td>
<td>-</td>
<td>29.06 (1.35)</td>
<td>-</td>
</tr>
<tr>
<td># External Details</td>
<td>-</td>
<td>3.51 (.58)</td>
<td>-</td>
</tr>
<tr>
<td># Modified Internal Details</td>
<td>-</td>
<td>34.04 (1.68)</td>
<td>-</td>
</tr>
<tr>
<td>Anxiety Rating</td>
<td>5.52 (.17)</td>
<td>3.78 (.15)</td>
<td>-1.73 (.18)***</td>
</tr>
<tr>
<td>Likelihood Good Outcome Rating</td>
<td>4.90 (.22)</td>
<td>6.71 (.17)</td>
<td>1.80 (.21)***</td>
</tr>
<tr>
<td>Likelihood Bad Outcome Rating</td>
<td>5.65 (.20)</td>
<td>3.80 (.22)</td>
<td>-1.85 (.22)***</td>
</tr>
<tr>
<td>Difficulty to Cope Rating</td>
<td>5.84 (.15)</td>
<td>4.27 (.20)</td>
<td>-1.57 (.15)***</td>
</tr>
</tbody>
</table>

*Note. SE in parentheses. * $p < .05$. ** $p < .01$. *** $p < .001$. 
Modified AI Scoring Results for Experiment 1

The number of internal details in Exp 1 scored via the modified AI protocol are as follows:

Control condition: $M = 34.24$ internal details, $SE = 1.58$

Specificity condition: $M = 38.72$ internal details, $SE = 1.68$

Boost in modified internal details (specificity condition – control condition): $M_{\text{difference}} = 4.48$, $SE = .83$

The boost observed in Exp 1 when using the modified AI scoring protocol is similar to the boost we observed and reported using the regular AI protocol (i.e., 4.64 details). Thus, it seems there is a similar quantity of the most specific type of internal details in both the control and specificity conditions. The specificity induction is not preferentially boosting the quantity of the most specific internal details as assessed via modified AI scoring.