More Than a Feeling: How Affective Forecasts and Threat Breadth Contribute to Self-Affirmation Effects

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Abstract

This dissertation examines the relationship between self-affirmation interventions and two previously-unexplored factors: affective forecasts and threat breadth. I hypothesize that, in general, affirmed individuals will make more moderate (less catastrophic) affective forecasts about self-threats compared to non-affirmed people. I also predict an interaction with threat breadth, such that affirmed participants who perceive a narrow threat will show traditional affirmation effects while affirmed participants who perceive a broad threat will demonstrate undesirable "backfiring" effects, such as providing more extreme affective forecasts, being more defensive, and underperforming compared to control participants. Study 1 begins to explore the link between affirmation and affective forecasting, while Studies 2 through 4 examine the interactive effects of affirmation and threat breadth on various outcomes including affective forecasting (all studies), reactions to threatening information (Study 3), and performance on a difficult creativity test (Study 4). In each of the reported studies, I find evidence for threat breadth as a moderator of affirmation's effects, with broad threats producing a backfiring effect, while the relationship between affirmation and affective forecasting appears to be less straightforward.

Keywords: self-affirmation, affective forecasting, threat, breadth, moderation

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When faced with information or a situation that threatens their sense of self, people are often motivated to reduce the uncomfortable feelings they experience. Many everyday situations can create this need to resolve identity-threatening emotions, such as performing poorly on a test, learning about evidence that conflicts with one's current beliefs, or failing to live up to personal standards and values. How do people deal with the unpleasant emotions and thoughts that arise after self-threatening experiences like these? And is there any way for individuals to reduce their anxiety without ignoring possibly useful information about their mistakes or weaknesses? One answer to such questions is offered by self-affirmation theory, which proposes that people are able to negate the distress caused by self-threats by reminding themselves of important traits or values they possess that are unrelated to the threat.

As an illustration of self-affirmation theory, consider the example of an environmentalist who finds herself throwing several recyclable items in the trash for convenience. While she is throwing away the items, she is seen by a friend who knows about her beliefs and gives her a hard time for failing to recycle. Afterward, she feels embarrassed, angry with herself, and worried about her reputation as an environmentalist. What can she do next to resolve all these negative feelings and move past her hypocritical behavior?

One thing she might do is start to question the ultimate importance of recycling. She could try to convince herself that recycling all the time isn't really that helpful to the environment or maybe even that the environment is not as important to her as she previously thought. In fact, there is a psychological theory—cognitive dissonance theory—that predicts exactly these kinds of responses (Festinger, 1957). Unfortunately for the environmentalist,

cognitive dissonance theory predicts that in order to feel better she will most likely have to deal with the threat to her self-esteem directly, by changing her attitudes in the domain that was threatened—in this case, her environmental beliefs.

Self-affirmation theory, on the other hand, predicts that the environmentalist can restore positive feelings about herself without changing her attitudes or denying the importance of her inconsistent behavior (Steele, 1988). Instead, self-affirmation theory suggests that she should focus on something else that she is good at or that is important to her, such as her career accomplishments or her generosity or her relationship with her family. Reminding herself that there are other important things in her life besides her environmental beliefs should help her not only to get past her negative emotions but also to accept that the criticism from her friend was justified and try to figure out how to change her future behavior to be more in line with her values.

Although the effects of self-affirmation may seem less intuitive than classic dissonance theory responses to self-threat, there is now a wealth of research evidence supporting self-affirmation's efficacy. Early studies suggested, for instance, that participants who are given a chance to affirm a self-relevant value unrelated to a threat (Liu & Steele, 1986; Steele & Liu, 1983) or to demonstrate that they are generally good people via helping behavior (Steele & Liu, 1981) show less attitude change following dissonance induction than those who are not given a chance to self-affirm. Since the publication of these preliminary findings, researchers have repeatedly used self-affirmation to obtain powerful effects ranging from improved academic performance (e.g., Cohen, Garcia, Apfel, & Master, 2006) to increased consumption of produce (Epton & Harris, 2008). Studies have yielded positive outcomes for affirmation manipulations across a variety of operationalizations (McQueen & Klein, 2006) and in numerous important

domains (see Cohen & Sherman, 2014 for review) such as health (see Harris & Epton, 2009 for review), social belonging (Burson, Crocker, & Mischkowski, 2012; Cook, Purdie-Vaughns, Garcia, & Cohen, 2012; Jaremka, Bunyan, Collins, & Sherman, 2011; Knowles, Lucas, Molden, Gardner, & Dean, 2010; Stinson, Logel, Shepherd, & Zanna, 2011), motivation (Brunstein & Gollwitzer, 1996; Sivanathan, Molden, Galinsky, & Ku, 2008; Trope & Pomerantz, 1998; Vohs, Park, & Schmeichel, 2012), and self-control (Burson, Crocker, & Mischkowski, 2012; Fujita, Trope, Liberman, & Levin-Sagi, 2006; Schmeichel, & Vohs, 2009).

An issue that remains unresolved in the research summarized above, however, is the precise mechanism by which individuals are able to reduce anxiety by affirming important traits or values. Early theorists speculated that affirmation operated primarily by directly boosting self-esteem (Steele, 1988; Tesser, 2000; Tesser & Cornell, 1991; Tesser, Martin, & Cornell, 1996), which allowed individuals to maintain their sense of integrity and worth in the face of self-threats. Subsequent research, however, has found inconsistent effects of affirmation on self-esteem measures and most studies that include explicit measures of self-esteem fail to find a relationship between affirmation and reported self-esteem (e.g., Galinsky, Stone, & Cooper, 2000; Harris & Napper, 2005; Schmeichel & Martens, 2005). In response to the limited evidence for a self-enhancement explanation for affirmation's effects, a few researchers have recently begun to explore the possibility that affirmation might have less impact on self-esteem and instead might operate through its ability to boost feelings of self-connectedness, belonging, and other-directed positive emotion (Burson, Crocker, & Mischkowski, 2012; Crocker, Niiya, & Mischkowski, 2008; Kumashiro & Sedikides, 2005; Shnabel et al, 2013).

Whether it boosts self-esteem or leads to a self-connectedness perspective, however, it is uncertain how these changes in self-perception might produce the observed effects of

affirmation. That is, what are the physiological or psychological consequences of having higher self-esteem or feeling more connected to others that allow a person to experience reduced anxiety, decreased defensiveness, and improved performance in the face of a threatening situation? Several possible mechanisms have been proposed, including the experience of positive affect, reduced physiological stress and reactivity, and cognitive construal level (see Sherman & Hartson, 2011 for a review). In the present research, however, I explore an alternative mechanism: affective forecasting. If the affirmation process is able to correct errors and biases in expectations that are experienced by people who face a self-threat, perhaps this improved outlook could be an important mediator or moderator of self-affirmation's powerful effects. Or, if they do not directly relate to affirmation's mechanism, affective forecasts could prove to be a valuable new outcome measure for evaluating the efficacy of affirmation manipulations.

The way that individuals respond to a threatening situation, and even whether a situation is perceived as threatening to begin with, is highly related to their predictions about what is likely to happen and how they will feel. Affective forecasting research has shown that people make systematic errors in their predictions about their future emotions, such as paying too much attention to the target event to the exclusion of other important factors—known as focalism—when predicting how they will feel in the future (Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000). Because people focus too much on the impact that one future event will have on their lives and neglect to consider how much other events will impinge on their well-being, focalism has been shown to cause misprediction (particularly exaggeration) of future emotional states (e.g., Wilson & Gilbert, 2005). This could in turn result in both defensiveness and poor performance under stress (classic targets of affirmation). For instance, if people overestimate

how upset they will feel after a poor test performance, and focus excessively on the severity of the negative outcome, they might become overly distressed during the testing situation and perform poorly. If affirmation defocalizes people by making them more aware of the aspects of their life that are unrelated to the area of threat and highlights the importance of those areas to their identity, this could cause affirmed individuals to take these other factors into account when considering the potential impact of a threatening event like failing a test, thus moderating the dire expectations focalism tends to produce and allowing individuals to be less anxious and perform better.

Additionally, affirmation could impact affective forecasting by making individuals more aware of the resources they have that will allow them to recover from a threat (that is, their psychological immune system). Immune neglect, the tendency to discount the beneficial effects of the psychological immune system on emotional adaptation to events, has been identified as another substantial contributor to inaccurate affective forecasts (Gilbert et al., 1998). In particular, immune neglect causes individuals to predict that their emotional reactions to a threatening or negative event will continue to negatively impact them for a long time. Usually, however, the experience of intense emotions leads to the activation of a system of processes that work to return an individual to his or her emotional set point or baseline. This could include processes such as discounting the importance of the event and rationalizing (traditional dissonance reduction techniques) as well as strategies like seeking out social interactions from supportive others or engaging in self-affirming activities. Altogether, these mental reactions and behavioral responses work to reduce the intense emotional response more quickly than individuals typically expect (e.g., Hoerger, 2012). Although Gilbert and colleagues (1998) argue that the psychological immune system should function best when individuals are not consciously

aware of its operation, it seems plausible that self-affirmation makes people more aware that they have resources and can be resilient to threats without necessarily hindering the operation of the immune system. If affirmation allows individuals to anticipate that they will recover from a threat without impairing the psychological immune system's functioning, this too could make affirmed individuals more realistic about the potential impact of negative events.

By reducing focalism and immune neglect—that is, by making people aware of the many other determinants of future happiness and the resources they will have to combat negative emotions, self-affirmation has the potential to moderate affective forecasts and thereby reduce the distress associated with the threatening situation (see Figure 1). Such an explanation is also consistent with the research findings that affirmed participants tend to be more objective (Steele & Liu, 1983) and calibrated (Griffin & Harris, 2011) in their responses. That is, affirmed individuals are less likely to evaluate threatening information negatively purely due to defensiveness and are more likely to respond to threatening data appropriately depending on its self-relevance.

[Figure 1 about here]

Being less defensive and more aware of potential resources, however, might not always mean that one is calmer or more self-assured in the face of a threat. Surely there are times when greater openness to threatening information could lead individuals to be more alarmed or worried as they correctly grasp the magnitude of the threat or even to exaggerate its potential impact. Although this response may be unlikely for the kinds of narrowly-focused threats targeted by a typical affirmation intervention, the potential for a backfiring effect seems clear and has even been observed in the published literature on occasion (Blanton, Cooper, Skurnik, & Aronson,

1997). While the published literature on self-affirmation tends to represent affirmation as a simple, universal, and relatively infallible solution to a wide range of social and personal problems, it is important to remember that the published studies on this topic represent a targeted subset of the possible affirmation interventions that could be attempted. Sherman and Hartson (2011) point out that, although there is little published literature on the potential negative consequences of affirmation, there is certainly potential for affirmation interventions to reduce motivation or hinder performance in certain situations.

It is not inherent to the theory of self-affirmation that the effects of affirmation have positive consequences; affirmations are always expected to boost self-resources, but sometimes increased self-resources may lead to complacency, overconfidence, or reduced commitment. Vohs, Park, and Schmeichel (2012), for example, found that affirmed participants reported greater willingness to give up on a life goal and, after experiencing failure, reported less motivation and were less persistent compared to control participants. Assuming affirmation does impact predictions about future emotional states, this de-motivating effect could also be predicted in light of the affective forecasting literature. Although I speculated earlier that more moderate affective forecasts among affirmed participants will improve performance and motivation by reducing anxiety, other researchers have suggested that affective forecasting biases are actually beneficial in boosting motivation (Morewedge & Buechel, 2013). If this is the case, then an observed effect of affirmation making affective forecasts less extreme might not always predict desirable outcomes.

On the other hand, affirmations could also backfire if they inadvertently cause individuals to perceive the self-threat as more catastrophic than they would have if they remained unaffirmed. McQueen and Klein (2006), for instance, present some evidence that affirmations

can backfire when the affirmed domain is relevant to the threat (e.g., Blanton, Cooper, Skurnik, & Aronson, 1997). That is, affirming a value or trait that it somehow linked to the threat itself might lead to even greater defensiveness or worse performance. In a situation where there is a lot of overlap between the affirmed resource and the threat, or where the threat is broad enough that it could negatively impact the affirmed resource, it seems likely that affirmation would actually lead to increased anxiety and more extreme affective forecasts. For example, if a student is faced with the possibility of poor performance on a statistics exam, he or she might spontaneously self-affirm by thinking about positive past performance on evaluations in other academic subjects. On the one hand, this kind of affirmation could restore a positive self-perception and help the student to regain confidence, but alternatively it could enhance the perceived importance of academic performance to the student's self-worth and make the student even more worried about his or her performance on the threatening test. Unfortunately, the boundaries of affirmation's effectiveness are not well understood. The present research begins to address this gap in the literature by examining a potentially important moderator of affirmation's effects.

Hypotheses

The present research will test hypotheses regarding the mechanisms and boundary conditions of self-affirmation. In my studies, I have begun to examine the relationship between affirmation and affective forecasting, giving additional attention to possible interactions with the breadth of the self-threat. Although the published literature offers only hints of what results might be expected for this line of research, I predicted that, in general, affirmed individuals would make more moderate (less catastrophic) forecasting predictions about self-threats compared to non-affirmed people. Although the prior literature has given little attention to the

effect of affirmation on perception of positive events, I expected that affirmation would also reduce the intensity of predictions for non-threatening events. If the process of affirming leads people to be more aware of the other important factors in their lives beyond the forecasted event, as I hypothesized, this should lead to more accurate (which generally means more moderate) forecasts regardless of whether the forecasted event is positive or negative in valence.

In light of the results for Study 1, I also developed additional hypotheses related to threat breadth which I tested in Studies 2, 3, and 4. Specifically, I predicted that affirmation may backfire when the threat is broad, leading to increased anxiety or defensiveness. In terms of affective forecasting, I expected this backfiring effect to cause more extreme forecasts. That is, I expected that affirmed participants who are presented with a narrow threat will continue to make more moderate forecasts compared to control participants, but affirmed participants who are asked to rate a broad threat will actually show the opposite pattern and make more extreme forecasts than control participants.

In terms of outcome measures that are typically examined in affirmation research, I predicted the same interactive effect for reactions to threatening information and performance on a difficult test. Specifically, I expected that affirmed participants who received a narrow threat would be less defensive about threatening information and perform better than non-affirmed participants (as observed in most of the published affirmation literature), while affirmed participants who received a broad threat would instead be more defensive about threatening information and perform worse than control participants. This hypothesized interaction was tested in Studies 3 and 4.

The Present Studies

Researchers have attempted affirmation interventions in a variety of contexts with a number of distinct dependent variables, but the fundamental paradigm is often quite similar across studies. For the following studies, I employed a standard values affirmation paradigm modified for web administration using the survey platform Qualtrics. After being recruited to participate in a psychology study through Amazon's Mechanical Turk website, participants completed a brief affirmation intervention. During the intervention, participants were asked to assess a list of several personal values such as sense of humor, relationships with friends and family, and athletic ability and select either their most important or least important value from the list. After selecting their most important value, participants in the affirmation condition were instructed to write about why their most important value matters to them (e.g., Cohen, Aronson, & Steele, 2000).

To strengthen the manipulation, participants in Studies 2, 3, and 4 were also instructed to give examples of times when the value had an impact on their life and were required to spend at least one minute responding to the affirmation prompt (see Appendix A for complete affirmation and control condition materials). For all studies, participants in the control condition were asked to write about why their least important value might matter to someone else.

After completing the affirmation intervention, participants in all four studies evaluated and provided affective forecasts for hypothetical scenarios. The specific content of the scenarios differed across studies and is described in more detail for each study below. Because the link between affirmation and affective forecasting had not been studied in previous research, I included several forecasting variables to capture a variety of potential effects. Across studies, participants rated (1) how upset [for negative events] or happy [for positive events] they would feel immediately following the hypothetical event, (2) how upset or happy they would feel one

day or one week after the event, and (3) how likely the event was to occur, each on a 7-point Likert scale (*1 = not at all, 7 = extremely*). After providing their forecasts, participants completed a variety of other mood and cognitive measures and provided basic demographic information.

Study 1 begins to explore the link between affirmation and affective forecasting, while Studies 2 through 4 examine the interactive effects of affirmation and threat breadth on various outcomes including affective forecasting (all studies), reactions to threatening information (Study 3), and performance on a difficult creativity test (Study 4).

Study 1: Affirmation and Forecasting in Three Important Domains

Study 1 began to explore the relationship between affirmation and affective forecasting by randomly assigning participants to affirm or not and then asking them to evaluate scenarios of their own choosing in several key life domains: professional/academics, financial, and health. For each of these domains, participants were instructed to think about an upcoming event that could cause their situation to improve in that domain (good scenario) and another event that might cause their situation to get worse (bad scenario). After describing each of the six scenarios, participants provided affective forecasts about how they predicted they would feel if the scenario actually occurred. Based on the existing literature and the theoretical model of the relationship between affirmation and affective forecasting developed earlier (see Figure 1), I hypothesized that affirmed participants would report significantly less extreme affective forecasts compared to control participants for all six scenarios.

Method

Mechanical Turk as a participant pool. Participants for Study 1, as well as for each of the studies that follow, were recruited online via Amazon's Mechanical Turk website. On this

site, “requesters” can post brief jobs or tasks (known as “HITs”) for “workers” to complete in exchange for a small monetary payment. While originally intended as an affordable method of crowdsourcing tasks that require human effort but do not necessitate any particular expertise (such as testing web links or identifying typos), Mechanical Turk quickly became a popular resource for researchers as well (Buhrmester, Kwang, & Gosling, 2011). Psychologists especially have been attracted to the site as a source of participant samples that is independent from the traditional undergraduate psychology participant pools.

Although not perfectly representative of the population, U.S. Mechanical Turk workers are substantially more diverse than a typical undergraduate participant pool population (e.g., Berinsky, Huber, & Lenz, 2012; Paolacci, Chandler, & Ipeirotis, 2010). As demonstrated by the participant demographics reported below for each study, Mechanical Turk participants are older on average, represent a wider age range (although older adults tend to be underrepresented), and are more likely to be in the labor force (although as many as one third report being students) than most college samples. Like an undergraduate sample, most Mechanical Turk participants (around 85-90% in my studies) have at least some college education, but the range of educational attainment is somewhat greater than an undergraduate sample as the pool is not restricted to current students. Also similar to an undergraduate sample, the majority of participants in my studies identified as White and all but one study had more female than male participants. It is also possible that Mechanical Turk workers differ from the average person in the U.S. in other, unmeasured ways (such as possessing greater cognitive processing abilities or being more conscientious) that may affect the generalizability of results.

Additionally, although probability-based sampling methods have historically been preferred when the goal is to generalize from a sample to the general population, there have also

been compelling arguments in favor of the use of convenience samples for experimental research (e.g., Mook, 1983). The goal of the present research is primarily to test the existence of a possible relationship between affirmation and affective forecasting and a potential interaction between affirmation and threat breadth, not to make claims about the precise magnitude of this effect in the general population. To claim that a relationship *can* exist does not require evidence that the relationship usually does exist in normal circumstances (Mook, 1983).

However, it is also likely reasonable to assume that the findings presented here would generalize to other situations in which values affirmation might be employed. As Druckman and Kam (2011) convincingly argue, convenience samples probably do not pose as great a threat to external validity as is typically imagined. In the case of the present research, it is not obvious why individuals on Amazon's Mechanical Turk site would respond differently than other individuals in the U.S. adult population to a values affirmation intervention or to potentially threatening events, information, or performance evaluations. And because values affirmation itself is primarily of interest as an intervention (rather than a spontaneously-occurring phenomenon), the experimental realism of the present research is not particularly compromised by the artificiality of the manipulation.

The recruitment of participants via Mechanical Turk has many benefits to researchers, including an affordable price tag (a typical psychology study would cost no more than a few hundred dollars) and a quick turnaround time (typically a matter of days rather than weeks or months) (Buhrmester et al., 2011). However, there are also potential drawbacks and limitations to using Mechanical Turk for participant recruitment. First, of course, is the restriction to online data collection. The current studies were designed to be completed online, but there are many variables about the participant environment that cannot be controlled in a web study and there

are also limitations to what kinds of data can be collected (Rand, 2012). On the other hand, there are limitations to any single mode of data collection and using a consistent mode across studies allows for greater comparability of results.

A second potential drawback that is more specific to the Mechanical Turk site is the prevalence of "professional participants" – Mechanical Turk workers who have a preference for research studies when selecting their HITs and may complete hundreds or even thousands of studies online. Researchers rarely know how many other studies their participants have completed in the past or if any of those studies contained related study content, but with many psychologists worldwide using the same pool of workers to conduct research with similar goals it seems unavoidable that some participants will see the same manipulations and measures more than once. Research on this subject has indicated that productive participants often have prior experience with popular research paradigms (Chandler, Mueller, & Paolacci, 2014). The default setting on Mechanical Turk to use "Master" workers – individuals who have received high approval ratings on a number of prior tasks – may exacerbate experience effects by limiting the pool of potential participants only to highly experienced workers. For this reason, I do not restrict participation to Master workers in my studies. Nevertheless, self-reported data drawn from the current research suggest that approximately one-third of participants in each of my studies had already participated in at least one values affirmation study.

Although there is no perfect method for recruiting participants, Mechanical Turk provides an appealing alternative to undergraduate participant pool samples for psychological researchers whose studies are suitable for online implementation. Even survey researchers, who are especially concerned with the generalizability of research results, have found it necessary to consider the merits and potential benefits of popular non-probability samples like Mechanical

Turk (Brick et al., 2013). As the AAPOR Task Force on Non-Probability Sampling (Brick et al., 2013) concludes, a primary consideration in the selection of sampling methods is fit for use. Because of the need for numerous studies, fairly large samples (for experimental research), and somewhat extensive pilot testing, Mechanical Turk seemed to be an ideal sample source for the current research, despite its potential drawbacks. For each of the reported studies, the total cost per complete was less than $1.00 and the time required to obtain the desired sample size was always less than three business days (not including pilot testing). Attempts were made to measure potential confounds, such as prior experience with the primary manipulation, and the analyses reported below consider the impact of these variables. As described below, additional efforts were made to minimize the number of participants who completed multiple studies within this same research series. Additional research is still needed, however, to determine the generalizability of the reported results to other data collection methods and other populations.

Power. Expecting a medium to large-sized main effect of values affirmation on affective forecasts, but with the potential for extra noise resulting from the research design (in which each participant selected personally-relevant scenarios to provide forecasts for), I set the target sample size for Study 1 at 60 completes per affirmation condition. This target was slightly exceeded with a final count of just over 70 participants per condition.

Participants. Participants were recruited online through Amazon Mechanical Turk in exchange for a payment of $0.50. A total of 142 participants (56 men, 85 women, and 1 individual who declined to indicate sex) completed the study. An additional 34 participants began the study and dropped out before reaching the end. Of these, only 10 actually began the affirmation (the first substantive task) and only five completed the forecasting section. When available, these partial data were also included in the analyses presented below.

The majority of participants were between the ages of 21 and 40, with 40.1% aged 21-29 and 22.5% aged 30-39. Almost two-thirds of participants (63.4%) indicated that they were currently employed either part time or full time and nearly a third (32.9%) reported being a full-time or part-time student. Most participants (84.5%) reported greater than a high school education, with the modal educational attainment being "some college" (33.1%).

Materials. Study 1 used a values affirmation modeled after Cohen, Aronson, and Steele (2000), as described earlier. In the affirmation condition, participants selected their *most* important value from a list of potential values and spent a few minutes writing about why that value was important to them. In the control condition, participants selected their *least* important value and spent a few minutes writing about why that value might be important to someone else. See Appendix A for the exact wording and instructions provided in the affirmation and control conditions.

Rather than reading standardized scenarios that might not be equally relevant across individuals, participants in Study 1 were permitted to describe self-relevant scenarios of their own choosing. The instructions encouraged participants to think of one positive and one negative event that could happen in their own lives for each of three domains: professional/academic, financial, and health. The instructions for a negative professional/academic scenario (bad professional), for example, were as follows: "Take a moment to imagine an upcoming situation in which your PROFESSIONAL OR ACADEMIC situation will get WORSE (such as losing your job, receiving a bad performance review, or failing an important test)." The instructions for the remaining five scenarios (good professional, bad financial, good financial, bad health, and good health) used parallel instructions with domain-specific examples. Participants then explained the imagined scenario in a few sentences

in an open text box. For this study, each participant rated all six scenarios and the scenarios were always presented in the same order.

In addition to providing a brief description of each of the six scenarios, participants also completed affective forecasting measures that assessed how they would feel if each of the six scenarios actually occurred. As in each of the studies that follow, participants in Study 1 rated (1) how upset [for negative events] or happy [for positive events] they would feel immediately following the hypothetical event, (2) how upset or happy they would feel one week after the event, and (3) how likely the event was to occur, each on a 7-point Likert scale (*1 = not at all, 7 = extremely*).

To assess possible cognitive precursors to changes in affective forecasts, participants also completed a measure of global-local focus (Kimchi task; Kimchi, 1988), a measure of cognitive construal level (BIF; Vallacher & Wegner, 1989), and a measure of mindfulness (MAAS; Brown & Ryan, 2003).

Procedure. After being recruited through Amazon's Mechanical Turk, participants were randomly assigned to complete an affirmation or a control writing task. Next, participants spontaneously described and then rated six scenarios. The instructions encouraged participants to think of one positive and one negative event that could happen in their own lives for each of three domains: professional/academic, financial, and health. Participants then explained the imagined scenario in a few sentences and provided affective forecasts using the measures described above. To assess possible cognitive precursors to changes in affective forecasts, participants also completed a measure of global-local focus, a measure of cognitive construal level, and a measure of mindfulness at the end of the study, before providing basic demographic information.

Results and Discussion

Exclusions and outliers. For Study 1 and each of the studies that follow, preliminary analyses were run both with and without various groups of participants, including those who reported prior experience with values affirmation, individuals who provided incorrect responses to questions designed to "catch" participants who weren't paying attention (typically, this group only made up a very small percentage of the total participants), apparent outliers in terms of time taken to complete the overall study or specific tasks, and anyone who was flagged as a potential repeat participant who might have also completed similar or related studies I had conducted on Amazon's Mechanical Turk. In almost all cases, the pattern of results was not substantially changed by the exclusion of these different groups of participants. As a result, unless otherwise noted, I have chosen to include all participants in the reported analyses for Study 1 as well as the studies that follow.

Affective forecasts. The main results and significance tests are displayed in Table 1. To examine the overall effect of affirmation condition on forecasting, I first conducted a 2 (Condition: affirmation vs. control) by 3 (Domain: professional/academic, financial, health) by 2 (Valence: good and bad) between-within ANOVA for each of the key affective forecasting measures: immediate emotional impact, delayed emotional impact, and likelihood. None of the between-within ANOVAs showed an overall effect of condition on forecasts (all Fs < 2.10, ps > .150). The interaction terms, however, suggested that there was an effect of condition that was not consistent across scenario domain and valence. The Condition x Domain x Valence interaction on immediate emotional impact, for example, was significant, indicating that the effect of condition was not constant across topics and valence, $F(2,136) = 9.57$, $p < .001$, partial $\eta^2 = .065$. This same three-way interaction was also significant for one-week emotional impact

($F(2,137) = 3.35$, $p = .037$, partial $\eta^2 = .024$), but not for ratings of likelihood ($F(2,134) = 2.11$, *ns*). Depending on which outcome you look at, this interactive effect was either small (one-week forecast) or medium (immediate forecast) in size.

Because the relationship between affirmation condition and forecasting did not appear to be constant across topic and valence, I next conducted independent sample *t*-tests comparing affirmed and control participants on their ratings of each of the six scenario types. For the most part, as predicted, affirmed participants gave more moderate affective forecasts. The trend was the same across the three dependent measures (immediate emotional impact, delayed emotional impact, and likelihood), but was strongest in this study for ratings of immediate emotional impact.

[Table 1 about here]

As seen in Table 1, the difference in ratings across conditions was significant for the anticipated immediate emotional reaction to the professional-good, professional-bad, and financial-bad scenarios, as well as marginally significant for health-good in independent samples *t*-tests. As predicted, affirmed participants rated their predicted emotions as less strong compared to non-affirmed participants for all of these scenarios. For financial-good, there was virtually no difference between conditions. For health-bad, there was a backfiring effect, such that affirmed participants made more extreme forecasts (they thought the bad health outcome would be worse compared to controls). In a simple t-test, however, this backfiring effect was not significant even for the strongest effect, Immediate Affective Forecast: $t(144) = 1.41$, $p = .160$). Taken together, the results suggest that the pattern of forecasting results for the bad health

scenario, particularly for the anticipated emotional consequences, was different from the pattern for the other scenario types. Figures 2 and 3 graphically depict this pattern of results for immediate affective forecasts.

[Figures 2 and 3 about here]

Other measures. This study also provided some evidence, consistent with previous published findings (Wakslak & Trope, 2009), that affirming causes individuals to adopt a broader perspective. Affirmed participants chose significantly more globally-focused items on the Kimchi task compared to control participants, M_{aff} = 9.83 vs. M_{cont} = 7.96, $t(143) = 2.08$, p = .040. Affirmed participants also selected more abstract choices on the cognitive construal level measure, M_{aff} = 6.71 vs. M_{cont} = 6.23, but this difference was not significant, $t(142) = .99$, *ns*. Although this finding is compatible with existing theories that affirmation causes participants to focus more on the big picture and think at a more abstract level (Critcher & Dunning, 2009; Schmeichel & Vohs, 2009), I was unable to find evidence in the present research that global focus mediated any of the observed effects of affirmation on affective forecasting (all confidence intervals for the indirect effects encompass the zero point in bootstrapping analyses). Follow-up mediation analyses also showed no significant mediation for the opposite causal chain (that is, it did not seem to be the case that affirmation led first to more moderate forecasts which then caused participants to adopt a more global perspective). Thus, while Study 1 provides some evidence that affirmation broadens a person's perspective, this does not appear to be the primary mechanism for the observed effects of affirmation on affective forecasting. Finally, there was

not a significant difference between conditions on mindfulness scores (another possible cognitive consequence of affirmation), M_{aff} = 40.88 vs. M_{cont} = 40.71, $t(148) = .12$, *ns*,

Follow-up study. Why did self-affirmation moderate forecasts on most items, but increase the perceived emotional impact of a bad health outcome? One possibility is that the latter outcome has much broader effects than the others, in terms of how it impacts people's lives. I conducted a follow-up study to assess the plausibility of such a breadth-based explanation. In this study, 140 mTurk participants followed the same scenario invention procedure used in Study 1 and provided affective forecasts for those scenarios. The only difference from Study 1 was the absence of an affirmation manipulation. Then, following the forecasting procedure, participants provided additional ratings of the perceived breadth of their chosen scenarios, selecting from a series of checkboxes whether or not each of 15 life domains would be impacted by the scenario, such as relationships with family and friends, career, academic pursuits, athletic pursuits, and physical health.

[Table 2 about here]

As predicted, paired *t*-tests comparing the mean number of domains selected for the health-bad scenario to each of the other scenarios (see Table 2) revealed that participants gave higher breadth ratings for their bad health scenarios compared to all other scenario types (which all had similar mean ratings). The number of domains selected for the bad health scenario was significantly higher than the number for bad professional, bad financial, and good health scenarios (*t*s(137+) > 2.40, *p*s < .020, Cohen's *d*s > .41) and marginally higher than the number for good financial scenarios ($t(138) = 1.75$, $p = .083$, Cohen's $d = .30$). These results are

consistent with the hypothesis that self-affirmation will moderate affective forecasts for narrowly-focused threats that do not affect many areas of the individual's life, but make forecasts more extreme for threats that are broad or overlap with the domain of people's lives that they have affirmed.

General discussion. Study 1 provided a preliminary exploration of the link between affirmation and affective forecasting, looking at predicted reactions to possible future scenarios generated by the participants in several key life domains. The results of this study suggest that, in general, forecasts for potential future events are made more moderate by the process of affirming. However, the data also indicate a potentially important exception – the case of a negative health threat. In this one domain only, participants who were affirmed actually provided more extreme forecasts compared to controls. The unexpected outcome for a negative health scenario motivated further exploration in a follow-up study. Although there are many potential differences between a bad health scenario and other types of negative outcomes, I chose to focus on a particularly salient feature: threat breadth. The scenarios participants wrote about in Study 1 for a negative health outcome seemed to be substantially more wide-reaching in their possible side effects than the scenarios chosen in other domains. The follow-up study confirmed this observation; when participants spontaneously wrote about potential positive and negative outcomes in various life domains, they rated the negative health outcomes as the broadest (that is, affecting the greatest number of life domains).

Although the existing affirmation literature acknowledges a potential backfiring effect for affirmation when the threat domain overlaps with the affirmed value (e.g., Blanton, Cooper, Skurnik, & Aronson, 1997), to my knowledge there has been no consideration of a more general effect of threat breadth. In Studies 2 through 4, described below, I take various approaches to

examining this potential moderator, while continuing to examine the link between affirmation and affective forecasting.

Study 2: Affirmation and Narrow vs. Broad Bad Health Scenario

Although the above results are suggestive of a relationship between affirmation and affective forecasting, there are two primary limitations of Study 1. The first is the lack of equivalence between scenarios generated by different participants. Participants were given the opportunity to think of a scenario for each category spontaneously and then to rate the affective impact of that scenario. While this allows for a degree of realism and relevance that might not be possible when presenting the same scenario to all participants, it also produces uncertainty about the results. Perhaps affirmed participants perceive threats of the same severity to be less threatening than non-affirmed participants, as I hypothesized, but on the other hand perhaps affirmed participants spontaneously select future events that are objectively less threatening than those events selected by the non-affirmed participants while showing no difference in forecasting per se. Both effects would be interesting, but only the former supports affective forecasting as a potential mediator of affirmation's effects or as an additional consequence of affirmation.

The second limitation of Study 1 is its lack of direct manipulation of the breadth of self-threats. Most results followed the expected pattern of affirmed participants making more moderate forecasts, but the bad health scenario showed opposite effects. This could be consistent with the hypothesis that broad threats or those that overlap with the affirmed values cause a backfiring effect of affirmation—a hypothesis which received some support in the follow-up to Study 1—but without directly manipulating the breadth of the scenario it is difficult to determine whether this difference actually caused a change in forecasting for affirmed participants.

Study 2 attempted to address these limitations by assigning all participants to evaluate the same scenarios and by randomly assigning participants to consider a scenario that was either narrow or broad. Because I was also interested in further illuminating the backfiring effect for the negative health scenario from Study 1, the scenarios created for Study 2 both focused on a hypothetical bad health outcome: getting the flu. I hypothesized that affirmed participants who read the broad health scenario would show the same backfiring effect observed in Study 1, providing more extreme affective forecasts than control participants, while affirmed participants who read the narrow health scenario would give more moderate affective forecasts than controls, as they did for the other types of scenarios in Study 1.

Method

Power. Based on the small to medium-sized interaction observed in Study 1 and my belief that changes to the design would strengthen the interaction (by assigning all participants to read the same scenario and by directly manipulating threat breadth), I again set the target number of participants per affirmation condition at 60. Because of the 2x2 design (affirmation condition by threat breadth), this target sample size resulted in approximately 30 participants per cell.

Participants. Participants were recruited online through Amazon Mechanical Turk in exchange for a payment of $0.50. A total of 129 participants (46 men, 82 women, and 1 individual who declined to indicate sex) completed the study. All participants who began the study completed it. The majority of participants identified as White (83.7%) and reported being between the ages of 21 and 40, with 27.1% aged 21-29 and 29.5% aged 30-39. Almost two-thirds of participants (62.8%) indicated that they were currently employed either part time or full time and nearly a quarter (24.2%) reported being a full-time or part-time student. Most participants (91.5%) reported greater than a high school education, with the modal educational

attainment being a Bachelor's degree (34.9%). Overall, participant demographics in Study 2 were similar to Study 1 demographics, but participants were slightly older and more educated in Study 2.

Materials. The values affirmation and forecasting materials for Study 2 were minimally changed from those used in Study 1. As described at the end of the introduction, an additional writing instruction was added to strengthen the values affirmation manipulation (see Appendix A for exact wording). Additionally, in response to possible concerns that participants recruited online might be more motivated than participants in the lab to hurry through the questionnaire, I added item-level timing data in Study 2 to measure how long participants spent completing the key values affirmation manipulation. The data provide reassurance that participants were not simply rushing through the task. Even after excluding extreme time outliers (anyone who spent more than 15 minutes on the affirmation), respondents spent an average of 290 seconds (almost 5 minutes, *SD* = 155 seconds) responding to the affirmation writing prompt and 141 seconds (more than 2 minutes, *SD* = 145 seconds) responding to the shorter control writing prompt despite only being required to spend at least 60 seconds completing the task.

The primary change from Study 1 to Study 2 was the development of hypothetical scenarios that were read and rated by all participants and that deliberately manipulated the threat breadth. To pursue further the unexpected backfiring result from Study 1, the scenarios created for Study 2 both focused on a hypothetical bad health outcome: getting the flu. In the narrow condition, participants imagined getting the flu during a break from school or work during which they were not planning any activities. In the broad condition, on the other hand, participants imagined getting the flu during a break from school or work but also imagined that the illness would interfere with several important activities they were planning to participate in (see

Appendix B for complete scenario texts). Although there were two potential components to breadth that might have been manipulated—the number of different areas that are impacted by the threat as well as the degree to which the threat is expected to impact the affirmed resources—the scenarios in this study focused primarily on breadth in terms of quantity of impacted domains. To the extent possible, the broad scenario in this study did not overlap with the values participants typically choose to affirm (e.g., interpersonal relationships, religious beliefs). Prior research had already hypothesized a backfiring consequence when the threat and the affirmed resources overlap (e.g., Blanton, Cooper, Skurnik, & Aronson, 1997), but the goal of Study 2 was to explore breadth without explicit overlap as another potential moderator of affirmation's effects.

As a manipulation check for the narrow and broad scenarios, I also included a breadth rating similar to the one used in the Study 1 follow-up. Participants saw a list of fifteen life domains that might be impacted by the threat scenario and were asked to rate how much the threat scenario would affect each area on a 5-point Likert scale (1 = *not at all*, 5 = *a great deal*). The Study 1 follow-up used only a binary (yes/no) item for each life domain. It was expected that participants who read the broad scenario would give a higher total rating for the impact of the scenario on the fifteen life domains.

Hoping to replicate the difference in global-local focus scores across affirmation condition observed in Study 1 for the Kimchi task (Kimchi, 1988), I again included this measure in Study 2 along with an additional measure of thought style that is intended to assess relational vs. categorical thinking (Triad task; Ji, Zhang, & Nisbett, 2004). I speculated that, if affirmed participants are more attuned to the interconnections between events, perhaps they would demonstrate a tendency to select more relational pairs in a categorization task.

Procedure. As in Study 1, online mTurk participants were randomly assigned to affirm or not prior to evaluating the scenarios, then they were randomly assigned to read and rate one scenario which was either narrow or broad in its consequences. After completing the affective forecasting measures, participants provided a breadth rating similar to the one used in the Study 1 follow-up. Participants also completed a measure of global vs. local focus (Kimchi Task) and a measure of relational thinking (Triad Task) to assess possible cognitive differences across conditions.

Results and Discussion

Repeat participants and outliers. Using IP address as a possible indicator of repeat participation, five participants were identified as possible duplicate participants. Several potential time outliers were also identified. Because results were very similar whether these potential repeaters and outliers were included or excluded in the analyses, I elected to include them in the following analyses.

Affective forecasting. The results are displayed in Table 3 and Figure 4. In general, the results of Study 2 supported the hypothesis that affirmed participants make more moderate forecasts compared to controls for narrow threats but more extreme forecasts for broad threats, while unaffirmed participants show less differentiation between broad and narrow threats in their forecasts (see Figure 4). For immediate emotional impact, in addition to the significant main effect of breadth (people predicted they would be more upset overall by the broader scenario, $F(1, 125) = 9.00$, $p = .003$, partial $\eta^2 = .067$), affirmed participants predicted a slightly less intense emotional response to the narrow scenario compared to controls but a substantially stronger reaction to the broad scenario. In a 2x2 ANOVA, however, this interaction did not reach statistical significance, $F(1, 125) = 2.74$, $p = .100$, partial $\eta^2 = .021$. The interaction

between condition and scenario type was significant for the forecasts of emotional impact one week later, indicating that the effect was stronger when considering time-delayed affective forecasts, $F(1, 125) = 4.04$, $p = .047$, partial $\eta^2 = .031$. Unaffirmed participants and affirmed participants who read the narrow scenario gave fairly low ratings on this forecasting measure, but affirmed participants who read the broad scenario continued to give high ratings (see Table 3).

[Table 3 about here]

Collapsing across immediate and one week affective forecasts in a 2 (Time: immediate vs. one week) x 2 (Condition: affirmation vs. control) x 2 (Threat Framing: narrow vs. broad) between-within ANOVA showed that the overall interaction effect across both forecasting measures for affirmation condition and threat breadth was marginally significant, $F(1, 125) = 3.81$, $p = .054$, partial $\eta^2 = .030$. Note that the effect sizes for all of these interactions are fairly small, suggesting that the modifications to the experimental design may not have increased the strength of the effect as expected. As a result, the analyses reported above are most likely underpowered.

An even stronger crossover interaction was observed for estimates of how likely the events were, (see Figure 4), $F(1,125) = 6.89$, $p = .010$, partial $\eta^2 = .052$. If participants were being realistic in their forecasts, they should have rated the narrow scenario as being more likely than the broad scenario (which depends on a confluence of events). This is the pattern I observed for non-affirmed participants. But affirmed participants actually showed the opposite pattern, rating the broad scenario as more likely than the narrow one (see Figure 4).

[Figure 4 about here]

To evaluate the combined impact of these measures, a multiplicative score using immediate forecasts and likelihood was created. The two measures were recoded to contain a zero point so that participants who rated the event as having no impact on either measure would receive a multiplicative score of zero. Presumably if the event either had no emotional impact or was predicted to be very improbable, its psychological effect would be minimal regardless of the strength of the other rating. As shown in Figure 4, this multiplicative measure followed the same general interaction pattern; affirmed participants rated the narrow scenario as somewhat less impactful than did unaffirmed participants, but they rated the broad scenario as substantially more impactful compared to controls. The interaction was significant, F (1, 125) = 5.59, p = .020, partial η^2 = .043. The effect sizes for likelihood and the multiplicative measure, although still only small to medium in size, were more in keeping with expectations.

In sum, the effects of the affirmation manipulation were moderated by scenario breadth, consistent with the results of Study 1. The reason for this interaction, however, is not entirely clear. Unlike in Study 1, there were no differences across conditions on the two measures of breadth of construal in this study, (Kimchi: M_{aff} = 10.53 vs. M_{cont} = 10.83, t(127) = -0.23, p = .815; Triad (ratio of relational to categorical choices): M_{aff} = .68 vs. M_{cont} = .66, t(127) = 0.34, p = .733).

There was some indication that affirmed participants actually perceived the broad scenario to be more broad than did the narrow participants (not just that they forecasted its emotional impact differently). As expected, participants viewed the broad scenario as having a more extensive impact on a greater number of life domains (M_{broad} = 35.55 vs. M_{narrow} = 28.68, F (1, 120) = 13.04, p < .001), confirming the effectiveness of the breadth manipulation. But there was also a hint of an interaction between condition and scenario type, such that affirmed

participants perceived a greater distinction between the breadth of the broad and narrow scenarios (see Figure 5). Although this interaction was not significant, $F(1, 120) = 2.30$, $p = .132$, partial $\eta^2 = .019$, it adds an additional piece to the puzzle.

[Figure 5 about here]

Affirmation appears to have an impact on affective forecasting, as predicted, but the relationship seems to be sensitive to information about the breadth of the situation. Perhaps affirmation helps to put narrow threats as well as positive outcomes into a larger perspective but unintentionally leads individuals to be even more cognizant of the potential wide-ranging impacts of an event that has broader implications.

Another difference between the results of Studies 1 and 2 is that, in the latter study, the results primarily showed a difference between affirmed and non-affirmed participants who imagined a broad scenario, whereas the difference between affirmed and non-affirmed participants who imagined the narrow scenario was minimal. In Study 1, there was evidence for both a backfiring effect for a broad scenario and a main effect of affirmation moderating forecasts for the other scenarios. Additionally, both Study 1 and Study 2 employed outcome measures that had not been used previously in the values affirmation literature, making it difficult to assess whether the results were consistent with prior research.

Finally, the hypothetical threats considered by participants in Study 1 and Study 2 were not necessarily self-threats of the type typically studied in the affirmation literature. Although the negative scenarios always contained some kind of anxiety-producing outcome, they did not always threaten the participant's positive self-concept (particularly in Study 2). After all, most people would probably be distressed about being in poor health, but often that kind of threat

would not lead to self-blame or guilt. In many cases, getting sick or not is just a matter of chance. Prior values affirmation research, however, has focused almost exclusively on threats that could undermine a person's sense of competence or moral goodness. It was thus unclear whether the effects of affirmation in Study 1 and Study 2 on affective forecasting would be observed for other threat situations, particularly ones that involved a threat to participants' positive perceptions of themselves.

Study 3: Affirmation, Forecasting, and Cognitive Dissonance

I attempted to address several of these limitations in Study 3, primarily by creating threat scenarios that were more similar to those employed in the existing values affirmation literature and by incorporating outcome measures other than affective forecasts to improve comparability to prior studies. In this study, participants imagined a scenario in which their hypothetical decision to text while driving caused them to be in a car accident. The breadth of the scenario was manipulated by varying the consequences of the crash. As in the prior studies, participants provided affective forecasts about their predicted reactions to such an event.

Unlike the prior studies, however, Study 3 did not stop at collecting affective forecasts. Instead, participants continued on to read some factual information about texting while driving, paralleling prior affirmation studies which have presented participants with pamphlets or other informational materials about risky behaviors such as smoking (e.g., Harris, Mayle, Mabbott, & Napper, 2007) or drinking alcohol (e.g., Harris & Napper, 2005). As in similar studies, participants in Study 3 evaluated the factual information about texting while driving and also had an opportunity to express behavior change intentions. Additionally, participants learned about two free smartphone applications that are designed to help individuals avoid texting while driving and rated their interest in these applications.

Based on the results from Studies 1 and 2, I expected to find that affirmed participants in Study 3 provided more moderate forecasts for the narrow car crash scenario and more extreme forecasts for the broad car crash scenario compared to control participants. Although Studies 1 and 2 provided clear hypotheses about how affirmation might impact forecasting in Study 3, it was unknown how the forecasts would relate to other outcome measures that better paralleled the published affirmation literature. As a result, I expected one of two possible outcomes in terms of evaluations of the factual information, behavior change intentions, and ratings of the smartphone applications. Either affirmed participants would rate the information more favorably across the board (indicating less defensiveness) and express greater positive behavior change intentions, or affirmed participants who read the narrow scenario would show that positive pattern of responses but affirmed participants who read the broad scenario would show an opposite, more defensive pattern.

The prior affirmation literature has found that affirmation reduces defensiveness and increases intentions of behavior change in the face of self-threatening information, but has little to say about the interaction between affirmation and threat breadth. Perhaps affirmed participants who read about a broad threat are more aware of its far-reaching implications but are nevertheless open to accepting the threat and adopting positive behavioral change. In this case, I would expect affirmed participants receiving either the narrow or broad threat treatment to respond less defensively to the information evaluation items and to report greater behavior change intentions. Alternatively, maybe reading about a broad threat makes affirmed individuals even more defensive than they would have been if left unaffirmed. If so, then I would instead expect affirmed participants receiving the broad threat treatment to report greater defensiveness

and reduced behavior change intentions compared to controls, while affirmed participants who received the narrow threat would show a traditional positive pattern of results.

Method

Power. Again based on the small to medium-sized interaction observed in Studies 1 and 2 and my belief that changes to the design would strengthen the interaction (by bringing the threat and outcomes more in line with existing affirmation literature), I again set the target number of participants per affirmation condition at 60. Because of the 2x2 design (affirmation condition by threat breadth), this target sample size resulted in approximately 30 participants per cell.

Participants. Participants were recruited online through Amazon Mechanical Turk in exchange for a payment of $0.50. A total of 125 participants (86 men and 39 women) completed the study. An additional seven participants began the study but were not allowed to continue because they did not meet the selection criteria of owning a smartphone and having a valid driver's license. All participants who began the study completed it. The majority of participants identified as White (80.8%) and reported being between the ages of 21 and 40, with 45.6% aged 21-29 and 32.0% aged 30-39. Over three-fourths of participants (77.6%) indicated that they were currently employed either part time or full time and nearly a third (31.5%) reported being a full-time or part-time student. Most participants (92.7%) reported greater than a high school education, with the modal educational attainment being a Bachelor's degree (37.9%). Compared to the Mechanical Turk participants recruited for Study 1 and Study 2, the Study 3 participants were more likely to be male and employed but were similar in age, race, and education level.

Materials. In Study 3, participants again completed a values affirmation manipulation and provided affective forecasts about their reaction to a self-threatening scenario. The materials

used for these items were largely unchanged from Study 2. New materials were developed, however, to describe and measure reactions to a scenario about a car crash caused by texting while driving as well as some factual information about the dangers of texting while driving. To make the threat scenarios more comparable to those examined in the published affirmation literature, I decided to employ a threatening situation that could be caused by the participant's decision to engage in risky behavior. Specifically, I asked participants to imagine that they were in a minor car accident that was their fault, resulting from their choice to text while driving. As in Study 2, the breadth of the scenario was manipulated. In the narrow scenario, there was a financial cost of the accident but the participants were told that their car would be repaired immediately. In the broad scenario, participants were told that their car would be in the shop for a couple of weeks which would interfere with several important activities they were planning to participate in (see Appendix C for complete scenario texts). The wording of the broad scenario was made to be as similar as possible to the wording used in Study 2 for the sake of comparability.

In addition to reading the narrow or broad car crash scenario, participants also read some factual information about the risks of texting while driving that was drawn from several governmental sources, including the official U.S. government's website for distracted driving, D!straction.gov (NHTSA, 2013); a brochure created by the U.S. Occupational Safety and Health Administration (OSHA, 2012); and the California Department of Motor Vehicles (California DMV, 2013). These materials were intended to include the same kinds of information presented to participants in prior studies that have examined the effect of values affirmation on responses to threatening health information (e.g., Harris & Napper, 2005).

To assess reactions to the factual information, I also included items that asked participants to evaluate the information and to report behavior change intentions. These items were modeled after measures used in prior values affirmation research (Harris & Napper, 2005) and included self-reported anxiety and fear while reading the facts about texting while driving (e.g., "I felt fearful while reading the information about texting while driving."), perceived believability of the information ("How believable did you find the facts and statistics about texting while driving you just read?"), concern about changing texting while driving behavior ("I feel that my behavior of texting while driving is something I need to worry about"), and plans to change behavior during the next week ("I intend to cut down on how often I text while driving in the next 7 days"). All response items were rated on a 5- to 7-point Likert scale with item-specific value labels. Participants were also asked three items that assessed accuracy of memory for the factual information.

Additionally, participants were asked to read and evaluate information about two free smartphone applications that are available to individuals who are interested in minimizing their texting while driving behavior. Before reading the descriptions, participants indicated how much they would be willing to pay for an application that helped prevent texting while driving. Then, they rated each of the two applications on the following factors: how effective they expected it to be, how likely they were to look up more information about it, and how likely they were to purchase it. These items were rated on a 7-point Likert scale (1 = *very unlikely*, 7 = *very likely*). The Qualtrics program also recorded how much time participants spent reading the information about each application.

Finally, participants were asked about their own history of texting while driving. They reported whether or not they had ever texted while driving and, if yes, how frequently they had

done so in the past month. Although I did not necessarily expect that effects would be limited to participants with a history of texting while driving and realized that self-report was not an ideal method for assessing a socially undesirable behavior, I thought it could be useful to have at least some indication of the self-relevance of the scenarios for participants.

Together, these measures were intended to provide a more complete picture of participants' reactions to the scenarios and factual information, incorporating emotional reactions, memory, acceptance of potentially-threatening information, interest in behavior change, and behavior change intentions.

Procedure. As in the prior studies, mTurk participants were first randomly assigned to a values affirmation or a control condition, then read one of the two scenarios and provided their affective forecast ratings. To enhance the self-relevance of the scenarios to participants, I limited participation to individuals with a valid driver's license who reported owning a smartphone. After completing the affirmation manipulation and providing their affective forecasts, participants in this study also read a list of factual statements about the dangers of texting while driving and evaluated two smartphone applications that have been created to help drivers reduce their frequency of texting. Finally, participants provided basic demographic information and reported their personal experience with texting while driving and with psychological studies, including the values affirmation manipulation.

Results and Discussion

Repeat participants and outliers. As in the prior studies, participants were identified for possible exclusion based on presumed prior participation in related studies (as identified by IP address), time spent completing the tasks, responses to "catch" questions, and prior experience with values affirmation. Aside from those who reported prior experience with values

affirmation, few individuals were identified for possible exclusion based on these criteria. Because the pattern of results for the primary analyses did not appear to change substantially when different groups of participants were excluded, I elected to include them in all of the reported analyses.

Examination of the self-report texting while driving behavior item revealed that only about two-thirds of Study 3 participants (64%) acknowledged ever having texted while driving. The primary analyses were also run excluding the group of participants who indicated that they had never texted while driving. Again, applying this exclusion criterion did not appear to substantially alter the pattern of results (although results were somewhat less significant due to reduced sample sizes). Thus, in order to maximize the power of the statistical analyses and minimize concerns about socially desirable responding to this item, I decided to include participants who indicated no prior experience with texting while driving in all of the reported analyses.

Affective forecasts. The main results are displayed in Figure 6 and Table 4. A manipulation check confirmed that participants thought the broad scenario would have been more disruptive to their lives than the narrow scenario, $M_{broad} = 3.83$ vs. $M_{narrow} = 3.27$, $t(123) = 3.79$, $p < .001$, Cohen's $d = .68$. Contrary to expectations, however, threat breadth did not appear to moderate the relationship between affirmation and affective forecasting. Forecasting results were expected to show the same interactive pattern that was observed in Study 2. Affirmed broad participants were predicted to provide more extreme forecasts while affirmed narrow participants were anticipated to give more moderate forecasts compared to control participants who read either scenario. Instead, affirmed participants who read both the broad and the narrow scenarios gave more moderate affective forecasts compared to control participants, particularly

for the imagined emotional impact of the event after one week (see Figure 6), $F(1, 121) = 14.28$, $p < .001$, partial $\eta^2 = .106$. This main effect was also significant for immediate forecasts ($F(1, 121) = 4.16$, $p = .043$, partial $\eta^2 = .033$) and for a 2x2x2 between-within ANOVA that collapsed across immediate and one week forecasts ($F(1, 121) = 11.61$, $p = .001$, partial $\eta^2 = .088$). These results suggest a medium-to-large overall main effect of less extreme affective forecasts for affirmed participants that did not interact with threat breadth.

Information evaluation and behavior change intentions. There were two competing hypotheses for the relationship between affirmation and information evaluations and behavior change intentions. I predicted either that affirmation would have an overall main effect on these ratings – causing participants to be more open to the information, less anxious and fearful about the information, and more interested in behavior change – or that the effect of affirmation would be moderated by whether participants read the narrow or broad threat. The results primarily supported an unmoderated main effect explanation. Consistent with a main effect hypothesis, affirmed participants reported being less anxious and fearful while reading the factual information regardless of scenario breadth (Anxious: $F(1, 120) = 4.06$, $p = .046$, partial $\eta^2 = .033$; Fearful: $F(1, 120) = 7.11$, $p = .009$, partial $\eta^2 = .056$), but in contrast to expectations and the findings of prior research they did not report any greater intention of behavioral change ($F(1, 120) = 0.37$, *ns*) and did not correctly recall a greater number of facts compared to control participants ($F(1, 121) = 0.97$, *ns*).

Smartphone applications. Additionally, affirmed participants gave lower overall evaluations of the smartphone applications compared to non-affirmed participants ($F(1, 120) = 5.05$, $p = .027$, partial $\eta^2 = .040$), evaluating the apps as less effective and rating themselves as less likely to find out more about or purchase the apps (see Figure 6).

[Figure 6 about here]

This result was in the opposite direction of the expected effect. I predicted that affirmed participants, because they were more open to the potential consequences of texting while driving, would be more interested in the applications and more willing to use them compared to control participants. Instead, although they reported being less anxious and afraid of the factual information, affirmed participants expressed greater disinterest in the applications. That is, whether they read a broad or a narrow scenario, affirmed participants seemed less concerned about the potential bad outcomes associated with texting while driving. While this result was unexpected, it is consistent with prior findings that affirmation might sometimes unintentionally reduce motivation (Vohs, Park, & Schmeichel, 2012) despite making participants more open to threatening information. This finding is also consistent with the relationship between affective forecasting and motivation proposed by Morewedge and Buechel (2013), who found that a bias toward extreme forecasts was associated with increased motivation. Perhaps the moderate forecasts and disinterest in the smartphone applications observed in Study 3 were both related to this key third variable of motivation.

Moderated mediation. Although the overall response of affirmed participants to the scenarios and texting while driving information appeared to be similar regardless of scenario breadth, a couple of observed interactions between condition and scenario type suggest that the psychological motivation behind these similar responses may differ. As seen in Figure 6, affirmed participants who read the broad car crash scenario reported substantially lower fear and anxiety in response to the factual information compared to controls, while the reduction was more modest for affirmed participants who read the narrow scenario. Similarly, there was an

interaction between condition and scenario type for perceived believability of the factual information ($F(1,121) = 6.01$, $p = .016$, partial $\eta^2 = .047$) and perceived importance of changing texting while driving behavior ($F(1,121) = 3.82$, $p = .053$, partial $\eta^2 = .031$) (see Figure 7).

[Figure 7 about here]

While non-affirmed participants did not differ much on these ratings by scenario type, affirmed participants differed depending on the breadth of the scenario they read. Specifically, affirmed participants who evaluated the narrow car crash scenario reported that the facts were more believable and that they were marginally more worried about changing their texting while driving behavior compared to affirmed participants who evaluated the broad scenario. Although this interaction was only present for a few outcome variables and represented a fairly small effect, it does raise the possibility that the psychological consequences of affirmation may differ depending on scenario type. It could be that affirmation coupled with a narrow scenario causes participants to feel somewhat more open to threatening information and less distressed by it (consistent with previous literature), while linking affirmation to a broad scenario leads to increased defensiveness and avoidance of the information.

Study 3 included no direct measures of defensiveness or openness, but numerous items were related to these concepts. Two items specifically stood out as potential proxies for measures of openness (i.e. “How believable did you find the facts and statistics about texting while driving you just read?”) and defensiveness (i.e., “I feel that my behavior of texting while driving is something I need to worry about.”). Higher ratings of the believability item suggest greater openness, while lower ratings of the worrisome behavior item could be seen to indicate greater defensiveness. Although these are not perfect proxies, and indeed it may be difficult

using any self-report measure to obtain accurate information about either openness or defensiveness, I nevertheless chose to use them to explore the possibility of different psychological effects of affirmation on participants who read the broad or the narrow scenario. To do this, I performed a series of moderated mediation analyses with bootstrapping for key outcome variables using the Hayes (2012) PROCESS Macro in SPSS. Results are reported in Table 5.

[Table 5 about here]

Several of the key outcome differences across conditions for the broad scenario type were significantly mediated by the defensiveness proxy, which did not significantly mediate the relationship for the narrow scenario. On the other hand, the openness proxy mediated several of the observed relationships between condition and key outcome variables for the narrow scenario but not for the broad scenario. The key outcomes that showed this moderated mediation effect range from the affective forecast measures to anxiety in response to the factual information to the evaluation of the smartphone applications for reducing texting while driving behavior. This pattern of results suggests that the observed effects of affirmation that appeared to be the same across scenario types may actually have been mediated by different psychological processes.

Specifically, affirmation followed by a narrow scenario appears to cause participants to become more open to threatening information (as indicated by greater perceived believability of the texting while driving statistics), while affirmation followed by a broad scenario seems to increase defensiveness (as indicated by reported reduced concern for potentially dangerous texting while driving behavior). Although the observed outcomes for these two different processes seem to be similar—less extreme affective forecasts, reduced anxiety and fear, and

lower interest in options that could facilitate behavior change—the path taken to get to these outcomes may differ substantially. In one case, the observed outcomes reflect a sense of calm and acceptance of the threatening information that puts the threat into perspective but also reduces motivation. In the other case, however, affirmation seems to be having the undesirable effect of increasing anxiety and creating a greater need to defend against potentially threatening thoughts and information. Study 4 further investigated these opposing effects of affirmation.

Study 4: Affirmation, Forecasting, and Creativity Performance

The goal of affirmation is to rally the resources a person needs to combat an impending self-threat and to restore a feeling of self-integrity. Prior research has shown that affirmed individuals are more able to face a discrete threat openly, whether it be poor academic performance or health risk. The present research, however, suggests that affirmation may backfire when the threat is broad enough to impact the resources rallied by affirmation. That is, affirmed individuals seem to be more sensitive to the potentially wide-ranging impacts of a broad threat than are unaffirmed individual, triggering defensive processes (Study 3). This defensiveness leads to more moderate affective forecasts, as it does for affirmed participants who imagine a more narrow threat, but also unwillingness to confront threatening information and reduced interest in positive behavior change. Thus, when a threat is narrowly-focused, affirmation may help individuals to face the challenge with less anxiety and greater openness. When a threat is broad, on the other hand, affirmation has the potential to backfire and create even greater anxiety or defensiveness than that experienced by individuals who did not experience any intervention.

To my knowledge, no prior affirmation research has manipulated how broadly the participants' lives might be impacted by the potential threat. By design, most affirmation

research has looked at threats that are fairly constrained and domain-specific. Studies 1 through 3, however, suggest that adding a breadth component may alter the effects of affirmation and even cause affirmation to backfire, in the sense that it causes participants to be more defensive. Compared to a narrow threat, greater breadth might increase (rather than decrease) defensiveness in a person who has recently affirmed an important value. Because the boundaries of affirmation's effectiveness are poorly understood, further exploration of the breadth component could offer a substantial benefit to current affirmation theory.

Study 4 was designed to evaluate the potential activation of defensiveness, rather than openness, in the presence of a broad internal threat.[1] This effect was hinted at by the significant moderated mediation analyses in Study 3 and could have substantial implications for the use of affirmation in applied contexts. If a minimal expansion of the threat causes affirmation to backfire, this would suggest that affirmation is a more capricious intervention than previously supposed. It would also highlight the importance of framing the target threat as narrowly as possible to avoid exacerbating defensiveness or poor performance. If, on the other hand, affirmation is found to improve performance regardless of threat framing, this would provide additional evidence for affirmation as a robust intervention and would necessitate a reevaluation of the link between affirmation, affective forecasting, and outcomes.

[1] Note that, prior to conducting Study 4, I explored another potentially-relevant factor suggested by Studies 1 through 3: internal vs. external threat. Based on some of the prior results, I speculated that affirmation may have a different impact on affective forecasting for a broad threat depending on whether the threat was something within the personal control of the participant or not. I conducted a study designed to test this hypothesis, but unfortunately observed a confounding differential dropout rate across the affirmation and control conditions which had not been observed in prior studies. I retested the study with a new control condition that made the affirmation and control more similar and did not observe any effect of threat type in the data. Comparing results from the original and modified versions of the study, there was some suggestion that my new control condition was unintentionally acting as a partial affirmation. Given the problems with these two initial studies and the non-centrality of the internal vs. external threat issue to the present research, I elected to focus instead on the more promising narrow vs. broad element (Study 4). As in most prior affirmation research, Study 4 focuses exclusively on a threat that is somewhat within the participant's control (in this case, performance on a test).

Another goal of Study 4 was to increase the variety of outcomes being measured in this research program. Affirmation research has typically examined two primary dependent variables: cognitive dissonance/defensiveness and performance. Studies 1 and 2 used affective forecasts as the only dependent measure (not used in any prior affirmation research), while Study 3 expanded the scope to include measures of cognitive dissonance and defensiveness. Study 4 incorporated performance as a key outcome variable. Using a real performance task also allowed us to get beyond the limitations of using hypothetical scenarios. In Study 4, participants imagined how they would feel if they performed poorly on a task that they then actually completed. Incorporating a measure of actual performance also gave me the opportunity to identify more clearly the parallels and distinctions of the present findings in relation to the results reported in the published literature.

With this study design, I expected to replicate conceptually the results from Study 3. That is, in terms of forecasting, I predicted that affective forecasts for the reaction to poor performance would be less extreme for affirmed participants regardless of whether they considered the broad or narrow implications of the performance measure. But I also expected to see differences in openness and defensiveness, such that affirmed broad participants are more defensive while affirmed narrow participants are more open to threatening information about their performance compared to control participants. I hypothesized that openness would mediate the relationship between affirmation and forecasts for those who imagined the narrow scenario, while defensiveness would mediate the relationship for the broad scenario participants.

With regard to performance, it was expected that affirmed narrow participants would outperform control participants (replicating the classic affirmation effect) while affirmed broad participants would actually perform worse than control participants (more clearly demonstrating

the hypothesized backfiring effect). Unlike the outcome measures used in Study 3, performance is a relatively unambiguous dependent variable. In Studies 3, lower interest in the information about texting while driving and reduced behavior change intentions could reflect the presence of cognitive dissonance but could also indicate apathy or unconcern (a possible side effect of perceiving a situation as less threatening). In Study 4, on the other hand, there is no reason to believe that both increased and decreased threat could produce the same effect on performance. As in classic stereotype threat literature, I expected that individuals who were more threatened by the possibility of a negative outcome on the task would perform worse than individuals who are less concerned. If both affirmed groups outperformed the control participants, this would indicate that affirmation had the intended effect regardless of threat breadth. If instead, as predicted, affirmed participants outperformed the control group when the threat was narrow but performed worse than controls when the threat was broad, this would provide strong evidence that threat breadth impacts the efficacy of affirmation and can actually cause the intervention to hurt rather than help performance.

Methods

Power. Although several unexpected and fairly large main effects resulted in improved power for many of Study 3's analyses, both Study 2 and Study 3 suggested that the interaction between affirmation and threat breadth is only a small-to-medium-sized effect that does not increase in size as the result of deliberate experimental manipulation. As a result, the analyses of interactive effects in Studies 2 and 3 were likely underpowered. To avoid this problem in Study 4, I substantially increased the target sample size, with a goal of 75 participants per cell. With a sample of this size, I expected that all analyses would have adequate power to detect a small-to-

medium-sized interaction effect despite Study 4's 2x2x2 (affirmation condition by threat breadth by forecasting/no forecasting) design.

Participants. Participants for Study 4 were again recruited through the Amazon Mechanical Turk online job site in exchange for a payment of $0.75. A total of 445 participants (139 men, 301 women, and 5 individuals who declined to indicate sex) completed the study. The majority of participants identified as White (78.4%) and reported being between the ages of 21 and 40, with 36.0% aged 21-29 and 32.2% aged 30-39. Over two-thirds of participants (70.8%) indicated that they were currently employed either part time or full time and nearly a quarter (24.1%) reported being a full-time or part-time student. Most participants (87.3%) reported greater than a high school education, with the modal educational attainment being a Bachelor's degree (35.0%). Overall, participant demographics in Study 4 were similar to demographics for Studies 1 through 4.

Materials. Most of the materials for Study 4 made only minor modifications to the manipulations and items used in Studies 1 – 3, including the affirmation manipulation, the affective forecasting questions, and the demographics. The only substantial change from earlier studies was the inclusion of creativity performance measures. The first performance measure, included as the main dependent measure for Study 4, was a version of the Remote Associates Test (RAT), which presents participants with a series of three word groups that appear to be unrelated but share an association with a common fourth word (for example the three words SENSE, COURTESY, and PLACE all share an association with the fourth word COMMON, which is one correct solution to this RAT item). The modified RAT used in this study included 15 word problems drawn from various sources (Bowers, Regehr, Balthazard, & Parker, 1990; Mednick & Mednick, 1967; Slepian et al., 2010). Modeling a procedure used successfully by

another research lab (J. Huntsinger, personal communication, October 8, 2014), I presented RAT items one at a time and allowed participants a limited time to enter a solution to each item (based on pretesting, 20 seconds was selected as an appropriate time limit for this sample). As several participants commented at the end of the study, this test is quite challenging, but most participants were able to answer a few of the items correctly and there was good variability in scores. See Appendix D for the specific RAT items used and the answer used for scoring.

Although I intended the RAT as my primary performance measure—and for that reason always asked participants to complete the RAT first in the study order—I felt it might be a stressful task for some participants and wanted to include a second performance measure that might be perceived as easier. I also wanted this to be a test for which performance was less obvious, so that all participants might have a chance to feel that they performed well (given that they actually completed the task). For this reason, I included a single-item version of Guilford's Alternative Uses Test (AUT; Guilford, 1967), in which participants were given two minutes to list as many uses as they could think of for an everyday object (in this case, a brick). The traditional scoring methods for the AUT involve numerous dimensions to assess the overall creativity of the response (originality, fluency, flexibility, and elaboration), but I was most interested in simpler metrics for examining AUT responses that might get more at participant motivation and engagement as opposed to creative performance (for example, word count and number of uses listed). Primarily, the measure was included to reduce anxiety that may have resulted from completing the more demanding RAT items.

In addition to developing a creativity performance measure for Study 4, I also needed to develop a new manipulation of breadth that focused on creativity. In a pretest, I asked participants to list areas of their lives that were impacted by or depended on creativity. Based on

these open-ended responses, I developed a list of 18 life domains that participants repeatedly identified as areas of creative expression (see Appendix E). This list was developed into a narrow and broad framing manipulation. In the narrow framing condition, participants were asked to "select the ONE area of YOUR OWN life that allows your creativity to come through the most". In the broad framing condition, on the other hand, participants were instructed to "select ALL of the areas of YOUR OWN life that are impacted by or depend on creativity". A pretest revealed that participants selected numerous life areas when given the opportunity in the broad framing manipulation (M = 7.56, SD = 4.72). Two manipulation check items confirmed that participants who received the broad framing treatment, when compared to participants who received the narrow framing, felt that creativity impacted more areas of their lives (M_{broad} = 4.88 vs. M_{narrow} = 3.94 on a 6-point scale, t (65) = 2.48, p = .016) and were more likely to report that their own creative ability was spread out across many different areas of life as opposed to concentrated in one major area (M_{broad} = 5.26 vs. M_{narrow} = 4.29 on a 7-point scale, t (64) = 2.08, p = .041).

Finally, in order to identify possible mediators of a differential effect of threat breadth, I included a brief social desirability scale as a measure of general defensiveness (Strahan & Gerbasi, 1972) and two questions that were intended to assess openness – the first asked participants to rate their willingness to reading some tips about increasing creativity (6-point Likert scale, 1 = *very unwilling*, 6 = *very willing*) and the other gave participants the option of viewing the correct responses to the RAT (binary yes/no response scale). The two openness indicators were directly related to the threatening content of the study, while the defensiveness measure was more general.

Procedure. After being recruited through Amazon's Mechanical Turk, participants were randomly assigned to complete an affirmation or a control writing task, as in the prior studies. Next, participants were told that one goal of the study was to learn more about creativity and were randomly assigned to receive either a narrow or a broad framing of creativity. In the narrow framing, participants were asked to choose from a list of life domains the ONE area of their life in which their creativity was expressed the most. In the broad framing, participants selected ALL of the life areas from the same list of domains that they felt depended on or were impacted by creativity. In this way, participants were induced to focus in on either a single, key area of their life that was impacted by creativity (narrow framing) or on the many diverse areas of life that all use creativity in some way (broad framing).

After receiving an affirmation and framing manipulation, half of the participants were told that they would soon complete a creativity test and were asked to forecast how they would feel if they did poorly or well on that test (using a modified version of the forecasting items from Studies 1 through 3). Because previous research has shown that the simple act of providing a forecast can impact subsequent behavior (Hahn, Wilson, McRae, & Gilbert, 2013), I randomly assigned half of the participants to make affective forecasts about their performance on a creativity test and half not to make forecasts. Next, participants completed two creativity tasks, the Remote Associates Test (RAT) as described earlier, and an Alternative Uses Test (AUT) in which participants listed as many alternate uses for a brick as they could think of in two minutes. Finally, participants completed the measures of openness and defensiveness and provided basic demographic information.

Results and Discussion

Repeat participants and outliers. During pretesting for this study, I learned of a method for eliminating repeat participants from related studies conducted on Amazon Mechanical Turk. This new method adds additional HTML script (which can be developed at the following location: https://uniqueturker.myleott.com; Unique Turker, 2014) to the survey invitation page that flags each unique participant who enters the study and prevents repeat participation by the same Mechanical Turk worker ID. If the same HTML code is added to related studies within Mechanical Turk, the flags created by the script will also prevent the same participant from completing multiple related studies. Flags for past participants can also be created retroactively to prevent them from completing later studies (using the procedure described on the following site: http://www.tylerjohnburleigh.com/?p=496; Burleigh, 2014). This new method of excluding past participants and preventing repeat participation in the same study was implemented in Study 4. Although this method may not identify participants who are able to repeat a study or studies more than once by having multiple Mechanical Turk worker accounts, it is preferable to using IP addresses to examine potential duplicates retroactively (especially since IP addresses change regularly and are also reused by internet service providers to identify different devices). Examining the information available directly from Mechanical Turk for Study 4, I was not able to identify any duplicate worker IDs either within Study 4 or across the prior studies (although there were a few IP addresses that repeated from prior studies). No participants were excluded from the analyses based on potential repeat status.

Performance. The main performance results are presented in Table 6. After an initial examination that showed no significant difference in RAT scores based on being assigned to forecast or not ($M_{forecast}$ = 4.68 vs. M_{none} = 4.26, $t(443) = 1.34$, $p = .179$) or any interaction with the experimental conditions (3-way interaction with affirmation condition, breadth framing, and

forecasting: $F(1, 434) = .018$, $p = .893$), I combined the performance scores of participants who provided forecasts and those who did not for all remaining analyses. Another factor—self-reported prior experience with the RAT—was found to affect RAT performance, $M_{experienced} = 5.71$ vs. $M_{inexperienced} = 3.85$, $t(441) = 5.78$, $p < .001$, Cohen's $d = .55$. Rather than dropping participants who reported prior RAT experience (33.9% of participants), I chose to include it as a covariate in the performance analysis. The results are very similar and in fact are more significant when experience is not included as a covariate.

[Table 6 about here]

As predicted, and as seen in Table 6, there was a significant interaction of affirmation condition and breadth framing on Remote Associates Test scores, $F(1, 438) = 4.70$, $p = .031$, partial $\eta^2 = .011$.[2] As hypothesized, affirmed participants performed better when they received the narrow framing of the threat ($M = 4.84$, $SD = 3.29$) than when they received the broad framing of the threat ($M = 4.06$, $SD = 3.38$), indicating a backfiring effect for affirmation when the threat is perceived to be broad (see Figure 8). Consistent with the findings from Studies 2 and 3, this interactive effect was fairly small in size. When looking only at affirmed participants in a regression analysis that controlled for prior RAT experience, threat breadth framing was not a significant predictor of RAT scores, $B = -0.62$ ($SE = 0.46$), $t(206) = -1.36$, $p = .175$. Comparing simple means alone (without controlling for RAT scores), this difference across threat breadth for affirmed participants is marginally significant, $t(207) = -1.70$, $p = .090$.

[Figure 8 about here]

[2] This reported result is from an ANCOVA that controls for self-reported prior experience with the RAT. When not controlling for this additional explanatory factor, the interaction between affirmation condition and breadth framing was even more significant, $F(1,441) = 7.53$, $p = .006$, partial $\eta^2 = .017$.

Contrary to my initial expectations but consistent with the pretest of the framing manipulation, there was also an effect of threat framing on control participants that was the reverse of the finding for affirmed participants. This interaction can be seen in Figure 8. Specifically, when participants were not affirmed, receiving the broad threat framing was actually associated with improved performance (M = 4.90, SD = 3.47) compared to the narrow framing (M = 3.95, SD = 3.07). When looking just at control participants in a regression analysis that controlled for prior RAT experience, threat breadth framing was a marginally significant predictor of RAT scores, B = 0.72 (SE = 0.42), $t(235) = 1.71$, p = .088. Comparing simple means alone (without controlling for RAT scores), this difference across threat breadth for control participants is significant, $t(234) = 2.20$, p = .029. The reason for the relationship between threat breadth and performance in the control condition is unknown, although exploring this effect would be an interesting direction for future research.

With regards to the "easier" AUT task that followed the RAT, there were no significant differences across affirmation condition and threat framing for number of words written ($F(1, 441) = 0.02$, *ns*), number of uses listed ($F(1, 441) = 0.81$, *ns)*, or log time spent on the task ($F(1, 440) = 1.75$, *ns*). No specific hypotheses were proposed for the AUT, but interactions on any of these measures might have given clues about potential motivational differences across the conditions.

Further exploration of the interaction for RAT performance suggested that one important factor explaining the poorer RAT scores of affirmed participants in the broad framing condition might be "freezing" (i.e., being unable to solve any of the test items). Chi-square analyses looking at the number of participants in the various treatment conditions who received a score of zero as opposed to solving at least one RAT item did not show a significant difference in the

distribution for the control participants ($\chi 2$ (4, N = 236) = 2.27, p = .147) but did show a significant difference for affirmed participants ($\chi 2$ (4, N = 236) = 6.06, p = .019, φ = .16). Specifically, affirmed participants were more likely to receive an RAT score of zero when they received a broad framing of the threat (21.2%) than why they received a narrow framing (9.1%). The proportions for all cells are reported in Table 7. The interactive effect was also significant in a logistic regression that controlled for prior RAT experience, Wald = 5.11, p = .024.

[Table 7 about here]

This "freezing" effect did not result in lowered effort; analysis of the log-transformed time spent on the RAT items for participants who received a zero score revealed that this effect was NOT due to affirmed participants who received the broad framing abandoning the task more quickly than other participants (F(1,61) = 0.70, p = *ns*). If anything, among participants who did not get any items correct on the RAT, affirmed participants who received a narrow framing spent less time on the items (M = 149.83 seconds, SD = 42.93) than participants who received the broad framing (M = 180.96 seconds, SD = 65.95). Based on this evidence, it seems that affirmed participants who perceived a broadly-framed threat were more likely than other participants to freeze when faced with a difficult creativity test. This further supports my hypothesis that affirmations backfires when combined with a broad threat.

Because all participants received a narrow or broad framing of the creativity test, it is uncertain what the average performance would have been for control participants who received no framing manipulation. Because the narrow framing seems conceptually most similar to no manipulation, I expect that the scores of entirely unmanipulated participants would be similar to the mean for control participants who received this framing, or perhaps scores would instead fall

somewhere in between the scores of control participants in the narrow and broad framing conditions.

Regardless of what the "true control" performance on the RAT may have been, it seems clear that the predicted backfiring effect of perceiving a broad threat while affirmed did occur, as RAT scores were lower for affirmed participants who received a broad framing as opposed to a narrow framing (while the reverse was observed for non-affirmed participants).

Affective forecasting. The affective forecasting results are presented in Table 8. Based on the results of Study 3, I predicted that affective forecasts in Study 4 would be more moderate for affirmed participants regardless of assignment to the narrow or broad framing conditions. Whether looking at simple t-tests (affirmed vs. control) or 2x2 ANOVAs (affirmation condition by framing condition), there is a clear main effect of affirmation on affective forecasting regardless of threat framing. However, this effect is in the opposite direction from my predictions. Across numerous forecasting items, affirmed participants provided more extreme forecasts than control participants. That is, affirmed participants thought they would be more upset if they performed poorly and happier if they performed well compared to control participants.[3]

The main effect of condition was significant for predicted immediate upset after poor performance ($F(1, 218) = 6.18$, $p = .014$, partial $\eta^2 = .028$), upset one day after poor performance ($F(1, 217) = 10.71$, $p = .001$, partial $\eta^2 = .047$), immediate happiness after good performance ($F(1, 215) = 10.59$, $p = .001$, partial $\eta^2 = .047$), happiness one day after poor performance ($F(1,$

[3] Due to an oversight, creativity was not removed from the list of values that participants could choose to write about in the affirmation manipulation. Although I did not have an a priori hypothesis that this would impact the results, I have made an effort in the prior studies to separate threat breadth from threat overlap with the affirmed value. Fortunately, only 12 participants chose to affirm based on creativity and no participants selected creativity as their least important value for the control condition. Running the analyses without these 12 participants did not have any effect on the pattern of results, so I elected to include them in the reported analyses.

216) = 9.07, p = .003, partial η^2 = .040), and self-reported importance of the creativity test (F(1, 218) = 4.10, p = .044, partial η^2 = .018). The effect was marginal for perceived likelihood of performing well on the creativity test (F(1, 216) = 3.63, p = .058, partial η^2 = .017), and there was no significant difference across conditions for perceived likelihood of performing poorly on the creativity test (F(1, 218) = 0.43, *ns*). Also in contrast to my predictions based on Study 3, there was no evidence of moderated mediation for the affective forecasts. Responses to my defensiveness and openness items did not appear to vary by affirmation condition and breadth (although there was a slight trend for affirmed participants to be less defensive) and moderated mediation analyses (that is, the effect of affirmation condition on forecasts moderated by breadth framing and mediated by defensiveness or openness) showed no mediating effect of defensiveness or openness.

[Table 8 about here]

Although the relationship between affirmation and forecasting was in the opposite direction of my prediction, it appears to be a strong, small-to-medium-sized effect that is reliable across different forecasting measures. With the available data, I cannot determine why the pattern of forecasting results was different in Study 4 compared to the prior studies. One possibility relates to the breadth of the threat. In Study 2, I found that forecasts were more extreme for affirmed participants when the threat was framed broadly, though this was not replicated in Study 3. Perhaps the creativity performance threat used in Study 4 was perceived to be fairly broad by all participants, explaining the more extreme forecasts for affirmed participants (note, however, that this explanation does not appear to be consistent with the performance results reported above).

Another possibility is that the relationship between affirmation and forecasting is different for a potential future threat (Studies 1-3) as opposed to a definite immediate threat (Study 4). When a threat is distant, the results from Studies 1 through 3 suggest that affirmation results in more moderate forecasts. But perhaps this effect is flipped when a threat is imminent: the potential downsides as well as the potential rewards associated with the threatening situation are even more salient to affirmed individuals. It is noteworthy that the only forecasting item that showed no hint of an effect across conditions in Study 4 was the perceived likelihood of performing poorly on the creativity test. While affirmed individuals perceived the test as more important and reported marginally higher likelihood of performing well on the test, they were not more extreme about the probability of a bad outcome. When currently facing a threat, it could be that affirmed individuals actually become more invested in the outcome than non-affirmed people but differ in their positivity (that is, they have greater expectations of a good outcome). This explanation could also be consistent with the aforementioned theory of Morewedge and Buechel (2013) that affective forecasting biases have beneficial effects on motivation. On the other hand, there could be some alternative intervening factor that I have not yet identified. As discussed earlier, it is possible for several different paths to lead to the same affective forecasting results, which is why I also included a performance task in this study as the primary dependent measure.

Openness and defensiveness. The measures of defensiveness and openness did not appear to interact with affirmation condition and threat breadth. Defensiveness scores were slightly lower for affirmed participants compared to controls, although this difference was not significant (M_{aff} = 2.72 vs. M_{cont} = 2.88, $F(1,441) = 1.47$, *ns*), but there was no hint of an interaction with threat breadth ($F(1,441) = 0.01$, *ns*). Nor was there any interaction with

willingness to read tips about improving creativity ($F(1,440) = 0.02$, *ns*) or the binary decision to view the RAT answers ($\chi^2(4, N = 445) = .96$, *ns*). If affirmation did interact with threat breadth by affecting defensiveness and openness, these measures did not capture the effect. It should be noted, however, that these measures were very brief (in consideration of total study time) and were presented at the end of the study. Perhaps more elaborate indicators or those presented closer to the affirmation and breadth manipulations (like the affective forecasting measures in this study) would show the expected interactive effect.

Moderated mediation. Because performance was affected by an interaction between affirmation condition and breadth framing and therefore there was no main effect of either manipulation, a simple mediation analysis would not be informative. Although it seemed improbable that affective forecasts would be found to mediate the relationship between affirmation and threat breadth and RAT performance, given that forecasts showed only a main effect of affirmation, I nevertheless conducted a series of moderated mediation analyses to evaluate this possibility. Controlling for prior RAT experience, I computed models with affective forecasts as the mediator variables for the relationship between affirmation condition interacted with breadth framing on RAT performance. None of the indirect effects were significant, however, indicating that affective forecasts (including immediate positive and negative affect, positive and negative affect one day later, and likelihood of performing well and poorly) did not mediate the relationship between the crossed experimental conditions and RAT performance. Had an effect been observed, this would have indicated that affective forecasts mediated the relationship between affirmation condition and performance differentially for the narrow and broad framing conditions. I also found no effect using self-rated importance of the creativity test as the mediator. These results indicate that the unexpected extreme affective

forecasts for affirmed participants in Study 4 do not explain the RAT scores for participants whether they received the narrow or the broad framing.

General Discussion

The primary goals of this research were (1) to explore a new possible mechanism for the effects of an important psychological intervention—self-affirmation, and (2) to explore the limits of this popular intervention. By making people aware of the many other determinants of future happiness and the resources they have to combat negative emotions, I hypothesized that self-affirmation might reduce people's tendencies to make extreme predictions about their future emotions and thereby reduce the distress associated with the threatening situation. When the severity of threats is put into proper perspective, individuals may be better able to respond in positive and productive ways to negative information.

Although it is well-established that affirmation *can* have beneficial consequences, the limitations of self-affirmation are relatively unknown. Are there specific characteristics of the threat that determine whether affirmation is helpful or harmful to openness, performance, motivation, or behavior change? In addition to investigating affective forecasting as a potential mechanism for self-affirmation's effects, the present research also considers the impact of a key aspect of the targeted threats: breadth. Collectively, the findings presented in this paper indicate that (1) there is a relationship between affirmation and affective forecasting; (2) contrary to my hypotheses, this relationship does not predict the effectiveness of affirmation for preventing cognitive dissonance or improving performance in a straightforward manner; and (3) threat breadth is a potentially important moderator of the effects of affirmation that could result in unintended backfiring.

In terms of outcomes of affirmation that have been explored in prior research, Study 3 offered mixed results regarding the interactive effects of affirmation and threat breadth. Some outcome measures suggested a straightforward effect of affirmation—affirmed participants were less anxious and fearful while reading potentially threatening facts about texting while driving and were overall less interested in smartphone applications to help reduce texting while driving behavior—while others, in particular perceived believability of the texting while driving facts and perceived importance of changing texting while driving behavior—suggested an interaction between affirmation and threat breadth (see Figures 6 and 7). Interactions for believability and importance of behavior change, as well as differential mediation of the other observed effects by responses to these items, overall suggested that there was an interactive effect of affirmation and threat breadth but that this interaction sometimes produced similar responses on the self-report items (see Figure 9).

[Figure 9 about here]

Study 4, which examined performance as an alternative outcome of affirmation that presumably would not show similar outcomes in response to different psychological effects, supported an interactive relationship between affirmation and threat breadth. Specifically, I found that affirmed participants who thought broadly about the importance of creativity in their lives performed worse on a brief Remote Associates Test compared to participants who thought more broadly about the importance of creativity, while this pattern was reversed for participants who were not affirmed (see Figure 8). Because performance is less constrained by the limitations of self-report, the results of Study 4 provide stronger evidence for a potential backfiring effect of affirmation when the threat is broad. Although the size of this interactive

effect seems to be fairly small, the practical implications are large if threat breadth could make the difference between an affirmation intervention being helpful and being harmful.

In addition, all four studies reported above provide evidence that there is some relationship between affirmation and affective forecasting, although the results are not as straightforward as hypothesized. In Study 1, affirmation generally led to more moderate affective forecasts, except in the domain of negative health outcomes (see Table 1). In the face of a negative health threat, affirmed participants seemed to give more extreme forecasts than control participants. Follow-up research suggested threat breadth as a possible explanatory factor (see Table 2). When I further examined this backfiring effect of affirmation on forecasting for broad health threats in Study 2, I replicated the unexpected result. While there was little evidence in Study 2 that affirmation actually *decreased* extreme forecasting for a narrow threat, the pattern for the broad threat was fairly consistent—affirmed participants were *more* extreme in their forecasts than control participants (see Figure 4).

Extending the research beyond a personal bad health outcome, Study 3 examined the relationship between affirmation and forecasting for a hypothetical negative scenario involving texting while driving. Contrary to expectations, forecasting results in this study did not appear to vary by threat breadth. Instead, all affirmed participants gave more moderate forecasts (see Figure 6). Responses to some items (specifically believability of the texting while driving facts and perceived importance of behavior change), however, suggested that participants in the broad and narrow framing conditions might have been getting to their affective forecasting responses through different psychological paths. Specifically, the forecasts for affirmed participants with a narrow framing seemed to be mediated by greater openness to the threatening information. Forecasts for affirmed participants with a broad framing, on the other hand, appeared to be

mediated instead by defensiveness (see Table 3). Overall, the results for affective forecasts in Study 3 supported my initial hypothesis that affirmation leads to more moderate forecasts but called into question the presence of an interactive effect of affirmation and threat breadth on forecasting.

Finally, Study 4 explored the link between affirmation and forecasting in a performance situation. In contrast to Studies 1 through 3 and contrary to expectations, affirmed participants in Study 4 actually provided more extreme predictions for all affective forecasting items as compared to control participants. The effect seemed to be consistent and strong, but the available data do not provide a clear explanation for the unexpected result. Perhaps there is something unique about forecasting related to a performance threat that differs from forecasting for other types of threat, or possibly affirmed participants respond differently to forecasting items for an immediate threat as opposed to a distant threat. While the effect was in an unexpected direction, the results of Study 4 continue to support the theory that affirmation and affective forecasting are related (albeit maybe in a fairly complicated manner). However, despite Study 3's promising mediation results, Study 4 did not find any evidence for a mediating role of affective forecasts in the relationship between affirmation and performance even when allowing for differential effects by breadth framing.

Several key limitations of the present research should be acknowledged and, ideally, addressed in future research. The first is the exclusive collection of data online via Amazon's Mechanical Turk. Although participants on this site appear to be motivated to provide quality data and are demographically more diverse than an undergraduate student population, it is impossible for researchers to observe participants who complete studies online or to control the external environment that participants experience. Additionally, many participants on

Mechanical Turk have already taken part in dozens or even hundreds of other psychological research studies. Based on self-reported responses alone, I observed that as many as one third of the participants in my studies have at some point completed another affirmation study of some kind. Although the results of the above studies did not differ when I excluded participants with prior affirmation experience (aside from being somewhat less significant due to smaller sample sizes), it is unknown what effect being a "professional participant" might have on an individual's reaction to psychological experiments. Conducting lab studies with undergraduate participants does not provide a perfect solution to these problems, of course, but replicating my results with different types of samples and using different data collection methods is important for understanding generalizability.

An additional limitation of the reported research is the extensive reliance on affective forecasting for hypothetical future outcomes. Since the one study that included forecasts for a more immediate threat that participants knew they would experience (Study 4) found opposite results for the effect of affirmation on forecasting, it is important for future research to examine forecasting for threats that are not hypothetical and to tease out any differential effects of affirmation on forecasts for immediate versus future threats. Incorporating affective forecasting into existing popular paradigms for exploring the benefits of affirmation, such as randomly assigning students to an affirmation or control condition and then observing differences in academic performance over time, would provide an ideal opportunity to address this limitation.

Finally, the present research is limited in its ability to test affective forecasting as a potential mediator of affirmation's effects because of the incorporation of threat breadth as an additional element in the reported studies. My ability to detect a mediational relationship was substantially reduced by the presence of an interaction between affirmation and threat breadth on

outcomes. Future research studies that focus exclusively on affective forecasting as a mediating variable should be conducted that elicit a reliable and easily-measured effect of affirmation, do not include additional potential moderators of the effect, and have sufficient power to detect a mediating effect.

I believe the program of research presented here provides a unique contribution to the self-affirmation literature. Not only did I examine a new psychological consequence of affirmation and a possible mediator of its effects (affective forecasting), but I also sought to improve the field's understanding of the limitations of self-affirmation and the conditions under which it might not be beneficial. Much of the published affirmation literature has focused on discrete threats that are somewhat narrow in nature (such as the threat of poor performance in an academic course). Additionally, traditional paradigms do not make specific mention of the threat or call attention to its potential consequences when implementing a values affirmation intervention, so that perceived threat breadth is unlikely to be manipulated. In the well-known affirmation studies in which Cohen and colleagues (2006) were able to reduce the racial achievement gap by implementing a simple values affirmation intervention in the classroom, for example, the possible academic threat experienced by minority students was never directly mentioned and was most likely not something the students consciously evaluated during the course of the study. In at least one of the reported studies, the affirmation intervention was presented only at the beginning of the term (in the replication study, the intervention was repeated at an unspecified time during the academic term) and was chronologically dissociated from the key measures of performance, such as tests. In that kind of research context, there is no reason to expect that threat breadth would become salient to participants or that there would be any systematic variation among participants in the perceived breadth of threat. The present

research may not indicate any need to reconsider the design of such studies, although it does suggest that caution should be exercised when researchers change standard paradigms in ways that might impact perceptions of the threat.

In other popular research contexts for values affirmation, however, such as interventions to improve health outcomes, many minor modifications to the research design are likely to impact perceived threat breadth. For instance, in the Logel and Cohen (2012) study examining the relationship between values affirmation and women's body weight, the health threat posed by overweight was presumably much more salient to participants throughout the course of the study, including immediately after the affirmation intervention, than it was for the students in the research of Cohen and colleagues. Not only were participants asked explicit questions about their satisfaction with their weight, but the women were also asked to step on a scale during the course of the first lab session. Although the researchers observed a beneficial effect of affirmation on weight loss in this study and there is nothing in the description of the procedure that suggests the authors inadvertently manipulated threat breadth (by making weight seem to be either a more narrow or broad issue), it is easy to imagine how such a manipulation could have occurred and might occur in similar studies. For instance, if the authors had decided to include any questionnaire items that assessed the impact of weight on happiness in various life domains, this could have caused an accidental broadening of the threat that might have eliminated the positive effects of affirmation or even led to a backfiring effect. Or imagine that the addition of a free writing task allowing women to express their concerns about their weight caused some women to broaden and others to narrow the threat spontaneously, resulting in the apparent absence of an affirmation effect. The research presented in this paper indicates that even minor manipulations of threat breadth can have an impact on affirmation's effects. As a result,

affirmation research that explicitly mentions and measures reactions to the target threat faces a substantial risk of affecting results via unintentional variation in perceived threat breadth.

Making researchers aware of potential moderators of affirmation's effects, including threat breadth, should allow for more careful and successful research designs. The findings presented in this paper should also encourage researchers who have conducted unsuccessful self-affirmation studies in the past to re-examine their research paradigms, considering breadth as a key factor that may impact results. Given the somewhat mixed results in the literature on self-affirmation and health, particularly when it comes to positive health behavior change, it might also be worthwhile to reconsider studies (see Harris & Epton, 2009 for review) that have failed to find a beneficial effect of affirmation on health behavior in the context of threat breadth. Self-affirmation may not be psychology's silver bullet intervention, but it certainly promises to have wide applicability while requiring minimal resources to implement. Thus, gaining a better understanding of its consequences and constraints should help researchers to apply self-affirmation with greater effectiveness in future studies as well as in applied settings.

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Appendix A

Affirmation Manipulation Used in All Studies

Affirmation Condition:

Page 1

Below is a list of characteristics and values, some of which may be important to you, some of which may be unimportant. Please mark the value/quality that is MOST IMPORTANT TO YOU.

- Being good at art
- Creativity
- Relationships with family and friends
- Government or politics
- Independence
- Learning and gaining knowledge
- Athletic ability
- Belonging to a social group (such as your community, racial group, or school club)
- Music
- Spiritual or religious values
- Sense of humor

Page 2

Instructions: We would like you to think carefully about your responses on this page, so we have included a timer that will not allow you to move on to the next portion of the survey until at least 1 minute has passed. After 1 minute, the arrow will appear allowing you to move on to the next page if you are ready, although we usually find that is takes people 3 or 4 minutes to answer this question fully.

Now, please write a couple sentences about why your MOST IMPORTANT VALUE, [returns value selected on previous page], is IMPORTANT TO YOU. Write as much or as little as you wish, and don't worry about how well it's written. Just focus on expressing the importance of the value to you.

[RESPONSE ENTERED IN TEXT BOX]

Extended Affirmation (Studies 2, 3, and 4)

Next, please take a few minutes to write about three or four personal experiences you have had when the value "[returns value selected on previous page]" was important to you and made you feel good about yourself.

[RESPONSE ENTERED IN TEXT BOX]

Control Condition:

Page 1

Below is a list of characteristics and values, some of which may be important to you, some of which may be unimportant. Please mark the value/quality that is LEAST IMPORTANT TO YOU.

- Being good at art
- Creativity
- Relationships with family and friends
- Government or politics
- Independence
- Learning and gaining knowledge
- Athletic ability
- Belonging to a social group (such as your community, racial group, or school club)
- Music
- Spiritual or religious values
- Sense of humor

Page 2

Instructions: We would like you to think carefully about your response on this page, so we have included a timer that will not allow you to move on to the next portion of the survey until at least 1 minute has passed. After 1 minute, the arrow will appear allowing you to move on to the next page if you are ready.

Now, please write a couple sentences about why your LEAST IMPORTANT VALUE, [returns value selected on previous page], might be IMPORTANT TO SOMEONE ELSE. Write as much or as little as you wish, and don't worry about how well it's written. Just focus on expressing the importance of the value to others.

[RESPONSE ENTERED IN TEXT BOX]

Appendix B

Study 2 Narrow and Broad Scenarios

Narrow Scenario – Study 2:

Take a few moments to imagine the following scenario: You have come down with a bad case of the flu. You have a fever of 102 degrees, severe achiness, and general discomfort that lasts for 5 days. You get sick right before you have a week off from work or school. You were planning on spending a quiet vacation at home, so becoming sick does not affect your work or school performance. You were really looking forward to relaxing around the house. Instead, you spend most of the week in bed, feeling pretty miserable.

Broad Scenario – Study 2:

Take a few moments to imagine the following scenario: You have come down with a bad case of the flu. You have a fever of 102 degrees, severe achiness, and general discomfort that lasts for 5 days. You get sick right before you have a big project due at work or school. Also, you have to miss several activities that you had scheduled for the weekend, including three or more of the following (whichever you would be most likely to do in your free time): volunteer work, religious services, an athletic event, exercise, gardening, fixing up your house, spending time on your hobbies.

Appendix C

Study 3 Narrow and Broad Scenarios

Narrow Scenario – Study 3:

Take a few moments to imagine the following scenario: You are driving your car and you hear that you've received a new text message on your cell phone. At a stop light, you read the text and start to reply. The light turns green so you finish the text while you resume driving. Just when you are about to press send, you realize that you have drifted out of your lane and are about to crash into the guard rail. You attempt to correct your direction but it is too late and you hit the rail going 25 MPH. No one is hurt in the accident, but you do receive a $100 fine from the county government for the damaged guard rail and have to pay $100 to fix a scratch on the fender of your car. Your car is towed and the mechanic is able to have it ready for you to pick up the next day.

Broad Scenario – Study 3:

Take a few moments to imagine the following scenario: You are driving your car and you hear that you've received a new text message on your cell phone. At a stop light, you read the text and start to reply. The light turns green so you finish the text while you resume driving. Just when you are about to press send, you realize that you have drifted out of your lane and are about to crash into the guard rail. You attempt to correct your direction but it is too late and you hit the rail going 25 MPH. No one is hurt in the accident, but you do receive a $100 fine from the county government for the damaged guard rail and have to pay $100 to fix a scratch on the fender of your car. Your car is towed, but the mechanic is not able to have it ready for you to pick up until two weeks later. This causes you to miss several activities that you had scheduled for the next few weeks, including three or more of the following (whichever you would be most likely to do in your free time): volunteer work, religious services, an athletic event, exercise, gardening, attending a play or show, spending time on your hobbies.

Appendix D

Study 4 RAT Items and Solutions

Sense Courtesy Place = Common
Main Sweeper Light = Street
Carpet Alert Ink = Red
Hound Pressure Shot = Blood
Basket Eight Snow = Ball
Pie Luck Belly = Pot
Falling Actor Dust = Star
Gold Stool Tender = Bar
Blood Music Cheese = Blue
Envy Golf Beans = Green
Strike Same Tennis = Match
Note Dive Chair = High
Stalk Trainer King = Lion
Shopping Washing Picture = Window
Sore Shoulder Sweat = Cold

Appendix E

Study 4 Life Domains for Creativity Breadth Framing*

1. Career/Job
2. Academic Achievement
3. Relationships
4. Childcare/Parenting
5. Artistic Ability
6. Musical Ability
7. Organization
8. Finances/Budget
9. Decorating/Interior Design
10. Event Planning
11. Games/Sports
12. Cooking
13. Fashion/Personal Appearance
14. Hobbies
15. Writing
16. Problem Solving
17. Spirituality/Faith
18. Home Improvement/Car Maintenance

*Note that these domains were presented to participants in a random order

Table 1

Average Forecast by Scenario Type – Study 1

Scenario Type	Forecast	Condition	Mean (SD)	t-value	p-value
Professional Good	Immediate Impact	Affirmed	6.10 (1.52)	2.00*	.047
		Control	6.51 (0.85)		
	One Week Impact	Affirmed	5.75 (1.61)	1.25	.214
		Control	6.04 (1.17)		
	Likelihood	Affirmed	4.58 (1.88)	1.04	.302
		Control	4.90 (1.85)		
Professional Bad	Immediate Impact	Affirmed	5.61 (1.74)	2.52*	.013
		Control	6.23 (1.20)		
	One Week Impact	Affirmed	4.93 (1.79)	1.81+	.072
		Control	5.43 (1.53)		
	Likelihood	Affirmed	3.62 (1.87)	.02	.987
		Control	3.63 (1.78)		
Financial Good	Immediate Impact	Affirmed	6.16 (1.42)	.53	.596
		Control	6.28 (1.21)		
	One Week Impact	Affirmed	5.73 (1.57)	-.28	.781
		Control	5.68 (1.53)		
	Likelihood	Affirmed	4.79 (1.96)	-.19	.846
		Control	4.73 (1.86)		
Financial Bad	Immediate Impact	Affirmed	5.58 (1.74)	2.30*	.023
		Control	6.15 (1.23)		
	One Week Impact	Affirmed	5.03 (1.80)	1.75+	.082
		Control	5.51 (1.57)		
	Likelihood	Affirmed	3.67 (1.95)	2.13	.035
		Control	4.35 (1.92)		
Health Good	Immediate Impact	Affirmed	5.93 (1.37)	1.82+	.071
		Control	6.31 (1.18)		
	One Week Impact	Affirmed	5.62 (1.47)	.39	.697
		Control	5.72 (1.48)		
	Likelihood	Affirmed	4.85 (1.90)	.78	.438
		Control	5.08 (1.68)		
Health Bad	Immediate Impact	Affirmed	6.18 (1.41)	-1.41	.160
		Control	5.84 (1.52)		
	One Week Impact	Affirmed	5.65 (1.76)	-1.06	.292
		Control	5.33 (1.91)		
	Likelihood	Affirmed	3.84 (1.84)	.76	.448
		Control	4.07 (1.84)		

+ $p < .10$, * $p < .05$, ** $p < .01$

Table 2

Mean Breadth Ratings by Scenario Type—Study 1 Follow-Up

Scenario	**N**	**Max**	**Min**	**Mean**	**SD**
Professional Good	140	1.00	15.00	**4.89**	2.88
Professional Bad	140	1.00	15.00	**4.34**	2.57
Financial Good	140	1.00	15.00	**4.65**	3.00
Financial Bad	139	1.00	15.00	**4.36**	2.61
Health Good	139	1.00	15.00	**4.47**	2.73
Health Bad	139	1.00	15.00	**5.20**	3.41

Table 3

Average Forecast by Condition and Scenario Type – Study 2

Forecast	Scenario Type	Condition	Mean	SD	N
Immediate Emotional Impact	Narrow	Affirmation	3.38	1.37	29
		Control	3.63	1.68	32
	Broad	Affirmation	4.57	1.76	31
		Control	4.00	1.47	37
One Week Emotional Impact	Narrow	Affirmation	2.59	1.58	29
		Control	2.56	1.61	32
	Broad	Affirmation	3.74	1.90	31
		Control	2.57	1.41	37
Likelihood	Narrow	Affirmation	3.62	1.57	29
		Control	4.69	1.51	32
	Broad	Affirmation	4.58	1.46	31
		Control	4.24	1.52	37

Table 4

Forecasts and Ratings by Condition and Scenario Type – Study 3

Measure	Scenario Type	Condition	Mean	SD	N
Immediate Emotional Impact	Narrow	Affirmation	5.77	1.33	30
		Control	6.07	1.25	29
	Broad	Affirmation	5.68	1.23	38
		Control	6.32	1.31	28
One Week Emotional Impact	Narrow	Affirmation	4.17	1.32	30
		Control	4.79	1.29	29
	Broad	Affirmation	4.29	1.54	38
		Control	5.46	1.00	28
Likelihood	Narrow	Affirmation	2.63	1.71	30
		Control	3.17	1.73	29
	Broad	Affirmation	2.79	1.36	38
		Control	2.93	1.56	28
Anxiety While Reading Facts	Narrow	Affirmation	2.76	1.15	29
		Control	2.86	1.16	29
	Broad	Affirmation	2.18	1.06	38
		Control	2.86	0.85	28
Fear While Reading Facts	Narrow	Affirmation	3.07	1.19	29
		Control	3.31	1.20	29
	Broad	Affirmation	2.53	1.18	38
		Control	3.39	0.99	28
Believability of Facts	Narrow	Affirmation	5.30	0.65	30
		Control	4.62	1.05	29
	Broad	Affirmation	4.66	1.30	38
		Control	4.96	1.29	28
Concern About Behavior Change	Narrow	Affirmation	2.90	1.24	30
		Control	2.72	1.31	29
	Broad	Affirmation	2.32	1.23	38
		Control	3.04	1.32	28
Accuracy of Fact Memory	Narrow	Affirmation	1.67	0.80	30
		Control	2.03	0.91	29
	Broad	Affirmation	1.84	0.94	38
		Control	1.79	0.83	28
Evaluation of Smartphone Applications	Narrow	Affirmation	3.29	1.30	29
		Control	3.53	1.49	29
	Broad	Affirmation	3.14	1.34	38

		Control	4.00	1.31	28

Table 5

Moderated Mediation Analyses of the Relationship between Affirmation Condition and Key Outcome Variables—Study 3

Outcome Variable	**Mediator**	**Moderator**	**Indirect Effect (Narrow)**	**Indirect Effect (Broad)**
Scenario Likelihood	Defensiveness	Scenario Type	.12 [-.32, .59]	-.50 [-1.03, -.09]*
Anxiety	Defensiveness	Scenario Type	.07 [-.11, .26]	-.20 [-.49, -.04]*
Evaluation of Apps	Defensiveness	Scenario Type	.04 [-.08, .27]	-.15 [-.44, -.02]*
Immediate Forecast	Openness	Scenario Type	.14 [.01, .35]*	-.06 [-.29, .05]
One Week Forecast	Openness	Scenario Type	.18 [.05, .42]*	-.08 [-.37, .07]
Anxiety	Openness	Scenario Type	.12 [.03, .28]*	-.05 [-.23, .05]

*Significant indirect effect

Table 6

Performance and Forecasts by Condition and Scenario Type – Study 4

Measure	Scenario Type	Condition	Mean	SD	N
RAT Score	Narrow	Affirmation	4.84	3.29	110
		Control	3.95	3.07	104
	Broad	Affirmation	4.06	3.38	97
		Control	4.90	3.47	132
RAT Time	Narrow	Affirmation	208.32	52.82	110
		Control	210.80	49.05	104
	Broad	Affirmation	205.94	54.28	97
		Control	207.22	54.93	132
AUT Word Count	Narrow	Affirmation	22.37	16.86	110
		Control	22.00	14.77	104
	Broad	Affirmation	20.51	13.81	99
		Control	19.70	14.00	132
AUT Uses Listed	Narrow	Affirmation	7.65	3.28	110
		Control	7.91	3.11	104
	Broad	Affirmation	7.58	3.67	99
		Control	7.27	3.21	132
AUT Time	Narrow	Affirmation	103.66	29.22	110
		Control	106.81	24.76	104
	Broad	Affirmation	105.91	27.64	99
		Control	103.83	28.19	132

Table 7

Distribution of Zero Scores by Condition and Scenario Type – Study 4

Scenario Type	Condition	% RAT Score = 0	% RAT Score > 0	Chi Squared	*p*-value
Control	Narrow	19.2%	80.8%	2.27	.147
	Broad	12.2%	87.9%		
Affirmation	Narrow	**9.1%**	90.9%	6.06	.019*
	Broad	**21.2%**	78.8%		

+ $p < .10$, * $p < .05$, ** $p < .01$

Table 8

Affective Forecasts by Condition and Scenario Type – Study 4

Forecast	Scenario Type	Condition	Mean	SD	N
Immediate Emotional Impact: Poor Performance	Narrow	Affirmation	3.69	1.75	59
		Control	2.98	1.55	48
	Broad	Affirmation	3.64	1.69	53
		Control	3.31	1.25	62
Delayed Emotional Impact: Poor Performance	Narrow	Affirmation	2.44	1.66	59
		Control	1.60	0.96	48
	Broad	Affirmation	2.40	1.46	52
		Control	2.03	1.23	62
Likelihood: Poor Performance	Narrow	Affirmation	3.03	1.47	59
		Control	3.15	1.34	48
	Broad	Affirmation	3.26	1.43	53
		Control	3.40	1.43	62
Immediate Emotional Impact: Good Performance	Narrow	Affirmation	5.59	1.15	59
		Control	4.92	1.33	48
	Broad	Affirmation	5.42	1.10	53
		Control	5.00	1.35	59
Delayed Emotional Impact: Good Performance	Narrow	Affirmation	4.41	1.57	59
		Control	3.71	1.60	48
	Broad	Affirmation	4.47	1.50	53
		Control	3.93	1.41	60
Likelihood: Good Performance	Narrow	Affirmation	4.83	1.43	59
		Control	4.38	1.45	48
	Broad	Affirmation	4.58	1.38	53
		Control	4.30	1.48	60
Overall Importance of Performance	Narrow	Affirmation	3.81	1.48	59
		Control	3.40	1.51	48
	Broad	Affirmation	4.08	1.37	53
		Control	3.71	1.38	62

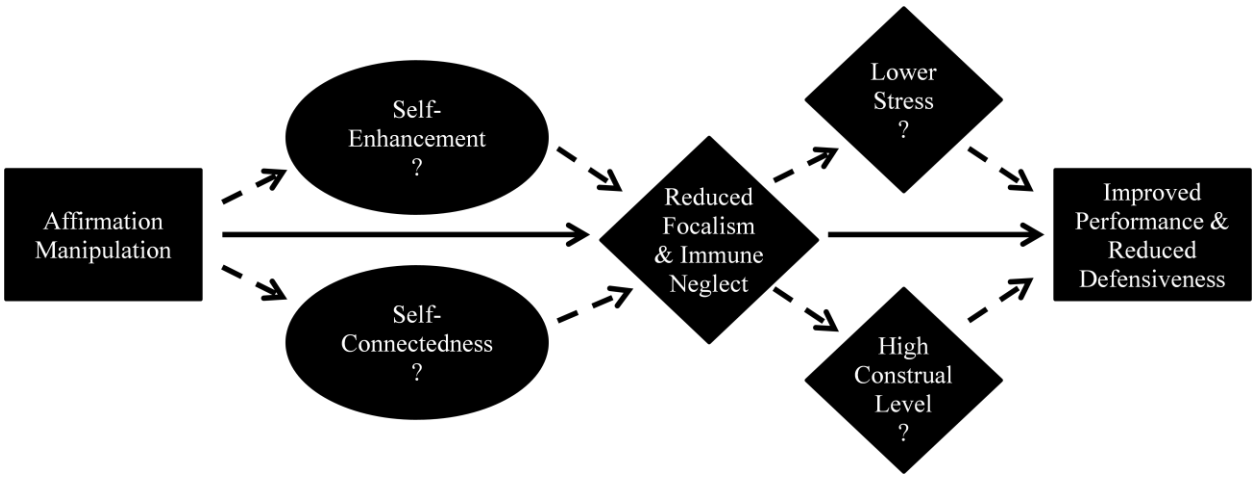

Figure 1. Path diagram depicting the proposed intermediary mechanism of affective forecasting.

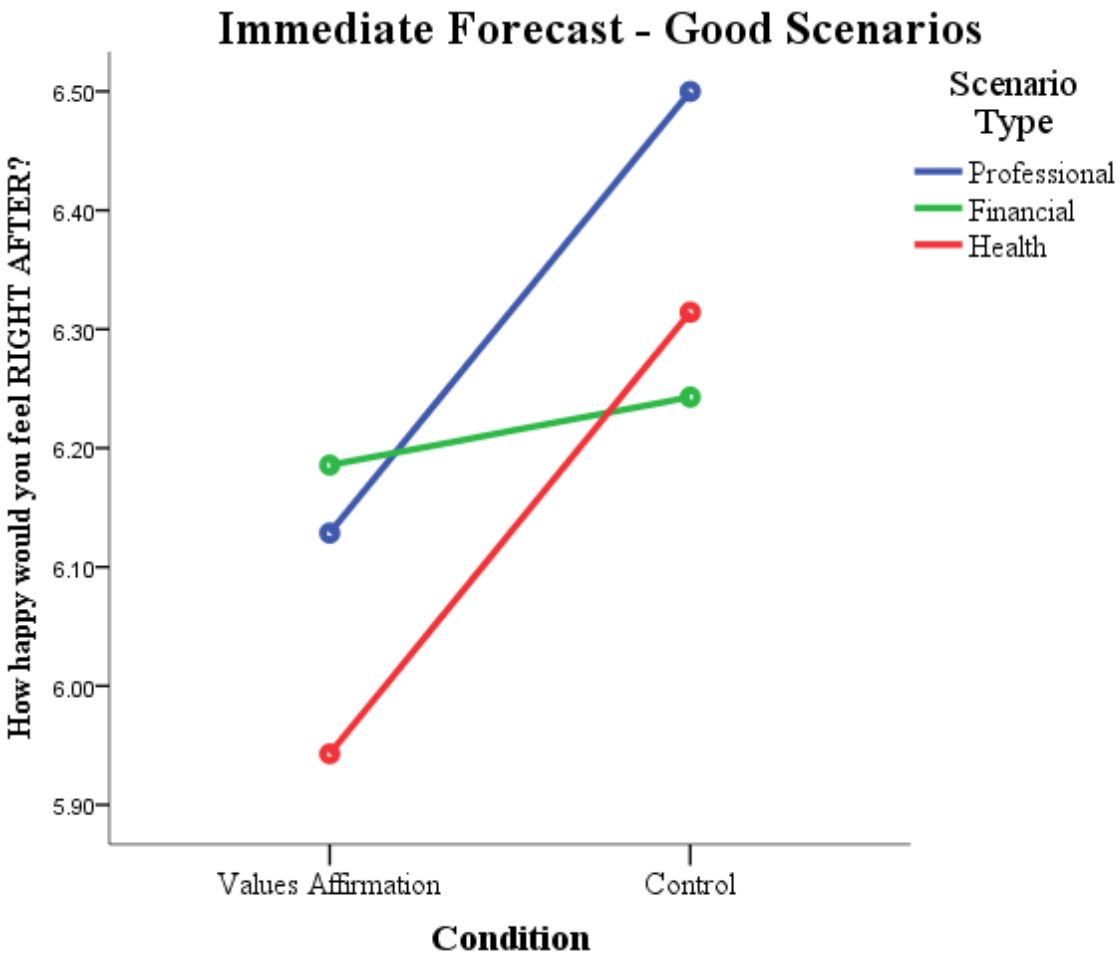

Figure 2. Study 1: Differences in immediate affective forecasting across conditions – positive scenarios.

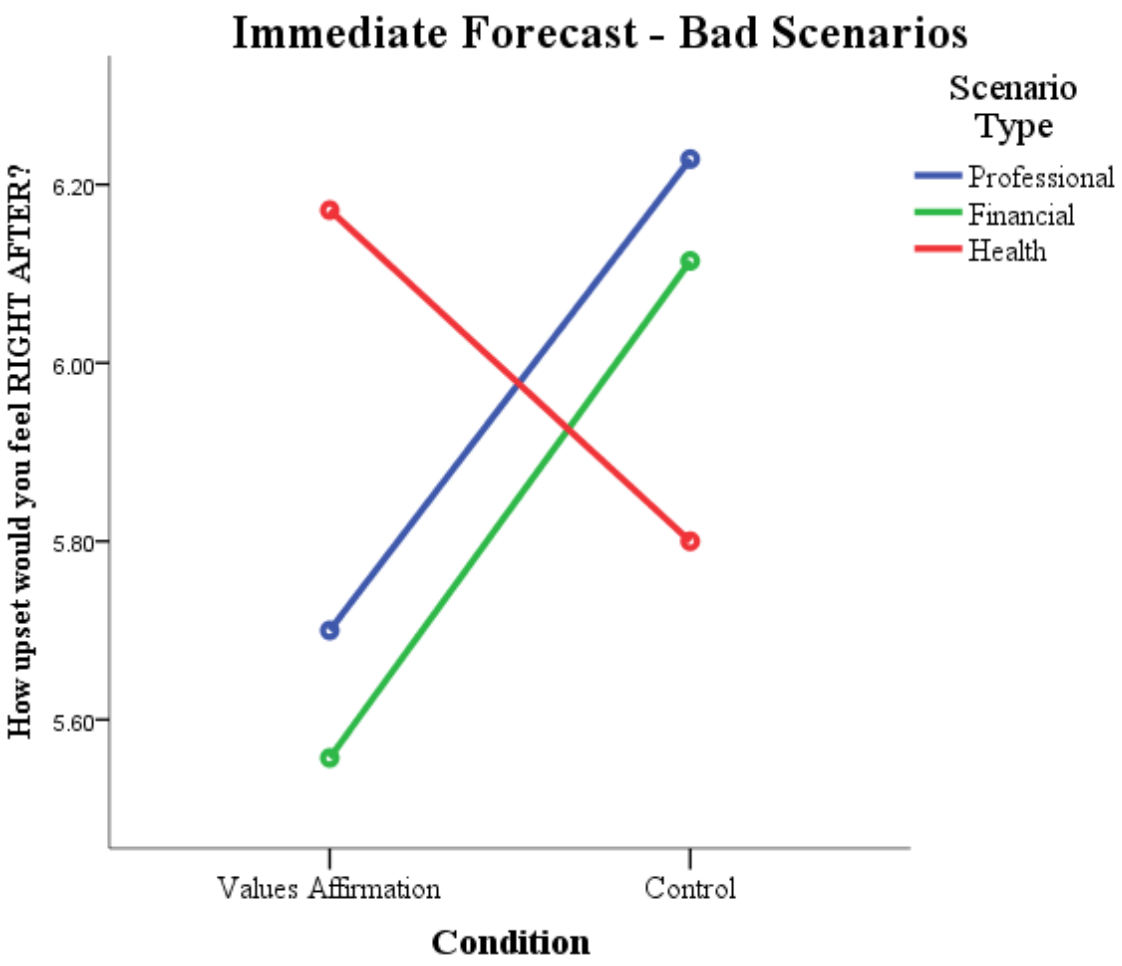

Figure 3. Study 1: Differences in immediate affective forecasting across conditions – negative scenarios.

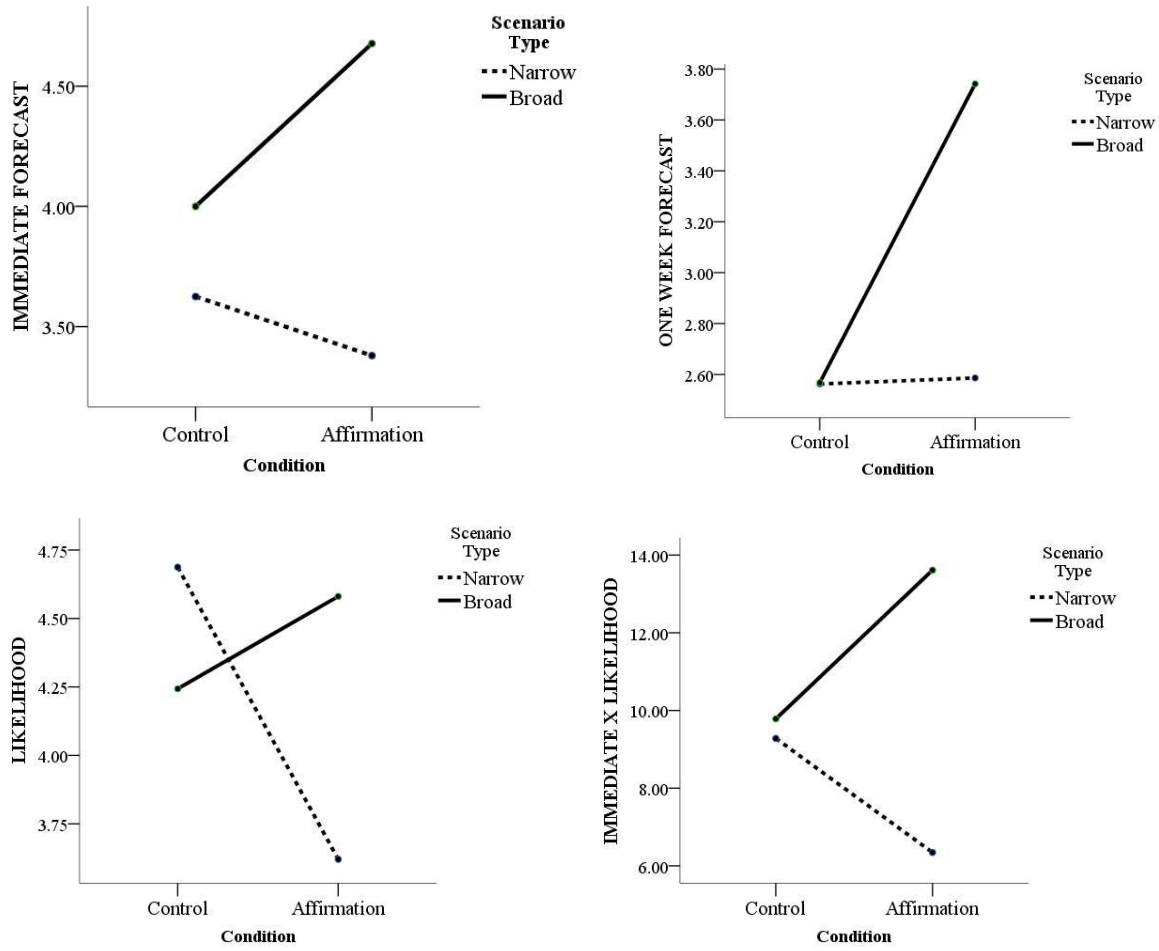

Figure 4. Study 2: Interaction between scenario type and condition for immediate affective forecast (upper left), one week affective forecast (upper right), perceived likelihood (bottom left), and the multiplicative impact of immediate forecast and likelihood adjusted to contain a 0 point (bottom right).

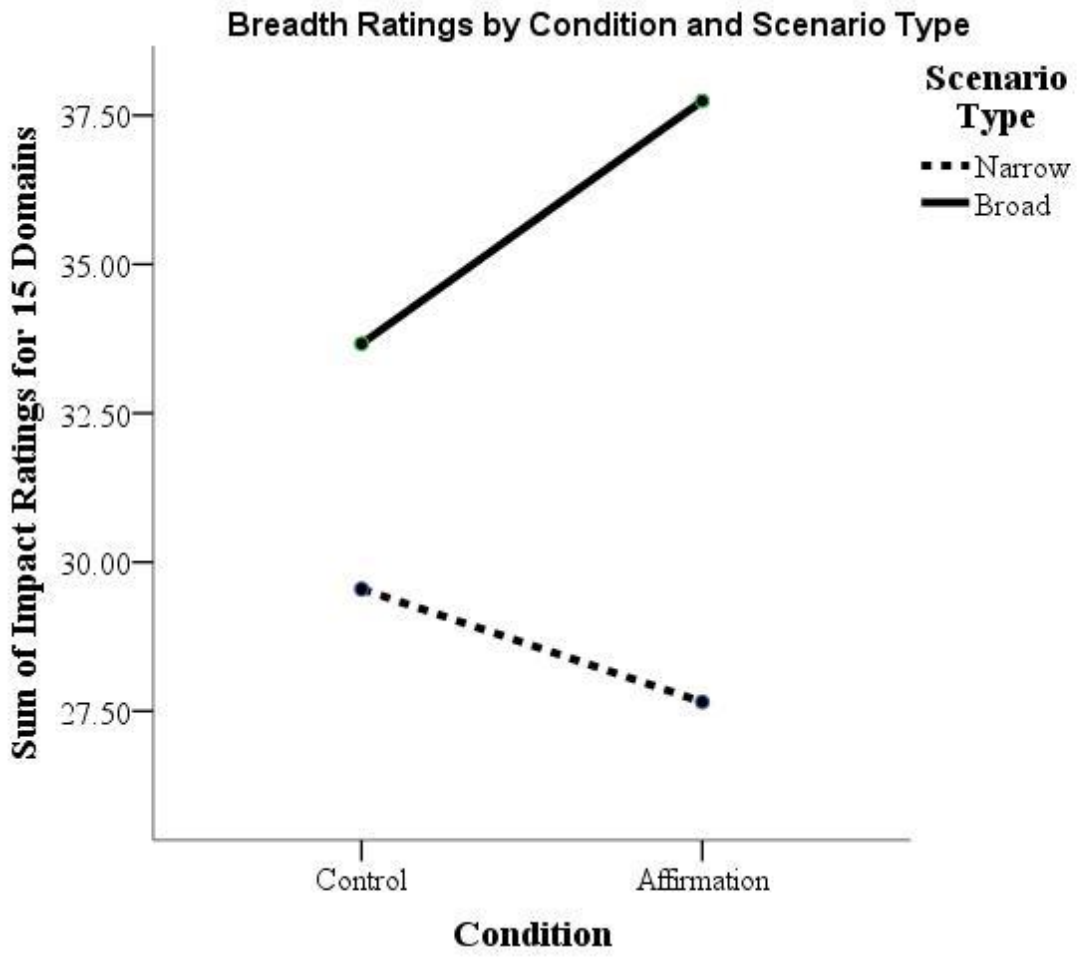

Figure 5. Study 2: Interaction between condition and scenario type on perceptions of scenario breadth.

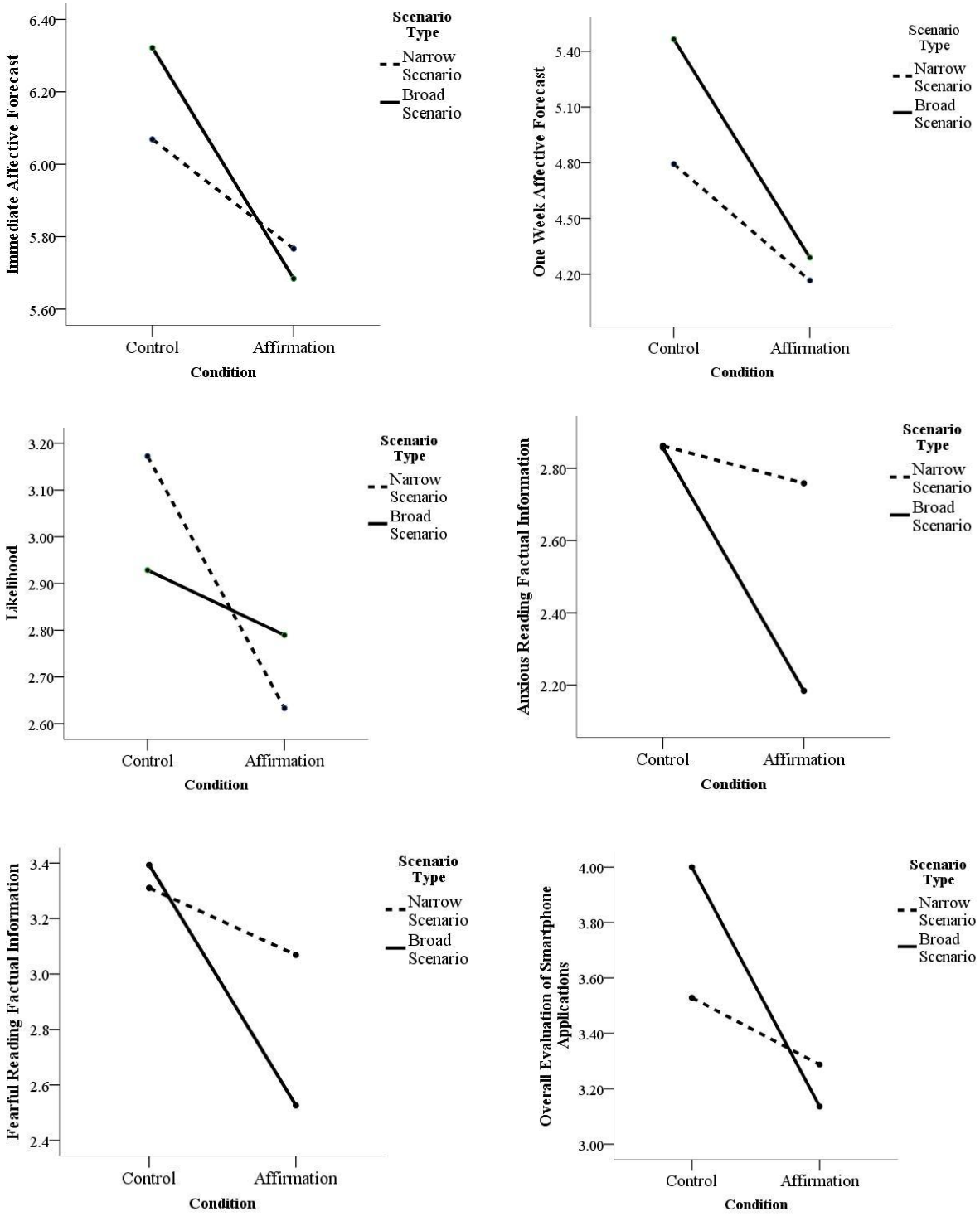

Figure 6. Study 3: Effect of affirmation and interaction with scenario type on immediate affective forecast for scenario (upper left), one week affective forecast for scenario (upper right),

perceived likelihood of scenario (middle left), anxiety while reading texting while driving facts (middle right), fear while reading texting while driving facts (bottom left), and overall evaluation of smartphone applications (bottom right).

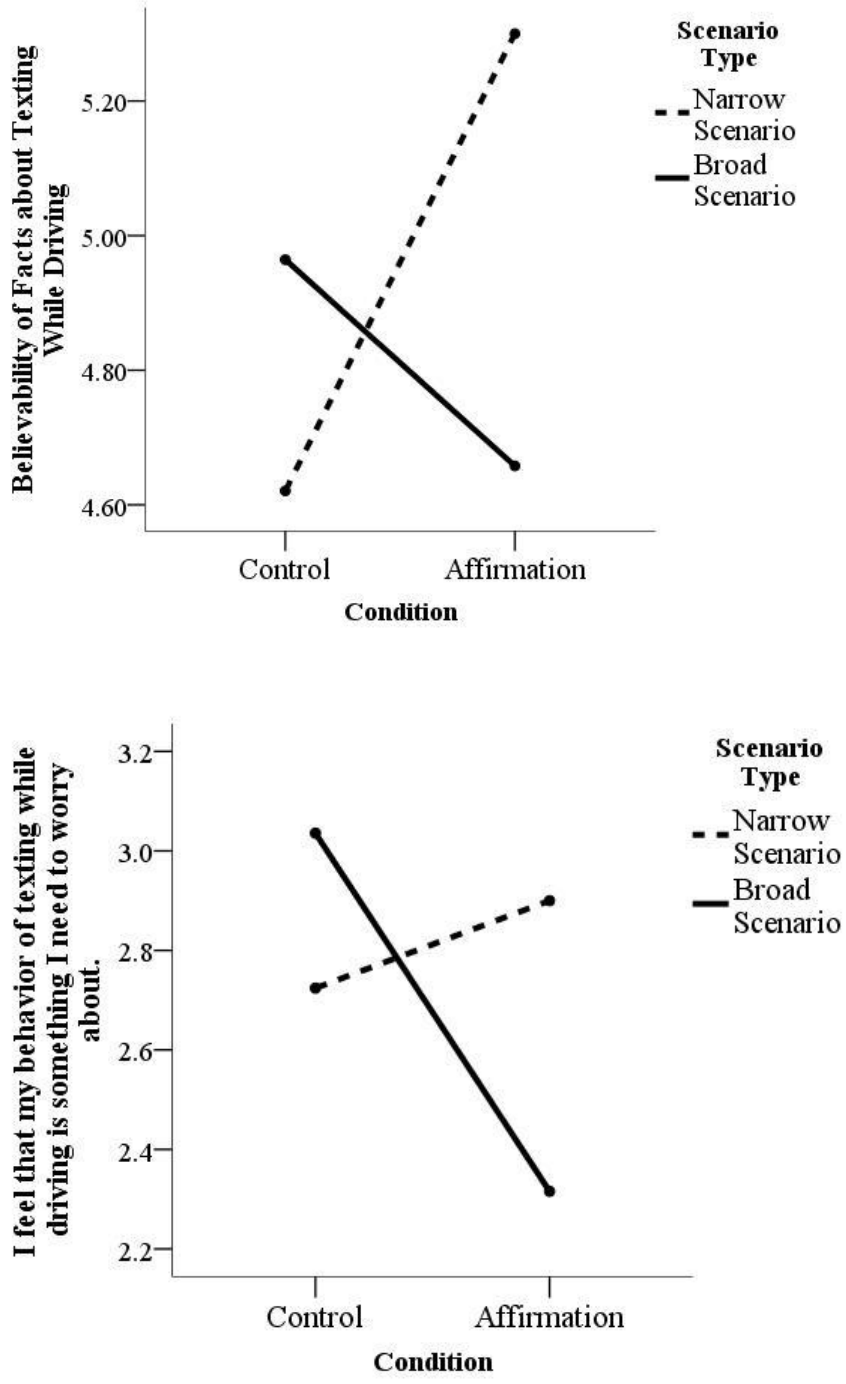

Figure 7. Study 3: Interaction between condition and scenario type on perceived believability of factual information (top) and importance of behavior change (bottom).

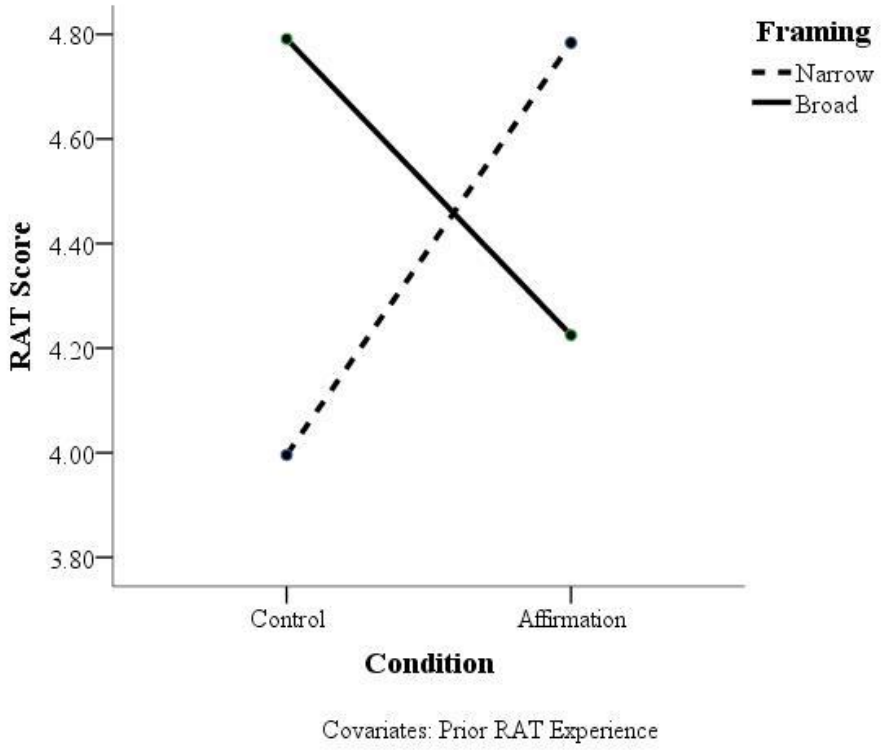

Figure 8. Study 4: Interaction between condition and threat framing on RAT performance.

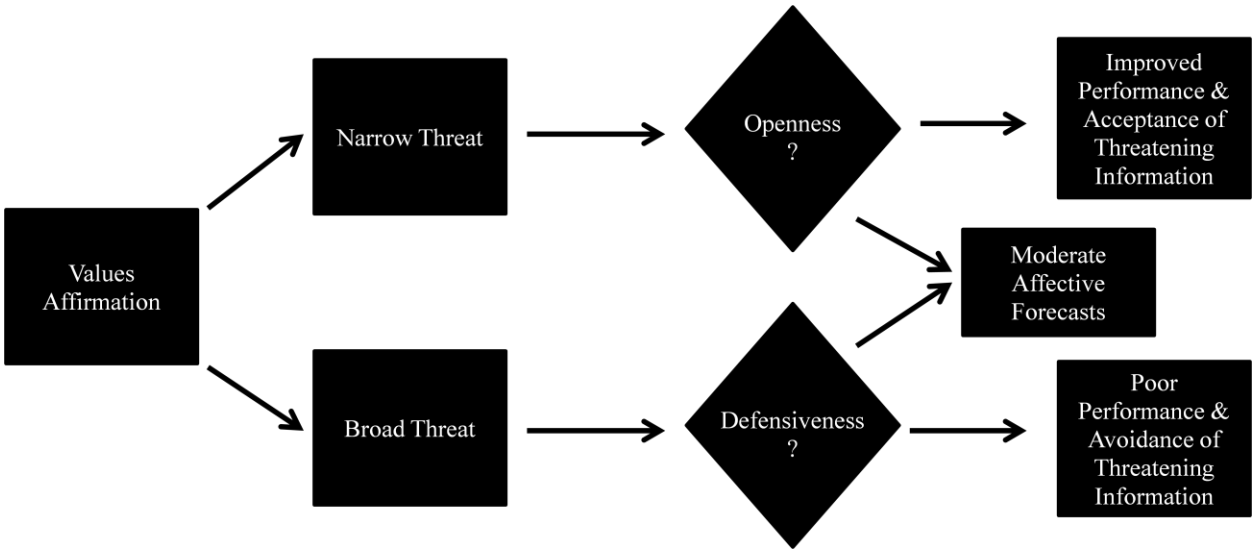

Figure 9. Path diagram depicting the proposed moderation of values affirmation effects by threat breadth.