MOTIVATIONAL SPECIFICITY IN THREAT-COMPENSATION:

TESTING THE EFFECTS OF MORTALITY SALIENCE ON SELF-CONTROL

By

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## Abstract

Self-control is the mental function that allows people to actively suppress unwanted thoughts, emotions, and urges, as well as re-prioritize their goals in accordance with situational demands. However, self-control requires effort, and exerting effort can lead to mental fatigue—a state termed "ego depletion" (Inzlicht & Schmeichel, 2012). When self-control is depleted, people are less motivated to continue exerting effort, and increasingly motivated to pursue other more rewarding activities; satisfying these motives can, in turn, *restore* one's capacity for self-control. The present research investigates the idea that the salience of certain psychological threats—such as the awareness of one's mortality—can impose limits on the sorts of behaviors that people will be motivated to engage in after exerting self-control, and, by extension, what sorts of rewards will be sufficient for restoring mental resources. Across three studies, I draw from research on TMT and the shifting-priorities model of self-control to explore this hypothesis. I investigate whether specialized threat-compensatory motives for coping with the awareness of one's mortality can constrain the types of rewards that serve to effectively counteract the ego depletion effect. The results of these studies did not support this hypothesis, but critical methodological issues arose that prevented a proper evaluation of its accuracy. These issues are explored further within the General Discussion. I then conclude with a brief overview of the potential neural mechanisms thought to undergird the regulation of self-control and the processing of motivationally salient reward-based stimuli.

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# Motivational Specificity in Threat-Compensation: Testing the Effects of Mortality Salience on Self-Control

## Introduction

Self-control is critical for coping effectively with psychological threats (Folkman, 1984; Jonas et al., 2014; Taylor, 1983; Tullet, Teper, & Inzlicht, 2011). At their core, psychological threats are cues that signal actual or potential goal conflicts (cf. Jonas et al., 2014), and produce aversive mental states such as anxiety (Kool, McGuire, Rosen, & Botvinick, 2010), uncertainty (Van den Bos, 2001), or the potential for paralyzing terror (Greenberg et al., 1992). Efforts to prevent or reduce these aversive states require self-control—that is, the ability to actively suppress thoughts, emotions, or behaviors that conflict with one's current goals, and to re-prioritize overarching goals in accordance with situational demands (Inzlicht, Berkman, & Elkin-Brown, 2016; Tullet et al., 2011). Failures of self-control, then, can lead to difficulties in regulating one's emotions and inhibiting disruptive urges. Importantly, these sorts of difficulties are thought to underlie an array of public health issues, such as substance abuse and obesity (see Baumeister & Heatherton, 1996). Thus, by supporting the maintenance of goal-relevant representations via the inhibition of competing impulses (i.e., goal-discrepant thoughts, emotions, and urges), self-control acts as a key component in facilitating adaptive responses to threat and promoting psychological wellbeing.

Although self-control affords people this impressive ability to suppress threatening and/or distracting information, research has shown that exerting self-control can impair effortful processing on subsequent tasks (Baumeister & Heatherton, 1996; Muraven & Baumeister, 2000). The suppression of unwanted thoughts and emotions, for example, has been shown to impair

further attempts at control, just as exerting mental effort in other domains, such as difficult decision-making, can impair further attempts at suppression (Muraven, Tice, & Baumeister, 1998; Wegner, Erber, & Zanakos, 1993; Wenzlaff & Wegner, 2000). The reliability of this so-called "*ego depletion*" effect has been supported by a meta-analysis that computed a medium total effect size ($d$ = 0.62, $CI_{95}$ = 0.57, 0.63) across 83 different studies (Hagger, Wood, Stiff, & Chatzisarantis, 2010).

Although "ego depletion" was originally thought to describe a literal expenditure of physical resources needed for self-control (Baumeister et al., 1998; Gailliot et al., 2007), many theorists now reject this idea in light of findings suggesting that it more accurately reflects the reallocation of *cognitive resources* (e.g., attention; signal-processing fidelity) in the service of shifting motivations (Beedie & Lane, 2010; Inzlicht & Schmeichel, 2012). Previous studies, for instance, have shown that engaging in effortful control increases people's motives to seek out and attend to rewarding stimuli, while decreasing their motives to continue exerting effort (Francis & Inzlicht, in press; Schmeichel et al., 2010). Indeed, some tests have even shown that by satisfying one of these appetitive motives or introducing a rewarding incentive, the depletion effect can be eliminated (e.g., Muraven & Slessareva, 2003; Tice, Baumeister, Shmueli, & Muraven, 2007). Given that self-control failure is thought to ground a host of maladaptive health behaviors, understanding how to circumvent it is a notable goal for psychological research. However, one important question remains unanswered in the current literature: what *kinds* of stimuli and incentives will be 'rewarding' enough to counteract depletion?

Drawing from research showing that certain types of threat can produce specialized compensatory motives for defense (e.g., Shepard, Kay, Landau, & Keefer, 2011), I argue that the potential for any given 'reward' to counteract depletion depends upon the salience of threats that

introduce *content-specific* constraints on motivational processes. Past research has provided support for the idea that specialized defenses emerge when core psychological structures—such as personal control (Kay, Gaucher, Napier, Callan, & Laurin, 2008), self-integrity (Steele, 1988), and symbolic immortality (Greenberg et al., 1992)—are threatened. An important aspect of these findings is that some rewards, which would otherwise be desirable, and may even be unconditioned (e.g., positive affect), are not always capable of satisfying the particular motives that arise in response to threat. Thus, in the current studies I set out to explore whether this motivational specificity observed in threat-compensation processes also extends to the domain of self-control, focusing specifically on how it might influence the means of counteracting depletion when uniquely threatening information is salient.

Based on the findings of terror management theory (TMT; Rosenblatt et al., 1988), I hypothesize that the awareness of one's mortality (i.e., mortality salience: MS) influences self-control processes differently than the awareness of other types of threat. If self-control is fundamentally a process of shifting motivations, and reminders of death trigger specialized motives for compensatory defense, the salience of death thoughts should constrain the types of 'reward' that the individual seeks when attempting to regain self-control strength. Before detailing the specific predictions tested by the three experiments reported herein, I will begin by reviewing relevant literature—first providing more of a background on the *shifting-priorities* model of self-control, then giving an account of TMT to highlight the unique effects of *mortality salience* on threat-compensation processes.

**The Shifting-Priorities Model**

According to the theory of shifting-priorities, a dynamic coupling between motivational and attentional processes drives self-control—together, these interrelated processes operate to

identify and reduce conflicts between competing impulses (Inzlicht et al., 2016). Advocates of this model suggest that the depletion effect is a consequence of initial self-control acts leading to motivational shifts *away* from continued mental effort and *towards* personal gratification (Inzlicht & Schmeichel, 2012). Moreover, as one's motives begin shifting away from continued exertion, so does attention. When self-control begins to wane, a bias to focus more on cues that signal reward, and less on those that signal restraint, increases. Indeed, experimental studies utilizing functional neuroimaging have even found evidence for heightened neural responses to rewarding cues and motivationally salient stimuli during induced states of depletion (Wagner & Heatherton, 2013; Wagner et al., 2013). Due to the tight coupling of these motivational and attentional systems, the waning of self-control after exertion may be initiated by a shift in either process, with the other following close behind.

Support for this shifting-priorities mechanism comes from social psychological and cognitive neuroscience research showing that a variety of reward-based interventions designed to either satisfy a momentary impulse or manipulate the subjective value of continued mental effort can eliminate the depletion effect (Boksem, Meijman, & Lorist, 2005; Inzlicht & Schmeichel, 2012). Some examples of rewards that have been shown to successfully restore self-control strength after initial bouts of effort include: smoking cigarettes (Heckman, Ditre, & Brandon, 2012), drinking a sugary beverage (Gailliot et al., 2007), watching a favorite TV show (Derrick, 2012), receiving a boost in positive mood (Tice, Baumeister, Shmueli, & Muraven, 2007), and affirming core values (Schmeichel & Vohs, 2009).

Incentivizing self-control has also been effective at counteracting the depletion effect, such that offering monetary rewards for completing a difficult task (Muraven & Slessareva, 2003) or reframing temptations as tests of a valued ability ("willpower"; Magen & Gross, 2007)

appear to restore people's capacity for continued control. From research in neuroscience, one EEG study measuring select neural correlates of self-control (i.e., error-related negativity signals) found that adding reward-value to task persistence resulted in otherwise diminished ERPs rebounding back to pre-depletion levels (Boksem et al., 2005).

Critically, these findings support the shifting-priorities model because they reflect the diverse means by which self-control can be restored after its initial impairment. Such results are incompatible with purely resource-based accounts of the depletion effect (Muraven & Baumeister, 2000; Gailliot et al., 2007), as a variety of different inputs appear to restore self-control even when no physical resources are literally replenished. Nevertheless, while a wealth of evidence has shown that satisfying one's motives for immediate gratification can restore self-control, no research to date has focused directly on the role of *motivational specificity* in moderating the efficacy of different restorative techniques. With regard to threat-compensation, however, terror management theorists have shown that motivational specificity is critical in organizing effective and unique defenses against what is arguably the most unsettling psychological threat of all: awareness of our own mortality.

**Terror Management Theory**

According to TMT (Solomon, Greenberg, & Pyszczynski, 1991), the awareness that one's death is always potentially imminent and ultimately inevitable creates the possibility to experience paralyzing anxiety. This potential for terror is an inherent aspect of the human condition, resulting from the conflict between our biologically-rooted orientation towards continued life and the knowledge of our own inevitable death. To buffer themselves from threatening death-related ideation, people invest in three main psychological structures (Hart, Shaver, & Goldenberg, 2005). The first is a cultural worldview that imbues life with meaning,

the second is attachment security, and the third is self-esteem: the feeling that one is a person of value whose existence will continue in some manner after physical death. Whether this sense of value is achieved through beliefs about literal immortality (e.g., an afterlife; reincarnation) or symbolic immortality (e.g., via culture, craft, or kin), it is proposed that the individual must set out to establish and maintain self-esteem in order to experience value in life and prevent existential terror (Pyszczynski, Greenberg, Solomon, Arndt, & Schimel, 2004). Self-esteem is thus our primary mode of psychological defense against the ever-looming threat of death, and is essential for buffering against the anxiety brought about by our awareness of it (Greenberg et al., 1992).

**Mortality salience hypothesis.** The majority of studies assessing TMT test some variant of the *mortality salience hypothesis*: if a given psychological structure serves to protect the self from mortality concerns, then the awareness of one's mortality should trigger motives to defend and bolster that structure (Pyszczynski et al., 2004). In one such study, Kasser and Sheldon (2000) found that people were significantly more likely to overestimate their future financial success after MS compared to an aversive comparison prime (i.e., reminders of physical pain). Other self-enhancing reactions to MS have also been documented, showing that MS can lead to increases in optimism about the future successes of a valued sports team (Dechesne, Greenberg, Arndt, & Schimel, 2000), greater self-serving attributional biases (e.g., taking personal responsibility for one's successes but not one's failures; Mikulencer & Florian, 2002), and an increase in the perceived accuracy of horoscopes—but *only* those that reflect positively on the self (Dechesne, Janssen, & von Kippenberg, 2000). In fact, the drive for self-esteem that is triggered by MS can sometimes be so strong that it leads to self-enhancing behaviors that,

objectively speaking, jeopardize one's physical health (e.g., smoking, skin-tanning; Arndt et al., 2009; Jessop, Albery, Rutter, & Garrod, 2008).

**Anxiety-buffering hypothesis.** Another prevalent hypothesis that has been tested in TMT research is the *anxiety-buffering hypothesis*: if self-esteem serves to reduce the potential for experiencing anxiety, then increases in self-esteem should reduce one's susceptibility to anxiety during a situation of threat (e.g., when mortality is salient; Pyszczynski et al., 2004). This hypothesis has been confirmed in several different studies demonstrating that people dispositionally high in self-esteem become less defensive after MS than those dispositionally low in self-esteem (suggesting that they experience less threat; Greenberg et al., 1993), and that temporarily *boosting* self-esteem can serve to attenuate the defensive reactions that typically follow from reminders of death (Greenberg et al., 1992).

Critically, on this account, self-esteem is not simply a general source of positive affect; rather, it is the primary psychological means of allaying concerns about mortality because it portrays the self as a valuable member of something larger and longer lasting, and not merely as a mortal animal fated to die. Greenberg et al. (1992) tested one alternative hypothesis that enhancing self-esteem may simply be a means of increasing *positive affect*, but found that neither self-reports of positive or negative affect contributed significantly to the influence of self-esteem on terror management striving. Additional TMT studies have since replicated this result, consistently finding that mere boosts in mood are not sufficient for reducing the accessibility of death-related thoughts (Hart, Shaver, & Goldenberg, 2005). Thus, it is clear that the unique threat of death's inevitability motivates an equally unique means of defense: self-esteem striving.

## Motivational Specificity and Death-Thought Suppression

According to TMT and the MS hypothesis, reminders of death amplify our basic motives to bolster and maintain self-esteem (Pyszczynski et al., 2004), as well as an immediate motive to remove thoughts of death from awareness. Terror management theorists often argue that, following MS, the motive to enhance self-esteem does not become active until after a short delay, thereby allowing focal thoughts about death to be suppressed or denied at their outset (Pyszczynski, Greenberg, & Solomon, 1999; Greenberg et al., 2008). This is thought to be because *direct* strategies of threat-reduction (e.g., suppression) are typically more efficient than *indirect* strategies (e.g., boosting self-esteem) at immediately reducing the potential for anxiety (Tullett, et al., 2011). However, directly suppressing thoughts about death requires mental effort, especially when such thoughts are highly salient (Gailliot, Schmeichel, & Baumeister, 2006). So, what happens when an individual, faced with the awareness of her own inevitable demise, lacks the cognitive resources necessary to control or suppress her thoughts and anxieties?

A series of experiments investigating this very question showed that disruptions in self-regulatory strength prior to MS lead to measurable increases in self-esteem striving and worldview defense directly after the prime, without requiring the presence of a delay (Arndt et al., 1997). Further analyses revealed that high levels of death-thought accessibility (DTA) mediated the effect, indicating a failure in death-thought suppression. DTA is typically observed at low levels immediately after MS, reflecting the fact that suppression is the human animal's initial response to threat (Greenberg et al., 1992). When this direct defense is impaired as a result of ego depletion, however, indirect terror management strategies are immediately engaged—*not* merely towards seeking rewards in general (e.g., positive affect), but towards the acquisition of more specific existential nutriments, such as *meaning, value,* and *security.*

The studies described herein are aimed at further exploring how motivational specificity in threat-compensation extends beyond the realm of defense and into the broader domain of executive functioning. That is, if the proposed shifting-priorities model represents an underlying mechanism of cognitive control, then motivational specificity should constrain the entire goal-prioritization process, particularly when mental resources are depleted. This should have effects not only on motivation and behavior, but also on attention and one's unconscious biases to maintain focus on certain stimuli at the expense of others (e.g., Schmeichel et al., 2010). Essentially, if motivational specificity constrains threat-compensation processes, as indicated by TMT and other 'core-threat' theories (e.g., compensatory control theory; Kay et al., 2008), then it should be expected to impose similar constraints on other overlapping cognitive functions as well—particularly, on the regulation of self-control.

**Overview of Present Studies**

The foregoing discussion brings us to the primary hypothesis explored within the following studies: that, under conditions of ego depletion and MS, a boost in self-esteem—and *not* merely positive affect—will restore people's capacity for self-control. Furthermore, the MS effects are here compared with other aversive threats not expected to generate specialized compensatory motives (e.g., reminders of physical pain). When non-specialized threats are salient, they will not introduce qualitative limits on the restorative capacity of 'mere' boosts in mood. This latter hypothesis has been confirmed in past research (Schmeichel & Vohs, 2009; Tice et al., 2007), but the former claim represents a novel prediction that incorporates findings from the literature on self-control, terror management theory, and threat-compensation. The unique contribution of this research lies in its attempt to show that the effects of ego depletion are not restricted solely to the *amount* or *degree* of mental resources exerted, but rather depend

on the mental *contents* involved in that exertion as well. The predicted effect is thought to be a potential indicator of the motivational specificity elicited by MS and its influence on self-control. We investigated these assumptions in three different experiments.

The design for these studies was constructed with modifications to the basic *dual task paradigm*—a common experimental procedure that involves giving participants two unrelated self-control tasks in order to test the basic depletion effect, namely that effort expended during Task 1 will lead to measurable fatigue in Task 2 (Hagger et al., 2010). Since we are mainly interested in strategies that counteract the impairments brought about by exertion, the current studies have generally followed this procedural timeline: **A)** a depleting or non-depleting task that involves reminders of death or other aversive thoughts (e.g., physical pain), followed by **B)** an intervention designed to either boost self-esteem, boost mood, or have no effect on either, and then **C)** a quantitative measure of self-control strength.

In order to establish a manipulation that could be adapted to accommodate each version of the intervention, we utilized a writing exercise that has been used in previous research to manipulate *self-affirmation* (Cohen, Aronson, & Steele, 2000). For this task, participants in the *self-affirmation* condition rank a list of 12 personal values (e.g., athleticism, creativity, relations with friends and family) and then write about the importance of their top-ranked value (Appendix A), while participants in the *no-affirmation* condition see the same list of values but write instead about why one of their low-ranked values might be important to the average college student (Appendix A). These inductions have been effective in past research; the self-affirmation task reliably increases self-esteem, and the control task reliably has no such effect (Cohen et al., 2000; Fein & Spencer, 1997). Indeed, some TMT researchers have even successfully used this manipulation to bolster self-esteem after MS (Landau & Greenberg, 2006; Schmeichel &

Martens, 2005), giving us confidence that the task will be equally appropriate for our current purposes.

In order to prepare for testing our primary hypothesis, we first set out to establish a version of this task that would reliably boost *positive affect*, but not self-esteem. Doing this would allow us to test and compare the relative efficacy of self-esteem and positive affect in restoring self-control after it has been depleted, particularly when reminders of death are salient. For the *mood boost* manipulation, we gave participants similar instructions as in the *self-affirmation* and *no-affirmation* conditions, but instead asked them to rank 12 pleasant activities that can put them in a good mood (e.g., sitting in a warm cozy chair; being outside; eating ice-cream), and then write about why their top-ranked activity puts them in a good mood (Appendix A). The idea behind using this comparison task was that all the activities listed are enjoyable and pleasurable to think about, but are also far enough removed from the self that writing about them would be unlikely to bolster self-esteem or help the individual affirm a broader sense of meaning and value.

## Pilot Testing

A pilot test was conducted through Amazon Mechanical Turk in order to establish the validity of these three inductions.

### Method

We randomly assigned 94 participants (48% female*;* $M_{AGE} = 33$, $SD_{AGE} = 12$) to complete the *self-affirmation* task, the *mood boost* task, or the *no-affirmation* task. Afterward, all participants filled out the State Self-Esteem Scale (Heatherton & Polivy, 1991) and a 20-item Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988). The State Self-Esteem Scale consists of 20 statements that are rated on a 5-point scale (e.g., “I feel good

about myself," 1= *not at all;* 5 = *extremely*; Heatherton & Polivy, 1991). The PANAS also consists of 20 items rated on a 5-point scale (1 = *very slightly, or not at all;* 5 = *extremely*), but contains 10 items that refer to positive emotions (e.g., alert; interested; inspired), and 10 that refer to negative emotions (e.g., distressed; jittery; hostile; Watson et al., 1988). In order to establish a global index of *positive affect,* we created a composite variable by reverse-coding the 10 negative emotion-words and then averaging participants' scores across all 20 PANAS items.

We had three main predictions that, if affirmed, would signal the appropriateness of these inductions for testing our primary hypothesis in a more elaborate set of lab experiments: 1) The *self-affirmation* task should increase self-esteem relative to the *mood boost* and *no-affirmation* tasks, 2) the *mood boost* and *no-affirmation* groups should not significantly differ with respect to self-esteem, and 3) the *mood boost* task should increase positive affect relative to the *no-affirmation* task.

**Results**

**Self-esteem.** Internal consistency, after reverse coding 13/20 items, was high ($\alpha = .95$). A one-way analysis of variance (ANOVA) was conducted to examine the effect of condition on self-esteem scores, and returned a marginal effect, $F(2, 91) = 2.74$, $p = .07$, $\eta^2_p = .06$ (see Figure 1). Post-hoc comparisons using the Fisher LSD test revealed that participants who completed the *self-affirmation* task reported significantly higher levels of self-esteem ($M = 4.16$, $SD = 0.67$) than those who received the *no-affirmation* task ($M = 3.70$, $SD = 0.93$), $p = .025$, $d = .57$. The *mood boost* group did not significantly differ from the *no-affirmation* group on state self-esteem, as predicted ($M = 4.00$, $SD = 0.67$ | $M = 3.70$, $SD = .93$), $p = .11$, $d = .37$. However, they did not significantly differ from the *self-affirmation* group either ($M = 4.00$, $SD = 0.67$ | $M = 4.16$, $SD = .67$), $p = .43$, $d = .24$. Means and standard deviations are reported in Table 1.

**Positive affect.** The 20-item composite for positive affect (with 10 negative items reverse-coded) displayed a high level of internal consistency ($\alpha = .88$). Submitting positive affect composite scores to an ANOVA returned a significant main effect, $F(2, 91) = 3.82$, $p = .025$, $\eta^2_p = .077$ (see Figure 2). Post-hoc comparisons using the Fisher LSD test revealed that participants who completed the *mood boost* task reported significantly higher levels of positive affect ($M = 4.00$, $SD = 0.41$) than those who received the *no-affirmation* task ($M = 3.63$, $SD = 0.68$), $p = .007$, $d = .66$. No other pairwise comparisons reached statistical significance (see Table 2).

**Discussion**

Critically, while these data do support some of our main predictions, we did not find that participants in the *self-affirmation* condition reported significantly higher levels of self-esteem than those in the *mood boost* condition. Nevertheless, they did report significantly higher self-esteem than participants in the *no-affirmation* group, while those in the *mood boost* group did not. Thus, we found only partial support for our main prediction that the *self-affirmation* group would report higher self-esteem than both the *no-affirmation* and *mood boost* groups. Still, we found supporting evidence for our other two predictions, namely that the *mood boost* group and *no-affirmation* group would not differ in self-esteem, but that the *mood boost* group would report significantly higher positive affect.

Overall, these findings only weakly suggest that our inductions are successful in discriminating between the constructs of self-esteem and positive affect; that is, our *mood boost* task appears to have some effect on both of these constructs. Nonetheless, although our results show that the *mood boost* task might inadvertently increase self-esteem, they also indicate that it is not as effective at doing so as the *self-affirmation* task ($d = .37$ vs. $d = .57$, relative to the *no-affirmation* condition). Thus, we proceeded to apply these three inductions towards testing our

primary hypothesis in Study 1, noting that any results associated with them must be interpreted with caution.

## Study 1

Study 1 tests the hypothesis that interfering with the suppression of death-related thoughts during MS leads to a state of ego depletion (Arndt et al., 1997), and that a subsequent boost in self-esteem—but not merely positive affect—can restore one's capacity for self-control. To test this idea, we set out to show that it is the *interaction* between ego depletion and the awareness of death, specifically, and not simply reminders of death, that produces the hypothesized effects. This is presumably because ego depletion interferes with death-thought suppression, having the ironic effect of increasing death-thought accessibility (Arndt et al., 1997; Wenzlaff & Wegner, 2000) along with the concomitant motive to manage potential terror by reaffirming core aspects of the self (or other sources of meaning, see Pyszczynski et al., 2004). By satisfying this specialized compensatory motive, salient concerns about mortality are effectively allayed and no longer in demand of attentional resources, thereby restoring the capacity for self-control in other domains. Previous studies have shown that task performance in non-depleted individuals, however, is unaffected by inductions designed to restore self-regulatory strength, following the idea that a state of depletion is necessary for restoration to be observed (Tice et al., 2007; Schmeichel & Vohs, 2009).

In Study 1 we exposed participants to subtle reminders of death that were embedded in an attention control video, asking half of them to suppress the urge to look at the words, and the other half to simply watch the video as they would normally. Then, participants were given a writing task designed to either enhance self-esteem or improve positive mood (from Pilot), and were subsequently provided with an effortful cognitive task aimed at measuring their remaining

self-control strength. We predicted that participants who received the *no-suppression* task would display equally high levels of control on the cognitive task, regardless of whether they received the self-affirmation or mood boost activity. Critically, we predicted that participants in the *suppression* condition would show significantly higher levels of self-control if they received the self-affirmation exercise as opposed to the mood boost task. These findings would provide support for the idea that positive affect is insufficient for restoring self-control after it has been depleted and threatening reminders of death are salient.

**Method**

Participants were 129 students recruited from undergraduate psychology courses at KU during the 2015 spring semester (57% female; $M_{AGE}$ = 19, $SD_{AGE}$ = 1.36). An a priori power analysis conducted with the software package G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) indicated that a total sample of 114 individuals would be needed within a 2x2 factorial design to detect the predicted medium-to-large effect ($d$ = .75; see Schmeichel, Vohs, & Baumeister, 2003) with 80% power and $\alpha$ = .05.

**Attention control manipulation.** One validated laboratory procedure that reliably induces ego depletion involves showing participants a 6-minute (silent) video of a woman being interviewed by an off-screen interviewer, and asking them to *suppress* the urge to look at the series of words appearing in bottom-right corner of the screen (Baumeister, Bratslavsky, Muraven, & Tice, 1998; Muraven et al., 1998). This induction has been used effectively in past research on ego depletion, and in one meta-analysis was shown to produce a reliable effect of $d$ = 0.61 ($CI_{95}$ = 0.48, 0.74; Hagger et al., 2010). Moreover, functional imagining studies that have used this task as a manipulation of ego depletion found that it produced a temporary, but reliable,

decrease in the functional connectivity between cortical regions involved in effortful control (Wagner & Heatherton, 2013).

There were 34 distracting words shown throughout the 6-minute video. In the current experiment, we manipulated this list to include 6 death-related words that were each repeated once during the video (i.e., DEATH, DIE, GRAVE, CORPSE, SKULL, DEAD; see Appendix B). These words appeared in white font over a black background, making them easy to see and very hard to avoid looking at during the video. Prior to this task, participants who were randomly assigned to the *suppression* condition were explicitly instructed to suppress the urge to look at the words throughout the video and to maintain focus on the woman being interviewed, whereas those who were assigned to the *no-suppression* condition were simply instructed to watch the video as they would normally.

***Manipulation checks.*** After watching the video, participants were asked three questions that assessed: **1)** difficulty following instructions ("How difficult was it to follow the instructions while you watched the video?" *1 = very easy; 7 = very difficult*), **2)** how often they looked at the words on the screen ("How often did you find yourself glancing at the words that appeared on the screen?" *1 = never; 7 = very frequently*), and **3)** the difficulty of avoiding distraction ("How difficult was it to avoid being distracted by the words that appeared on your screen?" *1 = very easy; 7 = very difficult*). We predicted that participants in the *suppression* condition would perceive the task as more difficult than those in the *no-suppression* condition, and would report looking at the distracting words less frequently. This latter item is critical for determining whether or not *suppression* participants actually followed instructions to control their attention, and has been used in previous studies for this purpose as well (e.g., Gailliot et al., 2007).

**Restoration manipulation.** Following the manipulation checks, participants were randomly assigned to either the *self-affirmation* condition or the *mood boost* condition tested in the pilot study. The *no-affirmation* task was not included due to time constraints on data collection. Because the critical comparison is between groups in the *suppression* condition who receive either the *self-affirmation* or *mood boost* task, these conditions were prioritized in Study 1 to attempt to establish the basic effect.

**Cognitive control measure.** The Biber Cognitive Estimation Test (BCET) is one cognitive measure of self-control that has demonstrated reliability in a variety of different populations (Bullard et al., 2004; Wagner, MacPherson, Parente, & Trentini, 2011; see Appendix C). The test consists of 20 items that ask participants to make a variety of estimates regarding quantity (e.g., "How many seeds are there in a watermelon?"), distance/length (e.g., "How far could a horse pull a farm cart in one hour?"), weight (e.g., "How much does a folding chair weight?"), and time/age (e.g., "How long does it take to iron a shirt?"). Each answer is scored on a 0-2 scale, and the criteria for scoring are based on the responses from a normative sample of 113 healthy adults (58% female, 95% White, $M_{AGE}$ = 37, $SD_{AGE}$ = 16.1) that has also been cross-validated with an additional 49 participants (61% female, 90% White, $M_{AGE}$ = 40, $SD_{AGE}$ = 14; Bullard et al., 2004).

Estimates that fall between the 25$^{th}$ and 75$^{th}$ percentile of this normative distribution receive a score of 2, while estimates that fall within the 95$^{th}$ percentile—but outside the 25$^{th}$ and 75$^{th}$—receive a score of 1. All other responses that are made outside the 95$^{th}$ percentile receive a score of 0. For this measure, a higher score is indicative of greater self-control strength. That is, a higher score shows that one's responses are more closely aligned with the normative sample's

responses to the questions, and thus that the individual is applying effortful thought in considering reasonable estimations as opposed to just guessing without deliberation.

**Results**

**Manipulation checks.** ANOVAs were performed on each of the three items with the *depletion* factor as the only predictor, revealing no significant difference between the *suppression* group ($n$ = 66) and *no-suppression* group ($n$ = 63) on perceived difficulty following instructions ($M$ = 3.24, $SD$ = 1.82 | $M$ = 3.29, $SD$ = 1.64), $F(1, 127) < 1$, $p = .89$, $\eta^2_p < .001$, and difficulty avoiding distraction during the video ($M$ = 3.53, $SD$ = 1.72 | $M$ = 3.73, $SD$ = 1.79), $F(1, 127) < 1$, $p = .52$, $\eta^2_p = .003$. However, participants in the *no-suppression* condition reported looking at the distracting words significantly more often ($M$ = 4.22, $SD$ = 1.48) than those in the *suppression* condition ($M$ = 3.17, $SD$ = 1.47), $F(1, 127) = 16.52$, $p < .001$, $\eta^2_p = .12$.

**Self-control strength.** Submitting BCET scores to a two-factor (*depletion* X *restoration*) ANOVA revealed no significant main effect for the *depletion* factor, $F(1, 125) < 1$, $p = .47$, $\eta^2_p = .004$, or for the *restoration* factor, $F(1, 125) = 1.20$, $p = .13$, $\eta^2_p = .01$, but did uncover a significant interaction between the two on participants' estimation scores, $F(1, 125) = 5.06$, $p = .026$, $\eta^2_p = .04$ (see Figure 3; means and standard deviations are reported in Table 3). Post-hoc comparisons revealed that among participants who suppressed attention while being presented with reminders of death, those who self-affirmed exhibited higher self-control ($M$ = 1.48, $SD$ = .12) than those who received a mood boost ($M$ = 1.38, $SD$ = .22), $p = .018$, $d = .56$.

Among participants who received the mood boost task, those who suppressed the urge to look at death-related cues displayed lower self-control ($M$ = 1.38, $SD$ = .21) than those who simply watched the video without trying to control their attention ($M$ = 1.47, $SD$ = .12), $p = .04$,

$d$ = .51. In contrast, suppressing attention while being exposed to reminders of death did not impair self-control among those who had self-affirmed, $p$ = .28, $d$ = .26.

**Discussion**

These results provide some initial support for our hypothesis that interfering with death-thought suppression impairs self-control, but that self-regulatory strength can be restored via self-affirmation, and *not* simply positive affect. In the *no-suppression* conditions, participants did not statistically differ in self-control efforts regardless of whether they affirmed a cherished value or merely wrote about activities that put them in a good mood. This finding supports our prediction that different restorative techniques should not influence self-control differently when the individual is not in a state of mental depletion. These techniques are means of *counteracting* the effects of depletion by satisfying an active motive to defend against threat or seek reward, not raw sources of cognitive "fuel" that one can simply use to "fill up the tank."

Thus, the effects observed in the *suppression* conditions follow exactly what we predicted: when reminders of mortality were salient, the depletion effect was not counteracted by a mere boost in mood. Rather, in this particular study, it required a re-affirmation of one's personal sense of value and purpose to effectively cope with the MS threat and unburden the attentional resources needed for effortful control. Indeed, participants in the *suppression* condition who also received the self-affirmation task demonstrated subsequent levels of control that did not differ from their counterparts in the *no-suppression* condition. However, this was not the case for recipients of the mood boost task, who showed significantly less self-control strength in the *suppression* versus *no-suppression* condition.

**Limitations.** While these data are in support of our main predictions, a variety of limitations to this study compel the need for additional evidence. First, our manipulation checks

revealed that people in the *suppression* condition did not report any more difficulty following instructions or avoiding distraction than those in the *no-suppression* condition. The lack of difference on these items might suggest that participants in the *suppression* condition were not actually put into a state of ego depletion, as intended, but rather exerted an equal amount of mental effort as those in the *no-suppression* condition. Despite this possibility, there are two factors that nevertheless support our interpretation of the task's influence on self-control: 1) the *suppression* group reported glancing at the distracting words significantly less often than those in the *no-suppression* group, indicating that they followed instructions to control their attention, and 2) previous researchers who used this attention control manipulation have obtained null effects on similar manipulation check items while still demonstrating both behavioral and neurobiological evidence for the depletion effect (Muraven & Slessareva, 2003; Gailliot et al., 2007; Wagner & Heatherton, 2013). Indeed, researchers who utilize the sequential task paradigm have noted that depleted individuals don't often report greater fatigue than controls until after Task 2, indicating that self-control strength begins to wane before they are even explicitly aware (see Bayne & Levy, 2006).

Second, we did not include the *no-affirmation* condition in Study 1. This condition is critical for establishing confidence in our primary hypothesis, as well as for getting an idea of the extent to which our experimental design actually produces the depletion effect. Third, we did not compare the salience of death thoughts, in particular, with the salience of other aversive thoughts. In order to show that the efficacy of cognitive restoration is constrained by the compensatory (i.e., motivational) specificity generated by particular types of threat, we must compare the effects of MS to those of other threats that do not elicit such motivationally specific

coping strategies. Thus, Study 2 was designed to further explore our hypothesis and address these limitations.

## Study 2

Overall the design for Study 2 is very similar to Study 1, but with the following critical changes: **1)** In this design, *all* participants were given instructions to suppress the urge to glance at the distracting words in the video, but half were given a version that included twelve *death-words* (i.e., the six unique words, each presented twice; Appendix B), and the other half received a version with twelve *pain-words* instead (i.e., HURT, PAIN, ACHE, SORE, SICK, PAINS, each presented twice). Previous TMT researchers have also compared reminders of physical pain with MS, finding that such "mortality-nonsalient" threats are still aversive but do not elicit the same motives for self-enhancement as MS (Greenberg et al., 1992; Landau & Greenberg, 2006). **2)** The *no-affirmation* task has now been included within the design. **3)** In order to establish convergent validity of our hypothesized effect, we employed a different measure of self-control by replacing the cognitive estimation test with a measure of task persistence—another important ability that requires both attentional and motivational resources to be controlled effectively (Baumeister et al., 1998; Muraven et al., 1998; Inzlicht et al., 2016).

Incorporating these changes into the current design allows us to test three additional hypotheses: 1) If MS enhances specialized motives for terror management (Greenberg et al., 1992), then when reminders of death are highly accessible a mere boost in mood should be as ineffective at counteracting depletion as receiving no boost at all (i.e., receiving the non-affirming task); 2) If self-affirmation can effectively counteract depletion under conditions of MS, then it should reliably lead to higher levels of self-control than does the non-affirming task; and 3) While we predict that a boost in mood after attention suppression will be ineffective at

restoring self-control under conditions of MS, we expect that it will otherwise be sufficient when specialized compensatory motives are inactive—thus, the mood boost task should lead to greater task persistence than *no-affirmation*, but only among individuals in the pain-salience condition.

The primary goal of Study 2 is to test these additional hypotheses and expand our understanding of how MS, compared with other aversive psychological threats, uniquely affects the exertion and regulation of self-control.

**Method**

Participants were 257 students recruited from undergraduate psychology courses at KU during the 2015 fall semester (60% female; $M_{AGE}$ = 19, $SD_{AGE}$ = 1.07). The procedure for this study follows the same overall structure as Study 1, but with the three critical changes described in the foregoing section. An a priori power analysis conducted with the software package G*Power (Faul et al., 2007) indicated that a total sample of 315 individuals would be needed within a 2x3 factorial design to detect the predicted medium effect ($d$ = .61; see Hagger et al., 2010) with 80% power and $\alpha$ = .05. Time constraints prevented us from acquiring a sample of this size, but a post-hoc power analysis indicated that a slightly larger effect size (i.e., $d$ = .67) would be adequately detectable within our sample.

**Attention control manipulation.** For this study we used the same attention control task as in Study 1 (e.g., Baumeister et al., 1998). However, in this study *all* participants were instructed to suppress the urge to glance at the distracting words that came up during the video. We also created two different versions of the task: depending on random assignment, participants were exposed to either *death-words* or *pain-words* while controlling their attention throughout the video.

***Manipulation checks.*** We used the same three manipulation check items that appeared in Study 1—these were used to measure the perceived difficulty of following instructions, the difficulty of avoiding distraction during the video, and how often participants glanced at the distracting words. Because in this study we asked all participants to suppress their attention during the video, we expected to see no statistical differences between the death and pain groups on these items.

**Restoration manipulation.** After answering these questions, participants randomly received either the *self-affirmation* task, *mood boost* task, or the *no-affirmation* task in which they were asked to write about a low-ranked value that might be important to the average college student. During this task, we also measured the amount of time people spent responding to the prompt (TIME).

**Task persistence—Unscrambling anagrams.** For our dependent measure of self-control, we asked participants to try and unscramble six difficult anagrams while, unbeknownst to them, they were being timed so that we could measure how long they *persisted* at trying to solve each one (Appendix C). Moreover, three of the anagrams were entirely unsolvable; we are particularly interested in people's persistence on these puzzles, since it is guaranteed that all of their efforts will be met with failure. Past research has shown that persisting on a difficult task in the face of perpetual failure requires a substantial degree of self-regulatory strength (Baumeister et al., 1998; Muraven et al., 1998). Furthermore, this task has been used successfully in previous studies as a measure of ego depletion (Hill, DelPriore, & Vaughn, 2011; Gailliot et al., 2007), and has been confirmed as an effective test of self-control strength ($d$ = 0.60, $CI_{95}$ = 0.44, 0.76; Hagger et al., 2010). At the end of the study, participants were fully debriefed and informed that some of the anagram puzzles were actually unsolvable.

## Results

**Manipulation checks.** Three one-way ANOVAs with *threat* as the only predictor revealed no significant difference between the death group ($n$ = 133) and pain group ($n$ = 124) on perceived difficulty following instructions ($M$ = 3.62, $SD$ = 1.59 | $M$ = 3.52, $SD$ = 1.82), $F(1, 255) < 1$, $p = .64$, $\eta^2_p = .001$, difficulty avoiding distraction during the video ($M$ = 3.95, $SD$ = 1.50 | $M$ = 3.75, $SD$ = 1.74), $F(1, 255) < 1$, $p = .33$, $\eta^2_p = .004$, and word-glance frequency ($M$ = 3.40, $SD$ = 1.25 | $M$ = 3.27, $SD$ = 1.43), $F(1, 255) < 1$, $p = .46$, $\eta^2_p = .002$.

**Time spent on restoration task.** An independent samples t-test revealed no difference in time spent writing on the restoration task (TIME) as a function of *threat*, $t(255) < 1$, $p = .81$. However, a one-way ANOVA with *restoration* predicting TIME returned a significant main effect, $F(2, 254) = 10.62$, $p < .001$, $\eta^2_p = .077$. Post-hoc testing with Fisher's LSD showed that participants in the self-affirmation condition spent significantly longer writing about their chosen subject ($M$ = 113.23*s*, $SD$ = 5.78*s*) than those in the mood boost condition ($M$ = 87.92*s*, $SD$ = 5.85*s*), $p = .002$, $d = 4.35$, as well as those in the no-affirmation condition ($M$ = 76.50*s*, $SD$ = 5.74*s*), $p < .001$, $d = 6.38$. The latter two groups did not differ significantly on TIME, $p = .17$, $d = 1.97$, but the trend suggests that participants tended to spend slightly longer on the mood boost task than the no-affirmation task.

TIME also explained a significant proportion of the variance in participants' average time persisting on the six anagrams as a whole, $R^2 = .09$, $F(1, 255) = 23.79$, $p < .001$, $\eta^2_p = .078$, with TIME positively predicting time spent on the anagrams, $b = .28$, $t(255) = 4.88$, $p < .001$. This same effect was found with respect to the subsets of three unsolvable anagrams, $R^2 = .10$, $F(1, 255) = 27.89$, $p < .001$, $\eta^2_p = .100$, and three solvable anagrams, $R^2 = .05$, $F(1, 255) = 14.24$, $p <$

.001, $\eta^2_p$ = .053. Given the robust relationships that exist between TIME and our critical independent and dependent variables, it will be used as a covariate in all primary analyses.

**Primary analyses.** There were three dependent measures that were derived from the anagram persistence task: 1) average time spent on the three unsolvable, non-words (NON_T), 2) average time spent on the three solvable words (WORD_T), and 3) average time spent on all six anagrams (TOTAL_T). Each of these measurement variations of persistence has been used in past research on self-control and ego depletion (Hagger et al., 2010), so effects of the tested interaction on each version of the measure are reported below.

***Persistence—Unsolvable anagrams.*** Participants' time spent on each of the three unsolvable anagrams was averaged to form a single composite measure of unsolvable anagram persistence (NON_T). Submitting NON_T to a 2x3 factorial ANCOVA (*threat* X *restoration*) with TIME as a covariate revealed no significant main effect of either *threat*, $F(1, 250) < 1$, $p = .96$, $\eta^2_p < .001$, or *restoration*, $F(2, 250) = 1.54$, $p = .22$, $\eta^2_p = .012$, and uncovered a marginally significant interaction between these two factors on unsolvable anagram persistence, $F(2, 250) = 2.78$, $p = .06$, $\eta^2_p = .022$ (see Figure 4a).

Post-hoc comparisons using Fisher's LSD revealed that among participants who received the mood boost task, those who were exposed to death-words tended to persist longer on the task ($M = 37.06s$, $SD = 25.03s$) than those who saw the pain-words ($M = 27.80s$, $SD = 20.63s$), but this difference did not reach statistical significance, $p = .06$, $d = .40$. Furthermore, among participants who were exposed to pain-words during the depleting task, those who received the mood boost task persisted significantly less on the unsolvable anagrams ($M = 27.80s$, $SD = 20.63s$) than those who received either the self-affirmation task ($M = 45.45s$, $SD = 32.31s$), $p = .013$, $d = .65$, or the no-affirmation task ($M = 39.72s$, $SD = 32.13s$), $p = .015$, $d = .44$. The latter

two conditions barely differed on NON_T, $p = .96$, $d = .18$. No other pairwise comparisons reached statistical significance (see Table 4a).

***Persistence—Solvable anagrams.*** Participants' time spent on each of the three solvable anagrams was averaged to form a single composite measure of solvable anagram persistence (WORD_T). Submitting WORD_T to a 2x3 factorial ANCOVA (*threat* X *restoration*) with TIME as a covariate revealed no significant main effects, but uncovered a significant interaction between these two factors on solvable anagram persistence, $F(2, 250) = 3.21$, $p = .04$, $\eta^2_p = .025$ (see Figure 4b).

Post-hoc comparisons using Fisher's LSD revealed a similar pattern to what was observed for NON_T, namely that among participants who received the mood boost task, those who were exposed to death-words persisted significantly longer on the solvable anagrams ($M = 39.22s$, $SD = 36.34s$) than those who saw the pain-words ($M = 27.53s$, $SD = 20.74s$), $p = .027$, $d = .40$. In comparing the *mood boost* and *no-affirmation* conditions across *threat,* mood boost participants tended to persist less than no-affirmation participants after being exposed to pain-words ($M = 27.53s$, $SD = 20.74s$ | $M = 36.53s$, $SD = 20.74s$), $p = .07$, $d = .43$, but had a small yet noteworthy tendency to persist more after being exposed to death-words ($M = 39.22s$, $SD = 36.34s$ | $M = 28.61s$, $SD = 21.12s$), $p = .085$, $d = .36$. No other pairwise comparisons were close to reaching statistical significance (see Table 4b).

***Persistence—All anagrams.*** Participants' time spent on each of the six anagrams was averaged to form a single composite measure of total anagram persistence (TOTAL_T). Submitting TOTAL_T to a 2x3 factorial ANCOVA (*threat* X *restoration*) with TIME as a covariate revealed no significant main effects, but showed a significant interaction between these two factors on total anagram persistence, $F(2, 250) = 3.35$, $p = .037$, $\eta^2_p = .026$ (see Figure 4c).

Post-hoc comparisons using Fisher's LSD revealed that among participants who received the mood boost task, those who were exposed to death-words persisted significantly longer on the anagrams ($M$ = 76.27*s*, $SD$ = 57.10*s*) than those who saw the pain-words ($M$ = 55.33*s*, $SD$ = 39.55*s*), $p$ = .028, $d$ = .43. Furthermore, among those who were exposed to *pain-words* during the depleting task, participants who received the mood boost task persisted significantly less on the anagrams ($M$ = 55.33*s*, $SD$ = 39.55*s*) than those who received either the self-affirmation task ($M$ = 81.59*s*, $SD$ = 55.55*s*), $p$ = .050, $d$ = .54, or the no-affirmation task ($M$ = 75.97*s*, $SD$ = 59.55*s*), $p$ = .022, $d$ = .41. The latter two conditions differed very little on TOTAL_T, $p$ = .74, $d$ = .10. No other pairwise comparisons reached statistical significance (see Table 4c).

**Discussion**

Results on the manipulation check items confirmed that subtle reminders of death, as opposed to pain, did not influence the perceived difficulty of the attention control task or the degree to which participants were distracted by the intrusive words. Furthermore, the predicted interaction between *threat* and *restoration* on task persistence was significant (or marginal: $p$ = .06) across all three variations of the measure. Post-hoc analyses, however, revealed mean patterns that differed substantially from our hypotheses, some of which were even significant in the *opposite* direction of what was expected. These patterns were more or less the same across all three versions of the dependent measure, so I will discuss them collectively rather than in terms of each specific variation.

We predicted, and found, a significant difference on task persistence among participants who received the mood boost task and were exposed to death-words versus pain-words, but observed it in the opposite direction: *mood boost* participants demonstrated greater persistence after being exposed to reminders of death instead of pain. It also appeared that while the

restoration manipulation had little influence overall on persistence among participants who saw the death-words, a more complex pattern emerged among those who saw the *pain-words*; namely, the mood boost task tended to elicit significantly less persistence on the anagrams than either the self-affirmation or no-affirmation task.

Interpreting these results in light of our theorizing is difficult, as the overall pattern of effects is inconsistent with even our most basic predictions. For instance, one effect that has been demonstrated in past research is that self-affirmation, relative to a non-affirming comparison task, counteracts the depletion effect on cognitive measures of persistence (e.g., Schmeichel & Vohs, 2009). Within both the MS and pain-salience groups from this study, however, participants who received the self-affirmation versus no-affirmation task did not differ significantly on anagram persistence. One plausible interpretation of these null effects could be that our attention control task was ineffective at impairing self-control. This construal would fit with past research showing that depletion is necessary for the restorative effects of positive affect or self-affirmation to be observed (Tice et al., 2007; Schmeichel & Vohs, 2009), and is consistent with the lack of difference on our measures of perceived task difficulty in Study 1 (because Study 1 included a *no-suppression* condition). But this interpretation would still not explain the effects observed in the mood boost conditions, which suggest that some underlying process is active: either *diminished* self-control in the pain-salience condition, *enhanced* self-control in the MS condition, or something else altogether.

Given the inconsistency of these findings and the present challenges with interpreting them, we moved to test the same process in Study 3 with a conceptually identical procedure. That is, we evaluated the same hypotheses tested in Study 2, but with some changes to the tasks employed. Due to concerns about the efficacy of our depletion induction, the primary change

implemented in Study 3 was replacing the attention control task with a new, potentially more powerful, induction of ego depletion.

## Study 3

Study 3 aims to test the same interaction effect from Study 2 but with three important changes to the procedure: **1)** We replaced the comparison threat (i.e., reminders of physical pain) with a new threat: reminders of uncertainty, **2)** utilized a new depletion task that involves having participants regulate their typing while responding to a pair of open-ended prompts, and **3)** conducted the study on MTurk so as to utilize a more diverse sample in testing our predictions.

The same predictions made in Study 2 were tested in this study as well. The modifications described in the foregoing paragraph reflect purely operational changes that are not intended to affect the psychological processes being investigated. That is, Study 3 is a procedural replication of Study 2 with the same temporal structure, but with changes in the nature of the depletion task, contents of the comparison threat, and the sample population from which data were collected.

### Method

Participants were 231 adults recruited from Amazon Mechanical Turk (46% female; $M_{AGE}$ = 33, $SD_{AGE}$ = 9). An a priori power analysis conducted with the software package G*Power (Faul et al., 2007) indicated that a total sample of 203 individuals would be needed within a 2x3 factorial design to detect the predicted medium-to-large effect ($d$ = .77; see Hagger et al., 2010) with 80% power and $\alpha$ = .05.

**Depletion induction—threat manipulation.** Instead of employing the attention control video from studies 1 and 2, we used a different induction of ego depletion that has been validated in past research (Bertrams, Englert, & Dickhäuser, 2010). This task involves having participants

engage in a writing task for five minutes while actively avoiding use of the letter '*e*' and the space bar. Meta-analytic data suggest that this task produces a reliable depletion effect $d = .77$ ($CI_{95} = [0.65, 0.90]$; Hagger et al., 2010). This manipulation of mental effort was embedded within a mortality salience or uncertainty salience prime, which at its core involves having participants respond to two open-ended questions about their feelings on either mortality or uncertainty (Rosenblatt et al., 1989; Van den Bos, 2001; Appendix B). As in Study 2, all participants were given instructions to engage in effortful control during the task.

***Manipulation check.*** After completing the writing task, we had participants answer a single manipulation check item: "How much did you have to control yourself while typing on that last task?" (*1 = not at all; 7 = very much*). Because participants in both conditions were instructed to regulate their typing in the same manner, we expected to see no difference in perceived difficulty of the task across *threat* conditions.

**Restoration manipulation.** As in Study 2, participants randomly received the *self-affirmation* task, *mood boost* task, or the *no-affirmation* task.

**Task persistence—Unscrambling anagrams.** Our dependent measure of self-control strength remained the same as in Study 2: participants were asked to try and solve six anagrams—three of which were solvable, and three of which were not. Again, participants were unaware that they were being timed while working on the anagrams, and these measurements represented indicators of task persistence. At the end of the study, participants were fully debriefed and informed that some of the anagram puzzles were actually unsolvable.

**Results**

**Manipulation check.** An independent samples t-test revealed no difference in self-reported effort on the depletion task as a function of *threat*, $t(229) < 1$, $p = .66$.

**Time spent on restoration task.** An independent samples t-test showed no difference in time spent writing on the restoration manipulation (TIME) as a function of *threat* condition, $t(229) < 1$, $p = .55$. Although TIME once again explained a significant proportion of the variance in participants' average persistence on the six anagrams as a whole, $R^2 = .052$, $F(1, 229) = 12.60$, $p < .001$, $\eta^2_p = .052$, as well as the subsets of three unsolvable anagrams, $R^2 = .042$, $F(1, 229) = 9.93$, $p = .002$, $\eta^2_p = .042$, and three solvable anagrams, $R^2 = .032$, $F(1, 229) = 7.47$, $p = .007$, $\eta^2_p = .032$, it was not affected by the *restoration* condition: $F(1, 229) < 1$, $p = .55$, $\eta^2_p = .002$. Thus, while people who spent longer on the *restoration* task also tended to spend longer on the anagram task, this did not appear to be related to the task that they received.

One potential confound that emerged in this study was participants' performance on the anagram task; that is, what percentage of the solvable anagrams they unscrambled correctly (CORRECT). This variable correlated significantly with persistence on the three unsolvable anagrams, $r(229) = .33$, $p < .001$, the three solvable anagrams, $r(229) = .32$, $p < .001$, and on the six anagrams as a whole, $r(229) = .38$, $p < .001$. Performance on the anagram task is psychologically distinct from persistence, and doesn't necessarily impose additional demands on self-control (Baumeister et al., 1998; Muraven et al., 1998). It is not as if the incorrect versus correct answer is 'vying for expression' and must be actively inhibited, but rather that people have more or less experience solving analytic problems and applying well-learned heuristics (Hagger et al., 2010). One confound in operationalizing self-control strength as task persistence is *task enjoyment*, which may conceivably be indexed by performance. That is, people who perform better on the anagram task may also be those who are more likely to intrinsically enjoy such tasks. Because this dispositional enjoyment may lessen the impact of persistence on self-controlled processing (Inzlicht & Schmeichel, 2012), and is thought to be reflected by the

significant correlations between performance and our measures of persistence, we used this variable as a covariate in two of the three primary analyses.[1]

**Primary results.** The same three dependent measures of persistence derived in Study 2 were examined here: 1) average time spent on the three unsolvable, non-words (NON_T), 2) average time spent on the three solvable words (WORD_T), and 3) average time spent on all six anagrams (TOTAL_T).

***Persistence—Unsolvable anagrams.*** Submitting NON_T to a 2x3 factorial ANOVA (*threat* X *restoration*) revealed no significant main effect of either *threat*, $F(1, 225) = 1.18$, $p = .28$, $\eta^2_p = .005$, or *restoration*, $F(2, 225) = 1.54$, $p = .22$, $\eta^2_p = .013$, nor did it uncover a significant interaction effect between the two factors on unsolvable anagram persistence, $F(2, 225) < 1$, $p = .56$, $\eta^2_p = .005$.

***Persistence—Solvable anagrams.*** Submitting WORD_T to a 2x3 factorial ANCOVA (*threat* X *restoration*) with CORRECT as a covariate revealed no significant main effects, and a significant interaction between the two independent factors on solvable anagram persistence, $F(2, 224) = 3.25$, $p = .04$, $\eta^2_p = .028$ (see Figure 5).

Post-hoc comparisons using Fisher's LSD reveal that among participants who received the mood boost task, those who were asked to think about their mortality persisted significantly longer on the solvable anagrams ($M = 32.36s$, $SD = 25.88s$) than those who were asked to think about the feeling of being uncertain ($M = 23.15s$, $SD = 13.44s$), $p = .030$, $d = .45$. In comparing the *mood boost* and *no-affirmation* conditions across levels of *threat,* mood boost participants tended to persist less than no-affirmation participants after thinking about the experience of uncertainty ($M = 23.15s$, $SD = 13.44s$ | $M = 30.21s$, $SD = 23.08s$), $p = .10$, $d = .37$, but tended to

[1] CORRECT was not included as a covariate in the factorial model testing the interaction effect on unsolvable anagram persistence, as none of the anagrams that constituted this measure had a solution.

persist more than non-affirmed participants after considering their mortality ($M$ = 32.36*s*, $SD$ = 25.88*s* | $M$ = 24.18*s*, $SD$ = 20.05*s*), $p$ = .091, $d$ = .35. No other pairwise comparisons reached statistical significance (see Table 5).

***Persistence—All anagrams.*** Submitting TOTAL_T to a 2x3 factorial ANCOVA (*threat* X *restoration*) with CORRECT as a covariate revealed no significant main effects, nor did it uncover a significant interaction effect, $F(2, 224) = 1.54$, $p = .22$, $\eta^2_p = .014$.

**Discussion**

Although we used an entirely different depletion task, comparison threat, and sample demographic, the pattern of results in Study 3 was quite similar to what was observed in Study 2. While in this case the interaction only had a significant effect on the 3-item composite measuring persistence on the *solvable* anagrams, the pattern of means and standard deviations was structurally very similar on both the *unsolvable anagram* and *overall anagram* measures.

The largest pairwise effect that emerged in Study 3 conceptually replicated the most consistent effect seen in Study 2 (with respect to the three measurement variants): in Study 3, participants who received the *mood boost* task persisted significantly longer on the solvable anagrams after thinking about their mortality instead of uncertainty. Also, mood boost participants seemed to persist less on the anagrams than those in the non-affirmed group if they first thought about the feeling of uncertainty, but tended to persist *more* on the anagrams than those in the non-affirmed group if they instead thought about their mortality. These directional comparisons merely reflect trends in the data with small effect sizes and non-significant *p*-values, but are interesting in that they emerged with approximately the same magnitude as those seen in Study 2, and on the same measure—solvable anagram persistence. So, while Study 3 did not produce results in support of our hypotheses, it did appear to conceptually replicate the

effects that were found in Study 2, particularly the effect of threat-salience on solvable anagram persistence after a boost in positive affect.

While this effect did not emerge in the predicted direction, it does signal two important things: 1) There is something about mortality salience that affects people's willingness to persist on an effortful problem-solving task differently than reminders of physical pain or uncertainty, particularly after they've been thinking about simple hedonic pleasures that put them in a good mood; and 2) The mood boost task must have some psychological effect that is not produced by the self-affirmation or no-affirmation task (or at least not at the same magnitude). These findings suggest that a closer look needs to be taken towards the mood boost task in order to determine what sort of effect it might be having on people's self-control capabilities under different types of threat. Furthermore, a major limitation of both Studies 2 and 3 is that neither study is capable of assessing whether or not the presumably taxing mental activity impaired self-controlled processing; this will be discussed further in the limitations section of the General Discussion.

A new pilot study was conducted after the completion of Study 3 in order to clarify the effects of the mood boost task on self-esteem and positive affect. Due to the ambiguous results observed in Studies 2 and 3, testing our restoration tasks in a larger sample than in the first pilot is critical for obtaining a better understanding of their effects. Moreover, a new control (i.e., no-affirmation) task was created and tested in an attempt to establish a more 'neutral' comparison than the one used thus far (i.e., Version 1; Appendix A). A closer investigation of the written responses to Version 1 of the no-affirmation task revealed that some people may have used the task as an opportunity to self-affirm (e.g., "*I studied music theory at NYU. I was a President for the Musical group in High School and I was a choir and band kid. Music reminds me of good memories but more than that, music inherently elicits emotion*"). Indeed, this is a concern that

has been echoed by past researchers as well (Cohen et al., 2000). While participants in the no-affirmation condition did tend to report the lowest levels of self-esteem in the first pilot study, the main effect of condition on state self-esteem was small and failed to reach statistical significance ($p$ = .07, $d$ = .25). Thus, we conducted a new pilot study to better understand the effects of these tasks and how they might be influencing self-control.

## Pilot Test 2: New control and larger sample

A pilot test was conducted on Amazon Mechanical Turk in order to test the effects of our three inductions on state self-esteem and mood with a larger sample, and to test a new and original version of the *no-affirmation* control task (see Version 2 in Appendix A). This task was designed to resemble the *self-affirmation* and *mood boost* tasks as closely as possible, and involves having participants rank 12 sentences about statistics, ostensibly from a textbook, in terms of how clear they would be to the average college student. Then, in the next section they are asked to write about a textbook they've read and thought was well written, as well as why it might be valuable to the average college student.

The reason for creating a new control condition was because it is not clear whether or not the original version of the no-affirmation task (Version 1) had an effect on self-esteem that was simply undetected in the first pilot due to a small sample size. Therefore, we designed a comparable task in which participants are still asked to rank 12 items and then write about their top choice, but in this case the items were merely declarative sentences about statistics (e.g., "Orthogonality is often desirable in statistical applications"). Although one could imagine a statistics-lover getting a potential boost in self-esteem from such a task, we believe that in general it will be relatively immune to providing such opportunities to self-affirm.

### Method

We randomly assigned 258 participants (44% female*;* $M_{AGE}$ = 34, $SD_{AGE}$ = 10) to complete the *self-affirmation* task, the *mood boost* task, or the *no-affirmation* task. Afterward, all participants filled out the State Self-Esteem Scale (Heatherton & Polivy, 1991) and a 20-item Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). Once again, we established a global index of *positive affect* by creating a composite variable averaging participants' scores across all 20 PANAS items, with the 10 negative emotion-items reverse-coded.

As in the initial pilot study, we had three main predictions that, if affirmed, would indicate that these tasks produce the same effects on mood and self-esteem that we intend them to: 1) The *self-affirmation* task should increase self-esteem relative to the *mood boost* and *no-affirmation* tasks, 2) the *mood boost* and *no-affirmation* groups should not be significantly different with respect to self-esteem, and 3) the *mood boost* task should increase positive affect relative to the *no-affirmation* task.

**Results**

Preliminary analyses revealed that the amount of time participants spent engaged in the writing task (WRITE_T) correlated significantly with a 10-item mean composite from the PANAS that represents *negative affect* (α = .95), $r(256) = -.19$, $p = .002$, such that the longer people spent writing, the less negative affect they reported. This variable was also marginally correlated our the 20-item mean composite for *positive affect* (α = .87), $r(256) = .11$, $p = .089$. WRITE_T was therefore included as a covariate in subsequent analyses.

**Self-esteem.** Internal consistency, after reverse coding 13/20 items, was high (α = .95). A one-way ANCOVA was conducted with WRITE_T as a covariate to examine the effect of the manipulation on state self-esteem scores, returning a significant omnibus effect, $F(2, 254) =$

3.17, $p = .044$, $\eta^2_p = .02$ (see Figure 6). Post-hoc comparisons using Fisher's LSD revealed that participants who completed the self-affirmation task reported significantly higher levels of self-esteem ($M = 3.95$, $SD = 0.66$) than those who completed the no-affirmation task ($M = 3.73$, $SD = 0.82$), $p = .032$, $d = .30$. Contrary to our prediction, however, the mood boost group also reported significantly higher levels of self-esteem ($M = 3.94$, $SD = 0.67$) than the no-affirmation group ($M = 3.73$, $SD = .82$), $p = .030$, $d = .28$. Moreover, participants in the self-affirmation condition barely differed from those in the mood boost condition, $p = .92$, $d = .02$ (see Table 6).

**Positive affect.** The 20-item composite for positive affect (with 10 negative items reverse-coded) displayed a high level of internal consistency ($\alpha = .89$). Submitting positive affect composite scores to an ANCOVA with WRITE_T as a covariate returned a significant omnibus effect, $F(2, 254) = 3.01$, $p = .050$, $\eta^2_p = .023$ (see Figure 7). Post-hoc comparisons using the Fisher LSD test revealed that participants who completed the mood boost task reported significantly higher levels of positive affect ($M = 3.85$, $SD = 0.54$) than those who received the no-affirmation task ($M = 3.70$, $SD = 0.61$), $p = .022$, $d = .26$. Also, participants who received the self-affirmation task reported higher positive affect ($M = 3.82$, $SD = 0.50$) than those in the no-affirmation condition ($M = 3.70$, $SD = 0.61$), but this difference was only marginally significant, $p = .06$, $d = .22$. *Mood boost* participants reported only slightly higher positive affect than self-affirmation participants, but this difference did not come close to statistical significance, $p = .76$, $d = .06$ (see Table 7).

**Discussion**

Overall, these findings illustrate that the self-affirmation and mood boost tasks lead to significant increases in both state self-esteem and positive affect (relative to the new no-affirmation task). These effects were small, but reliable due to the power afforded by such a large

sample. Thus, these findings confirm that this "mood boost" task does *not* have an isolated effect on positive affect, and was therefore not operating in the previous experiments as had been assumed. Indeed, relative to the control task, the self-affirmation and mood boost tasks appear to have nearly identical effects on measures of state self-esteem (Heatherton & Polivy, 1991) and positive affect (PANAS; Watson et al., 1988). As these findings are clearly important for interpreting the results of Studies 1-3, they also raise an interesting question: given that the self-affirmation and mood boost tasks increase self-esteem and positive affect to an equal degree, what psychological processes were present in Studies 2-3, following the threat-salience manipulation, that drove their divergent effects on self-control?

## General Discussion

Overall, these studies did not provide support for our hypotheses, but did not show strong evidence against them either. Among the various limitations discussed below, the one utterly ruinous problem within all of these studies was methodological: that is, our manipulation of cognitive restoration (particularly the *mood boost* task) did not operate in the way that had been intended or predicted, nor even in ways that would afford an investigation into the central aspects of our hypotheses. The most novel prediction that we attempted to test within these studies was that a boost in positive mood—without an accompanying boost in self-esteem—would be *ineffective* at restoring self-control among people who were reminded of their mortality, but *effective* at restoring control among those who were reminded of other aversive, but non-specialized, threats (i.e., pain; uncertainty). Due to the findings in both pilot studies (especially the second, which had greater power), the validity of this hypothesis cannot be assessed by the current data. Essentially, our results show that we failed to observe or even test the isolated effects of positive mood on self-control and threat-compensation processes.

While Study 1 was originally thought to support our predictions about the interaction, under MS, of restoration (self-esteem vs. mood) and depletion (suppression vs. no-suppression) on self-control strength, the pilot data drastically limit the interpretability of these findings. Furthermore, while the power analysis from this study indicated that we had sufficient power to detect a medium-to-large effect ($d$ = .75), the effect that we observed was small ($d$ = .41). This may be the sign of an underlying process at work, as predicted, but may also simply be an artifact of the sample size, given that a post-hoc power analysis in G*Power indicated that detecting a real effect of that size within our design (with 80% power and $\alpha$ = .05) would require a minimum sample size of 280, which is more than double what we collected.

Effect sizes for the predicted interactions in Studies 2-3 were also fairly small given our sample sizes ($d$ = .30-.33), but the results in these studies were nonetheless interesting for the reason that the most notable pairwise effect observed on task persistence was in the mood boost conditions across different exposures to threat. While the group means were initially interpreted as reflecting the opposite pattern to what was predicted (i.e., that MS participants would persist *less* after a mood boost than those exposed to other threats, not *more*), contrasting these findings with our original predictions is no longer meaningful because of what was observed in the pilots. Furthermore, this effect was *not* observed in the self-affirmation condition, which, according to the pilot data, improves both self-esteem and mood in an almost identical manner as the mood boost task—thus, some third variable must have driven the differential patterns seen within these two conditions, such that the mood boost and self-affirmation tasks led to differences on some construct that *did* interact differently with different sorts of threat. So, while these data are mostly uninformative for evaluating our specific hypotheses, they do seem to suggest that reminders of mortality have effects on task persistence that differ from other types of threat.

The same pattern of means on solvable anagram persistence emerged in both Studies 2 and 3; this occurred even though the nature of both the prime and comparison threat were entirely different. That is, reminders of pain and uncertainty elicited very similar effects on persistence across the restoration conditions, but the pattern of means seen in the mortality salience group was different (although similar to itself across the two studies). This finding, while difficult to interpret with respect to detail, supports our broader assumption that mortality salience elicits compensatory strategies that are unique from other types of aversive threat. These strategies are interpreted in TMT as reflecting the operations of underlying compensatory motives for threat-reduction (Pyszczynski et al., 2004), and therefore the idea that reminders of death promote some level of motivational specificity in comparison with other threats appears to be supported, at least minimally, by these findings. However, more research on this topic will be essential for developing a reliable understanding of these processes; the current studies, due to their limitations, can provide no more than suggestive hints as to what these effects might be.

I will now briefly review some other limitations within these studies, as well as how they can inform opportunities for future research. Then, I will describe some plausible neural mechanisms that may underlie the nature of motivational specificity with respect to self-control, as well as speculate on some future directions for studying the neurobiological underpinnings of cognitive restoration.

**Limitations and Future Directions.**

**Mood boost task.** As explained, the pilot data revealed that the mood boost task did not operate the way that was intended. In hindsight, there are clear reasons why this may have occurred. Although all of the ranked items were designed to reflect purely hedonic activities that don't require any ability or skill (e.g., "drinking a glass of ice-cold water"), it has been rightly

noted by past researchers that "students tend to turn almost any self-reflective writing task into a self-affirming one" (Cohen et al., 2000, p. 1154). And so future studies aimed at testing these hypotheses will likely be more successful at isolating the effects of positive affect if an alternative, non-free-writing induction is used.

Some potential alternative mood inductions that have been used in past research and don't intuitively appear to have self-affirming capacities include: showing participants a humorous video (Tice et al., 2007), exposing them to pre-validated images designed to increase positive affect (IAPS; Lang, Bradley, & Cuthbert, 2008), presenting them with simple melodies (Strube, Turner, Patrick, & Perrillo, 1983), and diffusing pleasant ambient odors in the testing room (particularly orange and lavender; Lehrner, Marwinski, Lehr, Johren, & Deecke, 2005).

**Establishing a baseline for the depletion effect.** None of the studies reported in this document were able to establish a baseline for the depletion effect. Rather, we assumed that depletion did or did not occur based on past research estimating the high reliability of the manipulations (e.g., Hagger et al., 2010). The intent was to utilize the no-affirmation conditions as means of representing baseline levels of depletion, but our ambiguous results prevented the potential usefulness of this strategy. Future studies that we conduct will utilize a control condition in which the explicit goal is to establish a baseline with which to compare the ostensibly depletive or restorative effects expected in other conditions.

**Measuring motives for self-enhancement.** Given that the primary hypothesis assumes that mortality salience triggers motives to enhance self-esteem, it is important to test this assumption in future studies to see whether explicit reports of motivation actually mediate subsequent levels of self-esteem striving. If the theorizing in TMT is correct, then we should see that participants exposed to MS subsequently report a stronger motive to enhance self-esteem

than those exposed to other types of threat, particularly after either a period of delay (Greenberg et al., 1992), or immediately after MS and a mentally depleting exercise (Arndt et al., 1997). A study aimed at testing this explicitly would benefit an understanding of the underlying motivational processes that are active under conditions of threat and self-control.

**Dependent measures of self-control strength.** The ego depletion effect has come under heavy criticism from researchers who suggest that it may be much smaller than estimated in meta-analytic studies, or may not even be real at all (Carter & McCullough, 2014). Motivation for adopting this perspective is warranted, I think, due to the widely diverse ways by which the effect is operationalized in psychological research. In their meta-analysis, for instance, Hagger et al., (2010) group popular measures of depletion into a variety of distinct categories reflecting tests of conceivably differentiable functions, such as response inhibition and the updating of working memory contents.

Research in cognitive neuroscience has even shown that certain brain regions involved in effortful control exhibit a reliable degree of intra-regional specialization that dissociates between functions such as response inhibition and the representation of expected values (with respect to the prefrontal cortex; PFC), as well as set-switching and conflict monitoring (with respect to the anterior cingulate cortex; ACC; Gläscher et al., 2012). Moreover, the findings from these studies also showed that lesions affecting particular brain regions could radically impair one executive function while leaving the others entirely intact (Gläscher et al., 2012). These results suggest that the depletion effect may be a much more function-specific phenomenon than initially assumed.

For instance, the cognitive estimation task used in Study 1 (this document) has been shown to elicit activity in certain brain regions that are distinct (i.e., ventromedial PFC; Bullard et al., 2004) from those activated by tasks requiring impulse inhibition (i.e., dorsal ACC; Inzlicht

et al., 2016). However, past research, as well as the current research, has used these tasks as operationalizations of the same effect: ego depletion. Recently, some psychologists have suggested that response inhibition is the only function truly involved in ego depletion, relying on evidence that ERPs generated by the ACC (i.e., ERN signals) can be modulated by bouts of effortful suppression, but not other forms of cognitive processing (Inzlicht et al., 2016; Inzlicht & Gustell, 2007; Wang & Yang, 2008). Thus, any further studies attempting to identify depletion and/or restoration effects in self-control should carefully attend to the specific measures and inductions being used, as well as how these might relate with findings from neuropsychological and neuroimaging studies.

**Potential Mechanisms of Restoration**

To me, the fundamentally compelling goal behind the current research is to investigate how salient threats constrain the nature of reward-recognition, and concerns whether or not motivational specificity limits cognitive processing by restricting the capacity for control until certain compensatory impulses are satisfied (i.e., those that are elicited by the threat).[2] In line with this goal, there is a great deal of neurobiological research focused on the various brain systems involved in processing rewards and motives.

According to the authors of one review of dopamine and reward research, reward is not a unitary process of valuation wherein dopamine signals are treated as a "common neural currency" for hedonic pleasure, but rather a collection of interacting and competing processes involving dopamine systems that signal *incentive value* (Berridge & Robinson, 1998). Studies have shown that these dopamine systems do not mediate the experience of pleasure that results from unconditioned stimuli (e.g., food, sex), but rather the *cognitive expectation* of reward;

[2] This relates with an idea generated by Dr. Mark Landau that we termed the "*compensation continuum.*" Revisiting this topic is likely to be fruitful in light of the present discussion.

specifically, this idea implies that the individual must have an explicit and intentional representation of the reward in order for dopamine signals to encode its incentive value (Dickinson & Balleine, 1995). Berridge and Robinson (1998, p.355) go on to note that: "If dopamine systems mediate incentive value […] then dopamine must mediate learning the explicit relation between actions and specific outcomes." Given our understanding of the shifting-priorities model of self-control, these findings appear to suggest that the mesolimbic and neostriatal dopamine systems encode the changing evaluations associated with stimuli that may or may not function as rewards depending on the motivational and attentional context.

In line with the idea that these dopamine systems represent information about incentive value, a '2 systems' hypothesis has also been suggested, proposing that two distinct reward systems show differential activation in accordance with *deprived* or *non-deprived* physiological states; studies testing this hypothesis were performed with rats, so here "deprived" refers to "hungry" (Bechara, Harrington, Nader, & Van der Kooy, 1992). Nevertheless, this hypothesis is congruent with the finding that dopamine systems represent incentive values, such that extended periods during which the satisfaction of a motive is frustrated (i.e., 'deprivation') may lead to the 'take-over' of a separate system encoding competing incentive values (i.e., those that represent the influence of categorical changes in motivation).

While these interpretations are speculative at best, they represent a potential avenue of identifying the neural reward mechanisms that underlie the shifting-priorities of self-control and depletion, as well as the motivational specificity in threat-compensation processes and cognitive restoration. If distinct and competitive reward systems are capable of representing *conflicting* incentive values for the same stimulus (an idea rejected by some; see Kable & Glimcher, 2009), then enhancement of a *content-specific* motive may involve, at the neural level, a system capable

of encoding the unique configuration of incentive values that represent the particular motive, and that will progressively vie for dominance over the current system that is busy encoding values relative to a different explicit motive (e.g., continued self-control). Because incentive values are established in part by explicit representations of the motives that they subsume, this hypothesis would suggest (if it can even be tested) that motivational specificity in threat-compensation emerges during learning and development—in this case, specialized psychological responses to threat (e.g., affirming sources of life's meaning) would theoretically be formed through practice and repetition; that is, the more often an individual is faced with threat X, the more often they would be expected to respond to future instances of threat X with motivationally specific compensatory strategies.

Indeed, although TMT argues that a motive for self-esteem/worldview defense/attachment security is activated by reminders of one's mortality, these compensatory motives are unlikely ever, if not rarely, adopted at such a broad level of abstraction. Rather, motives to achieve explicit, yet terror-managing, goals are what MS actually engenders in people; meaning that the man who expresses himself through his paintings might become motivated to paint when death comes to mind, and that the singer who feels most valuable when performing might be motivated to set up her next gig when reminded of her mortality. While I'm sure there is no one arguing against this obvious point, it reflects the potential for a real connection between complex threat-defense motives that are specialized for concerns about mortality, autonomy, and other core threats, and reward systems at the neural level that encode incentive values with respect to explicit goal representations.

**Conclusion**

Investigating the relationship between 1) neural systems that ground the processing of motivated rewards, and 2) the cognitive processes that underlie self-control and threat-compensation could offer an exciting direction for research aimed at developing a neurobiological understanding of specialized threat-defense systems, and why the capacity for self-control seems to oscillate between effort-modulated patterns of depletion and restoration. Returning back to the studies outlined in the current paper, however, more experiments in the lab are undoubtedly necessary for fully exploring the hypothesis that specific threats constrain self-control processes in unique ways. Indeed, while we did not find support for our predictions, this is most certainly a function of the failure in isolating an effect of positive affect relative to self-esteem within our manipulation. Furthermore, establishing what outcome within the context of our study reflects a baseline ego depletion effect will be critical for interpreting the results of future experiments. More studies will therefore need to be conducted in order to fully investigate this thesis. Although the studies described in the foregoing sections did not turn out as initially intended, they nevertheless serve the invaluable function of indicating what needs to be changed and what can be done in the next study to give this hypothesis a stronger test.

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# Appendix A

## Restoration inductions

***Self-affirmation task:***

*Instructions:* Below is a list of characteristics and values, some of which may be important to you, and some of which may be unimportant. Please rank these values and qualities in order of their importance to you, personally, from MOST important to LEAST important. Their current order is merely sorted alphabetically. Simply click-and-drag the items vertically on the screen to rank them in the order of your choosing. After you've ranked the items, click the arrows at the bottom of the screen.

*1 = MOST important to me; 12 = LEAST important to me.*

My values and qualities:

_____ Artistic skills/Aesthetic appreciation

_____ Sense of humor

_____ Relations with friends/family

_____ Spontaneity/living life in the moment

_____ Social skills

_____ Athletics

_____ Music ability/appreciation

_____ Neatness/tidiness

_____ Physical attractiveness

_____ Creativity

_____ Business/managerial skills

_____ Romantic values

Now, in the line just below, please write what value you ranked #1. Then, in the larger space, write a brief account of why this value is important to you, and an instance when your 1st-ranked value played an important role in your life. Take a moment and write at least a few sentences.

# Appendix A

***Mood boost task:***

*Instructions:* Below is a list of activities that can put people in a good mood, some of which may be important to you, and some of which may be unimportant. Please rank these activities in order of how much each one puts you in a good mood, from the activity that MOST puts you in a good mood, to the activity that LEAST puts you in a good mood. Their current order is merely sorted alphabetically. Simply click-and-drag the items vertically on the screen to rank them in the order of your choosing. After you've ranked the items, click the arrows at the bottom of the screen.

*1 = activity that MOST puts me in a good mood; 12 = activity that LEAST puts me in a good mood.*

My activities:

_____ Being outside

_____ Drinking delicious coffee

_____ Driving through multiple green lights in a row

_____ Eating ice-cream

_____ Enjoying a glass of ice-cold water

_____ Getting a massage

_____ Having a good breakfast

_____ Relaxing in the grass

_____ Seeing cute animals play

_____ Sitting in a warm and cozy chair

_____ Trying a piece of chocolate

_____ Watching quality TV

Now, in the line just below, please write what activity you ranked #1. Then, in the larger space, write a brief account of why you like this activity, what it feels like when you enjoy this activity, and a time when your 1st-ranked activity really put you in a good mood. Take a moment and write at least a few sentences.

# Appendix A

***No-affirmation task:***
***(VERSION 1)***

*Instructions:* Below is a list of characteristics and values, some of which may be important to the average college student, and some of which may be unimportant. Please rank these values and qualities in order of their importance to the average student, from MOST important to LEAST important. Their current order is merely sorted alphabetically. Simply click-and-drag the items vertically on the screen to rank them in the order of your choosing. After you've ranked the items, click the arrows at the bottom of the screen.

*1 = MOST important to the average student; 12 = LEAST important to the average student.*

Values and qualities:

_____ Artistic skills/Aesthetic appreciation

_____ Sense of humor

_____ Relations with friends/family

_____ Spontaneity/living life in the moment

_____ Social skills

_____ Athletics

_____ Music ability/appreciation

_____ Neatness/tidiness

_____ Physical attractiveness

_____ Creativity

_____ Business/managerial skills

_____ Romantic values

Now, in the line just below, please write one low-ranked value you placed near the bottom of the list. Then, in the larger space, write a brief account of why this value might be important to the average college student. Take a moment and write at least a few sentences.

***No-affirmation task:***
***(VERSION 2)***

*Instructions:* Below is a list of sentences from a statistics textbook, some of which may be easy for the average college student to understand, and some of which may be difficult. Please rank these sentences in order of how clear they would be to the average student, from MOST clear to LEAST clear. The order they are in now is just alphabetical. Click-and-drag the sentences vertically on the screen to rank them in the order of your choosing. After you've ranked them, click the arrows at the bottom of the screen.

*1 = MOST clear to the average student; 12 = LEAST clear to the average student.*

Sentences:

_____ "Bivariate correlation measures the association between two variables."

_____ "In non-experimental research, the levels of the independent variables are not manipulated by the researcher."

_____ "In statistics, an outlier is an observation that is distant from the other observations."

_____ "It is possible to keep the Type 1 error rate at 5% no matter how many variables are used."

_____ "Orthogonality is often desirable in statistical applications."

_____ "Plausibility of the normality assumptions needs to be considered in selection of the appropriate estimation technique."

_____ "Samples are measured to make generalizations about populations."

_____ "The assumption of linearity is that there is a straight-line relationship between two variables."

_____ "The distinction between continuous and discrete variables is not always clear."

_____ "The *F* ratio for mean square regression over mean square residual tests the significance of multiple *R*."

_____ "Variables are roughly dichotomized into two major types: independent and dependent."

_____ "Variance measures how far each number in a set is from the mean."

Now, think about a textbook that you've used for school and thought was well written. In the line just below, write what class this textbook was in. Then, in the larger space, describe why this textbook was better than others that you've read in school. Also, write about how this style of textbook might be valuable to the average college student. Take a moment and write at least a few sentences.

# Appendix B

## Mortality salience and comparison primes

***Attention control video—Death-word list:*** *Throughout the 6-minute video, the distracting words appear in the order listed below. Each word appears on the screen for 9 seconds, with a 1 second delay between words. All words appear in white font on a black background. The 6 once-repeated death-words are highlighted only for reference on this page.*

| | | | | | |
|---|---|---|---|---|---|
| 1. Glue | 7. Kite | 13. Hair | 19. Tree | 25. Book | 31. Corpse |
| 2. Crutch | 8. Grave | 14. Death | 20. Green | 26. Soup | 32. Ten |
| 3. Dead | 9. Shoe | 15. Pulse | 21. Die | 27. Grave | 33. Shirt |
| 4. Boot | 10. Gum | 16. Pole | 22. Bean | 28. Death | 34. Dead |
| 5. Tire | 11. Die | 17. Cane | 23. Skull | 29. Dog | 35. Large |
| 6. Skull | 12. Blue | 18. Corpse | 24. Jump | 30. Floor | 36. Cut |

***Attention control video—Pain-word list:*** *The video is identical to that shown in the death-word condition, with the exception that each death-word has been replaced with a pain-word. The 6 once-repeated pain-words are highlighted only for reference on this page.*

| | | | | | |
|---|---|---|---|---|---|
| 1. Glue | 7. Kite | 13. Hair | 19. Tree | 25. Book | 31. Pains |
| 2. Crutch | 8. Sore | 14. Ache | 20. Green | 26. Soup | 32. Ten |
| 3. Hurt | 9. Shoe | 15. Pulse | 21. Sick | 27. Sore | 33. Shirt |
| 4. Boot | 10. Gum | 16. Pole | 22. Bean | 28. Ache | 34. Hurt |
| 5. Tire | 11. Sick | 17. Cane | 23. Pain | 29. Dog | 35. Large |
| 6. Pain | 12. Blue | 18. Pains | 24. Jump | 30. Floor | 36. Cut |

# Appendix B

***Depletion task instructions:***

This study looks at how thinking about significant aspects of life affects typing speed and accuracy. So, while making your responses to the following questions, we ask that you make an effort to type without using the letter ***e*** and without using the space bar.

For example, if you wanted to say:

***The weather is nice.***

Then instead you would type:

***Thwathrisnic.***

---

We will ask you to write for 5 minutes. It is important that you keep two things in mind for the full 5 minutes:

- Continue typing for the entire time. If you feel as though you responded to questions before the 5 minutes is up, try elaborating a bit more or coming up with examples of what you wrote already.
- As you type, avoid using both the letter ***e*** and the space bar.

After 5 minutes, the computer will automatically go to the next part of the survey.

---

***Mortality salience prompts:***

1. Please describe the emotions aroused by the thought of your own death.

2. Describe, as specifically as you can, what it feels like to know that your life will end no matter what happens.

***Uncertainty salience prompts:***

1. Please describe the emotions aroused by the thought of your being uncertain.

2. Describe, as specifically as you can, what it feels like to know that you will feel uncertain in life no matter what happens?

# Appendix C

## Dependent measures: Self-control strength

***Biber cognitive estimation test (Bullard et al., 2004)***

Please answer the following questions as best you can. For most items, there is no perfectly correct answer – simply give your best estimate.

1. How many seeds are there in a watermelon? _____
2. How much does a telephone weigh? _____
3. How many sticks of spaghetti are there in a one pound package? _____
4. What is the distance an adult can walk in an afternoon? _____
5. How high off a trampoline can a person jump? _____
6. How long does it take a builder to construct an average-sized house? _____
7. How much do a dozen, medium-sized apples weigh? _____
8. How far could a horse pull a farm cart in one hour? _____
9. How many brushings can someone get from a large tube of toothpaste? _____
10. How many potato chips are there in a 40-cent, one-ounce bag? _____
11. How long would it take an adult to handwrite a one-page letter? _____
12. What is the age of the oldest living person in the United States today? _____
13. How long is a dinner spoon? _____
14. How much does a folding chair weigh? _____
15. How long does it take to iron a shirt? _____
16. How long is a giraffe's neck? _____
17. How many slices of bread are there in a one-pound loaf? _____
18. How much does a pair of men's shoes weigh? _____
19. How much does the fattest man in the United States weigh? _____
20. How long does it take for fresh milk to go sour in the refrigerator? _____

## Appendix C

***Anagram persistence task:***

---

On the pages that follow, you will be presented with a series of anagrams. Please rearrange the scrambled letters to form a single word (using all of the letters).

Example: OBKO

By re-arranging the letters, the solution is BOOK.

---

Let's practice. Please unscramble the following letters:

EDR
[text box goes here]

---

The solution is RED.

---

Some of the anagrams may have more than one solution. Remember, you should use all of the letters to make a single word. **If you are unable to solve an anagram, you can click the arrow to move on to the next anagram.** Let's begin...

---

GATMEEMNNA

Unscramble the letters above to create a single word (using all letters).

---

RTNUATOAS

Unscramble the letters above to create a single word (using all letters).

---

SMCYSAGITN

Unscramble the letters above to create a single word (using all letters).

---

LNAGPEATIR*

Unscramble the letters above to create a single word (using all letters).

---

---

LOIMCPRIS*

Unscramble the letters above to create a single word (using all letters).

---

IRCBOEEYR*

Unscramble the letters above to create a single word (using all letters).

---

* *Non-words; unsolvable items. All 6 anagrams are displayed in random order.*

# Figures

***Figure 1: Effect of Condition on State Self-Esteem***

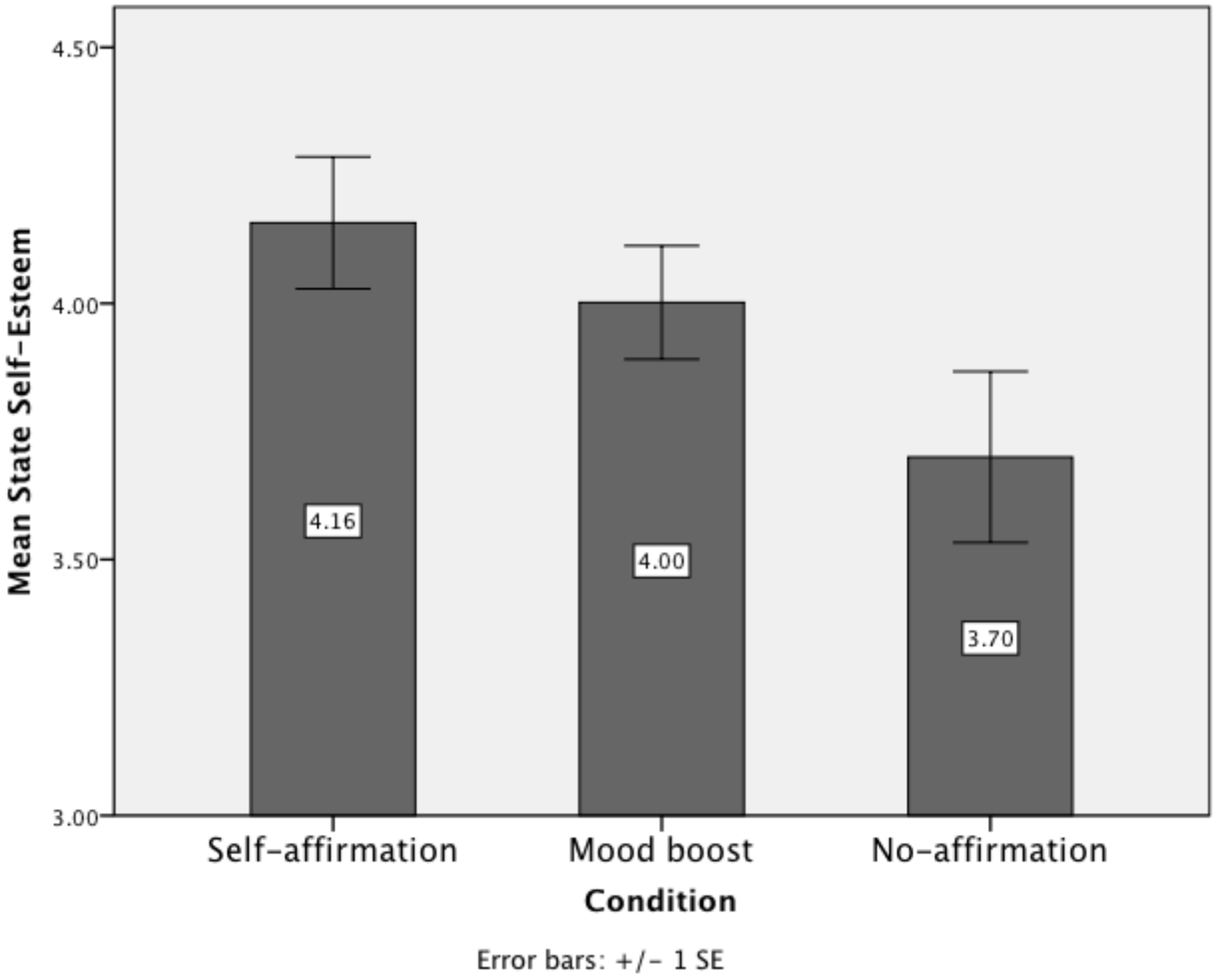


Table 1

*Means and Standard Deviations on the Measure of State Self-Esteem (Heatherton & Polivy, 1991) as a Function of Restoration Condition*

| | | State Self-esteem scores | |
|---|---|---|---|
| Restoration manipulation | *n* | *M* | *SD* |
| Self-affirmation | 27 | 4.16 | .67 |
| Mood boost | 36 | 4.00 | .67 |
| No-affirmation | 31 | 3.70 | .93 |
| Total | 94 | 3.95 | .78 |

*Note.* Participants rated their level of agreement with 20 statements by using a 5-point Likert scale (*1 = not at all; 5 = extremely*). Higher values indicate higher levels of self-esteem.

***Figure 2: Effect of Condition on Positive Affect***

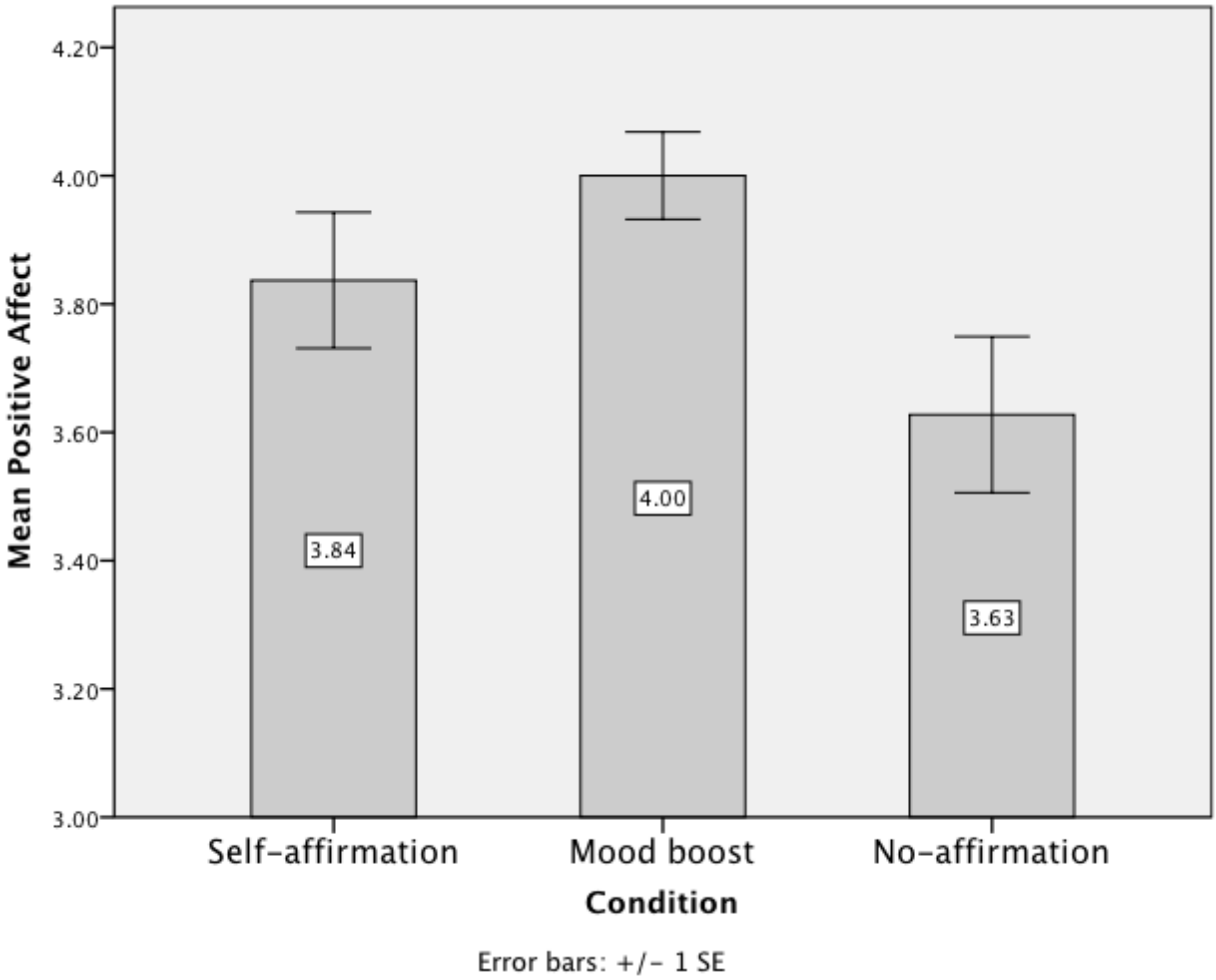


Table 2

*Means and Standard Deviations on the 20-item Composite Measure of Positive Affect (PANAS; Watson et al., 1988) as a Function of Restoration Condition*

| | | Positive affect scores | |
|---|---|---|---|
| Restoration manipulation | *n* | *M* | *SD* |
| Self-affirmation | 27 | 3.84 | .55 |
| Mood boost | 36 | 4.00 | .41 |
| No-affirmation | 31 | 3.63 | .68 |
| Total | 94 | 3.83 | .57 |

*Note.* Participants used a 5-point scale to rate how much they were currently feeling a particular emotion (*1 = not at all; 5 = extremely*). Ratings on 10 negative emotions were reverse-coded and averaged with ratings on 10 positive emotions. Higher values indicate more positive affect.

***Figure 3: Effects of Mortality Salience, Depletion, and Restoration on Self-Control Strength***

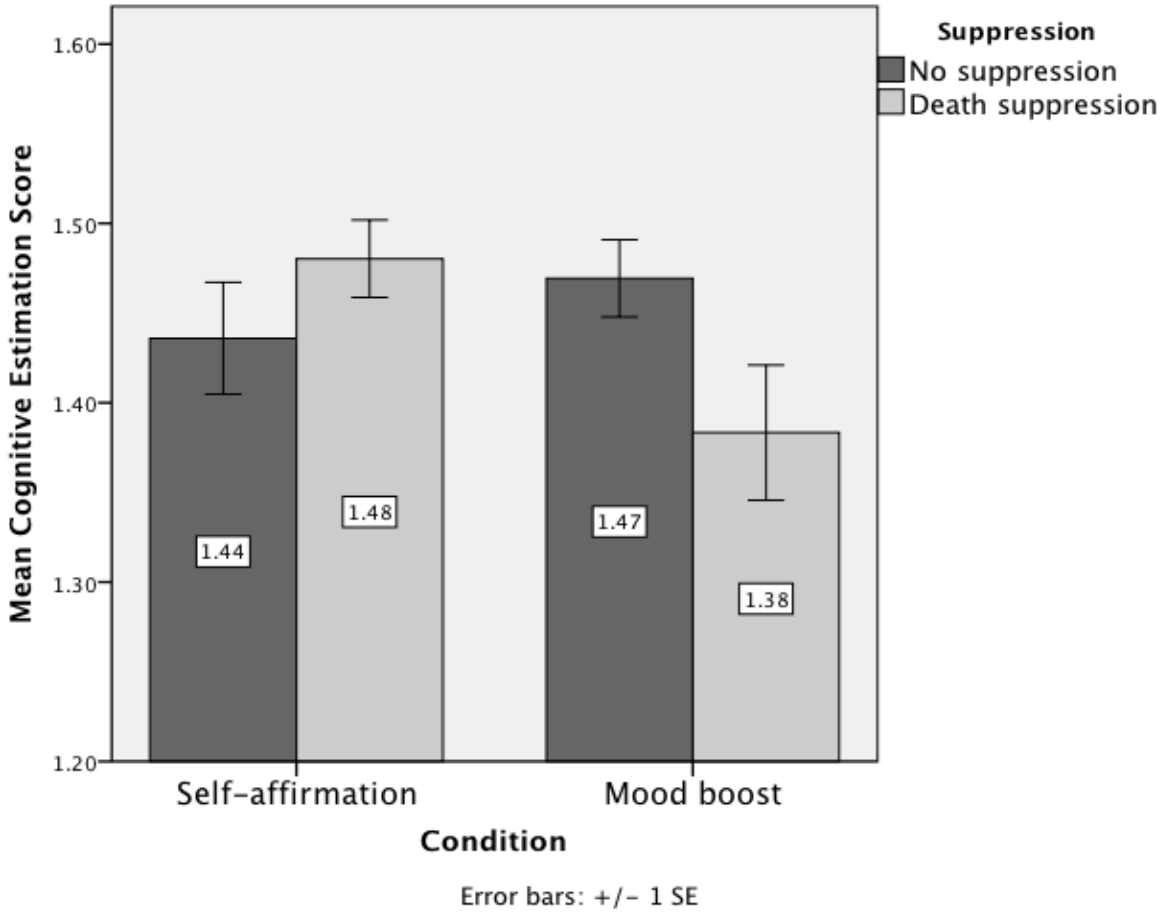


Table 3

*Means and Standard Deviations on the Measure of Self-Control Strength (BCET; Bullard et al., 2004) as a Joint-Function of Ego Depletion and Restoration*

| | | | Cognitive estimation scores | |
|---|---|---|---|---|
| Suppression | Restoration manipulation | *n* | *M* | *SD* |
| Death suppression | Self-affirmation | 33 | 1.48 | .12 |
| | Mood boost | 33 | 1.38 | .22 |
| No suppression | Self-affirmation | 32 | 1.44 | .18 |
| | Mood boost | 31 | 1.47 | .12 |
| | Total | 129 | 1.44 | .17 |

*Note.* Participants estimated 20 ambiguous values. Responses were coded 0-2 based on data from a normative sample. Higher scores indicate estimates closer to the norm; i.e., greater self-control.

***Figure 4a: Self-Control Strength as a Joint Function of Threat Salience and Restoration***

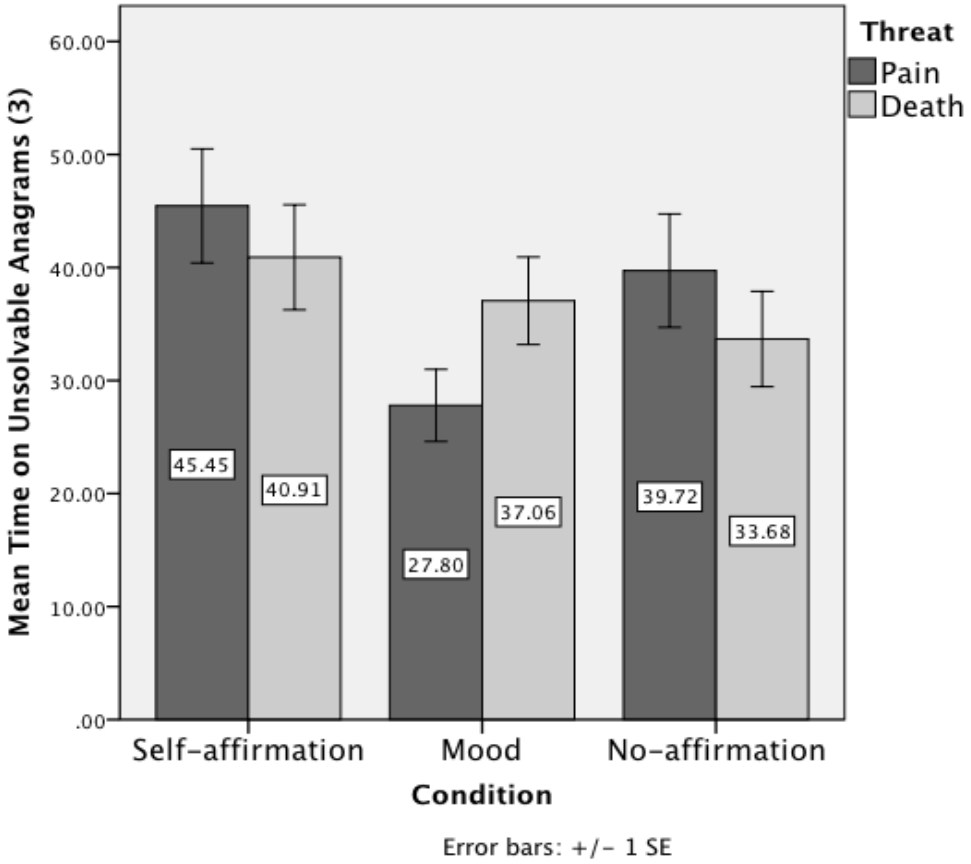


Table 4a

*Means and Standard Deviations on the Measure of Self-Control Strength (Unsolvable Anagram Persistence) as a Joint Function of Threat-Salience and Restoration*

| | | | Unsolvable anagram persistence | |
|---|---|---|---|---|
| Threat | Restoration manipulation | *n* | *M* | *SD* |
| Mortality salience | Self-affirmation | 45 | 40.91 | 31.14 |
| | Mood boost | 42 | 37.06 | 25.03 |
| | No-affirmation | 46 | 33.68 | 28.59 |
| Pain salience | Self-affirmation | 41 | 45.45 | 32.31 |
| | Mood boost | 42 | 27.80 | 20.63 |
| | No-affirmation | 41 | 39.72 | 32.13 |
| | Total | 257 | 37.38 | 28.89 |

*Note.* Mean values represent average number of seconds trying to solve three unsolvable anagrams. Covariates appearing in the model are evaluated at the values: TIME = 92.52.

***Figure 4b: Self-Control Strength as a Joint Function of Threat Salience and Restoration***

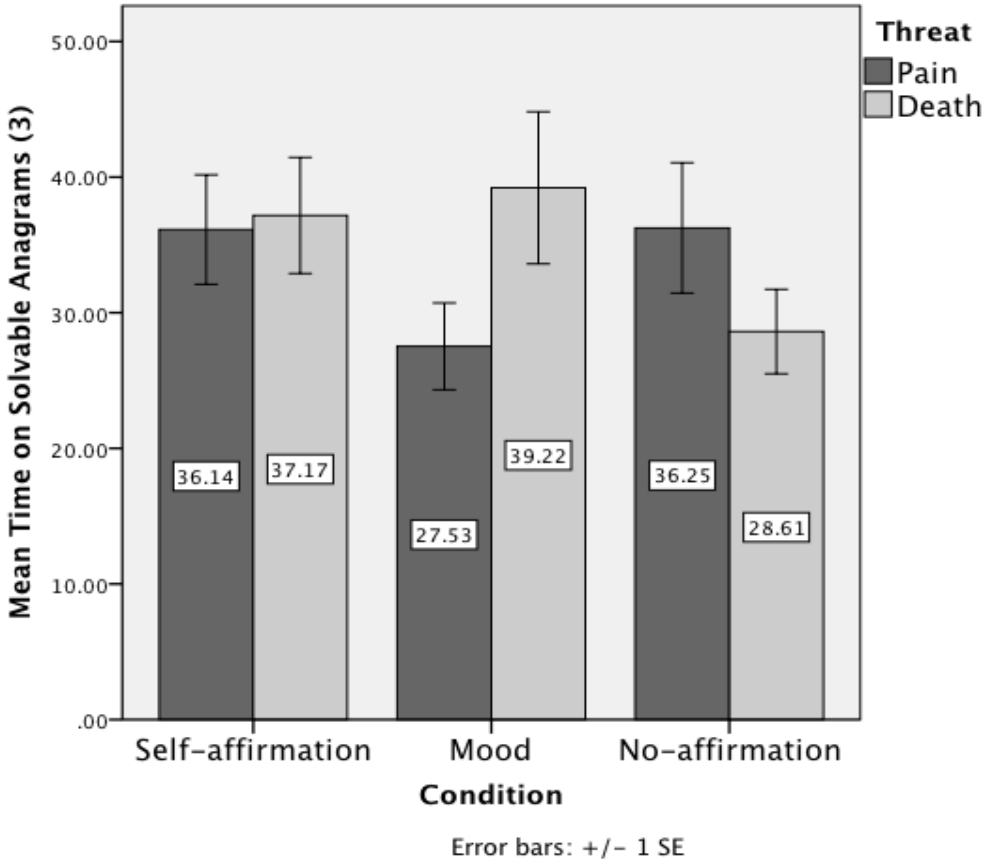


Table 4b

*Means and Standard Deviations on the Measure of Self-Control Strength (Solvable Anagram Persistence) as a Joint Function of Threat-Salience and Restoration*

| | | | Solvable anagram persistence | |
|---|---|---|---|---|
| Threat | Restoration manipulation | *n* | *M* | *SD* |
| Mortality salience | Self-affirmation | 45 | 37.17 | 28.69 |
| | Mood boost | 42 | 39.22 | 36.34 |
| | No-affirmation | 46 | 28.61 | 21.12 |
| Pain salience | Self-affirmation | 41 | 36.14 | 25.84 |
| | Mood boost | 42 | 27.53 | 20.74 |
| | No-affirmation | 41 | 36.26 | 30.78 |
| | Total | 257 | 34.09 | 27.80 |

*Note.* Mean values represent average number of seconds trying to solve three solvable anagrams. Covariates appearing in the model are evaluated at the following values: TIME = 92.52.

***Figure 4c: Self-Control Strength as a Joint Function of Threat Salience and Restoration***

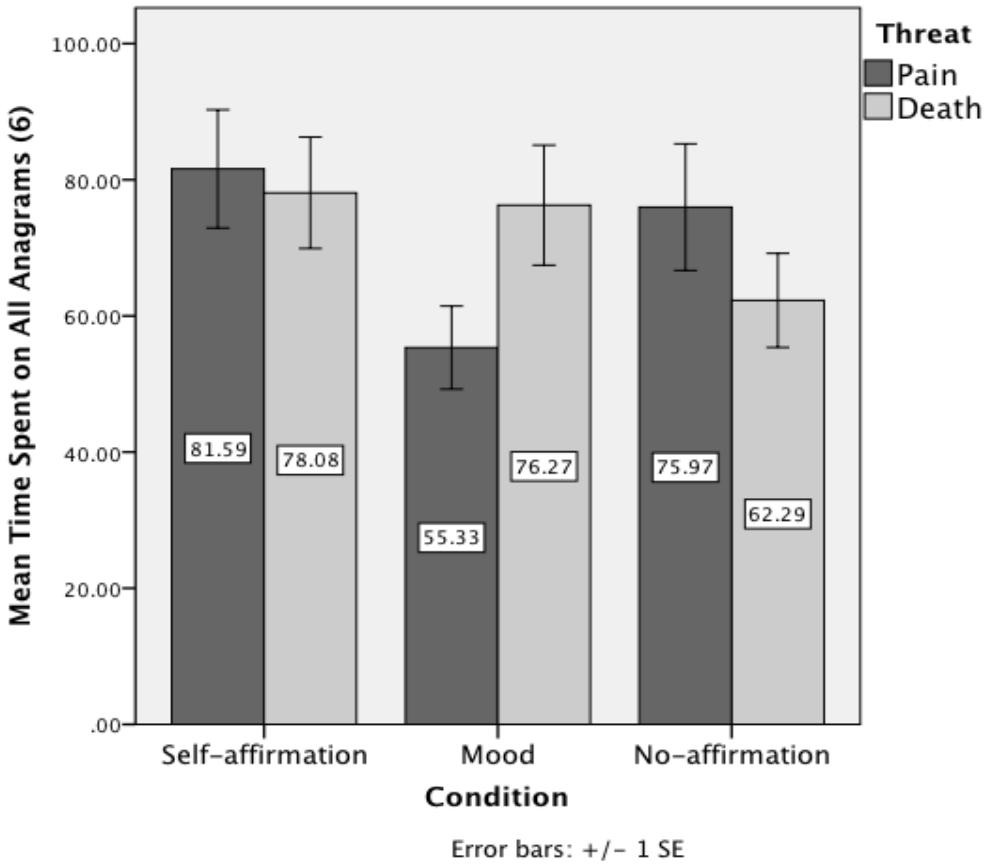


Table 4c

*Means and Standard Deviations on the Measure of Self-Control Strength (Overall Anagram Persistence) as a Joint Function of Threat-Salience and Restoration*

| | | | Overall anagram persistence | |
|---|---|---|---|---|
| Threat | Restoration manipulation | *n* | *M* | *SD* |
| Mortality salience | Self-affirmation | 45 | 78.08 | 54.95 |
| | Mood boost | 42 | 76.27 | 57.10 |
| | No-affirmation | 46 | 62.29 | 46.80 |
| Pain salience | Self-affirmation | 41 | 81.59 | 55.55 |
| | Mood boost | 42 | 55.33 | 39.55 |
| | No-affirmation | 41 | 75.97 | 59.55 |
| | Total | 257 | 34.09 | 27.80 |

*Note.* Mean values represent average number of seconds trying to solve all six anagram puzzles. Covariates appearing in the model are evaluated at the following values: TIME = 92.52.

***Figure 5: Self-Control Strength as a Joint Function of Threat Salience and Restoration***

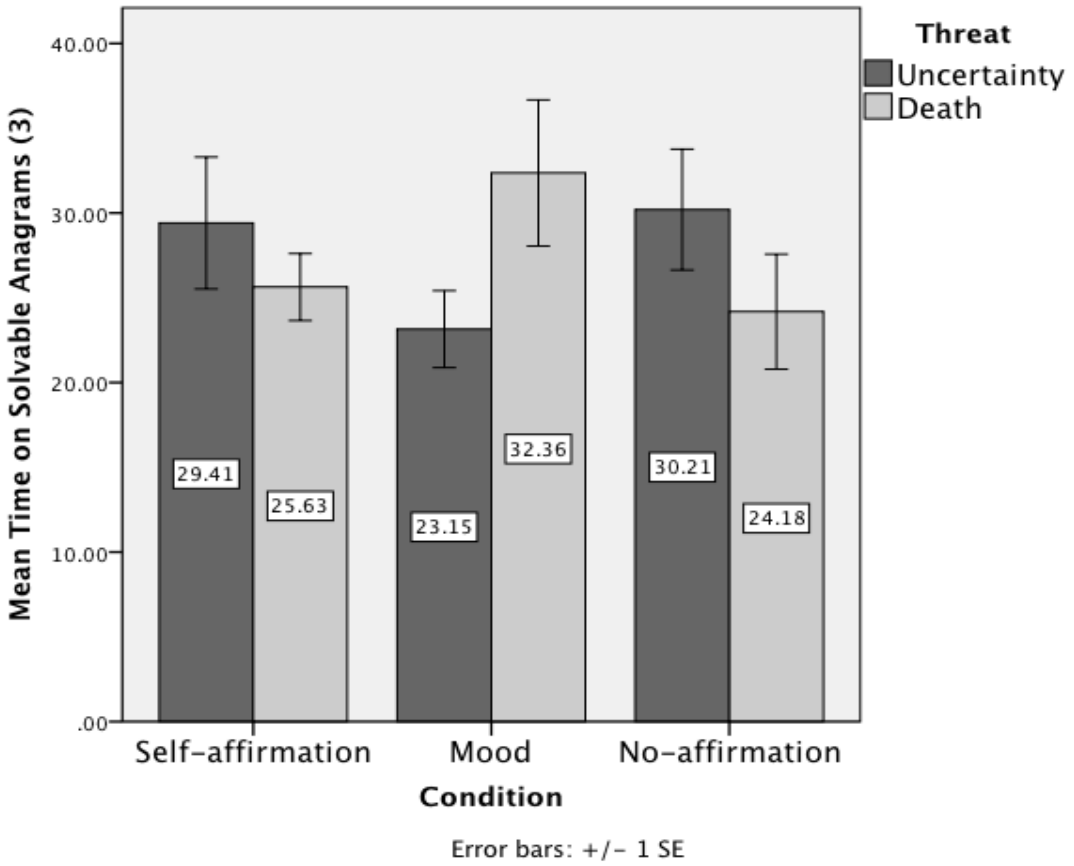


Table 5

*Means and Standard Deviations on the Measure of Self-Control Strength (Solvable Anagram Persistence) as a Joint Function of Threat-Salience and Restoration*

| | | | Solvable anagram persistence | |
|---|---|---|---|---|
| Threat | Restoration manipulation | *n* | *M* | *SD* |
| Mortality salience | Self-affirmation | 45 | 25.63 | 13.43 |
| | Mood boost | 42 | 32.36 | 25.88 |
| | No-affirmation | 46 | 24.18 | 20.05 |
| Uncertainty salience | Self-affirmation | 37 | 29.41 | 23.68 |
| | Mood boost | 42 | 23.15 | 13.44 |
| | No-affirmation | 41 | 30.21 | 23.08 |
| | Total | 231 | 27.52 | 20.42 |

*Note.* Mean values represent average number of seconds trying to solve three solvable anagrams. Covariates appearing in the model are evaluated at the following values: CORRECT = .47.

***Figure 6: Effect of Condition on State Self-Esteem***

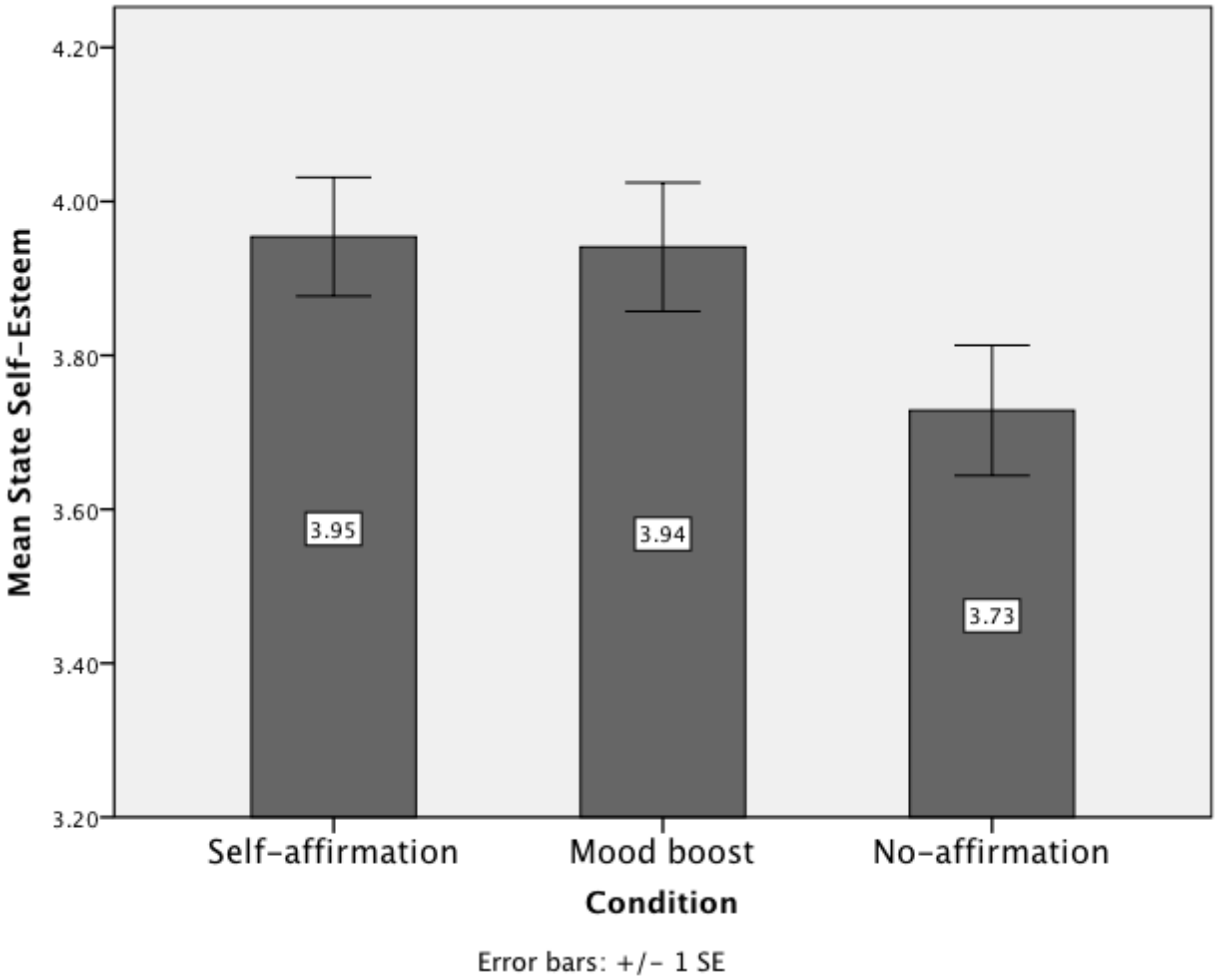


Table 6

*Means and Standard Deviations on the Measure of State Self-Esteem (Heatherton & Polivy, 1991) as a Function of Restoration Condition*

| | | State Self-esteem scores | |
|---|---|---|---|
| Restoration manipulation | *n* | *M* | *SD* |
| Self-affirmation | 73 | 3.95 | .66 |
| Mood boost | 92 | 3.94 | .80 |
| No-affirmation (*Version 2*) | 93 | 3.73 | .82 |
| Total | 258 | 3.87 | .77 |

*Note.* Participants rated their level of agreement with 20 statements by using a 5-point Likert scale (*1 = not at all; 5 = extremely*). Higher values indicate higher levels of self-esteem. Covariates appearing in the model are evaluated at the following values: WRITE_T = .29.

***Figure 7: Effect of Condition on Positive Affect***

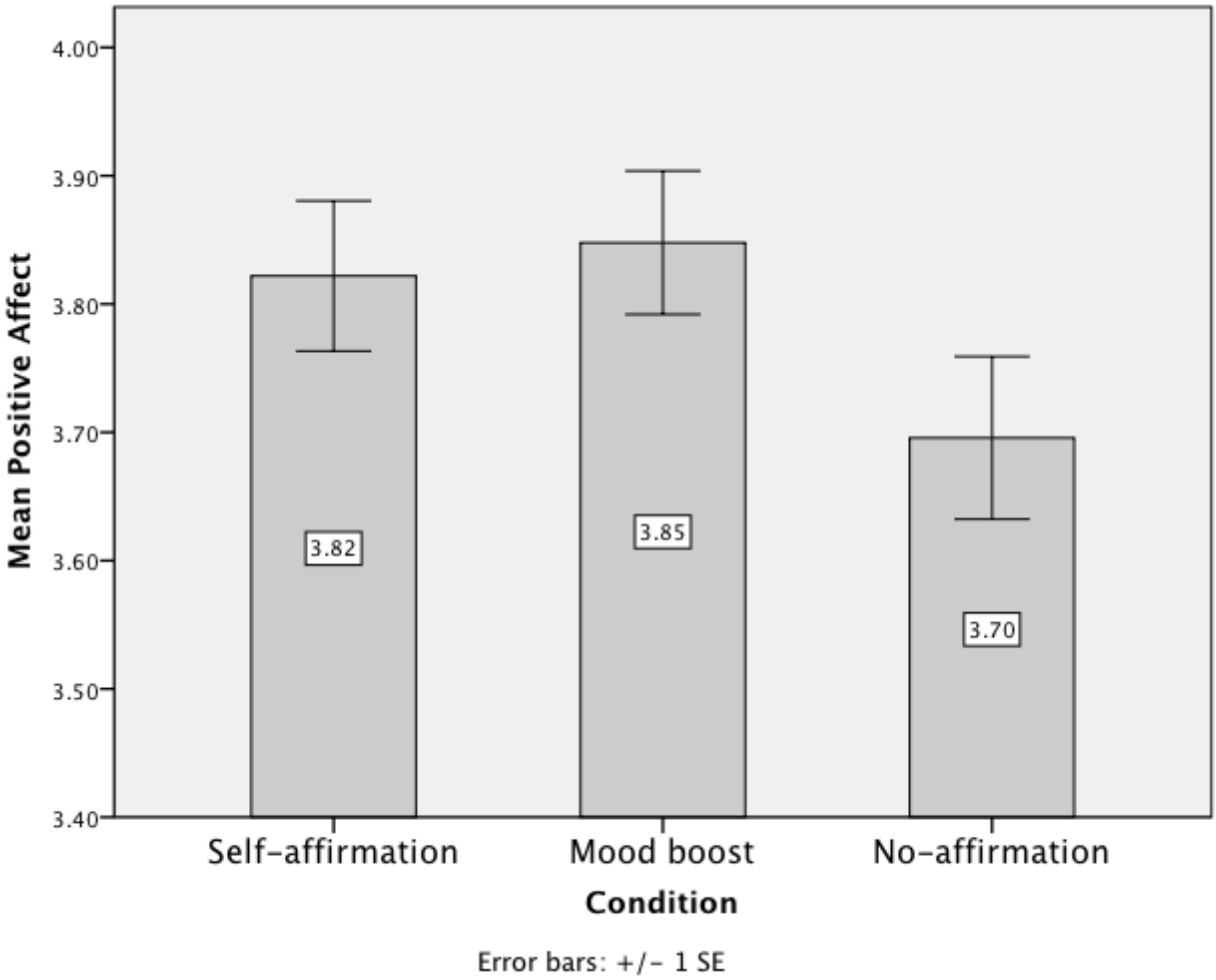


Table 7

*Means and Standard Deviations on the 20-item Composite Measure of Positive Affect (PANAS; Watson et al., 1988) as a Function of Restoration Condition*

| | | Positive affect scores | |
|---|---|---|---|
| Restoration manipulation | *n* | *M* | *SD* |
| Self-affirmation | 73 | 3.82 | .50 |
| Mood boost | 92 | 3.85 | .54 |
| No-affirmation (*Version 2*) | 93 | 3.70 | .61 |
| Total | 258 | 3.79 | .56 |

*Note.* Participants used a 5-point scale to rate how much they were currently feeling a particular emotion (*1 = not at all; 5 = extremely*). Covariates appearing in the model are evaluated at the following values: WRITE_T = .29.