Toward a Greater Understanding of the Emotional Dynamics of the Mortality Salience Manipulation: Revisiting the "Affect-Free" Claim of Terror Management Research

Alan J. Lambert, Fade R. Eadeh, and Stephanie A. Peak
Washington University

Laura D. Scherer
University of Missouri—Columbia

John Paul Schott and John M. Slochower
Washington University

The experimental manipulation of *mortality salience* (MS) represents one of the most widely used methodological procedures in social psychology, having been employed by terror management researchers in hundreds of studies over the last 20 years. One of the more provocative conclusions regarding this task is that it does not produce any reliable changes in self-reported affect, a view that we refer to as the *affect-free claim*. After reviewing 336 published studies that used the standard version of the MS task, we suggest that the evidence on which this claim is based may be less definitive than is commonly supposed. Moreover, we propose that the MS manipulation can, in fact, produce significant and meaningful changes in affect once one employs the appropriate measures and experimental design. In support of this position, we report 4 experiments, each of which demonstrates reliable activation of negative affect, especially with respect to fear-/terror-related sentiments. We discuss the implications of our findings for terror management theory as well as for research and theory on the measurement of mood and emotion.

Keywords: terror management, mortality salience, emotion, mood, fear

The experimental manipulation of *mortality salience* (MS) represents one of the most widely employed procedures in the history of experimental social psychology, having been employed by terror management (TM) researchers in hundreds of articles over the last 20 years (for overviews, see Burke, Martens, & Faucher, 2010; Hayes, Schimel, Arndt, & Faucher, 2010). The most often-used version of the MS task consists of two questions, in which participants are asked to (a) "briefly describe the emotions that the thought of your own death arouses in you" and to (b) "write down, as specifically as you can, what you think happens to you when you physically die and once you are physically dead."

Given its explicit focus on death, one might imagine that the MS task would instill a significant degree of negative affect. However, TM researchers have long claimed that this task does not produce any reliable change in emotional experience. For example, after reviewing the available evidence, one team of TM researchers concluded that "participants consistently have not reported elevated affect in response to mortality salience inductions" (Simon et al., 1997, p. 1133). A similar conclusion was reached by Arndt, Allen, and Greenberg (2001), who stated that "the extant literature . . . has yet to find consistent increases in negative affect following mortality salience" (p. 255). In this article, we refer to this as the *affect-free claim,* with the understanding that this refers to the idea that the MS manipulation does not reliably produce any increase in self-reported negative affect.[1]

In this article, we suggest that (a) there is far less evidence supporting this claim than is commonly supposed and (b) the claim is not actually true. In support of this position, we report four experiments showing that the MS manipulation reliably elicits self-reported negative affect. The increase in negative affect following the MS task (vs. a control group) accounted for upward of 9%–11% of the observed variance, depending on the type of analysis employed. This represents a medium-to-large effect (J.

Alan J. Lambert, Fade R. Eadeh, and Stephanie A. Peak, Department of Psychology, Washington University; Laura D. Scherer, Department of Psychology, University of Missouri—Columbia; John Paul Schott and and John M. Slochower, Department of Psychology, Washington University.

Correspondence concerning this article should be addressed to Alan J. Lambert, Department of Psychology, Washington University, 1 Brookings Drive, St. Louis, MO 63130. E-mail: alambert@wustl.edu

[1] Our use of the *affect-free claim* refers to changes (or lack thereof) in self-reported affective experience as assessed by standard mood inventories, which is the way that the vast majority of TM studies have measured affect. Also, although the affect-free claim technically refers to *any* changes in self-reported affect, we were most concerned with the conclusion that the MS task does not produce an increase in negative feelings.

Journal of Personality and Social Psychology, 2014, Vol. 106, No. 5, 655–678
 0022-3514/14/$12.00 DOI: 10.1037/a0036353

Cohen, 1992) comparable to the effects obtained in many well-known paradigms in our field, including the bystander intervention and dissonance literatures (Funder & Ozer, 1983). It is also comparable in magnitude to shifts in cultural worldview obtained in the TM literature (Burke et al., 2010).

Our version of the MS task is identical to that employed by previous TM researchers, and like the vast majority of studies in this area, we measured affect using a standard mood inventory. Why were we able to obtain such relatively robust effects, in apparent contrast to what previous TM researchers have found? One key to this puzzle is that the affective consequences of the MS task are extremely narrow, far more so than has typically been assumed. In particular, we argue that the reliable consequences of the MS task for affect can only be seen by focusing specifically on fear.

A Clarification of Our Goals Regarding Terror Management Theory as a Whole

TM theory is clearly an innovative and important framework, one that has spawned a vigorous and far-ranging body of work that has generated many important insights. Moreover, our challenge to the affect-free claim should not be taken to imply that we believe that TM theory is invalid. Indeed, we agree with at least four foundational principles of TM theory, that (a) the prospect of our own mortality is inherently frightening, (b) people are motivated to protect themselves from this threat, (c) this motivation can produce important changes in behavior/attitude, and (d) *certain aspects* of this motivational process may occur independently of affect. However, there is one important point on which we do disagree, and this bears on the claim—repeatedly made by TM theorists for over 20 years—that the MS manipulation produces *no* reliable changes in self-reported affect. We believe that this issue is critical to understanding the nature of the threat posed by MS in the first place, and this was the central goal of our research.

Why the Validity of the Affect-Free Claim Is Important on Theoretical Grounds

Before we discuss why the MS task is especially relevant to fear, it is useful to consider why the validity of the affect-free claim might matter in the first place. This is important because it would be easy to conclude that this article is merely concerned with methodological issues. This is not the case. On the contrary, our research has several important implications for TM theory, as noted below.

On the Uniqueness of Mortality Salience Vis-à-Vis Other Types of Threat

Most people face a myriad of potential threats in the course of their lifetime, ranging from those that are comparatively mild (e.g., apprehension over a possible speeding ticket) to more serious threats (e.g., fear of flying). Merely *thinking* about such threats—especially the more serious ones—is almost certainly likely to trigger negative feelings. According to TM researchers, however, inducing people to think about their own death in the context of the MS task produces no reliable changes in self-reported emotion whatsoever. If this claim were indeed true, this would represent an astoundingly counterintuitive finding that surely sets MS apart from virtually all other threats. Conversely, if one could show that this affect-free claim is false, this would undermine one type of evidence for a larger proposition within TM theory as to the ostensible uniqueness of MS as a psychological threat.

We are not the first to raise doubts about this issue. In a recent review of the TM literature, Tritt, Inzlicht, and Harmon-Jones (2012) argued that "the notion of separate and distinct biological mechanisms of MS effects on the one hand and all other threat defenses on the other runs counter to prevailing views in neuroscience" (p. 721) and, for this reason, "it makes little sense for a specialized 'death anxiety module' [to exist] in the brain" (p. 722). Similar skepticism about the ostensible uniqueness of the threat posed by MS has also been expressed by several other researchers (e.g., Kirkpatrick & Navarrete, 2006; Leary & Schreindorfer, 1997; Pelham, 1997; but see also Pyszczynski, Greenberg, Solomon, & Maxfield, 2006).

To be sure, there may be *some* elements of MS that may be different from other types of threat (Hayes et al., 2010). Nevertheless, we were concerned with the position that people are really so capable, and successful, at suppressing negative affect that is triggered by the MS task. As Tritt et al. (2012) noted, the failure of studies to find evidence of observable changes in self-reported affect could be due, in part, to the possibility that the self-report instruments most frequently employed by TM researchers "may not be sensitive enough" (p. 726) to detect consciously experienced changes in emotion following the MS task. Although they did not provide any new data addressing this issue, we believe that the proposal made by Tritt et al. is correct, namely, that the null findings obtained in previous research are due, at least in part, to measurement insensitivity.

Theoretical Connections Between the Affect-Free Claim and the Suppression Hypothesis

Strictly speaking, the affect-free claim is not a theoretical assumption of TM theory per se. Rather, it is an *interpretation* of a null finding that began to emerge as researchers began to study the consequences of the MS task. Indeed, as is apparent in the quote to follow, TM researchers were themselves somewhat surprised by these null effects. Nevertheless, after having concluded that the MS task is not producing any changes in self-reported affect, TM researchers used this conclusion as a basis for proposing a major tenet of their theory, involving suppression:

> Somewhat puzzled by the consistent lack of affective impact in response to mortality salience, terror management researchers began to explore the microlevel cognitive effects of consciously inducing thoughts of death. They reasoned that if thoughts of death are really so troubling, participants who have finished writing about the topic of death must attempt to banish death-related thoughts from conscious awareness to prevent death anxiety from taking hold. (Hayes et al., 2010, p. 701)

As suggested by this passage, the necessity of the suppression hypothesis only became apparent *after* TM researchers had concluded that the MS task was not producing changes in self-reported affect. However, suppose that TM researchers had, from the very beginning, known that the MS task was capable of producing reliable changes in negative affect. Based on the quote by Hayes et

al. (2010), one might wonder whether the suppression postulate would have been proposed in the first place. Once again, we are not the first to raise questions about this aspect of TM theory. For example, Kirkpatrick and Navarrete (2006) expressed doubt as to the adaptive function of pushing the unpleasant ramifications of death out of conscious awareness: "Assuming that anxiety and fear systems are themselves adaptive, then selection should strongly disfavor additional systems that inhibit [such] responses" (p. 291; for related critiques, see Leary, 2004; Leary & Schreindorfer, 1997; Pelham, 1997; see also Pyszczynski, Greenberg, Solomon, Arndt, & Schimel, 2004a, for a rebuttal). We shall return to these issues later in this article.

Reconciliation With Social Psychological Research on Mood and Emotion

There is a long history of research showing that mood/emotion can have important ramifications for attitude, thought, and behavior (Forgas, 2001; Frijda, 1986; Schwarz, 2012; Smith & Mackie, 2005). To date, there has been surprisingly little cross-talk between emotion researchers and TM theory investigators. Part of the reason for this lack of connectivity, of course, is that TM research seems to show that the MS task does not produce any changes in affect. Hence, by showing that the MS task does, in fact, produce reliable changes in mood/emotion, such findings could represent an important first step in reconciling the apparent—but, in our view, illusory—differences across these areas of research.

Why Fear Is Likely to Be Especially Relevant to the MS Task

One of the major points of this article is that the affective consequences of the MS task are specifically relevant to fear as opposed to other types of negative affect, including anxiety. Fear and anxiety are obviously related, and the distinction between these states is best regarded as fuzzy rather than sharp. Nevertheless, although the distinction between these states is far from simple (cf. McNaughton, 2011), there is a large body of neuroscience research that supports the value of making the distinction between fear-related versus anxiety-related reactions (R. J. Blanchard, Blanchard, Griebel, & Nutt, 2008; Chantarujikapong et al., 2001; Davis, 1998; Hettema, Prescott, Myers, Neale, & Kendler, 2005; Ohman, 2008; Zeidner & Matthews, 2011).

In particular, many of these theorists concur that fear, more so than anxiety, is triggered by serious, often life-threatening circumstances that are perceived as (a) *inevitable* and (b) *readily identifiable.* In contrast, anxiety tends to be associated with uncertainty, and the identification of the exact threat is more difficult: "Although definitions of anxiety frequently refer to fear, they contain the additional nuance of involving apprehension with regard to uncertain events" (D. C. Blanchard & Blanchard, 2008, p. 63). A similar view was offered by Ohman (2008, p. 722), who suggested that

> even though fear and anxiety are overlapping responses, they can be distinguished in terms of stimuli (presence vs. absence of a discrete eliciting stimulus), behavior (coping vs. noncoping), and neuroanatomy (central nucleus of the amygdala vs. bed nucleus of the stria terminalis).

Clinical psychologists have also posited fear versus anxiety as related but distinct factors in many disorders, including phobias, posttraumatic stress disorder, general anxiety disorders, and panic attacks (Chantarujikapong et al., 2001; Hettema et al., 2005).

Although TM researchers have yet to make a formal distinction between these two types of emotions, their framing of MS suggests a greater relevance to fear. For one thing, TM researchers often emphasize the inevitability of death as a critical component of MS: "From the perspective of TM, reminders of death are especially potent threats because *death is the only inevitable future event* [emphasis added]" (Greenberg, Kosloff, Solomon, Cohen, & Landau, 2010, p. 3). Similarly, Greenberg, Pyszczynski, Solomon, Simon, and Breus (1994) noted, "death is a problem that does not simply go away. Rather, it is an inevitable fate" (p. 636). Moreover, when TM researchers discuss the underlying reasons why MS poses such a foundational threat, they suggest that it is the absolute certainty of our own demise that is potentially terrifying, as opposed to the specific circumstances under which we might someday die (Greenberg et al., 1994). This latter point is important because TM researchers have argued that the specific threat of death is different from the dynamics surrounding uncertainty per se (Greenberg et al., 2010).

In summary, these considerations provide a basis for anticipating that the threat of MS—the potentially terrifying knowledge that we shall all die—might have greater connection to fear compared to anxiety. If this line of reasoning has merit, this suggests that the most relevant way of assessing the affective consequences of the MS task would be afforded by focusing on fear.

A Methodological Survey of 336 Mortality Salience Studies Conducted Between 1989 and 2013

Given our focus on the affect-free claim, it is important to consider what sorts of methodological/analytic criteria might be needed to test its validity. First, the study would need to measure affect immediately after the experimental manipulation. Second, the design needs to contain a neutral control group, in order to provide an appropriate baseline to which the MS manipulation could be compared. Third, the study would need to assess and report upon any changes in fear.

In order to address these considerations, we initially relied on a meta-analysis reported by Burke et al. (2010). The original purpose of the Burke et al. analysis was to ascertain the effect size of the MS manipulation on various belief systems. In our case, however, their analyses provided a useful starting point in order to assess the extent to which previous studies have (or have not) tested the merits of the affect-free claim. We supplemented the Burke et al. article by employing a search of all peer-reviewed articles available through our local library that were published between 2010 and 2013 that included *mortality salience* in the abstract. In combination, this generated a total of 336 individual studies.

In Figure 1, we present a branching tree diagram. The diagram begins with the consideration of whether the study in question met the first criterion: assessment of affect. Of the studies that did so, we then consider whether the study contained a neutral control group. Finally, among the studies that met the first two criteria, we then consider whether the researchers actually measured, and conducted separate analyses on, fear in particular. (A small number of investigations [e.g., Schimel et al., 1999; Yen & Cheng,

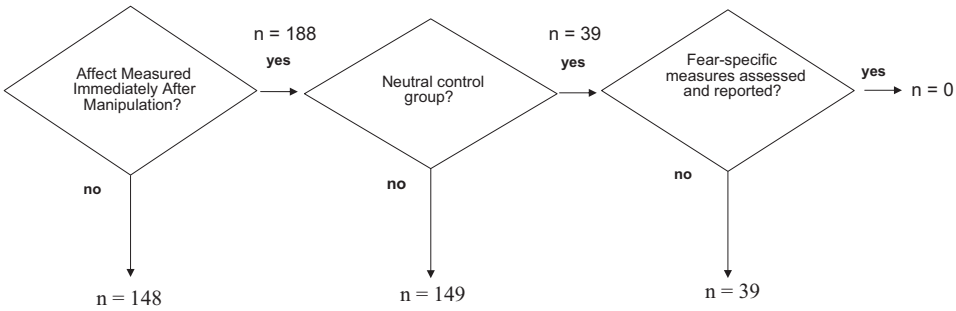

Figure 1. Branching tree diagram showing how many studies met three criteria: assessment of affect, inclusion of neutral control group, and separate analysis of fear. Total number of individual studies: 336.

2010] measured mood but did not report these findings in their article. These studies are excluded from consideration here.)

Criterion 1: Measurement of Affective Experience Immediately After the MS Manipulation

Of the 336 studies considered, a substantial number ($n = 148$) did not include any measure of affect immediately after the manipulation. This includes investigations in which researchers (a) did not measure affect at all or (b) only measured affect after participants had completed one or more blocks of judgments. In the latter case, the effects of the MS manipulation on affect are likely to be less detectable either because of the passage of time and/or the interfering nature of the interpolated tasks. This left a total of 188 studies that met the first criterion.

Criterion 2: Inclusion of a Neutral Control Group

Of the remaining 188 studies, a relatively small number of these ($n = 39$) included a neutral control group (cf. Figure 1). The most common control task in this group was one in which participants were asked to reflect on what it felt like to watch television (or other mundane tasks, such as shelving books), using two open-ended questions paralleling the structure of the MS task (i.e., "briefly describe the emotions that the thought of watching television arouses in you"). In other cases, the neutral control condition was established by omitting the MS task from the questionnaire packet.

We return to these remaining 39 studies presently. For the time being, however, it is worth noting that this means that a large number of studies ($n = 149$) did not include a neutral control group. Instead, they used a design in which the MS manipulation was compared to an aversive control condition deliberately designed to induce negative affect. The most common version of this type of task is one that asks participants to consider what it feels like to experience dental pain (cf. Burke et al., 2010). However, aversive controls also include tasks asking participants to imagine what it feels like to experience (a) loss of a limb (Cox et al., 2008), (b) intense pain (e.g., Greenberg et al., 2010), (c) a stressful exam (e.g., Landau, Greenberg, & Rothschild, 2009), (d) giving a speech in public (e.g., Vess, Arndt, Cox, Routledge, & Goldenberg, 2009), and (e) social exclusion (Landau, Greenberg, Solomon, Pyszczynski, & Martens, 2006a).

TM researchers have employed these aversive control tasks in order to rule out the possibility that any observed consequences of the MS task on behavior/attitudes are not simply a function of getting participants to complete a psychologically unpleasant task. For example, many studies have shown that the MS task—but not aversive control tasks—increases the tendency for participants to psychologically align themselves with meaningful ingroups, along with other types of effects that seem to indicate strengthened motives for a sense of psychological security (cf. Hayes et al., 2010). However, studies employing aversive controls do not contain a neutral baseline condition and hence are not relevant to a rigorous test of the affect-free claim.

Criterion 3: Did Researchers Measure and Conduct Separate Analyses on Fear-Related Experience?

Of the remaining 39 studies that met the first two criteria, the majority of these ($n = 31$; 79%) found no mood effects at all. Of the small number of studies that did find some effect, the results were somewhat mixed, with some finding more negativity following the MS task and others finding more positive mood (see ahead for details). However, the dominant finding among this group of studies was a null effect. This leads us to a question that lies at the heart of the third criterion: Out of the studies that met the first two criteria, how many of these measured/report analyses specific to fear? In fact, *none* of these studies did so. Because this aspect of the literature is likely to be surprising to many readers, it is useful to make a closer examination of these studies, which we list in the Appendix, organized by the type of measurement tool used to assess affect, the experimental design employed, the analytic approach taken, and an indication of whether null effects were found or not.

Multiple Affective Adjective Checklist (Zuckerman & Lubin, 1965): $n = 6$. Several of the early studies in the TM literature, including the first six listed in the Appendix, relied on the Multiple Affective Adjective Checklist (MAACL; Zuckerman & Lubin, 1965). The MAACL has been identified as having many problems of validity (e.g., Gotlib & Meyer, 1986), and these critiques have called into question the ability of this instrument to capture mood among normal (i.e., nonclinical) populations (see also Herron, 1969; Watson & Clark, 1997). Most important for us, the MAACL was designed to measure anxiety, not fear. This fact, coupled with the aforementioned psychometric problems of the

MAACL, explains why almost all of these studies produced a null effect.

Original version of the Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988): *n* = 20. Over half of these studies relied exclusively on the original version of the PANAS, which relies only on two broadband measures of affect (i.e., positive affect and negative affect). Notably, the negative affect index lumps together 10 discrete items into a single index (i.e., averages across *afraid, scared, nervous, jittery, irritable, hostile, guilty, ashamed, upset,* and *distressed*). Hence, even though nearly all of these studies found a null effect, the lack of an effect in this context is not particularly diagnostic, given that the negative affect index lumps together several different types of emotional experience (e.g., anger, guilt, shame) that have little if any direct relevance to the MS task.

Positive and Negative Affect Schedule–Expanded Form (PANAS-X; Watson & Clark, 1994): *n* = 12. A smaller number of studies employed a modification of the PANAS, which is often referred to as the PANAS-X. Unlike the original version of the PANAS, the PANAS-X allows tracking of mood with respect to 11 separate subscales. As one can see in the Appendix, the majority of these studies, too, found a null effect. However, there are several properties of the PANAS-X that almost certainly contribute to the preponderance of null effects obtained here.

To begin with, several of the studies listed here relied on an omnibus multivariate analysis of variance (MANOVA), in which all 11 subindices were submitted simultaneously in a single analysis. Although multivariate analyses are often touted for their power relative to univariate analyses, MANOVAs can often suffer from relatively *low* power, especially when (a) there is a large ratio of dependent variables to subjects and (b) the majority of the dependent variables are irrelevant to the manipulation (Cole, Maxwell, Arvey, & Salas, 1994; Cook & Campbell, 1979; see also Huberty & Morris, 1989). This state of affairs is likely to produce an underpowered test of the affective consequences of the MS task. This point was acknowledged by Harmon-Jones et al. (1997), insofar as they noted caution to be used when using the omnibus MANOVA test "because of the small ratio of cases to dependent variables, which might reduce the power of the MANOVA and produce a nonsignificant *F* [statistic]" (Harmon-Jones et al., 1997, p. 28).

As seen in the Appendix, several studies that used the PANAS-X conducted univariate analyses of variance (ANOVAs) on each of the 11 subindices, including the so-called "fear" subindex. However, this index has at least two problematic properties. First, it actually contains a heterogeneous mixture of different items, some of which would seem to be more relevant to anxiety than to fear (*afraid, frightened, scared, nervous, jittery, shaky*). This introduces some ambiguity as to what the so-called "fear" index is actually measuring. Importantly, this is not just our opinion. For example, TM researchers routinely refer to this as the *fear index,* which is consistent with terminology used by Watson and Clark (1994). However, other theorists have argued that this subindex is actually a measure of *anxiety,* not fear (cf. Phillips & Giancola, 2008). In fact, this particular subindex is a measure of both fear *and* anxiety. Furthermore, two of the items—*jittery* and *shaky*—have psychometric problems in their own right, as they do not appear to provide a reliable index of either anxiety *or* fear (Ebesutani et al., 2011).

Hence, (a) if it is true that the MS task is actually more relevant to fear than to anxiety and (b) given that the so-called "fear" subindex actually measures a combination of items pertaining to both states, then it follows that (c) this index is likely to produce a less consistent/weaker pattern of results than if researchers had actually focused on fear in particular. To clarify, we do not argue that this index is *completely* irrelevant to the MS task. Indeed, compared to all of the other subindices of the PANAS-X, it is most applicable to the likely dynamics of the MS task. Moreover, of the studies that met the first two criteria, two of these (Harmon-Jones et al., 1997, Experiments 1 and 3) did find evidence of greater negativity using this index. Our point is merely that the hybrid nature of this subindex is likely to introduce more inconsistency into the observed pattern of results than would otherwise be the case.

Global measure of uncertainty: *n* = 1. One of the studies listed in the Appendix, that by McGregor, Zanna, Holmes, and Spencer (2001, Experiment 3), operationalized its primary measure of mood in terms of a global index of uncertainty and found higher levels in the MS condition. However, we could locate only one study that was able to show a significant effect with this type of measure. Hence, its replicability and robustness across different studies are not yet clear.

A Logical Inconsistency Within the TM Literature?

Above and beyond the considerations raised above, there is another reason why the validity of the affect-free claim is important on theoretical grounds. This is because the affect-free claim would appear to be logically inconsistent with *other* claims made by TM researchers. To begin with, consider the typical findings obtained with studies that compare the MS task to aversive controls. Such studies almost always reveal null effects, with only rare exceptions (cf. Greenberg, Simon, et al., 1995). Yet, as we have noted, null effects are *also* commonly found when a neutral control is employed. Hence, when TM researchers have investigated the affective consequences of the MS task, *they have typically obtained null effects, regardless of whether a neutral or aversive control task is used.*

Something is clearly wrong here. If the affect-free claim is true, then one should reliably obtain significant differences when one compares the MS task to aversive controls, with the latter condition eliciting higher levels of negative affect compared to the former. But that is not what happens, as this comparison almost always yields a null effect. To our knowledge, we are the first to make note of this inconsistency. Resolution of this inconsistency is important, given that one marker of a strong theory is the extent to which the various assumptions/conclusions/tenets of the model are logically, conceptually, and empirically consistent with each other (Abelson, 1995).

Fortunately, there is a straightforward way to reconcile this apparent conundrum. One of the well-known problems with insensitive measures is that they tend to generate a plethora of null effects, regardless of whether actual differences are present or not (Shadish, Cook, & Campbell, 2002). Assume, for the moment, that the kinds of self-report measures used by previous researchers are inadequately capturing the affective consequences triggered by the MS task. If so, this could explain why, even when the MS task is

contrasted against truly neutral control conditions, most studies obtain a null effect.

As for the studies comparing the MS task to *aversive controls,* it may well be true that both tasks are eliciting negative affect. However, studies comparing the MS task against aversive controls are likely to suffer from the same sort of insensitivity problem proposed above, given that similar measures (typically, involving the original or modified version of the PANAS) tend to be used across all of these studies. Hence, the exact ways that the MS task differs from aversive controls in terms of posttask affect remains something of an open question, pending the use of more sensitive measures. Although a comparison of the emotional dynamics of the MS task versus aversive controls was not of central concern in this article, the last study in our series (Experiment 4) addresses this point.

Summary and Implications

We began our methodological review with a total of 336 studies that employed the standard version of the MS task. Among these, we were unable to find even one study that met all three criteria needed to rigorously test the validity of the affect-free claim with regard to fear (cf. Figure 1). Again, these considerations do not merely constitute a methodological critique. As we have noted, the affect-free claim represents an important aspect of TM theory, providing one basis for claiming that MS is distinct from all other types of threat (Tritt et al., 2012). Additionally, it served as a basis for proposing the suppression hypothesis (cf. earlier quote by Hayes et al., 2010). Moreover, if the affect-free claim were shown to be false, this highlights the possibility that the consequences of the MS task may involve at least some of the mood-infused processes as identified by previous research in the social psychological literature (e.g., Schwarz, 1990, 2012), providing a much-needed theoretical integration of TM theory with other important areas of social psychological research.

On Our Operationalization of Mood Composites

A popular approach in social psychology—including but not limited to the TM literature—is to measure mood using the PANAS. As we have already noted, this often involves the formation of two broadband indices of affect, one encompassing positive affect and the other encompassing negative affect (Watson et al., 1988). As supporters of this approach point out (Watson, 1988), factor analyses of mood ratings often reveal a two-factor solution, with one factor revealing high loadings of positive mood states and the other revealing high loadings of negative mood states (but see also Russell & Carroll, 1999). Indeed, we have often found this pattern in our own research. In particular, when we submit mood ratings to principal component analyses, we typically find two main factors, one pertaining to positive affect and one pertaining to negative affect.

Although factor/principal component analyses could justify a broadband approach to affect, there can be important theoretical reasons for making more fine-grained distinctions between mood states, even when they are highly correlated and even when they happen to load on the same underlying factor (see also Harmon-Jones, Harmon-Jones, Abramson, & Peterson, 2009; Pettersson & Turkheimer, 2013). For example, in our own studies, principal component analyses often fail to reveal a clear distinction between anger and sadness, but there is ample evidence from both the social psychological literature (Schwarz, 2012) and neuroscience (Carver & Harmon-Jones, 2009) that these represent meaningfully distinct types of affect. Similarly, even though we have never found clear-cut evidence for separate fear and anxiety factors, there is, as noted above, a long history of research suggesting that the distinction between these states, too, can be very important. For this reason, the mood composites we formed in this research and the specific items used to construct these indices were based largely on a priori considerations, taking into account the conceptual issues at stake in the present research.

Experiment 1

The primary goal of Experiment 1 was to test the validity of the affect-free claim as it applies to the MS task. Our primary approach was to assess mood immediately after the experimental manipulation using a standard mood inventory. Nevertheless, by explicitly presenting participants with a list of mood terms, this runs the risk of priming participants to think about a particular mood state (e.g., fear), causing that particular type of emotion to become more salient than it otherwise might be. For this reason, it was important for us to show converging evidence for our conclusions, using a different approach that did *not* involve explicit measurement of mood.

We satisfied this desideratum by conducting analyses on participants' written protocols in response to the task they were assigned to complete (MS vs. control). This analysis used the Linguistic Inquiry and Word Count (LIWC) program, as developed by Pennebaker, Booth, and Francis (2007). This approach thus allowed us to analyze the *spontaneous* emergence of linguistic content as participants completed their task, without any intervention on our part. Because the considerations surrounding these analyses are somewhat complex, we report data pertaining to this aspect of our analysis in a separate section, after the results of our primary mood data have been reported.

Measurement of Fear Versus Anxiety

Even though the distinction between fear and anxiety is well grounded in the neuroscience and clinical areas (see earlier discussion), we are not aware of any widely used indices in the social psychological literature on mood that measure fear apart from anxiety, and vice versa. Nevertheless, we attempted to maximize consistency with the TM literature, to the extent that this was possible given our research aims. In particular, we selected three items from the fear subindex of the PANAS-X that seemed to capture the everyday meaning of fear (i.e., *afraid, frightened, scared*) along with the clearly related item *fearful*. All four of these items are common words whose relation to the construct of fear seems self-evident. However, these intuitions were confirmed by (a) consultation of the Oxford English Dictionary, (b) the most recent version of Roget's Thesaurus (Kipfer, 2011), and (c) an online generator of latent semantic associations (cf. Landauer, Foltz, & Laham, 1998). All three of these approaches confirmed our assumption that these terms are closely related to one another, each having a common link to the underlying construct of fear.

As for our measure of anxiety, things were complicated somewhat by the fact that, as noted above, two of the items from the

PANAS-X—*jittery* and *shaky*—have questionable psychometric properties (cf. Ebesutani et al., 2011). Hence, we used the items *nervous, anxious,* and *worried* for our composite measure of anxiety. Here, again, our choice in this matter was verified by three independent approaches (latent semantic associations, the Oxford English Dictionary, Roget's Thesaurus). It is also worth noting that these terms often appear in several well-known measures of state anxiety (e.g., Spielberger, Gorssuch, Lushene, Vagg, & Jacobs, 1983; Taylor, 1953).

In summary, our main focus was thus on two composite measures, one designed to capture fear (based on an average of *fearful, afraid, scared,* and *frightened*; $\alpha = .92$), and one designed to capture anxiety (based on an average of *anxious, nervous,* and *worried*; $\alpha = .86$). Again, we do not make the strong claim that these are completely different mood states, as fear and anxiety are clearly overlapping. Nevertheless, we believe that this distinction is important in a functional sense, as it would provide strong leverage in showing the stronger relevance of fear (vs. anxiety) to the MS task. We predicted that (a) the affective consequences of the MS task would be much larger for fear compared to anxiety, (b) any effects found with anxiety, if they existed at all, would disappear after controlling for fear, and (c) the effects of the MS task on fear would remain significant after controlling for anxiety.

For the sake of completeness, we formed several additional mood composites corresponding to what most emotion theorists would regard as reasonably basic emotional states, including composite measures of (a) sadness, based on an average of *sad, dejected,* and *distressed* ($\alpha = .79$); (b) happiness, averaging across *happy* and *satisfied* ($\alpha = .88$); and (c) anger, averaging across *angry, mad, irate,* and *irritable* ($\alpha = .69$). In summary, we formed a total of five composites (fear, anxiety, sadness, anger, and happiness). The relationships among these are shown in Table 1.[2]

Method

Participants and design. A total of 168 participants (73 male and 95 female) were recruited from Amazon.com's Mechanical Turk online survey program, which allows researchers to post questionnaires to be completed by users in return for small gift vouchers. Participants were randomly assigned to one of two conditions (MS vs. control). Participant gender was retained as a factor in these analyses, but none of the effects involving the MS manipulation was contingent on gender, and hence, the findings to be reported below collapse over this factor.[3]

Experimental manipulation. The wording of the MS manipulation was identical to the task used by previous TM researchers and asked participants to "describe the emotions that the thought of your own death arouses in you" and to "write down, as specifically as you can, what you think happens to you when you physically die and once you are physically dead." For each of these questions, participants were provided with a text box in which to record their answers. Participants assigned to the control condition were asked to write down the mundane activities that they typically perform during the average day. We conducted formal analysis of the content of the written protocols generated by participants in these two conditions; these data are reported in a separate section entitled Linguistic Analyses of Open-Ended Protocols.

Assessment of affect. Immediately after completing their assigned task, participants rated their mood with respect to a large number of mood items, presented in a randomized order for each participant: *afraid, scared, nervous, fearful, anxious, frightened, jittery, shaky, irritable, hostile, guilty, ashamed, upset, distressed, active, alert, attentive, determined, enthusiastic, excited, inspired, interested, proud, strong, confident, bold, daring, happy, satisfied, mad, angry, irate, sad, worried, dejected, serene, calm, tranquil, comforted, soothed, relaxed, at ease, uncertain, unsure,* and *insecure*; each of these was rated along a scale ranging from 1 (*not at all*) to 5 (*very much so*). As noted above, our operationalization of mood composites was based on a priori theoretical considerations, and our main concern was on the aforementioned composites of fear, anxiety, sadness, anger, and happiness (cf. Table 1). Unless noted otherwise, our reference to these terms refers to the composite index, not any single mood item.

Results

Primary analyses on fear versus anxiety. As predicted, participants expressed significantly higher levels of fear if they had been assigned to the MS versus neutral control condition (Ms = 1.80 vs. 1.29), $F(1, 164) = 21.15$, $p < .001$, $\eta_p^2 = .11$. As for anxiety, we also found a significant effect, but this effect was smaller, in terms of both the difference between means (Ms = 1.88 vs. 1.57) and the effect size in comparison with the aforementioned

Table 1
Correlations Among Mood Composites (Experiment 1)

Variable	1	2	3	4	5
1. Fear	(.92)				
2. Anxiety	.76***	(.86)			
3. Sadness	.75***	.71***	(.79)		
4. Anger	.57***	.61***	.63***	(.69)	
5. Happiness	−.31***	−.43***	−.45***	−.37***	(.88)
M	1.54	1.72	1.58	1.31	2.98
SD	0.82	0.81	0.74	0.48	1.04

*** $p < .001$.

[2] Most of our measures of negative affect, including the fear composite, revealed high levels of positive skew (all skews > 1.5). To correct for this, all of the statistical analyses on mood, reported ahead, are based on the natural log of these composites. For ease of interpretability, however, mean levels of these composites are reported in their original metric. We obtained a very similar, although slightly weaker, pattern of results when we conducted analyses on the nontransformed data.

[3] Among the participants who completed our studies, there were an extremely small number of individuals who seemed disinclined to take the task seriously, based on their written responses to the MS or control task. Participants were excluded if their protocols contained substantial amounts of nonsensical content or extraneous material unrelated to the task at hand. This led to the exclusion of two participants in Experiment 1 and one participant in Experiment 3.

findings involving fear, $F(1, 164) = 6.42$, $p < .01$, $\eta_p^2 = .04$. More important, the effect obtained with anxiety was no longer significant after controlling for fear, $F(1, 163) = 0.50$, $p > .50$, whereas the effects obtained with fear remained highly significant after controlling for anxiety, $F(1, 163) = 17.30$, $p < .001$, $\eta_p^2 = .09$.[4]

Additional analyses of mood composites. In Table 2, we present the means and effect sizes obtained with all of our composites as a function of experimental condition. We also present (a) the adjusted means for anxiety, sadness, anger, and happiness after controlling for fear (middle of Table 2) as well as (b) the effects of the MS task on fear after controlling for all of the other composites. As seen here, the effects of the MS task on fear remained significant even after controlling for all of the other mood indices, and that all of the other effects completely disappeared after controlling for fear.

Supplemental analyses on individual items from the "fear" subindex of the PANAS-X. One of our assumptions is that the items contained within the so-called "fear" subindex of the PANAS-X vary greatly in terms of their relevance to the MS manipulation. To verify this assumption, we conducted individual analyses on each of the items on that subindex, including *afraid, scared, frightened, nervous, jittery,* and *shaky*. The results of these analyses are shown in Table 3. As seen here, all three of the fear-related items (*afraid, scared, frightened*) were highly significant and accounted for a relatively large amount of variance in comparison to the effects obtained with the remaining anxiety-related items (*nervous, jittery,* and *shaky*).

Linguistic analyses of open-ended protocols. Use of the LIWC program essentially involves submitting a given body of text for semantic analysis, which then provides a tabulation of the relative frequency with which participants are generating words that fall within a predesignated semantic category (for further details, see Pennebaker et al., 2007). Once this frequency count is generated, one can then conduct further analyses to investigate whether these frequencies differ as a function of the condition to which participants were assigned.

The LIWC program allows researchers to create their own linguistic categories above and beyond the default categories that come preloaded on the program. Consequently, we created two new categories, one specifically designed to include anxiety-related terms and the other tailored to include fear-related items. (We constructed these categories a priori, in advance of the analyses to be reported below. Hence, inclusion and exclusion of words were neither informed nor constrained by the words actually generated by our participants.) In particular, we generated, with the assistance of a standard dictionary, a list of words that were clearly related to fear-related emotions, along with a second list of items pertaining to anxiety. In generating this list, we deliberately avoided ambiguous terms that could arguably belong to either list. The list of terms included in these two categories is presented in Table 4.

We expected a main effect of experimental condition, such that participants would be more likely to generate words related to fear, as well as anxiety, if they had been assigned to the MS condition compared to the control condition. A second and more noteworthy prediction was for a within-subject interaction involving the categories of fear versus anxiety, such that the difference between the MS versus control condition should be greater in the case of fear-related, as opposed to anxiety-related, concepts.

These analyses were conducted on all 168 participants in Experiment 1, submitting the text generated in their respective tasks to the LIWC program. This ended up generating two values of interest for us, one representing the proportion of words that represented fear-related concepts and the other representing anxiety-related concepts. These proportions were computed against the *total number* of words generated by each participant, including any and all grammatical terms (e.g., *a, the, with, by, under, when, after*) along with any other term that might have been written down, regardless of whether that term was positive or negative. This point is important because the percentage with which fear as well as anxiety terms appeared in the MS task was low. However, this does not mean that the emergence of these concepts were trivial but, merely, that these percentages took into account the total number of words generated by each subject, the vast majority of which had nothing to do with emotion at all.

Table 2
Effect of Experimental Manipulation in Mood Composites (Experiment 1)

Mood composite	Mortality salience condition	Neutral control	Difference	Effect size (η_p^2)
Fear	1.80	1.29	.51***	.11
Anxiety	1.88	1.57	.31**	.04
Sadness	1.79	1.37	.42***	.09
Anger	1.35	1.25	.10	.01
Happiness	2.81	3.15	−.34*	.02
Controlling for fear				
Anxiety	1.67	1.76	−.09	.01
Sadness	1.61	1.54	.07	.00
Anger	1.26	1.34	−.08	.01
Happiness	2.89	3.06	−.17	.01
Controlling for anxiety, sadness, anger, and happiness				
Fear	1.63	1.43	.20***	.07

* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 3
Analyses on Individual Items From the Original "Fear" Subindex of the Positive and Negative Affect Schedule–Expanded Form: Experiment 1

Individual item	Mortality salience condition	Neutral control	Difference	Effect size (η_p^2)
Afraid	1.87	1.30	.57***	.11
Scared	1.78	1.36	.42***	.07
Frightened	1.72	1.24	.48***	.09
Nervous	1.73	1.48	.25*	.03
Jittery	1.55	1.37	.18	.01
Shaky	1.55	1.31	.24	.02

* $p < .05$. *** $p < .001$.

[4] In all relevant analyses involving computation of the unique variance accounted for by correlated mood constructs, we found that variance inflation factors were all under 2.0, which is well below the threshold at which concerns are typically raised about multicollinearity (J. Cohen & Cohen, 1983).

Table 4
List of Words Included in the Fear Versus Anxiety Lexical Search: Experiment 1

Fear-related words	Anxiety-related words
afraid	ambiguous*
fear	ambivalent
feared	anxious
fearful*	anxiety
fearing	apprehens*
fears	nervous*
frantic*	shake*
fright*	shaki*
horr*	shaky
panic*	jittery
scare*	tense
scaring	tensing
scary	tension*
terrified	uncertain*
terrifies	unsure*
terrify*	uneasy
terror*	worr*

Note. Terms ending with an asterisk (*) allow for grammatical variations on the core semantic concept.

The results generated by these analyses are displayed in Figure 2. As seen here, the MS task was more conducive to the generation of fear-related compared to anxiety-related words. This asymmetry was confirmed by multivariate analyses, which revealed a two-way Experimental Manipulation (MS vs. control) × Linguistic Category (fear vs. anxiety) interaction, $F(1, 164) = 15.69$, $p < .001$, $\eta_p^2 = .09$. Follow-up univariate analyses showed that the effect size obtained for fear, $F(1, 164) = 67.60$, $p < .001$, $\eta_p^2 = .29$, was over twice that of anxiety, $F(1, 164) = 21.74$, $p < .001$, $\eta_p^2 = .12$. After controlling for anxiety, the effect of the MS task on fear-related words remained strong, $F(1, 164) = 50.86$, $p < .001$, $\eta_p^2 = .24$, and much stronger than the effects of the MS task on anxiety, after controlling for fear, $F(1, 164) = 8.52$, $p < .01$, $\eta_p^2 = .05$.

Additional analyses revealed a significant correlation between (a) the frequency with which fear-related concepts appeared in the written protocols and (b) explicit ratings of fearful mood ($r = .30$, $p < .001$). Hence, while it is important to keep in mind that the emergence of fear-related concepts in the protocols does not represent a measure of mood (at least in the conventional sense), correlational analyses revealed that these two constructs were clearly related. A similar albeit slightly smaller correlation emerged between the mood ratings of anxiety and the frequency with which anxiety-related concepts appeared in the protocols ($r = .20$, $p < .001$).

Discussion

Experiment 1 showed that the MS task is indeed capable of eliciting changes in affective experience, but these effects are narrowly constrained to the domain of fear. Our primary basis for this conclusion was derived from our analyses of posttask mood (cf. Tables 2 and 3). Additional evidence for our framework was provided by linguistic analyses of written responses to the MS task itself, which showed that participants were more likely to spontaneously generate words related to fear, as opposed to anxiety (cf. Figure 2). In addition to providing converging support for our predictions, these analyses help to rule out an alternative priming interpretation of our primary mood data. In particular, one might argue that the explicit inclusion of fear-related adjectives in the mood inventory induced participants to focus their attention on this particular emotion more than they otherwise would. As the linguistic analyses showed, however, participants *spontaneously* generated a greater preponderance of fear (vs. anxiety) terms on their written protocols, without any explicit prompting by us.

Experiment 2

As we have noted, the fact that the MS task elicits self-reported affect is of theoretical interest in its own right. However, it is also of interest to consider the downstream consequences of such affect.

Demonstrating the consequences of affective experience following the MS task would begin a much-needed reconciliation and integration with literally hundreds of studies in the mood/emotion literature. This body of work has shown the power of such affect to affect a wide variety of judgments, including judgments of the self, impressions of other people, perceptions of responsibility, and overall life satisfaction, to name just a few (e.g., Bless, Bohner, Schwarz, & Strack, 2001; Bodenhausen, Kramer, & Susser, 1994; Ellsworth & Scherer, 2003; Esses, Haddock, & Zanna, 1993; Forgas, 2001; Isen, Shalker, Clark, & Karp, 1978; Keltner, Ellsworth, & Edwards, 1993; Lambert, Khan, Lickel, & Fricke, 1997; Lambert et al., 2010; Lerner & Keltner, 2000; Martin, Abend, Sedikides, & Green, 1997; Ottati & Isbell, 1996; Schwarz, 1990, 2012; Schwarz & Bless, 1991; Schwarz & Clore, 1983, 2007).

To date, this literature has been virtually ignored in the TM literature. This gap is understandable in the sense that TM researchers have heretofore been under the impression that the MS task does not produce any self-reported affect at all. Given the results of Experiment 1, we now have reason to suppose that this

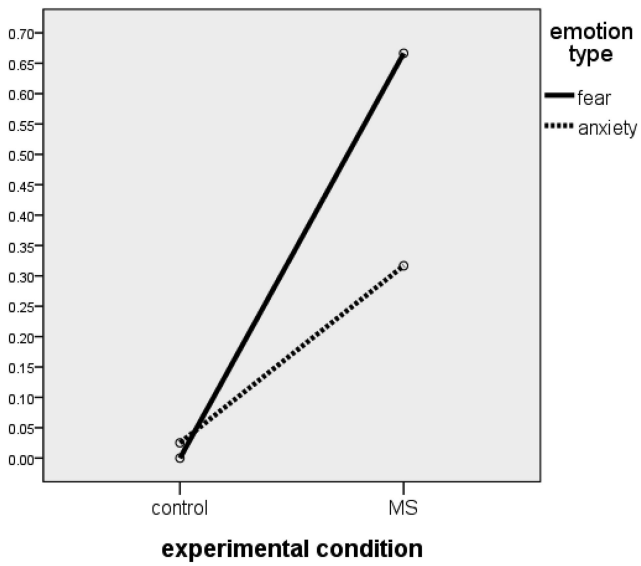

Figure 2. Mean percentage of fear versus anxiety words relative to the total number of words generated in the mortality salience (MS) versus control task (Experiment 1).

claim is false. As such, this opens the door for a fresh new way of thinking about the MS task, one that incorporates the theoretical implications of the aforementioned literature on mood and emotion.

Theoretical Tenets of the Mood-as-Information Framework

Several models of mood and social judgment have been proposed in the literature (e.g., Bower, 1991; Forgas, 2001; Schwarz & Clore, 2007). For our purposes, however, the most relevant framework is the mood-as-information model (Schwarz, 1990, 2012; Schwarz & Clore, 2007). According to this model, "people attend to their momentary feelings as a source of information in forming judgments, essentially asking themselves, 'How do I feel about this?'" (Schwarz, 2012, p. 291). During this process, people may "misread their current feelings as a response to the object of judgment, resulting in more favorable evaluations under positive rather than negative moods, unless their informational value is discredited" (Schwarz & Clore, 2003, p. 299). This process is presumed to be (a) relatively automatic and (b) not bound by rational rules or deliberate reasoning.

On the Affective Consequences of the MS Task for Self-Esteem

Given the robust effect of affect on a variety of different types of dependent variables (see above), we could have selected from any number of outcome variables. However, a number of considerations led us to focus on self-esteem. To begin with, self-esteem is an important psychological construct that has been investigated in well over 20,000 published articles and chapters, making it one of the most frequently studied constructs in the entire history of psychology (cf. Leary, 1999). Moreover, several studies in the mood literature have shown that a variety of self-appraisals, including measures of self-esteem (Levine, Wyer, & Schwarz, 1994) and subjective well-being (Schwarz & Clore, 1983; Schwarz, 2012), are robustly affected by situational variations in mood. Hence, by showing the impact of mood on self-esteem, this allows us to show the consequences of the MS task on a variable that is already acknowledged to be of great importance in a variety of different types of experimental paradigms.

There are two different ways of framing the predicted link between fear and self-esteem. One possibility is to view this as a mood congruence effect (Schwarz & Clore, 2007; see also Isen et al., 1978). That is, given that fear is obviously negative, the unpleasantness of this feeling has the potential to lead to more negative appraisals of the self. Another possibility, not exclusive of the first, is that the kind of fear elicited in the aftermath of the MS task may contain specific information value, introducing (mild) existential doubts as to the ultimate value of the self. This perspective does *not* rule out the possibility that people may attempt to find meaning in one's existence in the face of such threat. Rather, this merely suggests that the activation of MS-driven fear may exert its own negative effect on self-esteem, independent of any other motivational efforts to address the threat posed by MS. For our purposes, it was not particularly critical whether the predicted link between fear and self-esteem reflected a mood congruence effect or whether this reflected some dynamics that are specific to fear per se. Either way, the basic prediction is the same, with the activation of fear leading to lower levels of self-esteem.

Summary

Experiment 2 was designed with two primary goals in mind. First, given the novelty of our findings from Experiment 1, it seemed prudent to replicate these effects with an independent sample. As in our first study, we again relied on two different types of methodological approaches, one involving the direct assessment of mood and a second approach involving the linguistic analysis of participants' output from the task itself. A second goal was to gain insight into the downstream consequences of MS-elicited affect, in the form of fear. Our main prediction was that the fear elicited by the MS task should, in turn, lead to lower levels of self-esteem. Although there are a number of different measures of self-esteem, we relied on the Rosenberg (1965) measure, which is among the most widely used measures of this construct. Our main prediction for this variable was that the MS task should influence self-esteem indirectly, via increased levels of fear.

Method

Participants and design. A total of 210 participants (84 male and 126 female) were recruited from Amazon.com's Mechanical Turk online survey program. Participants were randomly assigned to one of two conditions (MS vs. control). As in the first study, our analyses included gender as a factor along with the MS manipulation, but we collapse over this factor given that none of our findings were qualified by gender.

Experimental manipulation. The MS task, as well as the neutral control condition, was identical to that of Experiment 1. As a supplement to our main analyses of mood, we again conducted additional linguistic analyses on the content of participants' protocols to provide converging evidence regarding the strong relevance of the MS to fear in particular.

Assessment of affect. As in Experiment 1, participants completed a mood inventory immediately after the experimental manipulation, rating their current affective state for each presented item along a scale ranging from 1 (*not at all*) to 5 (*very much so*). The composite indices were identical to those used in Experiment 1.

Posttask measures. Immediately following the experimental manipulation, participants were asked to complete a variety of different questionnaires pertaining to different aspects of their beliefs and attitudes. Embedded in this block of surveys was a short questionnaire on self-esteem, which was assessed using the Rosenberg (1965) instrument. This instrument consists of a series of 10 statements (e.g., "I feel that I am a person of worth, at least on an equal plane with others"; "I feel that I have a number of good qualities"). For each of these items, participants were asked to express their level of agreement versus disagreement along a scale ranging from 1 (*strongly disagree*) to 4 (*strongly agree*). A composite measure was based on an average of all 10 items, after reverse-scoring as needed ($\alpha = .92$).

Results

Primary mood analyses. Replicating the findings obtained in Experiment 1, participants expressed significantly higher levels of fear if they had been assigned to the MS versus neutral control condition (*M*s = 2.01 vs. 1.54), $F(1, 206) = 11.72$ $p < .001$, $\eta_p^2 = .05$. The effects for anxiety were much weaker, as we only observed a marginal effect of the experimental manipulation (*M*s = 2.22 vs. 1.95), $F(1, 206) = 3.05$, $p = .08$, $\eta_p^2 = .02$. After controlling for fear, there was no hint of higher anxiety in the MS condition. Indeed, if anything, participants showed a marginal tendency to report *lower* levels of anxiety following the MS task ($M_{adjusted}$s = 2.01 vs. 2.17), $F(1, 205) = 3.10$, $p = .08$, $\eta_p^2 = .02$. In contrast, the higher levels of fear in the MS condition remained highly significant after controlling for anxiety ($M_{adjusted}$s = 1.90 vs. 1.66), $F(1, 205) = 13.27$, $p < .001$, $\eta_p^2 = .06$.

Additional analyses on other mood composites. Compared to those assigned to the control condition, participants who completed the MS task reported higher levels of sadness (*M*s = 1.99 vs. 1.74), $F(1, 206) = 3.94$, $p < .05$, $\eta_p^2 = .02$. However, this effect disappeared after controlling for fear, $F(1, 205) = 1.08$, $p = .30$. There was a weak tendency for participants to express lower levels of happiness in the MS condition (*M*s = 2.75 vs. 2.92). However, this effect was not reliable, $F(1, 206) = 2.03$, $p = .16$, and controlling for fear eliminated even this small hint of any changes in happiness, $F(1, 205) = 0.26$, $p = .60$. As for anger, initial analyses revealed no differences in anger as a function of whether participants were assigned to the MS task versus control (*M*s = 1.64 vs. 1.67; $F < 1.00$). After controlling for fear, however, the level of anger was significantly *lower* in the MS compared to the control condition ($M_{adjusted}$s = 1.50 vs. 1.83), $F(1, 205) = 12.74$, $p < .01$, $\eta_p^2 = .06$. Conversely, controlling for anger served to substantially strengthen the effect obtained with fear ($M_{adjusted}$s = 2.04 vs. 1.53), $F(1, 205) = 25.32$, $p < .001$, $\eta_p^2 = .11$.

Linguistic analyses of written protocols. Replicating findings obtained in Experiment 1, linguistic analysis of the written protocols revealed a much more pronounced increase in fear (*M*s = 0.86 vs. 0.01) compared to anxiety (*M*s = 0.34 vs. 0.01). This asymmetry was responsible for an interaction involving the between-subject factor of task type (MS vs. control) and the within-subject factor of emotion (fear vs. anxiety), $F(1, 206) = 23.56$, $p < .001$, $\eta_p^2 = .10$.

Analyses of self-esteem. Ratings of self-esteem were virtually identical regardless of whether participants were assigned to the MS versus control condition (*M*s = 3.02 vs. 3.07), $F(1, 206) = 0.02$, $p > .50$. However, higher levels of fear were associated with lower levels of self-esteem ($r = -.35$, $p < .01$). (The magnitude of this negative relation was approximately the same regardless of whether participants were assigned to the MS vs. control condition; *r*s = −.31 vs. −.44, respectively, both *p*s < .001).

Given that the MS task had a causal effect on fear, this provided an initial indication that the MS task produced an indirect effect on self-esteem via fear. Formal evidence for such mediation was shown through the INDIRECT program of Preacher and Hayes (2008). In this analysis, the MS manipulation was treated as the independent variable, fear was treated as the mediator, and self-esteem was treated as the dependent variable. The results of this analysis, shown in Figure 3, provide support for the predicted role of fear as a mediator. Interestingly enough, the residual effect of the MS task, after controlling for fear, resulted in a marginal tendency for participants to express higher self-esteem in the MS condition. We consider the implications of this effect in more detail below.

Discussion

Experiment 2 generated several findings of interest. Two of these findings, involving assessment of fearful mood and emergence of fear-related language from the MS task itself replicated effects found in Experiment 1. A third finding pertained to the downstream consequences of mood, showing that the emergence of fear following the MS task produced a significant decrease in self-esteem.

One of the principles of TM theory is that self-esteem has the capacity to serve as a psychological buffer, to protect against the threat of MS. One may therefore wonder if the findings obtained in Experiment 2—which showed that fear negatively impacted self-esteem—run counter to this buffering assumption. However, closer scrutiny of the TM literature reveals that there is no contradiction, and indeed, our findings help to clarify a long-standing ambiguity in the TM literature. In order to more clearly understand the issues at hand, however, it is necessary to make the distinction between two different types of experimental paradigms, each of which operationalizes self-esteem in a different way.

Operationalization of Self-Esteem Within the TM Literature

Self-esteem as an independent variable. The most well-known use of self-esteem within the TM literature is as an independent variable. In particular, researchers assess self-esteem prior to the experimental manipulation and then examine whether a priori levels of self-esteem moderate the impact of the MS manipulation. The main prediction of TM is that self-esteem, when

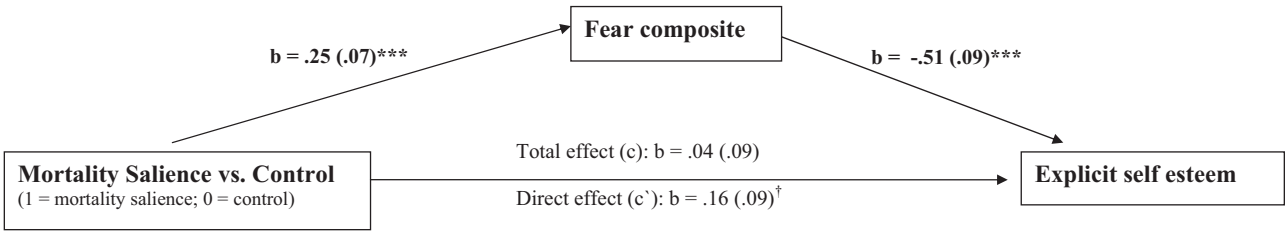

Figure 3. Experiment 2. Analyses via INDIRECT program (Preacher & Hayes, 2008) indicate significant mediation via fear (lower/upper confidence intervals = −.2040/−.0596, $p < .05$). † $p < .10$. *** $p < .001$.

measured in this way, essentially serves as a buffer in the sense that the threat of MS is postulated to be less if participants' level of self-esteem is relatively high than if it is low. There is some evidence for this aspect of TM theory (e.g., Greenberg et al., 1993; Harmon-Jones et al., 1997), although a recent literature review has revealed some inconsistency in such support (cf. Schmeichel et al., 2009).

Self-esteem as a dependent variable. A conceptually distinct issue is how the MS manipulation may affect self-esteem when it is operationalized as a dependent variable. Surprisingly, very few published TM studies have directly examined such a possibility. For example, in the 277 studies reviewed in the meta-analysis reported by Burke et al. (2010), we could not locate any that directly examined the impact of the MS task on explicit levels of self-esteem. Our own findings provide much-needed theoretical and empirical clarity to this important gap. To begin with, it is worth noting that Experiment 2 had an indirect, negative effect on self-esteem, mediated by fear. If we had never measured fear, we would not have been able to discern this important effect in the first place.

After controlling for fear, Experiment 2 revealed a marginal tendency for participants to express higher levels of self-esteem in the MS condition. We did not predict this effect, but it is interesting because it suggests that there may have been some aspect of the MS task that was producing a drive toward *higher* self-esteem. (This again highlights the importance of measuring fear because detection of this positive effect would not have been possible if we had not taken this emotion into account during our analyses.) Because the positive effect of the MS manipulation on self-esteem in Experiment 2 was relatively weak, it seemed best to avoid overinterpreting its meaning pending a replication. This was one of the secondary goals of Experiment 3.

Experiment 3

Thus far, we have shown that the MS task reliably elicits fear and that such feelings can have important consequences in their own right. However, this in no way rules out the possibility that the MS task is capable of producing *other* types of consequences that may not *necessarily* involve the activation of fear. In Experiment 3, we consider these issues in the context of studying the role of the MS task in producing shifts in what TM researchers refer to as cultural worldview beliefs.

A Brief Overview of Research on Cultural Worldview Beliefs

One of the major principles of TM theory is that the threat of MS motivates people to psychologically align themselves with one or more belief systems that provide a subjective sense of comfort and security. TM researchers collectively refer to these and other types of beliefs as cultural worldview beliefs, which have been defined as beliefs that "imbue the world with order, meaning, and permanence and provide a set of standards of valued behavior" (Jonas, Fritsche, & Greenberg, 2005, pp. 130–131). As suggested by this definition, the conceptualization of worldview beliefs is extremely broad, and one of the most frequent critiques of TM theory has centered on the overly vague nature of this construct (Buss, 1997; Jost, Glaser, Kruglanski, & Sulloway, 2003; Leary & Schreindorfer, 1997; Nail, McGregor, Drinkwater, Steele, & Thompson, 2009; Paulhus & Trapnell, 1997; Tritt et al., 2012; Wicklund, 1997; but see also Jonas et al., 2008; Pyszczynski et al., 2006). Nevertheless, setting aside (for now) these disagreements over the exact definition of these beliefs, an impressive body of evidence leaves little doubt that the MS task is capable of eliciting meaningful shifts in compensatory attitudes and values (cf. Hayes et al., 2010).

As noted in a recent meta-analysis by Burke et al. (2010), a wide variety of different types of cultural worldview beliefs have been identified by TM researchers. However, one of the more frequently obtained effects of the MS task is to induce greater positivity toward meaningful ingroups (Arndt, Greenberg, Schimel, Psyzczynski, & Solomon, 2002; Castano et al., 2011; F. Cohen, Ogilvie, Solomon, Greenberg, & Pyszczynski, 2005; Dechesne, Greenberg, Arndt, & Schimel, 2000) and/or a tendency to derogate individuals and groups holding values running contrary to the self/fellow ingroup members (Arndt & Greenberg, 1999; Arndt et al., 2002; Gailliot, Schmeichel, & Maner, 2007; Greenberg, Arndt, Simon, Pyszczynski, & Solomon, 2000).

What Is the Possible Role of Fear in MS-Driven Shifts in Ingroup Favoritism?

TM researchers have long claimed that these and other shifts occur independent of affect. They have based this claim on three types of evidence. First, although the number of studies using neutral control groups is not large (cf. Figure 1), significant shifts in worldview beliefs are often observed, even when the MS task produces no changes in affect at all. Second, even in the rare cases in which the MS task does produce changes in affect, the observed shifts in worldview beliefs still remain, even after controlling for mood. Third, among the (much larger) number of studies that have compared the MS task against aversive controls, researchers still find reliable effects of the MS task.

In combination, these findings might seem to suggest that self-reported affect plays little if any role in these observed shifts in cultural worldview beliefs. However, none of the studies in this area have actually measured fear, which we now see as critically relevant to the MS task. Hence, it remains an open question as to whether these shifts in cultural worldview beliefs do, or do not, involve mood. This raises an obvious question: What role, if any, would fear play in these kinds of shifts? We see at least two possibilities, which we briefly outline below. Notably, we saw each of these possibilities as having merit in its own right (i.e., one could make a case for either set of predictions). Because we did not have a strong, a priori preference for either of these predictions, these are most accurately framed as hypotheses.

Hypothesis 1: Affective mediation. Some research has suggested that induction of aversive emotion, including fear, can sometimes increase people's need for affiliation (cf. Cottrell & Epley, 1977; Kulik, Mahler, & Earnest, 1994). Although this work was not specifically concerned with the emergence of ingroup favoritism, it is not difficult to see how affiliation needs might lead to greater levels of positivity toward fellow ingroup members. Although this proposal seems straightforward, it suggests a fairly complex set of dynamics. For example, although MS-induced fear could instigate higher levels of ingroup favoritism at the outset, the emergence of ingroup favoritism as a palliative (i.e., comforting)

agent should presumably lead to lower levels of fear (i.e., MS task → higher fear → stronger ingroup favoritism → lower fear). This introduces a measurement dilemma for researchers, because the same two psychological variables—ingroup favoritism and fear—could be correlated in diametrically opposite ways, depending on the precise moment one assesses those variables.

Hypothesis 2: No affective mediation. There is a second, and relatively more simple, possibility. Suppose for the moment that TM researchers are indeed correct that shifts in ingroup favoritism do occur independent of all types of self-reported affect, including fear. Statistically, this would mean that one should observe a main effect of the MS manipulation independent of and in addition to any effects involving mood. This suggests that the MS task is capable of eliciting two classes of psychological consequences, one of which involves affect and one of which does not. We have already demonstrated evidence of the former type of consequence with respect to the observed changes in self-esteem. In addition to such affect-driven effects, the MS task could also trigger processes that do not involve self-reported affect at all, such as the tendency for people to favor the ingroup. This possibility has several attractive properties. In particular, it offers a process-level explanation that builds on existing principles of TM theory, but it also allows for a conceptual integration with theory and research from the mood area (Schwarz, 2012). That is, affect can play an important role in the context of a MS manipulation (thus solidifying ties with research and theory on mood), but it may also be true that TM researchers are correct that the MS task is capable of producing important consequences, independent of affect.

Summary

Experiment 3 was designed to address four goals. One goal was to further demonstrate the replicability and strength of the primary mood effects shown in Experiments 1 and 2. A second goal was to replicate the findings involving self-esteem shown in Experiment 2. A third goal was to show that our paradigm is capable of replicating previous findings obtained in the TM literature, which has shown that the MS task often triggers stronger affiliation to meaningful ingroups. A fourth and even more important goal was to determine whether such effects do, or do not, involve fear. As noted in our consideration of the two hypotheses above, it was not entirely clear on a priori grounds whether fear would play a critical role in these latter effects. Regardless of which hypothesis ends up being supported, however, our findings offer valuable leverage on an important principle of TM theory, offering for the first time a strong test of whether changes in cultural worldview beliefs involve self-reported affective experience.

Method

Participants and design. A total of 85 participants (24 male and 61 female), all residing within the United States, were recruited from Amazon.com's Mechanical Turk online survey program. Participants were randomly assigned to one of two experimental conditions (MS vs. control).

Experimental manipulation. The MS manipulation (including the use of the same neutral control condition) was identical to that of Experiments 1 and 2.

Assessment of affect. As in Experiments 1 and 2, participants completed a mood inventory immediately after the experimental manipulation, rating their current affective state for each presented item along a scale ranging from 1 (*not at all*) to 5 (*very much so*). The composite indices were identical to those used in our earlier studies, with only one exception: We slightly expanded our fear index to include *terrified*. This addition was not expected to substantially change the observed pattern of results, given that we anticipated that the term *terrified* would be closely related to items tapping fear. (This expectation was confirmed by our data, which revealed strong correlations with all of the other fear-related adjectives in our composite.) Nevertheless, it is notable that TM researchers never actually measured *terror* in their own studies, even though their own theoretical model—*terror* management theory—makes explicit reference to this emotion. Hence, in this study, our composite measure of fear was based on five, highly correlated mood items (*fearful, afraid, scared, frightened,* and *terrified*; $\alpha = .94$). An alternative set of analyses using a composite measure of fear based on the first four items, excluding *terrified,* produced a very similar pattern of results.

Posttask measures. Following the experimental manipulation, participants were asked to complete a variety of different questionnaires pertaining to different aspects of their beliefs and attitudes. Embedded in this block of surveys was a short questionnaire designed to measure patriotic attitudes toward the United States using a set of five items that we have employed in other research in our laboratory: "I am proud to be an American," "When I see the American flag flying I feel great," "The fact that I am an American is an important part of my identity," "I love my country," "Being an American is central to my sense of who I am." For each of these five items, participants were asked to express their attitudes toward it by selecting any number between 1 (*strongly disagree*) and 7 (*strongly agree*). A composite measure of patriotism was based on an average of all five items ($\alpha = .94$). In a later block of judgments, explicit self-esteem was measured, as in Experiment 2, using the same 10-item Rosenberg (1965) instrument ($\alpha = .93$).

Results

Mood analyses. Results revealed only one significant effect, with participants in the MS (vs. neutral) condition reporting higher levels of fear (Ms = 1.74 vs. 1.30), $F(1, 81) = 4.53$, $p < .05$, $\eta_p^2 = .05$. This replicates findings from Experiments 1 and 2. No effects were found for any of the other composites, including (a) anxiety (Ms = 1.99 vs. 1.82), $F(1, 81) = 0.39$, $p = .53$; (b) sadness (Ms = 1.78 vs. 1.58), $F(1, 81) = 2.50$, $p = .12$; (c) anger (Ms = 1.35 vs. 1.36), $F(1, 81) = 0.72$, $p = .40$; and happiness (Ms = 2.76 vs. 3.23), $F(1, 81) = 2.09$, $p = .15$. Indeed, controlling for all of the other composites served only to strengthen the impact of the MS task on fear (M_{adjusted}s = 1.64 vs. 1.34) $F(1, 77) = 7.70$, $p = .007$, $\eta_p^2 = .09$.

Self-esteem. Replicating Experiment 2, we again found an indirect effect of the MS manipulation on self-esteem, via fear. Initial analyses revealed no direct effects of the MS (vs. neutral) condition on self-esteem (Ms = 3.18 vs. 3.10), $F(1, 81) = 1.26$, $p = 26$. However, fear was significantly and negatively correlated with self-esteem ($r = -.47$, $p < .001$). The affective mediation implied by this pattern of results was formally confirmed by the INDIRECT program (Preacher & Hayes, 2008), which revealed significant links between (a) the experimental manipulation and

fear (i.e., the a path; $b = .25$, $p < .01$) and (b) fear and self-esteem (i.e., the b path; $b = -.71$, $p < .01$) and (c) no significant effect of the experimental manipulation on self-esteem (i.e., the c path; $b = .08$, $p > .25$). This corresponded to a significant mediational effect involving fear (lower/upper bias corrected confidence intervals = −.4000/−.0706, $p < .05$). This replicates the effect found in our earlier study (cf. Figure 3).

We again found evidence of higher self-esteem in the MS condition after controlling for fear. This was the same pattern found in Experiment 2, although this time, the path was reliable ($b = .25$, $p < .05$, for the c‘ path). Another way of showing the same effect is to conduct an ANOVA while controlling for fear. This revealed a main effect of the experimental manipulation on self-esteem, controlling for fear (M_{adjusted}s = 3.30 vs. 2.97), $F(1, 80) = 5.29$, $p < .05$, $\eta_p^2 = .06$. In combination, this suggests that the MS task is indeed capable of producing higher levels of self-esteem when operationalized as a dependent variable. In order to see this effect, however, one must first control for the effects of the MS task on fear.

Ingroup favoritism (patriotism). Participants expressed higher levels of patriotism toward the United States if they had been assigned to the MS (vs. neutral) condition (Ms = 5.81 vs. 5.30), $F(1, 81) = 4.36$, $p < .05$, $\eta_p^2 = .05$. Additional analyses revealed no reliable relation with fear ($r = -.06$, $p > .25$), and this was true regardless of whether participants were assigned to the MS or control condition (rs = −.01 vs. −.21, both ps > .15). Hence, in contrast to the analyses involving self-esteem (see above), there was no relation between fear and patriotism. As one might expect given this null relation, the impact of the experimental manipulation on patriotism was unaffected by controlling for fear, $F(1, 80) = 5.12$, $p < .05$, $\eta_p^2 = .06$. The effect of the MS task on patriotism also remained significant even after controlling for all of the mood composites (fear, anxiety, sadness, anger, happiness) in a single analysis, $F(1, 77) = 4.91$, $p < .05$, $\eta_p^2 = .06$.

Supplemental analyses. Additional analyses revealed no relation between ingroup favoritism and (a) anxiety ($r = -.05$), (b) sadness ($r = -.05$), (c) or anger ($r = -.19$; all ps *ns*). However, ingroup favoritism was correlated with the happiness composite, such that participants who were in a happy mood tended to report higher levels of ingroup favoritism, and vice versa ($r = .32$, $p < .01$). We discuss the theoretical implications of this finding below, after summarizing the main findings of this study.

Discussion

Experiment 3 replicated the fear effects from Experiments 1 and 2, once again showing a significant impact of the MS task compared against a neutral control group. We also replicated an important finding from Experiment 2, showing that the activation of fear led to lower levels of self-esteem. Experiment 3 also replicated an effect often obtained in the TM literature, showing that the level of ingroup favoritism toward the United States was higher if participants had completed the MS task than if they had not. The main effect of the MS manipulation remained significant after controlling not only for fear but for all of the other mood composites as well.

The fact that we found higher levels of ingroup favoritism in the MS condition is consistent with many studies in the TM literature (Burke et al., 2010). Moreover, the fact that these effects occurred independent of affective mediation is consistent with a major tenet of that theory. However, this aspect of our findings does *not* merely replicate previous TM studies. In the present research, we show, in the context of a single study, evidence for two processes, one of which involves affect and the other of which does not. The former process involved affective mediation and resulted in lower levels of self-esteem. The latter process occurred independent of affective mediation and resulted in higher levels of ingroup favoritism.

More generally, our findings provide fresh insight into the MS task in regard to its capacity to simultaneously trigger at least two different types of consequences, which can conveniently be regarded as involving *threat* (negative consequences) and *buffering* (positive consequences). On the one hand, our findings highlight the *threat* component of the MS manipulation, insofar as we show that the MS manipulation triggers (a) unpleasant sensations in the form of fear, (b) explicit verbalization of fear-related linguistic content, and (c) lower levels of self-esteem. On the other hand, we also provide evidence for a *buffering* component, which represents an issue of long-standing concern to TM researchers (Hayes et al., 2010). We found two pieces of evidence in support of this latter process. First, after controlling for fear, we found higher levels of self-esteem in the MS (vs. control) condition. Second, we found higher levels of ingroup favoritism in the MS condition, an effect that occurred independent of fear. To our knowledge, we are the first to provide empirical evidence of the simultaneous emergence of affectively driven threat processes and such buffering processes in the same study. We return to these issues in more detail in the General Discussion section.

Some additional aspects of our findings involving ingroup favoritism are worth noting Although such judgments were not contingent on fear, supplemental analyses revealed a significant positive correlation of ingroup favoritism with the happiness composite. One cannot draw causal conclusions from this finding, given that happiness was not actually affected by the MS manipulation. Nevertheless, this finding is compatible with previous research, which has shown that people in happy states are more likely to engage in heuristic (i.e., category-based) judgments of social categories (Bodenhausen et al., 1994; Mackie & Worth, 1989; Schwarz & Clore, 2007), a mode of processing that can potentially lead toward stronger ingroup biases (cf. Ziegler & Burger, 2011). Alternatively, it could be that affiliation with a valued ingroup (here, the United States) afforded participants some general level of psychological benefit in the form of higher levels of happiness (cf. Tajfel & Turner, 1986).

Setting aside the exact mechanisms involved in this happiness effect (an issue that surely merits further research), this finding is useful for present purposes because it rules out the possibility that assessment of ingroup favoritism was completely immune to any sorts of mood effect at all. Apparently, ingroup favoritism *can* be contingent on mood, and the fact that this effect occurred with respect to happiness in particular is consistent with past research (Bodenhausen et al., 1994; Mackie & Worth, 1989; Schwarz & Clore, 2007). Hence, our results showed that mood played a role in both of our outcome variables (i.e., self-esteem as well as ingroup favoritism), even though the exact nature of the mood in question and its relation to the MS manipulation were clearly different.

Experiment 4

The distinction between mood and emotion is best regarded as fuzzy rather than clear-cut (Frijda, 1986). However, according to many scholars, emotions always have clear referents (e.g., we are happy about or angry toward something specific). Moods, on the other hand, "lack a specific reference and are of a more diffuse nature" (Bless & Schwarz, 1996, p. 391). Thus, for example, one can meaningfully speak of being in an anxious mood without having to specify the exact referent of that feeling. Strictly speaking, our first three studies—along with the vast majority of studies in the TM literature—measured mood, rather than emotion.

For this reason, it seemed useful to conduct an additional study, employing a measure of emotion, that might be able to provide more direct evidence as to the type of affective experience triggered by the MS task. To this end, we simply asked participants a straightforward question derived directly from the task itself—"please consider what sort of emotions that the thought of your own death arouses in you"—and we then directly asked participants to mark, on a series of scales to follow, how these thoughts made them feel. In light of the findings obtained in our first three studies, we expected that this approach would reveal, in a more direct way, that thoughts of one's own death are more likely to trigger fear, as opposed to anxiety.

More direct assessment of the affective consequences of MS allows us to address a secondary, but still important, issue: How might the affective dynamics associated with MS compare to other types of threat? Among the hundreds of studies in the TM literature that have engaged in this sort of comparison, the vast majority of these have focused on dental pain as an aversive control task. In particular, of the 336 studies included in our methodological survey, over one third ($n = 126$) included the MS and dental pain tasks in the same design. In those cases in which mood was measured (typically, with the original or modified version of the PANAS), the majority of these studies reported a null effect. As noted earlier, these null findings present something of a puzzle for TM theory, in light of TM researchers' claims that the MS task does not elicit any negative affect. In other words, if the affect-free assumption *were* true, this would be logically inconsistent with the fact that null effects are typically found between the MS and aversive control conditions.

We are now in the position to clarify this ambiguity. It seems safe to conclude at this point that the affect-free claim is not true. Hence, MS tasks, as well as aversive controls, are likely to elicit self-reported negative affect. This conclusion is attractive because it is consistent with a rather large number of studies finding null effects when the MS task is compared to aversive controls. That is, by *disconfirming* one aspect of TM theory—the affect-free claim—this helps to clarify other findings obtained in the TM literature. Our primary prediction for Experiment 4 was that (a) thoughts of death are likely to initiate somewhat stronger feelings of fear, as opposed to anxiety, but (b) thoughts of a painful dental exam are likely to be dominated by anxiety more than fear.

These predictions were based not only on the results of our first three studies but also on the implications of a large scholarly body of work on the distinction between fear and anxiety (cf. D. C. Blanchard & Blanchard, 2008; Craske, 2003; Ohman, 2008; Zeidner & Matthews, 2011.) As noted earlier in this article, this distinction pertains to the tendency for fear to be associated with *identifiable* threats that are perceived as *inevitable*. This state of affairs corresponds to the threat of death. In contrast, the threat posed by a future dental exam, although surely upsetting, would seem to have significantly more uncertainty associated with it (e.g., the dentist *might* find a tooth that needs to be pulled, the exam *might* necessitate the use of needles, etc.).

Method

Participants and design. A total of 51 participants (25 male and 26 female) were recruited from Amazon.com's Mechanical Turk online survey program. There was one variable, manipulated within subjects, as to the type of threat that participants were asked to consider (death vs. painful dental exam).

Procedure. In one block of judgments, participants were asked to asked to "please indicate what sort of emotions that the thought of your own death arouses in you. In other words, as you think about your own death, how does that make you feel?". This query was then followed by a series of rating scales that ranged from 0 (*not at all*) to 100 (*very much so*): *afraid, angry, anxious, distressed, irritable, frightened, mad, nervous, sad, scared, unhappy,* and *worried.* In another block of judgments, participants were given the identical question, except that "thought of your own death" was replaced with "a painful dental exam." This was then followed by the same set of rating scales.[5]

Coding of emotion composites. Our primary interest was in comparing and contrasting the capacity of thoughts of death versus dental pain to elicit emotions of fear versus anxiety. To this end, our main focus was on a composite measure of fear, based on an average of *afraid, frightened,* and *scared,* as well as a composite measure of anxiety, averaging across *anxious, nervous,* and *worried.*[6] The reliabilities of these composites were high and virtually identical regardless of whether they were constructed in the context of death or dental pain (all αs $> .85$). For the sake of completeness, we also constructed composite measures for anger (*angry, irritable,* and *mad*) and sadness (averaging across *sad* and *unhappy*). The reliability of the anger composite was high for both tasks (both αs $> .85$). The reliability of the sadness composite, although acceptable, was somewhat lower, and this was true regardless of whether these emotions pertained to thoughts of death ($\alpha = .70$) or dental pain ($\alpha = .60$).

[5] For all participants, the type of threat (thoughts of death vs. dental pain) was manipulated within subjects. For approximately half of our participants, we held the order of task constant (dental pain followed by death), whereas, for the other half of our sample, order of task was randomized. However, the overall pattern of results was virtually identical regardless of how order was operationalized. Hence, the data presented below collapse over this variation.

[6] Unlike the other studies presented in this article, we predicted significant effects to occur with both the fear and the anxiety composites, depending on whether participants were focused on MS versus dental pain. For this reason, it was important to have equal numbers of fear-related and anxiety-related items in our mood inventory in order to maximize the extent to which the accessibility of these two constructs were held approximately equal while mood was assessed. This necessitated a reduction of the number of fear-related adjectives in order to match those used to measure anxiety. As we show ahead, however, we replicated the effects obtained in Experiments 1–3 even though the way we operationalized the fear composite was slightly different.

Results

Consistent with predictions, participants who were instructed to think about the emotions surrounding their own death reported higher levels of fear compared to anxiety (*M*s = 59.68 vs. 55.88), but this was reversed in the dental pain task (*M*s = 57.69 vs. 63.36). This finding was responsible for the emergence of a two-way interaction involving type of threat (death vs. dental pain) along with the type of emotion (fear vs. anxiety), $F(1, 49) = 6.00$, $p = .02$, $\eta_p^2 = .11$. Analyses revealed only one other effect, a tendency for participants to associate higher levels of sadness with death compared to a dental exam (*M*s = 57.44 vs. 42.87), $F(1, 49) = 9.90$, $p < .01$, $\eta_p^2 = .17$. The level of anger was generally low and did not differ as a function of whether the threat was death or dental pain (*M*s = 27.88 vs. 31.97; $F < 1.00$).

Another approach to analyzing our data is to ignore the distinction between fear and anxiety, forming a general six-item composite averaging across all six mood items noted above (i.e., *afraid, frightened*, *scared, anxious, nervous,* and *worried*). As applied to the measurement of emotion in the case of thoughts about death as well as about a painful dental exam, both of these composites were highly reliable (αs = .93 and .96, respectively). Scores on this general composite were slightly lower in the case of thoughts about death compared to that of a dental exam (*M*s = 57.78 vs. 60.53), but this difference was not even close to being significant, $F(1, 49) = 0.41$, $p > .50$. Hence, although formation of a general composite of this sort could certainly be justified on psychometric grounds, ignoring the distinction between fear and anxiety would have led us to the conclusion that the affective consequences of these two types of threat are no different from one another, which would have been an incorrect conclusion.

Discussion

Unlike our first three experiments, the primary objective in this study was to contrast the emotional dynamics associated with two different types of threat (death vs. a painful dental exam). Our findings reinforce a general theme running throughout our previous studies, namely, that one can gain important leverage by making distinctions among and between different types of affective experience. Most notably, these findings provided converging evidence to support the implications of our first three experiments that the threat of death has stronger connections to fear compared to anxiety.

General Discussion

The overriding goal of the present research was to gain greater insight into the foundational nature of the threat posed by the MS task. Our main concern here was with a long-standing claim made by TM researchers, namely, that the standard version of the MS task does not produce any reliable changes in consciously experienced affect. We provide strong evidence showing that this affect-free claim is incorrect, through the use of three different types of methodological/analytic approaches, including assessment of mood (Experiments 1–3), emotional reactions (Experiment 4), and linguistic analyses of what participants are actually writing during the MS task itself (Experiments 1–2). Critical to this endeavor is the need to focus specifically on fear, apart from other variables with which it is correlated, including anxiety. Experiments 2 and 3 showed that this activation of fear can have important consequences in its own right, as both studies showed that such affect led to lower levels of self-esteem. Finally, Experiment 3 also provided some reconciliation with previous studies in the TM literature, as we showed that the MS task strengthened ingroup favoritism, as operationalized by attitudes toward the United States. Moreover, we showed that these latter effects occurred independent not only of fear but of all of the other mood composites as well.

Theoretical Contribution to the TM Literature

Our research offers at least five theoretical contributions to TM research. First, our work provides a challenge to one of the more important tenets of TM theory, which is that MS is distinct from all other types of serious threat insofar as it does not elicit any changes in affective experience (see especially Tritt et al., 2012). Second, our research identifies—and resolves—something of an internal inconsistency within the TM literature involving claims pertinent to the comparison of the MS task to neutral versus aversive control groups. Third, our research proposes and tests the viability of a framework articulating affect-driven consequences of the MS task for at least certain types of outcome variables, including self-esteem. Fourth, our findings from Experiment 3 provide what we believe to be the first rigorous support for the idea that changes in worldview beliefs may occur independent of affect. Fifth, our research raises questions about the necessity of suppression processes, which, according to TM researchers, was based directly on their conclusions that the MS task produces no effect on emotion (cf. earlier quote from Hayes et al., 2010).

The last implication of our research, having to do with suppression, does not definitively prove that this aspect of TM theory is categorically false. For example, one important aspect of TM research is that reliable changes in worldview defense occur only after a delay (Hayes et al., 2010). TM researchers have interpreted these findings as supporting their view that participants are ultimately inhibiting the more unpleasant associations with MS as part of the proposed emergence of worldview defense. This line of research is interesting, and it seems to support the idea that people need time and resources in order to successfully implement such defense systems. Nevertheless, the concept of suppression, as it appears within the context of TM theory, is built on the premise that *people are pushing the unpleasant sensations of MS out of conscious awareness.* Our research clearly shows that this perspective is incorrect. Of course, it is possible that the strength/intensity of MS-induced fear may fade over time, at the same time that people start to show evidence of these worldview defenses. However, this could be more parsimoniously interpreted as simply showing the decay of affective experience over time, rather than active inhibition of emotion.[7]

[7] Several studies in the TM literature have suggested that the MS manipulation can lead to heightened levels of death thought accessibility (Hayes et al., 2010). Although such findings are interesting and potentially important, we do not see such findings as directly relevant to the validity of the suppression proposition. In particular, these findings show that thoughts related to MS (i.e., death) are more accessible after the manipulation of MS. These findings do not, however, speak to the issue of whether these or other threatening constructs had previously been *suppressed.*

None of these insights would have been possible if we had continued to take the broadband approach to conceptualizing and measuring affective experience that has dominated research and theory not only in the TM area but in other areas of social psychology as well (cf. Harmon-Jones et al., 2009; Huddy, Feldman, & Cassese, 2007; Pettersson & Turkheimer, 2013). If we had taken this approach to our own work, we never would have been able to show the importance of making distinctions between fear versus anxiety. Our research also highlights the importance of focusing on anger apart from other types of negative affect. In our case, we found a small but consistent tendency for the MS task to produce lower levels of anger compared to the control group. Here again, this aspect of our results would have been obscured if we had formed the kind of broadband index of negative affect such as the negative affect index of the PANAS/PANAS-X scale (cf. Harmon-Jones et al., 2009).

A "Threat + Buffer" Perspective

As noted earlier in this article, one may frame our results in the context of a "threat + buffer" perspective. Although we believe that this perspective could eventually give rise to a formal theoretical model of threat and social judgment, it is meant here only as a heuristically convenient device, a way of understanding the present results and clarifying their implications. Even so, we believe that this perspective may be useful as a way of highlighting the possible ways that TM theory might need some modification pending the outcome of future research.

The main idea here is that the MS task appears capable of simultaneously activating two distinct processes, which we have heuristically labeled as *threat* and *buffer*. In contrast to previous theorizing in the TM area, we believe that the MS task is fully capable of triggering threatening elements *about which participants are fully aware*. This most obviously includes self-reported fear but also includes explicit verbalization of fear-related linguistic content, as well as changes in explicit self-esteem. These findings are more important than they might seem, given the long insistence by TM researchers that the affective threat of MS is almost entirely experienced on an *unconscious* level (for a related discussion, see Tritt et al., 2012).

At the same time, the MS task *also* appears to be eliciting a distinct process that reflects a motivation to buffer oneself from the unpleasant implications of contemplating one's own morality. Our findings provided two kinds of support for this idea. For one thing, Experiment 3 showed that the MS task led to increased ingroup favoritism (patriotism), independent of fear. In addition, we also found evidence of a direct, positive effect of the MS task on self-esteem, although this effect was most clearly observed only after taking fear into account. In other words, the negative effects of fear on self-esteem could be conceptualized as a suppressor effect, which masked the tendency for the MS task to produce higher levels of self-esteem.

In an extremely general sense, this perspective is not radically different from what TM researchers have been saying for years, namely, that MS represents a potential source of threat and that people have developed a variety of mechanisms in order to cope with that threat (Hayes et al., 2010). Nevertheless, TM researchers have been studiously devoted to the position that the MS task poses only the *potential* for threat, not the actual, conscious experience of threat (cf. Tritt et al., 2012). We believe that their strong emphasis on unconscious threat may be overstated.

Moreover, the present perspective could potentially account for a curious gap in the TM literature that, to date, has not yet shown any reliable effects of the MS manipulation on self-esteem. One important part of the defensive process of TM theory involves a bolstering of one's level of self-esteem, along with what TM researchers have called *self-esteem striving* (Pyszczynski, Greenberg, Solomon, Arndt, & Schimel, 2004b; but see also Leary, 2004). However, at the same time that these defensive processes are pushing self-esteem ratings in a positive direction, the activation of fear may tend to have the opposite effect. Because these two processes are operating at the same time, they may, in essence, have the tendency to cancel each other out, giving the *illusion* that the MS task is not having any effect on this variable at all. Stated another way, the emergence of fear may represent a suppressor variable, insofar as it may be masking the kinds of self-bolstering processes that have long been of interest to TM theorists.

One additional aspect of our results, as they were relevant to the observed increase in ingroup favoritism, should be noted. On the one hand, our findings do not specify the exact reasons *why* the MS task might have produced this effect. However, this is an ambiguity that actually pervades much of the TM literature and is not specific to our study. Indeed, there is now lively debate as to whether the effect of the MS task on various aspects of the cultural worldview have anything to do with death at all, as noted in a recent critical review by Tritt et al. (2012):

> Other studies that have compared the effects of MS to threats to other salient psychological needs have suggested that cultural worldview defense *may not be specific to death anxiety* [emphasis added]. Threats to psychological needs such as certainty (e.g., McGregor, Zanna, Holmes, & Spencer, 2001; van den Bos, 2001), meaning (Proulx & Heine, 2008; Proulx, Heine, & Vohs, 2010; Randles, Proulx, & Heine, 2011; Simons & Rensink, 2005), affiliation/attachment security (Baumeister & Leary, 1995; Hart, Shaver, & Goldenberg, 2005), faith in the social system (e.g., Jost & Banaji, 1994; Lerner, 1980), and personal control (Kay, Gaucher, Napier, Callan, & Laurin, 2008; Whitson & Galinsky, 2008)—like MS threats—have all been found to evoke cultural worldview defense. (p. 720)

Nevertheless, while our findings pertaining to ingroup favoritism do not offer any additional leverage on this still-ongoing debate, our results bolster one aspect of claims made by TM researchers, which is that such effects occur independent of self-reported affect.

On the Size of Our Effects

It would be a gross distortion of our findings to assert that our participants were anything close to being flooded with fear following the MS manipulation. Nevertheless, the changes in fear-related sentiments accounted for upward of 9%–11% of the observed variance, depending on the study and the particular type of analyses involved. In statistical terms, the magnitude of our effects is easily comparable to findings obtained in other well-known paradigms in experimental social psychology (Funder & Ozer, 1983) and is equivalent, too, in the size of the cultural worldview shifts obtained in the TM literature (Burke et al., 2010).

This point is worth noting in light of the fact that the numbers of participants in two our studies were relatively large. In partic-

ular, the sample sizes of Experiments 1 and 2 (*n*s = 168 and 156, respectively) are easily twice the size of the typical study in the TM literature. However, the fact that we obtained significant effects in our paradigm is *not* simply due to our use of larger-than-usual samples. Indeed, given the relatively robust effect size of our effects, we would expect to find statistically significant effects with samples half the size used here. In order to demonstrate this point more formally, we used the "sample" command in SPSS to generate five independent subsamples from Experiment 1, each selecting a random sample of approximately 50% of the cases from the original data set. (The sample sizes of these five subsets ranged between 71 and 93, which is much more in line with the average samples in the TM literature for a two-cell experimental design.) We then conducted separate ANOVAs on each of these data sets, each time assessing the impact of the MS manipulation on the same fear composite used in our main studies. In each case, ANOVAs yielded a significant effect (all five *p*s $< .01$), with the percentage of variance accounted for ranging between 7% and 11%.

A Practical Guide for Researchers

Aside from their larger theoretical implications, our findings are useful insofar as they offer easily achievable guidelines for future researchers who wish to understand the affective consequences of the MS manipulation. To begin with, researchers must include some sort of neutral control group in combination with the MS task. This may seem to be an obvious point, but a surprisingly large proportion of the studies in this area do not do so (cf. Figure 1). Researchers also need to measure a broad diversity of affective states immediately following the experimental manipulation and, equally important, construct and use composite measures that allow one to distinguish between different types of negative affect.

We recommend that researchers avoid the use of (a) broadband indices of mood, such as the negative affect index of the PANAS, as well as (b) omnibus MANOVAs on affective subscales of the PANAS-X. As we have discussed, both of these practices are likely to hinder one's ability to detect actual changes in mood. Also, to the extent that the MS task is capable of eliciting different types of effects on different types of correlated emotional states, analysis of covariance may be useful in order to gain insight into the unique effects of this task on any given type of affective experience. Important, too, is the need to make empirical distinctions between fear and anxiety. As we have noted, this recommendation does *not* constitute a formal, theoretical argument as to the clear-cut distinction between these two states. Rather, we simply suggest that the distinction may be useful, given that our data clearly indicate that measures of fear more readily allow one to detect shifts in mood following the MS manipulation. At this stage of our research, we are not prepared to make ironclad recommendations as to exactly how one should operationalize one's measure of fear. That said, our findings suggest that some combination of commonly employed words (e.g., *scared, afraid, frightened, fearful*) would certainly be appropriate. However, investigators should be open to using a variety of different fear-related adjectives in their own research.

Beyond MS: Implications of Our Research for Assessing Other Types of Threat

One of the larger messages of our studies is that researchers should use emotion-specific measures that are appropriately tailored to measure the particular type of threat at hand. For example, reminding people of the threat of terrorism is likely to elicit a mixture of several different types of negative emotion, including fear, anxiety, sadness, and anger. However, the nature of this emotion may depend on a number of factors, including how long ago the event actually occurred. Indeed, as we have shown in our recent work on the 9/11 attacks (Lambert et al., 2010), retrospective reminders of this terrorist event, several years after the fact, elicit much stronger feelings of anger compared to anxiety.

As a related point, use of broadband indices, coupled with other methodological problems we have identified in this article (e.g., absence of a true control group), runs a serious risk of underestimating the affective impact of priming participants with threat in laboratory contexts. For example, a recent study by Landau et al. (2004) randomly assigned participants to one of three threats: (a) the standard MS condition, (b) an aversive control task (i.e., the threat posed by a future exam), and (c) an explicit reminder of the 9/11 attacks (e.g., "please describe the emotions that the thought of the terrorist attacks on September 11, 2001, arouses in you"). Posttask measurement of mood, using the PANAS-X, revealed no effects of mood whatsoever.

Given the implications of our research, these null results are almost certainly spurious, stemming from two problems. First, the design of Landau et al. (2004), like many studies in the TM literature, failed to include a neutral control group. Second, the use of the PANAS-X may also have hampered ability to detect emotion-specific consequences that are likely to be different for each type of threat. In particular, the affective consequences of the MS task are likely to be specific to fear, a future exam is likely to be specific to anxiety, and reminders of a previous terrorist attack are likely to be specific to anger (Lambert et al., 2010).

Directions for Future Research

The present results highlight the need for several avenues of future research. We have already noted the need for additional evidence in support of our "threat + buffer" perspective, which, if confirmed, offers a valuable way of integrating the TM literature with research and theory on the effects of mood on social judgment. In addition, further research is clearly needed to determine which outcome variables in the context of TM research involve affect and which do not. The charting of this theoretical landscape may take quite a bit of time, but such efforts are likely to eventually provide insight into the various ways that the MS task influences different types of outcome variables.

Additional research is also needed to more fully understand the content of what participants are actually writing during the task itself. We have already shown how this approach can be useful in terms of providing converging evidence with respect to the affective consequences of the task, but there are a host of other issues that remain to be studied using this approach. Recent work in our laboratory (Eadeh, Peak, Slochower, & Lambert, 2014) suggests that for many (but not all) participants, the MS task essentially represents an explicit prime of religious/quasi-religious views, and the degree to which the protocols contain such manifest content is strongly predictive of posttask shifts in certain types of conservative ideology. These and other lines of future research should be instrumental in affording greater insight into what we consider to

be one of the more intriguing and noteworthy manipulations in the history of experimental social psychology.

References

Abelson, R. P. (1995). *Statistics as principled argument*. Hillsdale, NJ: Erlbaum.

Arndt, J., Allen, J. J. B., & Greenberg, J. (2001). Traces of terror: Subliminal death primes and facial electromyographic indices of affect. *Motivation and Emotion, 25,* 253–277. doi:10.1023/A:1012276524327

Arndt, J., & Greenberg, J. (1999). The effects of a self-esteem boost and mortality salience on responses to boost relevant and irrelevant worldview threats. *Personality and Social Psychology Bulletin, 25,* 1331–1341. doi:10.1177/0146167299259001

Arndt, J., Greenberg, J., Schimel, J., Pyszczynski, T., & Solomon, S. (2002). To belong or not to belong, that is the question: Terror management and identification with gender and ethnicity. *Journal of Personality and Social Psychology, 83,* 26–43. doi:10.1037/0022-3514.83.1.26

Blanchard, D. C., & Blanchard, R. J. (2008). Defensive behaviors, fear, and anxiety. In R. J. Blanchard, D. C. Blanchard, G. Griebel, & D. J. Nutt (Eds.), *Handbook of anxiety and fear* (pp. 63–79). San Diego, CA: Elsevier.

Blanchard, R. J., Blanchard, D. C., Griebel, G., & Nutt, D. J. (Eds.). (2008). *Handbook of anxiety and fear*. San Diego, CA: Elsevier.

Bless, H., Bohner, G., Schwarz, N., & Strack, F. (2001). Mood and persuasion: A cognitive response analysis. In W. Parrott (Ed.), *Emotions in social psychology: Essential readings* (pp. 216–226). New York, NY: Psychology Press.

Bless, H., & Schwarz, N. (1996). Mood: Its impact on cognition and behavior. In A. Manstead & M. Hewstone (Eds.), *The Blackwell encyclopedia of social psychology* (pp. 391–396). Oxford, England: Blackwell Publishers.

Bodenhausen, G. V., Kramer, G. P., & Susser, K. (1994). Happiness and stereotypic thinking in social judgment. *Journal of Personality and Social Psychology, 66,* 621–632. doi:10.1037/0022-3514.66.4.621

Bower, G. H. (1991). Mood congruity of social judgment. In J. Forgas (Ed.), *Emotion and social judgment* (pp. 31–53). Oxford, England: Pergamon Press.

Burke, B. L., Martens, A., & Faucher, E. H. (2010). Two decades of terror management theory: A meta-analysis of mortality salience research. *Personality and Social Psychology Review, 14,* 155–195. doi:10.1177/1088868309352321

Buss, D. M. (1997). Human social motivation in evolutionary perspective: Grounding terror management theory. *Psychological Inquiry, 8,* 22–26. doi:10.1207/s15327965pli0801_3

Carver, C. S., & Harmon-Jones, E. (2009). Anger is an approach-related affect: Evidence and implications. *Psychological Bulletin, 135,* 183–204. doi:10.1037/a0013965

Castano, E., Leidner, B., Bonacossa, A., Nikkah, J., Perrulli, R., Spencer, B., & Humphrey, N. (2011). Ideology, fear of death, and death anxiety. *Political Psychology, 32,* 601–621. doi:10.1111/j.1467-9221.2011.00822.x

Chantarujikapong, S. I., Scherrer, J. F., Xian, H., Eisen, S. A., Lyons, M. J., Goldberg, J., . . . True, W. R. (2001). A twin study of generalized anxiety disorder symptoms, panic disorder symptoms, and post-traumatic stress disorder in men. *Psychiatry Research, 103,* 133–145. doi:10.1016/S0165-1781(01)00285-2

Cohen, F., Ogilvie, D. M., Solomon, S., Greenberg, J., & Pyszczynski, T. (2005). American roulette: The effect of reminders of death on support for George W. Bush in the 2004 presidential election. *Analyses of Social Issues and Public Policy (ASAP), 5,* 177–187. doi:10.1111/j.1530-2415.2005.00063.x

Cohen, J. (1992). A power primer. *Psychological Bulletin, 112,* 155–159. doi:10.1037/0033-2909.112.1.155

Cohen, J., & Cohen, P. (1983). *Applied multiple regression/correlation analysis for the behavioral sciences* (2nd ed.). Hillsdale, NJ: Erlbaum.

Cole, D. A., Maxwell, S. E., Arvey, R., & Salas, E. (1994). How the power of MANOVA can both increase and decrease as a function of the intercorrelations among the dependent variables. *Psychological Bulletin, 115,* 465–474. doi:10.1037/0033-2909.115.3.465

Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimentation: Design and analysis issues for field settings*. Boston, MA: Houghton Mifflin.

Cottrell, N. B., & Epley, S. W. (1977). Affiliation, social comparison and socially mediated stress reduction. In J. M. Suls & R. L. Miller (Eds.), *Social comparison process: Theoretical and empirical perspectives* (pp. 43–68). New York, NY: Halsted Press.

Cox, C. R., Arndt, J., Pyszczynski, T., Greenberg, J., Abdollahi, A., & Solomon, S. (2008). Terror management and adults' attachment to their parents: The safe haven remains. *Journal of Personality and Social Psychology, 94,* 696–717. doi:10.1037/0022-3514.94.4.696

Craske, M. G. (2003). *Origins of phobias and anxiety disorders: Why more women than men?* Oxford, England: Elsevier Science.

Davis, M. (1998). Are different parts of the extended amygdala involved in fear versus anxiety? *Biological Psychiatry, 44,* 1239–1247. doi:10.1016/S0006-3223(98)00288-1

Dechesne, M., Greenberg, J., Arndt, J., & Schimel, J. (2000). Terror management and the vicissitudes of sports fan affiliation: The effects of mortality salience on optimism and fan identification. *European Journal of Social Psychology, 30,* 813–835. doi:10.1002/1099-0992(200011/12)30:6<813::AID-EJSP17>3.0.CO;2-M

Eadeh, F., Peak, S. A., Slochower, J., & Lambert, A. J. (2014). *Death demystified: Towards a greater understanding of the mortality salience manipulation*. Unpublished manuscript.

Ebesutani, C., Smith, A., Bernstein, A., Chorpita, B. F., Higa-McMillan, C., & Nakamura, B. (2011). A bifactor model of negative affectivity: Fear and distress components among younger and older youth. *Psychological Assessment, 23,* 679–691. doi:10.1037/a0023234

Ellsworth, P. C., & Scherer, K. R. (2003). Appraisal processes in emotion. In R. J. Davidson, K. R. Scherer, & H. H. Goldsmith (Eds.), *Handbook of affective sciences* (pp. 572–595). New York, NY: Oxford University Press.

Esses, V. M., Haddock, G., & Zanna, M. P. (1993). Values, stereotypes, and emotions as determinants of intergroup attitudes. In D. M. Mackie & D. L. Hamilton (Eds.), *Affect, cognition, and stereotyping: Interactive processes in group perception* (pp. 137–166). San Diego, CA: Academic Press.

Forgas, J. P. (2001). The affect infusion model (AIM): An integrative theory of mood effects on cognition and judgment. In L. L. Martin & G. L. Clore (Eds.), *Theories of mood and cognition: A user's guidebook* (pp. 99–134). Mahwah, NJ: Erlbaum.

Frijda, N. H. (1986). *The emotions*. Cambridge, England: Cambridge University Press.

Funder, D. C., & Ozer, D. J. (1983). Behavior as a function of the situation. *Journal of Personality and Social Psychology, 44,* 107–112. doi:10.1037/0022-3514.44.1.107

Gailliot, M. T., Schmeichel, B. J., & Maner, J. K. (2007). Differentiating the effects of self-control and self-esteem on reactions to mortality salience. *Journal of Experimental Social Psychology, 43,* 894–901. doi:10.1016/j.jesp.2006.10.011

Gotlib, I. H., & Meyer, J. P. (1986). Factor analysis of the Multiple Affect Adjective Check List: A separation of positive and negative affect. *Journal of Personality and Social Psychology, 50,* 1161–1165. doi:10.1037/0022-3514.50.6.1161

Greenberg, J., Arndt, J., Simon, L., Pyszczynski, T., & Solomon, S. (2000). Proximal and distal defenses in response to reminders of one's mortality: Evidence of a temporal sequence. *Personality and Social Psychology Bulletin, 26,* 91–99. doi:10.1177/0146167200261009

Greenberg, J., Kosloff, S., Solomon, S., Cohen, F., & Landau, M. (2010). Toward understanding the fame game: The effect of mortality salience on the appeal of fame. *Self and Identity, 9,* 1–18. doi:10.1080/15298860802391546

Greenberg, J., Porteus, J., Simon, L., & Pyszczynski, T. (1995). Evidence of a terror management function of cultural icons: The effects of mortality salience on the inappropriate use of cherished cultural symbols. *Personality and Social Psychology Bulletin, 21,* 1221–1228. doi:10.1177/01461672952111010

Greenberg, J., Pyszczynski, T., Solomon, S., Pinel, E., Simon, L., & Jordan, K. (1993). Effects of self-esteem on vulnerability-denying defensive distortions: Further evidence of an anxiety-buffering function of self-esteem. *Journal of Experimental Social Psychology, 29,* 229–251.

Greenberg, J., Pyszczynski, T., Solomon, S., Simon, L., & Breus, M. (1994). Role of consciousness and accessibility of death-related thoughts in mortality salience effects. *Journal of Personality and Social Psychology, 67,* 627–637. doi:10.1037/0022-3514.67.4.627

Greenberg, J., Simon, L., Harmon-Jones, E., Solomon, S., Pyszczynski, T., & Lyon, D. (1995). Testing alternative explanations for mortality salience effects: Terror management, value accessibility, or worrisome thoughts? *European Journal of Social Psychology, 25,* 417–433. doi:10.1002/ejsp.2420250406

Halloran, M. J., & Kashima, E. S. (2004). Social identity and worldview validation: The effects of ingroup identity primes and mortality salience on value endorsement. *Personality and Social Psychology Bulletin, 30,* 915–925. doi:10.1177/0146167204264080

Harmon-Jones, E., Harmon-Jones, C., Abramson, L., & Peterson, C. K. (2009). PANAS positive activation is associated with anger. *Emotion, 9,* 183–196. doi:10.1037/a0014959

Harmon-Jones, E., Simon, L., Greenberg, J., Pyszczynski, T., Solomon, S., & McGregor, H. (1997). Terror management theory and self-esteem: Evidence that increased self-esteem reduces mortality salience effects. *Journal of Personality and Social Psychology, 72,* 24–36. doi:10.1037/0022-3514.72.1.24

Hayes, J., Schimel, J., Arndt, J., & Faucher, E. H. (2010). A theoretical and empirical review of the death-thought accessibility concept in terror management research. *Psychological Bulletin, 136,* 699–739. doi:10.1037/a0020524

Herron, E. W. (1969). The Multiple Affect Adjective Check List: A critical analysis. *Journal of Clinical Psychology, 25,* 46–53. doi:10.1002/1097-4679(196901)25:1<46::AID-JCLP2270250113>3.0.CO;2-O

Hettema, J. M., Prescott, C. A., Myers, J. M., Neale, M. C., & Kendler, K. S. (2005). The structure of genetic and environmental risk factors for anxiety disorders in men and women. *Archives of General Psychiatry, 62,* 182–189. doi:10.1001/archpsyc.62.2.182

Hohman, Z. P., & Hogg, M. A. (2011). Fear and uncertainty in the face of death: The role of life after death in group identification. *European Journal of Social Psychology, 41,* 751–760. doi:10.1002/ejsp.818

Huberty, C. J., & Morris, J. D. (1989). Multivariate analysis versus multiple univariate analyses. *Psychological Bulletin, 105,* 302–308. doi:10.1037/0033-2909.105.2.302

Huddy, L., Feldman, S., & Cassese, E. (2007). On the distinct political effects of anxiety and anger. In A. Crigler, M. MacKuen, G. Marcus, & W. R. Neuman (Eds.), *The affect effect: The dynamics of emotion in political thinking and behavior* (pp. 202–230). doi:10.7208/chicago/9780226574431.003.0009

Isen, A. M., Shalker, T. E., Clark, M., & Karp, L. (1978). Affect, accessibility of material in memory, and behavior: A cognitive loop? *Journal of Personality and Social Psychology, 36,* 1–12. doi:10.1037/0022-3514.36.1.1

Janssen, J., Dechesne, M., & van Knippenberg, A. (1999). The psychological importance of youth culture: A terror management approach. *Youth and Society, 31,* 152–167.

Jonas, E., Fritsche, I., & Greenberg, J. (2005). Currencies as cultural symbols: An existential psychological perspective on reactions of Germans toward the Euro. *Journal of Economic Psychology, 26,* 129–146. doi:10.1016/j.joep.2004.02.003

Jonas, E., Martens, A., Kayser, D. N., Fritsche, I., Sullivan, D., & Greenberg, J. (2008). Focus theory of normative conduct and terror-management theory: The interactive impact of mortality salience and norm salience on social judgment. *Journal of Personality and Social Psychology, 95,* 1239–1251. doi:10.1037/a0013593

Jost, J. T., Glaser, J., Kruglanski, A. W., & Sulloway, F. (2003). Political conservatism as motivated social cognition. *Psychological Bulletin, 129,* 339–375. doi:10.1037/0033-2909.129.3.339

Kay, A. C., Gaucher, D., Napier, J. L., Callan, M. J., & Laurin, K. (2008). God and the government: Testing a compensatory control mechanism for the support of external systems. *Journal of Personality and Social Psychology, 95,* 18–35. doi:10.1037/0022-3514.95.1.18

Keltner, D., Ellsworth, P., & Edwards, K. (1993). Beyond simple pessimism: Effects of sadness and anger on social perception. *Journal of Personality and Social Psychology, 64,* 740–752. doi:10.1037/0022-3514.64.5.740

Kipfer, B. A. (2011). *Roget's international thesaurus* (7th ed.). New York, NY: Collins Reference.

Kirkpatrick, L. A., & Navarrete, C. (2006). Reports of my death anxiety have been greatly exaggerated: A critique of terror management theory from an evolutionary perspective. *Psychological Inquiry, 17,* 288–298. doi:10.1080/10478400701366969

Kulik, J. A., Mahler, H. M., & Earnest, A. (1994). Social comparison and affiliation under threat: Going beyond the affiliate-choice paradigm. *Journal of Personality and Social Psychology, 66,* 301–309. doi:10.1037/0022-3514.66.2.301

Lambert, A. J., Khan, S. R., Lickel, B. A., & Fricke, K. (1997). Mood and the correction of positive versus negative stereotypes. *Journal of Personality and Social Psychology, 72,* 1002–1016. doi:10.1037/0022-3514.72.5.1002

Lambert, A. J., Scherer, L. D., Schott, J., Olson, K. R., Andrews, R. K., O'Brien, T. C., & Zisser, A. R. (2010). Rally effects, threat, and attitude change: An integrative approach to understanding the role of emotion. *Journal of Personality and Social Psychology, 98,* 886–903. doi:10.1037/a0019086

Landau, M. J., Goldenberg, J., Greenberg, J., Gillath, O., Solomon, S., Cox, C., . . . Pyszczynski, T. (2006). The siren's call: Terror management and the threat of men's sexual attraction to women. *Journal of Personality and Social Psychology, 90,* 129–146.

Landau, M. J., Greenberg, J., & Rothschild, Z. K. (2009). Motivated cultural worldview adherence and culturally loaded test performance. *Personality and Social Psychology Bulletin, 35,* 442–453. doi:10.1177/0146167208329630

Landau, M. J., Greenberg, J., Solomon, S., Pyszczynski, T., & Martens, A. (2006a). Windows into nothingness: Terror management, meaninglessness, and negative reactions to modern art. *Journal of Personality and Social Psychology, 90,* 879–892. doi:10.1037/0022-3514.90.6.879

Landau, M. J., Greenberg, J., Sullivan, D., Routledge, C., & Arndt, J. (2009). The protective identity: Evidence that mortality salience heightens the clarity and coherence of the self-concept. *Journal of Experimental Social Psychology, 45,* 796–807. doi:10.1016/j.jesp.2009.05.013

Landau, M. J., Solomon, S., Greenberg, J., Cohen, F., Pyszczynski, T., Arndt, J., . . . Cook, A. (2004). Deliver us from evil: The effects of mortality salience and reminders of 9/11 on support for President George W. Bush. *Personality and Social Psychology Bulletin, 30,* 1136–1150. doi:10.1177/0146167204267988

Landauer, T. K., Foltz, P. W., & Laham, D. (1998). Introduction to latent semantic analysis. *Discourse Processes, 25,* 259–284. doi:10.1080/01638539809545028

Leary, M. R. (1999). The social and psychological importance of self-

esteem. In R. M. Kowalski & M. R. Leary (Eds.), *The social psychology of emotional and behavioral problems: Interfaces of social and clinical psychology* (pp. 197–221). doi:10.1037/10320-007

Leary, M. R. (2004). The function of self-esteem in terror management theory and sociometer theory: Comment on Pyszczynski et al. (2004). *Psychological Bulletin, 130,* 478–482. doi:10.1037/0033-2909.130.3.478

Leary, M. R., & Schreindorfer, L. S. (1997). Unresolved issues with terror management theory. *Psychological Inquiry, 8,* 26–29. doi:10.1207/s15327965pli0801_4

Lerner, J., & Keltner, D. (2000). Beyond valence: Toward a model of emotion-specific influences on judgment and choice. *Cognition & Emotion, 14,* 473–493. doi:10.1080/026999300402763

Levine, S. R., Wyer, R. S., & Schwarz, N. (1994). Are you what you feel? The affective and cognitive determinants of self-judgments. *European Journal of Social Psychology, 24,* 63–77. doi:10.1002/ejsp.2420240105

Lieberman, J. D. (1999). Terror management, illusory correlation, and perceptions of minority groups. *Basic and Applied Social Psychology, 21,* 13–23. doi:10.1207/s15324834basp2101_2

Mackie, D. M., & Worth, L. T. (1989). Processing deficits and the mediation of positive affect in persuasion. *Journal of Personality and Social Psychology, 57,* 27–40. doi:10.1037/0022-3514.57.1.27

Martin, L. L., Abend, T., Sedikides, C., & Green, J. D. (1997). How would it feel if . . .? Mood as input to a role fulfillment evaluation process. *Journal of Personality and Social Psychology, 73,* 242–253. doi:10.1037/0022-3514.73.2.242

McGregor, I., Zanna, M. P., Holmes, J. G., & Spencer, S. J. (2001). Compensatory conviction in the face of personal uncertainty: Going to extremes and being oneself. *Journal of Personality and Social Psychology, 80,* 472–488. doi:10.1037/0022-3514.80.3.472

McNaughton, N. (2011). Fear, anxiety and their disorders: Past, present and future neural theories. *Psychology & Neuroscience, 4,* 173–181. doi:10.3922/j.psns.2011.2.002

Nail, P. R., McGregor, I., Drinkwater, A. E., Steele, G. M., & Thompson, A. W. (2009). Threat causes liberals to think like conservatives. *Journal of Experimental Social Psychology, 45,* 901–907. doi:10.1016/j.jesp.2009.04.013

Öhman, A. (2008). Fear and anxiety: Overlaps and dissociations. In M. Lewis & J. M. Haviland-Jones (Eds.), *Handbook of emotions* (3rd ed., pp. 709–728). New York, NY: Guilford Press.

Ottati, V. C., & Isbell, L. M. (1996). Effects on mood during exposure to target information on subsequently reported judgments: An on-line model of misattribution and correction. *Journal of Personality and Social Psychology, 71,* 39–53. doi:10.1037/0022-3514.71.1.39

Paulhus, D. L., & Trapnell, P. D. (1997). Terror management theory: Extended or overextended? *Psychological Inquiry, 8,* 40–43. doi:10.1207/s15327965pli0801_8

Pelham, B. W. (1997). Human motivation has multiple roots. *Psychological Inquiry, 8,* 44–47. doi:10.1207/s15327965pli0801_9

Pennebaker, J. W., Booth, R. J., & Francis, M. E. (2007). Linguistic Inquiry and Word Count (LIWC2007): A computer-based text analysis program [Computer software]. Austin, TX: LIWC.net.

Pettersson, E., & Turkheimer, E. (2013). Approach temperament, anger, and evaluation: Resolving a paradox. *Journal of Personality and Social Psychology, 105,* 285–300. doi:10.1037/a0033046

Phillips, J. P., & Giancola, P. R. (2008). Experimentally induced anxiety attenuates alcohol-related aggression in men. *Experimental and Clinical Psychopharmacology, 16,* 43–56. doi:10.1037/1064-1297.16.1.43

Preacher, K. J., & Hayes, A. F. (2008). Asymptotic and resampling strategies for assessing and comparing indirect effects in multiple mediator models. *Behavior Research Methods, 40,* 879–891.

Proulx, T., & Heine, S. J. (2008). The case of the transmogrifying experimenter: Affirmation of a moral schema following implicit change detection. *Psychological Science, 19,* 1294–1300. doi:10.1111/j.1467-9280.2008.02238.x

Pyszczynski, T., Greenberg, J., Solomon, S., Arndt, J., & Schimel, J. (2004a). Converging toward an integrated theory of self-esteem: Reply to Crocker and Nuer (2004), Ryan and Deci (2004), and Leary (2004). *Psychological Bulletin, 130,* 483–488. doi:10.1037/0033-2909.130.3.483

Pyszczynski, T., Greenberg, J., Solomon, S., Arndt, J., & Schimel, J. (2004b). Why do people need self-esteem? A theoretical and empirical review. *Psychological Bulletin, 130,* 435–468. doi:10.1037/0033-2909.130.3.435

Pyszczynski, T., Greenberg, J., Solomon, S., & Maxfield, M. (2006). On the unique psychological import of the human awareness of mortality: Theme and variations. *Psychological Inquiry, 17,* 328–356. doi:10.1080/10478400701369542

Rosenberg, M. (1965). *Society and the adolescent self-image*. Princeton, NJ: Princeton University Press.

Rosenblatt, A., Greenberg, J., Solomon, S., Pyszczynski, T., & Lyon, D. (1989). Evidence for terror management theory: I. The effects of mortality salience on reactions to those who violate or uphold cultural values. *Journal of Personality and Social Psychology, 57,* 681–690. doi:10.1037/0022-3514.57.4.681

Russell, J. A., & Carroll, J. M. (1999). On the bipolarity of positive and negative affect. *Psychological Bulletin, 125,* 3–30. doi:10.1037/0033-2909.125.1.3

Schimel, J., Simon, L., Greenberg, J., Pyszczynski, T., Solomon, S., Waxmonsky, J., & Arndt, J. (1999). Stereotypes and terror management: Evidence that mortality salience enhances stereotypic thinking and preferences. *Journal of Personality and Social Psychology, 77,* 905–926. doi:10.1037/0022-3514.77.5.905

Schmeichel, B. J., Gailliot, M. T., Filardo, E., McGregor, I., Gitter, S., & Baumeister, R. F. (2009). Terror management theory and self-esteem revisited: The roles of implicit and explicit self-esteem in mortality salience effects. *Journal of Personality and Social Psychology, 96,* 1077–1087. doi:10.1037/a0015091

Schwarz, N. (1990). Feelings as information: Informational and motivational functions of affective states. In R. M. Sorrentino & E. T. Higgins (Eds.), *Handbook of motivation and cognition: Foundations of social behavior* (Vol. 2, pp. 527–561). New York, NY: Guilford Press.

Schwarz, N. (2012). Feelings-as-information theory. In P. M. Van Lange, A. W. Kruglanski, & E. Higgins (Eds.), *Handbook of theories of social psychology* (Vol. 1, pp. 289–308). Thousand Oaks, CA: Sage.

Schwarz, N., & Bless, B. (1991). Happy and mindless, but sad and smart? The impact of affective states on analytic reasoning. In J. Forgas (Ed.), *Emotion and social judgment* (pp. 55–71). Oxford, England: Pergamon Press.

Schwarz, N., & Clore, G. L. (1983). Mood, misattribution, and judgments of well-being: Informative and directive functions of affective states. *Journal of Personality and Social Psychology, 45,* 513–523. doi:10.1037/0022-3514.45.3.513

Schwarz, N., & Clore, G. L. (2003). Mood as information: 20 years later. *Psychological Inquiry, 14,* 296–303.

Schwarz, N., & Clore, G. L. (2007). Feelings and phenomenal experiences. In A. W. Kruglanski & E. Higgins (Eds.), *Social psychology: Handbook of basic principles* (2nd ed., pp. 385–407). New York, NY: Guilford Press.

Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. Boston, MA: Houghton Mifflin.

Simon, L., Greenberg, J., Harmon-Jones, E., Solomon, S., Pyszczynski, T., Arndt, J., & Abend, T. (1997). Cognitive-experiential self-theory and terror management theory: Evidence that terror management occurs in the experiential system. *Journal of Personality and Social Psychology, 72,* 1132–1146. doi:10.1037/0022-3514.72.5.1132

Smith, E. R., & Mackie, D. M. (2005). Aggression, hatred, and other

emotions. In J. F. Dovidio, P. Glick, & L. A. Rudman (Eds.), *On the nature of prejudice: Fifty years after Allport* (pp. 361–376). doi:10.1002/9780470773963.ch22

Spielberger, C. D., Gorssuch, R. L., Lushene, P. E., Vagg, P. R., & Jacobs, G. A. (1983). *Manual for the State–Trait Anxiety Inventory*. Palo Alto, CA: Consulting Psychologists Press.

Strachan, E., Schimel, J., Arndt, J., Williams, T., Solomon, S., Pyszczynski, T., & Greenberg, J. (2007). Terror mismanagement: Evidence that mortality salience exacerbates phobic and compulsive behaviors. *Personality and Social Psychology Bulletin, 33,* 1137–1151. doi:10.1177/0146167207303018

Tajfel, H., & Turner, J. C. (1986). The social identity theory of intergroup behavior. In S. Worchel & L. W. Austin (Eds.), *Psychology of intergroup relations* (pp. 7–24). Chicago, IL: Nelson-Hall.

Taylor, J. A. (1953). A personality scale of manifest anxiety. *Journal of Abnormal and Social Psychology, 48,* 285–290. doi:10.1037/h0056264

Tritt, S. M., Inzlicht, M., & Harmon-Jones, E. (2012). Toward a biological understanding of mortality salience (and other threat compensation processes). *Social Cognition, 30,* 715–733. doi:10.1521/soco.2012.30.6.715

van den Bos, K. (2001). Reactions to perceived fairness: The impact of mortality salience and self-esteem on ratings of negative affect. *Social Justice Research, 14,* 1–23. doi:10.1023/A:1012501506803

van den Bos, K., & Miedema, J. (2000). Toward understanding why fairness matters: The influence of mortality salience on reactions to procedural fairness. *Journal of Personality and Social Psychology, 79,* 355–366. doi:10.1037/0022-3514.79.3.355

van den Bos, K., Poortvliet, P., Maas, M., Miedema, J., & van den Ham, E. (2005). An enquiry concerning the principles of cultural norms and values: The impact of uncertainty and mortality salience on reactions to violations and bolstering of cultural worldviews. *Journal of Experimental Social Psychology, 41,* 91–113. doi:10.1016/j.jesp.2004.06.001

Vess, M., Arndt, J., Cox, C. R., Routledge, C., & Goldenberg, J. L. (2009). Exploring the existential function of religion: The effect of religious fundamentalism and mortality salience on faith-based medical refusals. *Journal of Personality and Social Psychology, 97,* 334–350. doi:10.1037/a0015545

Watson, D. (1988). Intraindividual and interindividual analyses of positive and negative affect: Their relation to health complaints, perceived stress, and daily activities. *Journal of Personality and Social Psychology, 54,* 1020–1030. doi:10.1037/0022-3514.54.6.1020

Watson, D., & Clark, L. A. (1994). *The PANAS-X: Manual for the Positive and Negative Affect Schedule–Expanded Form*. Ames, IA: University of Iowa.

Watson, D., & Clark, L. A. (1997). Measurement and mismeasurement of mood: Recurrent and emergent issues. *Journal of Personality Assessment, 68,* 267–296. doi:10.1207/s15327752jpa6802_4

Watson, D., Clark, L. A., & Tellegen, A. (1988). Development and validation of brief measures of positive and negative affect: The PANAS scales. *Journal of Personality and Social Psychology, 54,* 1063–1070. doi:10.1037/0022-3514.54.6.1063

Wicklund, R. A. (1997). Terror management accounts of other theories: Questions for the cultural worldview concept. *Psychological Inquiry, 8,* 54–58. doi:10.1207/s15327965pli0801_12

Wisman, A., & Goldenberg, J. L. (2005). From the grave to the cradle: Evidence that mortality salience engenders a desire for offspring. *Journal of Personality and Social Psychology, 89,* 46–61. doi:10.1037/0022-3514.89.1.46

Wisman, A., & Koole, S. L. (2003). Hiding in the crowd: Can mortality salience promote affiliation with others who oppose one's worldviews? *Journal of Personality and Social Psychology, 84,* 511–526. doi:10.1037/0022-3514.84.3.511

Yen, C., & Cheng, C. (2010). Terror management among Taiwanese: Worldview defence or resigning to fate? *Asian Journal of Social Psychology, 13,* 185–194. doi:10.1111/j.1467-839X.2010.01328.x

Zeidner, M., & Matthews, G. (2011). *Anxiety 101*. New York, NY: Springer Publishing Company.

Ziegler, R., & Burger, A. M. (2011). Mood and the impact of individuating information on the evaluation of ingroup and outgroup members: The role of mood-based expectancies. *Journal of Experimental Social Psychology, 47,* 1000–1006. doi:10.1016/j.jesp.2011.04.002

Zuckerman, M., & Lubin, B. (1965). *Manual for the Multiple Affect Adjective Checklist*. San Diego, CA: Educational and Industrial Testing Service.

Appendix

Studies That Met the First Two—But Not the Third—Criteria (N = 39)

Investigation	Measure	Design	Analytic approach	Brief summary of mood effects
Dechesne, Greenberg, Arndt, & Schimel (2000, Exp. 1)	MAACL	MS vs. TV	ANOVAs on subscales	Null effects
Greenberg, Simon, et al. (1995, Exp. 1)	MAACL	MS vs. TV	ANOVAs on subscales	Null effects
Janssen, Dechesne, & van Knippenberg (1999)	MAACL	MS vs. TV	ANOVAs on subscales	Null effects
Rosenblatt, Greenberg, Solomon, Pyszczynski, & Lyon (1989, Exp. 1)	MAACL	MS vs. absence of MS	ANOVAs on subscales	Null effects
Rosenblatt et al. (1989, Exp. 2)	MAACL	MS vs. absence of MS	ANOVAs on subscales	Null effects
Rosenblatt et al. (1989, Exp. 4)	MAACL	MS vs. absence of MS	ANOVAs on subscales	PA higher in MS condition
Greenberg, Simon, et al. (1995, Exp. 2)	PANAS	MS vs. TV vs. exam	ANOVAs on PA and NA	NA higher in exam condition
Greenberg, Porteus, Simon, & Pyszczynski (1995)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Greenberg, Pyszczynski, Solomon, Simon, & Breus (1994, Exp. 1)	PANAS	Four MS variations vs. TV	ANOVAs on PA and NA	Null effects
Greenberg et al. (1994, Exp. 2)	PANAS	Four MS variations vs. TV	ANOVAs on PA and NA	Null effects
Halloran & Kashima (2004, Exp. 1)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Hohman & Hogg (2011, Exp. 1)	PANAS	MS vs. TV vs. DP	ANOVAs on PA and NA	Null effects
McGregor, Zanna, Holmes, & Spencer (2001, Exp. 4)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Proulx & Heine (2008, Exp. 1a)	PANAS	MS vs. entertainment	ANOVAs on PA and NA	NA higher in MS condition
Proulx & Heine (2008, Exp. 1b)	PANAS	MS vs. entertainment	ANOVAs on PA and NA	Null effects
Strachan et al. (2007, Exp. 1)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Strachan et al. (2007, Exp. 2)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
van den Bos & Miedema (2000, Exp. 1)	PANAS	MS vs. no MS	ANOVAs on PA and NA	NA higher in MS condition
van den Bos (2001, Exp. 1)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
van den Bos, Poortvliet, Maas, Miedema, & van den Ham (2005, Exp. 2)	PANAS	MS vs. uncertainty vs. TV	ANOVAs on PA and NA	Null effects
van den Bos et al. (2005, Exp. 5)	PANAS	MS vs. uncertainty vs. TV	ANOVAs on PA and NA	Null effects
Wisman & Koole (2003, Exp. 1)	PANAS	MS vs. TV	Omnibus MANOVA on PA/NA	Null effects
Wisman & Koole (2003, Exp. 2)	PANAS	MS vs. TV	Omnibus MANOVA on PA/NA	Null effects
Wisman & Koole (2003, Exp. 3)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Wisman & Goldenberg (2005, Exp. 1)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
Wisman & Goldenberg (2005, Exp. 4)	PANAS	MS vs. TV	ANOVAs on PA and NA	Null effects
F. Cohen, Ogilvie, Solomon, Greenberg, & Pyszczynski (2005)	PANAS-X	MS vs. TV	ANOVAs on subscales	Null effects
Greenberg et al. (1994, Exp. 3)	PANAS-X	Four MS variations vs. TV	ANOVAs on subscales	Null effects
Greenberg et al. (1994, Exp. 4)	PANAS-X	Two MS variations vs. TV	ANOVAs on subscales	Null effects
Greenberg, Arndt, Simon, Pyszczynksi, & Solomon (2000)	PANAS-X	MS vs. TV	Omnibus MANOVA only	Null effects
Harmon-Jones et al. (1997, Exp. 1)	PANAS-X	MS vs. TV	Omnibus MANOVA, ANOVAs on subscales	Null effects on MANOVA, but higher fear in MS condition
Harmon-Jones et al. (1997, Exp. 2)	PANAS-X	MS vs. TV	Omnibus MANOVA, ANOVAs on subscales	No significant effects
Harmon-Jones et al. (1997, Exp. 3)	PANAS-X	MS vs. TV	Omnibus MANOVA, ANOVAs on subscales	Null effects on omnibus MANOVA, but higher levels of NA and fear in MS condition
Lieberman (1999, Exp. 1)	PANAS-X	MS vs. TV	Omnibus MANOVA only	Null effects
Lieberman (1999, Exp. 2)	PANAS-X	MS vs. TV	Omnibus MANOVA only	Null effects

(*Appendix continues*)

Appendix (*continued*)

Investigation	Measure	Design	Analytic approach	Brief summary of mood effects
Landau et al. (2006, Exp. 4)	PANAS-X	MS vs. uncertainty salience vs. shelving books	ANOVAs on subscales	Null effects
Landau, Greenberg, Sullivan, Routledge, & Arndt (2009, Exp. 5)	PANAS-X	MS vs. job worries vs. shelving books	ANOVAs on subscales	Null effects
Schimel et al. (1999, Exp. 1)	PANAS-X	Standard MS vs. TV	ANOVAs on subscales	Significantly higher levels of PA/happiness in MS condition
McGregor et al. (2001, Exp. 3)	Uncertainty	MS vs. temporary discontinuity vs. TV	ANOVA	Significantly higher levels of uncertainty in MS condition

Note. Exp. = experiment; ANOVA = analysis of variance; DP = dental pain; MAACL = Multiple Affective Adjective Checklist; MANOVA = multivariate analysis of variance; MS = mortality salience; NA = negative affect; PA = positive affect; PANAS = Positive and Negative Affect Schedule; PANAS-X = Positive and Negative Affect Schedule–Expanded Form; TV = television watching (control).

Received June 17, 2013
Revision received February 19, 2014
Accepted February 19, 2014 ■