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The 2013 Boston Marathon Attacks: An Experimental Investigation of Worldview Change Following Film Re-Exposure to Mass Trauma

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The 2013 Boston Marathon Attacks: An Experimental Investigation of Worldview Change Following Film Re-Exposure to Mass Trauma

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Abstract

Models of meaning making and post-traumatic growth contend that traumatic events are catalysts for significant and long-lasting change in a person's *worldview*—that is, their beliefs about social, metaphysical, theological, and political reality. Nearly all tests of this contention have employed retrospective and cross-sectional methods. However, the limitations inherent in these designs preclude causal conclusions. The current study represents the first longitudinal, laboratory-based experimental examination of belief change following trauma exposure using the *trauma film paradigm*. Findings reveal that the trauma film paradigm can successfully induce posttraumatic affective responses with live news coverage of a mass trauma (i.e., the 2013 Boston Marathon bombing), and that these affective responses are associated with retrospective accounts of having reexamined core beliefs. However, neither experimental condition effected significant directly observable change in beliefs. We consider our findings through the lens of alternative theoretical accounts of posttraumatic response (i.e., meaning making theory, narrative theory, and terror management theory). Finally, we conclude by recommending that researchers employ the trauma film paradigm in future research on meaning making and posttraumatic change processes.

Keywords:

Mass trauma, meaning making, posttraumatic growth, narrative theory, Boston Marathon bombing

The 2013 Boston Marathon Attacks: An Experimental Investigation of Worldview Change Following Reminders of Mass Trauma

Traumatic events bring people face-to-face with the finitude of their humanity. Suddenly and unexpectedly, a person exposed to a traumatic event suffers the actual or threatened loss of life, loved ones, property, safety, or security. Cherished goals are derailed, life course trajectories are permanently altered, and a person's understanding of the self, the world, and others is turned upside-down. A critical re-appraisal of one's worldview ensues. Beliefs and worldviews that were taken for granted—the worthiness of the self, the goodness of others, the safety of the world, the nature of the divine, and the fairness of society—are suddenly violated. The safety, predictability, and meaningfulness of life are undermined, casting the trauma survivor into a world of doubt, fear, and uncertainty. The individual must then reconcile the troubling significance of the traumatic event with a set of beliefs that is cannot explain or account for it. For some, the pieces can never be put back together, and the trauma ignites a lifelong struggle with posttraumatic stress, anxiety, depression, and substance abuse. For others, however, a phoenix rises from the ashes: From the darkness of loss and suffering, a wiser and stronger person emerges, emboldened by the struggle and empowered by the lessons it offered.

This is the story of meaning making and posttraumatic growth that has been told in the psychological literature for three decades (Janoff-Bulman, 1992; Park, 2010; Park & Folkman, 1997; Taylor, 1983; Tedeschi & Calhoun, 2004). It is a powerful and alluring narrative of change, empowerment, and resiliency. Just as these psychological models describe how victims find meaning after trauma, psychologists have themselves found meaningful work in subjecting this narrative to the tests of empirical research: The stories of suffering, overcoming, and redemption that run deep within the American psyche now fill the pages of the scholarly

literature (McAdams, 2006). As a result, posttraumatic growth and meaning making have become universal models of deliverance from the suffering and hardship of traumatic experience—discovered, tested, and proven by psychological science.

Or are they? Given that meaning making and posttraumatic growth lie at the nexus of a blossoming interest in positive psychology, American religious culture, and an abiding American belief in the capacity for the individual to overcome adversity, it is unsurprising that it has received significant interest from researchers in the United States (McAdams, 2006; Seligman & Csikszentmihalyi, 2000). The lived experiences of many trauma survivors, such as those that have been documented in qualitative research on coping and meaning making, underscore the importance and validity of the construct (e.g., Mattis, 2002; Morris, Shakespeare-Finch, & Scott, 2012; Tedeschi & Calhoun, 2004). On this basis, the "reality" of the process of meaning making or the phenomenon of posttraumatic growth would be hard to dispute. Nevertheless, much remains to be discovered concerning the mechanisms that drive the meaning making process and the experience of posttraumatic change. Additionally, concerns about the methods that have been used to create the current body of knowledge raise important questions about the validity of these widely embraced models (Frazier et al., 2009; Park, 2010).

The current study aims to advance an understanding of the cognitive sequelae of trauma through an experimental investigation of the ways in which exposure to a film of mass trauma (the 2013 Boston Marathon attacks). Theoretical work in trauma research provides many perspectives from which to generate hypotheses concerning the impact of highly stressful events on worldview change. Despite the great interest in and development of theoretical perspectives on meaning making and posttraumatic change, the methodological challenges affecting trauma research have compromised researchers' capacity to rigorously test the hypotheses generated by

theory (Park, 2010). We begin by reviewing theories that incorporate hypotheses related to worldview change and trauma, including shattered assumptions theory (Janoff-Bulman, 1992), the posttraumatic growth model (Tedeschi & Calhoun, 2004), the meaning making model (Park, 2010; Park & Folkman, 1997), and terror management theory (Arndt & Vess, 2008). We then summarize empirical research that has been undertaken to test these theories and models, drawing particular attention to the limitations of study designs that predominate in the meaning making literature. Finally, we introduce the film trauma paradigm as an innovative methodological approach that has yet to be employed in the study of meaning making and worldview change following exposure to trauma.

Theoretical Approaches to Worldview Change following Trauma

Cognitive approaches to posttraumatic change find their contemporary origin in the work of Taylor (1983), Lazarus and Folkman (1984), and Janoff-Bulman (1992). The work of Taylor (1983) and Lazarus and Folkman (1984) drew attention to the critical role that appraisal plays in responses to trauma. These scholars recognized that events are traumas by virtue of their appraisal as such, and that there is no essential property of traumatic events that distinguishes them from non-traumatic events: What constitutes a trauma for one person might not constitute a trauma for another. Janoff-Bulman's (1992) shattered assumptions theory, building on the work of Taylor (1983), recognized that illusory beliefs about the self and the world play a critical role in maintaining psychological well-being, and that trauma violates this protective worldview. Based on her research with victims of sexual assault, Janoff-Bulman (1989; 1992) identified the three assumptions most critical to well-being as the worthiness of the self, the benevolence of the world, and the meaningfulness of the world. Factor analytical research has not supported the psychometric integrity of this three-factor model, instead suggesting that world assumptions that

are affected by trauma might be more plentiful than shattered assumptions theory initially postulated (Elklit, Shevlin, Solomon, & Dekel, 2007; Kaler et al., 2008). Nevertheless, the essential theoretical underpinnings of the model—that traumatic events shatter deeply-held beliefs about the goodness and safety of the world—have remained enormously influential on later theoretical work.

Perhaps the most underdeveloped aspect of shattered assumptions theory pertains to the rebuilding of the assumptive world following trauma. While Janoff-Bulman (1992) argues that recovery from trauma requires that the assumptive world be reconstructed, the mechanisms underlying how these processes are supposed to occur, and how or whether the pretraumatic assumptive world differs from the posttraumatic assumptive world, remain vague.

Tedeschi and Calhoun's (2004) posttraumatic growth model attempts to fill this theoretical gap. The posttraumatic growth model draws deeply from the wellspring of the distinctly American religio-cultural redemption narrative in recognizing that many individuals who experience trauma report leading more enlightened, meaningful, and satisfying lives as a result of having suffered from and struggled with the consequences of a traumatic event (McAdams, 2006; Tedeschi & Calhoun, 2004). Posttraumatic growth, like shattered assumptions theory, "implies an established set of schemas that are changed in the wake of trauma" (Tedeschi & Calhoun, 2004, p. 4). Calhoun and Tedeschi's (1998) model posits that posttraumatic growth is preceded by a seismic event that causes emotional distress, challenges fundamental belief and goal schemas (comparable to the world assumptions in shattered assumptions theory), and upends autobiographical narratives. Though initially followed by distress management and rumination, trauma victims engage in cognitive processing with the aid of social support from others and acquaintance with cultural models for posttraumatic coping and growth. Ultimately,

schemas and worldviews are altered and autobiographical narratives reconfigured to accommodate the significance of the traumatic event, which provide a newfound sense of meaning and wisdom.

Of course, not all change is positive, and some people might fail to successfully rebuild a worldview that adaptively accommodates the implications of the trauma. The meaning making model (Park, 2010; Park & Folkman, 1997), while allowing for the potential for posttraumatic growth as conceptualized by Tedeschi and Calhoun (2004), offers a descriptive framework for meaning making following trauma that draws indirectly from Festinger's (1957) work on cognitive dissonance to highlight how traumatic events thrust individuals into having to reconcile the seemingly irreconcilable. The meaning making model posits that an individual's global meaning is comprised of beliefs and goals that guide behavior and are tied to the individual's identity. Life events are appraised for their situational meaning, or the extent to which they impact an individual's beliefs, goals, and sense of purpose. For most events, global and situational meaning are congruent; the event is either aligned with the individual's beliefs and goals or is minimally impacted by them. Traumatic events, however, carry situational meanings that violate global meaning; core beliefs are challenged and progress towards life goals are delayed or terminated. In this context, meaning making occurs through repeated attempts to resolve the discrepancy between global and situational meaning (Park, 2010). This occurs through cognitive processing, wherein either situational meanings are reframed in order to assimilate them into pre-existing global meanings, or global meanings are altered to accommodate the situational meaning of the traumatic event. According to this model, the elimination of discrepancies in meaning results in adaptive adjustment to the event.

Shattered assumptions theory, the posttraumatic growth model, and the meaning making model find their theoretical origins in clinical trauma research. However, the concept of the worldview extends far beyond those routinely investigated by trauma researchers (see Koltko-Rivera, 2004). Perhaps the most influential theoretical paradigm to shape our contemporary understanding of worldviews has been terror management theory (Arndt & Vess, 2008; Greenberg, Solomon, & Pyszczynski, 1997). Terror management theory, which finds its conceptual roots in existential philosophy, psychodynamic theory, and evolutionary psychology, begins with the premise that humans have an awareness of their own mortality that causes fear of death and distressing feelings of existential anxiety. Individuals seek symbolic immortality to combat this terror through the defense and maintenance of robust self-esteem and a meaningful cultural worldview. Viewed through the lens of shattered assumptions theory, maintaining self-esteem and defending a cultural worldview have much in common with believing in an assumptive world where the self is worthy and the world is meaningful. Unlike shattered assumptions theory, however, terror management theory has primarily attended to the ways in which mortality salience impacts changes in beliefs concerning in-group solidarity, political and cultural identity, and intergroup relationships (Arndt & Vess, 2008; Greenberg et al., 1995; Pyszczynski et al., 2006).

Increasingly, scholars have ventured to bridge the gap between meaning making research and terror management theory. Edmondson and colleagues (2011) made the connection between the two more concrete by drawing attention to the anxiety buffering role that worldviews have in warding off the fear of death and the relationship that the loss of such defenses have to PTSD symptomatology. Traumas of a social or political nature (e.g., traumas caused by criminal acts, combat, or terrorism) might effect changes in beliefs that are frequently studied in terror

management research but that receive less attention in the meaning making literature, such as those related to in-group identification. For instance, Iranian students given a mortality salience prime were more likely to support religious martyrdom compared to controls, whereas American students given a mortality salience prime became more supportive of American military as compared to controls (Pyszczynski et al., 2006). Likewise, mortality salience primes have been shown to lead individuals to stereotype out-group members and humanize in-group members (Schimel et al., 1999; Vaes, Heflick, & Goldenberg, 2010). While researchers have sought to explore how worldviews change following exposure to war or terrorist attacks (e.g., Butler et al., 2005; Freh, Chung, & Dallos, 2013), these studies have not assessed worldviews central to self-construal that might be affected by traumatic confrontations with death, such as cultural identification, that have been commonly studied in the terror management literature. The incorporation of measures that assess cultural identity in the study of posttraumatic worldview change would allow for making further theoretical ties between the clinical and experimental meaning making and worldview literatures.

Methodological Limitations in Research on Worldviews after Trauma

As reviewed, the literature offers a cornucopia of theoretical perspectives on worldview change following trauma. While empirical work has endeavored to test hypotheses drawn from these rich theoretical frameworks, the methodologies employed to do so have come under increased scrutiny in recent years. Frazier and colleagues (2009) highlight that scholars investigating meaning making and posttraumatic growth have primarily based their conclusions regarding the frequency of posttraumatic growth, the impact of posttraumatic growth on well-being, and the mechanisms underlying meaning making on findings from retrospective self-report studies, wherein participants are asked to undertake the mental gymnastics of recalling

aspects of the pre-trauma self, recalling aspects of the post-trauma self, comparing the two, determining whether or not growth occurred, and then assessing the extent to which this growth was attributable to the traumatic event. Given the (disputed) impact of trauma on autobiographical recall (Berntsen & Rubin, 2014) and the propensity for some individuals to derogate their past selves following trauma (McFarland & Alvaro, 2000), retrospective approaches to the study of posttraumatic change have come under increased scrutiny (Frazier et al., 2009; Kaler et al., 2008). Given the breadth of studies that have employed this methodology—indeed, such research comprises the near-totality of empirical work on shattered assumptions theory through the 1990s and early 2000s—a full review of this literature is well beyond the scope of this paper. In summarizing her review of the meaning making literature, Park (2010) notes that the dearth of prospective designs and the reliance on retrospective self-report constitute a significant limitation to the advancement of a science of meaning making and posttraumatic change.

As a result of these critiques, the trauma literature has seen an increase in published studies utilizing prospective and longitudinal designs to investigate meaning making and posttraumatic change. While this change is welcome, methodological limitations remain. Prospective designs can be costly: Given that only a minority of participants recruited in such a study will experience a trauma during the course of the study, significant effort and resources are expended recruiting participants who will not experience a trauma during the course of the study. While this can be counteracted by extending the duration of a prospective longitudinal study, this too adds burdensome costs to study design, and attrition becomes more problematic. For instance, Anders, Frazier, and Shallcross (2014), Frazier et al. (2009), Kaler et al. (2008), and Schuler and Boals (2015) each used an eight-week-long prospective longitudinal design to assess

how traumatic events experienced between baseline data collection and final data collection impacted changes to core worldview beliefs and relevant indicators of well-being: While their definitions of trauma varied slightly, these studies found, respectively, that 8.5% (Kaler et al., 2008), 14.1% (Anders, Frazier, & Shalcross, 2014), 21.0% (Frazier et al., 2009), and 19.1% (Schuler & Boals, 2015) of non-attrited participants reported experiencing a significant trauma during the course of the study. Barring the rare occurrence of a mass trauma taking place in the midst of data collection (e.g., as in the case of the September 11th, 2001, and research conducted by Silver et al., 2002), researchers can expect that these costly study designs will yield small samples of individuals who have experienced a clinically significant trauma (Park, 2010).

Even researchers who have the resources to execute a well-designed prospective longitudinal design are limited in their ability to draw generalizable conclusions from their work. While the aforementioned longitudinal studies note many important limitations to their findings, such as the reliance on undergraduate samples and their self-reports of trauma, it is surprising that so little mention is made of the heterogeneity among reported traumas experienced as a limitation (Anders, Frazier, & Shallcross, 2014; Frazier et al., 2009; Kaler et al., 2008; Schuler & Boals, 2015). The possibility that different traumas effect different changes in different world assumptions due to the contexts under which the traumas occurred, and the implications for those traumas on the individuals involved, requires that traumatic events of a particular type be studied independently. Consider two hypothetical trauma victims: The first is a victim of sexual assault, and the second is an individual who recently lost a loved one to sudden illness. Can we compare worldview changes between these two individuals and justifiably claim that we are comparing apples to apples? The notion implicit in this research that the specific content of a traumatic event does not bear upon the worldviews affected by the event is neither argued by theory nor

supported by the data, but is rather a byproduct of researchers' efforts to study events that are, by their nature, heterogeneous, unpredictable, and sufficiently rare.

Finally, reliance on cross-sectional designs has prevented researchers from exploring the persistence of alterations to worldviews. This point is not trivial: If trauma does, in fact, change an individual's worldviews, these changes might reflect substantive and permanent changes in personality; alternatively, they might be short-lived peritraumatic reactions that subside with time. Consider, for instance, that individuals who did not meet diagnostic criteria for PTSD one year after they had received a PTSD diagnosis that resulted from a traumatic injury had worse psychological, physical, social, and environmental quality of life than did individuals who never received a PTSD diagnosis (Bryant et al., 2015). If the residua of trauma include poor quality of life and a shattering of worldviews and global meaning, one wonders whether the reconstructed assumptive world that results from posttraumatic cognitive processing contributes to poorer quality of life and, if so, how. Yet, in the absence of longitudinal research that tracks worldviews well after a trauma has occurred, such questions will remain unanswered.

The Trauma Film Paradigm: A Laboratory-Based Approach to Trauma Research

The limitations inherent in correlational and retrospective study designs can be overcome by implementing prospective and longitudinal designs in trauma research, but even these study designs do not afford the control that experimental designs can offer. Whereas trauma research in the fields of meaning making and belief development have emphasized prospective and longitudinal designs (e.g., Frazier et al., 2009; Park, 2010), researchers interested in the clinical, biological, and neurological sequelae of trauma have increasingly studied these phenomena in the laboratory through the use of the trauma film paradigm (Holmes & Bourne, 2008). The trauma film paradigm is an experimental analogue study design that uses videos with pseudo-

traumatic content to induce peritraumatic responses among study participants. While such a design forfeits the ecological validity of a self-report study, it offers researchers the ability to standardize a "traumatic" event and determine when, where, and how it takes place. Pre-film measures are administered to collect prospective data and these measures are re-administered post-film to assess how target outcomes were affected by the film. After the film, longitudinal data can be collected to examine the long-term impact of the manipulation.

The trauma film literature has reliably demonstrated the ability of the technique to induce peritraumatic responses such as negative mood, distress, intrusive thoughts, and state dissociation (Holmes & Bourne, 2008). Researchers have made use of Steil's (1996) 12min 30s compilation of five road accidents to explore the impact of viewing this potentially traumatic video on intrusive thoughts, emotional reactivity, state dissociation, heart rate, and cortisol (Chou, La Marca, Steptoe, & Brewin, 2014; Clark, Mackay, & Holmes, 2015; Holmes, Brewin, & Hennessey, 2004). Cinematic footage has also been used in trauma film paradigm research (e.g., the rape scene from the 1988 film *The Accused*; Salters-Pedneault et al., 2009). Nevertheless, recent research employing the trauma film paradigm has relied primary on Steil's (1996) road traffic accident footage in the context of research on intrusive flashbacks. While the benefit of this approach allows scholars working within this field of research the ability to draw direct statistical comparisons across studies utilizing identical footage, the potential for this literature to become over-reliant on a single quasi-traumatic stimulus runs the risk of narrowing the generalizability of the literature's findings to traumatic events that do not involve road traffic accidents. The rapid film presentation of five unrelated traumatic events (i.e., as in Steil, 1996) further lacks the ecological validity of a single traumatic experience.

Moreover, Holmes and Bourne's (2008) review of the use of the trauma film paradigm concentrates on the use of the method to test hypotheses derived from cognitive processing theories of PTSD that seek to describe the causal pathways and neural substrates that link visuospatial and verbal processing to PTSD-related symptomatology. It is noteworthy that they make no mention of implementing the technique to study trauma reactions of a social or political nature, even in brainstorming possible future applications of the trauma film paradigm (Holmes & Bourne, 2008). While Brewin, Bourne, Holmes, and their colleagues have not yet applied the trauma film paradigm to the study of meaning-making and posttraumatic change, few scholars working in these latter fields of research have given much credence to the experimental study of traumatic meaning making. Park (2010), in discussing the validity of shattered assumptions theory, states that the most methodologically robust way to empirical test its hypotheses would "ideally… occur in a randomized experimental context that would control for alternate explanations; such studies will almost certainly not be conducted" (p. 283). The trauma film paradigm, despite being an analog design, offers a framework for simulating such a study in an ethical fashion. Moreover, the application of the trauma film paradigm to meaning making and posttraumatic change research offers the opportunity to bridge the gap between traumatologists investigating cognitive processing of trauma and those who are studying meaning making and posttraumatic change processes.

The Impact of Indirect and Televised Mass Trauma

Films such as those typically used in the trauma film literature (e.g., Steil, 1996) or cinematic representations of traumatic events (e.g., *The Accused*) can be used to expose individuals to traumatic content independent of an ecologically valid context (Holmes & Bourne, 2008), but exposure to trauma vis-à-vis a computer or television screen can, and often is, a very

powerful and impactful experience. The advent of 24-hour cable news cycle brought horrifying images from wars, natural disasters, and terrorist attacks into the living rooms of millions of Americans. The majority of cable news viewers prove resilient to such images; however, a significant minority experience symptoms of PTSD as a result of indirect traumatic exposure through televised news broadcast (Neria & Sullivan, 2011). Research by Silver and colleagues (2004) has demonstrated that a person need not have had direct exposure to a mass trauma to suffer a harmful traumatic response: Following the 9/11 attacks, posttraumatic stress responses were experienced by individuals across the U.S. who were exposed to the event via media coverage. Galea and Resnick's (2005) review of post-9/11 research on New York City residents found that while individuals directly affected by the 9/11 terrorist attacks were more likely to experience symptoms of PTSD as compared to those who were not directly affected, the overall PTSD symptom presentation between those who were directly affect and those who were indirectly affected was negligible. Repeated exposure to news coverage of terrorist attacks and war have been found to sustain accurate recall of these events (Hirst et al., 2015) and exacerbate posttraumatic stress associated with indirect trauma exposure (Silver et al., 2013).

Mass traumas differ from personal traumas in important ways. For those who are not directly affected, the harms and losses associated with mass traumas are often symbolic in nature and their experiences of them are affected by the damage rendered to communities, the violation or destruction of cherished landmarks and sacred spaces, and the ways in which these events are processed through and reflected by collective histories and cultural practices (Maček, 2014). Research suggests that meaning making following mass trauma shares many essential similarities with meaning making following personal trauma: Individuals who were searching for meaning in the 9/11 attacks were found to have higher levels of posttraumatic stress as compared

to individuals who had successfully found meaning in the event (Updegraff, Silver, & Holman, 2008)—a finding that echoes research on personal traumas (Steger, Kashdan, Sullivan, & Lorentz, 2008). However, identification with a group affected by a mass trauma can play a significant role in how an individual responds to the event. A study of Spaniards' emotional responses to the March 11th, 2004 Madrid train bombings found that respondents' strongest emotions following the attacks were sadness, disgust, anger, and contempt, and that these emotions were significantly and positively correlated with Spanish cultural identification (Conejero & Extebarria, 2007). Morgan, Wisneski, and Skitka (2011) drew on value protection theory (Tetlock et al., 2000) and terror management theory (Arndt & Vess, 2008) to account for the role that cultural identification and worldview defense played in the negative (e.g., diminished civil liberties, discrimination towards Muslims, and war) and positive (e.g., blood donation, volunteerism, and political engagement) responses of the American public following the September 11th attacks. In light of the personal and collective impact that mass traumas have, the study of meaning making and posttraumatic change requires attention to changes in schemas that reflect personal worldviews (e.g., assumptive worldviews and a sense of meaning in life; Janoff-Bulman, 1992; Steger, Frazier, Oishi, & Kahler, 2006) alongside those that reflect sociocultural and political belief systems (e.g., religious beliefs and cultural identity; Leach et al., 2008; Park, 2005).

The Current Study

Events such as the September 11th attacks and similar violent mass traumas that have followed—the Virginia Tech massacre, the Sandy Hook shootings, and the Boston Marathon bombings among them—are not merely traumatic in nature, but have significant ramifications for the mental health of the public and the social and political life of a society (Silver et al., 2004;

Maček, 2014; Morgan, Wisneski, & Skitka, 2011). Nevertheless, the putative impact that such events might have on individual-level belief systems remains understudied. Because past research has relied primarily on cross-sectional and retrospective designs, the long-term impact of trauma on belief change has been left to conjecture. Moreover, the emphasis on personal traumas among scholars of meaning making and posttraumatic change has left wanting a richer empirical analysis of the impact of mass trauma on these phenomena.

The current study employs the trauma film paradigm to experimentally, prospectively, and longitudinally investigate belief change and meaning making following a simulated exposure to mass trauma. The two research questions that guide the current investigation are:

1) Can recorded news footage of a mass trauma be employed in a trauma film paradigm study to simulate a trauma response?
2) Can exposure to a film recording of a mass trauma produce short- and long-term changes in an individual's beliefs, worldviews, or identity?

Based on meaning making theory and past findings from the literature, we offer the following hypotheses:

H1) Participants in the trauma film (i.e., experimental) condition will experience more negative affect and distress than will participants in the non-trauma film (i.e., control) condition immediately following film exposure.

H2) Participants in the trauma film (i.e., experimental) condition will exhibit greater changes in beliefs and worldviews immediately following film exposure as compared to participants in the non-trauma film (i.e., control) condition.

H3) Participants in the trauma film (i.e., experimental) condition will retrospectively report greater impact of the film on beliefs and worldviews compared to participants in the non-trauma film (i.e., control) condition.

The 2013 Boston Marathon attacks

To test these hypotheses, we used recorded news footage covering the 2013 Boston Marathon Bombings. On April 15th, 2013, two perpetrators detonated homemade explosive devices near the finish line of the 2013 Boston Marathon, killing three and injuring 264; these attacks constituted the first major terrorist attack on American soil since September 11th, 2001 (Massachusetts Emergency Management Agency, 2013). In the days following, police and federal investigators conducted a high profile manhunt that resulted in the death of one suspect and the apprehension of another (Massachusetts Emergency Management Agency, 2013). Echoing past research on the distal impact of national traumas (e.g., Silver et al., 2004), a study of reactions to the Boston Marathon bombings in the United States found that Bostonians experienced the greatest levels of acute stress in the days and weeks following the attacks, but that the intense media coverage transmitted the harmful effects of this event well beyond the Boston metropolitan area (Holman, Garfin, & Silver, 2014). Owing to the recent occurrence of these events, the public's widespread familiarity with them, and the significant impact they had on national stress, we chose to use footage from this event under the assumption that participants would be expected to experience a stress response as a result, and that the appraised meaning of this event would challenge assumptive worldviews and serve as a mortality salience prime.

Methods

Participants

Participants were recruited from the University of Connecticut's Psychology Department participant pool website during the Spring 2014 and Fall 2014 semesters. Due to the University of Connecticut's proximity to Boston, it was possible that a prospective study enrollee might have been directly affected by the 2013 Boston Marathon attacks. To protect these students from potentially harmful re-exposure to this traumatic material, all participant pool members were prescreened with a single *yes* or *no* question which asked, "Did you or a loved one suffer direct physical and/or emotional harm as a result of the Boston Marathon attacks of April 15th, 2013?" Participants who responded "yes" to this question were prohibited from enrolling in the study. Of the 2,796 students in the participant pool, 98 (3.5%) responded "yes" to this prescreen.

One hundred and sixty-eight (74.0%) of the 227 participants who enrolled in the study completed surveys at all three time points and were retained for data analysis. Of these 168 participants, women (*n*=136, 81.0%) outnumbered men (*n*=32, 19.0%). Participants had a mean age of 18.6 years (*SD*=0.99). One hundred and thirteen (67.3%) participants were White, 28 (16.7%) were Asian or Asian American, 12 (7.1%) were Black or African American, 7 (4.2%) were multiracial, and 8 (4.8%) did not specify their race. First year college students (*n*=106, 63.1%) comprised more than half of the sample. Ninety-four (56.0%) participants identified as Christian, among which 26 participants (27.7%) identified with a Protestant denomination, 46 participants (48.9%) identified as Roman Catholic, and the remaining 22 participants (23.4%) did not identify with a specific Christian denomination. Thirty-two (19.1%) participants identified with no religion, 17 (10.0%) identified their religious affiliation as "other," and the remaining 25 participants (14.9%) identified as either Jewish, Muslim, Hindu, or Buddhist.

Tasks and Measures

Trauma films. Two 10-min trauma films[1] were selected for administration in the experimental condition and the control condition. The film selected for the experimental condition consisted of live CNN coverage of the 2013 Boston Marathon attacks as the event was unfolding on April 15th, 2013, reported by CNN news correspondent Wolf Blitzer. The film contains approximately 2.5 min of unsettling live amateur video of the first explosion and the reactions of victims and first responders, followed by approximately 7.5 min of in-studio news analysis of the amateur video. The film selected for the control condition consisted of recorded CNN coverage, aired on June 6th, 2013, of a discussion between two national security experts about the defection of former CIA contractor Edward Snowden. Both films were trimmed from their original lengths to 10 min to minimize method variance and set film lengths to be comparable to those typically used in the trauma film literature (Holmes & Bourne, 2008). Films were selected to meet the desired criteria of being matched in terms of ecologically validity (i.e., real news footage) and style of presentation (i.e., CNN news broadcasts anchored by Wolf Blitzer). The films were also selected on the basis of their difference in content: Whereas the film in the experimental condition contained coverage and live images of a traumatic event of national scope, the control condition contained pre-recorded news analysis of a non-traumatic nature on a subject that we anticipated would be of comparatively minimal interest to participants. The films were displayed on a 43-cm Dell P170Sb monitor at a viewing distance of approximately 60cm. Participants listened to the videos with around-ear stereo headphones.

Attentional check. To assess the extent to which participants paid adequate attention to the films, we created a brief five-item true-or-false quiz to gauge retention of the film content.

[1] The films used in the experimental and control conditions were retrieved from YouTube.com and modified for study purposes. The modified films can be accessed at https://osf.io/aikbp/.

Separate quizzes were developed for each film. The items of these quizzes and their correct answers are presented in Appendix A.

Assumptive worldviews. Non-religious worldviews were assessed with the World Assumptions Scale (WAS; Janoff-Bulman, 1989). The WAS contains eight 4-item subscales that measure beliefs about self-worth (e.g., "I am very satisfied with the kind of person I am") luck (e.g., "I am luckier than most people"), randomness (e.g., "In general, life is mostly a gamble"), justice (e.g., "By and large, people get what they deserve in this world"), benevolence of the world (e.g., "If you look closely enough, you will see that the world is full of goodness"), benevolence of others (e.g., "People are basically kind and helpful"), controllability (e.g., "Through our action, we can prevent bad things from happening to us"), and self-control (e.g., "I take actions necessary to protect myself against misfortune"). Items were scored from 1 (*strongly disagree*) to 6 (*strongly agree*). Research has supported the eight-factor structure of the WAS (Elklit et al., 2007).

National identity. National identity was measured with the Multicomponent In-Group Identification Scale (Leach et al., 2008). This measure allows for modification to measure five forms of in-group identification with a target social group: solidarity ("I feel a bond with [in-group]"), satisfaction ("I think that [in-group] have a lot to be proud of"), centrality ("The fact that I am [in-group] is an important part of my identity"), self-stereotyping ("I am similar to the average [in-group] person"), and group homogeneity ("[in-group] people have a lot in common with each other"). To explore whether and how the experimental manipulation effected changes in worldviews related to cultural identity, we adapted this measure to assess changes in participants' identity as Americans (hereafter, the American Identity Scale, or AIS). The AIS was only administered to participants who were born in the United States, held American

citizenship, lived most of their lives in the United States, or personally identified as American (*n*=163, 97.0%). Items were scored from 1 (*strongly disagree*) to 7 (*strongly agree*).

Meaning in life. Subjective sense of meaning in life was assessed with the Meaning in Life Questionnaire (MLQ; Steger, Frazier, Oishi, & Kahler, 2006). The MLQ contains two 5-item subscales: search for meaning in life (e.g., "I am looking for something that makes my life feel meaningful"), and presence of meaning in life (e.g., "I have a good sense of what makes my life meaningful"). Items were scored from 1 (*absolutely untrue*) to 7 (*absolutely true*).

Affective response to film. Affective responses to the films was measured at Time 2 with five of the eleven three-item subscales drawn from the expanded version of the Positive and Negative Affect Scale (PANAS-X; Watson & Clark, 1992). Four of the subscales chosen—fear, hostility, sadness, and surprise—were chosen on the basis of their relevance to affective responses typically experienced in response to trauma (e.g., Orth & Wieland, 2006; Resick & Miller, 2009). We included self-assurance, a form of affect with no anticipated relationship to mass trauma, as the fifth subscale in order to check for affective response bias. Items were scored on a six-point scale, from 0 (*not at all*) to 5 (*extremely*).

Subjective distress. Subjective distress was measured with the Subjective Units of Distress Scale (SUDS; Wolpe, 1969), a single-item measure of subjective distress. Participants were presented with a slider, scored from 0 to 100, accompanied with the following prompt: "Imagine that you have a 'distress thermometer' that measures your fear, anxiety, or discomfort on a scale from 0 to 100. Using this scale, please indicate the level of distress you are currently experiencing." The SUDS was administered at Time 2 and Time 3.

Subjective appraisal of post-film response. Participants' subjective appraisals of their own reactions to the experimental manipulation were assessed with the Impact of Events Scale

(IES; Weiss & Marmar, 1997) and the Core Beliefs Inventory (CBI; Cann et al., 2010) one month following the film administration at Time 3. The IES contains 22 items that assess participants' stress-related physiological (e.g., "I had trouble staying asleep"), affective (e.g., "Any reminder brought back feelings about it"), and cognitive (e.g., "I tried not to think about it") responses to a specific traumatic event. The instructions provided to participants for responding to the IES and CBI were dependent on the condition to which participants were assigned, such that participants in the experimental condition were asked to what extent the Boston Marathon attacks impacted them and led them to reflect on their core beliefs, and participants in the control condition were asked to what extent the government surveillance controversy impacted them and led them to reflect on their core beliefs. Despite the scale's inclusion of post-traumatic responses across symptom types, Weiss and Marmar (1997) found that scale items best fit a single factor (see also Cann et al., 2010). Items were scored on a five-point scale from 0 (*not at all*) to 4 (*extremely*).

Whereas the IES assesses the subjective experience of posttraumatic symptoms, the CBI assesses subjective assessments of worldview reexamination following trauma. Example items include, "Because of the event, I seriously examined the degree to which I believe things that happen to people are fair" and, "Because of the event, I seriously examined my beliefs about the meaning of my life." The CBI contained nine items scored on a six-point scale from 0 (*not at all*) to 5 (*to a very great degree*).

Procedure

Following from past trauma film paradigm research (Holmes & Bourne, 2008), the current study employed a longitudinal experimental design. Data were collected at three time points: pre-film (Time 1), immediately post-film (Time 2), and four weeks post-film (Time 3). A

schedule of study procedures, measures, and time points of administration is presented in Table 1. Pre-film measures were administered online to assess participants' beliefs, worldviews, and sense of meaning in life at baseline. At Time 2, participants came to the laboratory and were randomly assigned to either the experimental (i.e., Boston Marathon bombing film) or control (i.e., national security interview film) condition. After viewing the film, participants completed all Time 2 measures to assess the impact of the manipulation on beliefs, worldviews, meaning in life, distress, and affect. Finally, to assess whether the experimental manipulation had long term impacts on key outcomes of interest, participants completed a final battery of measures at Time 3, which occurred one month following Time 2. We chose to administer Time 3 four weeks after Time 2 on the basis of the DSM-5's stipulation that posttraumatic symptomatology must be present for at least one month in order for an individual to meet diagnostic criteria for Posttraumatic Stress Disorder (American Psychiatric Association, 2013). Time 3 measures once again included our belief, worldview, and meaning measures in addition to our measures of distress and subjective post-event appraisal. The order of measure administration, and the order of items within each measure, were randomized for each participant to control for order effects.

To ensure that prospective participants did not choose to enroll in the study on account of its focus on trauma, mild deception was used in describing the purpose of the study. During consent, participants were informed that they were "invited to participate in a research study to understand how exposure to televised news media impacts beliefs and attitudes," and that "the video you will watch might contain news coverage of a disturbing or unsettling event." Participants were not told that the intent of the study was to examine the impact of mass trauma on belief change. To ensure that participants understood the potential for exposure to disturbing

material, participants were consented to the study at both Time 1 and Time 2. On completing the study at Time 3, participants were debriefed and informed of the use of deception.

All study procedures were approved by the University of Connecticut's Institutional Review Board. The rationale, hypotheses, and materials for this study were preregistered at Open Science Framework prior to data collection (https://osf.io/cm6pt/).

Results

Hypothesis 1: Participants in the Experimental Condition will Experience Higher Levels of Negative Affect and Distress

The utility of the trauma film paradigm rests on the assumption that a traumatic film produces a psychological response that is commensurate with an ecologically valid experience of a traumatic event. Past research has made repeated use of films (e.g., Steil, 1996) that meet this standard (Holmes & Bourne, 2008). Because the current study uses films that have yet to be validated, an analysis of findings must first establish the efficacy of the films employed. To do so, we ran independent samples t-tests comparing the differences in affective response on the PANAS-X and the SUDS following the film administration at Time 2. Means, standard deviations, and internal consistency coefficients are presented in Table 2. Findings revealed that participants in the experimental condition experienced significantly higher levels of fear ($t(166)=11.12$, $p\leq.001$, $d=1.72$), hostility ($t(166)=10.84$, $p\leq.001$, $d=1.67$), sadness ($t(166)=17.40$, $p\leq.001$, $d=2.69$), surprise ($t(166)=6.34$, $p\leq.001$, $d=0.98$), and subjective distress ($t(166)=7.81$, $p\leq.001$, $d=1.20$) as compared to participants in the control condition. As expected, self-assurance, a form of affect with no conceptual link to trauma exposure, did not differ between conditions, $t(166)=0.27$, $p=n.s.$, $d=0.04$.

To explore the possibility that differences in affective response to the videos was a byproduct of differences in attention, we compared the mean attentional check quiz scores of the two groups. This comparison revealed a significance difference, $t(166)=4.01$, $p\leq.001$, $d=0.62$, with participants in the control condition ($M=4.42$, $SD=0.75$) having better scores than participants in the experimental condition ($M=4.81$, $SD=0.45$). A further analysis suggested that these differences in scores might have been primarily driven by the inclusion of a poor item in the attention check quiz in the experimental condition (i.e., "President Obama described the event as a 'terrorist attack.'"), which was answer incorrectly by 29.4% of experimental condition participants. However, the observations that 70.2% ($n=115$) got all attentional items correct across conditions and that no participants answered fewer than three questions incorrectly suggest that participants paid adequate attention to and retained content from the films.

Hypothesis 2: Participants in the Experimental Condition will Exhibit Greater Changes in Beliefs and Worldviews

Mixed model ANOVAs. Having established the efficacy of the manipulation as measured by negative affect and self-reported distress, we then investigated whether participants experienced differences in belief change as a function of time and condition. To do so, we ran a series of 2 (condition) by 3 (time) ANOVAs on our 15 belief and worldview measures: The eight subscales of the WAS, the five subscales of the AIS, and the two subscales of the MLQ. Within our application of the ANOVA framework, a significant interaction between time and condition would indicate support for H2; specifically, we would anticipate that, due to random assignment, beliefs and worldviews would not differ between groups at T1 and—if H2 holds true—beliefs would then differ between groups following the experimental manipulation at T2.

Findings from these analyses are presented in Table 3. A significant main effect for time was found for beliefs about justice, $F(2, 498)=4.05$, $p\leq0.05$, $\eta_p^2=0.02$, and for beliefs in self-control, $F(2, 498)=3.35$, $p\leq0.05$, $\eta_p^2=0.01$. Post-hoc analysis of these differences using Tukey's Honest Significant Difference (HSD) test revealed that, for beliefs about justice, the significant main effect of time was driven exclusively by a -.24 change in the mean score for justice beliefs across conditions between T1 and T2 (90% CI [-0.47, -0.02], $p\leq0.05$). On further analysis, this change was found to be almost exclusively attributable to participants in the experimental condition. However, the difference in justice beliefs among this group between T1 and T2 narrowly failed to meet conventional standards of statistical significance (90% CI [-0.38, 0.02], $p=0.052$). Post-hoc investigation found that the significant main effect of time on self-control beliefs was driven by a -.30 change in the mean score for self-control beliefs across conditions between T1 and T3 (90% CI [-0.50, -0.09], $p\leq0.01$). We found no differences in this trend as a function of condition.

A significant main effect for condition was found for beliefs about the benevolence of the world, $F(1, 498)=4.98$, $p\leq0.05$, $\eta_p^2=0.01$, the centrality of American identity, $F(1, 498)=5.87$, $p\leq0.05$, $\eta_p^2=0.01$, satisfaction with American identity, $F(1, 498)=4.70$, $p\leq0.05$, $\eta_p^2=0.01$, and perceived solidarity among Americans, $F(1, 498)=4.82$, $p\leq0.05$, $\eta_p^2=0.01$. While post-hoc analysis estimated participants in the experimental condition to have a .14 greater mean score for the benevolence of the world as compared to participants in the control condition, this difference was not statistically significant (95% CI [-.01, .29], *p=ns*). However, participants in the experimental condition had reliability stronger American identification across time points as compared to the control condition with respect to the centrality of American identification ($M_D=.24$, 95% CI[.06, .43], $p\leq0.01$), satisfaction with American identity ($M_D=.19$, 95% CI[.08,

.30], $p \leq 0.001$), and perceptions of in-group solidarity among Americans (M_D=.20, 95% CI[.04, .35], $p \leq 0.05$).

No significant interactions were found between time and condition for any of the belief and worldview measures assessed, $Fs(2, 498) \leq 1.15$, ps=ns, $\eta_p^2 s \leq 0.01$.

Residual belief change by condition from Time 1 to Time 2. Whereas mixed-model ANOVAs assess the observed average differences in belief scores across time and condition, residual change score modeling assesses the extent to which the experimental manipulation accounts for a significant proportion of instability in belief over time (for further discussion on the differences between difference score and residual change score models, see Gollwitzer, Christ, & Lemmer, 2014). Stated otherwise, residual change score modeling allows for the analysis of the impact of the experiment on post-manipulation beliefs above and beyond the impact of pre-manipulation beliefs. Given the established variability and change in beliefs experienced among emerging adult attending college (e.g., Bryant, 2010; Gutierrez & Park, 2015), we would expect to observe changes in beliefs and worldviews among participants in our sample even in the absence of the experimental manipulation. Therefore, the analysis of residual change scores, rather than observed difference scores, allows for a more nuanced within-group assessment of the effects of the manipulation on belief change.

Residual change score regression models predicting our 15 belief outcomes (i.e., world assumptions, American identity, and meaning in life) for Time 2 are presented in Table 4. Models assessing residual belief change immediately following the experimental manipulation regressed Time 2 beliefs onto Time 1 beliefs, study condition, and an interaction term between Time 1 beliefs and study condition. Although residual change models regressing Time 2 world assumptions onto Time 1 world assumptions accounted for a significant proportion of variance in

Time 2 beliefs, $Fs(2, 165) \geq 29.37$, $p \leq .001$, $R^2s \geq 0.26$, the experimental condition did not significant predict residual belief change within these models.

Residual change models regressing Time 2 American identity beliefs onto Time 1 American identity beliefs accounted for a significant proportion of variance in Time 2 beliefs, $Fs(2, 160) \geq 56.00$, $p \leq .001$, $R^2s \geq 0.41$. Experimental condition did not significant predict residual belief change within these models.

Residual change models regressing Time 2 meaning in life attitudes onto Time 1 meaning in life attitudes accounted for a significant proportion of variance in Time 2 beliefs, $Fs(2, 165) \geq 50.23$, $p \leq .001$, $R^2s \geq 0.39$. Experimental condition did not significant predict residual belief change within these models.

Residual belief change by condition through Time 3. To subject the efficacy of our manipulation to the more rigorous test of assessing the long-term impact of experimental trauma exposure on belief and worldview change, we ran a series of regression models predicting Time 3 beliefs from experimental condition, Time 2 beliefs, and Time 1 beliefs, in order examine the extent to which Time 3 beliefs can be predicted by baseline beliefs, immediate post-manipulation beliefs, and the impact of the experimental condition.

Residual change score regression models predicting our 15 belief outcomes (i.e., world assumptions, American identity, and meaning in life) for Time 3 are presented in Table 4. Residual change models regressing Time 3 world assumptions onto Time 1 and Time 2 world assumptions accounted for a significant proportion of variance in Time 3 beliefs, $Fs(3, 163) \geq 25.48$, $p \leq .001$, $R^2s \geq 0.32$. In the model predicting Time 3 beliefs in the benevolence of people, experimental condition ($b=0.17$, $SE=0.8$, $\beta=0.10$, $p \leq .05$) significantly accounted for a residual variance in belief change, $F(3, 163)=69.58$, $p \leq .001$, $R^2=0.56$. A scatterplot depicting

change in belief in the benevolence of people from Time 2 to Time 3 is presented in Figure 1.

Residual change models regressing Time 3 American identity beliefs onto Time 1 and Time 2 American identity beliefs accounted for a significant proportion of variance in Time 3 beliefs, $Fs(3, 159) \geq 62.14$, $p \leq .001$, $R^2s \geq 0.55$. Experimental condition did not significantly predict residual belief change within these models.

Residual change models regressing Time 3 meaning in life beliefs onto Time 2 and Time 1 meaning in life accounted for a significant proportion of variance in Time 3 beliefs, $Fs(3, 163) \geq 36.70$, $p \leq .001$, $R^2s \geq 0.40$. In the model predicting Time 3 beliefs in the presence of meaning in life, experimental condition ($b=0.28$, $SE=0.10$, $\beta=0.13$, $p \leq .01$) significantly accounted for a residual variance in belief change, $F(3, 163)=94.23$, $p \leq .001$, $R^2=0.63$. A scatterplot depicting change in presence of meaning in life from Time 2 to Time 3 is presented in Figure 2.

Hypothesis 3: Participants in the Experimental Condition will Retrospectively Report Greater Impact of Film on Beliefs and Worldviews

Whereas our previous analyses tested hypotheses concerning objective belief change as a function of our experimental manipulation, our final analyses aimed to analyze participants' retrospective and subjective appraisals of belief change as a function of experimental condition. We independently analyzed two measures that assess the subjective impact of a traumatic event: The Impact of Events Scale (IES; Weiss & Marmar, 1997), which assesses participants' posttraumatic symptomatology and affective impact of a specific event, and the Core Beliefs Inventory (CBI; Cann et al., 2010), which measures the extent to which participants perceive an event as having reexamined their core beliefs and worldviews.

At Time 3, A two-sample t-test revealed no significant difference between conditions on the IES, $t(163)=-1.30$, $p=ns$, $d=0.20$, suggesting that the newsreel shown in the experimental

condition (M=1.59, SD=0.67) did not have a greater or lesser affective or clinical impact than did the newsreel shown in the control condition (M=1.75, SD=0.91). However, a two-sample t-test revealed significant group differences between conditions with respect to perceived changes in core beliefs on account of the manipulation, $t(161)=2.03$, $p\leq.05$, $d=0.32$, whereby participants in the experimental condition (M=2.37, SD=1.08) endorsed seriously re-examining their beliefs to a greater degree than did participants in the control condition (M=2.00, SD=1.24).

In light of the observation that the experimental and control conditions significantly differed in term of immediate post-manipulation affect and subjective core belief examination at one-month follow-up, we conducted a hierarchical linear regression to disentangle whether group differences in core belief examination at follow-up held after controlling for post-manipulation differences in affect. Findings from these analyses are presented in Table 5. In step one of the model, CBI is predicted exclusively from condition; following from the previously reported t-test, condition significantly and positively predicts a significant proportion of variance in CBI scores, b=0.37, SE=0.18, $p\leq.05$, and the step one model is significant, $F(1, 161)=4.12$, $p\leq.05$, R^2=0.02, Adjusted R^2=0.02. In step two, we include regression terms for each of the five PANAS subscales administered at Time 2. After controlling for affect, experimental condition no longer predicted a significant proportion of variance in CBI scores, b=-0.12, SE=0.30, $p=ns$. Fear, hostility, and self-assurance were positive and significant predictors of CBI scores, $bs\geq0.22$, $SEs\leq0.12$, $ps\leq0.05$; surprise was a negative and significant predictor of CBI scores, b=-0.20, SE=0.09, $p\leq0.05$; and sadness was not a significant predictor of CBI scores, b=-0.02, SE=0.12, $p=ns$. The step two model was significant, $F(6, 156)=4.41$, $p\leq.001$, R^2=0.15, Adjusted R^2=0.11, and accounted for a significantly greater proportion of variance in CBI scores over the step one model, $F(5, 156)=4.38$, $p\leq.001$, ΔR^2=0.13. Lastly, interaction terms for condition by affect were

entered into step three of the model; while this model was statistically significant, $F(11, 151)=2.43$, $p\leq.01$, $R^2=0.15$, Adjusted $R^2=0.09$, it did not account for a significantly greater proportion of variance than the step two model, $F(5, 151)=0.19$, p=ns, $\Delta R^2\leq.01$. Owing both to the non-significance of the regression coefficients within the model and the lack of an improved accounting for CBI score variance over the step two model, the step three model suggests that there were no significant interactions between condition and post-manipulation affect.

Discussion

As a result of booming interest in the meaning making process and posttraumatic growth, the psychological literature is now replete with studies assessing and analyzing the ways in which beliefs and worldviews are impacted by exposure to traumatic events. Scholars working in this area have developed a breadth of rich theoretical perspectives through which to view and study these processes. Despite significant advances in the understanding of meaning making and posttraumatic change, researchers have increasingly raised concerns about overreliance on one methodological paradigm for studying meaning making—the cross-sectional, retrospective, self-report design—and have drawn attention to its limited utility in helping researchers answer outstanding questions regarding the meaning making process (Frazier et al., 2009; Park, 2010). However, trauma researchers working outside of the meaning making and posttraumatic change literatures have readily adopted the use of film in experimental settings as a means to explore pre- to post-traumatic change and track target outcomes longitudinally (Holmes & Bourne, 2008; Holmes, Brewin, & Hennessey, 2004). The current study aimed to explore the viability of employing the trauma film paradigm for studying belief and worldview change following exposure to a recording of news coverage of a mass trauma—the 2013 Boston Marathon bombings. By employing a prospective, longitudinal, and experimental design for the present

study, our study took an innovative approach to examining belief and worldview change following indirect exposure to mass trauma.

Because our application of the film trauma paradigm utilized film that had not been validated in previous research, our first step was to test whether our experimental and control condition films differed in their impact on participants. To assess this, we measured affective responses and subjective distress immediately following the manipulation in each condition. We found that participants exposed to the traumatic live footage of the Boston Marathon attacks reported significantly higher levels of anger, fear, sadness, surprise, and subjective distress as compared to participants in the control condition, providing evidence that participants in the experimental condition had an affective response that is congruent with a traumatic stress response (e.g., Ehlers & Clark, 2000; Rachman, 2001). We assessed a form of affect, self-assurance, that bore no conceptual relationship to a traumatic stress response; our finding that mean affective scores for self-assurance did not differ between conditions provided further support for the claim that affective responses to the experimental condition film were associated with a traumatic stress response and not simply a general affective arousal. Moreover, these findings corroborate research demonstrating that exposure to televised news reports of the Boston Marathon bombings produced traumatic stress responses in individuals who had no direct exposure to the event (Holman, Garfin, & Silver, 2014).

Having thus supported our first hypothesis that participants in the experimental condition would demonstrate an affective response to the experimental manipulation that would not be observed in the control condition, we then assessed if and how the experimental manipulation differentially impacted changes in beliefs and worldviews. Per our second hypothesis, if exposure to a mass traumatic event produced changes in beliefs, we would anticipate a

significant time-by-condition interaction, wherein beliefs and worldviews for participants in the experimental condition would change pre- to post-trauma exposure, whereas belief and worldviews for participants in the control condition would remain unaffected by the non-traumatic control condition film. Our two (condition) by three (time) analyses of variance produced no significant interactions, thereby failing to support our second hypothesis. While significant main effects were found by time (e.g., as in beliefs about justice) and by condition (e.g., for beliefs in the centrality, satisfaction, and solidarity associated with American identity), these effects do not support our second hypothesis: In the case of the effects of time, the absence of a significant main effect of condition can be accounted for by the possibility that both the experimental and control conditions impacted these beliefs. Considering the high profile manhunt that followed the Boston Marathon attacks and the criminal charges brought against Edward Snowden, it is plausible that the substantive content of these films, and not their traumatic characteristics, drove condition-independent changes in beliefs about justice. Likewise, because the between-condition differences with respect to American identity were observed both pre- and post-manipulation, they cannot be attributed to the impact of the films in each condition.

Despite the absence of a time-by-condition interaction, the manipulations might have still had an impact on belief change at the within-group level. In other words, while mean differences in beliefs might not have differed between conditions over time, the experimental manipulation might still have accounted for a significant proportion of residual variance in change scores between time points (Gollwitzer, Christ, & Lemmer, 2014). While these analyses by-and-large confirmed our conclusions drawn from the analyses of variance—namely, that the experimental manipulation had little effect on changes in measured beliefs and worldviews—the experimental condition was found to account for a significant proportion of residual variance in change in

beliefs about the benevolence of people and presence of meaning in life between post-test (Time 2) and follow-up (Time 3).

Viewed through the conventional lens of null hypothesis significance testing, these findings suggest that our experimental manipulation failed to produce a reliable change in beliefs. Even the two significant effects pertaining to beliefs in the benevolence of people and the presence of meaning in life at Time 3 are suspect; considering the number of analyses performed, it is very plausible that these effects reflect Type 1 chance errors due to sample-specific characteristics as opposed to genuine and dependable population wide effects. Indeed, many researchers within the psychological scientific community have raised alarms about the dubious practice of lending undue theoretical weight to effects that narrowly pass the conventional $p<.05$ benchmark for statistical significance, and have drawn attention to the ways in which these practices produce a scientific corpus littered with false-positive findings (Bakker, van Dijk, & Wicherts, 2012; Simmons, Nelson, & Simonsohn, 2011).

For these reasons, we are hesitant to place too great a value in these two significant findings, and maintain that our second hypothesis is not supported by these data. We strongly believe that replication of this work or appropriate variations thereof (e.g., employing video from a different mass trauma) is necessary (for a discussion of replication practices, see Lindsay, 2015). This is especially true with regard to the significant finding that experimental condition accounted for a significant proportion of variance in residual change in beliefs about the benevolence of people from Time 2 to Time 3. The fact that this finding was only narrowly significant (i.e., p very near .05) raises suspicions concerning its replicability. An examination of group means raises more eyebrows: The remarkable longitudinal stability of mean scores among participants within the experimental condition contrasted against the comparatively greater

longitudinal variably in mean scores within the control condition suggests random variability, and not the quasi-traumatic nature of the film in the experimental condition, is the primary cause of this statistically significant and theoretically inconsistent finding.

Whereas the statistically significant finding regarding the impact of the manipulation on beliefs in the benevolence of others seems suspect on closer examination, the significant finding that experimental condition accounted for a significant proportion of variance in the presence of meaning in life from Time 2 to Time 3 appears more empirically and theoretically valid. Not only is the magnitude of this effect comparatively larger than any other observed manipulation effect among the analyses performed, but this finding corroborates theory in expected ways: According to the meaning making model, we would anticipate that exposure to traumatic material that violates core beliefs would be followed by a search for meaning that, ultimately, would increase presence of meaning in life (Park, 2010). Certainly, there are other plausible accounts for this finding: For instance, research demonstrates that college students who feel supported in their college environments report a greater sense of meaning in life as compared to those who do not (Shin & Steger, 2016); considering that a majority of our sample were first year college students, observed increases in presence of meaning in life could reflect adaptive adjustment to collegiate life rather than the end result of a meaning making process following our manipulation. Yet, we would not expect such an effect to be isolated exclusively to participants in the experimental condition. As such, we see this finding as tentative evidence in support of the efficacy of the trauma film paradigm in effecting changes in a sense of meaning in life. However, considering the novelty of our use of the trauma film paradigm in research on meaning making and belief change, future research should replicate this work in order to assess the durability of this finding.

In addition to finding that presence of meaning in life among participants in the experimental condition was impacted by the manipulation, we also found a significance group difference with respect to participants' retrospective appraisal of the extent to which the events depicted in each condition initiated a re-examination of core beliefs, thereby providing tentative support for our third hypothesis. Further analysis, however, revealed that this difference was attributable primarily to affective responses to the films. This finding underscores the central role of affect and emotional processing in trauma (e.g., Ehlers & Clark, 2000; Rachman, 2001). Moreover, it is consistent with the trauma film paradigm literature: A meta-analysis of 16 trauma film paradigm experiments found that an absence of posttraumatic flashbacks following the manipulation were associated with low affective response (Clark, Mackay, & Holmes, 2014). To our knowledge, this finding is the first to demonstrate that affective responses to a trauma film paradigm manipulation are associated with participants' perceptions of retrospective core belief change.

Intriguingly, however, participants' perceptions of retrospective belief change were not corroborated by our aforementioned attempts to directly measure belief change. Taken together, these findings echo the conclusions drawn by Frazier et al. (2009) regarding differences in perceived posttraumatic growth versus actual posttraumatic growth—namely, that retrospective, self-reported posttraumatic growth is not reflected in directly measured longitudinal changes. Based on the current study, we tentatively conclude that perceived core belief change is not reflected by actual core belief change. However, our findings have the advantage of being drawn from an experimental study that exposed participants to a single quasi-traumatic event, thereby providing greater internal validity to our results by reducing variance in the timing of the stressful event and eliminating the heterogeneity in traumatic events reported in correlational

designs, such as are typically employed in meaning-making research (Frazier et al., 2009; Park, 2010). Moreover, our direct measurement of affect immediately following the manipulation gave us the capacity to assess its role in the meaning making process. The critical role that immediate post-traumatic event affective responses plays in the meaning making process has been heretofore under-examined on account of the near impossibility of making such assessments using correlational study designs.

For these reasons, findings from our study may provide new insight into the meaning making process. For instance, research by Heintzelman and King (2014) have brought to the fore a conception of "meaning-as-information," wherein perceptions of meaninglessness and incoherence may catalyze the meaning making process independent of affective experience. From this perspective, traumatic incidents may produce an experience that is high in negative affect and low in sense of meaning, and while these two traumatic response often co-occur with one another, they may not always do so, and therefore may have different impacts on posttraumatic change processes (Heintzelman & King, 2014). Search for meaning did increase immediately following the manipulation among participants in the experimental condition (albeit not significantly), which would lend some credence to the notion that our manipulation produced an experience of low meaning, and the significant growth in presence of meaning in life in the experimental group from post-manipulation to one-month follow-up provides further evidence, consistent with the "meaning-as-information" approach, that meaning may instigate the meaning making process.

By the same token, our data also provide tentative support to the perspective that meaning making occurs by way of emotional processes and the construal of posttraumatic growth narratives (Eid, Johnsen, & Saus, 2005; Maček, 2014; McAdams, 2006; Rachman, 2001;

Tedeschi & Calhoun, 2004). Affective responses to the experimental condition film significantly predicted retrospective reports of participants having "seriously re-examined" their core beliefs, but in the absence of any directly measurable belief change. From this vantage point, it could be argued that perceiving oneself as having interrogated or reformed one's worldview in the aftermath of trauma is more important than having *actually* interrogated or reformed them (e.g., as in Frazier et al., 2009). Taken to an extreme, we might surmise that perceptions of posttraumatic belief change, rather than serving as the actual mechanism by which posttraumatic growth and meaning making processes occur, is simply an unrecognized and potentially fictitious component of the redemption myth that characterizes the life narratives of so many Americans (McAdams, 2006).

Perhaps our enduring fascination with the myth of the phoenix lies in a deeply human longing that, through loss and tragedy, we can be reborn as people far greater than we previously imagined—even when we are, in essence, the same people we have always been. Or, perhaps, the limitations and conventions of our statistical analyses (e.g., Bakker, van Dijk, & Wicherts, 2012) prevent us from appreciating the very real changes that traumas inspire in the context of our experiment. As an example, consider these data through the lens of terror management theory: Terrorist attacks such as the Boston Marathon bombings simultaneously raise awareness of human mortality and, in instances such as this, desecrate cherished cultural landmarks or shared cultural spaces. On the basis of theory and prior research, we should expect that mass terrorist incidents like these should instigate a defense of cultural worldviews and a strengthened identification with a valued in-group (Arndt & Vess, 2008; Morgan, Wisneski, & Skitka, 2011). The solidarity among Americans generally, and Bostonians specifically, following the Boston Marathon that was captured by the "Boston Strong" movement emphasized national solidarity

with Bostonians, unity among Bostonians, and the strength and resilience of the city's affected communities (e.g., Buhrmester et al., 2015; Ferrer & Conley, 2015). Even so, by the conventions of null hypothesis significance testing, the remarkable collective meaning making process that was embodied in the "Boston Strong" movement were not seen in our data.

The time lapse between the actual event and our experimental manipulation may accurately account for this. Even so, our data do show non-significant increases in relevant outcomes (i.e., in-group homogeneity and in-group solidarity) immediately post-manipulation that would have supported our hypotheses were they statistically significant. It is not our intention to make a mountain out of a molehill for the purpose of inflating the importance of our data; rather, we raise these considerations to encourage future researchers to consider employing the trauma film paradigm in the context of exploring hypotheses derived from terror management theory.

Limitations

In applying the trauma film paradigm to the study of meaning making and belief change, we sought to apply a methodology that would eschew the limitations of the frequently employed retrospective, correlational study designs commonly used in this field of research. Nevertheless, the trauma film paradigm, and our particular application of it, is not without its own limitations. First and foremost, both study conditions employed films whose content focused on national events that have garnered heavy and repeated news coverage. With respect to the Boston Marathon bombing, past news coverage itself has already been demonstrated to have had an impact on national mental health (Holman, Garfin, & Silver, 2014). As such, the events shown to participants were, in all likelihood, ones with which they had great familiarity. It is conceivable that meaning violations caused by these events might have been successfully resolved by

participants long before exposure to our manipulation. As previously noted, we chose to use news footage from the Boston Marathon bombing on account of the anticipated salience of this event for American cultural worldviews and the proximal nature of the threat for the typical American. However, future research might employ film from a traumatic event that researchers can reasonably assume Americans have not previously witnessed and have not assimilated into their pre-existing worldviews (e.g., footage of a mass trauma from overseas that has not gotten significant coverage in the United States).

The current study measured beliefs and worldviews that have theoretical relevance to shattered assumptions theory, meaning making theory, and terror management theory. Despite our attempts to be reasonably comprehensive in our measurement of beliefs and worldviews that bear on extant theory, the cornucopia of beliefs and worldviews that might be measured far outnumber those we assessed (Koltko-Rivera, 2004). Considering the nature of the films we employed, beliefs pertaining to intergroup relations, spiritual growth, and/or political goings-on, to name just a few, might have been affected by our manipulation. Future research should diversify the range of measured worldviews in an effort to identify those that have the most significant implications for meaning and belief development.

Our study relied on a sample of college students. Emerging adults attending college are besieged by a competing array of developmental, relational, and contextual forces that simultaneously influence their beliefs and worldviews (Arnett, Ramos, & Jensen, 2001; Gutierrez & Park, 2015; Nelson & Padilla-Walker, 2013). Amidst the *sturm und drang* that characterizes this critical phase of personality development, the impact of a 10-minute film-based manipulation may have simply been drowned in a sea of other more proximal conflicts and crises affecting this particular population. It behooves future researchers employing the trauma

film paradigm to account for personal traumas in assessing the impact of their manipulations on meaning-related outcomes. In addition, future research should consider recruiting non-college samples in studies that aim to assess meaning and belief change with the trauma film paradigm.

Lastly, while nomothetic conclusions concerning the process of belief change may, with qualification, be drawn from trauma film research, mass traumatic events are embedded in a time, place, and culture that make them inherently idiographic (Maček, 2014). Our study's methodology relied on the use of a traumatic event situated in a very specific place and time. As time passes and specific mass traumatic events recede into history, individuals' assessments of the meaning of these events will undoubtedly be affected by other interceding historical events in tandem with changes in their personal lives. Depending on the theoretical aims of a given research project, this observation may alternatively be viewed as a strength or a weakness: While such a consideration complicates the production of time-independent generalizable knowledge about psychological processes pertaining to posttraumatic change and meaning making following mass trauma, it gives cultural psychologists, ethnographers, sociologists, and historians a snapshot of people's understanding of the cultural events affecting them in a specific historical moment in time. Only through the application of nomothetic models of meaning making to idiographic events situated in specific time and place can we refine the science of posttraumatic change and meaning making in order to best conceptualize, predict, and respond to mass traumatic events and the responses that people have to them.

Conclusion

On the basis of our findings, we believe that the trauma film paradigm holds great promise for advancing the psychological science of posttraumatic change and meaning making processes, and we encourage traumatologists and meaning making researchers to add the trauma

film paradigm to their methodological toolkits. Our findings demonstrate that the trauma film paradigm produces changes in affect and meaning that correspond to theorized posttraumatic responses and meaning making processes. In light of the plethora of theoretical perspectives on these phenomena, however, our data can be reasonably accounted for by equally plausible theoretical accounts. Through diversification and repeated application of this methodology, we anticipate that the trauma film paradigm will extend and inform the science of posttraumatic change and meaning making.

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Table 1

Schedule of Study Administration and Measures

Methods and Measures	Prescreen (T0)	Pre-film (Time 1)	Film (Time 2)	Post-film (Time 3)
Administration				
Administration Site	Online	Online	In Laboratory	Online
Informed Consent		✔	✔	
Experimental Manipulation (Video)			✔	
Attentional Check			✔	
Debrief				✔
Measures				
Directly affected by Boston Marathon bombing	✔			
Demographic Information		✔		
World Assumptions Scale (WAS; Janoff-Bulman, 1989)		✔	✔	✔
American Identity (adapted from Leach et al., 2008)		✔	✔	✔
Meaning in Life Questionnaire (MLQ; Steger et al., 2006)		✔	✔	✔
Affective Response (adapted from PANAS-X; Watson & Clark, 1992)			✔	
Subjective Units of Distress Scale (SUDS; Wolpe, 1969)			✔	✔
Core Beliefs Inventory (CBI; Cann et al., 2010)				✔
Impact of Events Scale-Revised (IES-R; Weiss & Marmar, 1997)				✔

Note. Prescreen (T0) occurred at the beginning of either the Fall 2014 or Spring 2014 semester. Pre-film (T1) measures were administered two weeks prior to the film (T2) administration, and post-film (T3) measures were administered four weeks following the film (T2) administration.

Table 2

Means, Standard Deviations, and Internal Consistencies of Study Variables

	Time 1 (Two weeks pre-film)					Time 2 (Immediately post-film)					Time 3 (One month post-film)				
	Experimental		Control			Experimental		Control			Experimental		Control		
Variable	*M*	*SD*	*M*	*SD*	Cronbach's α	*M*	*SD*	*M*	*SD*	Cronbach's α	*M*	*SD*	*M*	*SD*	Cronbach's
World Assumptions															
Benevolent world	4.25	0.80	3.95	0.78	0.75	4.03	0.95	3.94	0.87	0.82	4.04	0.98	4.01	0.87	0.85
Benevolent people	4.27	0.76	4.16	0.60	0.64	4.33	0.80	4.31	0.69	0.71	4.33	0.80	4.10	0.78	0.72
Controllability	3.49	0.84	3.37	0.81	0.73	3.19	0.96	3.21	0.93	0.82	3.26	0.92	3.30	1.04	0.85
Justice	3.63	0.85	3.40	0.82	0.72	3.25	0.94	3.30	0.89	0.76	3.43	0.76	3.42	0.98	0.75
Luck	4.44	0.81	4.19	0.95	0.79	4.45	0.83	4.36	0.93	0.79	4.37	0.97	4.28	0.93	0.87
Randomness	3.79	0.84	3.56	0.81	0.67	3.79	1.01	3.64	0.88	0.77	3.73	0.87	3.72	0.94	0.77
Self-control	4.62	0.67	4.53	0.73	0.76	4.44	0.79	4.36	0.83	0.76	4.31	0.88	4.25	0.89	0.83
Self-esteem	4.80	1.08	4.64	0.93	0.85	5.04	0.93	4.91	0.97	0.87	4.78	1.05	4.63	1.01	0.87
American Identity															
Centrality	4.85	1.32	4.36	1.34	0.82	4.91	1.12	4.42	1.34	0.81	4.75	1.25	4.48	1.30	0.86
In-group homogeneity	4.09	1.45	4.00	1.31	0.89	4.43	1.39	4.43	1.41	0.91	4.46	1.23	4.57	1.39	0.92
Satisfaction	5.76	1.04	5.44	0.95	0.87	5.70	0.88	5.44	0.92	0.88	5.56	0.90	5.30	0.96	0.88
Self-stereotyping	4.86	1.37	4.60	1.20	0.87	4.94	1.23	4.71	1.26	0.89	4.78	1.29	4.61	1.19	0.91
Solidarity	5.12	1.16	4.72	1.14	0.84	5.25	1.03	4.83	1.28	0.88	5.12	1.06	4.82	1.29	0.91
Meaning in Life															
Presence	4.70	1.23	4.70	1.08	0.84	4.70	1.30	4.71	0.94	0.86	4.83	1.08	4.55	1.12	0.84
Search	4.91	1.35	5.11	1.17	0.89	5.17	1.16	5.11	1.13	0.90	4.91	1.14	4.90	1.14	0.90
Affect															
Fear	-	-	-	-	-	3.41	1.20	1.69	0.74	0.87	-	-	-	-	-
Hostility	-	-	-	-	-	3.87	1.20	2.11	1.02	0.78	-	-	-	-	-
Sadness	-	-	-	-	-	3.88	1.10	1.44	0.66	0.89	-	-	-	-	-
Self-assurance	-	-	-	-	-	1.87	0.81	1.90	0.89	0.65	-	-	-	-	-
Surprise	-	-	-	-	-	3.19	1.18	2.15	0.94	0.72	-	-	-	-	-
Attentional Check						4.42	0.75	4.81	0.45	-					
Subjective Units of Distress	-	-	-	-	-	44.93	24.66	18.82	18.12	-	22.64	24.55	31.48	24.08	-
Impact of Events Scale	-	-	-	-	-	-	-	-	-	-	1.59	0.67	1.75	0.91	.96
Core Beliefs Inventory	-	-	-	-	-	-	-	-	-	-	2.37	1.08	2.00	1.24	.94

Note.

Table 3

Factorial Analyses of Variance assessing Effects of Time (3) by Condition (2) on Study Outcome Variables (N=168)

Variables	Time				Condition				Time X Condition			
	SS	MS	$F(2, 498)$	η_p^2	SS	MS	$F(1, 498)$	η_p^2	SS	MS	$F(2, 498)$	η_p^2
World Assumptions												
Benevolent world	2.68	1.34	1.74	0.01	3.83	3.83	4.98*	0.01	1.73	0.86	1.12	<0.0
Benevolent people	0.22	0.11	0.20	<0.01	0.45	0.45	0.81	<0.01	0.88	0.44	0.80	<0.0
Controllability	4.07	2.03	2.41	0.01	0.60	0.60	0.71	<.01	0.59	0.30	0.35	<0.0
Justice	6.22	3.11	4.05*	0.02	2.14	2.14	2.79	0.01	1.77	0.89	1.15	<0.0
Luck	0.35	0.17	0.21	<0.01	2.64	2.64	3.24	0.01	0.77	0.38	0.47	<0.0
Randomness	0.17	0.08	0.11	<0.01	2.22	2.22	2.78	0.01	1.00	0.50	0.63	<0.0
Self-control	4.31	2.16	3.35*	0.01	0.35	0.35	0.55	<0.01	0.03	0.01	0.02	<0.0
Self-esteem	3.44	1.72	1.73	0.01	1.06	1.06	1.06	<0.01	0.02	0.01	0.01	<0.0
American Identity												
Centrality	1.05	0.53	0.32	<0.01	9.58	9.58	5.87*	0.01	1.26	0.63	0.39	<0.0
In-group homogeneity	6.85	3.43	1.84	0.01	0.35	0.35	0.19	<0.01	0.84	0.42	0.23	<0.0
Satisfaction	1.71	0.85	0.95	<0.01	4.21	4.21	4.70*	0.01	0.10	0.05	0.06	<0.0
Self-stereotyping	1.15	0.58	0.36	<0.01	2.74	2.74	1.73	<0.01	0.20	0.10	0.06	<0.0
Solidarity	1.04	0.52	0.39	<0.01	6.51	6.51	4.82*	0.01	0.38	0.19	0.14	<0.0
Meaning in Life												
Presence	0.98	0.49	0.38	<0.01	<0.01	<0.01	<0.01	<0.01	2.23	1.17	0.91	<0.0
Search	3.76	1.18	1.34	0.01	1.68	1.68	1.20	<0.01	1.59	0.80	0.57	<0.0

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

Table 4

Residual Change Score Regression Models for Time 2 and Time 3 Beliefs and Worldviews

	World Assumptions															
	Benevolent World		Benevolent People		Controllability		Justice		Luck		Randomness		Self-Control		Self-Esteem	
	b	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE
T2 Belief																
Condition	-0.13	0.11	-0.05	0.09	-0.10	0.12	-0.20	0.11	-0.04	0.12	-0.02	0.11	-0.02	0.10	0.01	0.09
T1 Belief	0.73***	0.07	0.71***	0.06	0.71***	0.07	0.67***	0.07	0.51***	0.07	0.73***	0.07	0.64***	0.07	0.76***	0.04
F	56.63***		61.92***		50.65***		49.05***		29.37***		57.29***		37.25***		156.70***	
df	2/165		2/165		2/165		2/165		2/165		2/165		2/165		2/165	
R^2	0.41		0.43		0.38		0.37		0.26		0.41		0.31		0.66	
Adjusted R^2	0.40		0.42		0.37		0.37		0.25		0.40		0.30		0.65	
T3 Belief																
Condition	-0.09	0.10	0.17*	0.08	-0.04	0.11	-0.04	0.10	-0.04	0.12	-0.07	0.12	-0.02	0.10	0.05	0.10
T2 Belief	0.59***	0.07	0.42***	0.07	0.63***	0.07	0.46***	0.07	0.46***	0.08	0.53***	0.08	0.55***	0.08	0.68***	0.09
T1 Belief	0.25**	0.08	0.49***	0.08	0.20*	0.08	0.32***	0.07	0.32**	0.08	0.01	0.09	0.1**	0.09	0.18*	0.09
F	64.60***		69.58***		60.22***		55.27***		35.64***		25.48***		45.99***		80.77***	
df	3/164		3/163		3/163		3/164		3/163		3/164		3/164		3/163	
R^2	0.54		0.56		0.53		0.50		0.40		0.32		0.46		0.60	
Adjusted R^2	0.53		0.55		0.52		0.49		0.39		0.31		0.45		0.59	
	American Identity										**Meaning in Life**					
	Centrality		Homogeneity		Satisfaction		Self-Stereotyping		Solidarity		Presence		Search			
	b	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE	*b*	SE		
T2 Belief																
Condition	0.14	0.13	-0.05	0.17	0.03	0.09	0.05	0.14	0.12	0.13	-0.01	0.11	0.17	0.14		
T1 Belief	0.71***	0.05	0.65***	0.06	0.70***	0.05	0.70***	0.05	0.74***	0.05	0.75***	0.05	0.56***	0.06		
F	119.30***		56.00***		123.90***		89.92***		98.56***		118.00***		50.23***			
df	2/160		2/160		2/160		2/160		2/160		2/165		2/165			
R^2	0.60		0.41		0.61		0.53		0.55		0.59		0.39			
Adjusted R^2	0.59		0.40		0.60		0.52		0.55		0.58		0.37			
T3 Belief																
Condition	-0.11	0.13	-0.11	0.14	0.08	0.10	-0.02	0.12	-0.01	0.13	0.28**	0.10	0.02	0.14		
T2 Belief	0.52***	0.08	0.59***	0.07	0.55***	0.08	0.47***	0.07	0.50***	0.08	0.43***	0.07	0.49***	0.08		
T1 Belief	0.30**	0.07	0.14*	0.07	0.24**	0.07	0.36***	0.07	0.30***	0.08	0.38***	0.07	0.18**	0.07		
F	79.25**		62.14**		74.88***		86.61***		65.36***		94.23***		36.70***			
df	3/159		3/159		3/159		3/159		3/159		3/163		3/163			
R^2	0.60		0.54		0.59		0.62		0.55		0.63		0.40			
Adjusted R^2	0.59		0.53		0.58		0.61		0.54		0.63		0.39			

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

Table 5

Hierarchical Multiple Regression Analysis Predicting Examination of Core Beliefs (CBI) by Condition and Post-Manipulation Affect

	Step One		Step Two		Step Three	
Variables	B	SE	B	SE	B	SE
Condition[1]	0.37*	0.18	-0.12	0.30	0.02	0.74
Affect						
Fear			0.22*	0.11	0.13	0.23
Hostility			0.22*	0.11	0.18	0.15
Sadness			-0.02	0.12	0.09	0.23
Self-assurance			0.24*	0.11	0.31	0.16
Surprise			-0.20*	0.09	-0.20	0.16
Condition by Affect Interactions						
Condition X Fear					0.10	0.26
Condition X Hostility					0.09	0.22
Condition X Sadness					-0.16	0.27
Condition X Self-assurance					-0.12	0.23
Condition X Surprise					-0.01	0.20
F	4.12*		4.41***		2.43***	
df	1/161		6/156		11/151	
R^2	0.02*		0.15***		0.15	
Adjusted R^2	0.02		0.11***		0.08	
ΔR^2	-		0.13***		>0.01	

Note. [1]Experimental condition=1, control condition=0. *p < .05, **p < .01, ***p <.00.

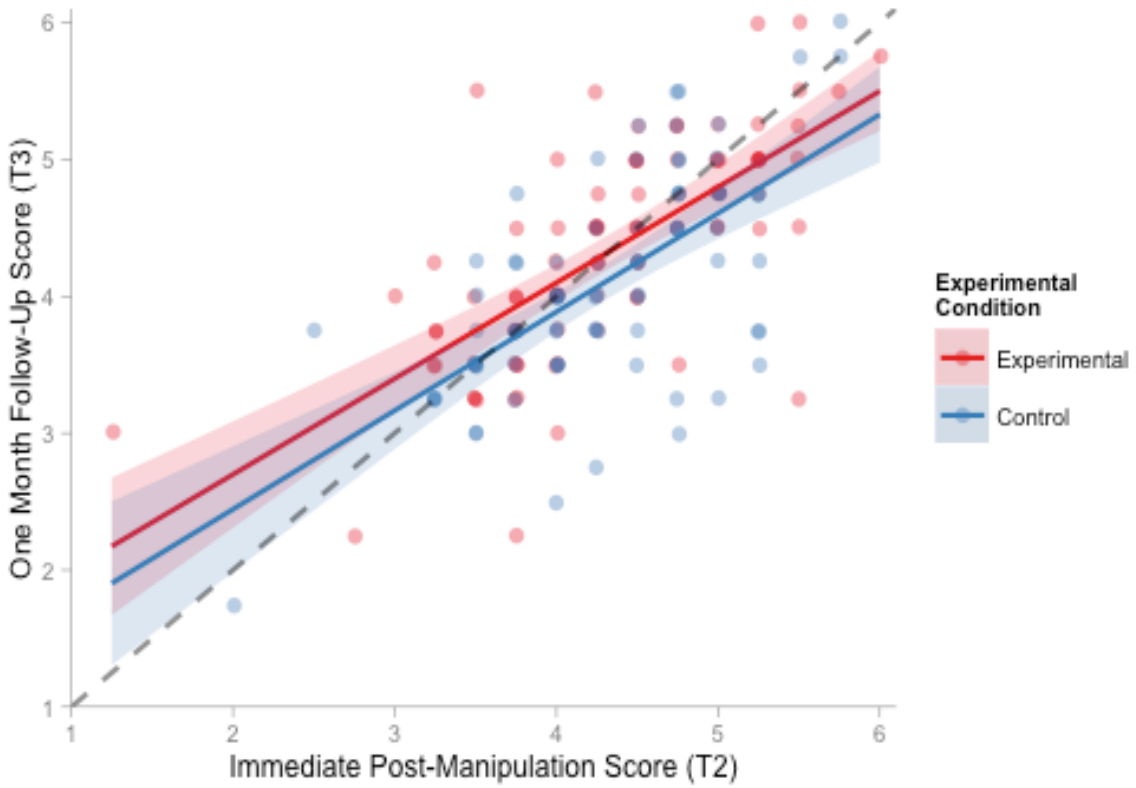

Figure 1. Scatterplot of beliefs in the benevolence of people at immediate post-manipulation (x-axis) and one-month follow-up (y-axis) by experimental condition. Regression lines for each condition are accompanied by confidence interval band in gray. Shaded area represents 95% confidence interval. Dotted gray line depicts points of no change in belief (i.e., Time 2 = Time 3).

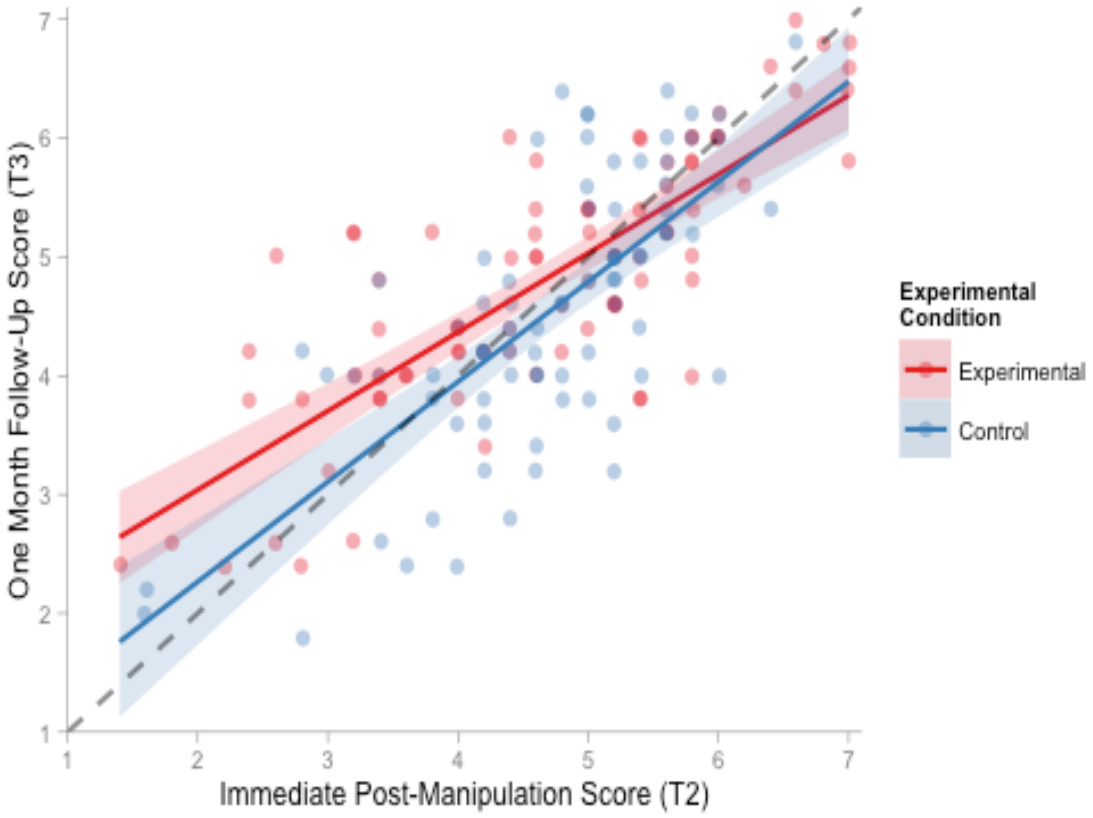

Figure 2. Scatterplot of presence of meaning in life at immediate post-manipulation (x-axis) and one-month follow-up (y-axis) by experimental condition. Regression lines for each condition are accompanied by confidence interval band in gray. Shaded area represents 95% confidence interval. Dotted gray line depicts points of no change in belief (i.e., Time 2 = Time 3).

Appendix A

Attentional check questionnaire for the experimental condition:

Based on the information presented in the video you just watched, please answer the following true or false questions to the best of your ability:

(1) The attacks in Boston consisted of four distinct explosions. **False**
(2) The explosions took place in downtown Boston. **True**
(3) The explosives were planted near the finish line of the Boston Marathon. **True**
(4) President Obama described the event as a "terrorist attack." **False**
(5) April 15th is a state holiday in Massachusetts. **True**

Attentional check questionnaire for the control condition:

Based on the information presented in the video you just watched, please answer the following true or false questions to the best of your ability:

(1) U.S. government intelligence data mining started under the Bush administration. **True**
(2) Ari Fleisher (man on right) argued that the government should not conduct surveillance on law-abiding citizens. **False**
(3) Jim Walsh (man on left) participated in this broadcast from a news studio in Watertown, Massachusetts. **True**
(4) Senator Dianne Feinstein argued that government data mining is designed to protect Americans from future terrorist attacks. **True**
(5) Wolf Blitzer (host) took a strong stance against government data mining. **False**