Rally Effects, Threat, and Attitude Change: An Integrative Approach to Understanding the Role of Emotion

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Rally ‘round the flag effects (J. E. Mueller, 1970) represent sudden and dramatically powerful situation-specific shifts in attitudes toward the American president. However, the extant literature has yet to fully clarify the nature of the psychological dynamics associated with this effect. These ambiguities reflect fundamental differences of opinion among scholars on some very basic questions such as whether overtly experienced emotion should mediate these attitudinal shifts or whether these changes reflect more general shifts in conservative ideology. Across 4 experiments, the authors sought to gain greater clarity on these and other important matters using a multimethod approach in which the authors varied whether participants viewed documentary footage of the 9/11 attacks (Experiments 1–2), generated autobiographical memories of that event (Experiment 3), or retrieved nonpolitical memories from their past (Experiment 4). The authors discuss the relevance of the present findings for theory and research across a variety of different theoretical and methodological paradigms, including social psychological models of threat, emotional appraisal models, and the political science literature.

Keywords: threat, emotion, political attitudes, rally ‘round the flag

Following the attacks on the World Trade Center and Pentagon on September 11, 2001 (9/11), President George W. Bush saw his job approval ratings soar 39 points to 90% in just a few days. This represents an example of a “rally ‘round the flag effect,” characterized by dramatic spikes in popularity of the president following a catastrophic crisis involving the nation as a whole (Mueller, 1970). One of the more interesting aspects of rally effects is their capacity to involve rapid transformation of attitudes among huge numbers of people. In the case of the 9/11 attacks, millions of Americans who did not support George W. Bush—or even harbored strong resentment toward him—on the day before the 9/11 attacks may have experienced an upward surge in attitudes the very next day.

Such effects vividly illustrate a core principle in social psychology: the “power of the situation” in shaping judgment and behavior (Allport, 1935). However, there are many important ambiguities regarding rally effects, and an initial goal of our research was to gain more insight into these effects under controlled laboratory settings. We investigated this issue in the context of a broader objective, examining how threatening contexts can affect a wide range of attitudes, including but not limited to appraisals of the American president. Apart from the theoretical importance of studying rally effects, there are practical reasons to do so as well. Changes in presidential approval can have important, long-lasting consequences, most obviously including the outcome of national elections. Indeed, the re-election of George W. Bush in 2004 was almost certainly due, in part, to the lingering effects of the 9/11 attacks. Moreover, surges in presidential approval can influence, directly or indirectly, public support for controversial policies such as the Patriot Act, or even decisions to launch military attacks. Hence, understanding the nature of these dramatic surges in presidential support may also be of interest to scholars interested in understanding and predicting the dynamics of democratic political systems.
Potential Perspectives on Rally Effects

Unlike other types of well-researched psychological phenomena, there is no single theory of rally effects, nor has there been an organized body of research specifically devoted toward understanding the processes that are responsible for this phenomenon. Nevertheless, rally effects can be studied through the lens of different literatures and bodies of research, with each offering their own unique perspective. In this section, we consider four such perspectives. Although these are not the only perspectives that could be considered, these are the ones most relevant to our experimental paradigm.

Research on Presidential Approval Polls

Much of what is known about rally effects is derived from archival (i.e., after-the-fact) analysis of presidential approval polls (cf. Baker & O'Neal, 2001; Brody & Shapiro, 1989). This literature has perhaps been most successful in identifying the boundary conditions under which rally effects are believed to most likely appear, and identifying those factors that contribute to their intensity and duration. Early models of rally effects (Mueller, 1970) suggested that any sudden international crisis could trigger sustained boosts in presidential popularity. However, a recent review of this literature (Baker & O'Neal, 2001) suggests that an emerging military crisis is one of the few factors known to be sufficient, in and of itself, to trigger sustained increases in support for the president. As we discuss later, the apparent specificity of rally effects to militant or “aggressive” events provides an initial clue as to the type of emotional experience that might play an important role in mediating such effects. Nonetheless, this literature affords little insight into the cognitive and/or affective processes that are responsible for their emergence. Also, because political surveys typically generate correlational data, they provide limited leverage in testing causal models of threat and attitude.

Social Identity Theory

The basic feature of rally effects—surges in presidential approval in times of war—is generally consistent with broad principles of social identity theory (Tajfel & Turner, 1986), which suggests that an attack against the United States should galvanize support for the nation’s leader and the country as a whole. Although social identity theory is not a model of rally effects per se, it can account for counterintuitive findings not easily explained by other models. For example, social identity theory suggests that boosts in presidential approval are most likely to occur when the attack accentuates or “primes” national identity. Other things being equal, this is most likely to happen when the people deemed responsible for the attack are not Americans. In theory, this could provide one possible reason why rally effects did not occur following the Oklahoma City bombings, a terrorist attack perpetrated by American citizens. Although social identity theory provides a useful foundation for understanding rally effects in this and other respects (cf. Dumont, Yzerbyt, Wiggoldus, & Gordijn, 2003), it makes no specific predictions as to the precise role of emotion in mediating attitude change, an issue of central concern in the present article.

Social Psychological Theories of Threat

Social psychologists have long been interested in the notion that situationally based threat can trigger powerful shifts in attitudes. Of particular concern for our purposes are four distinct but interrelated lines of research, including that on (a) terror management theory (Greenberg, Solomon, & Pyszczynski, 1997), (b) uncertainty management theory (Van den Bos, Poortvliet, Maas, Miedema, & Van den Ham, 2005), (c) the motivated social cognition framework (Jost, Glaser, Kruglanski, & Sulloway, 2003), and (d) anxiety-based formulations of authoritarianism (Doty, Peterson, & Winter, 1991).

Although these models differ in some important ways, they share two general assumptions. First, they emphasize the capacity of situational contexts to temporarily threaten one’s sense of psychological security. Second, they predict systematic changes in attitude and values to occur in the service of restoring such security. There is presently some debate as to the exact nature of these shifts and whether they are inevitable across all people and contexts (Jonas et al., 2008; Jost, Fitzsimons, & Kay, 2004; McGregor & Marigold, 2003). Nevertheless, the most well-documented effect in this literature is for people to adopt certain aspects of conservative ideology (especially, authoritarianism), including unabashed patriotism (“One should respect the flag, no matter what”), unwavering support for the president, dislike of “unconventional” lifestyles, along with an aggressive, “hawkish” foreign policy oriented toward protecting Americans from threatening outgroups. It is not hard to see how this literature could be used as a basis for accounting for rally effects, and this connection has recently been made most explicitly by terror management researchers (Landau et al., 2004; Pyszczynski, Solomon, & Greenberg, 2003). According to this literature, reminders of the 9/11 attacks should not only trigger more support for the American president but also might lead to changes with respect to other kinds of attitudes unrelated to presidential support per se, such as greater dislike of homosexuals or opposition to abortion.1

1 Although terror management theory offers an interesting framework for understanding rally effects, some work in the terror management literature appears to suggest that threat should produce a general polarizing effect, insofar as conservatives should become more conservative and liberals should become more liberal (Greenberg, Simon, Pyszczynski, Solomon, & Chatel, 1992; but see also Jonas et al., 2008). In contrast, research and theory by Jost and colleagues suggest that threat-induced conservative shifts should be expected to occur among all people regardless of their preexisting ideology (Jost et al., 2003; see also Nail et al., 2009). To a certain extent, the latter position arguably provides a more plausible account for the widespread nature of rally effects. For example, it could explain why vast segments of the population could become predisposed to adopt traditionally conservative views (e.g., a hawkish perspective on military interventionism) in the context of threat, even if this hawkishness was contrary to the kinds of attitudes people might normally embrace. Although this debate is interesting, it is less relevant to present concerns than might be apparent at first blush. This is because (a) our research was explicitly concerned with the mediational role of anger and anxiety on attitude change (an issue not addressed by this prior research) and (b) the aforementioned literature has been mostly concerned with the consequences of mortality salience manipulations, which is quite different from the way that we operationalized threat in our main studies.
Given the security-based motives implicated by these models, one might imagine that anxiety-related emotions would play a central role in producing these attitude shifts. However, this is not always the case. Of particular interest for us are the terror management and uncertainty management models, which essentially excludes any role of emotion in mediating threat-induced attitude change. Indeed, researchers in both camps have repeatedly claimed that the manipulations used in their respective literatures—including reminders of the 9/11 attacks—produce no changes in overt emotions at all (cf. Landau et al., 2004). In fact, many of these studies do report null effects of their manipulations on emotion. However, we believe that these and other researchers have been too hasty to rule out the important role that emotion can play in threat-driven changes in attitude.

In our view, this state of affairs is due to the way that researchers in these areas have typically measured negative emotion. Instead of forming a separate index of anxiety (or related constructs of uncertainty/lack of control), these researchers typically form a broad-based index of negative affect on the basis of an average of several different types of affective experience, some of which are related to anxiety and some of which are not (e.g., distress, hostility, irritability, anxiety, shame, and guilt). There are two different reasons why the use of such indices can potentially obscure detection of the actual role of emotion. First, any given manipulation (say, a reminder of the 9/11 attacks) may be more relevant to certain negative emotions (e.g., anger) compared with others (e.g., guilt). Lumping together emotions in the same index, regardless of whether they are actually related to the manipulation, obviously makes that measure less sensitive than it otherwise would be. Second, there may be cases in which two of the emotions included in the composite (e.g., anger and anxiety) are actually exerting opposite effects on the criterion variable, leading to a state of affairs in which these effects, in essence, “cancel each other out” in terms of the apparent impact on the criterion variable. We return to these issues later in this article.

Emotional Appraisal Models

In contrast to the aforementioned models, several lines of research suggest that affective experience (emotion, mood) can play a direct role in driving judgment and behavior (Forgas, 2002; Frijda, 1986; Harmon-Jones, 2004; Lambert, Khan, Lickel, & Fricke, 1997; Lerner & Keltner, 2000; Schwarz, 1990; Smith, 1993). The emotional appraisal model, articulated by Lerner and Keltner (2000), is particularly relevant for present concerns because this framework has most explicitly emphasized the idea that different types of emotion are likely to be associated with different motives and, as such, are likely to have different types of effects on judgment and behavior.

Even more relevant is research on the 9/11 attacks by a number of researchers working within the emotional appraisal framework (Huddy, Feldman, & Cassese, 2007; Lerner, Gonzalez, Small, & Fischhoff, 2003; Sadler, Lineberger, Correll, & Park, 2005; Skitka, Bauman, Aramovich, & Morgan, 2006). All of these studies suggest, in one way or another, that anger and anxiety are related in opposite ways to “hawkish,” high-risk policies of the government (e.g., decision to launch military strikes as part of an antiterrorism campaign). In particular, support for such policies is positively correlated with anger but negatively correlated with anxiety. This research has tended to focus more specifically on policy preference rather than on presidential approval per se. However, decisions to launch military strikes are precisely the sorts of decisions made by war-time presidents, and, as such, this research has obvious relevance to the psychological dynamics of rally effects. These considerations suggest a perspective of rally effects that is strikingly different from present social psychological theories of threat, especially that of the terror management and uncertainty management models. For one thing, emotion plays no direct role in attitude/belief change according to these formulations, whereas this is obviously not the case for emotional appraisal models. Also, the work by emotional appraisal theorists suggests that surges in support for the president may actually reflect motives relating to aggression (cf. Berkowitz, 1990), as opposed to issues of security or safety per se. As such, the appeal of the president in such contexts lies not in his capacity to make people safe/secure, but rather by the capacity of the president to retaliate against perceived aggressors.

Without question, the emotional appraisal literature has already offered strong evidence regarding the different consequences of anger versus anxiety for a number of different judgmental domains. Nevertheless, it is important to note that all but one of the aforementioned studies on the 9/11 attacks (Huddy et al., 2007; Lerner et al., 2003; Sadler et al., 2005; Skitka et al., 2006) used a purely correlational design. The one exception, that by Lerner et al. (2003), used an experimental design, but all participants were asked to think about the 9/11 attacks; the manipulation in question involved whether the experimenter gave participants specific instructions to focus on anger or anxiety. Hence, this study offers limited leverage on causality because there was no control group. Aside from the basic lack of causal evidence for the ideas under consideration, at least three interrelated but distinct ambiguities arise from the aforementioned studies.

First, people who report higher-than-average states of anger tend to be the same people who endorse authoritarian values, and vice versa (Jost et al., 2003; Tomkins, 1995). This means that an observed correlation between anger and support for “hawkish” policies on the part of the president may not actually reflect the influence of anger per se. Rather, it may reflect chronic and/or situational differences in authoritarianism. Alternatively, observed variation in people’s support of hawkish policies could reflect the combined influence of anger and authoritarianism. In order to fully disentangle these variables, one must use a design that is capable of assessing the causal impact of anger while controlling for authoritarianism, and vice versa. None of the studies cited above were capable of addressing this issue.

Second, one of the goals of our research was to understand the different kinds of spontaneous, unrestricted reactions that people have to reminders of the terrorist attacks. This is important because terrorist attacks can elicit different types of negative affect (e.g., anger, anxiety, sadness), and it is important to know which of these emotions naturally tends to “dominate” when people are reminded of these events, absent any specific prompts by the experimenter as to what kind of emotion they should be focusing on. None of the existing studies in this area were capable of assessing these spontaneous reactions, either because there was no basis of comparing emotional reactions across threat versus no-threat conditions and/or because of restrictions in the kinds of emotions that participants were supposed to feel depending on the condition to which they were assigned.
A third issue concerns the possible role of preexisting ideology as a moderator (cf. Nail, McGregor, Drinkwater, Steele, & Thompson, 2009). As noted earlier (see Footnote 1), there has been some debate on this point in the literature, but there is, at present, surprisingly little data that speaks specifically to the consequences of terrorist attacks, and how the dynamics of emotional mediation might (or might not) be dependent on a priori differences in ideology. To the extent that rally effects are driven primarily by anger, one possibility is that such effects might be strongest among conservatives, given that anger-related goals and motives are likely to be more consistent with the chronic predisposition of conservative, compared with liberal, individuals. Alternatively, it is possible that our findings would not be moderated by preexisting ideology at all. This idea was particularly intriguing to us given that our sample contained a large percentage of “superliberals” who walked into the laboratory with intense dislike toward the sitting president at the time, George W. Bush. For both theoretical as well as practical reasons, it would be important to show that brief reminders of the 9/11 attacks, years after the fact, could induce a significant increase in positivity toward the president by citizens completely lacking in any support for him. In other words, if there were ever a group of people who would not be likely to show evidence of increased support for the president after being reminded of the 9/11 attacks, it would be precisely the kinds of participants studied in our research.

The Present Research

As we have seen, there are several ambiguities surrounding rally effects, and many of these are relevant to more basic uncertainties regarding the consequences of threat on attitudes. For one thing, it is not completely clear why, exactly, rally effects occur in the first place. Do such shifts reflect a general “shift to the right” in general ideology, or do these effects reflect the more circumscribed consequences of making people angry? Several social psychological models of threat (see earlier discussion) suggest the former, but research and theory in the emotional appraisal area offer some indirect (i.e., correlational) evidence for the latter. A related ambiguity concerns whether emotional experience directly guides such attitude change. Terror and uncertainty management theories specifically postulate that emotion plays no direct role in this regard, but emotional appraisal models suggest that emotional experience could, in fact, directly mediate such change. Finally, it is unclear whether preexisting differences in ideology would moderate the impact of threat on attitudes. The present research was oriented toward addressing these and other related issues.

We report four experiments conducted over a span of 5 years (2003–2008). We experimentally manipulated the salience of the 9/11 attacks in Experiments 1 and 2 by randomly assigning half the participants to watch a video documentary of the events of that day; control participants completed a task unrelated to the 9/11 attacks. In Experiment 3, we manipulated salience of these memories in a different way by having participants personally reminisce either about the 9/11 attacks or about the mundane events of their typical day. In Experiment 4, we manipulated these emotions using mood induction techniques unrelated to the 9/11 attacks, which allowed us to test several alternative explanations and also demonstrate the generalizability of our findings apart from the 9/11 attacks per se.

Experiment 1: Fall 2003

Method

Participants and design. A total of 136 college undergraduates (47 men and 87 women; two participants failed to indicate their gender) participated in return for partial completion of course credit. The design consisted of one between-subjects factor, 9/11 prime versus control. None of the effects produced by experimental manipulations in this or any of the other three studies reported in the present article were contingent on gender (all Fs < 1.0), and hence results are collapsed over this variable.

Assessment of individual differences. Upon entering the laboratory, participants were escorted into separate cubicles, at which point they completed the Right Wing Authoritarianism (RWA; Allmeyer, 1988) scale. For each item, participants rated it using a scale ranging from −3 (strongly disagree) to +3 (strongly agree). A composite index of RWA was formed on the basis of an average of all 32 items in the scale (after reverse coding, where appropriate; \( \alpha = .93 \)).

Participants next completed a measure of political expertise. In particular, they were presented with a set of 16 public figures for participants to identify, which (at the time the study was conducted) would capture to a reasonable degree our participants’ tendency to follow national and international politics, defined broadly: Vladimir Putin, Tony Blair, Kofi Annan, Dick Cheney, Colin Powell, Condoleezza Rice, Donald Rumsfeld, John Ashcroft, Ari Fleischer, John Kerry, Arnold Schwarzenegger, Dick Gephardt, Hillary Clinton, Jeb Bush, John Edwards, and Ariel Sharon. Next to each name was a blank line, and for each name participants were asked to fill in the blank with the name of that person’s occupation; participants were encouraged to make their best guess if they were not sure of the person’s exact title. One point was awarded for a completely correct response; a half point was awarded if the participant seemed to have a general understanding of the identity of the person in question. Analyses indicated moderate expertise on average (\( \text{Mdn} = 9.0 \)), but scores ranged widely, from .5 to 16 (\( \text{SD} = 3.60 \)).

Experimental manipulation. Participants were then randomly assigned to complete one of two tasks. Participants in the control condition were given 10 min to complete a set of anagrams, whereas those in the 9/11 condition were assigned to watch an initial portion of a CNN-produced DVD entitled America Remembers (CNN, 2002). The video included (a) “background” shots of the CNN headquarters in the morning prior to the attack followed by (b) brief stories told by members of the media or other individuals prior to, during, and after the attack, and (c) scenes of the towers themselves, including footage of the planes crashing into them, followed by their eventual collapse. This portion of the film did not contain footage of, or any allusions to, overt patriotic symbols (e.g., the American flag), George Bush, or to other aspects of the United States government. At the 8-min and 40-s mark, the experimenter reentered the room and stopped the DVD, and then restarted the computer program, which began with an assessment of emotion.

Assessment of emotion. Participants were then presented with 25 mood adjectives, in a different randomized order for each participant: interested, bored, edgy, happy, alert, irritated, satisfied, mad, upset, tense, sad, pleased, relaxed, unhappy, angry, determine, irate, dejected, anxious, comfortable, jittery, nervous,
worried, confident, calm. Participants were told to circle the number that best described how they felt at that moment using a scale that ranged between 0 (not at all) and 5 (very much so). Across all of the studies, principal component analyses consistently revealed two distinct components corresponding to the dimensions of anger and anxiety. For the anger component, the highest loading items on this dimension, and the items that elicited the highest internal reliability, included angry, mad, and irritated. For the anxiety component, therapist composite included (after reverse scoring, where appropriate) the items relaxed, anxious, worried, nervous, calm, tense, and comfortable. Both indices had excellent internal reliability (both αs = .87) and were significantly correlated with one another (r = .69, p < .001).

**Attitude assessment.** In an initial block of judgments, participants expressed their reaction toward a list of 33 attitude objects, presented in a different randomized order for each participant: George W. Bush, Donald Rumsfeld, Dick Cheney, conservatives, liberals, Republicans, United States, Statue of Liberty, American flag, Congress, capitalism, democracy, increases in military spending, U.S. involvement in Iraq, military draft, U.S. Army, the Central Intelligence Agency (CIA), the Federal Bureau of Investigations (FBI), feminists, gays/lesbians, Muslims, Jews, government-sponsored health care, affirmative action, prayer in public schools, women’s right to abortion, African Americans, Asians, Nazis, Ku Klux Klan, communism, Saddam Hussein, and Osama bin Laden. Participants evaluated each of these along a scale ranging from −3 (not at all favorable) to +3 (extremely favorable). In a second block, participants were asked to express their opinion toward a randomized series of statements using the war in Iraq: “It’s important that we all support our President during this difficult time”; “I am strongly in favor of going to war against Iraq”; “America did not have the right to invade Iraq”; “No one ‘likes’ war, but in this case, it was necessary to invade Iraq”; “Well-meaning protesters don’t realize that war is sometimes necessary to protect our freedom”; “If someone asked me to march in an anti-war rally next week, I would probably go.” After completing these tasks, participants were debriefed, thanked for their participation, and dismissed.

**Formation of attitude indices.** Principal components analyses on all of the items from both blocks revealed a strong primary factor, which included general ratings of Bush along with several attitude objects relevant to the war in Iraq and general sentiments toward the military. This strong coupling of attitudes toward Bush and people’s support for the war was not surprising, but for the present purposes, it was important to form a separate index that roughly approximated a “general approval rating” of the sort assessed by national polls. Hence, a Bush Approval index was formed on the basis of an average of attitude ratings of George W. Bush and their response toward the item In general, I am happy with George Bush’s performance as President (α = .85). A separate Pro-War index was formed on the basis of increased military spending, U.S. involvement in Iraq, and the remaining eight war-related statements described above, after reverse coding where necessary (α = .89). A separate component clearly represented patriotic attitudes toward the United States and iconic images associated with it; a Patriotic Symbols index was formed on the basis of averaging across three high-loading items (United States, Statue of Liberty, American flag; α = .85). Finally, an additional component corresponded to conservative/liberal politics, and thus a Political Attitudes index was formed after averaging across three highly loading items (prayer in public school, and reverse-scored gays/lesbians and legalized abortion; α = .92). Higher numbers on this index indicate more proconservative views.

Table 1 shows the intercorrelations among these composites, along with their relation to individual differences in RWA and political expertise. As seen here, all four attitude indices and RWA tended to be correlated with each other, a pattern that confirmed the common grounding of these measures in contemporary American conservatism. Expertise was not correlated with any of the attitude indices, although expertise did show a modest tendency to be negatively correlated with RWA.

**Results**

**Emotional reactions to the video.** Compared with participants in the control condition, participants in the video condition reported significantly higher ratings of anger (M = 2.62 vs. 1.10), F(1, 134) = 55.00, p < .001, as well as anxiety (M = 2.75 vs. 1.60), F(1, 134) = 49.44, p < .001. Supplemental analyses using multiple regression showed that neither political ideology (as assessed by the RWA scale) nor political expertise moderated the impact of the video on anger or anxiety. Hence, participants reacted in similar ways to the video, regardless of differences in general ideology or political expertise. We measured individual differences in political expertise in all of the remaining experiments, but this variable did not moderate any of our findings. Thus, we do not consider this variable in any of the analyses to be reported in this article.

**Initial analyses.** Figure 1 displays the patterns of attitudes as a function of experimental condition. Univariate analyses revealed significant effects for the (a) Bush approval index, F(1, 130) = 11.76, p < .001; (b) Pro-War index, F(1, 130) = 6.07, p < .01; and (c) Patriotic symbols index, F(1, 130) = 4.60, p = .05, with all three indices showing higher levels among participants assigned to the 9/11 condition. However, there was no evidence of

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<td>5. RWA</td>
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<td>6. Political expertise score</td>
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*Note.* Values represent zero-order correlations among variables, collapsed over experimental condition. Political attitudes scored such that higher numbers indicate greater preference for conservatism. RWA = right-wing authoritarianism.

*p < .01. ***p < .001.
any differences on the Political Attitudes index \((F < 1.0)\). (Because this index was not affected by the experimental manipulation, analyses involving it are not considered further in this study.) The different effects of the manipulation across different indices was confirmed by a multivariate analyses, which revealed a significant Manipulation \( \times \) Attitude Type interaction, \( F(3, 128) = 3.30, p < .05 \).

**Tests for moderation.** Although preexisting ideology did not moderate affective reactions to the video, a distinct question is whether it moderated the impact of this manipulation on attitudes. To this end, we conducted a series of regression analyses in which we first entered the main effects of the 9/11 prime and the continuous variable of RWA, followed by the interaction between these two factors. None of these analyses revealed any interaction effects (all \( ps > .25 \)), indicating that participants were affected in similar fashion, regardless of whether they were relatively liberal or conservative. For illustrative purposes, it is useful to plot the effects of the 9/11 manipulation on the attitude indices separately for different “categories” of participants along the liberal-conservative spectrum. These results are shown in Figure 2, which displays attitudes as a function of a quartile split on the RWA scale. As seen here, the experimental manipulation exerted an independent effect of equal magnitude, independent of and in addition to the effects of participants’ own political ideology.

**Regression analyses.** As noted above, the manipulation had a significant impact on anger and anxiety along with three of the attitude indices (Bush approval, Pro-War, and Patriotic symbols). This raises the possibility that anger and/or anxiety may have mediated, at least, in part, the effect of the experimental manipulation. Preliminary analyses revealed a virtually identical pattern of results for the Bush approval and the Pro-War indices, and so to avoid redundancy, we combined the first two indices into a single Bush/Pro-War index. Figure 3 summarizes the relevant paths involving the Bush/Pro-War composite. As seen here, (a) anger, but not anxiety, was clearly responsible for these attitudinal shifts and (b) effects of the manipulation was reduced, although did not disappear, once anger was controlled for. Although not shown in Figure 3, a similar but weaker pattern emerged for the Patriotic symbols index. There was a statistically significant effect of the experimental manipulation on the Patriotic symbols index \((\beta = .18, p < .05)\). In these analyses, there was a small positive effect of anger on the Patriotic symbols index \((\beta = .15)\), which was weakly reversed for anxiety \((\beta = -.06)\). Although the magnitude of the anger effect was modest, the direct effect of the manipulation was reduced in magnitude and no longer significant once anger was controlled for \((\beta = .11)\).

**A note on estimating indirect effects.** Given the type of findings reported in Figure 3, one would normally generate estimates for the indirect effects of anger using either Sobel tests or bootstrapping techniques (Preacher, Rucker, & Hayes, 2007). However, the situation here is complicated by the fact that anger was strongly correlated with anxiety, but the sign of the anxiety effect was opposite to that of anger. However, estimates of multiple indirect effects require the use of structural equation modeling techniques, an approach that was not feasible for most of the studies in this article due to their relatively small sample sizes (Preacher et al., 2007). Nevertheless, this limitation is less problematic than might otherwise be the case, given that one of our main goals was to compare and contrast the different roles of anger versus anxiety. In this regard, the analysis that is arguably of greater interest to formally test whether the coefficient of anger was significantly different from that of anxiety. A \( t \) test of the difference in regression coefficients (using the pooled estimate of error) confirmed that this was indeed the case for the analyses shown in Figure 3 \((p < .01)\). Hence, in each of our remaining studies, we use an analytic technique similar to that used here.

**Discussion**

Experiment 1 yielded four major findings. First, reminders of the 9/11 attacks led to significantly more favorable attitudes toward the president, a general increase in hawkish, “pro-war” sentiments, as well as increased positivity toward the United States and iconic objects associated with it. Second, although the experimental manipulation had a strong and significant impact on anger and anxiety, these effects on attitude were entirely mediated by anger. Indeed, once anger was controlled for, there was a small tendency for anxiety to be related in the opposite way to these attitudes. This finding is consistent with several studies in the emotional appraisal area (Huddy et al., in press; Lerner et al., 2003; Sadler et al., 2005; Skitka et al., 2006), but, unlike those studies, we provide causal (as opposed to correlational) evidence in this regard. Third, these effects arose independent of a priori differences in participants’ general levels of political ideology, which had a strong impact in its own right (cf. Figure 2). Fourth, the experimental manipulation had no effects on an index of conservative political attitudes. In other words, although reminding participants of the 9/11 attacks increased their support for a politically conservative president (and boosted their support for his war-time policies in Iraq), these effects did not generalize to our measure of general political attitudes.

**Experiment 2: Fall 2004**

Nearly all of the questions about the president in Experiment 1 were concerned with issues of a military nature. In other words,
attitudes toward the president were almost completely confounded with promilitary attitudes and vice versa. This raises an important question: Did our reminder of the 9/11 attacks produce a general increase in support for Bush, which would extend to any issue or policy directly connected to the president? Or, alternatively, did our findings reflect a narrower boost in support for just one aspect of the presidency, namely, his connection to and support of pro-military policies?

Figure 2. Effects of the 9/11 manipulation on the attitude indices along the liberal-conservative spectrum in Experiment 1. RWA = right-wing authoritarianism.

Figure 3. Relevant paths involving the Bush/Pro-War composite in Experiment 1. All values shown are standardized beta coefficients. Values in parentheses reflect the simple relation of the variables in question, not controlling for the other emotion. *p < .10. **p < .05. ***p < .01. ****p < .001.
This issue is related to a broader ambiguity in the polling literature. When people’s attitudes toward the president are assessed, these queries are typically framed in an extremely broad manner, including the ubiquitous “approval ratings,” which typically offers respondents one of three choices (approve, disapprove, no opinion), as well as global “thermometer ratings.” It can be difficult to interpret responses made to such queries. For example, if a person expressed approval for the president in a general poll in (say) October of 2004, it is not clear how much of this approval reflected specific appraisals of Bush’s handling of the war in Iraq (an issue likely to be highly salient at the time) as opposed to a generalized support for the president.

One approach to this issue is to include, in the same judgmental setting, a wider range of questions spanning different dimensions of people’s impressions of the president. If rally effects involve a general surge in support for the nations’ leader, one should find broad-band support for the president across these different dimensions. In our case, participants who had been primed with memories of the 9/11 attacks should show more support for any issue connected to the president, regardless of whether it pertained to matters of a military nature or not. The plausibility of such broad support rests, in part, on research on halo effects, in which the presence or introduction of one positive attribute increases the favorableness of one’s impressions of the person as a whole (e.g., Dion, Berscheid, & Walster, 1972).

Alternatively, rally effects might reflect a more “narrow” phenomenon restricted to the domain of the president’s military policies. There is some scattered evidence in the political science literature that is consistent with that view. For example, Lindsay and Smith (2003) made some interesting observations of George W. Bush in the period following the 9/11 attacks, when he was enjoying some of the highest presidential approval ratings in history. In particular, in the period following the 9/11 attacks:

Democrats blocked his economic stimulus plan, rejected his proposal to drill for oil in the Arctic National Wildlife Refuge, and stonewalled his judicial nominees even though U.S. forces had orchestrated the rout of the Taliban. Indeed, even as bombs were falling on Iraq, Bush rediscovered that a Congress that is deferential abroad can be defiant at home. On the second day of the war, the Republican-controlled Senate voted once again to kill his proposal to drill for oil in the Arctic. It later voted to cut his $726 billion tax cut in half. (Lindsay & Smith, 2003, p. 23).

Lindsay and Smith (2003) noted similar problems besetting Bush’s father (George H. W. Bush) in the period immediately following the Gulf War of 1991, in which the senior Bush was riding a similarly strong wave of popularity (see also Hetherington & Nelson, 2003). Hence, aside from the obvious benefits of replicating and extending our findings 1 year later, one of our goals in Experiment 2 was to investigate whether increased support for the president following the 9/11 manipulation would, or would not, generalize to increased support for the president’s policies and stances in nonmilitary domains.

**Method**

**Participants and design.** A total of 48 undergraduates (12 men and 35 women; gender was not recorded for one participant) participated in return for partial completion of course credit. The design consisted of one between-subjects factor, 9/11 prime versus control, which was identical to that of our first study. As in Experiment 1, individual differences in political ideology and political expertise were assessed prior to the manipulation. However, regression analyses revealed no moderator effects involving these variables and, hence, are not considered further.

**Experimental manipulation and measurement of emotion.** The two experimental conditions as well as measurement/coding of emotion were identical to that of Experiment 1.

**Assessment of attitudes.** Participants next completed a block of judgments in which they responded to a number of randomized statements pertaining to Bush’s policies. Some of these amounted to presidential approval ratings or appraisal of the president’s role in military matters, similar to those in Experiment 1: To what extent do you think Bush has the leadership skills to combat terrorism?; To what extent do you think Bush has the skills to inspire the American public?; To what extent do you think Bush is a good president?; To what extent do you think Bush has the skills needed to be America’s leader?; To what extent do you think Bush has the skills needed to negotiate with other world leaders?; Does the president’s attitude toward terrorists reflect your own attitude?; Does the president’s approach toward the war on terror reflect decisions and actions you would have taken?; To what extent do you think Bush took appropriate actions immediately after the events of 9/11?; Do you agree with the Bush’s decision to go to war in Iraq? In addition, however, a number of items addressing specific non-military related issues were included: Do you approve of Bush’s approach toward environmental issues?; To what extent does Bush’s attitude toward gays reflect your own attitude?; To what extent do you think Bush has the skills to address America’s economic problems?; To what extent do you approve of Bush’s approach toward tax cuts? Participants responded to each of these all along a scale ranging from 0 (not at all) to 5 (very much so).

Principal components analyses on the 14 Bush-related items revealed three factors with eigenvalues greater than 1.0. The first component represented approval of war-related issues, and a War policy index was formed on the basis of five items that loaded highly on it: Do you agree with the Bush’s decision to go to war in Iraq?; To what extent do you think Bush has the leadership skills to combat terrorism?; Does the president’s approach toward the war on terror reflect decisions and actions you would have taken?; To what extent do you think Bush took appropriate actions immediately after the events of 9/11?; Does the president’s attitude toward terrorists reflect your own attitude? (α = .92). A second component concerned appraisals of Bush with respect to his stand on liberal positions, and a Liberalism policy index was formed after averaging the two highest loading items on this factor, To what extent does Bush’s attitude toward gays reflect your own attitude?; Do you approve of Bush’s approach toward environmental issues? (α = .94). A third component captured general support for Bush on which the following items loaded highly: To what extent do you think Bush has the skills to inspire the American public?; To what extent do you think Bush has the skills needed to be America’s leader? Along with the item To what extent do you think Bush is a good president (which loaded moderately on all three components), these items were used to form a General approval index (α = .88).
Results

Mood. Compared with participants in the control condition, those assigned to the 9/11 video condition reported higher levels of anger (Ms = 2.68 vs. 0.42), F(1, 46) = 63.73, p < .001, and anxiety (Ms = 2.85 vs. 1.50), F(1, 46) = 30.47, p < .01.

Analyses of attitudes. Figure 4 shows the pattern of means for the three attitudinal indices as a function of experimental condition. As seen here, the 9/11 video led to higher ratings of Bush with respect to the War Policy and General Approval indices, but not the Liberalism policy index. This was confirmed by univariate analyses, which showed significant effects for the War policy index, F(1, 45) = 4.96, p < .01, and the General approval index, F(1, 45) = 5.36, p < .01, but not the Liberalism policy index (F < 1.0). This asymmetry was responsible for a Manipulation × Attitude Index interaction, F(2, 44) = 2.95, p = .06.

Regression analyses. Analyses with the War policy index revealed (a) a direct effect of the experimental manipulation (β = .27, p < .05), (b) a significant effect of anger, controlling for anxiety (β = .51, p < .05), and (c) the effect of the experimental manipulation was no longer significant once anger and anxiety were controlled for (β = −.03, ns). In these analyses, the effect of anxiety (controlling for anger) was opposite in sign to anger (β = −.13). Although the anxiety effect was not significant, it was significantly different from that of anger (p < .01). A similar but somewhat weaker pattern emerged with the General approval index, as analyses revealed (a) a direct effect of the experimental manipulation (β = .28, p < .05), (b) a trend for anger, controlling for anxiety (β = .18, ns), and (c) the effect of the experimental manipulation was no longer significant once anger and anxiety were controlled for (β = .10, ns). Here, too, the effect of anxiety was noticeably (albeit not significantly) different from that of anger (β = .01).

Discussion

Experiment 2 replicated and extended several implications of Experiment 1. As in our earlier study, we showed that reminders of the 9/11 attacks had the capacity to reliably impact attitudes, even years after this event occurred. However, the impact of these memories was entirely mediated by anger, but not by anxiety. Indeed, as in Experiment 1, we once again found a small tendency for anxiety to produce a pattern of results opposite to that of anger. Also paralleling our first study, reminders of these attacks had no impact on participants’ more general liberal or conservative policies not pertaining to war policies.

The fact that our manipulation affected presidential approval independent of changes in more general ideological preferences appears to be somewhat at odds with several social psychological theories of threat, including terror management theory (Pyszczynski et al., 2003), uncertainty management theory (Van den Bos et al., 2005), research on authoritarianism and threat (Doty et al., 1991), and a motivational model of conservatism by Jost et al., 2003. All four of these formulations suggest that reminders of the 9/11 attacks should have produced broad-based changes in ideology, but neither of our experiments revealed any hint of this effect. In contrast, our findings fit quite well within the basic framework of emotional appraisal theory (Lerner & Keltner, 2000). In particular, the kind of threat under investigation here can be “deconstructed” into at least two affective components, one involving anger and one involving anxiety. In these two experiments, at least, the consequences of reminding participants of the 9/11 attacks was dominated by anger insofar as the attitudes that ultimately were affected were those that were related to this emotion in particular.

Experiment 3: Fall 2005

Experiments 1 and 2 relied on a “prepackaged” reminder of the 9/11 attacks in the form of a video documentary. Even though no part of the video contained any overt political content, it would not be far-fetched to suggest that part of the (explicit or implicit) motivation in producing this video was to bolster American’s spirit following the 9/11 attacks. Moreover, this could have been done in any number of ways (e.g., involving background music). This raises the possibility that our results have as much or even more to say about people’s reactions to the video than about the consequences of remembering the 9/11 attacks. To rule out this alternative, participants were given instructions to write a short essay describing their own personal thoughts/feelings surrounding the 9/11 attacks, a manipulation that was identical to that used by researchers in the terror management area (Landau et al., 2004; see below for details). A control group was instructed to write about “the mundane events that typically occur during an average day.” Choice of this manipulation was guided, in part, by a desire to more directly contrast predictions made by a terror management versus an emotional appraisal approach. In particular, Landau et al. reported a null effect of their manipulation on negative emotion, but this result could have been driven, in part, by the fact that their measure of negative affect lumped together different types of emotion, some of which (e.g., guilt, shame) may have had little if any relation to participants’ feelings about the 9/11 attacks.

Method

Participants and design. A total of 52 undergraduates (16 men and 36 women) participated in return for partial completion of course credit. Participants were randomly assigned to one of two
conditions (control essay vs. 9/11 essay). As in Experiment 1, individual differences in RWA and political expertise were measured, but neither of these variables moderated any of the effects of the experimental manipulations.

Experimental manipulations. Participants assigned to the 9/11 condition were given instructions identical to that given to participants in a recent study by Landau et al. (2004; Study 3), specifically, to (a) “describe the emotions that the thought of the terrorist attacks on September 11, 2001 arouses in you” and to (b) “write down as specifically as you can, what happened during the terrorist attacks on September 11, 2001.” A control group of participants were instructed to write in as much detail as possible about “the mundane events that typically occur during an average day.” In both conditions, participants were given approximately 10 min to complete their brief essays. Following this task, participants completed the same battery of measures that were used in Experiment 1.2

Results

Analyses of mood ratings. Participants assigned to write about the 9/11 attacks reported significantly higher levels of anger compared with participants who wrote about the mundane events in their day ($M_s = 1.69$ vs. $0.84$), $F(1, 50) = 7.18, p < .01$. There was a weak tendency for participants to report higher levels of anxiety in the former compared with the latter condition ($M_s = 1.81$ vs. $1.40$), but this effect was not reliable ($p = .18$).

Analyses of attitude composites. Two key findings were replicated from Experiment 1. First, participants expressed relatively more favorable (or, more precisely, less unfavorable) ratings of the president on the Bush approval index if they had been assigned to the 9/11 memory condition than if they were not ($M_s = -0.78$ vs. $-1.76$), $F(1, 49) = 3.91, p < .05$. A similar finding emerged for the Pro-war index ($M_s = -0.44$ vs. $-1.28$), $F(1, 49) = 4.42, p < .05$. In contrast to the first experiment, we found no reliable effects with the Patriotic symbols index, although these data were in the same direction as the previous study ($M_s = 1.97$ vs. $1.61, ns$). As in Experiment 1, there were no effects involving the conservative attitudes index ($F < 1.0$).

Regression analyses. As in Experiment 1, analyses revealed a virtually identical pattern for the Bush approval and Pro-war indices, and hence we again combined these two composites into a single Bush/Pro-War index. Although anxiety was not affected by the manipulation in this study, this emotion was strongly correlated with anger. Hence, in order to more effectively show the dissociable effects of these two emotions (and to facilitate comparison with Experiment 1), we show in Figure 5 the paths associated with the experimental manipulation, the two emotion indices, and this composite attitude index. Once again it was anger, not anxiety, that was associated with people’s attitudes toward the president and his war policies. As in Experiment 1, controlling for emotion reduced but did not eliminate the effect of the manipulation. On the basis of our a priori expectations (as well as the results of Experiment 1), we anticipated and found that the relation of anger to the outcome variable would be significantly different from that of anxiety, and a one-tailed $t$ test of regression weights confirmed this expectation, $t(47) = 1.76, p < .05$.

Discussion

In Experiment 3, we used a personal reminiscence technique to remind participants of the 9/11 attacks, which allowed participants complete latitude to write about anything they wanted with respect to the 9/11 attacks. This approach was markedly different than the way that we had primed memories of the 9/11 attacks in Experiments 1 and 2, which had exposed participants to a professionally produced video. Despite this difference in methodology, results obtained here generally converged on several key aspects of our earlier findings, including the key role of anger, apart from anxiety.

Our reminder of the 9/11 attacks was identical to that used by terror management theorists (cf. Landau et al., 2004) but our findings are again best understood by deconstructing threat into distinct albeit correlated elements of affective experience. Once again, our findings reflected the consequences of activating anger, apart from other sorts of negative experience such as anxiety. This sort of interpretation naturally fits within the framework offered by emotional appraisal models, but is less compatible with the tenets of terror management theory. Moreover, our findings showed no effect of our experimental manipulation on conservative attitudes, another finding that is seemingly at odds with the predictions of terror management. Nevertheless, it is worth noting that the experimental manipulation had a direct effect on attitudes toward the president, even after controlling for emotion. Hence, although our findings clearly indicate an important role of emotion, these findings did provide some hint that the impact of our manipulation may have also involved more “cognitive” processes, independent of and in addition to those involving affective experience. We return to this issue later in this article.

Experiment 4: Fall 2008

In interpreting the findings from the first three studies, we have assumed that emotion plays a role in temporarily changing the inherent favorableness of any given piece of information, in accordance with whatever goals or motives active at the time. One way to frame this process is in terms of “value trade-offs.” Solomon Schwartz (1992) and others have suggested that social judgment often entails an implicit or explicit balancing of competing values (e.g., freedom vs. security; approach vs. avoidance). Hence, anger could have played a role by increasing the favorableness of motives such as aggressiveness or risk taking, over other potential desiderata (e.g., diplomacy, risk aversiveness). One could also frame our model in terms of information-processing

2 In this experiment, there was a tendency for moods associated with general unhappiness (e.g., sad, dejected, unhappy) to be negatively correlated with the attitude indices. This constitutes a type of mood congruency effect identified in previous work by Schwarz and colleagues (Schwarz, 1990). However, because unhappiness was strongly and positively correlated with anger ($r = .60, p < .001$), the effects of unhappiness served as a suppressor variable, partially masking the effects of anger on the attitude indices. It is not clear why this sort of strong mood effect occurred here but not in the other studies (some experiments showed hints of this effect, but they were much weaker). To control for this suppressor effect in this experiment, we included unhappiness as a covariate in the relevant analyses; the pattern of results without taking unhappiness into account was similar to but somewhat weaker than the findings reported ahead.
models (Wyer & Srull, 1989), in which emotion affects the way that people encode and integrate information prior to the final judgmental output. Either way, however, both perspectives presume that everyone is processing (more or less) the same information.

Nevertheless, there is an alternative explanation of our findings that does not involve differential construal at all. Rather, one could argue that our effects involved the selective retrieval of information from long-term memory. For example, our manipulation could have elicited positive memories of the President (e.g., those associated with speeches made immediately after the 9/11 attacks) that were relatively inaccessible for participants in the control condition. It is important to take this perspective seriously, because it calls into question whether our manipulations were actually producing any bona fide change in these attitudes at all (cf. Scherer & Lambert, 2009). In other words, it could be that people were formulating different opinions of the president because they were retrieving different information about him. Moreover, in light of research and theory on the relation between memory and emotion (Blaney, 1986), it is possible that emotion played an important role in this sort of selectivity. For example, there are cognitive as well as motivational reasons why anger might have led to the increased accessibility of memories consistent with an image of the president as an effective military leader. To the extent that such images are more favorable than other “competing” images of the president (e.g., as indecisive or weak), this could well explain much or even all of our previous findings.

In order to address this issue, participants were presented with descriptions of ostensibly real prowar (“hawkish”) politicians about whom participants had no prior knowledge. (Unbeknownst to our participants, these politicians were, in fact, fictitious.) This meant that participants’ impressions of the candidates were limited to the information that was actually presented to them in the context of this experiment. Hence, the issue of “selective retrieval” (i.e., differential recall of memories about this person in particular) is moot in this case, because—unlike the president—no such memories existed in the heads of our participants in the first place. Nevertheless, we expected to replicate and extend our previous findings, showing strong support for this novel target via the activation of anger, but not anxiety.

### Beyond 9/11: Direct Experimental Induction of Affect

Concern about selective retrieval of “political” information stored in long-term memory are also exacerbated by the fact that our previous studies primed memories of the 9/11 attacks, an event strongly infused with political dynamics directly relevant to our attitude measures (e.g., attitudes toward the use of military force by the government). In our view, the effects we obtained in Experiments 1–3 were due to the activation of anger per se, not to the activation of memories associated with the 9/11 attacks. Hence, our framework suggests that we should obtain similar effects of anger, even when the initial source of that affect was completely unrelated to politics. To this end, we used a task that was designed to directly elicit anger by having participants remember an event in their life in which they were treated extremely unfairly. We expected this to be an excellent way of eliciting anger, given that one of the most well-documented triggers of anger is the perception that rules of justice and fair play have been violated (Keltner, Ellsworth, & Edwards, 1993). Using this task in conjunction with a control condition (identical to that used in Experiment 3) was expected to provide stronger leverage for our framework.

### Additional Considerations

Although none of the effects involving anxiety in the first three studies were strong, our data provided several hints that anxiety might be exerting some effects opposite to that of anger. This evidence was most apparent in Experiments 1 and 2, which showed a modest but reasonably consistent pattern of negative correlation between anxiety and attitudes toward politicians/policies associated with the Iraq war. Although this effect was not always in evidence (cf. Experiment 3), this finding is compatible with previous studies that have shown that anxiety tends to be associated with a lack of support for hawkish policies, a pattern opposite to that observed with anger (Huddy et al., in press; Lerner et al., 2003; Sadler et al., 2005; Skitka et al., 2006). As noted earlier, however, these studies were not able to generate the kind

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*This point is related to a larger issue in the literature that has been concerned with the effects of affective experience on judgment. Experimental induction of emotion/mood often involves activation not only of the desired affective state but also of associated cognitions. Hence, this can sometimes introduce ambiguity as to whether the observed effects reflect the impact of the emotion per se, the cognitions associated with that experimental manipulation, or both. There have been a number of techniques used to address this issue, including those developed by Schwarz and Clore (1983); Strack, Schwarz, and Gschneidinger (1985), and Keltner et al. (1993).*
of data needed to make causal conclusions, including the effect of anger versus anxiety on attitudes.

There were a number of reasons why our studies did not provide optimal opportunity for testing the different consequences of anger versus anxiety. For one thing, all of our studies perfectly confounded prowar attitudes with participants’ sentiments toward the president. Our understanding of anxiety was also hindered by the fact that the experimental manipulations used in Experiments 1–3 appeared to be more effective at activating anger, as opposed to anxiety. Hence, in order to gain leverage on the predicted disassociation between anger and anxiety, we used a third condition in our experiment, one that was designed to elicit anxiety as opposed to anger. In contrast to anger, we predicted that activation of anxiety should lead to decreased, not increased, support for a prowar candidate.

**Method**

**Participants and design.** Participants consisted of 131 undergraduates (52 men and 65 women; four participants did not indicate their gender) who completed the experiment in return for partial course credit or for payment of $10. Participants were randomly assigned to one of three conditions (uncertainty, justice violation, control).

**Experimental manipulations.** The control condition was identical to that used in Experiment 3. The induction of emotion was patterned after a procedure used by Van den Bos et al. (2005), except that the focus of what participants were directed to think about during the task was varied. As for the justice violation condition, participants were asked to describe the emotional/physical sensations they felt at a time in their life in which they were treated unfairly. The format of the uncertainty induction task was very similar, except that participants were asked to describe the sensations they felt at a time in which they felt uncertain.

**Assessment of emotion/cognitive uncertainty.** After the manipulation, participants completed a measure of emotion similar to earlier studies in the present article, but the list with items relevant to anxiety (uncertain, unsure, indecisive, hesitant, insecure) were augmented in order to strengthen measurement of this construct. An anxiety composite was thus based on an average of these items along with several of the old items (anxious, comfortable, jittery, worry, nervous, confident, calm) after reverse coding where appropriate. The reliability of this index was identical to that of the anger composite (which was, as in the earlier studies, based on an average of mad, angry, and irritated; both as = .89).

**Assessment of attitudes.** Participants next completed a task in which they were informed at the outset that they would be presented some speeches, each one made by a different politician in the context of a political debate. Participants were further instructed to read each speech carefully and to consider their own feelings and attitudes toward it. Each participant was then presented with a description of two different politicians; the order of presentation was determined randomly for each participant. Each description was presented on its own computer screen, which consisted of a small (3 × 4 cm) picture of a White man along with the politician’s name (“Frank Jenkins”), and some mundane details about him (age, years in office, district). Party affiliation was not mentioned. Immediately below this information was a short passage, ostensibly an excerpt taken from one of his recent speeches.

In both cases, the politician’s speech clearly and unambiguously represented a strong “prohawk” position (although the exact manner in which these positions differed somewhat across the two politicians). Below is a sample of one of the speeches.

Historically, the United States has never shied away from using military force when it’s in our best interest to do so. There’s no reason to change this policy now. The enemies of the United States are determined to acquire their own weaponry to compete with our own military force. No one “likes” war, but we should not hesitate to employ force when needed. Thus, when the time comes, we must be prepared to send our troops overseas in order to dismantle the nuclear capabilities of rogue nations and, if necessary, remove corrupt dictators. The best way to accomplish these goals is through decisive and powerful military action. By crushing the known enemies of America, we can maintain our dominance as an effective military superpower well into the 21st century. Our strength comes from force, not diplomacy.

After reading both descriptions, participants were then presented with the full set of information about the first politician they had seen and were asked (with all of the information about that politician, including the speech, still under display) to rate how much they agreed with the speech along a scale ranging from +4 (strongly agree) to −4 (strongly disagree). After making a response on the keyboard, participants were asked (again, with all of the information about the politician displayed) to indicate their overall impression of the candidate along a scale ranging from +4 (extremely supportive) to −4 (not at all supportive). This procedure was then followed for a second politician, who was described as supporting a similar set of militaristic policies. Although we were initially interested in comparing and contrasting participants’ impressions of the two candidates, there were no differences between the two candidates in terms of how the experimental manipulations affected participants’ impressions of them. Hence, in the analyses below, we average across ratings of the two candidates. Also, because the two attitudinal ratings (agreement, support) were very highly correlated (rs > .75), an overall composite of attitude was formed on the basis of the average of these ratings.

**Results**

**Anger ratings.** Anger was highest among participants assigned to the justice violation condition (M = 2.16), lower in the uncertainty condition (M = 1.37) and lowest in the control condition (M = 1.07), F(2, 128) = 8.69, p < .01. Contrast analyses indicated that the justice violation condition was different from the control as well as from the anxiety induction conditions (both ps < .01), the latter of which did not differ from each other (p > .25). This indicates that anger was effectively elicited only by the justice violation task.

**Anxiety ratings.** Initial analyses indicated a main effect of experimental manipulation on anxiety, F(2, 128) = 3.55, p < .01. This effect was due, in part, to the fact that anxiety in the uncertainty condition (M = 2.18) was significantly different from the low levels of anxiety in the control condition (M = 1.74), F(1, 128) = 4.13, p < .05. Nevertheless, it is also true that the levels of anxiety in the justice violation condition were high and not distinguishable from that in the uncertainty condition (M = 2.20),
**Additional analyses.** One implication of the preceding analyses is that the justice violation condition primarily elicited anger and that any "peripheral" effects of the justice violation task on anxiety was actually elicited by the experience of anger. In other words, participants in the justice violation task may have been anxious because they were angry. This leads to two testable predictions. First, controlling for anxiety, the high levels of anger in the justice violation condition should remain (as they did in the analyses above), with lower levels of anger in the other two conditions. This was in fact the case: Controlling for anxiety, anger remained highest in the justice violation condition (adjusted M = 2.05), with low levels in the uncertainty and control conditions (adjusted Ms = 1.27 and 1.31, respectively). Contrast analyses on these adjusted means confirmed that the anger in the justice violation condition differed from the other two, $F(1, 127) = 14.74, p < .01$.

Of greater interest, controlling for anger should lead to a state of affairs in which the highest level of anxiety should emerge for the uncertainty condition compared with the other two conditions. This is what the data show. After controlling for anger, anxiety was highest in the uncertainty condition (adjusted $M = 2.25$) and different from anxiety in the justice violation and control conditions, which did not differ from one another (adjusted Ms = 1.96 vs. 1.93, respectively). Contrast analyses pitting the uncertainty condition against the control and justice conditions confirmed that the level of anxiety in the uncertainty condition was significantly higher than the other two, $F(1, 127) = 4.76, p < .05$.

**Regression analyses.** Given our interest in comparing anger versus anxiety (and to facilitate comparisons with the previous studies), we again show the unique effects of anger and anxiety in driving changes in the outcome variable. In Figure 6, we present the results of two parallel sets of analyses, each focusing on the manipulation relevant to the emotion under consideration. In one set of analyses, we contrasted participants assigned to the justice violation versus the control condition only; these analyses are most relevant to understanding the effects of anger. In another set of analyses, we contrasted participants assigned to the uncertainty induction versus control, the analyses most relevant to understanding the effects of anxiety.

**Justice violation versus control.** As seen in the top half of Figure 6, the justice violation manipulation (vs. control) led to a significant increase in anger, and this rise in anger led, in turn, to a significant increase in positive reactions toward the hawkish politicians. No effects were found with anxiety. Direct comparisons of the effect of anger versus anxiety revealed a significant difference in the relationship to the outcome variable ($p < .01$). The experimental induction $\rightarrow$ anger $\rightarrow$ attitude change effect found here replicates and extends the implications of our previous studies.

**Uncertainty versus control.** Turn now to the bottom half of Figure 6, which shows the effect of the uncertainty manipulation (vs. control). As seen here, anxiety elicited by the uncertainty induction task led, as predicted, to an effect opposite to that of anger. In particular, higher levels of anxiety tended to produce more negative reactions toward the hawkish candidates, which is the opposite of what happened with anger, and once again the coefficient for anger was different from that for anxiety ($p < .01$).

**Discussion**

The findings involving anger reported in this study replicate and extend the effects obtained in our first three experiments. This was true even though nearly every aspect of the methodology differed from those previous studies. For one thing, we used a completely different technique to activate anger, one completely unrelated to the 9/11 attacks. Also, instead of measuring attitudes toward the president and his war policies, we asked for participants’ opinions of hypothetical prowar politicians. In addition to replicating our
earlier findings with anger, these data help to rule out at least two alternative explanations of our earlier findings. In particular, the results from those studies were not an artifact of priming idiosyncratic associations linked to the 9/11 attacks. In addition, we also cast serious doubt on the idea that our findings merely reflected selective memories of the president. These findings also provide clearer evidence of the dissociable consequences of anger and anxiety. Consistent with predictions, activation of anxiety led to a significant decrease in support for the prowar candidates—a pattern opposite to that of anger. Moreover, although the effect of the uncertainty induction on anxiety was relatively modest after controlling for anger, it is important to reiterate that the uncertainty condition successfully elicited higher levels of anxiety relative to the other two conditions (see the analysis of covariance, reported earlier).

A few additional aspects of these data are worth noting. Suppose that we had not constructed separate measures of anger apart from anxiety. If we had lumped anger and anxiety together in the same index, these two emotions would have essentially “cancelled each other out” leading to an apparent null relation with the outcome variable. Indeed, a reanalysis of our data with a measure of negative affect averaging across anger and anxiety revealed no relation to the outcome variable, in both the first set of analyses (top of Figure 6) as well as the second (see bottom of Figure 6; \( p_1 = .14 \) and \( p_2 = .01 \), respectively, both \( p > .20 \)). As a related point, there was no direct effect of the experimental manipulation on attitude (cf. Figure 6). This almost certainly reflects the fact that manipulations designed to elicit anger nonetheless activate some degree of anxiety (see top of Figure 6) and vice versa (see bottom of Figure 6). To this extent, any manipulation that triggers some mixture of anger and anxiety can potentially lead to situations in which the unique effects attributable to each emotion cancel each other out, resulting in an apparent null effect of the manipulation.

In this regard, our results strongly resonate with and bolster the main message of prior work in the emotional appraisal literature (Huddy et al., in press; Lerner et al., 2003; Sadler et al., 2005; Skitka et al., 2006).

However, our work builds on that prior work in a number of important ways. First, we were able to show direct causal effects of anger versus anxiety by using an experimental manipulation of emotion that was unrelated to the 9/11 attacks in particular. This is distinct from the approach taken in the studies cited above, all of which activated these emotions by directly reminding participants of the 9/11 attacks or terrorist threats more generally. Second, our dependent variable consisted of attitudinal responses toward fictional candidates about which participants had no specific prior knowledge stored in long-term memory. In combination, this approach allowed us to (a) rule out an alternative selective, emotion-based retrieval account and (b) show that our findings were due to something about anger and anxiety per se, as opposed to cognitive content possibly idiosyncratic to the 9/11 attacks in particular.

**General Discussion**

The research reported in this article was initially stimulated by an interest in what is generally known as the “rally ’round the flag effect” (Mueller, 1970). Over the course of our research, however, our interest extended more broadly to understanding the various ways that threatening contexts can affect a wide range of socio-political attitudes, and the ways that these effects might involve affective experience in the form of emotion or mood. The main implications of our findings can be summarized as follows:

First, reminders of the 9/11 attacks reliably affected attitudes, but the nature of this attitudinal effect was quite specific, and far narrower than would have been expected by extant social psychological models of threat, including terror management and uncertainty management theory. In particular, this manipulation affected attitudes toward the president, but only those aspects having to do with his role as military commander in chief. More general political ideology was not affected at all, nor did preexisting differences in ideology moderate our effects, even though these individual differences exerted a powerful effect in their own right (cf. Figure 2).

Second, reminders of the 9/11 attacks significantly increased feelings of anger, which, in turn, led to systematic support for both real (Experiments 1–3) and fictional (Experiment 4) political hawks. In contrast, anxiety tended to have the opposite effect on such attitudes, a finding that was shown most clearly in Experiment 4. This latter finding replicates and extends important work in the emotional appraisal literature (Huddy et al., 2000; 2001; Sadler et al., 2005; Skitka et al., 2006) using an experimental design oriented toward generating evidence for a causal (threat → emotion→ attitude) model.

Third, in the final study in this series, Experiment 4, we were able to independently manipulate anger and anxiety by using two separate manipulations in combination with a third, control (baseline) condition. Importantly, none of the former two conditions were related to the 9/11 attacks or politics more generally. In addition to confirming and extending the implications of the first three studies, this study allowed us to rule out an alternative explanation of our own findings, which could attribute our findings to mood-mediated selective retrieval of information from long-term memory. In addition, because the manner in which we activated anger and anxiety was not related to the 9/11 attacks, this showed that our findings were due to the consequences of emotion, rather than reflecting something idiosyncratic about the cognitions or other unusual elements associated with this historical event.

We obtained converging evidence for our conclusions in several ways by using several different types of methodological approaches. Evidence regarding the unique roles of anger was demonstrated using three types of experimental manipulations, including videos of the 9/11 attacks (Experiments 1 and 2), personal reminiscences of the events of that day (Experiment 3), along with experimental inductions of mood that involved the retrieval of nonpolitical memories (Experiment 4). Moreover, we demonstrated applicability of our framework to cases in which participants were judging actual (Experiments 1–3) or fictitious (Experiment 4) politicians. Taken as a whole, our findings appear to be fairly generalizable, as our results cannot be attributed to an idiosyncratic feature of one particular type of experimental manipulation or measurement technique.

The fact that our findings support an anger-based framework of rally effects may seem obvious in retrospect. However, suppose that we had found that anxiety-related processes were directly or indirectly responsible for eliciting more positive attitudes toward the president. These findings could also have been regarded as “obvious,” insofar as they would support several lines of work that suggest, in one way or another, that people regard the American
On the Search for Boundary Conditions: Does Emotion Always Mediate Threat-Based Attitude Change?

In a provocative article, William McGuire (1983) suggested that researchers should avoid the temptation of pitting conceptualizations against each other in order to find an ultimate “winner” and “loser.” Rather, McGuire (1983) suggested that researchers should systematically search for the boundary conditions under which the critical assumptions of each of the models do or do not hold. Stated differently, this perspective suggests that “opposing” or “competing” models often turn out to be correct under their own respective boundary conditions (see also Greenwald, 1975).

These considerations loom rather large in light of an apparent contradiction between two emergent perspectives of threat-induced attitude change. One perspective, derived from the emotional appraisal literature, is that such attitude change is often likely to be mediated by the overt experience of emotion. The four experiments reported thus far in the present article are generally consistent with this view. However, one of the most popular social psychological models of threat—terror management theory—represents a rather different perspective. According to this formulation, certain types of threats to the self (most notably, reminding people of their own mortality) can produce systematic shifts in attitude in a manner completely independent of emotion. Indeed, as noted earlier, theorists in this area have long maintained that mortality salience manipulations produce no changes in overt emotion at all, making the issue of emotional mediation moot. (Many of the points we make here also apply to uncertainty management theory, but we focus here on terror management given its prominence in field and, also, because it has generated a much larger body of research devoted toward the issue of whether emotion does or does not mediate attitude change under threat.)

On the basis of our findings, it would be tempting to conclude that terror management is incorrect, at least in terms of the aforementioned assumption regarding the “emotion free” impact of threat on attitudes. This conclusion would seem to be most convincingly rendered by the results of Experiment 3, in which we used an experimental manipulation—a reminder of the 9/11 attacks—which was identical to that used by terror management researchers (cf. Landau et al., 2004). For one thing, contrary to the claims made in the terror management area, we found a strong impact of this manipulation on emotion, mostly in the form of anger. Moreover, this emotion played an important role in mediating the impact of the experimental manipulation on attitudes. As we have noted, our ability to detect the important role of anger would have been severely hampered if we had relied on the same overly broad index of negative emotion that is used almost exclusively in this literature. This raises a more general, and far more provocative, possibility, that many or even all of the previous findings obtained in the terror management literature could be subsumed and explained by theoretical models in the emotional appraisal literature.

Another possibility, however, is that there are two routes by which threat can influence attitudes. In particular, there may be certain types of threats for which emotion plays a major role in attitude change, but there may be other types of threats that do not involve emotion. Our most recent work (Schott, Scherer, & Lambert, 2009) has been oriented toward this very issue. In one study (N = 69), we randomly assigned participants to either a mortality salience or control condition. The mortality salience condition was identical to that used by an extremely large number (>200) of published studies in the literature and included instructions for participants to (a) “briefly describe the emotions that the thought of your own death arouses in you and to (b) “jot down, as specifically as you can, what you think will happen to you as you physically die and once you are physically dead.” Participants in the control condition were asked to describe the mundane events of
their typical day. Following this manipulation, participants completed the same battery of mood items used in our previous studies, and, following this, they completed a large battery of items that were designed to measure various aspects of conservatism.

According to the tenets of terror management theory, one should expect participants to report significantly higher levels of conservatism if they had been assigned to the mortality salience condition than if they had not. This is generally what our findings showed. This effect was particularly pronounced when measuring that aspect of conservatism most tightly connected to authoritarianism. Most critical for present purposes, even with our more sensitive approach to measuring emotion, we found no evidence that mortality salience triggered higher levels of negative emotion, and this was true across several different indices of negative affect (e.g., anger, anxiety, sadness). The fact that the mortality salience affected attitudes without a concomitant change in emotion—with no evidence of affective mediation—is consistent with the idea, noted above, that there may well be a “non-emotional” route of threat-induced attitude change.

It should be noted that these findings are not able to resolve several questions that have been raised about terror management theory over the years (e.g., Kirkpatrick & Navarrette, 2006; Leary & Schreindorfer, 1997; Wicklund, 1997). This includes questions regarding the exact nature of the (apparently cognitive) processes underlying attitude change and why such effects would tend to be localized for certain, but not all, aspects of political ideology. Nevertheless, these findings are useful in that they highlight the importance of the general perspective offered by McGuire (1983).

It is not so much a matter of whether an emotion-based conceptualization of threat is “right” and terror management theory is “wrong” (or vice versa). Rather, each of these two conceptualizations is likely to be more or less applicable depending on the nature of threat under consideration. In light of the findings reported in this article, the emotional appraisal literature appears to offer a more useful conceptualization compared with terror management theory when focusing on terrorist attacks, but the reverse may be true for mortality salience manipulations.

It could also be the case that any given type of threat could involve both “emotional” and “cognitive” processes of threat-driven attitude change. Our findings from Experiment 3 provided a hint of this possibility, insofar as the impact of the experimental manipulation on attitudes, although partially mediated by emotion, still did have a significant residual effect, even after controlling for emotion. Nevertheless, additional research is clearly needed to more fully understand the differences (as well as possible similarities) pertaining to “emotion-based” versus “cognitive” processes of threat-induced attitude change. Further avenues of future research are articulated in the section to follow.

Caveats and Directions for Future Research

Our research was conducted during 2003–2008. Hence when we measured attitudes toward the president, these always were directed toward George W. Bush. On the one hand, this aspect of our research represents less of a limitation that might be apparent at first blush, because our manipulations had an impact (directly or indirectly) on a wide range of measures that went beyond attitudes toward the president. It is nonetheless true that George W. Bush represents, more than any other president in recent memory, the ultimate “war time president.” Indeed, the United States was involved in major military activity during virtually all of the 8 years of the Bush presidency. One issue of obvious interest is how a state of anger might affect attitudes toward a more “dovish” American president who is less strongly associated with the use of military force in international conflict. This is certainly an issue that merits further investigation. However, the results from Experiment 2 may already provide one possible answer to this question: Anger should lead to selective support for any elements of the president that are consistent with proggressive motives. Hence, anger may actually lead to more negative appraisals of a president (or indeed, any other politician) who takes a strong stance against the use of military force.

Finally, it is important to reiterate that the dynamics that exist in one culture may not necessarily generalize to other cultures. For example, the role of the American president as military commander in chief almost certainly plays a large role by which anger can influence perceptions of the president in combination with attitudes toward the use of military force. In other countries, however, these dynamics may be very different depending on the political, psychological, and historical considerations that apply to that setting. In our own recent work, we have begun to explore these considerations in the ex-soviet republic of Georgia and neighboring countries (cf. Lambert, Scherer, Rogers, & Jacoby, 2009), with an eye toward identifying the aspects of the present framework that do, and do not, generalize to other cultures.

Looking Forward

Our findings raise issues of theoretical and practical importance that may be relevant to future political events. As of this writing, Barack Obama is in the initial stages of his presidency and is coping with the worst financial crisis since the great depression. As noted at the beginning of this article, financial crises do not appear, in and of themselves, to trigger the kinds of dramatic spikes in presidential approval one sees with a sudden attack by one or more hostile outgroups. Nonetheless, financial crises obviously do involve much uncertainty and, to this extent, may well lead to changes in people’s view of the government along with other potential sources of psychological security (Kay, Gaucher, Napier, Callan, & Laurin, 2008). Whatever the dynamics associated with this financial crisis, our framework suggests that a sudden outbreak of hostilities (e.g., in Iran) would strengthen support for Obama.

4 In most of our studies using the mortality salience manipulation, we have found that the effects of this manipulation on authoritarianism is somewhat greater for male compared with female participants. The meaning of this contingency is not entirely clear, although it is worth noting that the construct of authoritarianism is manifestly paternalistic in its emphasis on punishment/aggression and, as such, is consistent with traditional expectations for how men should respond in stressful contexts. This could, in theory, provide one explanation for this contingency, which is similar to that found in at least one recent study in the literature (McGregor, Haji, Nash, & Teper, 2008). It should be noted, however, that we did not find analogous gender effects using reminders of the 9/11 attacks, which suggests that gender effects, if and when they occur, may well depend on the manner in which psychological threat is operationalized. This is consistent with our larger point, articulated above, which is that different psychological principles are likely to apply depending on the context in which the antecedents and consequences of threat are studied.
with respect to his role as military leader, but should do little to shore up support for his capacity to resolve the present financial crisis. As the presidency of Barack Obama and other subsequent presidents unfold, it will be important to understand how different sorts of threats are relevant to different aspects of the presidency along with other elements of social and political judgment.

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