

Terrified or Enraged?

Emotional Micro-Foundations of Public Counterterror Attitudes

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SUPPLEMENTARY INFORMATION

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Appendix A Sampling

A.1 Survey Firm

This survey was conducted in March 2021 using the Lucid online opt-in panel. The complete sample consists of 5,499 US adults, with quotas for partisanship and other demographics to ensure a sample that broadly reflects the US population. See Section A.4 for more information on sample descriptives.

Lucid offers several benefits over other online panels: 1) results from Lucid have been validated by other researchers as matching those of other popular survey platforms (Coppock and McClellan 2019), 2) Lucid is a panel aggregator, meaning that they have access to a much larger and more diverse sample than any single online panel would have, and 3) as compared to panels such as MTurk, where many respondents are “professional survey takers” familiar with psychological manipulations, many respondents on Lucid are more casual survey-takers. Because this study includes deception and common psychological treatments (emotion inductions), having a more naive survey taking population is highly desirable. Subjects are paid directly by the panel that advertised the survey to them, in the form of points, gift cards or cash rewards. See Section A.2 for more information on respondent quality.

A.2 Respondent Quality

As with all platforms, the prevalence of non-serious or fraudulent survey takers continues to be an issue on Lucid. To address potential challenges to inference from these types of non-quality respondents (bots, outside-US, non-serious survey takers), I employ several filters, all of which occur pre-treatment. First, I begin with a CAPTCHA question that removes respondents who cannot answer (n=111). Second, I employ a VPN and IP-address check, not allowing survey takers employing a VPN or with IP addresses outside the US to continue (Kennedy et al. 2020) (n=402). After removing these ineligible respondents, survey takers view the consent language. This language specifically references that subjects will be required to write out responses for some questions (e.g. that it will not be all multiple-choice) to deter non-serious survey takers from consenting (n=866). In line with best practices for bot filtration, immediately after subjects consent to begin the study, they are asked a qualitative question “where did you hear about this study,” which auto-filters out respondents who provide standard bot responses

such as “nice” and “good” to this question (n=908).¹ Fifth, two pre-treatment attention check questions are included in the early moderators modules of the survey (Berinsky, Margolis and Sances 2014). If subjects fail *both*, they are also automatically removed from the study (n=703). The high quality of the sample is further reflected in the high percentage of respondents who recall details of the treatment in the manipulation checks at the end of the study (See Section D).

A.3 Power Analysis

In total, this experiment has 20 treatment arms. In traditional factorial designs where researchers are interested in the main effect of a one factor (say, target type), regardless of the levels of other factors (e.g. anger), researchers can use a reduced sample size, as analyses will collapse across treatment conditions to estimate the main effect of one factor. However, though this experimental design is still set up as a factorial with multiple treatments, the effects I am interested in estimating are *simple* effects. That is, I am interested in understanding the effect of one factor (target type) when the levels of another factor (emotions) are set at a specific level, which requires each individual treatment to be sufficiently powered. This necessitates quite a large sample. In the main model specification, I run an OLS model with a 3- or 4- level treatment variable, without demographic controls.

Running a power analysis for linear models, the inputs are thus as follows: Numerator degrees of freedom: $u = 4$, effect size: $f^2 = .02$, significance level: $p = .05$, and standard power level of $power = .8$. Cohen (1992) suggests f^2 values of 0.02, 0.15, and 0.35 represent small, medium and larger effect sizes, respectively. Thus, I input $f^2 = .02$ in order to sufficiently power the study to pick up significant differences even on relatively small effects.

The inputs for OLS analysis indicate a sample size of at least $n = 200$ subjects per condition is warranted. For ANOVA analyses, Cohen’s 1992 estimate of what constitutes a small effect size is $f = .1$. Power analyses for this test suggest a somewhat larger sample may be needed, $n = 274$ per condition. Using this more conservative input, my aim was to collect a sample of $n = 5500$ respondents in this study across 20 treatment arms (275 per condition).

¹<https://timryan.web.unc.edu/2020/12/22/fraudulent-responses-on-amazon-mechanical-turk-a-fresh-cautionary-tale/>

A.4 Descriptive Statistics

The sample for this study is not a nationally representative probability sample; however, quotas were put in place on key demographics to ensure that the demographic distribution of the sample largely reflects the population of the United States (see Table A.1). The final sample was 49.5% male, 82% White (US Census: 76%), 69% Christian (US PEW Data: 71%), and 19% rural (US Census: 20%). The regional breakdown of respondents in the sample was 21% Northeast, 21% Midwest, 37.5% South, and 20% West. According to the US Census, the population breakdown of the United States is 17% Northeast, 21% Midwest, 38% South, and 24% West. Partisanship quotas were also in place, leading to a partisan distribution of 44% Republicans, 46% Democrats, and 9.5% true Independents. This breakdown largely reflects the partisan breakdown of the United States (PEW), which in 2020 was 44% Republican, 49% Democrat, and 7% Independent. 38.5% of the sample reported voting for Donald Trump in 2020. Of those who voted, 44% of the sample voted for Trump and 53% for Biden. This closely resembles the final popular vote count in the 2020 US Presidential Election, which was 46.9% Trump and 51.3% Biden (CNN).

Table A.1: Descriptive Statistics of Sociodemographic Variables (complete surveys)

Variables	N	Mean	St. Dev.	Min	Max
Gender (male)	5,491	0.495	0.500	0	1
Education	5,473	4.056	1.556	1	7
Income	5,485	3.709	1.745	1	6
Ethnicity (white)	5,491	0.822	0.382	0	1
Religion (Christian)	5,489	0.693	0.461	0	1
Rural	5,489	0.189	0.391	0	1
Suburban	5,489	0.472	0.499	0	1
Urban	5,489	0.339	0.474	0	1
Northeast	5,499	0.213	0.409	0	1
Midwest	5,499	0.212	0.409	0	1
South	5,499	0.372	0.483	0	1
Prev. Terror Victim	5,499	0.174	0.485	0	2
Military Service	5,492	0.146	0.354	0	1
Combat Experience	803	0.422	0.494	0	1
Republican	5,456	0.442	0.497	0	1
Democrat	5,456	0.462	0.499	0	1
Independent	5,456	0.096	0.294	0	1
Trump 2020 Voter	5,491	0.386	0.487	0	1
Political Interest	5,492	2.395	1.180	1	5

Appendix B Measurement

B.1 Pre-Treatment Moderators

Pre-treatment psychological batteries included in this study are:²

- Risk Orientations (Kam and Simas 2010): “Some people say you should be cautious about making major changes in life. Suppose these people are located at 1. Others say that you will never achieve much in life unless you act boldly. Suppose these people are located at 7. And others have views in between. Where would you place yourself on this scale?”
- Right Wing Authoritarianism (Feldman and Stenner 1997): 4-item child-rearing scale, where subjects are asked to select qualities they prefer their children have (independence/respect for elders, obedience/self-reliance, curiosity/good manners, being considerate/being well-mannered).
- Trait Anger & Trait Anxiety (Costa Jr and McCrae 2008): 12-item trait- anger and anxiety scales where subjects are asked to what extent statements such as “I worry about things” and “I lose my temper” accurately describe them.
- Need for Cognition (Arceneaux and Vander Wielen 2013): 8-item need for cognition scale, where subjects are asked to what extent statements such as “I would prefer complex to simple problems” describe them.
- Need for Affect (Arceneaux and Vander Wielen 2013): The full need for affect scale is 24 items long. In this study, subjects are randomly presented with 6 out of the 24 statements and asked whether they agree/disagree with statements such as “It is important for me to be in touch with my feelings.”

B.2 Experimental Treatment 1: Exposure to Terrorism

Subjects were exposed to one of four different news stories about terrorism, as shown in Figure B.1. These treatments vary the victim of the attack (civilians at a shopping mall versus soldiers at a military base) and the perpetrator of the attack (Jihadists versus White Nationalists). These two attributes of treatment are repeated in several places throughout the story: the main image (of a mall or barracks), the secondary image (of Jihadist versus White Nationalist militants), the title of the news story, the chyron text, the “story highlights” text, and within

²Note: As preregistered, none of the analyses reported in the body of the paper use these moderators, since treatment is randomized and no imbalance on covariates across treatment arms was observed. However, the SEM models and mediation models reported in SI E use these variables as controls.

the body of the story itself. The survey ran for three weeks. Each night, the date on the news story was changed so subjects always believed they were reading a story from the day before.

To further enhance believability, the attack was located in a less populous US state (Wyoming). Historically, from an external validity perspective, it is important to note that there have already been Islamic terror attacks in so-called “heartland” states in the years since 9/11. For example, in 2009 a soldier was killed in Little Rock, Arkansas outside a military recruiting center “in the name of Allah.” In 2014, a Sharia advocate beheaded a woman in Moore, Oklahoma while shouting “Allahu Akbar.” Other attacks by Islamist extremists have occurred in Texas (Ft. Hood, Corpus Christi), Tennessee (Chattanooga), and Ohio (OSU). Attacks by White Nationalists are also not unrealistic: according to the Anti-Defamation League (ADL), right-wing extremists have been responsible for at least 16 extremist-related murders in the United States and 75 percent of all such murders over the past decade. Prominent White Nationalist groups like the Patriot Front also maintain a strong presence in multiple states, including Wyoming, according to the Southern Poverty Law Center (SPLC). Situating the attack in the center of the country also helps contribute to the perception that anyone could be a future target of terrorism, not just those individuals living in major population centers such as New York City. Importantly, respondents who live geographically close to the location of the attack – in Wyoming or one of the states directly bordering it ($n = 469$) – for whom we might expect the attack to invoke more fear, *also* exhibit higher anger (but not fear) after news exposure.

The treatment text was formatted to look and feel like a real news story produced by AP. To reduce the risk of dissemination of a fictional news story, the text-only version of the treatment stimuli is provided in Figure B.1 below. Subjects in the control condition read a real unrelated article about a sports-related topic (edited for length), see Figure B.2. There were three different control texts respondents could have received.

Two killed, dozens injured in apparent white nationalist/jihadist attack on Wyoming shoppers/military base

Cody, Wyoming – Three attackers killed at least 2 US citizens and wounded dozens more at a Wyoming shopping mall/military base Thursday evening, in what authorities are investigating as a potential act of domestic terrorism, the latest in a string of attacks around the world perpetrated by individuals inspired by white nationalist/jihadist ideology.

The attack began shortly after 7 p.m. Thursday evening, officials said, when two gunmen opened fire with automatic weapons after gaining passage through the main entrance of the mall/base. Outside the mall after the brazen attack, ambulances streamed in, while dazed bystanders sat on the sidewalk waiting for information.

This bloodshed highlights the ongoing challenges security officials face in eliminating this type of violence. Though the US defense community has been attempting to combat white nationalist/jihadist groups for years, its adherents remain stubbornly active and resilient.

This attack follows a string of other lethal attacks against civilians/the military or police inspired by white nationalism/jihadism in the United States, including: (WN/Civilian) the 2019 El Paso Walmart shooting, a 2018 attack at a Tallahassee yoga studio, and the 2015 ambush of worshippers at a South Carolina church, among others. (Jihad/Civilian) the 2017 truck ramming in New York, a 2016 stabbing attack at Ohio State University, and the Orlando nightclub shooting, among others. (WN/Military) the 2014 Fort Hood shooting, 2014 ambush of Las Vegas police officers, and a 2009 shooting of police forces in Pittsburgh, among others. (Jihad/Military) the 2019 attack by a Saudi officer at a navy base in Pensacola, the 2015 Chattanooga navy reserve center shooting and 2009 Fort Hood attack, among others.

Figure B.1: Subjects in the treatment condition viewed one of four fictional articles describing an attack by white nationalist or jihadist perpetrator against a civilian or military target

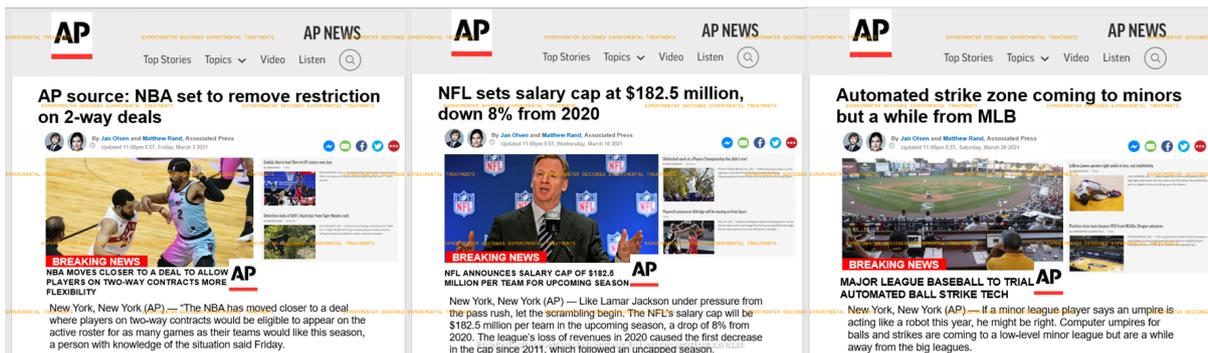


Figure B.2: Subjects in the control condition viewed one of three real non-political control stories, edited for length

B.3 Experimental Treatment 2: Emotional Primes

The emotions manipulation in this study is an Autobiographical Emotional Memory Task (Searles and Mattes 2015). The AEMT task can be used to prime either incidental or integral affect – emotions unrelated or related to the attitude object of interest (terrorism), respectively. In this study, I use an *integral* affect manipulation, asking subjects to reflect about the aspects of terrorism that make them feel particularly angry, scared or reassured.

Using an integral affect treatment makes more sense with the factorial design of this particular study, because subjects are exposed to a terrorism news story prior to receiving the AEMT task. As such, asking respondents to reflect about “things in their daily life” that make them

afraid/angry after reading this type of news story in practice would lead many respondents to nonetheless still reference terrorism and other political attitudes in their answers. The text for the three manipulated mediator arms with the AEMT task is as follows:

- **Anger:** We may ask you some questions about this story later, but we would like to first ask you some other questions about terrorism. When you think about terrorism, you may feel a variety of different emotions. Regardless of what your dominant feeling about terrorism is, we would like you to reflect about one emotion in particular: anger. What are the three things that make you (or might make other people) most angry about the threat from terrorism? Even if you do not feel very angry, why might other people feel angry? Please write two to three sentences about each thing that makes you (or might make other people) angry. Why do these things make you so angry? Try to be as detailed as possible. If you can, please write your answer so that someone might even get angry just from reading it.
- **Fear:** We may ask you some questions about this story later, but we would like to first ask you some other questions about terrorism. When you think about terrorism, you may feel a variety of different emotions. Regardless of what your dominant feeling about terrorism is, we would like you to reflect about one emotion in particular: fear. What are the three things that make you (or might make other people) most afraid about the threat from terrorism? Even if you do not feel very afraid, why might other people feel afraid? Please write two to three sentences about each thing that makes you (or might make other people) afraid. Why do these things make you so scared? Try to be as detailed as possible. If you can, please write your answer so that someone might even get scared just from reading it.
- **Reassurance:** We may ask you some questions about this story later, but we would like to first ask you some other questions about terrorism. When you think about terrorism, you may feel a variety of different emotions. Regardless of what your dominant feeling about terrorism is, we would like you to reflect about one emotion in particular: feeling reassured. What are the three things that actually make you (or might make other people) reassured about the threat from terrorism? Even if you do not feel very reassured, why might other people feel reassured? Please write two to three sentences about each thing that makes you (or might make other people) reassured. Why do these things make you feel reassured? Try to be as detailed as possible. If you can, please write your answer so that someone might even feel reassured just from reading it.
- **No Prime:** We may ask you some questions about this story later, but we would like to first get your thoughts on what you just read. Please write down some of your thoughts after reading this story here. Please write two to three sentences.

For these treatments, copy/paste functionality was disabled on the survey page to ensure subjects would manually type in their responses, rather than pasting something from the internet. These two treatments leads to a total of 20 treatment arms in the study.

B.4 Dependent Variables

B.4.1 Political Attitudes:

The core dependent variables in this study ask respondents about the policies they support (retaliation or conciliation) and their rationales for supporting retaliation. First, respondents answer the following two questions, presented in random order:

- *Support for Retaliation:* How important or unimportant is it that the United States forcefully retaliate against the people and organizations responsible for terrorist attacks?
- *Support for Conciliation:* How important or unimportant is it that the United States consider policy changes that address some of these grievances outlined by the people and organizations responsible for terrorist attacks?

Then, on the next screen, respondents are asked to report their rationales for supporting retaliation, as follows (the rationales are presented in random order). These rationales are adaptations of scale items often utilized in criminology and psychology research on the criminal justice system (McKee and Feather 2008):

Retaliating against the people and organizations responsible for terrorist attacks can serve several aims. How important or unimportant are each of the following aims to you?³

- Rehabilitating the perpetrators and reintegrating them into society
- Punishing the perpetrators to make them pay for the wrongs they have committed
- Incapacitating the perpetrators so they are unable to commit more attacks
- Deterring potential other perpetrators from committing similar attacks

B.4.2 Self-reported Emotions

These questions are asked after subjects answer the primary dependent variables to assess baseline emotions (in the natural mediator arms) and as an additional manipulation check (in the manipulated mediator arms). Subjects are asked, “To what extent do you feel each of the following (presented in random order) about the threat posed by terror attacks?” Emotions are presented in randomized order in a matrix: Worried, Anxious, Afraid, Angry, Outraged, Disgusted, Reassured, Confident, Calm, Optimistic, Resigned. Emotions are measured with a composite index, where anger is the average of anger, outrage and disgust ($\alpha = 0.84$) and fear is the average of fear, anxiety and worry ($\alpha = 0.86$). Previous research (Valentino et al. 2011) has found these measures load onto two distinct factors.

³A follow-up to this question explicitly asks subjects to rank order these four goals.

B.5 Demographic Measures

The following demographic questions were asked: Age, Gender, Ethnicity, Residence, Religion, Education, Income, Partisanship, Political Interest, News Exposure, 2020 Vote Choice, Past Exposure to Terrorism, and Past Military & Combat Service. Lucid also collects its own demographic variables for: region, partisanship, gender, age, ethnicity, education, and income which are used when fielding the surveys to correctly fill various demographic quotas. See Table A.1 for detailed subject demographics.

Partisanship is tested at the end of the survey. Because this introduces a modest risk of post-treatment bias (Klar, Leeper and Robison 2020), I use the Lucid-provided partisanship measure for all models that include partisanship. Lucid partisanship data is missing for 349 participants; for these individuals, I use their self-reported partisanship collected at the end of the survey.

Appendix C Experimental Ethics

In 2020, the APSA council approved *Principles and Guidance for Human Subjects Research*, which outlines the principles that should guide political science research with human subjects. Here, I describe how the research conducted for this study adheres to these important principles. Because this experiment involves deception, I pay particular attention to this principle, including as it relates to participant autonomy and relevant laws and regulations. This study was designed and received IRB approval from Washington University in St. Louis as a “minimum risk” study. The IRB ID number for this study is: 202001016.

C.1 Power

Political scientists should be aware of potential power differentials that may affect the voluntariness of consent (APSA 2020). In the case of this study, the sample (U.S. adults) is not a vulnerable community or a high-power institution. This diminishes to some extent concerns about power dynamics between the researcher and the sample pool. However, power imbalances may still exist and were considered in two key ways. First, the survey was run through a third party (Lucid), with whom participants had already consented to be a part of the panel population. Participants receive multiple opportunities to participate in research studies every day, diminishing the likelihood that respondents feel obligated (financially or socially) to participate in any one study. Second, compensation for the study was set at a level that would be fair

(above federal minimum wage) but not so high as to make participation coercive (e.g. making subjects unwilling to decline participation or drop out because they needed the high payout) (Fisher 2013).

C.2 Consent

Political scientists should seek informed consent, especially if the research involves more than minimal risk (APSA 2020). The study was deemed minimum risk by the author’s institutional IRB, meaning subjects are not exposed to stimuli that is more dangerous what than they might otherwise encounter in their daily life (e.g. news of political violence). As is standard for minimum risk studies, all participants in this study read an informed consent document that they acknowledged prior to participating in the study. This document includes information on who the researchers are, the sample criterion, length of survey, information regarding subject data confidentiality, and subject compensation. Subjects were not informed of the specific goal of the study – to examine the effects of emotions on political attitudes – as this would have potentially influenced their answers (e.g. due to the Hawthorne Effect, see Bortolotti and Mameli (2006)), invalidating the study’s conclusions. However, they were told that the study would ask questions about their emotional reactions to important life events and their opinions about current political topics. Thus, subjects were aware that questions would touch on both emotions and politics before they provided consent. Subjects were also informed that they might find the survey content upsetting. Finally, subjects were told that they were free to skip any questions they prefer not to answer and that they could discontinue their participation at any time. They were also provided with the contact information of the researcher and the university IRB and encouraged to contact both with questions or if they believed they had been harmed by their participation in the study. No questions or complaints were received.

C.3 Harm & Trauma

Political scientists should consider the potential harms associated with their research to participants and in terms of broader social or economic harms (APSA 2020). In this study, subjects read about political violence, which may be upsetting. Reading about this subject is, of course – by design in this case – likely to engender a degree of fear or anger in participants. However, it is also important to note this discomfort should be no more than that encountered in their daily lives reading, listening to, or watching the news on television, which frequently reports on terrorism, in addition to other political violence topics. The news stories in these

treatments are, indeed, modeled on real news articles about this type of violence and, if anything, are somewhat less graphic than many media treatments of political violence (e.g. there is no video content). Nonetheless, various steps were taken to address potential psychological harm subjects may experience during the study. First, subjects were informed in the consent form that the study may contain upsetting content and could choose not to consent. Second, when participating in the survey, subjects were able to skip any questions they did not feel comfortable answering. Third, after completing the study, all subjects are fully debriefed and told that the news stories in the study are completely fictional (in red, bold, underlined text). The debrief also included details as to why this deception was necessary, in keeping with best practices (Murphy et al. 2020). Fourth, because exposure to these topics may nonetheless cause stress, including strong emotions (Pearlman 2022), subjects are all provided with a link to the Anxiety & Depression Association of America’s “find help” page, in order to help cope with any traumatic feelings aroused by participating in the study. Finally, the study included a “comments for the researcher” question at the end of the survey where subjects could write whatever they wanted. Reviewing these comments, no subjects reported any stress or trauma from reading these stories. No participants contacted the researcher or the IRB to raise concerns about potential harms they may have suffered from taking part in the study.

C.4 Confidentiality

Political scientists should generally keep the identities of research participants confidential (APSA 2020). For this study, at no point did the researcher have access to personally identifiable information from subjects, as the study was run through a third party panel, Lucid. Lucid is a well-established survey firm in the U.S. with strong safeguards in place to protect survey participants’ identity. While Lucid shares demographic information provided by their survey-takers with researchers, they will not share any personally identifying information. In the consent document, subjects are assured of the anonymity of their responses, but are informed that their fully anonymized survey responses will be used in public presentations and papers of the research.

C.5 Impact

Political scientists should consider the broader social impacts of their research at the individual and societal level, including the integrity of political processes (APSA 2020). This study does not intervene directly in politics. However, the use of deception may raise some concerns

regarding the broader impacts of misinformation or trust in news organizations. For example, are debriefs effective at correcting misinformation that subjects are exposed to in fake news studies? Encouragingly, studies on the efficacy of debriefing in false memory studies have found that careful and thorough debriefing procedures significantly reduce false memories (Greenspan and Loftus 2022; Murphy et al. 2023). Subjects exposed to fake news in survey experiments and subsequently debriefed are significantly less likely to report false memories of the story *even six months later* than are new subjects (Murphy et al. 2020), which is a strikingly durable effect.

Another consideration is whether exposure to fake news affects subjects' trust in news in general or in the specific outlets used in the experiment. Importantly, these studies also find strong evidence that debriefed subjects are no less likely to believe or recall future *true* news stories that are presented to them (Murphy et al. 2020, 2023), suggesting that participants do not develop a generalized distrust of the media due to their exposure to past fake news articles in academic research. Whether the use of specific news agency and reporter names undermines their professional reputation or brand image in the eyes of research participants is also an empirical question. I cannot directly test this in my study and this question has not been extensively studied.

To probe the plausibility of these more specific reputational effects, I thus fielded a small study ($n = 483$) on Lucid (the same platform used in the main study) that exposes research participants to randomly assigned *real* news stories from 4 different neutral news organizations – AP, Reuters, BBC and NPR (see Figure C.1) – about a current foreign policy topic (the Ukraine-Russia War).⁴ The articles included the news organization logo at the top of the story, in the lead (for AP and Reuters) and in the real URL at the bottom of the story. After viewing the article, participants answered various questions about their emotional responses to the article and their political attitudes as in the original study. I then ask subjects to recall the topic of the article, specific details in the story, the news agency name, and the reporter names. While the majority of subjects readily recall the topic (76%) and details of the story (51%), very few subjects can correctly recall the news organization (9.5%)⁵ or the specific reporters' names (4.3%).⁶ Since subsequent mistrust in these specific organizations or reporters is predicated on remembering them in the first place, these results help mitigate concerns that the reputation of

⁴I chose this topic so that it would a) be about a similar topic as my study (political violence) and b) be prominent enough that all news agencies would likely have stories about the same topic at the same time (March 2023). The study received IRB approval as an exempt human subjects study (IRB ID: 202303189).

⁵Answer choices were: AP, Reuters, BBC, NPR, CNN, FOX, PBS or MSNBC.

⁶Prior to viewing a list of potential news reporters to select from, I also asked respondents to recall the names from memory. Only 2 out of 483 research participants are able to do this.

specific media sources or reporters are being unduly harmed by this study.

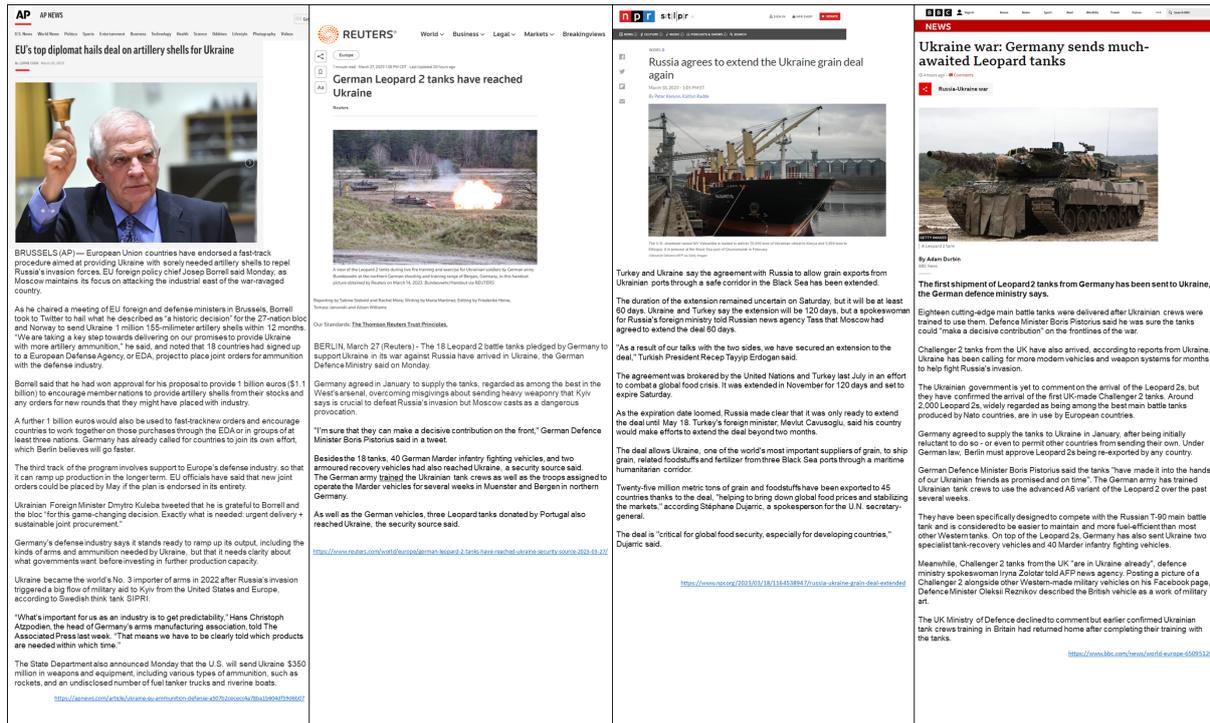


Figure C.1: Real News Stories Shown in Memory Study

One other broader impact consideration is the potential affects of the use of deception for subjects' broader trust or confidence in academic researchers. In part, this question reflects longstanding disciplinary divides in the use of deception in human subjects research. While deception is common in sociology, psychology and political psychology, economists largely eschew its use due to the concern that deception will contaminate the subject pool (See e.g., Ariely and Norton (2007); McDermott (2002); Sell (2008) for a sampling of these debates). This is a particularly prominent concern in in-person lab settings, where there is a finite population of potential subjects (e.g. undergraduate students) who may become suspicious of future deception after participating in other deception studies (see e.g. Hey 1998; Ledyard 1995). Using an online sample from a panel aggregator like Lucid alleviates this concern to some extent, since Lucid "is made up of hundreds of suppliers with a diverse set of recruitment/sourcing methodologies... ensur[ing] that the Marketplace is not overly reliant or dependent on any particular demographic or segment of the population" (Lucid FAQ).

Nonetheless, these disciplinary debates have generated a large body of empirical research that we can draw on to assess how participation in deception studies impacts respondents' future trust in academic researchers, from both an ethical and practical standpoint. Many

studies have found no negative effects either on participant behavior or trust in researchers (e.g. Christensen 1988; Soliday and Stanton 1995; Bonetti 1998; Barrera and Simpson 2012; Kimmel 1998; Krasnow, Howard and Eisenbruch 2020; Rahwan, Fasolo and Hauser 2022). Indeed, several studies have found that – with careful and thorough debriefings – research participants report a very high degree of satisfaction and interest following their participation in deception studies, often even higher than in non-deceptive studies (Boynton, Portnoy and Johnson 2013; Greenspan and Loftus 2022; Murphy et al. 2023; Smith and Richardson 1983; Soliday and Stanton 1995). However, other researchers have found contrasting results, including modest changes in participant distrust of researchers (Smith and Richardson 1983), altered behavior in the experimental setting (Stang 1976) and decreased willingness to participate in future studies (Jamison, Karlan and Schechter 2008) after deception was revealed or suspected. Ultimately, as one prominent review article concludes, “the available empirical evidence does not allow us to finally settle the methodological debate on deception, and there is room for honest differences in evaluating the ultimate impact of deception” (Hertwig and Ortmann 2008, p. 81).

Settling this longstanding debate is outside the scope of the present research. However, this important discussion should continue, particularly as new political challenges such as the proliferation of misinformation and conspiracy theories impact electoral politics. For now, the discipline of political science allows the use of deception, including in the 2020 APSA Principles and it remains a broadly utilized tool in experimental political science (Dickson 2011).

C.6 Laws, Regulations and Prospective Review

Political scientists should be aware of relevant laws and regulations that apply to their research (APSA 2020). The research in this study follows applicable laws in the United States. One particularly relevant consideration is the category of “fair use” with respect to copyrighted material, since the study uses a fictitious news story that is attributed to a real news organization. Fair use stipulates that copyrighted material (such as a company name or logo) can be used without permission of the copyright owner in certain situations. The use of news organization names and logos in fictitious experimental treatments is quite common in academic studies (e.g. Brader, Valentino and Suhay 2008; Druckman and Nelson 2003; Gadarian 2010; Arceneaux 2012; Jamieson and Van Belle 2018; de Benedictis-Kessner et al. 2019; Lajevardi 2021; Peterson and Allamong 2022). However, it is still important to evaluate fair use considerations for this particular study. Section 17 U.S. Code 107 summarizes fair use as follows: “The fair use of a copyrighted work, including such use by reproduction in copies or phonorecords or by any other

means specified by that section, for purposes such as criticism, comment, news reporting, teaching (including multiple copies for classroom use), scholarship, or research, is not an infringement of copyright.” This is also reiterated by the International Trademark Association’s online fact sheet which explains: “Nominative fair use generally applies to comparative advertising, parody and non-commercial use of trademarks in academic articles, media reports, etc.”

There are four main principles governing the use of copyrighted material under fair use.⁷ First, the purpose and character of the use is for *nonprofit educational* purposes, which is the case here. Second, fair use typically applies to work that is *already published* and publicly available rather than unpublished, which is the case here. Third, fair use usually involves using a *small amount* of copyrighted material rather than an entire e.g. article, chapter or book, also the case in this study. Finally, fair use should *not adversely affect the market* for the copyrighted work. This study does not directly impact profits, as it does not freely disseminate articles. It is also very unlikely that the broader reputation or market share of this news organization (whose articles they estimate are viewed by 4 billion people every day) would be affected by this study, especially given that all subjects read a debrief at the end of the study that specifically states the news story is fictitious and constructed for the purpose of the study only (Murphy et al. 2023), as well as outlining the specific reasons for the use of deception in the study. These types of detailed, “enhanced” debriefs have been shown to be particularly effective at debunking any misinformation effects of treatment (Greenspan and Loftus 2022). The treatment stimuli provided in the Appendix includes the text only to prevent further dissemination of the stimuli upon publication of the research. Given these four criterion, the use of the news organization’s name, logo and reporter bylines falls under fair use.

C.7 Deception

Political science researchers should carefully consider if deception is necessary for a research study and how it may impact research subjects’ autonomy (APSA 2020). This study involves the use of deception. Research participants believe they are reading real news stories about terror attacks in the United States, but these stories are developed by the researcher. Using fake or altered news stories attributed to real news media organizations is a frequent approach in political science research (e.g. Brader, Valentino and Suhay 2008; Druckman and Nelson 2003; Gadarian 2010; Arceneaux 2012; de Benedictis-Kessner et al. 2019; Lajevardi 2021; Peterson

⁷A detailed guide for academics published by Harvard’s office of general counsel is available here: Harvard OGC Fair Use Guidelines.

and Allamong 2022). However, it is nonetheless important to consider the potential risks and tradeoffs involved in its use (Bortolotti and Mamedi 2006; Cook and Yamagishi 2008; McDermott 2013). The APSA principles describe five relevant considerations for researchers using deception in research studies. I consider each in turn:

Does the research involves more than minimal risk of harm? While deception is allowable according to the APSA principles even if a study involves more than minimal harm, the use of deception in those scenarios would require “exceptional justification.” In this case, however, the experimental study is minimum risk. While research participants may, by design, feel distressed reading news about terrorism, this distress should be no more than that encountered in their daily lives reading, listening to, or watching the news on television, which frequently reports on terrorism, in addition to other political violence topics. Nonetheless, various steps were taken to address potential psychological harm subjects may experience during the study (See *Harm & Trauma* above). This included a clear consent form that indicated the study would include potentially upsetting content and a full and thorough debrief that included information about mental health resources for participants. Researcher and IRB contact information was also provided. No participants contacted the researcher or the IRB to raise concerns about potential harms they may have suffered from taking part in the study.

It is also important to consider potentially broader societal harms that may come from participating in the study. This issue is more readily apparent in field experiments, in which experimenters interact with subjects outside of a lab/survey context or alter real-world environmental stimuli and subjects often never know they have been experimented on (McDermott and Hatemi 2020). However, it is important to consider this issue in survey experiments as well (See *Broader Impact* above). As described above, the results of empirical studies examining the efficacy of debriefs in this context is reassuring and suggests that false memories and beliefs are corrected by debriefs and that exposure to fake news in research settings does not increase mistrust of news media more generally (see e.g., Murphy et al. 2020, 2023).

Would engaged individuals have withheld consent if fully informed consent were sought? The news treatments were pilot-tested for readability and believability prior to the full study. This offered an important opportunity to assess whether research participants would have potentially withheld consent had they known the study contained deception prior to fielding the full study. In the pilot, no subjects contacted the researcher or asked to withdraw their data after finding out about the use of fake news stories in the debrief. In addition, no subjects indicated concerns with the study in a qualitative text box provided for subjects to optionally write in their personal

thoughts on the study. This provided a degree of reassurance regarding subjects' comfort with the use of deception in this study. In the full study as well, no subjects contacted either the researcher or IRB after learning that deception was involved in the study. This suggests that research participants' were unlikely to have withheld consent had they known about deception at the outset of the study. This comports with past empirical tests of subjects' reported satisfaction with their participation in deception studies generally (Boynton, Portnoy and Johnson 2013; Greenspan and Loftus 2022; Murphy et al. 2023; Smith and Richardson 1983; Soliday and Stanton 1995).

The relations of power between subject and researcher: When using deception, it is important to carefully consider dynamics of power between the research participants and the researcher (see *Power* above). In this case, power imbalance concerns are diminished by the use of a non-vulnerable sample (U.S. adults), a third-party online survey platform, and a reasonable but not coercively high compensation rate (Fisher 2013).

Is debriefing of subjects possible? Full debriefing took place in this study. As past research has found that careful and thorough debriefs are particularly important for retracting the effects of misinformation (Greenspan and Loftus 2022; Murphy et al. 2023), careful attention was paid to the content and structure of the debrief. First, subjects are told in red, bold, underlined text that the news stories and all events described in them were completely fictional. The debrief then explains why the researchers deemed deception necessary for the study: "In this case, we use deception to study the real emotional impact of exposure to news stories like this, which requires participants to feel that the story they are hearing has actually occurred." The debrief also outlines the specific theoretical goals of the study: "We are interested in understanding how citizens' gut emotional reactions to stories about different kinds of terrorism impact their political attitudes and policy preferences. The primary goals of this research are to understand better how citizens react to events such as this and how their reactions differ depending on their emotional state, whether they are angry or fearful." Subjects also read more about the broader societal public goods the study aims to promote: "Research such as this can serve a number of purposes, including improving the quality of journalism, enhancing communication in times of crisis, learning more about public priorities and what citizens expect of their government, and the factors that drive public concerns, uncertainty, and perceptions of fairness and responsibility." Finally, subjects are directed to mental health resources "For some resources on how to cope with anxiety or depression after seeing events like this in the news, please see: <https://adaa.org/find-help>." Subjects are also provided contact information for

the researchers to follow up with any questions or concerns.

Is deception necessary for the integrity of the research? In this study, deception is critical because the experiment aims to examine the role that visceral emotional reactions to encountering news about terrorism play in shaping participants' political attitudes and policy preferences. In this case, research participants are shown treatments that look like real news articles about terror attacks and are debriefed at the conclusion of the study that the articles they read are fictitious and no such attacks occurred. This experimental realism is critical to make sure that the causal process and outcomes measured in the experiment – terrorism's indirect effect on political attitudes through anger – adhere as closely as possible to their real-world analogues. While hypothetical scenarios or abstract vignettes may be suitable for the study of some political attitudes (Brutger et al. 2022), these tools are less well-suited for inducing the strong emotional responses citizens feel after exposure to terrorism news and may “risk both internal and external validity by failing to engage subjects' attention or interest” (McDermott 2011, p. 36). Abstract scenarios can be particularly problematic when addressing issues concerning morality and justice (Bauman et al. 2014; Bloom 2011; Schein 2020), as this study does, because research participants' answers to “decontextualized hypothetical scenarios may not accurately reflect moral decisions in everyday life” (FeldmanHall et al. 2012, p. 432).

The importance of experimental realism in this study is further compounded by the nuanced emotional responses the experiment seeks to nail down, namely, the relative prominence and importance of fear versus anger in shaping political attitudes. A hypothetical scenario or a treatment that included, for example, just a news headline of past attacks without the details usually present in real-world news coverage of terror attacks such as “scary” visual imagery (Gadarian 2010; Iyer et al. 2014), or news articles that described abstract threats rather than a specific, realized attack would likely change the amount of fear people feel, making it difficult to ascertain whether anger is simply more prominent because of the watered down nature of the experimental stimulus. In other words, these other potential types of treatment would not make for a hard test of the study's core hypothesis – that anger is often more prominent and more central to shaping citizens' political attitudes than fear. For the same reason, it is important that the treatment be believable as a real news story. As such, the treatment was formatted to look like a real online news article, including visual imagery and detailed information about the attack in the body of the article. Reading about terrorism in a news article that looks and feels similar to real new media also more accurately simulates the real-world ways in which the mass public is exposed to terrorism through the press (Nacos 2016), increasing the external validity

of the study. This stylization of the treatment also increases believability, another important design feature. If most subjects did not believe the news story was real, they would likely report less fear (and more anger) because they read the report or vignette as hypothetical, harming both the internal and external validity of the study (McDermott 2011).⁸

Finally, because the study aims to examine heterogeneous effects of exposure to terrorism news by political party, it was important to use a reputable, relatively neutral news network as the source of the article (i.e. the Associated Press). This helps ensure that the content of the article is not automatically discounted by partisans as motivated by ideological aims (Landreville and Niles 2019; Hovland and Weiss 1951) and, crucially, isolates the effect of perpetrator and victim identity on partisan responses to terrorism, rather than the inferred partisan bias of an unstated or hypothetical news agency. In other words, naming the news agency reduces the signal that is otherwise conveyed by the organization’s choice to cover certain types of attacks (Acharya, Blackwell and Sen 2018). Without naming the news organization, most of the total effect of exposure to terrorism news could be attributed to the inferred partisan bias of the type of news organization that would cover this type of violence (e.g. Republicans may assume that only “liberal” news organizations would cover White Nationalist terrorism and their responses may be due to that differential attribution of authorship). Holding the news organization constant and using a relatively well-known, neutral source is thus a key feature of the experimental treatment.

Since experimental realism is so important to the validity of this design, one might consider using real news articles about past attacks in the study, rather than fictitious articles. However, using real news articles is also quite problematic from a causal inference perspective, for multiple reasons. Because real-world attacks naturally vary on so many different dimensions – location, lethality, mode of attack, time period, previous news coverage, and so on – disentangling the effects of the attributes of interest (victim and perpetrator identity) on emotional and political responses to terrorism would be impossible to isolate. Indeed, this is the key advantage of an experimental design when studying public reactions to terrorism news, being able to “hold fixed a range of attributes of incidents that could confound observational studies on the topic...[which make it] extremely difficult to disentangle the effects of our attributes of interest” (Huff and Kertzer 2018, pp. 59-60). This problem of using real articles is further compounded by the fact that the news media has been shown to cover different types of terror attacks quite differently (Dreier et al. 2022; Kearns, Betus and Lemieux 2019), which would also influence how subjects

⁸See Section D.1 for more information on treatment believability.

respond emotionally and politically to the news article.

Even if it were possible to identify two real attacks that were quite similar to each other except on the dimensions of interest and to alter the tone of the articles so that the language used to describe the events was similar, the widespread real-world coverage of these types of attacks in the news would likely mean that respondents have already developed a broad set of preconceived beliefs about the attacks and what should (or already has been) done by the United States in response. These beliefs could be shaped by a host of other medium- and longer-term processes that take place following terror attacks, including elite framing of the issue (Norris, Kern and Just 2004; Nacos, Bloch-Elkon and Shapiro 2011), presidential rhetoric (Gershkoff and Kushner 2005), any reprisal attacks that have occurred, the results of legal proceedings against perpetrators, protests by the public and so on. Because this study aims to explore the immediate, bottom-up responses of the public to news of *new* terror attacks, these various longer-term factors would confound assessments of the independent causal role of immediate emotional reactions to terrorism news on political attitudes.

Ultimately, this research investigates core questions regarding how the public responds emotionally to terrorism, shaping the incentives of political leaders formulating counterterror policy. In the United States, estimates of the monetary cost of the decades-long ‘War on Terror’ are estimated to be close to \$5.8 trillion dollars (Matthews 2021) and over 172,000 people have been killed in the War in Afghanistan alone (AP News). Given the considerations highlighted above, I conclude that deception is necessary to effectively assess the role of public outrage in potentially exacerbating vengeful cycles of terrorist and counterterrorist violence, a question of immense political importance with stark real-world consequences for the populations impacted by this violence.

C.8 Shared Responsibility

Finally, the APSA guidelines encourage researchers to take shared responsibility for promoting learning within campus communities and across universities about the ethical dilemmas involved in social science research and helping IRBs to address the unique issues involved in political science studies (APSA 2020). This shared responsibility to promote ethical research is a crucial component of conducting human subjects research that all researchers should accept and welcome.

Appendix D Assessing Potential Failure to Treat

D.1 Treatment Believability & Manipulation Checks

The first treatment subjects were exposed to in the study was a news story about a terror attack. These stories were pre-tested for believability. At the end of the survey, subjects were also asked questions regarding the extent to which they believed the news stories they read about were true as well as questions about the content of the story they read. Because these questions were administered post-treatment, respondents were included in all analyses regardless of whether they indicated that they thought the story was fake or got key details of the story incorrect.

First, respondents were asked if they had already heard about the event described in the news story. Responses were similar across different news stories: 72.4% in Jihadi-civilian thought they might have already heard about the attack, 72.7% in Jihadi-military, 66.8% in the White Nationalist-civilian, and 68.8% in the White Nationalist-military. In the “no attack” conditions, where the news stories covered real events that had in fact happened (e.g. NBA rule changes), 64% of respondents thought they may have already heard about the story. Next, subjects were asked if they thought the news stories they read were true. Asking this question may prime subjects to believe the stories were not true, even if they earlier believed them, so the numbers are lower: 62% in Jihadist-civilian believed the story at least somewhat likely to be true, 63% in Jihadist-military, 58% in White Nationalist-civilian, and 64% in White Nationalist-military. The control stories (which were in fact true) had the lowest believability scores of 43%.

I also ask two manipulation check questions about the reported attack: who the victims were and who the perpetrators were. There is significantly better recall of the victim when the attack is on civilians (74.5% in Jihadist-civilian and 76.4% in White Nationalist-civilian) than when the attack is on the military (65.3% in Jihadist-military and 65.5% in White Nationalist-military). There is also significantly better recall of the perpetrators when the attack is perpetrated by Jihadists (81% in Jihadist-civilian and 76.3% in Jihadist-military) than when it is perpetrated by White Nationalists (74.6% in White Nationalist-civilian, 65.2% in White Nationalist-military). This is unsurprising, as the default image of terrorism is a Jihadist attack on civilians, leading to a slightly higher error rate in recall when the attack does not fit this stereotype.

D.2 Treatment Uptake in the Autobiographical Emotional Memory Task

The second treatment subjects were exposed to was the Autobiographical Emotional Memory Task (AEMT), designed to induce specific emotions: anger, fear, or reassurance. A core

assumption of these analyses is that the emotion primes *work* in actually eliciting the target emotion, rather than a bundle of similarly valenced emotions (Mills and D’Mello 2014). To test this, I regress self-reported emotions by emotion prime condition, as reported in the main body of the article (Table D.1).

I find that the fear prime *only* significantly increases fear and not any other emotions, as expected. However, the anger prime increases *both* self-reported fear and self-reported anger. This means that comparisons between the fear and anger treatments will necessarily be a conservative test of the differences between fear and anger responses to terrorism. In addition, the reassurance prime, whose aim was primarily to reduce the negative emotions of fear and anger respondents might naturally feel in the wake of exposure to terrorist violence, did not significantly reduce either as compared to “no prime,” though it did increase self-reported reassurance. In fact, those in the reassurance conditions exhibited somewhat increased preferences for retaliation and punitive rationales for violence (though not significantly so), as compared to the “no prime” conditions. As such, when analyzing the ANIE of anger and fear on these attitudes, I compare respondents in these arms to respondents who received no emotional prime, rather than those who received the reassurance prime as was the pre-registered plan. This also makes sense given that earlier results indicated that anger responses to these attacks are at least partially contingent on partisanship – e.g. not all subjects become “naturally” more angry after exposure to an attack. Results are, however, substantively similar when comparing respondents in the anger and fear arms to the reassurance arms, as was the pre-registered plan. See SI F.1.

As an additional check of the validity of the AEMT treatment, I ran an interaction model regressing self-reported emotions on the interaction of the emotion treatment they received and their partisan affiliation. There are no significant heterogeneous effects on either self-reported fear or anger. Though, on average, there is a main effect of partisanship such that, unsurprisingly, Republicans are more likely to self-report anger and Democrats are more likely to self-report fear. I also run a triple interaction model regressing self-reported emotions on the interaction of the emotion treatment they received, their partisan affiliation and the the perpetrator ID in the news story (Table D.2). This likewise uncovered no significant interaction effects. From these models, I conclude that the AEMT treatment does not affect some partisans (e.g. Republicans) more strongly than others after reading about different types of attacks.

Table D.1: Manipulation Check:
Effect of Emotion Treatments on Self-Reported Emotions after Terrorism News

	<i>Dependent variable:</i>		
	Reported Anger	Reported Fear	Reported Reassurance
Fear Treatment	0.010 (0.044)	0.106* (0.046)	0.031 (0.046)
Anger Treatment	0.111* (0.044)	0.109* (0.046)	-0.038 (0.047)
Reassurance Treatment	-0.054 (0.045)	0.021 (0.047)	0.215*** (0.047)
Constant	3.724*** (0.031)	3.022*** (0.032)	2.241*** (0.032)
Observations	4,448	4,448	4,446
F Statistic	4.636** (df = 3; 4444)	2.951* (df = 3; 4444)	11.160*** (df = 3; 4442)

Note: $\hat{p} < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Appendix E SEM & Mediation Analyses

The parallel encouragement design also allows researchers to explore causal mechanisms using standard mediation or structural equation models with the natural mediator arms of the experiment. These analyses lead to the same substantive results as those in the main paper.

E.1 Structural Equation Models

To examine the degree to which anger and punitive motives mediate the relationship between exposure to terrorism and support for retaliation, I use a structural equation model of the hypothesized causal pathway in this paper.⁹ I find strong support for this causal model (Figure E.1). Exposure to terrorism has no direct effect on support for retaliation after accounting for this mediated pathway whereby terrorism affects anger ($\beta = 0.21$), anger affects punitive motivations ($\beta = 0.54$), and punitive motives in turn affect support for retaliation ($\beta = 0.41$). In a placebo SEM model (Figure E.2) with fear and incapacitation motives as the hypothesized causal pathway, the link between exposure to an attack and fear is non-significant. Though fear

⁹As is best practice, I include in this model a vector of demographic and attitudinal covariates to account for potential omitted variable bias: party ID, age, gender, ethnicity, religion, education, income, past exposure to terrorism, trait anxiety, trait anger, right-wing authoritarianism, risk acceptance, need for cognition and need for affect. See SI B for more details on measured traits.

Table D.2: Triple Interaction of AEMT on Emotions by Perpetrator and Partisan ID

	<i>Dependent variable:</i>	
	Reported anger	Reported fear
	(1)	(2)
Fear Treatment	0.039 (0.094)	0.123 (0.098)
Anger Treatment	0.214* (0.094)	0.191 [^] (0.098)
Independent	0.075 (0.154)	-0.203 (0.160)
Republican	0.393*** (0.094)	-0.334*** (0.098)
WN Attacker	0.199* (0.092)	0.126 (0.095)
Fear × Independent	-0.455* (0.214)	-0.190 (0.222)
Anger × Independent	-0.227 (0.223)	0.010 (0.233)
Fear × Republican	-0.126 (0.133)	0.083 (0.138)
Anger × Republican	-0.206 (0.133)	-0.047 (0.139)
Fear × WN Attacker	0.026 (0.130)	-0.056 (0.135)
Anger × WN Attacker	-0.065 (0.130)	-0.118 (0.135)
Independent × WN Attacker	-0.240 (0.210)	-0.279 (0.219)
Republican × WN Attacker	-0.405** (0.131)	-0.077 (0.136)
Fear × Independent × WN Attacker	0.548 [^] (0.302)	0.264 (0.315)
Anger × Independent × WN Attacker	-0.070 (0.312)	-0.160 (0.325)
Fear × Republican × WN Attacker	0.152 (0.186)	-0.077 (0.193)
Anger × Republican × WN Attacker	0.183 (0.186)	0.009 (0.194)
Constant	3.545*** (0.067)	3.156*** (0.070)
Observations	4,413	4,413
R ²	0.020	0.030
Adjusted R ²	0.015	0.025
Residual Std. Error (df = 4389)	1.044	1.087
F Statistic (df = 23; 4389)	3.910***	5.882***

Note:

[^]p<0.1; *p<0.05; **p<0.01; ***p<0.001
E-24

increase incapacitation motives and these motives increase support for retaliation, exposure to an attack does not increase fear.

Figure E.1: Structural Equation Model of Hypothesized Causal Pathway

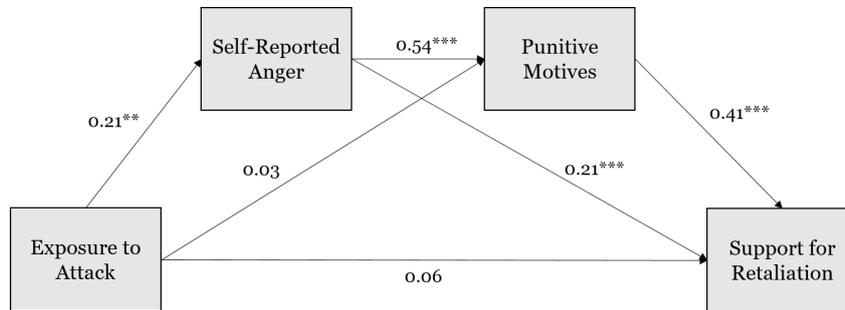


Figure E.1 displays a structural equation model modeling the association between exposure to an attack, self-reported anger, punitive motives, and support for retaliation, comparing those who read a terrorism news story and those who do not (natural mediator arms only). Because anger is *not* randomly assigned in these arms, this model includes the following covariates: party ID, age, gender, ethnicity, religion, education, income, past exposure to terrorism, trait anxiety, trait anger, right-wing authoritarianism, risk acceptance, need for cognition, and need for affect (See SI B for full measures).

Note: \wedge p<0.1; *p<0.05; **p<0.01; ***p<0.001.

Figure E.2: Placebo Structural Equation Model with Fear and Incapacitation as Mediators

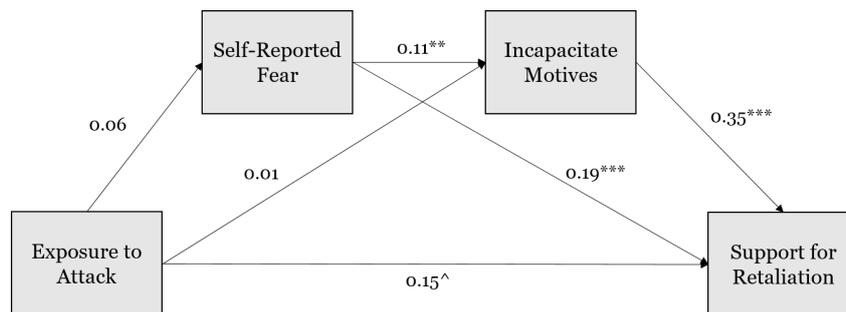


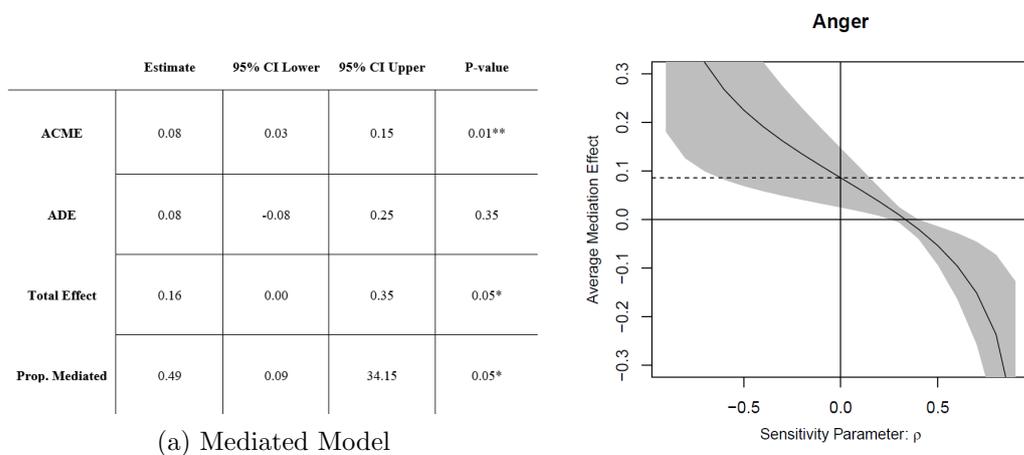
Figure E.2 displays a structural equation model modeling the association between exposure to an attack, self-reported fear, incapacitation motives, and support for retaliation, comparing those who read a terrorism news story and those who do not (natural mediator arms only). Same covariates as Figure E.1.

E.2 Mediation Models

A mediated model (Figure E.3) of anger's effect on support for retaliation demonstrate the same results as shown in the structural equation model: anger mediates the effect of exposure on support for retaliation. A placebo mediation model (Figure E.4) with fear as the hypothesized mediator demonstrates that fear does not mediate the relationship between exposure to terrorism and support for retaliation. Models control for party ID, age, gender, ethnicity, religion, education, income, past exposure to terrorism, trait anxiety, trait anger, right-wing

authoritarianism, risk acceptance, need for cognition and need for affect.

Figure E.3: ACME Model of Anger on Support for Retaliation

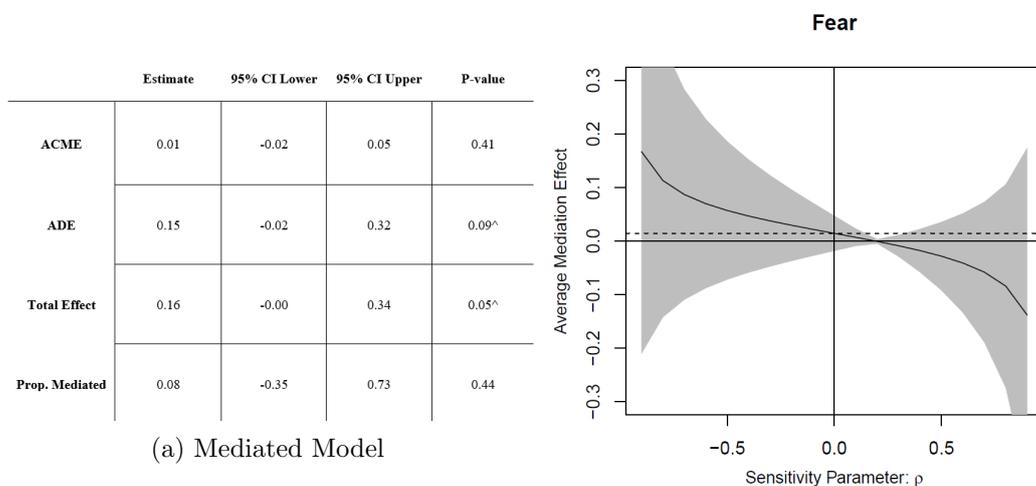


(a) Mediated Model

(b) Sensitivity Analysis

Figure E.3 displays a mediation model (plus sensitivity analyses) modeling the association between exposure to an attack, self-reported anger, and support for retaliation, comparing those who read a terrorism news story and those who do not (natural mediator arms only). Note: $\wedge p < 0.1$; $* p < 0.05$; $** p < 0.01$; $*** p < 0.001$.

Figure E.4: ACME Placebo Model of Fear on Support for Retaliation



(a) Mediated Model

(b) Sensitivity Analysis

Figure E.4 displays a mediation model (plus sensitivity analyses) modeling the association between exposure to an attack, self-reported fear, and support for retaliation, comparing those who read a terrorism news story and those who do not (natural mediator arms only). Note: $\wedge p < 0.1$; $* p < 0.05$; $** p < 0.01$; $*** p < 0.001$.

Appendix F Other Preregistered Analyses

F.1 Indirect Effect of Anger: Comparison to Reassurance Treatment

The pre-registered hypotheses regarding the indirect effect of emotions on attitudes after a terror attack hypothesized that support for retaliation and conciliation in the anger and fear primes would be significantly different than those in the reassurance conditions. This was because the reassurance prime was designed to reduce the negative emotions of fear and anger respondents might naturally feel in the wake of exposure to terrorist violence. However, this prime did not lead to any significant differences in self-reported anger or fear. As such, the main analysis instead makes comparisons between the fear/anger conditions and the “no prime” conditions. However, results are in fact substantively similar when comparing fear/anger to the reassurance arms, as Tables F.1 and F.2 show. The main difference is the coefficient for anger’s association with support for retaliation changes to $\beta = 0.094$ with a p-value of 0.109, just outside of marginal significance. Compared to “no prime” the coefficient for anger’s association with support for retaliation was $\beta = 0.100$ with a p-value of 0.079. Anger’s relationship with punitive preferences is unchanged.

Table F.1: Indirect Effect of Emotion Treatments on Self-Reported Emotions
(Comparison to Reassurance Arms)

	<i>Dependent variable:</i>	
	Retaliate	Conciliate
Fear Treatment	0.117* (0.058)	-0.070 (0.076)
Anger Treatment	0.094 (0.059)	0.009 (0.076)
Constant	5.646*** (0.042)	4.886*** (0.055)
Observations	4,490	4,490
F Statistic (df = 3; 4486)	2.430 [^]	0.600

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table F.2 shows the results of two OLS models regressing the randomly assigned emotion prime on support for retaliation and support for conciliation. This analysis compares respondents who viewed different stories about terrorism but received the same emotion prime to those who read about terror attacks but received the *reassurance* prime (the omitted baseline condition).

Table F.2: Indirect Effect of Emotions on Motives for Retaliation
(Comparison to Reassurance Arms)

	<i>Dependent variable:</i>			
	Rehab	Incapacitate	Deter	Punish
Fear Treatment	-0.183* (0.087)	0.090 (0.059)	0.052 (0.054)	0.046 (0.057)
Anger Treatment	-0.026 (0.088)	0.088 (0.060)	0.062 (0.055)	0.127* (0.058)
Constant	4.206*** (0.063)	5.759*** (0.043)	5.947*** (0.039)	5.802*** (0.041)
Observations	4,309	4,310	4,310	4,475
F Statistic	1.757 (df = 3; 4305)	4.205** (df = 3; 4306)	0.993 (df = 3; 4306)	2.414 [^] (df = 3; 4471)

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table F.2 shows the results of OLS models regressing the randomly assigned emotion prime on four different motives for retaliation. This analysis compares respondents who viewed different stories about terrorism but received the same emotion prime to those who read about terror attacks but received the *reassurance* prime (the omitted baseline condition).

F.2 Direct Effects of Emotions on Attitudes

In the main text of the article, I examine the effects of exposure to terrorism on attitudes and the indirect effects of experienced emotions in the wake of terrorism on attitudes. However, one might also be interested in the *direct* effects of emotions on political attitudes, not as a reaction to a terror attack, but in general. Does general anger about terrorism have the same effects as anger that is experienced in the wake of an attack? Treatment arms where no attack occurs and only an emotional prime is given could be used to explore these questions.

To begin, I first examine whether the AEMT treatments have the same effects on experienced emotions in the direct effect arms of the experiment (Table F.3) as they did in the indirect effect arms. Surprisingly, however, I find that self-reported anger is no higher in the anger-only arm than in the no prime arm. Anger is actually higher in the fear-only arm (3.67) than it is in the anger-only arm (3.66). Fear and reassurance are also only marginally higher in the fear/reassurance arms than the no prime arm. Essentially, in the direct effects arms (where only emotions are manipulated), there is a failure to treat problem that is *not* present in the indirect effect arms (where exposure and emotions are manipulated) reported in the body of the paper. In addition – perhaps in part due to these failure to treat issues in these treatment

arms – I find surprising results regarding the direct effect of the emotion primes on attitudes. In the absence of exposure to a terror attack, the fear and reassurance primes increase support for retaliation and punitive motives, but the anger prime does not (Tables F.4 and F.5), in contrast to the *Direct Effects* hypotheses.

Table F.3: Manipulation Check:
Direct Effect of Emotion Treatments on Self-Reported Emotions
(Manipulated Mediator Only Arms)

	<i>Dependent variable:</i>		
	Anger	Fear	Reassurance
Fear Treatment	0.145 (0.092)	0.161 [^] (0.094)	0.066 (0.093)
Anger Treatment	0.140 (0.092)	0.137 (0.094)	-0.045 (0.093)
Reassurance Treatment	-0.079 (0.094)	0.073 (0.095)	0.174 [^] (0.094)
Constant	3.525 ^{***} (0.063)	2.957 ^{***} (0.064)	2.323 ^{***} (0.064)
Observations	1,102	1,102	1,102
F Statistic (df = 3; 1098)	2.633 ^{**}	1.198	1.925

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table F.3 displays the results of OLS models testing whether the AEMT task significantly increases the targeted emotions. The null effects here suggest that there is a failure to treat problem of the AEMT in the direct effects arms.

These findings underscore the importance of the parallel encouragement study design, simultaneously randomizing *both* exposure type and emotional response. Studies that use a “causal chain” approach (Imai, Tingley and Yamamoto 2013), with Study 1 establishing the main effect and Study 2 independently assessing the proposed mediator’s effect cannot directly model how the treatment effects outcomes through the mediator – they only establish what each does in isolation. As I show here, the direct effect of emotions on attitudes *in the absence of a terror attack* is different than emotion’s indirect effect on attitudes in the wake of terrorism.

Table F.4: Direct Effects of Emotions on Political Attitudes
(Manipulated Mediator Only Arms)

	<i>Dependent variable:</i>	
	Retaliate	Conciliate
Fear Treatment	0.444*** (0.111)	0.262 [^] (0.147)
Anger Treatment	0.070 (0.111)	-0.031 (0.146)
Reassurance Treatment	0.230* (0.113)	0.065 (0.149)
Constant	5.490*** (0.076)	4.771*** (0.101)
Observations	1,108	1,108
F Statistic (df = 3; 1104)	6.159***	1.541

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table F.4 displays the results of OLS models testing the direct effect of manipulated fear and manipulated anger on support for retaliation (model 1) and support for conciliation (model 2).

Table F.5: Direct Effects of Emotions on Motives for Retaliation
(Manipulated Mediator Only Arms)

	<i>Dependent variable:</i>			
	Punish	Deter	Incapacitate	Rehab
Fear Treatment	0.218* (0.108)	0.300** (0.107)	0.288* (0.113)	-0.138 (0.164)
Anger Treatment	0.042 (0.108)	0.038 (0.107)	0.042 (0.112)	-0.160 (0.164)
Reassurance Treatment	0.233* (0.110)	0.091 (0.109)	0.120 (0.114)	-0.231 (0.167)
Constant	5.657*** (0.074)	5.807*** (0.074)	5.660*** (0.077)	4.164*** (0.113)
Observations	1,104	1,104	1,104	1,103
F Statistic	2.368 [^] (df = 3; 1100)	2.995* (df = 3; 1100)	2.488 [^] (df = 3; 1100)	0.686 (df = 3; 1099)

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table F.5 displays the results of OLS models testing the direct effect of manipulated fear and manipulated anger on motives for retaliation: rehabilitation (model 1), incapacitation (model 2), deterrence (model 3), and punishment (model 4).

Appendix G Twitter Analysis

G.1 Case Selection

The list of relevant attacks for the observational Twitter analysis is generated using the Global Terrorism Database (GTD) (LaFree and Dugan 2007). The following criteria were used to identify relevant attacks: 1) occurred between 2010-2019, 2) took place in the United States, 3) caused at least two fatalities, not including the attackers themselves, 4) was perpetrated by attackers GTD identified as Islamist or as White Nationalist. I also exclude school shootings from this analysis, even though some of these attackers do exhibit some evidence of White Nationalist belief systems. There are seventeen attacks in GTD that meet this criteria. These attacks are listed in Table G.1 below.¹⁰

Table G.1: Lethal Terrorist Attacks in the United States (2010-2019)

	Date	Attack Name	Perpetrator	Lethality	# of Tweets
1	08/05/2012	Sikh Temple Shooting	WN	6	21,237
2	04/15/2013	Boston Marathon Bombing	Islamist	3	485,053
3	04/13/2014	Overland Park Shooting	WN	2	4,200
4	02/10/2015	Chapel Hill Shooting	WN	3	4,684
5	06/17/2015	Charleston Church Shooting	WN	9	48,031
6	07/16/2015	Chattanooga Military Shootings	Islamist	5	12,198
7	12/02/2015	San Bernardino Shooting	Islamist	14	19,839
8	06/12/2016	Orlando Pulse Shooting	Islamist	49	150,237
9	01/06/2017	Ft. Lauderdale Airport Shooting	Islamist	5	21,126
10	05/26/2017	Portland Train Attack	WN	2	3,844
11	10/31/2017	NYC Truck Ramming	Islamist	8	29,889
12	10/24/2018	Jeffersontown Kroger Shooting	WN	2	2,594
13	10/27/2018	Pittsburgh Tree of Life Shooting	WN	11	28,456
14	12/06/2019	Naval Air Base Pensacola Shooting	Islamist	3	5,045
15	07/28/2019	Gilroy Garlic Festival Shooting	WN	3	10,830
16	08/03/2019	El Paso Walmart Shooting	WN	23	74,934

¹⁰One of these seventeen attacks listed in the GTD – the racially motivated killings of two Black civilians in Seattle, is excluded since it happened the same week as a school shooting in Seattle, making it difficult to isolate tweets about this event.

G.2 Scraping Methodology

I use the Twitter Academic API to scrape the tweets related to each incident. Specifically, I build a query that selects keywords for each incident to search and scrape tweets. To minimize irrelevant tweets, I first include the city or town of the incident as a required keyword. To further increase the likelihood that the tweets are referencing the attacks, I include a set of additional keywords where at least one must explicitly appear in the tweet. These keywords include more specific locations relevant to each attack (e.g., “Walmart,” “Tree of Life”, etc.), the ideological or religious elements of the incident (e.g., “Islam,” “White Supremacy,” “Anti-immigrant,” “Islamophobic”), the possible organization involved in the incident (e.g., “ISIS”), as well as general keywords that describe the incident (e.g., “attack,” “terror,” “killing”). I restrict the time window to seven days, starting from the exact time when the incident happened.

G.3 LIWC Dictionary

In 2022, LIWC published an extensive update of their software, which included an overhaul of how emotions are coded (Boyd et al. 2022). In LIWC2015 (Pennebaker et al. 2015), non-emotion words were included in the emotions category (e.g. “attack” and “kill” were in the anger dictionary, as were all swear words). In the 2022 version, these words are no longer included and the dictionaries have only clear emotion words: the anger dictionary includes words like “hate” and “annoyed,” while the fear dictionary includes “nervous” and “afraid.” After reviewing every word in each dictionary, the words “terror” and “terroriz*” were removed from the fear dictionary and the words “violent” and “violently” were removed from the anger dictionary to ensure that Tweets simply describing, e.g. “a violent terror attack” were not coded as fearful or angry. Other than that, no changes were made to the base LIWC22 dictionary.

G.4 Analysis

Paired sample t-tests (Table G.2) comparing the average anger scores of tweets for each event to the average fear scores of tweets indicate that anger content is significantly higher than fear content in 9 out of 16 attacks. Fear is significantly higher than anger in only 4 attacks.

Table G.2: Paired Sample T-Tests Comparing Mean Anger to Mean Fear Scores (Figure 8)

Attack	Mean Anger	SD Anger	Mean Fear	SD Fear	t-stat	df
Oak Creek 2012	0.18	1.03	0.10	0.82	-8.37***	21,236
Boston 2013	0.09	0.81	0.16	1.06	38.65***	485,051
Overland Park 2014	0.86	2.49	0.09	0.73	-19.11***	4,198
Chapel Hill 2015	0.53	1.84	0.09	0.76	-15.22***	4,681
Charleston 2015	0.42	1.66	0.14	0.94	-31.82***	48,030
Chattanooga 2015	0.10	0.87	0.11	0.87	0.37	12,197
San Bernardino 2015	0.07	0.66	0.16	1.04	11.14***	19,801
Orlando 2016	0.40	1.57	0.22	1.14	-36.35***	150,236
Ft Lauderdale 2017	0.04	0.51	0.29	1.32	25.13***	21,124
Portland 2017	0.51	1.61	0.13	0.82	-13.20***	3,841
NYC 2017	0.12	0.82	0.14	0.87	2.47**	29,888
Jeffersontown 2018	0.60	1.48	0.13	0.65	-14.92***	2,591
Pittsburgh 2018	0.54	1.41	0.21	0.88	-33.91***	28,455
Gilroy 2019	0.14	0.73	0.20	0.88	4.70***	10,827
El Paso 2019	0.36	1.11	0.14	0.69	-46.45***	74,933
Pensacola 2019	0.09	0.60	0.10	0.59	1.01	5,044

Note: $\wedge p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

G.5 Robustness to Dictionary Specification

One potential problem with the LIWC22 overhaul is that the coding may now result in more false negatives for emotional content, since the dictionary is now more restricted. To address this potential issue with false negatives, I rerun the analysis including all the words from the “risk” dictionary (e.g. danger, flee) in the anxiety category and all the words from the new “moral” dictionary (e.g. unjust, reprehensible) in the anger category. Many of these risk and morality words used to be in the LIWC2015 anxiety and anger dictionaries, respectively, before the 2022 overhaul. The mean anxiety and anger scores with these more expansive dictionaries are displayed below in Figure G.1. Results are similar to those reported in the main paper, with anger being predominant in 12 out of 16 attacks and fear predominant in 3. Using the original, more expansive LIWC2015 dictionaries, all 16 attacks are significantly more likely to invoke angry tweets than fearful tweets (Figure G.2).

Figure G.1: Sentiment Analysis of Tweets Surrounding Real World Terror Attacks (Anger+Moral, Fear+Risk)

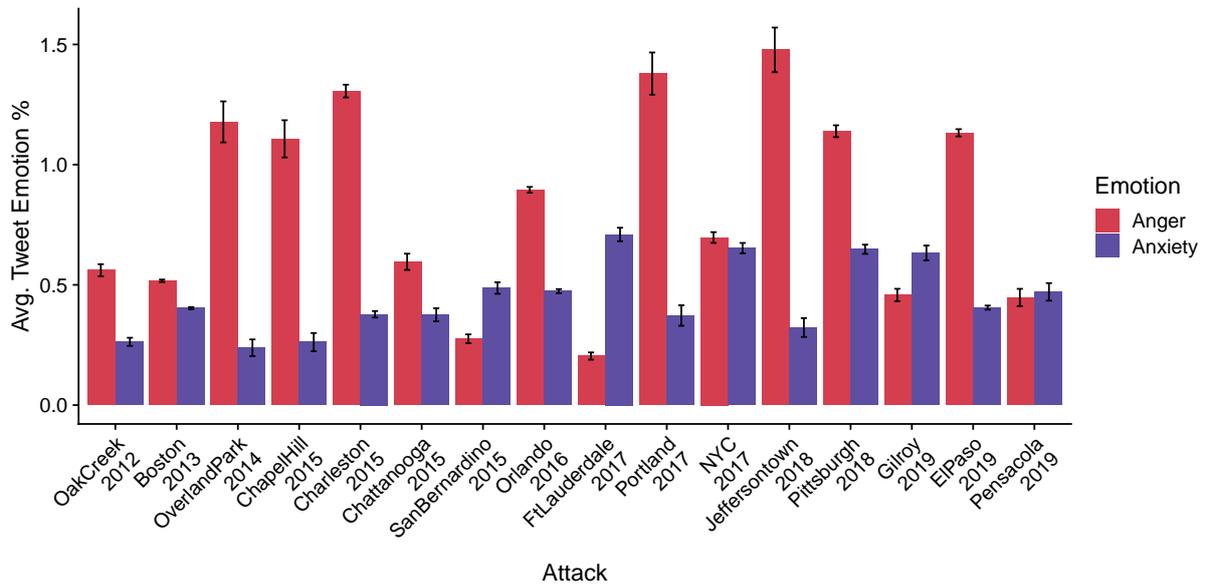


Figure G.1 displays the mean percentage of anger/morality (maroon) and anxiety/risk (purple) words in all tweets identified as relevant during the 7 days following each of the listed attacks. Error bars represent 95% confidence intervals.

Figure G.2: Sentiment Analysis of Tweets Surrounding Real World Terror Attacks (LIWC 2015)

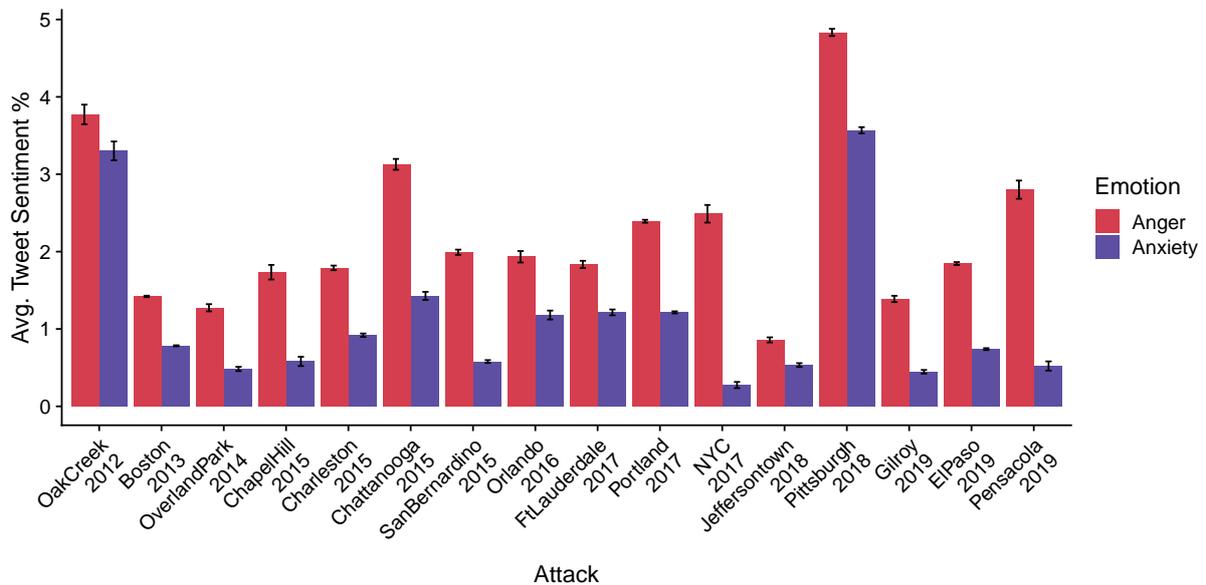


Figure G.2 displays the mean percentage of anger/morality (maroon) and anxiety/risk (purple) words in all tweets identified as relevant during the 7 days following each of the listed attacks. Error bars represent 95% confidence intervals.

Appendix H Regression Tables of Main Results

This section includes the full tables of all results reported in figure form or in the text of the main body of the article.

Table H.1: Paired Sample T-Tests of Reported Anger & Fear after News Exposure (Figure 3a)

Condition	Mean Anger	SD Anger	Mean Fear	SD Fear	t-stat
True Control	3.52	1.13	2.96	1.14	-8.71***
Jihadist-Civilian	3.75	1.06	3.09	0.99	-10.75***
Jihadist-Military	3.70	1.07	2.88	1.09	-11.41***
WN-Civilian	3.70	1.04	3.06	1.15	-10.61***
WN-Military	3.73	1.07	3.04	1.16	-11.05***

Note: $\wedge p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table H.1 displays the t-test results depicted visually in Figure 3a in the paper: comparing average self-reported anger to average self-reported fear within each of the natural mediator arms of the experiment.

Table H.2: ATE of Exposure to Different Attack News on Anger & Fear (Figure 3b)

	<i>Dependent variable:</i>	
	Anger	Fear
Jihadist-Civilian	0.230** (0.088)	0.131 (0.091)
Jihadist-Military	0.179* (0.090)	-0.076 (0.093)
White Nationalist-Civilian	0.174* (0.087)	0.107 (0.090)
White Nationalist-Military	0.214* (0.088)	0.086 (0.090)
Constant	3.525*** (0.062)	2.957*** (0.064)
Observations	1,466	1,466
F Statistic (df = 4; 1461)	2.215 [^]	1.685

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.2 displays the regression results depicted visually in Figure 3b in the paper: the effect of exposure to different forms of terrorism on self-reported anger and fear, using the natural mediator arms of the experiment only. The reference category is the true control (arm 1).

Table H.3: Support for Conciliation by Terrorism Type & Partisanship

	<i>Dependent variable:</i>				
	Support for Conciliation				
	(1)	(2)	(3)	(4)	(5)
Any Attack	0.136 (0.113)				
Military Victim		-0.261* (0.102)			-0.270^ (0.151)
White Nationalist Attacker			0.142 (0.103)	-0.101 (0.152)	
Independent				-0.542* (0.255)	-0.305 (0.251)
Republican				-0.819*** (0.156)	-0.612*** (0.150)
WN Attacker × Ind.				0.427 (0.350)	
WN Attacker × Rep.				0.412^ (0.216)	
Mil. Victim × Ind.					0.001 (0.349)
Mil. Victim × Rep.					0.003 (0.216)
Constant	4.771*** (0.100)	5.035*** (0.072)	4.834*** (0.074)	5.261*** (0.111)	5.340*** (0.106)
Observations	1,474	1,474	1,474	1,458	1,458
F Statistic	1.467	3.998**	1.689	6.127***	6.292***
df =	(1; 1472)	(2; 1471)	(2; 1471)	(8; 1449)	(8; 1449)

Note: ^p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.3 displays regression results described in Section 6.2 of the text: five OLS models regressing attack type on support for conciliation. Model 1 compares respondents exposed to any attack to those who were not. Model 2 compares attacks on the military to attacks on civilians. Model 3 compares attacks by White Nationalists to attacks by Jihadists. Models 4 and 5 include a 3-level partisanship variable interacted with attack type, with Democrats as the reference category. Analysis includes the natural mediator arms only.

Table H.4: Punitive Motives by Terrorism Type & Partisanship

	<i>Dependent variable:</i>				
	Punitive Motives				
	(1)	(2)	(3)	(4)	(5)
Any Attack	0.135 (0.086)				
Military Victim		0.006 (0.078)			-0.151 (0.117)
White Nationalist Attacker			0.063 (0.078)	0.482*** (0.116)	
Independent				-0.072 (0.194)	-0.375 [^] (0.193)
Republican				0.649*** (0.119)	0.069 (0.116)
WN Attacker × Ind.				-0.131 (0.266)	
WN Attacker × Rep.				-0.892*** (0.165)	
Mil. Victim × Ind.					0.454 [^] (0.268)
Mil. Victim × Rep.					0.239 (0.166)
Constant	5.657*** (0.076)	5.789*** (0.055)	5.759*** (0.057)	5.475*** (0.085)	5.806*** (0.081)
Observations	1,470	1,470	1,470	1,454	1,454
F Statistic	2.486	1.245	1.562	5.649***	2.135**
df =	(1; 1468)	(2; 1467)	(2; 1467)	(8; 1445)	(8; 1445)

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.4 displays regression results described in Section 6.2 of the text: five OLS models regressing attack type on punitive motives. Model 1 compares respondents exposed to any attack to those who were not. Model 2 compares attacks on the military to attacks on civilians. Model 3 compares attacks by White Nationalists to attacks by Jihadists. Models 4 and 5 include a 3-level partisanship variable interacted with attack type, with Democrats as the reference category. Analysis includes the natural mediator arms only.

Table H.5: Anger & Punitive Motives by Perpetrator Identity & Partisanship (Figure 4)

	<i>Dependent variable:</i>	
	Anger	Punitive Motives
White Nationalist Attacker	0.199* (0.094)	0.482*** (0.116)
Independent	0.075 (0.157)	-0.072 (0.194)
Republican	0.393*** (0.096)	0.649*** (0.119)
WN Attacker × Ind.	-0.240 (0.216)	-0.131 (0.266)
WN Attacker × Rep.	-0.405** (0.134)	-0.892*** (0.165)
Constant	3.545*** (0.068)	5.475*** (0.085)
Observations	1,452	1,454
Residual Std. Error	1.070 (df = 1443)	1.320 (df = 1445)
F Statistic	3.768*** (df = 8; 1443)	5.649*** (df = 8; 1445)

Note: $\wedge p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table H.5 displays the regression results depicted visually in Figure 4 in the paper: the effect of exposure to an attack by White Nationalists (compared to an attack by Jihadists) on self-reported anger and punitive motives for Democrats, Independents, and Republicans, using the natural mediator arms of the experiment only. The reference category is a Jihadist attack.

Table H.6: Indirect Effect of Emotions on Political Attitudes (Figure 5)

	<i>Dependent variable:</i>	
	Retaliate	Conciliate
Fear Prime	0.123* (0.057)	-0.091 (0.074)
Anger Prime	0.101^ (0.057)	-0.013 (0.075)
Reassure Prime	0.007 (0.058)	-0.022 (0.075)
Constant	5.640*** (0.040)	4.908*** (0.052)
Observations	4,490	4,490
F Statistic (df = 3; 4486)	2.430^	0.600

Note: ^p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.6 shows the results of the OLS models graphed in Figure 5 of the main text: regressing the randomly assigned emotion prime on support for retaliation (model 1) and conciliation (model 2). This analysis compares respondents who read about different terror attacks but received the same emotion prime to those who read about terror attacks but received no prime (the omitted baseline condition).

Table H.7: Indirect Effect of Emotions on Motives for Retaliation (Figure 6)

	<i>Dependent variable:</i>			
	Rehab	Incapacitate	Deter	Punish
Fear Prime	-0.121 (0.083)	0.138* (0.058)	0.038 (0.053)	0.056 (0.056)
Anger Prime	0.025 (0.084)	0.163** (0.058)	0.059 (0.054)	0.137* (0.056)
Reassure Prime	0.065 (0.085)	0.058 (0.059)	-0.014 (0.054)	0.010 (0.057)
Constant	4.165*** (0.058)	5.659*** (0.041)	5.916*** (0.038)	5.792*** (0.039)
Observations	4,474	4,476	4,476	4,475
F Statistic	1.814 (df = 3; 4470)	3.264* (df = 3; 4472)	0.754 (df = 3; 4472)	2.414^ (df = 3; 4471)

Note: ^p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.7 shows the results of the OLS models graphed in Figure 6 of the main text: regressing the randomly assigned emotion prime on different motives for retaliation. Same conditions as Table H.6.

Table H.8: Interaction of Party ID and Perpetrator ID on Attitudes
(No Emotion Prime, Figure 7)

	<i>Dependent variable:</i>	
	Punitive Motives	Retaliation
White Nationalist Attacker	0.464** (0.117)	0.485** (0.118)
Independent	-0.116 (0.195)	-0.127 (0.197)
Republican	0.638** (0.119)	0.592** (0.121)
WN Attacker × Ind.	-0.033 (0.268)	-0.101 (0.271)
WN Attacker × Rep.	-0.877** (0.165)	-0.690** (0.167)
Constant	5.491*** (0.085)	5.323*** (0.086)
Observations	1,414	1,418
F Statistic	5.428*** (df = 8; 1405)	5.413*** (df = 8; 1409)

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.9 shows the results of the OLS models graphed in Figure 7 of the main text (left panel): regressing the interaction of party ID and attacker ID on punitive motives (model 1) and support for retaliation (model 1) among those in the *no prime* conditions.

Table H.9: Interaction of Party ID and Perpetrator ID on Attitudes
(Anger Prime, Figure 7)

	<i>Dependent variable:</i>	
	Punitive Motives	Retaliation
White Nationalist Attacker	0.120 (0.116)	0.222 [^] (0.120)
Independent	-0.070 (0.207)	0.056 (0.214)
Republican	0.221 [^] (0.118)	0.452 ^{**} (0.122)
WN Attacker × Ind.	-0.339 (0.296)	-0.484 (0.305)
WN Attacker × Rep.	-0.141 (0.167)	-0.332 [^] (0.172)
Constant	5.857 ^{***} (0.084)	5.539 ^{***} (0.087)
Observations	1,317	1,323
F Statistic	2.289 ^{**} (df = 8; 1308)	3.236 ^{***} (df = 8; 1314)

Note: [^]p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table H.9 shows the results of the OLS models graphed in Figure 7 of the main text (right panel): regressing the interaction of party ID and attacker ID on punitive motives (model 1) and support for retaliation (model 1) among those in the *anger prime* conditions.

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