

# How Terrorism Does (and Does Not) Affect Citizens' Political Attitudes: A Meta-Analysis

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**Abstract:** *How does terrorism affect citizens' political attitudes? Over the years, many scholars have tried to answer this question. This article performs a meta-analysis on this literature, reviewing about 325 studies conducted between 1985 and 2020 on more than 400,000 respondents. The findings confirm that terrorism is associated—to a small but significant extent—with outgroup hostility, political conservatism and rally-'round-the-flag effects. At the same time, the effects of terrorism vary widely, with studies on Islamist violence, conducted in the United States or Israel, and using cross-sectional data yielding stronger results on average. Finally, the review reveals remaining gaps in this field of study, including a lack of research on non-Islamist violence or conducted in non-Western contexts. Taken together, this meta-analysis consolidates existing evidence, determines which results hold across contexts, and identifies key gaps in our current knowledge. Its data can also be accessed interactively via a Shiny App.*

**Verification Materials:** The data and materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/K4L5YI>.

“Terrorism is [an] act of violence ... to try to influence a public body or citizenry, so it's more of a political act.” – James Comey, former FBI Director (2015)

Terrorists, unlike criminals, are assumed to fight for a *political* cause (Hoffman 2017; Richards 2014), often by using violent tactics aimed at attracting public attention and altering public opinion (Kydd and Walter 2006; see also Comey 2015, above). Consequently, a prominent tradition within political science posits that “the success of this strategy [terrorism] relies on key assumptions about how terrorism will impact public opinion” (Wayne 2019, 136) or that “the responses of ordinary citizens constitute a central causal mechanism through which terrorism operates” (Huff

and Kertzer 2018, 55; see also Crenshaw 1986). In short, public opinion plays a key role in our understanding of how terrorism works. But, how and to what extent does terrorism affect “ordinary citizens”? Are we, as often expected, more likely to vote for authoritarian politicians in times of terror? Are we more willing to compromise some of our civil liberties and democratic values for greater safety and security in the wake of an attack? And what or who are we talking about when we talk about “terrorism”?

Since the September 11 attacks in 2001 (9/11), an impressive body of literature has accumulated on these questions, but the answers point in different directions. For example, while some authors worry that public reactions to terrorism put “democracy at risk” (Merolla and Zechmeister 2009), others state that such reactions are

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“limited in size” (Sniderman et al. 2019, 246), “short-lived” (Arvanitidis, Economou, and Kollias 2016, 231), or even nonexistent (Castanho Silva 2018; Larsen, Cutts, and Goodwin 2020; Nussio 2020). Studies, however, often examine different types of outcomes, countries, and terrorist threats using a wide variety of methods. **Yet, to date, there is still no systematic overview of how we have been studying the public’s response to terrorism.** As a result, little is known about the main features of this field or how these features, in turn, color conclusions.

About 20 years after 9/11, this article takes stock of the quantitative literature on public responses to terrorism. After touching upon the literature’s theoretical foundations, **I use meta-analytical techniques to assess the empirical evidence for the relationship between terrorism and political attitudes.** Above all, I ask to what extent findings can be generalized across different types of outcomes (e.g., outgroup hostility versus political conservatism versus rally effects), terrorist threats (e.g., Islamist terrorism versus other threats), and methodological features (e.g., experimental versus correlational evidence). To do so, I collected an exceptionally large dataset ( $N_{\text{effect sizes}} = 1,733$ ;  $N_{\text{manuscripts}} = 241$ ;  $N_{\text{unique studies}} = 326$ ), which allows for a systematic analysis of data from more than 400,000 respondents spread over 35 years and about 30 countries. **The meta-analysis reveals that terrorism is associated with relatively small, yet statistically significant, increases in outgroup hostility ( $r = 0.126$ ), political conservatism ( $r = 0.131$ ), and rally-‘round-the-flag reactions ( $r = 0.090$ ).** At the same time, the effects are highly dependent on study characteristics. Meta-regressions show **how violent acts perpetrated by Islamist actors in Western countries, and especially in the United States, generate substantially stronger responses.** Rally-‘round-the-flag responses, in particular, seem to be driven by a rally ‘round President Bush effect in the wake of 9/11. However, it is important to note in this respect that studies on non-Muslim violence or conducted in non-Western contexts remain virtually nonexistent.

This study contributes to the literature on empirical, theoretical, and normative grounds. Empirically, it offers the most comprehensive overview of research into the effects of terrorism on citizens’ attitudes, including interactive access to the data via a so-called “Shiny App.”<sup>1</sup> Meta-analyses in political science are still scarce (see Blair, Christensen, and Rudkin 2021, 710, for a similar observation), even though they provide powerful tools to consolidate existing evidence and identify avenues for future research. In this regard, this meta-analysis questions whether terrorism truly puts “democracy at risk”

(Merolla and Zechmeister 2009) and draws attention to the lack of research into certain forms of political violence or carried out in certain parts of the world. The study also highlights the event-driven nature of this field and demonstrates how estimates of public responses to terrorism vary considerably depending on substantive and methodological factors. Consequently, theoretically, this review advocates complementing microlevel theorizing about affective and cognitive determinants of public reactions to terrorism with macrolevel theorizing about national and transnational scope conditions. As we will see below, most studies on public responses to terrorism draw on individual-level theories, whereas the meta-regressions suggest that such responses are often conditioned by the contexts in which they occur. As a result, an integrative approach, focusing on how determinants at different levels of analysis interact with one another, would enable a greater and more accurate accumulation of knowledge. Finally, normatively, the findings add to the debate about which acts are more likely to be considered, and studied, as “terrorism.” As this review demonstrates, the differential labeling of similar acts of political violence has important societal and scholarly implications. Before scientists can draw substantive conclusions that can reliably guide resilience-building strategies and policies in the wake of terrorism, these gaps must be addressed—both theoretically and empirically.

## How and Why Terrorism Affects Attitudes

For over two decades, and especially since the 9/11 attacks, researchers have sought to understand how and why the public reacts to terrorist acts and threats. Empirically, this literature shows how terrorism often evokes attitudinal and behavioral reactions characterized by *outgroup hostility* and *ingroup solidarity* (Van Hauwaert and Huber 2020). More specifically, acts and threats of (predominantly Islamist, see below) terrorism are found to harden people’s attitudes towards outgroups in general (Echebarria-Echabe and Fernández-Guede 2006) and towards Muslims, Arabs, and immigrants and refugees in particular (Ferwerda, Flynn, and Horiuchi 2017; Hopkins 2010; Panagopoulos 2006). This often translates into support for restrictions on the rights and liberties of those disliked groups (Davis 2007) or more stringent anti-immigration policies (Kim 2016). Yet, beyond policy preferences regarding outgroups, evidence also points in the direction of a more general conservative shift in times of terror. For example, studies have documented increased support for retaliatory, military, and

<sup>1</sup><https://ameliegodefroidt.shinyapps.io/terrorism-attitudes-metaanalysis/>

conflict-perpetuating—instead of conciliatory and diplomatic—solutions to terrorism (Bar-Tal and Labin 2001; Fisk, Merolla, and Ramos 2019; Rovenpor et al. 2019), a stronger preference for national security at the expense of civil liberties (Davis and Silver 2004), and a boost in popularity for far-right and authoritarian political parties and politicians (Lindén, Björklund, and Bäckström 2018; Marcus et al. 2019; Nagoshi, Terrell, and Nagoshi 2007; Vasilopoulos et al. 2019). Additionally, as yet another way of coping with terrorism-induced trauma, citizens equally tend to bolster their attachment to and trust in the nation and its leaders (Dinesen and Jæger 2013; Feinstein 2018; Ladd 2007; Lambert et al. 2010; Landau et al. 2004; Van Hauwaert and Huber 2020)—a tendency often labeled “rally-round-the-flag” (Mueller 1970). In sum, the extant literature indicates that terrorism, on average,<sup>2</sup> leads to surges in outgroup hostility, political conservatism, and rally-round-the-flag sentiments.

Now, which mechanisms help us to understand these responses? Theoretically, both cognition and emotion are argued to play a crucial role in motivating collective coping mechanisms in times of terror.<sup>3</sup> On a cognitive level, terrorism is thought to prime the inevitability and unpredictability of death (Greenberg et al. 1990; Pyszczynski, Solomon, and Greenberg 2003; Rosenblatt et al. 1989), trigger the idea that oneself and one's country is in danger (Huddy et al. 2002), heighten perceptions of injustice and moral violations (Lambert, Eadeh, and Hanson 2019; Skitka and Mullen 2002; Wayne 2019), and prompt particular blame attributions (Kimhi, Canetti-Nisim, and Hirschberger 2009; Sadler et al. 2005). At a

more abstract level, terrorism thus challenges basic human assumptions about the world as being predictable, safe, and benign (Canetti et al. 2013, 267; Janoff-Bulman 1992). The motivated social cognition approach argues that when confronted with “a world that appears dangerous and unpredictable,” people—even self-identified liberals—will adhere more strongly to “conservative, authoritarian, and right-wing candidates, policies, and ideologies” (Jost et al. 2003; 2017, 326–27).

On an affective level, terrorism elicits a complex state of negative emotional arousal. In the immediate aftermath of attacks, citizens often feel anxious and scared, angry and outraged, sad and dejected. One of the major advances in political psychology of the last two decades has been to move away from the dominant valence-based approach of identifying the unique roles played by positive versus negative affect in explaining human behavior. Instead, recent studies have demonstrated how discrete emotions of the same valence (e.g., various negative emotions) may entail different effects in the context of intergroup conflict (Pliskin and Halperin 2020). In the realm of terrorism-effects studies, feelings of anger in the wake of an “unjust” attack are believed to stoke a desire for more high-risk and retaliatory measures such as military action (Fisk, Merolla, and Ramos 2019; Liberman and Skitka 2019; Wayne 2019) or far-right voting (Vasilopoulos et al. 2019)—with fear being a driving force behind support for more risk-averse and precautionary measures such as the deportation of immigrants (Skitka et al. 2006), increased isolationism (Huddy et al. 2005), or ethnic profiling (Schildkraut 2009).

Despite these valuable insights, our current understanding of how terrorism shapes social and political attitudes remains incomplete. First, to date, a systematic overview of how scholars have been studying this topic is still lacking. As a result, little is known about the signature features of and remaining gaps within this field of study. Second, the average strength of the relationship between terrorism and public attitudes remains unclear—which gives rise to contradictory narratives. While some authors worry that public reactions to terrorism put “democracy at risk” (Merolla and Zechmeister 2009), others assert that such reactions are “limited in size” (Sniderman et al. 2019, 246), “short-lived” (Arvanitidis, Economou, and Kollias 2016, 231), or, in some cases, even nonexistent (Castanho Silva 2018; Larsen, Cutts, and Goodwin 2020; Nussio 2020). Third, it remains unclear to what extent findings can be generalized across different research contexts and designs or, conversely, to what extent substantive and/or methodological decisions have shaped conclusions. This study addresses these gaps by systematically reviewing, consolidating, and comparing existing empirical evidence on the relationship

<sup>2</sup>Recently, studies have started to unravel moderators affecting to what extent terrorism affects attitudes. *Individual-level* moderators include citizens' gender (Lindner 2018; Lizotte 2017), ethnicity (Lavi et al. 2014; Shoshani and Slone 2016), educational level (Ferrín, Mancosu, and Cappiali 2020), political predispositions (Castanho Silva 2018; Castano et al. 2011; Hetherington and Suhay 2011; Nail et al. 2009; Van de Vyver et al. 2016), political knowledge (Carriere, Hendricks, and Moghaddam 2019), ingroup attachment (Asbrock and Fritsche 2013; Bilali 2015), and motivation to control prejudice (Jacobs and van Spanje 2021; Sobolewska, Ford, and Sniderman 2017; Steen-Johnsen and Winsvold 2020). *Country- and context-level* moderators include unemployment rates (Castanho Silva 2018; Legewie 2013), the local migration context (Castanho Silva 2018; Nussio, Bove, and Steele 2019), geographical proximity (Finseraas and Listhaug 2013; Nussio, Bove, and Steele 2019), political-ideological climate (Ferrín, Mancosu, and Cappiali 2020), and prevailing social norms (Álvarez-Benjumea and Winter 2020). *Terrorism-specific* moderators include the terrorist's gender (Lindner 2018), ideology (Jacobs and van Spanje 2021; Piazza 2015), and the way terrorism is communicated and framed (Bruneau, Kteily, and Urbiola 2020; Canetti et al. 2018; Gadarian 2010; von Sikorski et al. 2017).

<sup>3</sup>The main datafile in the Replication Materials lists all theories on which the manuscripts included in this meta-analysis draw (see <https://doi.org/10.7910/DVN/K4L5Y1>).

between terrorism and outgroup hostility (Hypothesis 1), political conservatism (Hypothesis 2) and rally-'round-the-flag responses (Hypothesis 3).<sup>4</sup>

## Review Protocol

To assess the empirical evidence on public responses to terrorism, I conducted a meta-analysis. A meta-analysis is a statistical tool to, first, consolidate the overall effect size within a field of interest and, second, assess the conditions under which effects are larger or smaller (Borenstein et al. 2009). To this end, three main tasks have to be performed: (1) collecting as much published and unpublished work on the topic as possible, (2) converting previous findings into a common effect size to allow for cross-study comparisons; and (3) synthesizing all findings using models that account for complex clustering in the dataset (i.e., effect sizes are clustered within manuscripts; see below). These three steps are further explained in the next sections.

## Data Collection and Coding

In this article, I draw on a unique database encompassing 241 manuscripts that describe 326 studies and report 1,733 unique estimates. To obtain this large-scale database, a complementary four-step strategy was used, including an electronic search, a public call for additional studies, a screening of related review articles, and a forward and backward search (for more information, see SI Appendix §B.1, pp. 3–4). This yielded 12,133 possibly relevant records. After deleting duplicate files ( $N = 1,742$ ), 10,391 records were screened for inclusion in the meta-analysis based on a list of predefined inclusion and exclusion criteria (SI Table B.1, p. 4). Most importantly, key concepts of interest were defined and demarcated beforehand to ensure conceptual comparability between the included manuscripts. Based on these criteria, a final set of 241 manuscripts was retained. The PRISMA flowchart in Figure 1 summarizes the data collection and selection process.

These 241 manuscripts were then coded. Each reported association between terrorism and sociopolitical attitudes that met the selection criteria was coded as a separate case. A total of 1,733 associations were collected and coded. Based on a random selection of manuscripts

( $\pm 15\%$  of the final sample), the codebook to extract the necessary data was piloted and revised where necessary. The final codebook included six main categories of data. Specifically, for each association, information was extracted related to the manuscript (e.g., publication status), study (e.g., research design), sample and its setting (e.g., country of study), independent variable (e.g., type of terrorism), dependent variable (e.g., type of outcome measure, reliability), and original test statistic and effect size(s).<sup>5</sup>

## Data Preprocessing

Because the original manuscripts ( $j = 1, 2, \dots, J$ ) used different ways to report the association ( $i = 1, 2, \dots, I$ ) between terrorism and sociopolitical attitudes (e.g., correlation coefficients, regression coefficients, odds ratios, mean differences, etc.), I first converted each association into a Pearson's correlation coefficient and calculated its corresponding sampling variance. This ensured that the estimates were numerically comparable. Yet, to meaningfully compare and pool effect sizes, all correlations must also reflect the relation between terrorism and sociopolitical attitudes in the same direction. Hence, where necessary, the direction of the correlation coefficient was changed, so that a positive value reflected an association between terrorism and higher levels of prejudice, xenophobia, conservatism, authoritarianism, political trust, nationalism, etc. Last, the sampling distribution of the correlation coefficient is skewed (i.e., nonnormal) unless the population correlation is close to zero or unless the sample size is sufficiently large (Cheung 2015a). Therefore, a Fisher's Z transformation was applied to approximate normally distributed effect sizes, by using the following formula:

$$z = \frac{1}{2} \log \frac{1+r}{1-r}. \quad (1)$$

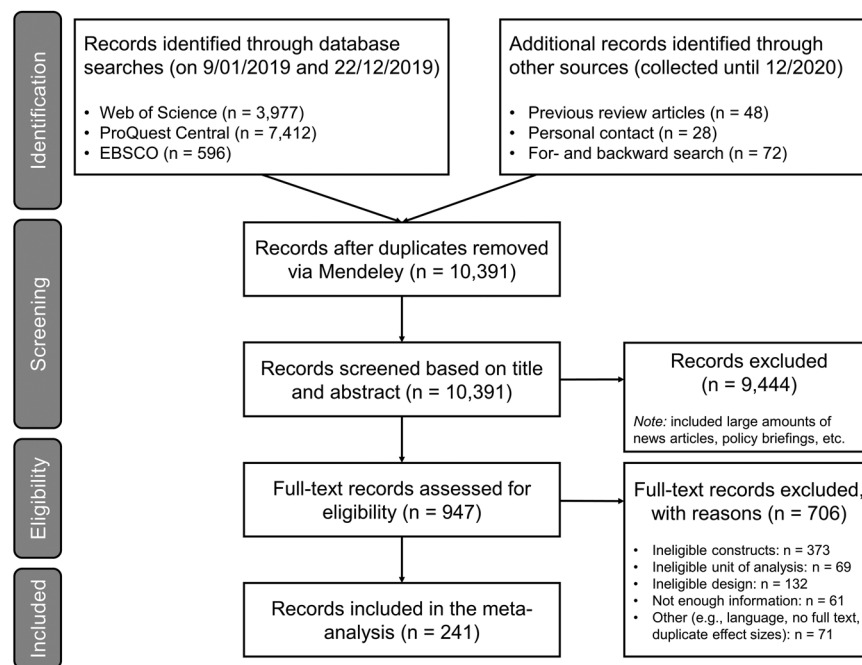
## Statistical Analyses

All Fisher's Z transformed correlation coefficients ( $N = 1,733$ ) were used as inputs to a *random-effects, three-level* meta-analysis. A three-level meta-analysis assumes that observed effect sizes differ because of (1) sampling variance, (2) variance between manuscripts, and (3) variance between the correlations from within the same manuscript (Cheung 2014; 2015a; Van den Noortgate

<sup>4</sup>Hypotheses 1 and 2 were preregistered; Hypothesis 3 was discussed in the review protocol but not preregistered (see supporting information [SI] Appendix §1, pp. 1–2).

<sup>5</sup>The full codebook is included in the Replication Materials at <https://doi.org/10.7910/DVN/K4L5YI>.



**FIGURE 1 PRISMA Flowchart of Selection Process**

Note: The flowchart shows the meta-analysis data-collection process. “Records excluded” were excluded because the title or abstract did not reflect the subject matter of the meta-analysis or because the records appeared to be incomplete, unavailable, or nonacademic. “Full-text articles excluded” were excluded due to a failure to meet the inclusion criteria (see SI Table B.1, p. 4). The “Records included in the meta-analysis” refers to a published or unpublished collection of unique studies.

et al. 2013).<sup>6</sup> Mathematically, I thus estimated the following model:

$$y_{ij} = \beta_0 + u_{(2)ij} + u_{(3)j} + e_{ij}, \quad (2)$$

where any observed effect size in manuscript  $j$  ( $y_{ij}$ ) is assumed to be equal to an overall population effect size ( $\beta_0$ ), plus a random deviation of the mean population effect size in manuscript  $j$  from this overall population effect size ( $u_{(3)j}$ ), plus a random deviation of the  $i$ -th population effect size in manuscript  $j$  from the mean effect in this particular manuscript ( $u_{(2)ij}$ ), plus a random error

<sup>6</sup>More commonly used two-level models only account for (1) sampling variance and (2) variance in effect sizes across different manuscripts. In other words, these models ignore that manuscripts often report more than one effect size. In this meta-analysis, manuscripts report seven effect sizes on average to quantify the relationship between terrorism and political attitudes ( $M = 7.191$ ,  $SD = 8.189$ , range = 1–52). This violates the assumption of nonindependent effect sizes because effect sizes within a manuscript are likely to be more similar than effect sizes from different manuscripts. Consequently, I used a three-level meta-analytic model accounting for the dependency between correlations from the same manuscript. See SI Appendix §B.2 (pp. 4–8) for more information on multilevel meta-analyses in general and three-level meta-analyses in particular.

deviation of the observed effect size from the population effect due to sampling fluctuation ( $e_{ij}$ ). All three error terms are assumed to be independent and normally distributed with zero mean. The parameters of interest in this meta-analysis are the overall effect size ( $\beta_0$ ), the between-manuscript variance component ( $\tau_{(3)}^2$ ), and the within-manuscript variance component ( $\tau_{(2)}^2$ ). While the former denotes the overall correlation found across all effect sizes, the latter two estimates indicate whether there are significant differences between effect sizes between and within manuscripts. Typically, the sampling variance ( $\tau_e^2$ ) is not estimated but considered as known given that it can be derived based on the sample size.<sup>7</sup>

In plain language, a three-level approach allows me to consolidate an overall effect size using all 1,733 effect sizes (while properly accounting for clustering in the data), to study heterogeneity in effect sizes both between and within manuscripts, and to explore moderator variables that explain part of this variation between and

<sup>7</sup>For a Fisher’s  $Z$ , as used in this meta-analysis, it has been shown that the sampling variance is equal to  $\frac{1}{(n-3)}$ , with  $n$  equal to the sample size (Cheung 2015a).

within manuscripts. To that end, the model above can be extended by including characteristics of the manuscripts ( $x_j$ ) and of the effect sizes within manuscripts ( $x_{ij}$ ) as moderators:

$$y_{kj} = \beta_0 + \beta_1 x_{ij} + \beta_2 x_j + u_{(2)ij} + u_{(3)j} + e_{ij}. \quad (3)$$

Importantly, when assessing moderator effects of categorical variables, one dummy indicator per category was included in the model, and the intercept was constrained to zero. The advantage of this parameterization is that the regression coefficients can be interpreted as the average effect sizes for each category. When assessing the moderator effect of continuous variables, the variables were centered around their mean to improve numerical stability (Cheung 2015a). Parameter estimates were considered significant when the 95% likelihood-based confidence intervals (LBCI) did not include zero.<sup>8</sup>

Finally, I probed the robustness of the empirical findings with several additional analyses. First, I conducted a series of sensitivity analyses, which shows that the results reported below are not driven by outliers in effect size or precision (SI Table C.6, p. 14) and that they are robust to alternative model specifications (SI Table C.5, p. 13), a basic assessment of study quality (SI Table C.7, p. 15), and excluding correlations derived from regression coefficients (SI Table C.8, p. 16). Second, I performed diagnostic tests to detect publication bias (SI Table C.9, p. 17) and assessed whether this skewed the estimates (SI Table C.10, p. 18). In general, there is not much evidence of publication bias (with an exception for the rally-round-the-flag subsample; see SI Appendix §C.3, pp. 16–17, for more details).

## Results

In this section, I first outline the signature features in this field of study. Here, data points refer to unique effect sizes quantifying the relationship between terrorism and political attitudes ( $N_{\text{effect sizes}} = 1,733$ ), whereas manuscripts ( $N_{\text{manuscripts}} = 241$ ) denote a published or unpublished collection of one or more unique studies ( $N_{\text{studies}} = 326$ ). Then, I assess the magnitude of the relationship between terrorism and each of the three outcomes of interest before exploring potential moderators of these relationships.

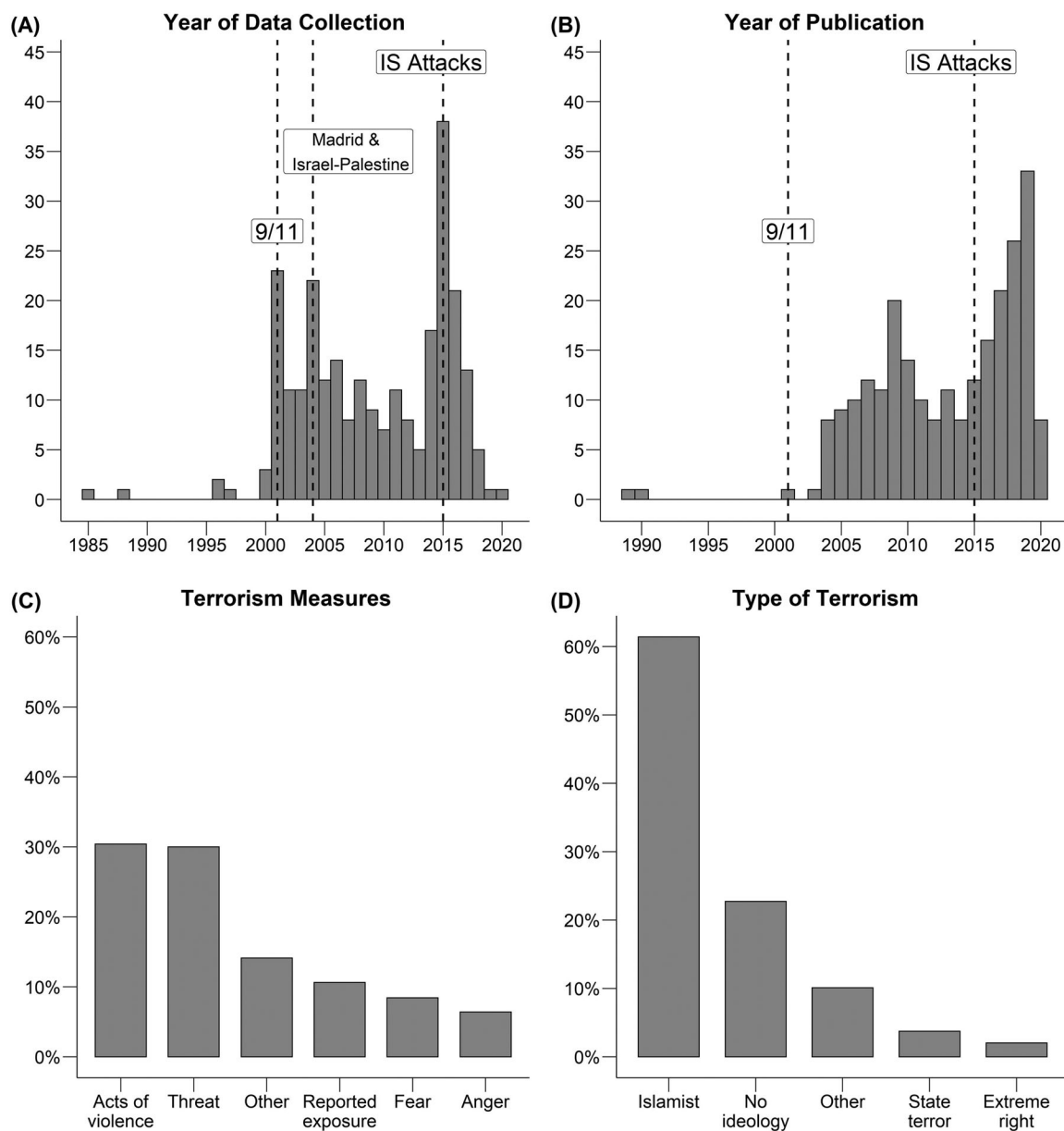
<sup>8</sup>All meta-regressions used the maximum-likelihood estimation procedure implemented in the meta3 function in R (Cheung 2015b). LBCIs are used instead of Wald confidence intervals (CIs) because they are somewhat more accurate (see Cheung 2015a for a comparison of the properties of Wald CIs and LBCIs).

## The Field of Terrorism Effects Studies

What are the signature features of this field of study? By answering this question, I aim to provide a rich description of this field of study and identify remaining gaps and issues. Academic interest in the association between terrorism and public attitudes clearly commenced after the 9/11 attacks and was given an extra boost with the 2015–16 Islamic State (IS) attacks (Figure 2(A) and (B)). In other words, Figure 2(A) and (B) suggest that this field of study is to a large extent event driven. Furthermore, over the years (i.e., 1985–2020), exposure to terrorism has been operationalized in various ways (Table 1, Figure 2(C)). Sometimes participants were exposed to a news article or video clip about a particular act of violence (e.g., 9/11), whereas at other times exposure happened more naturally when a terror attack occurred at the same time a survey was being fielded (acts of violence: 527 data points, 30%). Sometimes studies gauged or manipulated citizens' perception of the threat of terrorism (threat of violence: 520 data points, 30%), while other studies were more interested in affective appraisals (fear: 146 data points, 8%; anger: 111 data points, 6%). And yet other studies asked respondents to report their own, relatives' or media exposure to terrorist acts of violence (self-reported exposure: 184 data points, 11%).

The most frequently studied acts of violence are—perhaps unsurprisingly—9/11, the Israeli-Palestinian conflict, and the 2015 series of IS attacks in Paris. Consequently, regarding the ideology behind the violence (Figure 2(D)), the vast majority of effect sizes quantify the effects of or relationship with Islamist terrorism (1,064 data points, 61%), whereas the share of information on how the public reacts to extreme-right terror is remarkably low (35 data points, 2%). It is interesting to note in this respect that only a handful of studies includes an explicit definition of “terrorism”—leading to the implicit assumption within this field of study that “terrorism” is a clear-cut “you-know-it-when-you-see-it” phenomenon. Furthermore, studies on how the public responds to terrorism have been conducted in about 30 countries but predominantly in the United States (122 studies, 37%), Israel (63 studies, 19%), and 15 European countries (163 studies, 50%). Only seven studies (2%) are conducted in non-Western contexts (i.e., two in Nigeria and one each in Colombia, Egypt, Morocco, Pakistan, and South Africa).

In addition to a limited substantive and geographical scope, the review also reveals some important methodological gaps related to the research designs used. For instance, longitudinal studies are rare (22 studies, 7%),

**FIGURE 2 Summary of Data Included in the Meta-Analysis**

*Note.* Panel A displays the number of unique studies conducted over time, Panel B the number of manuscripts published over time, Panel C the frequency of used terrorism measures (in percentage), and Panel D the frequency of studied types of terrorism (in percentage).

making it harder to assess the long-term impact of terrorism. Instead, almost half of the studies employ a cross-sectional design (155 studies, 48%), with experiments and quasiexperiments used in 103 (32%) and 46 (14%) studies. Second, studies included in this meta-analysis are predominantly conducted among students (114 studies, 35%) and other convenience samples (such as online opt-in panels, snowball samples, or online/social media samples; 93 studies, 29%), while population samples

make up about a third of the data (119 studies, 36%). Third, most of what we know about how the public responds to terrorism is based on the responses of majority citizens. That is, most studies examine the opinions of a majority group in the country of study (e.g., Whites in the United States, Israeli Jews in Israel; 135 studies, 41%) or use a mixed sample (78 studies, 24%). Only 15 studies (5%) are conducted among participants of minority groups. The other 98 studies do not disclose

**TABLE 1 Measures and Manipulations of Citizens' Exposure to or Appraisals of Terrorism**

Overarching Category	Measures and Manipulations
Objective exposure	Pre- and postattack measures Days between an act of violence and survey day Newspaper vignette about an act of violence News clip about an act of violence
Self-reported exposure	Direct exposure to terrorism (e.g., witnessed, being injured) Indirect exposure via friends and family Indirect exposure via media reports
Cognitions	Self-reported concern/worry to become a victim of terrorism Self-reported concern/worry for an attack on the nation Manipulated vignette or writing task about the threat of terrorism (rather than about a specific act of violence) Manipulated percentage of the threat of terrorism Remuneration of the threat of terrorism (for oneself)
Emotions	Anger/outrage (targeted at "terrorism" in general or at a specific attack/organization) Fear/anxiety (idem) Sadness (idem) General negative valence (idem)
Other	Depression caused by terrorism Post-Traumatic Stress Disorder (PTSD) caused by terrorism Loss of resources (e.g., economic losses) caused by terrorism

information on the ethnic composition of their sample (30%). Finally, the mean age across all samples is 33 years ( $SD = 11.555$ ), studies include 55% women on average ( $SD = 13.554$ ), and the median effective sample size is 313, with a minimum of 22 and a maximum of 37,670 participants ( $M = 1338.939$ ,  $SD = 4160.132$ ).<sup>9</sup>

### Overall Effect Size

To what extent is terrorism associated with outgroup hostility, political conservatism, or rally-'round-the-flag responses? As these three types of outcome measures convey qualitatively different information about how the public responds to terrorism, they cannot easily be aligned with each other. Therefore, I split the dataset by hypothesis and estimated the overall correlation coefficient based on an intercept-only three-level meta-analytic model for each of the hypotheses separately (see Equation 2). Table 2 displays the estimated overall correlations ( $Z_r$ ). The correlations in this table are Fisher's  $Z$ -transformed, while those reported in the text below are

back-transformed to their normal correlation scale (denoted as  $\rho$ ) for ease of interpretation.

Table 2 shows that terrorism is significantly associated with outgroup hostility ( $\rho = 0.126$ ), political conservatism ( $\rho = 0.131$ ),<sup>10</sup> and, to a lesser extent, rally-'round-the-flag effects ( $\rho = 0.090$ ). In other words, the more someone is exposed to, concerned about, or angry because of terrorism, the more they will derogate "others," find solace in conservative policies, and bolster attachment to the nation and its leaders. At the same

**TABLE 2 Relationship between Terrorism and Political Attitudes**

Outcome Type	$k$	$j$	$Z_r$	LBCI
Outgroup hostility	645	126	0.126	[0.094; 0.159]
Conservative shift	728	144	0.132	[0.108; 0.156]
Rally effects	360	72	0.090	[0.055; 0.127]

Note.  $k$  = number of effect sizes.  $j$  = number of manuscripts.  $Z_r$  = Overall Fisher's  $Z$  correlation coefficients. LBCI = Likelihood-Based Confidence Interval. The  $Z_r$  estimates are considered significant when the LBCIs do not include zero.

<sup>9</sup>In this regard, it is noteworthy that 11 studies (3%) were coded as preregistered and are, therefore, assumed to have performed an a priori sample size calculation.

<sup>10</sup>The difference (at the third decimal) between the  $\rho$  reported in the text and the  $Z_r$  reported in Table 1 is due to the back-transformation.



time, it is important to note that all associations are relatively small, both following conventional standards (Cohen 1988; Gignac and Szodorai 2016) and compared to other meta-analyses in the social sciences.<sup>11</sup> To give an idea of what these estimates mean, consider a classic feeling thermometer in which respondents rate how warm they feel towards other people, groups, or institutions on a 0 (*cold*) to 100 (*warm*) scale. On average, terrorism moves a person who feels neutral about illegal immigrants or the Republican party (i.e., a rating of 50) about 7 points to mildly negative feelings toward illegal immigrants (43) and mildly positive feelings towards the Republican party (57).<sup>12</sup> Besides this substantive interpretation of the magnitude of the correlations, the results also entail practical implications for future studies. Specifically, when no other information (such as pilot estimates) is available, this meta-analysis recommends using sample sizes of at least 493, 453, and 967 to reach a power of 0.80 ( $\alpha = 0.05$ ) for future studies on the association between terrorism and outgroup hostility, conservatism, and rally effects, respectively.<sup>13</sup>

## Moderator Analyses

Finally, to what extent do these correlation coefficients hold across different contexts? The results indicate that the overall correlation coefficients gloss over substantial heterogeneity in effect sizes both within and between manuscripts.<sup>14</sup> Thus, when manuscripts report more than one effect size, these effect sizes within one

manuscript “are not merely direct replications of each other—there are true differences among them” (Cheung 2015, 205). Likewise, there are true differences between the results of different manuscripts. Therefore, in a final series of models, I investigate to what extent such differences in observed effect sizes can be explained by additional moderators. Below, I provide a short description and visual presentation of the significant moderators for each of the hypotheses (Figure 3), whereas all coefficients of the corresponding meta-regression models are available in SI Table C.1 (p. 10) for the outgroup-hostility hypothesis, SI Table C.2 (p. 11) for the conservative shift hypothesis and SI Table C.3 (p. 12) for the rally-‘round-the-flag hypothesis.<sup>15</sup> Readers can also further explore heterogeneity in responses via the replication files or the interactive tool (“Shiny App”) complementing this article.

**Outgroup Hostility.** First, several features of the independent variable impact the observed effect size quantifying the relationship between terrorism and outgroup hostility. For example, in addition to a dearth of research on non-Islamist terrorism, those few studies looking at non-Islamist terrorism also result in a weaker, and even nonsignificant, overall effect size. Specifically, the average correlation coefficient for studies on Islamist terrorism and outgroup attitudes is 0.121, compared to 0.056 for studies looking at non-Islamist terrorism. Additionally, more objective measures and manipulations of exposure to acts of violence lead to a lower, and again nonsignificant, average correlation ( $\hat{\rho} = 0.044$ ), compared to all other ways of measuring the independent variable (see SI Table C.1, p. 10, for all estimates and pairwise comparisons).

Similarly, several characteristics of the dependent variable influence the results. For example, the target outgroup under scrutiny shows a significant main effect. More specifically, although terrorism is associated with significant increases in hostility towards all outgroups studied, the average effect size is about twice the size when examining attitudes towards religious outgroups (predominantly Muslims;  $\hat{\rho} = 0.142$ ) and immigrants/refugees ( $\hat{\rho} = 0.128$ ) than towards other outgroups ( $\hat{\rho} = 0.073$ ). Besides, the data also support a so-called “guilt-by-association” effect. That is, the average correlation equals 0.153 when there is a strong association between the perpetrator of the attack (e.g., an

<sup>11</sup>For example, the detrimental association between terrorism and intergroup relations is less pronounced than the overall beneficial effects of intergroup contact on outgroup hostility ( $r = -0.205$ ; Pettigrew and Tropp 2006) or the success of more general interventions to reduce prejudice ( $r = -0.176$ ; Paluck et al. 2021). At the same time, the correlation between terrorism and different political attitudes is slightly stronger than/similar to the one between warfare and prosocial cooperation ( $r = 0.08$ ; Bauer et al. 2016).

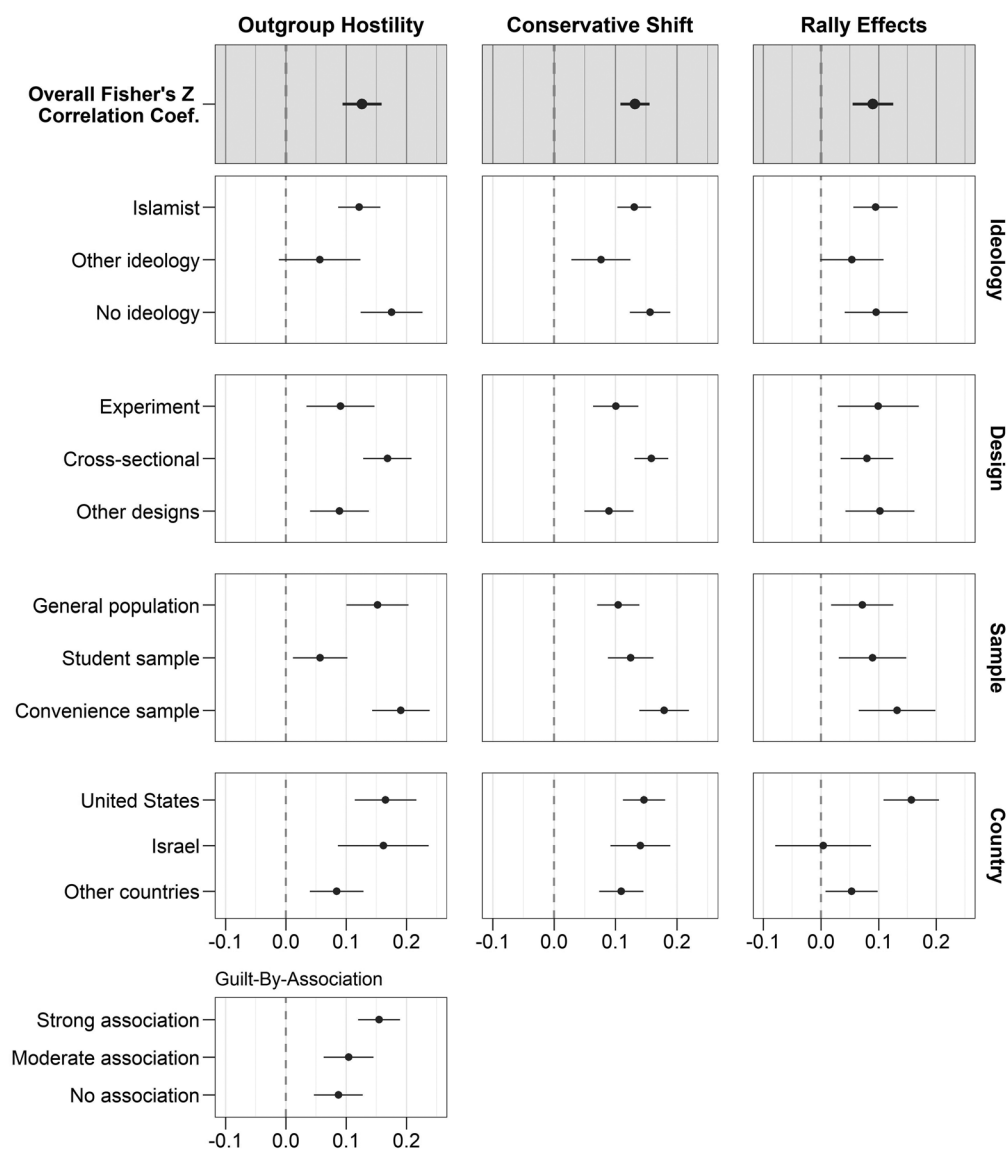
<sup>12</sup>These comparisons are inspired by Paluck and colleagues (2021), who used the standard deviation (*SD*) reported by the 2016 American National Election Survey (ANES) to make similar comparisons. The *SDs* for the 2016 ANES feeling thermometers toward illegal immigrants and the Republican party are 27.285 and 27.330, respectively.

<sup>13</sup>The next section and Shiny App provide more detailed effect sizes for researchers to use depending on specificities of their envisioned research designs.

<sup>14</sup>Likelihood ratio tests confirm that, for all three hypotheses, true differences are found between the effect sizes *within* and *between* manuscripts (SI Table B.2, p. 7). Also, the estimated Level-2 and Level-3 heterogeneity variances (i.e.,  $\tau^2_{(2)}$  and  $\tau^2_{(3)}$ ) are significant and the degree of heterogeneity (i.e.,  $I^2_{(2)}$  and  $I^2_{(3)}$ ) substantial for all three hypotheses (SI Table B.3, p. 8). See SI Appendix §B.2.3 (pp. 7–8) for more information.

<sup>15</sup>Any discrepancy between the coefficients reported in the manuscript and the ones in the SI is due to the fact that Fisher’s *Z*-transformed correlations ( $Z_r$ ) are listed in the SI but back-transformed to a Pearson’s correlation ( $\hat{\rho}$ ) in the manuscript for the ease of interpretation.

**FIGURE 3 Relationship between Terrorism and Political Attitudes, Conditional on Substantive and Methodological Factors**



Note. Dots represent the overall Fisher's Z correlation coefficient for each category, lines the corresponding 95% confidence intervals. Full meta-regression results are reported in SI Table C.1 (p. 10) for Outgroup Hostility, SI Table C.2 (p. 11) for Conservative Shift, SI Table C.3 (p. 12) for Rally Effects. To further explore heterogeneity in responses to terrorism, visit <https://ameliegodefroidt.shinyapps.io/terrorism-attitudes-metaanalysis/>.

Islamist organization) and the outgroup under scrutiny (e.g., Muslims), 0.104 when there is a moderate association (e.g., Islamist terrorism and immigrants or refugees as outgroup), and 0.087 when there is no such association.

Finally, several methodological factors equally affect the observed effect size. Notably, the findings indicate that correlational studies result in higher effect sizes ( $\hat{\rho} = 0.167$ ), compared to both experiments ( $\hat{\rho} = 0.090$ ) and

quasiexperimental and longitudinal studies ( $\hat{\rho} = 0.089$ ). In a similar vein, the mean effect size is significantly lower when there is at least 1 day between the measurement of the independent and dependent variables ( $\hat{\rho}_{\text{delay}} = 0.062$  versus  $\hat{\rho}_{\text{direct}} = 0.143$ ). Second, student samples result in a lower average correlation ( $\hat{\rho} = 0.057$ ), compared to general population samples ( $\hat{\rho} = 0.151$ ) and, particularly, other convenience samples ( $\hat{\rho} = 0.189$ ). Consequently, the mean age in the samples used is also a

significant predictor of the strength of the relationship between terrorism and outgroup hostility,  $\hat{b} = 0.004$ , 95% LBCI [0.003, 0.006]. Lastly, while studies conducted in the United States and Israel display similar average effect sizes ( $\hat{\rho} = 0.164$  and  $\hat{\rho} = 0.160$ , respectively), studies conducted in other contexts result in a significantly lower average correlation ( $\hat{\rho} = 0.084$ ).

**Conservative Shift.** As with the results for the outgroup-hostility hypothesis, the overall effect size for those few studies examining non-Islamist terrorism is significantly lower ( $\hat{\rho} = 0.076$ ), compared to studies assessing the impact of Islamist ( $\hat{\rho} = 0.130$ ) or unspecified terrorism ( $\hat{\rho} = 0.155$ ). Second, the exact measurement or manipulation used to gauge terrorism exposure also moderates the effect sizes. Again, studies using more objective measures of terrorism exposure (i.e., experiments and quasiexperiments related to a specific acts of violence) result in the lowest overall effect size ( $\hat{\rho} = 0.072$ ), whereas studies using cognitive threat perceptions or affective appraisals result in significantly higher effect sizes ( $\hat{\rho} = 0.158$  and  $\hat{\rho} = 0.178$ , respectively).

Furthermore, there is considerable variation in average correlations depending on the exact operationalization of the outcome variable. The highest effect sizes are found in studies examining changes in right-wing authoritarianism ( $\hat{\rho} = 0.170$ ) or support of stricter security policies at the expense of civil liberties ( $\hat{\rho} = 0.164$ ), whereas a lower effect size is found in studies looking at social dominance orientation ( $\hat{\rho} = 0.105$ ). A residual category (including, e.g., attitudes towards increased aid for terrorism victims or liberal environmental policies) fails to reach statistical significance ( $\hat{\rho} = 0.004$ ). All pairwise comparisons can be found in SI Table C.2 (p. 11).

Finally, the results for the conservative shift hypotheses are slightly more consistent across research design features. For example, contrary to the outgroup hostility literature, neither the country of a study nor a delay between the measurement of the dependent and independent variable significantly affects the overall effect size.<sup>16</sup> However, in keeping with the results of the outgroup-hostility hypothesis, correlational studies ( $\hat{\rho} = 0.157$ ) and studies using convenience samples ( $\hat{\rho} = 0.178$ ) result in considerably higher overall effect sizes compared to studies using other research designs or sampling mechanisms (Figure 3 below and SI Table C.2, p. 11).

<sup>16</sup>Studies with at least one day between the assessment of the independent and dependent variable generally result in a lower effect size ( $\hat{\rho}_{\text{delay}} = 0.084$  versus  $\hat{\rho}_{\text{direct}} = 0.137$ ). However, this moderation effect does not reach the a priori set significance level of 0.05 ( $p = .058$ ).

**Rally-'Round-the-Flag.** While rally effects are even more consistent across different methodological and substantive features (SI Table C.3, p. 12), the significant moderators suggest that rally effects are primarily driven by a post-9/11 rally-'round-the-U.S.-flag effect or by an even more idiosyncratic rally around the President Bush effect. First, only studies conducted in the United States result in a statistically significant and substantial overall effect size ( $\hat{\rho} = .155$ ). In contrast, studies conducted in Israel result in an insignificant effect of 0.004, and studies conducted in yet other settings result in a statistically significant but substantially smaller effect of 0.053. It is important to note in this respect that about 30% of the effect sizes stem from U.S.-based studies, 15% from Israeli-based studies, and the other 55% of the effect sizes are scattered across 22 countries.

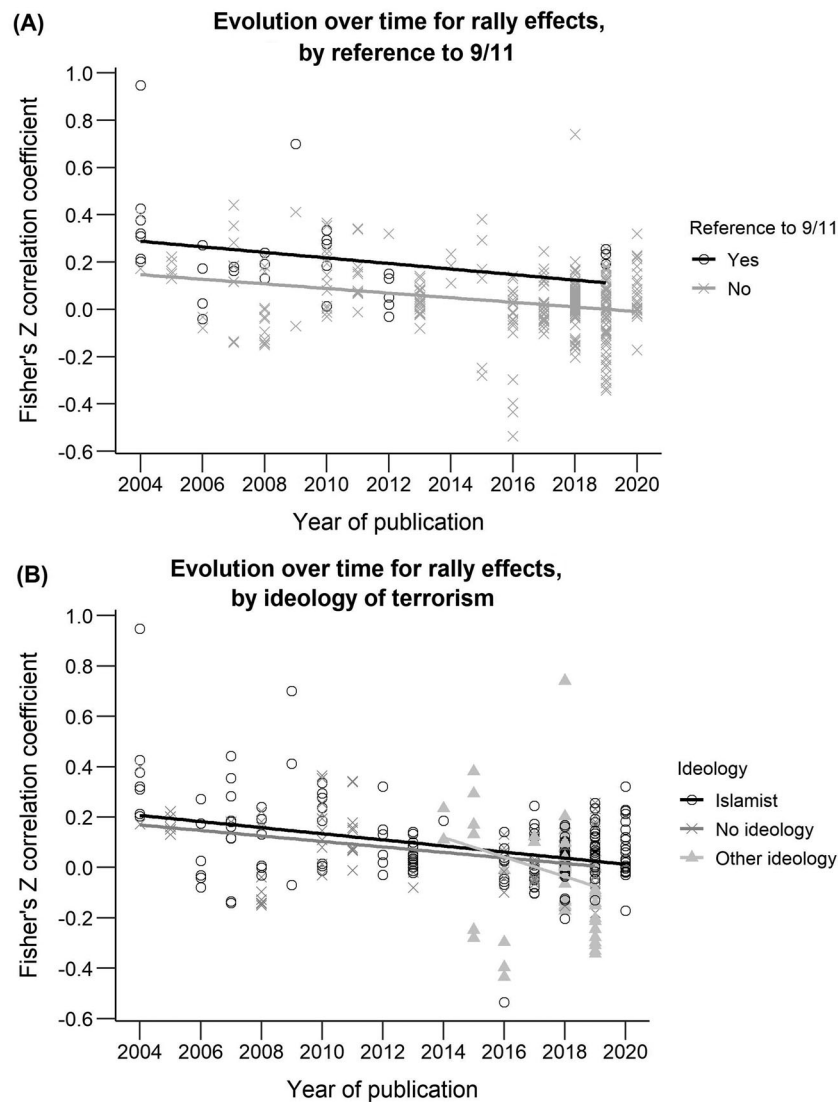
Next, as Figure 4(A) shows, the effect sizes decrease over time,  $\hat{b} = -0.009$ , 95% LBCI [-0.013, -0.004], with a particularly strong overall effect size found in those studies explicitly referring to 9/11 (i.e.,  $\hat{\rho}_{9/11} = 0.224$  versus  $\hat{\rho}_{\text{no } 9/11} = 0.061$ ). Even more, studying the impact of 9/11 accounts for a remarkable 32% of the variation in effect sizes between manuscripts. Figure 4(B), in turn, shows that studies on non-Islamist terrorism result in a lower and statically insignificant average correlation ( $\hat{\rho} = 0.054$ ), yet the comparison with other studies fails to reach significance due to a lack of studies addressing non-Islamist violence. Finally, studies examining rallies around Republican politicians, incumbents, and, particularly, around President Bush, all result in significantly higher effect sizes (SI Table C.3, p. 12).

## Discussion and Conclusion

Terrorist attacks often cause widespread concerns about their implications for core democratic values and ideals. For example, in the wake of 9/11, the United Nations expressed great concern about increases in prejudice, racism, and xenophobia (UNHCR 2001). In France, several Muslim places of worship were blown up in revenge attacks following the Charlie Hebdo shootings (Crone and Stanton 2015). And the recent series of terrorist attacks across Europe coincided with a steady rise of far-right parties (Fieschi 2020). But does terrorism really affect citizens' political attitudes? If so, how and to what extent?

About 20 years after 9/11, this article has taken stock of (most) previous work on how the public reacts to terrorism. Using advanced meta-analytical techniques, I estimated the magnitude of previous findings on the

**FIGURE 4** Correlation Coefficients over Time for Rally-‘Round-the-Flag Studies



*Note.* Graphs show the decreasing Fisher's Z correlation coefficients for the relationship between terrorism and rally-round-the-flag responses, for studies with and without a reference to 9/11 (Panel A) and by ideology (Panel B).

relationship between terrorism on public opinion and, importantly, explained some of the differences among these findings. On the one hand, the review confirms that exposure to terrorism—be it in terms of self-reported exposure, manipulated news exposure, exposure to a naturally occurring attack, anxious or angry appraisals of terrorism, or terrorist risk perceptions—is related to higher levels of outgroup hostility, political conservatism, and, albeit to a weaker extent, rally-round-the-flag responses. On the other hand, important features of this field of study warrant a certain caution

in drawing general conclusions. At its core, this meta-analysis reveals that the study of public responses to terrorism is in practice limited to the much narrower question: does *Islamist* terrorism affect attitudes within *Western* societies?

First, a vast majority of studies in this body of scholarship examine threats or a handful of acts of Islamist terrorism or refrain from identifying a specific ideology. This is unfortunate because comparing cases of terrorism with different ideological motives would allow scholars to separate those responses that are unique

to Islamist terrorism (in Western contexts, see below) from more general political-psychological coping mechanisms. In this respect, the empirical evidence indicates that Islamist terrorism is related to outgroup hostility and ingroup solidarity, whereas attitudinal responses to violence perpetrated by non-Islamist actors look quite different. Here, the overall effect sizes are much weaker (for conservative-shift outcomes) or nonsignificant (for outgroup hostility and rallying outcomes). This suggests that it might not be the threat of violence per se that is driving public reactions to terrorism, as is often assumed, but rather the threat of violence perpetrated by specific—often outgroup and low-status—actors. Violent acts perpetrated by those outgroup actors are more likely to be labeled “terrorism” and politicized in public discourse (Hopkins 2010; Meier 2020; Powell 2011), thereby stimulating distinct sociopolitical responses.

Yet, this is a hypothesis calling for further scrutiny, as currently only a handful of studies look beyond Islamist (or unspecified) terrorism, and it is even less common to compare attitudinal responses across different types of terrorism within a single study (for exceptions, see Jacobs and van Spanje 2021; Piazza 2015). This raises important questions. For example, which outgroups are more likely to be vilified in which societies, at what point in time, and why? Given the recent upsurge in far-right terrorism (Institute for Economics and Peace 2020), it seems particularly pertinent to understand how citizens react to this type of violence and whether these responses meaningfully differ from responses to other types of violence. Certainly, as others have noted before (e.g., Meier 2020), unpacking the causes and consequences of far-right violence has received extensive attention in the literature, yet these acts of political violence are rarely described as “terrorism” within scholarly work. This review shows how this semantic choice inhibits the exchange of knowledge between work on far-right violence and work on other types of political violence more regularly called “terrorism” (such as Islamist violence), thereby reducing opportunities to detect whether theories and findings travel across ideological lines.

Second, much of the literature is still focused on the United States, and particularly the political aftermath of 9/11, or the Israeli-Palestinian conflict. While this has provided valuable insights, generalizations beyond these specific contexts are, at best, complicated. Rally-‘round-the-flag responses, for example, seem to be primarily driven by a rally around President Bush effect in the wake of 9/11, and terrorism-induced reactions of outgroup hostility or political conservatism are also substantially weaker outside an American or Israeli context. As

a result, the generalizability of common theoretical predictions remains a pressing question for future research. To date, many of our extant expectations ignore particularities of the countries studied and their historical, institutional, and cultural differences with other countries. In particular, studies conducted in regions that have a history of being vulnerable to terrorist violence (e.g., Afghanistan, Iraq, Syria, Nigeria, and Pakistan) are missing in the current literature. Consequently, we still know surprisingly little about how people in countries most affected by terrorism cope with such severe and sustained threats. It is, of course, possible that the preponderance of studies from the United States and Israel is not entirely due to the field being dominated by scientists studying these countries. It is also possible that studies conducted in non-Western countries are more often published in different languages and outlets (and, hence, were not retrieved) or use different terms to denote the violence (and hence, again, were not retrieved).

Taken together, the results of this meta-analysis indicate that prejudiced, conservative, and inward-looking responses to terrorism are particularly noticeable in the United States and when attacks are carried out by outgroup members. Responses are also fairly limited—both in size and, possibly, duration (*possibly*, as studies with a delay between the measurement of the independent and dependent variable lead to smaller effects, but longitudinal studies are still scarce; see also Sniderman et al. 2019). So, why still studying public responses to terrorism? Although attitudinal responses to (Islamist) terrorism may be limited and context dependent, even small-scale and short-term changes in public opinion may have long-lasting consequences by, among other things, feeding reactionary policies (Tomz, Weeks, and Yarhi-Milo 2020; Wayne 2019) and/or the actions and narratives of extremist organizations (Bail, Merhout, and Ding 2018). Since political leaders are inherently motivated to stay or get in office, public responses to terrorism—or politicians’ perceptions of such responses—can encourage them to advocate for more militant or exclusionist policies (Wayne 2019) or even prompt them into action (Tomz, Weeks, and Yarhi-Milo 2020). It could also embolden extremist organizations to capitalize on these responses in their recruitment campaigns (Bail, Merhout, and Ding 2018). Such interaction between public opinion, politics, and extremist propaganda could provide a basis for future violence (Wayne 2019), making it all the more important to gain and disseminate a more nuanced understanding of how citizens do (and do not) respond to terrorism.



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## Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

**Appendix A:** Preregistration Motivations and Deviations

**Appendix B:** Additional Information on Data Collection and Analysis

**Appendix C:** Numerical Results and Additional Analyses

**Appendix D:** PRISMA Checklist