

The (Non)Impact of the 2015 Paris Terrorist Attacks on Political Attitudes

Bruno Castanho Silva¹ 

Personality and Social
Psychology Bulletin
2018, Vol. 44(6) 838–850
© 2018 by the Society for Personality
and Social Psychology, Inc
Reprints and permissions:
sagepub.com/journalsPermissions.nav
DOI: 10.1177/0146167217752118
journals.sagepub.com/home/pspb



Abstract

Previous research has consistently found widespread attitudinal impacts of terrorist attacks. Using data from the European Social Survey, which was conducting interviews in 11 countries when the *Charlie Hebdo* attacks happened in January 2015, I compare respondents from before and after the shootings to test whether the event shifted public opinion on several topics. There is no evidence of average impacts across a range of issues, from xenophobia to ideological self-placement and immigration policy preferences. Data collected when the Paris November 2015 shootings happened also present no evidence of public opinion change on immigration and refugee policy matters in France, but there appears to be an effect in other countries—which varies according to contextual vulnerability. Results suggest that views on immigration and immigrants have, to a certain extent, stabilized in Europe and are less susceptible to shifts from dramatic events.

Keywords

terror management, political psychology, structural equation modeling, attitudes

Received June 16, 2017; revision accepted December 1, 2017

“The Islamic State wants you to hate refugees.” So reads a headline in the *Washington Post* from November 16, 2015 (Taylor, 2015), 2 days after coordinated terrorist attacks left hundreds dead and wounded across several sites of Paris. The headline is part of a familiar warning, according to which terrorist attacks by radical Islamic groups have the consequence (perhaps intended by perpetrators) of fueling hatred by association against out-groups, most notably Muslims, among the general Western public.

Concerns are well justified based on the existing literature on public opinion consequences of terrorism. Most studies have found that attacks such as 9/11, Madrid 2004, and London 2005 have been followed by ideological shifts on a number of areas, which include increased xenophobia and anti-Muslim attitudes in Europe and the United States (e.g., Echebarria-Echabe & Fernández-Guede, 2006; Edling, Rydgren, & Sandell, 2016; Kam & Kinder, 2007). Between London 2005 and Paris 2015, however, there were no strikes by Islamic terrorist groups in Western Europe with more than 10 casualties, and impacts of distant or smaller scale terrorist acts on European public opinion are found to be less consistent (Finseraas, Jakobsson, & Kotsadam, 2011; Finseraas & Listhaug, 2013; Schüller, 2015).

This article asks whether such shifts can be observed in the aftermath of two major terrorist attacks to hit France in 2015: the *Charlie Hebdo* shootings in January and the coordinated attacks on the evening of November 13 across several sites of Paris. Both happened when cross-national surveys (the European Social Survey [ESS], 2015, and the

Eurobarometer, European Commission, 2016) were on the field, in France and other European countries, what allows us to treat them as quasi-experiments and compare respondents from before and after the events.

In both cases, against the majority of previous findings, we observe no impacts in the attacked country itself. For *Charlie Hebdo*, there are also no consistent impacts in other 10 European countries surveyed. The November shootings did have effects in EU countries other than France, which can be explained by contextual factors: Higher unemployment and a less-educated labor force predict more anti-immigrant reactions to terrorism, whereas larger immigrant populations are correlated with less xenophobic reactions to the attacks.

Attitudinal Consequences of Terrorist Attacks

Much research on the consequences of terrorism on public opinion has focused on 9/11 (e.g., Bonanno & Jost, 2006;

¹University of Cologne, Germany

All data and replication files for results reported in this paper are available at <https://github.com/bcastanho/PSPB2017>.

Corresponding Author:

Bruno Castanho Silva, Postdoctoral Researcher, Cologne Center for Comparative Politics, University of Cologne, Herbert-Lewin Str. 2, 50931 Cologne, Germany.
Email: bcsilva@wiso.uni-koeln.de

Gadarian, 2010; Huddy, Feldman, Capelos, & Provost, 2002). Its main findings are that feelings of threat, elicited by terrorism, trigger attitudinal reactions. When an attack happens, individuals in the region or country affected would feel both a sense of personal threat, believing they might themselves die in a future terrorist attack, and of national threat, feeling that the country is set for future hardships endangering its existence (Huddy et al., 2002).

Average Effects

Following system justification theory (Jost, Glaser, Kruglanski, & Sulloway, 2003), psychological needs to reduce insecurity and feelings of threat sit well with some aspects of conservative ideologies, namely, preference for a hierarchical stable social order and acceptance of inequality (Jost, Fitzsimons, & Kay, 2004; Jost et al., 2003). In this vein, terrorism was found to increase preferences for a trade-off of civil liberties for more security in the United States after 9/11 (Davis & Silver, 2004), and in the United Kingdom following the 2005 London bombings (Bozzoli & Müller, 2011). The relation between threat and conservatism has also been confirmed at the macro level, using country-level indicators of insecurity and threat (Onraet, Van Hiel, & Cornelis, 2013).

Besides general conservatism, anti-out-group prejudice is expected to increase under threat conditions. Inglehart and Welzel (2005) argue that, in a context of existential threat, in-group solidarity and out-group hostility are a defense mechanism that might be necessary for group survival. Different threat conditions have been shown to increase anti-immigrant prejudice (Caricati, Mancini, & Marletta, 2017) and radical right-wing voting (Green, Sarrasin, Baur, & Fasel, 2016).

Terrorism, eliciting feelings of threat, is therefore expected to generate conservative shifts and anti-out-group attitudes. Echebarria-Echabe and Fernández-Guede (2006) fielded surveys right before and after the March 2004 bombings in Madrid, and tested the differences between samples across several issues. They observe, after the attack, an increase in both anti-Arab and anti-Jew prejudice, and a shift in ideological orientation toward traditional conservative attachments. The same has been observed among American respondents after 9/11 (e.g., Hopkins, 2010; Kam & Kinder, 2007), and Berrebi and Klor (2008) find a consistent electoral advantage of the right-bloc in Israeli municipalities hit by terrorist attacks.

While attacks are expected to have large consequences in the target country, findings are mixed when it comes to the impacts of terrorist events abroad. Large country-variation among Europeans is found by Finseraas et al. (2011) and Finseraas and Listhaug (2013), on the impacts of, respectively, the assassination of filmmaker Theo Van Gogh and the 2008 Mumbai bombings. In both cases, effects on ideology are either small or restricted to a few countries. Among

European respondents reacting to the 2004 Madrid bombings and the 2002 Bali attacks, Legewie (2013) finds that previous contextual threat conditions—such as unemployment rates—predict whether the attacks were followed by surges in anti-immigrant attitudes.

Polarization and Ideological Moderation

Population mean changes toward conservatism, however, are only one way in which terrorism might affect individuals. Terror management theory suggests that fear of death makes people strengthen their initial worldviews, both left and right (Castano et al., 2011; Greenberg, Pyszczynski, & Solomon, 1986; Greenberg, Simon, Pyszczynski, Solomon, & Chatel, 1992), confirming that fear has been shown to increase out-group derogation among individuals at the extremes on both sides of the political spectrum (van Prooijen, Krouwel, Boiten, & Eendebak, 2015). Also, individuals who feel their identity or values are under threat become more identified with radical groups, as opposed to more moderate ones (Hogg, Meehan, & Farquharson, 2010). Therefore, it is possible that attacks trigger a polarizing effect in which liberals and conservatives become more entrenched in their original preferences (Schüller, 2015).

In parallel, there is another possibility for ideological moderation of effects: that only liberals or only conservatives change their position as a reaction. Anti-Muslim prejudice after the 2005 London bombings has been found to increase particularly among liberals (Van de Vyver, Houston, Abrams, & Vasiljevic, 2016). In fact, fear of mortality has been found to explain shifts toward more conservative positions among liberals even on issues that are not related to terrorism, such as opposition to same-sex marriage (Jost et al., 2004) and socialized medicine (Nail & McGregor, 2009). These findings, however, have not been consistently confirmed in other studies (e.g., Lambert et al., 2010; Malka, Soto, Inzlicht, & Leikes, 2014). For these reasons, it is essential to test whether terrorist attacks had different effects across right- and left-of-center respondents.

Je Suis Charlie/Paris. On the morning of January 7, 2015, two brothers entered the offices of *Charlie Hebdo*, a satirical newspaper, in Paris. They killed 11 people working for the paper, and a police officer outside the building. Five others were killed in related attacks the next 2 days around the city. By the evening of January 7, international news outlets were reporting that the suspected attackers seemed connected to radical Islamism.¹ On November 13, 2015, Paris was once again the target of terrorist attacks by a radical Islamist organization, this time the Islamic State (ISIS). On this occasion, victims were in soft targets—places with a large agglomeration of people and not particularly well-protected—and there was no sense that the locations were chosen as a form of specific retaliation.

Table 1. Descriptive Statistics for Baseline Characteristics of the ESS Samples.

	France			Pooled data			Range
	After	Before	T-stat.	After	Before	T-stat.	
Left-right	5.19 <i>2.14</i>	5.07 <i>2.38</i>	-1.44* —	4.86 <i>2.01</i>	4.99 <i>2.10</i>	0.03 —	0-10 —
Born	0.16	0.11	-1.81	0.1	0.08	0.34	0-1
Ethnic minority	0.94	0.96	0.91	0.96	0.96	0.58	0-1
Area	1.98	1.9	-1.33	1.78	1.7	0.00	1-3
Contact	5.06 <i>1.89</i>	4.64 <i>1.96</i>	-1.54 —	4.54 <i>2.12</i>	4.53 <i>2.08</i>	0.14 —	1-7 —
Female	0.5	0.55	0.21	0.51	0.51	0.50	0-1
Age	45.08 <i>20.36</i>	44.88 <i>18.36</i>	0.81 —	44.52 <i>17.6</i>	43.66 <i>18.62</i>	0.08 —	15-114 —
Education (median)	4	4	-1.03	4	4	0.14	1-7
Number of interviews	267	430	—	2,852	2,033	—	—

Note. After and before refer to means and medians 30 days after or before the *Charlie Hebdo* attacks. T-stat. is the *t*-statistic from a Welch two-sample *t* test. Left-right: 0 (left) to 10 (right) ideological self-placement; Born = born in the country; Ethnic minority = belongs to an ethnic minority; Area = how many people of other ethnicities live in the same area; Contact = how often has contact with people from different races; Education = highest degree obtained. Standard deviations in italics. Responses from the European Social Survey (2015). ESS = European Social Survey.

According to the theories outlined above, these attacks would increase a sense of threat among the French public. This was in fact observed on a nationally representative survey fielded 4 weeks after *Charlie Hebdo* (Ben-Ezra, Leshem, & Goodwin, 2015). We may expect such reaction in other European countries as well, albeit perhaps to a lesser extent. Considering that the attackers were in both cases associated with radical Islamic groups, I hypothesize a backlash reaction against out-groups and particularly Muslims. Moreover, increasing out-group hostility is expected to strengthen anti-immigrant feelings. We may also expect a conservative shift among the public even on areas unrelated to security or culture.

Study 1: *Charlie Hebdo*

The attacks are considered quasi-experiments as they are an exogenous shock unrelated to the schedule of survey interviews. We should not expect that respondents from right after January 7 or November 13 would have significantly different opinions in relation to those from right before, had the attacks not taken place. Therefore, a comparison of the two groups of respondents can give an unbiased estimate of the average effect of these terrorist attacks on public opinion, under the assumptions discussed in the following sections.

Data

When the *Charlie Hebdo* terrorist attacks happened, the ESS (2015) was conducting interviews in 11 countries for its Round 7, including France.² I use interviews from up to a month before or after the attacks, a time frame in which other studies have still identified strong effects (e.g., Echebarria-Echabe & Fernández-Guede, 2006; Van de Vyver et al., 2016). Respondents from the attack day itself are removed.³

The randomness assumption. Two common issues with surveys might cast doubt on whether pre- and postterrorist interviewees can indeed be treated as if randomly distributed: reachability bias and multistage survey implementation. The first is the simple fact that it is harder to reach some respondents than others. This difficulty is expected to correlate with factors such as the area one lives and employment situation, both of which may lead to systematic differences between individuals who take the survey earlier or later. The second potential source of bias rests on survey design: The strategy in several countries covered in the ESS is to use a multistage design in which the fieldwork is conducted in some regions before others.

While it is not possible to completely rule out such issues, we may turn to descriptive statistics of our samples to check if there are signs of systematic differences between respondents from before and after the attacks. Table 1 has descriptives for the ESS samples, and *t*-statistics from two-sample *t* tests. In none of eight baseline characteristics tested, we observe a statistically significant difference between respondents from before and after the attacks. On ideological self-placement, age, education, gender, or contact with individuals from other ethnicities (all of which may affect one's potentially xenophobic reaction to terrorist attacks) there are no significant differences between the groups. Respondents from before and after the attacks do not seem to significantly differ on a number of baseline characteristics, and therefore, it is possible to treat these events as a natural experiment. Nevertheless, as actual randomization was not applied, all baseline characteristics are controlled for in the estimated models.

Indicators. The breadth of questions in the ESS allows us to estimate effects across a range of issues that have been

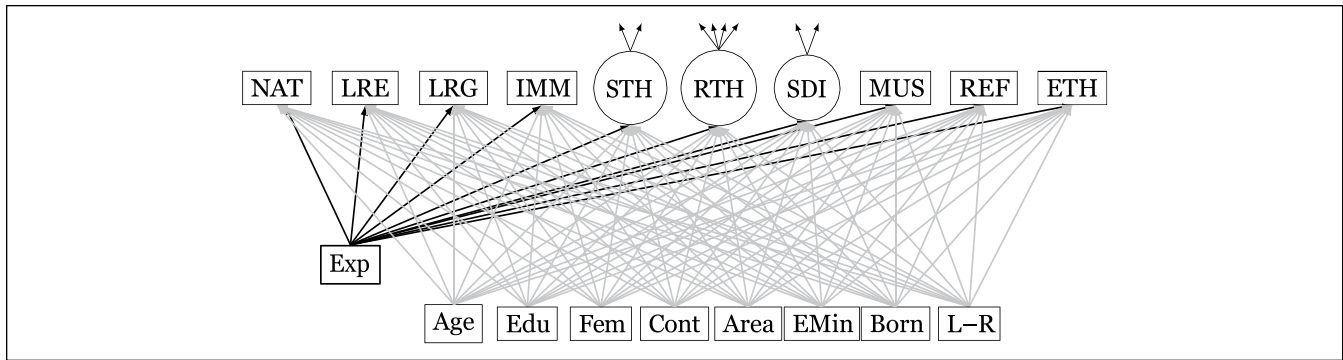


Figure 1. Basic model of exposure impact on attitudes.

Note. NAT = nationalism; LRE = L/R economy; LRG = L/R on gay rights; IMM = immigrants make country worse; STH = symbolic threat; RTH = realistic threat; SDI = social distance; MUS = anti-Muslim; REF = refugees; ETH = immigrants from different ethnic group; Exp = treatment (interview after the attacks); Age = square root of age; Edu = education; Fem = female; Cont = contact; Area = area; EMin = ethnic minority; Born = born in the country; L/R = left-right.

discussed or found in the related literature. Therefore, I group the selection of dependent variables according to the hypothesis to which they refer.

Xenophobia and Anti-Muslim attitudes. Anti-Muslim attitudes are measured with a question on whether the government should allow (a) many, (b) some, (c) a few, (d) or no Muslims to come and live in the country. For xenophobia, first I use the question if (a) many, (b) some, (c) a few, or (d) no immigrants of a different ethnic group from the majority should be allowed to live in the country. Second, I use the two items from the *Social Distance Scale* (Bogardus, 1933) included in the ESS: whether the respondent *would not mind* (0) or *mind a lot* (10) if someone from a different race came to live in the country and (a) married a close relative and (b) became the respondent's boss.

Realistic and symbolic threat. The literature identifies two kinds of out-group threats associated with immigrants: realistic and symbolic (Stephan, Ybarra, & Bachman, 1999). For realistic threat, there are four questions, all measured on a 0 to 10 scale: if people from other countries coming to live in the respondent's country are (a) bad for the economy, (b) taking away jobs, (c) taking in taxes more than contributing, and (d) making crime problems worse. For symbolic threat, there are two questions, also on a 0 to 10 scale: if people from other countries moving in undermine (a) the country's cultural life and (b) the country's religious beliefs and practices.

Conservative turn. To test for a general conservative shift, three dependent variables are used: first, a nationalism question, on how close one feels to the country (recoded, 1 = *not close at all*, 4 = *very close*)—this is expected to capture the “rally round the flag” effect (Mueller, 1970). The other two measure conservatism on economic and social issues, some of which part of the literature has found to be affected by terrorist attacks even though seemingly unrelated to them (Jost et al., 2004; Nail & McGregor, 2009): Whether the govern-

ment should take measures to reduce differences in income levels, and whether gay men and lesbians should be free to live their own life as they wish—both on a 1 (*agree strongly*) to 5 (*disagree strongly*) scale.

Immigration and refugee policy. Regarding immigration policy preferences, the questions used are (a) Was the country made a better (0) or worse (10) place by people coming to live in it, and (b) whether the government should be generous (1 = *agree strongly*, 5 = *disagree strongly*) when judging applications of refugee status.

Controls. I control for variables that could affect either the treatment condition (meaning, whether the respondent was interviewed earlier or later during the fieldwork) and also that might specifically affect the response to terrorist attacks. Control variables include whether the respondent is a citizen of the country, belongs to an ethnic minority group (binary—coded with 1 as not belonging to an ethnic minority), how often she has contact with people from a different race—what has been found, in intergroup contact theory, to reduce anti-out-group sentiments (Pettigrew, 1998)—whether there are few or many people from a different ethnic group in the area where she lives, besides age,⁴ left-right self-placement (0-10), gender, and education (highest degree obtained).

Analytical Approach

The main approach to estimate effects of the January attacks is a comparison of respondents a month before and after the attacks, with the structural equation modeling (SEM) described in Figure 1. There are 10 dependent variables: seven are observed, and three modeled as multiple-indicators latent variables: symbolic threat (two items), realistic threat (four items), and social distance (two items), described in the previous section. The regression of all dependent variables on all controls and the binary treatment indicator is estimated simultaneously. The model is fit to two samples: one with

Table 2. Estimates for the January Attacks.

	RTH	STH	SDI	LRG	LRE	NAT	IMM	REF	MUS	ETH
Average treatment effects—France										
Exposure	0.06 (0.16)	0.02 (0.20)	0.20 (0.20)	−0.09 (0.08)	−0.05 (0.10)	0.14 (0.11)	−0.02 (0.17)	0.04 (0.10)	−0.16 (0.10)	−0.11 (0.10)
Model fit	N = 601 (231 treated). χ^2 : 224.015, df = 109, p < .001; RMSEA = 0.042 (90% CI = [0.034, 0.050]); CFI = 0.962, TLI = 0.917									
Polarization effects—France										
Exposure	0.01 (0.10)	−0.05 (0.11)	0.02 (0.09)	0.04 (0.06)	−0.02 (0.06)	0.09 (0.11)	0.03 (0.13)	−0.07 (0.06)	−0.16 (0.13)	−0.06 (0.13)
Model fit	N = 601 (231 treated). χ^2 : 174.829, df = 107, p < .001; RMSEA = 0.032 (90% CI = [0.023, 0.041]); CFI = 0.951, TLI = 0.890									
Average treatment effects—Pooled sample										
Exposure	0.04 (0.10)	0.21 (0.12)	0.27 (0.26)	0.08 (0.08)	−0.01 (0.07)	0.09 (0.10)	0.10 (0.12)	0.10 (0.06)	0.16 (0.10)	0.16 (0.08)
Model fit	N = 4,139 (2,399 treated). χ^2 : 166.468, df = 109, p < .001; RMSEA = 0.011 (90% CI = [0.008, 0.015]); CFI = 0.966, TLI = 0.924									
Polarization effects—Pooled data										
Exposure	0.07 (0.04)	0.06 (0.03)	−0.01 (0.05)	−0.14 (0.10)	0.00 (0.05)	0.09 (0.10)	−0.05 (0.05)	−0.08 (0.06)	0.21* (0.07)	0.05 (0.06)
Model fit	N = 4139 (2,399 treated). χ^2 = 141.470, df = 105, p = .01; RMSEA = 0.009 (90% CI = [0.005, 0.013]); CFI = 0.926, TLI = 0.830									

Note. Full structural equation models estimated with a weighed least squares mean and variance adjusted (WLSMV) estimator. Robust standard errors in parentheses. Unstandardized estimates Exposure: whether the interview was before (0) or after (1) the attack. Dependent variables: RTH = realistic threat; STH = symbolic threat; SDI = social distance; LRG = left to right on gay rights; LRE = left-right on economy; NAT = nationalism; IMM = Immigrants make the country better; REF = refugees; MUS = anti-Muslim; ETH = immigrants from different ethnic group; CI = confidence interval; CFI = comparative fit index; TLI = Tucker–Lewis index; RMSEA = root mean square error of approximation.

* $p \leq .05$.

only French respondents and one with all other countries pooled together, excluding France. For the model estimated on the pooled data, clustered robust standard errors are calculated and reported. Observations with missing data are incorporated into the model by using full information maximum likelihood.⁵ To test polarization effects, I construct an individual measure of position extremity; following Fernbach, Rogers, Fox, and Sloman (2013), the midpoint of each scale is subtracted from the individual response, and the absolute value taken. Therefore, if the attacks have an effect of polarizing responses, that would appear in this test.

A multiple-group SEM is used to test for the potential moderation effect of ideology. First a dummy is created on whether the respondents' ideological self-placement is left or right of center.⁶ Next, the model from Figure 1 is fit to the data with two groups, but with indicators' intercepts, factor loadings, and all regression coefficients constrained to be the same across them (left and right respondents). A second model is then fit in which the coefficient of each dependent variable regressed on the treatment indicator is allowed to vary across the two groups. If the attacks had different impacts on people depending on their ideology, we should observe significantly different estimates between left and right respondents. This is tested with a χ^2 test of model difference (Satorra & Bentler, 2000, 2010). As the two models are nested, and each one has a likelihood and a χ^2

statistic indicating fit, we can compare the difference in these statistics between the two. If one model fits significantly worse than the other, it means there is a significant difference between coefficients in each group.

Results

Table 2 contains the estimated average effects for the January attacks, along with estimated polarization effects, for both the French and pooled ESS samples.⁷ RMSEA indicates good fit for all models, below .05. The polarization models have slightly worse fit than those for average effects: Tucker–Lewis index (TLI) is below .900 in both cases, and comparative fit index (CFI) is below .950 for the pooled data.⁸ Regarding results, we observe no average effects in France or the other European countries. Post-*Charlie Hebdo*, respondents were not more anti-out-group, feeling threatened, or xenophobic than those who took the survey before. For the pooled data, we may be more certain it is not a matter of low power: the sample size is above 4,000, enough to capture small treatment effects even if clustered standard errors reduce the effective statistical power.

For France, there are also no polarization effects. The attacks did not make respondents choose more extreme categories in any of the positions. This means that the lack of an average effect is not due to, for example, liberals becoming

Table 3. Estimates for Ideological Moderation of the January Attacks.

	RTH	STH	SDI	LRG	LRE	NAT	IMM	REF	MUS	ETH
Multiple-group model—France										
Exposure (left)	−0.04 (0.32)	−0.06 (0.43)	0.18 (0.33)	−0.14 (0.15)	−0.27 (0.21)	0.38 (0.23)	−0.26 (0.35)	−0.20 (0.19)	−0.18 (0.20)	−0.05 (0.22)
Exposure (right)	0.10 (0.21)	−0.04 (0.25)	0.19 (0.27)	−0.08 (0.11)	0.08 (0.14)	0.02 (0.13)	0.14 (0.22)	0.09 (0.12)	−0.26 (0.14)	−0.21 (0.13)
Model fit (free)	N: Left = 165, Right = 376. $\chi^2 = 404.950$, $df = 300$, $p < .001$; RMSEA = 0.036 (90% CI = [0.026, 0.045]); CFI = 0.957, TLI = 0.931									
Test of Model Difference $\Delta \chi^2$	$= 7.462$, $df = 10$, $p = .6812$									
Pooled data										
Exposure (left)	0.19 (0.18)	0.27 (0.23)	0.26 (0.28)	0.12 (0.07)	−0.01 (0.06)	−0.05 (0.10)	0.20 (0.12)	0.19* (0.07)	0.19 (0.13)	0.17 (0.10)
Exposure (right)	−0.10 (0.06)	0.16 (0.13)	0.27 (0.26)	0.07 (0.09)	0.02 (0.08)	0.18 (0.13)	0.02 (0.14)	0.08 (0.07)	0.18* (0.09)	0.16* (0.08)
Model fit (free):	N: Left = 1005, Right = 2686. $\chi^2 : 410.317$, $df = 300$, $p < .001$; RMSEA = 0.014 (90% CI = [0.011, 0.017]); CFI = 0.964, TLI = 0.942									
Test of Model Difference $\Delta \chi^2$	$= 10.042$, $df = 10$, $p = .4368$									

Note. Full structural equation models estimated with a weighed least squares mean and variance adjusted (WLSMV) estimator. Robust standard errors in parentheses. Unstandardized estimates Exposure: whether the interview was before (0) or after (1) the attack. Dependent variables: NAT = nationalism; LRE = left-right on economy; LRG = left to right on gay rights; IMM = immigrants make the country better; STH = symbolic threat; RTH = realistic threat; SDI = social distance; MUS = anti-Muslim; REF = refugees; ETH = immigrants from different ethnic group. CI = confidence interval; CFI = comparative fit index; TLI = Tucker–Lewis index; RMSEA = root mean square error of approximation.

* $p \leq .05$.

more liberal and conservatives more conservative simultaneously. In the pooled sample, there is one significant polarization effect: respondents' idea of how many Muslims should be allowed to come and live in the country. Due to the recoding for transforming the original variable into a position-extremity one, it changes from a 4-point scale to a dichotomous one. Therefore, the coefficient means a 0.20 difference on a 0 to 1 scale, substantively large. This indicates a reaction in which those who were moderately in favor of receiving more Muslim immigrants became strongly in favor of it, whereas those who were moderately against also moved toward a more extreme opposition.

Results in Table 3 test the moderation effect that ideology might have on reactions to the attacks, from models in which the coefficient of each dependent variable is allowed to vary across left- and right-wing respondents. In the French data, neither for liberals nor conservatives did the attacks have any impact. For both groups, there is no significant effect. Moreover, a χ^2 test of model difference between this model, with coefficients allowed to vary, and a more restricted model, in which coefficients are constrained to be the same across the two groups, shows that the two are not significantly different ($\chi^2 = 7.462$, $df = 10$, $p = .68$). The interpretation of this test is that the effects of the attacks are not significantly different for left- and right-wing respondents.

For the pooled data, we observe different effects across the groups on a few variables. The attacks made respondents on the left prefer more restrictive refugee policies than they did before. On the other hand, those on the right became more opposed to increasing the number of Muslims

and of immigrants from a different ethnic group to be allowed in the country. Nevertheless, the χ^2 model comparison does not indicate a significant difference between the more and the less restricted models. This means that the difference between coefficients in the two groups is not statistically significant, and therefore the significance of a few coefficients in one or the other might be a statistical artifact.

A Second Test—Regression Discontinuity

Impacts of terrorist attacks might be expected to appear and fade quickly. Therefore, looking at data from 1 month before and after could hide immediate effects. To test that, I use a regression discontinuity design (RDD). This method estimates two regression lines of an outcome of interest—here, opinion on a certain issue—on a “forcing variable.” Although the forcing variable is continuous, it has a cut point that marks a qualitative discontinuity. For example, this design has been widely used to study the effects of holding political office. Researchers regress a dependent variable of interest on candidates' vote-share—where 50% marks the qualitative difference between winning and losing an election (e.g., Broockman, 2014; Eggers & Hainmueller, 2009; Lee, 2008). Two (usually nonparametric) regression lines are fit, one on each side of the cutoff. The difference in intercepts of the two at the splitting point is considered the estimated treatment effect. Under the assumption that those units right before and after that point could have gone either way, that is, are as if randomly split

Table 4. RDD Estimates for the January Attacks.

	RTH	STH	SDI	LRG	LRE	NAT	IMM	REF	MUS	ETH
France										
LATE	-2.74	-2.15	2.69	-0.79	-1.59	-0.40	-3.34	-0.55	0.05	-1.25
SE	1.40	1.51	1.56	0.44	0.71	0.50	1.37	0.82	0.68	0.81
Adjusted <i>p</i>	.17	.22	.17	.17	.12	.54	.12	.55	.94	.21
N	69	72	70	72	92	72	69	71	43	43
LATE—Polarization	-0.53	0.58	-0.12	0.83	-0.15	-0.17	-1.39	0.24	0.38	0.07
SE	0.99	0.83	0.88	0.41	0.42	0.38	1.11	0.55	0.38	0.47
Adjusted <i>p</i>	.89	.89	.89	.43	.89	.89	.89	.89	.89	.89
N	69	92	70	72	69	69	66	68	43	43
Pooled ESS sample										
LATE	0.80	0.40	0.28	0.43	-0.68	-0.20	-0.16	0.54	-0.01	0.37
SE	0.37	0.47	0.62	0.33	0.37	0.19	0.62	0.32	0.30	0.29
Adjusted <i>p</i>	.31	.56	.82	.39	.31	.50	.89	.31	.97	.39
N	352	348	388	306	274	277	344	343	269	270
LATE—Polarization	0.58	0.05	0.18	-0.08	0.44	-0.15	0.51	0.03	0.06	0.14
SE	0.32	0.35	0.44	0.22	0.25	0.17	0.39	0.25	0.09	0.15
Adjusted <i>p</i>	.36	.89	.89	.89	.36	.72	.63	.89	.80	.72
N	352	348	317	273	274	277	344	270	164	270

Note. RDD = regression discontinuity design; LATE = Local average treatment effect of taking the survey after the attacks, using the Imbens and Kalyanaraman (2012) method for obtaining optimal bandwidths. Rounded optimal bandwidths for all models: 2 days. Adjusted *p* are *p* values adjusted for multiple testing (Benjamini & Hochberg, 1995; Benjamini & Yekutieli, 2001). Exposure: whether the interview was before (0) or after (1) the attack. Dependent variables: RTH = realistic threat; STH = symbolic threat; SDI = social distance; LRG = left to right on gay rights; LRE = left-right on economy; NAT = nationalism; IMM = immigrants make the country better; REF = refugees; MUS = anti-Muslim; ETH = immigrants from different ethnic group; ESS = European Social Survey.

**p* < .05.

into each group, this method gives an unbiased estimate of a causal effect (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

For this study, the forcing variable is the day of interviews, and the cut point set to January 7.⁹ This approach has been followed in other studies on attitudinal impacts of terrorist attacks, such as Finseraas et al. (2011), Finseraas and Listhaug (2013), Jakobsson and Blom (2014), and Legewie (2013). The assumption is that individuals interviewed in dates very close to January 7 are as if randomly divided between before and after that date. The estimation is made by fitting local linear regressions with a triangular kernel, following Lee and Lemieux (2010).¹⁰

RDD results. RDD estimates for France and the other ESS countries are in Table 4. They test both average effects and polarization.¹¹ None of the adjusted *p* values in Table 4 is below .05. Without adjusting for multiple testing, there is a significant average impact of the treatment on whether the country was made better by immigrants (IMM). However, that is in the opposite direction as theorized: The negative coefficient indicates that respondents right after the attacks are more positive about the contribution of immigrants to the country than those from before. None of the other estimates has a significant adjusted *p* value and several others are also negative. There is no significant polarization effect. In this case, no effect would be significant even if we were not to

control for multiple testing. Immediately after the attacks, therefore, they seem to have had no impacts.¹²

Discussion of Study 1

Most evidence points in the same direction: There seems to be little to no impact of *Charlie Hebdo* on public opinion (at least in the matters considered). We observe a few negative coefficients, going against expectations, and several small ones that point to the lack of substantive average differences between respondents. Moreover, considering the extent of coverage and practical political consequences of these events, effects were expected to be large—contrary to what is seen here.

Besides, there seems to be only one polarizing effect. We observe intense polarization among Europeans on whether more (or fewer) Muslims should be allowed to come to their countries. Abroad, it seems that individuals strengthened their previously held opinions on Islamic immigrants, what did not happen in France. The moderation tests show that the attacks did not affect differently conservatives and liberals in France. In the pooled data, however, there is a more interesting pattern: Liberals have shifted their position on refugees' policies, becoming more in favor of restrictive measures. However, for them, this is not accompanied by increased xenophobia. The exact contrary is observed among conservatives. Xenophobia increases, indicated by positive

Table 5. Descriptive Statistics for Eurobarometer Data.

	France		Rest of countries		Range
	After November 13	Before November 13	After November 13	Before November 13	
Immigrants	2.55 <i>1.00</i>	2.74 <i>0.95</i>	2.88 <i>0.99</i>	2.79 <i>0.97</i>	1-4
Refugees	2.31 <i>0.96</i>	2.34 <i>0.94</i>	2.43 <i>1.02</i>	2.19 <i>0.95</i>	1-4
Age	45.88 <i>19.53</i>	49.46 <i>19.77</i>	47.47 <i>18.31</i>	48.34* <i>18.79</i>	15-99
Unemployment	0.07	0.10	0.08	0.09	0/1
Female	0.52	0.52	0.51	0.52	0/1
Bills	1.66	1.60	1.52	1.49*	1-3
N	97	928	7,537	19,058	

Note. Immigrants = immigrants contribute a lot to our country—I = agree/4 = disagree; Refugees = our country should help refugees—I = agree/4 = disagree; Unemployed = categorical: unemployed, temporally not working (1), any other response (0); Bills = in the last year, had difficulties paying bills at the end of the month—I = almost never, 2 = from time to time, 3 = most of the time. Data from the Eurobarometer 84.3. Standard deviations in italics. All statistics calculated with poststratification weights.

*Indicates a statistically significant difference ($p < .05$) between pre- and postattacks groups on a two-tailed t test.

coefficients on how many Muslims and people from other ethnicities should be allowed into the country. But there is no increased preference for more restrictive refugees' policies writ large.

It is possible, though, that the mechanism linking terrorist attacks and public opinion changes was not activated in this case. Theories outlined previously state that individuals become more hostile to out-groups due to increased feelings of personal threat or death anxiety. Whereas Ben-Ezra et al. (2015) find that individuals did report an increase in threat perception, this study found no increase in either realistic or symbolic threats. The reason might be that the *Charlie Hebdo* attacks seemed to be clearly motivated: They targeted a group that several times had disrespected Islam and the prophet Muhammad. It is reasonable to expect, therefore, that many who did not draw the same cartoons would not feel personally threatened. Looking at the November shootings helps solve this issue: in that case, attacks were at seemingly random spots, and it is expected that the public, especially in France, would feel much more personally threatened on the day right after they happened.

Study 2: November Shootings

In November, the Eurobarometer 84.3 (European Commission, 2016) was on the field in the 28 EU countries between November 7 and November 17, meaning that the attacks happened right in the middle of its data collection (evening of November 13). Responses between 8:00 p.m. of November 13 and 8:00 a.m. of November 14 are removed.

Data

The Eurobarometer also uses a multistage sampling procedure. As the attacks happened on Friday evening, and surveys only continued for another 4 days (even less in some

countries), most postterrorism respondents completed the survey on the weekend, what might lead to biases due to availability. Looking at descriptives in Table 5, we observe that respondents in the treatment group are less likely to be unemployed and are slightly younger. However, on the French sample, no difference is statistically significant. On the pooled data, for two covariates there is a significant difference: age and difficulty in paying bills at the end of the month. None of which is a substantively large difference, and the statistical significance is likely a product of the large sample sizes: more than 7,000 people in the treatment group, and 19,000 in the control.

Indicators and Model—France

Questions available in the Eurobarometer mostly address policy preferences and the EU, and therefore I focus on one of the points analyzed before: perception of immigrants and preferences on refugee policy. I use two questions asked in a standard 1 (*totally agree*) to 4 (*totally disagree*) scale: whether “immigrants contribute a lot to the country,” and if “the country should help refugees.” Controls are gender, age, unemployment, and whether one had difficulties paying bills in the past year. The set of controls is different from Study 1 for two reasons: first, several demographic controls from the ESS are not available in the Eurobarometer: that is, the case for whether the respondent was born in the country or belongs to an ethnic minority, has contact with people from different races, and the amount of people from different ethnicities who live in the same area.¹³ Second, when investigating contextual effects, the estimation of a multi-level SEM (described below in more detail) requires parsimony in that there should not be more free parameters estimated than the number of clusters (Castanho Silva, Bosancianu, & Littvay, in press). Adding a larger number of

controls makes the estimated parameters less reliable in those cases.

Furthermore, a way of testing if the potential mechanism is in place is trying to assess respondents' perceptions of terrorism threat. Although there are no direct questions for this, I use as a proxy the question on "What are the two most important issues facing the country," for which "terrorism" is an option. If we observe a sensitive increase in the number of individuals who mention "terrorism" after the attacks, it suggests that they acknowledge it as being an urgent topic and are likely to have a sense of national threat in its regard. Similar to Study 1, there are three models estimated, one for each kind of effect: average changes, polarization, and ideological moderation. In each of those three, the effects of treatment and control variables on all three outcomes (immigrants, refugees, and mentions of terrorism) are estimated simultaneously. For the polarization test, only the two agree-disagree dependent variables are recoded to position extremity (Ferbach et al., 2013), by subtracting 2.5 (middle of the scale) from each response, and taking the absolute result. For the ideological moderation test, I use responses to an ideological self-placement question with 10 categories (from 1 = *left* to 10 = *right*). Responses from 1 to 5 place the individual into the group "left," while responses from 6 to 10 place them on the "right." Moderation effects are then tested with a multigroup SEM, similar to Study 1.

Results

Estimates for France are in Table 6. Once again, we observe no significant average or polarization impacts on public opinion. The difference between those who took the survey the weekend right after the shootings to those taking it the week before, regarding their views on whether France should accept more refugees, **is virtually zero**. For whether immigrants contribute a lot to the country, the estimate is also not statistically significant. The direction of both coefficients indicate increased *agreement* with the statements after the attacks, against the expected direction.¹⁴

In addition, we observe a large increase in the likelihood of respondents mentioning "terrorism" as one of the most important issues faced by the country after the attacks. This suggests that the shootings did trigger a sense of urgency around terrorism, which we can assume to be associated with a feeling of the nation being under threat. As the last set of results shows in the model testing ideological moderation, the increased salience of terrorism is driven by left-wing respondents. Although there is an increase also among right-of-center interviewees, that is not statistically significant. This test shows no significant differences between liberals' and conservatives' responses to the attacks: no coefficients are significant, and three out of four indicate more positive attitudes toward refugees and immigrants after the attacks. The χ^2 test of model difference shows that allowing coefficients to vary between left and right respondents does not lead to a significantly better

Table 6. Estimates of the November Attacks Effects in France.

	Immigrants	Refugees	Terrorism
Average			
Exposure	-0.18 (0.12)	-0.05 (0.12)	1.089* (0.14)
Model fit	$\chi^2 = 0.753$, $df = 3$, $p > .10$, RMSEA = 0.00 (90% CI = [0.00, 0.028]), CFI = 1.00, TLI = 1.00		
N	1,015 (96 treated)		
Polarization			
Exposure	-0.02 (0.15)	0.11 (0.15)	1.09* (0.14)
	$\chi^2 = 1.325$, $df = 3$, $p > .10$, RMSEA = 0.00 (90% CI = [0.00, 0.038]), CFI = 1.00, TLI = 1.00		
N	1,015 (96 treated)		
Ideological moderation			
Exposure (left)	-0.16 (0.20)	-0.32 (0.20)	1.38* (0.20)
Exposure (right)	0.07 (0.23)	0.04 (0.25)	0.53 (0.29)
Model fit (free)	$\chi^2 = 21.083$, $df = 12$, $p = .049$, RMSEA = .043 (90% CI = [.003, .072]), CFI = .993, TLI = .978		
N	Left: 553, Right: 279		
Test of Model Diff.	$\Delta \chi^2 = 7.245$, $df = 3$, $p = .065$		

Note. Estimator: weighed least squares mean and variance adjusted (WLSMV). Robust standard errors in parentheses. Dependent variables: Terrorism: Mentioned "terrorism" as one of the two most important issues facing the country; Immigration: Immigrants contribute to the country; Refugees: Country should help refugees; Exp: Exposure (interview happened after November 13). RMSEA = root mean square error of approximation. CI = confidence interval; CFI = comparative fit index; TLI = Tucker-Lewis index.

* $p \leq .05$.

model than constraining them to be the same across groups—meaning, no significant between-group differences.

Contextual Effects

Finally, I investigate if there are average effects of the November shootings on the remaining 27 EU countries, and how it varies across them. Figure 2 shows across-country variance: In some, average postattacks responses are more anti-immigrants/refugees, whereas in others they are less so. The hypothesis tested is that this variation can be explained by contextual factors that also represent threat scenarios, as suggested by Legewie (2013).

To test that, I use a multilevel structural equation model (MLSEM) with random slopes, depicted in Figure 3. The graphical notation is borrowed from Muthén and Muthén (1998-2010): *Within* denotes the individual-level part of the model, while *Between* indicates the model for country-level effects.¹⁵ This model aims at explaining under which conditions the attacks lead to more or less anti-immigrants/refugees' responses across countries. I do not include the question on the

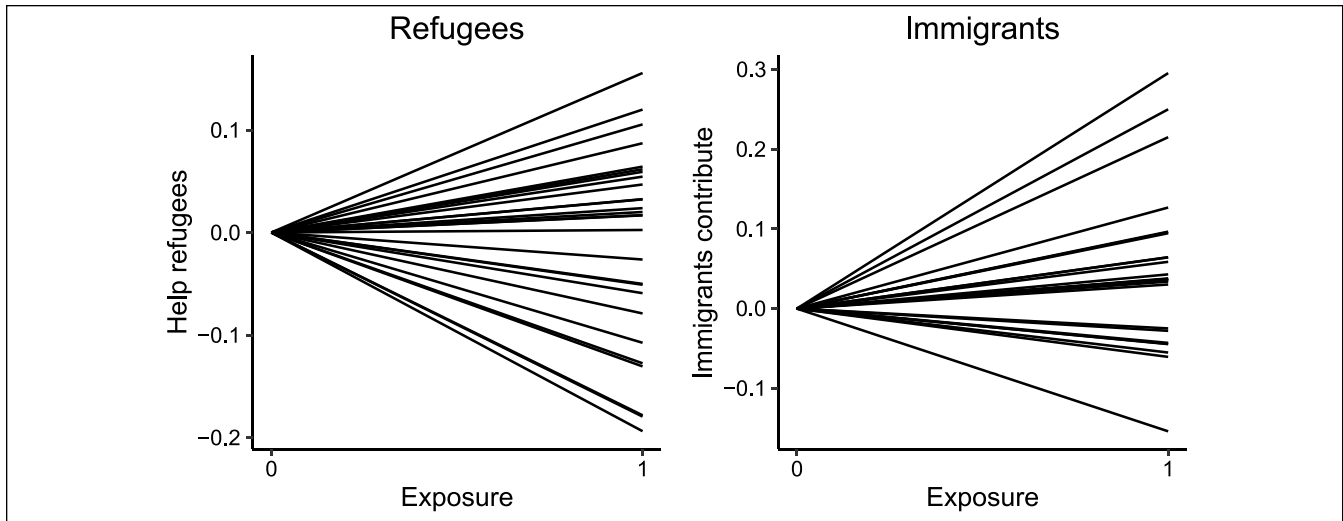


Figure 2. Country slopes of the impact of terrorism on dependent variables.

Note. Each line shows the difference on average responses to the respective dependent variable between respondents from before (exposure = 0) and after (exposure = 1) the November 13 shootings, in each of the 27 Eurobarometer countries. Variables were centered at the country averages for before the attacks.

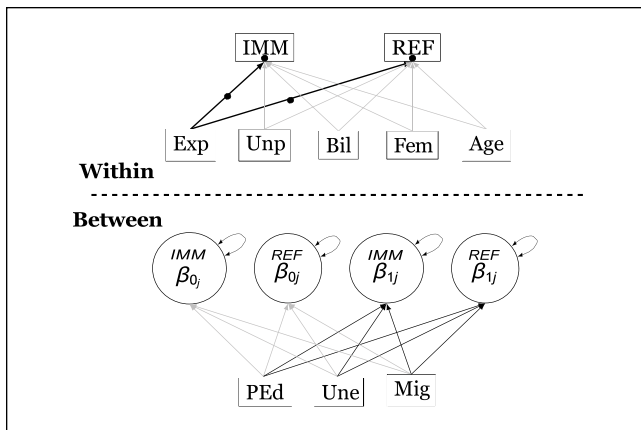


Figure 3. Multilevel structural equation model of exposure impact on attitudes in Europe—November attacks.

IMM = immigrants contribute to the country; REF = country should help refugees; Exp = Exposure (interview happened after November 13); Unp = unemployment; Bil = difficulty paying bills; Fem = female; Age = square root of age; PEEd = proportion of labor force with only primary education; Une = unemployment rate; Mig = share of immigrants in the population; IMM β_{0j} country-intercepts of IMM; REF β_{0j} country-intercepts of REF; IMM β_{1j} country-slopes of the regression of IMM on Exp; REF β_{1j} country-slopes of the regression of REF on Exp.

most important issues because here the focus is on finding contextual predictors of more/less xenophobic responses and not necessarily testing whether a theorized causal mechanism is in place.

Contextual predictors. Three country-level variables are used to try explaining variation in the effects of terrorist attacks on public opinion: *unemployment rate*, as a proportion of

the labor force; *migrant stock*, as a proportion of the population; and *primary education*, which indicates the percentage of the labor force whose highest educational level attained is the primary.¹⁶

Results

Table 7 contains results of a multilevel model fit stepwise. The first two columns indicate results for a model in which the intercepts of *immigration* and *refugees* are allowed to vary across countries. We observe a significant average effect of the attacks on both: .12 and .23. The next set of two columns is a model that allows the slopes of both dependent variables on exposure to vary across countries, but with no country-level (Level-2) predictors. They indicate mean slopes across countries significantly different from 0, at .29 and .31.

The last two columns have a random slopes model with country-level predictors. We see that the stock of migrant population is negatively related with both slopes, indicating that the more a country has immigrants living in it, the less an attack leads to anti-immigration and antirefugees sentiments. This seems to be a contextual confirmation of the finding that contact is a good predictor of more tolerant attitudes (Pettigrew, 1998). Contextual variables indicating vulnerability—unemployment rate and low educated workforce—however, are predictors of more anti-immigrant effects of terrorist attacks (confirming the finding in Legewie, 2013), and lower workforce education also predicts stronger antirefugees shifts. Finally, we also observe that the average effect of the attacks across countries are, respectively, .13 and .17, and statistically significant. This means that, on average, individuals across Europe did become more anti-immigrant and antirefugee after the November shootings.

Table 7. Effects of the November Attacks in Europe.

	Random intercepts		Random Slopes 1		Random Slopes 2	
	IMM	REF	IMM β_{1j}	REF β_{1j}	IMM β_{1j}	REF β_{1j}
Exp	0.12*	0.23*	.29*	.31*	0.13*	0.17*
	(0.04)	(0.06)	(.02)	(.12)	(0.05)	(0.01)
Mig	—	—	—	—	-1.68*	-1.06*
	—	—	—	—	(0.18)	(0.06)
Une	—	—	—	—	-0.17	0.40*
	—	—	—	—	(0.37)	(0.06)
PEd	—	—	—	—	1.10*	1.03*
	—	—	—	—	(0.14)	(0.05)
N	25,838 (27 clusters)		—		—	
AIC	113159.283		113233.747		—	
BIC	113306.156		113396.939		—	
					113189.758	
					113401.908	

Note. Robust standard errors in parentheses. IMM : slope of immigrants on treatment, varying across j countries; REF : slope of refugees on treatment, varying across j countries. Model *Random intercepts* is a two-level model with individuals nested in 27 European countries with random intercepts across countries; Model *Random Slopes 1* is a model with random intercepts and random slopes for the effect of exposure on the dependent variables; Model *Random Slopes 2* is a model in which the random slopes of exposure on the outcomes are predicted by country-level variables. IMM = immigrants contribute to the country; REF = refugees; Exp = exposure; Mig = share of immigrants in the population; Une: unemployment rate; PEd = proportion of labor force with only primary education; AIC = Akaike information criterion; BIC = Bayesian information criterion.

* $p \leq .05$.

Conclusion

Terrorism has been found to have large and long-lasting impacts on public opinion in countries that suffer such acts; 9/11 shaped American foreign policy, and Americans' preferences on several issues, for over a decade. The same effects are observed in Europe, for the Madrid bombings in 2004 or the London ones in 2005. It would be expected, therefore, that the barbaric shootings at the *Charlie Hebdo* headquarters and other locations of Paris, in January 2015, as well as the coordinated attacks on November 13, 2015, would have triggered changes in public opinion in France and other European countries. To a large extent, however, this is not what this study finds. We observe no significant differences in French public opinion across a large number of issues after each of these attacks. Across other European countries, only the November shootings had an impact on public opinion—especially in those countries with higher contextual vulnerability.

This cross-country variation could potentially be explained by factors other than contextual vulnerability. The way that national media and politicians in each country framed the attacks, and contextualized to their audiences, may be important in generating more or less xenophobic reactions. We may expect more negative reactions where main news outlets or

leading politicians engaged in politicizing the events to reinforce their antirefugee or anti-immigrant narratives. Further contextual factors might include the country's own history with terrorism (as well as immigration), which may contribute to more stable attitudes. These, and other factors, should be investigated in future research as contextual moderators on how terrorist attacks may influence public opinion.

Importantly, little evidence was also found for another two kinds of impacts of terrorist attacks: polarization and ideological interactions. For most issues, neither did respondents strengthen their initial views, nor did effects work differently across conservatives and liberals—One of a few important exceptions was how, after *Charlie Hebdo*, European liberals turned more restrictive on refugee policy without becoming more xenophobic, while conservatives became more xenophobic without calling for more restrictive refugee policies.

For social and political psychologists studying public opinion impacts of terrorism, these findings are informative on two accounts: First, following *Charlie Hebdo*, we find no increase in realistic or symbolic threat. After the November shootings, French liberals did become more concerned with terrorism, suggesting a higher sense of threat in that case. Nevertheless, these findings indicate that not all terrorist attacks have the same kinds of effects on the public. Second, impacts in Europe of the November shootings show that contextual conditions seem to moderate the size of attitudinal effects. While the connection between terrorism and ideological changes seems established, it is necessary to consider the contextual conditions under which this might be neutralized or, perhaps, strengthened.

These findings also have implications for policy making. Understanding what public opinion reactions are to be expected after terrorist attacks is essential to designing effective policies to counter potential anti-out-group backlashes. Furthermore, it might be informative on what kinds of measures the public could be more or less likely to support in the aftermath of such events. Although France entered a 2-year state of emergency after November 2015, it seems questionable whether the attacks made the public more likely to support it, or any more restrictive immigration policy, in spite of commonly held assumptions.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Notes


1. See, for example, <http://www.nytimes.com/2015/01/08/world/europe/charlie-hebdo-paris-shooting.html>

2. France, Austria, Belgium, Czech Republic, Denmark, Finland, Germany, Ireland, Slovenia, Sweden, and Switzerland.
3. While other shootings continued to happen on the following 2 days, the major event was the *Charlie Hebdo* offices shooting, so we may expect public opinion changes already on the following day.
4. All models use the square root of age so its scale is closer to that of other variables.
5. Models are run with the software Mplus 6.12 (Muthén & Muthén, 1998-2010). Poststratification weights provided by the surveys are not applied because the analysis deals with a subset of observations that does not reproduce, necessarily, the overall national sample, and therefore applying weights calculated based on the whole sample distribution has the potential to add bias to the subsamples.
6. Those who answered with 5, the midpoint, are not included.
7. Throughout the article, only unstandardized results are presented. The weighed least squares mean and variance adjusted (WLSMV) estimator, used due to there being categorical indicators and dependent variables, does not provide standardized standard errors for hypothesis testing.
8. For the model on polarization effects, a cross-loading is included for the indicator on whether immigration is bad for the country. It is included as an indicator both for the symbolic and realistic threats latent variables. The measurement models are available in the supplementary material.
9. Using the package “rdd” (Dimmery, 2016) for R.
10. Those variables that are modeled as multiple indicator latent variables in the structural equation modeling (SEM) approach are now constructed as an additive index as there is currently no integration between regression discontinuity design (RDD) and SEM.
11. Regression discontinuity lacks a test for interaction effects, and the multiple-group approach used before would result in too small sample sizes, with little statistical power to yield any meaningful inferences.
12. The supplementary materials contain a table with LATE (local average treatment effect of taking the survey after the attacks) estimates using bandwidths half the size and also two times larger than those presented here. Results do not change under those alternative specifications.
13. Education, in its turn, is only measured by asking how old respondents were when they stopped full-time education, instead of highest degree obtained.
14. Full results with controls are in the supplementary materials.
15. The same has not been used for the *Charlie Hebdo* part due to the small Level-2 sample size of only 10 countries in that case (see Stegmueller, 2013).
16. All data retrieved from the World Development Indicators, at <http://http://databank.worldbank.org/data/>

Supplemental Material

Supplementary material is available online with this article.

ORCID iD

Bruno Castanho Silva  <https://orcid.org/0000-0001-9363-4704>

References

- Ben-Ezra, M., Leshem, E., & Goodwin, R. (2015). In the wake of national trauma: Psychological reactions following the Charlie Hebdo terror attack. *The American Journal of Psychiatry*, 172, 795-796.
- Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society, Series B (Methodological)*, 57, 289-300.
- Benjamini, Y., & Yekutieli, D. (2001). The control of the false discovery rate in multiple testing under dependency. *The Annals of Statistics*, 29, 1165-1188.
- Berrebi, C., & Klor, E. F. (2008). Are voters sensitive to terrorism? Direct evidence from the Israeli electorate. *American Political Science Review*, 102, 279-301.
- Bogardus, E. S. (1933). A social distance scale. *Sociology & Social Research*, 17, 265-271.
- Bonanno, G. A., & Jost, J. T. (2006). Conservative shift among high-exposure survivors of the September 11th terrorist attacks. *Basic and Applied Social Psychology*, 28, 311-323.
- Bozzoli, C., & Müller, C. (2011). Perceptions and attitudes following a terrorist shock: Evidence from the UK. *European Journal of Political Economy*, 27, S89-S106.
- Broockman, D. E. (2014). Do female politicians empower women to vote or run for office? A regression discontinuity approach. *Electoral Studies*, 34, 190-204.
- Caricati, L., Mancini, T., & Marletta, G. (2017). The role of ingroup threat and conservative ideologies on prejudice against immigrants in two samples of Italian adults. *The Journal of Social Psychology*, 157, 86-97.
- Castanho Silva, B., Bosancianu, C. M., & Littvay, L. (in press). *Multilevel structural equation modeling: A basic introduction*. Thousand Oaks, CA: SAGE.
- Castano, E., Leidner, B., Bonaccossa, A., Nikkah, J. P., errulli, R., Spencer, B., & Humphrey, N. (2011). Ideology, fear of death, and death anxiety. *Political Psychology*, 32, 601-621.
- Davis, D. W., & Silver, B. D. (2004). Civil liberties vs. security: Public opinion in the context of the terrorist attacks on America. *American Journal of Political Science*, 48, 28-46.
- Dimmery, D. (2016). *rdd: Regression Discontinuity Estimation* (R package version 0.57). Retrieved from <https://CRAN.R-project.org/package=rdd>
- Echebarria-Echabe, A., & Fernández-Guede, E. (2006). Effects of terrorism on attitudes and ideological orientation. *European Journal of Social Psychology*, 36, 259-265.
- Edling, C., Rydgren, J., & Sandell, R. (2016). Terrorism, belief formation, and residential integration: Population dynamics in the aftermath of the 2004 Madrid terror bombings. *American Behavioral Scientist*, 60, 1215-1231.
- Eggers, A. C., & Hainmueller, J. (2009). Mps for sale? Returns to office in postwar British politics. *American Political Science Review*, 103, 513-533.
- European Commission. (2016). *Eurobarometer 83.4, November 2015* (ZA6643, dataset version 2.0.0). TNS opinion & social, Brussels [Producer]. Cologne, Germany: GESIS.
- European Social Survey. (2015). *ESS Round 7 (2014/2015)* (Data file edition 2.0, Data Archive and distributor of ESS data for ESS ERIC). Bergen: Norwegian Centre for Research Data.

- Fernbach, P. M., Rogers, T., Fox, C. R., & Sloman, S. A. (2013). Political extremism is supported by an illusion of understanding. *Psychological Science*, 24, 939-946.
- Finseraas, H., Jakobsson, N., & Kotsadam, A. (2011). Did the murder of Theo van Gogh change Europeans' immigration policy preferences? *KYKLOS*, 64, 396-409.
- Finseraas, H., & Listhaug, O. (2013). It can happen here: The impact of the Mumbai terror attacks on public opinion in Western Europe. *Public Choice*, 156, 213-228.
- Gadarian, S. K. (2010). The politics of threat: How terrorism news shapes foreign policy attitudes. *The Journal of Politics*, 72, 469-483.
- Green, E. G. T., Sarasin, O., Baur, R., & Fasel, N. (2016). From stigmatized immigrants to radical right voting: A multilevel study on the role of threat and contact. *Political Psychology*, 37, 465-480.
- Greenberg, J., Pyszczynski, T., & Solomon, S. (1986). The causes and consequences of a need for self-esteem: A terror management theory. In R. F. Baumeister (Ed.), *Public self and private self* (pp. 189-212). New York, NY: Springer-Verlag.
- Greenberg, J., Simon, L., Pyszczynski, T., Solomon, S., & Chatel, D. (1992). Terror management and tolerance: Does mortality salience always intensify negative reactions to others who threaten one's worldview? *Journal of Personality and Social Psychology*, 63, 212-220.
- Hogg, M. A., Meehan, C., & Farquharson, J. (2010). The solace of radicalism: Self-uncertainty and group identification in the face of threat. *Journal of Experimental Social Psychology*, 46, 1061-1066.
- Hopkins, D. J. (2010). Politicized places: Explaining where and when immigrants provoke local opposition. *American Political Science Review*, 104, 40-60.
- Huddy, L., Feldman, S., Capelos, T., & Provost, C. (2002). The consequences of terrorism: Disentangling the effects of personal and national threat. *Political Psychology*, 23, 485-509.
- Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *Review of Economic Studies*, 79, 933-959.
- Imbens, G. W., & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, 142, 615-635.
- Inglehart, R., & Welzel, C. (2005). *Modernization, cultural change, and democracy: The human development*. New York, NY: Cambridge University Press.
- Jakobsson, N., & Blom, S. (2014). Did the 2011 terror attacks in Norway change citizens' attitudes toward immigrants? *International Journal of Public Opinion Research*, 26, 475-486.
- Jost, J. T., Fitzsimons, G., & Kay, A. C. (2004). The ideological animal: A system justification view. In J. Greenberg, S. L. Koole, & T. Pyszczynski (Eds.), *Handbook of experimental existential psychology* (pp. 263-282). New York, NY: Guilford Press.
- Jost, J. T., Glaser, J., Kruglanski, A. W., & Sulloway, F. J. (2003). Political conservatism as motivated social cognition. *Psychological Bulletin*, 129, 339-375.
- Kam, C. D., & Kinder, D. R. (2007). Terror and ethnocentrism: Foundations of American support for the War on Terrorism. *Journal of Politics*, 69, 320-338.
- Lambert, A. J., Scherer, L. D., Scott, J. P., Olson, K. R., Andrews, R. K., & O'Brien, T. C. (2010). Rally effects, threat, and attitude change: An integrative approach to understanding the role of emotion. *Journal of Personality and Social Psychology*, 98, 886-903.
- Lee, D. S. (2008). Randomized experiments from non-random selection in U.S. House elections. *Journal of Econometrics*, 142, 675-697.
- Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, 48, 281-355.
- Legewie, J. (2013). Terrorist events and attitudes toward immigrants: A natural experiment. *American Journal of Sociology*, 118, 1199-1245.
- Malka, A., Soto, C. J., Inzlicht, M., & Leikes, Y. (2014). Do needs for security and certainty predict cultural and economic conservatism? A cross-national analysis. *Journal of Personality and Social Psychology*, 106, 1031-1051.
- Mueller, J. E. (1970). Presidential popularity from Truman to Johnson. *American Political Science Review*, 64, 18-34.
- Muthén, L. K., & Muthén, B. O. (1998-2010). *Mplus user's guide* (6th ed.). Los Angeles, CA: Author.
- Nail, P. R., & McGregor, I. (2009). Conservative shift among liberals and conservatives following 9/11/01. *Social Justice Research*, 22, 231-240.
- Onraet, E., Van Hiel, A., & Cornelis, I. (2013). Threat and right-wing attitudes: A cross-national approach. *Political Psychology*, 34, 791-803.
- Pettigrew, T. F. (1998). Intergroup contact theory. *Annual Review of Psychology*, 49, 65-85.
- Satorra, A., & Bentler, P. M. (2000). Scaled and adjusted restricted tests in multi-sample analysis of moment structures. In R. D. H. Heijmans, D. S. G. Pollock, & A. Satorra (Eds.), *Innovations in multivariate statistical analysis: A Festschrift for Heinz Neudecker* (pp. 233-247). London, England: Kluwer Academic Publishers.
- Satorra, A., & Bentler, P. M. (2010). Ensuring positiveness of the scaled difference chi-square test statistic. *Psychometrika*, 75, 243-248.
- Schüller, S. (2015). The 9/11 conservative shift. *Economics Letters*, 135, 80-84.
- Stegmüller, D. (2013). How many countries for multilevel modeling? A comparison of frequentist and Bayesian approaches. *American Journal of Political Science*, 57, 748-761.
- Stephan, W. G., Ybarra, O., & Bachman, G. (1999). Prejudice towards immigrants. *Journal of Applied Social Psychology*, 29, 2221-2237.
- Taylor, A. (2015, November 16). The Islamic State wants you to hate refugees. *Washington Post*. Retrieved from https://www.washingtonpost.com/news/worldviews/wp/2015/11/16/the-islamic-state-wants-you-to-hate-refugees/?utm_term=.23b98622373e
- Van de Vyver, J., Houston, D. M., Abrams, D., & Vasiljevic, M. (2016). Boosting belligerence: How the July 7, 2005, London bombings affected liberals' moral foundations and prejudice. *Psychological Science*, 27, 169-177.
- van Prooijen, J.-W., Krouwel, A. P. M., Boiten, M., & Eendebak, L. (2015). Fear among the extremes: How political ideology predicts negative emotions and out-group derogation. *Personality and Social Psychology Bulletin*, 41, 485-497.