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Author(s): Henning Finseraas and Ola Listhaug

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It can happen here: the impact of the Mumbai terror attacks on public opinion in Western Europe

Henning Finseraas · Ola Listhaug

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Abstract Do terror attacks have an impact on public opinion, even if the terror attacks happen far away? We exploit the fact that the fourth round of the European Social Survey was conducted in several West European countries at the time of the 2008 terror attacks in Mumbai, India, in order to identify the causal effect of the Mumbai attacks on public opinion. We identify a clear jump in fear of terrorism at home as a consequence of the terror attacks, but despite the increase in fear of terrorism, we find no significant effect of the attack on support for illiberal interrogation techniques or for liberal immigration policies. We do find indications of a shift in conservative direction on the left–right scale, but this shift is not significant in all time windows. Our findings suggest that a terror attack needs to have a very large impact on the fear of terrorism before people change their policy preferences.

Keywords Terrorism · Public opinion · Causal inference · Natural experiment

1 Introduction

On the evening of November 26, 2008, a series of terror attacks took place in Mumbai, India. The best-known attack happened at the Taj Mahal Palace hotel, where more than 200 hotel guests were held hostage, but attacks also took place at the Chhatrapati Shivaji Terminus, a Jewish center, and a café, and there were two car bombs. The attack on the Taj Mahal Palace hotel lasted until November 29, 2008, when the police finally managed to clear the hotel of terrorists. In total, 164 people were killed and more than 300 people were injured. The attacks were covered extensively in the Western media (see Gupta and Rajiv 2008). In this paper, we examine whether the attacks in Mumbai had observable effects on public opinion in Western Europe.

H. Finseraas (✉)
NOVA, PO Box 3223, Elisenberg, 0208 Oslo, Norway
e-mail: hfi@nova.no

O. Listhaug
Department of Sociology and Political Science, NTNU, Trondheim, Norway

A small but growing literature on the attitudinal consequences of terrorism suggests that terror attacks will influence attitudes by increasing the perception of an imminent threat to one's security. Davis and Silver (2004) argue that an increase in perceived threat might lead to more demand for government-provided security as individuals attempt to reduce the discomfort from the heightened threat. According to what is termed appraisal-tendency theory in social psychology, fear makes individuals more likely to support government-provided security by increasing individual pessimism and risk-aversion (Lerner et al. 2002). The rational choice literature on terrorism has for the most part been concerned with understanding the rationality of terrorists (e.g., Shughart 2011) and the determinants of terrorism (e.g., Krieger and Meierrieks 2011; Kurrild-Klitgaard et al. 2006).[1] However, as Frey et al. (2009) point out, security is a fundamental public good, the provision of which competes with the provision of other public goods, and it has been acknowledged that fear of terrorism among the voters might lead to a shift toward repressive policies (e.g., Shughart 2006: 12). A reasonable hypothesis from a rational choice perspective, however, would be that an increase in fear should change policy preferences if and only if the increase in fear reflects new information regarding the likelihood of terror attacks and not simply if it reflects an increase in the saliency of terrorism.[2]

Davis and Silver (2004) find results consistent with the expectation that fear drives preferences in their study of American survey data in the aftermath of September 11, 2001, as perception of threat is correlated with willingness to trade-off civil liberties for security (see also Morella and Zechmeister 2009), and Hetherington and Suhay (2011) find that respondents without authoritarian personality traits are less supportive of civil liberties if they have fear of terrorism. These studies hint at important attitudinal effects of terrorism, but unfortunately their research design does not allow for causal inference. Lerner et al. (2002), however, conduct an experiment where they randomly assign participants to different texts and questions intended to induce fear, anger or sadness, and report that assignment to fear is associated with more conservative policy preferences (see also Huddy et al. 2005). Although the Lerner et al. experimental design implies that causal inferences can be drawn, the external validity of results from laboratory experiments of this kind may be limited. Finally, there are some studies that combine real-world data with a research design suitable for causal inference. Research conducted in Spain at the time of the terror attack in Madrid provides evidence not only of a general shift toward more conservative political views as a consequence of the attack, but also an increase in anti-Arab sentiments (Echebarria-Echabe and Fernández-Guede 2006),[3] and Bozzoli and Müller (2009) report a large and persistent effect of the London bombings on concern about a future terror attack and less support for civil liberties after the attack. Moreover, research from Israel finds that an increase in the number of local fatalities due to terrorism is correlated with an increase in support for the Right political bloc (Berrebi and Klor 2008), but also a greater willingness among Israelis to grant territorial concessions to the Palestinians (Gould and Klor 2010). Our study is an important addition to this literature since we can directly examine the mechanism of threat, we

[1] But see Frey et al. (2009), who study how terrorism affects survey respondents' reported life satisfaction.

[2] For instance, Becker and Rubinstein (2010) argue that some people will invest effort to reduce the discrepancy between their subjective beliefs about the likelihood of terrorism and the objective danger, rather than changing the demand for a "terror-infected" good—the "terror-infected" good in our case being civil liberty.

[3] The Madrid attack took place only 3 days before the parliamentary election, an election where the incumbent conservative government suffered heavy defeats. Thus, the conservative shift on the left–right scale appears to have been completely reversed by the conservative government's handling of the attack (see Montalvo 2011, for an analysis of the electoral impact of the attack).

have data from several countries which might be affected differently, and we study the impact of terrorism abroad, which is important because relatively few developed democracies recently have experience of large-scale terror attacks.

A large-scale terror attack at home is probably more likely to have an impact on public opinion than a similar attack abroad. However, large-scale attacks in Western Europe and the United States are conducted by terrorists with an international agenda. Terrorist organizations like Al-Qaida regularly release threats against Western governments, and terrorist cells have been discovered across Europe. Such factors might reduce the importance of geographical distance from the attack (see Morella and Zechmeister 2009, for similar arguments). In addition, the rapid spread of news through many forms of media permits citizens in countries far away to get immediate live footage of unfolding events. For these reasons, even attacks abroad can heighten the fear of terror at home, and subsequently change political attitudes and preferences. Fear is an emotional reaction, and if the terror attacks in Mumbai triggered fear of terror at home, this could have an impact on attitudes in spite of the geographic distance.

We examine the relationship between terrorism, fear of terrorism, and three relevant political attitudes: support for illiberal interrogation techniques, opposition to liberal immigration policies, and self-placement on the left–right scale. The political attitude variables are varying in the degree of "closeness" in the form of cognitive links to the event (the terror attack), with support for illiberal interrogation techniques being closer than immigration policies and self-placement on the left–right scale, and thus more likely to be affected by the attack. We are able to address the issue of how terrorism influences political attitudes from a causal angle by exploiting the fact that the terror attacks in Mumbai represented an exogenous shock to citizens' expressed fear of terrorism. We are able to examine the impact of the shock since the European Social Survey was conducted at the time of the attacks and includes information about the date the respondent was interviewed. We have a large number of respondents interviewed immediately before and immediately after the attacks, and the distribution of answers just before the attacks is assumed to be a reasonable counterfactual to the distribution of answers after the attacks. Under this assumption, we can get a credible causal estimate of the impact of the terror attacks. The next section describes our data and research design, before we present the empirical results and conclusion.

2 Data and research design

Data collection for the fourth round of the European Social Survey took place simultaneously with the attacks in India and contains information about the date of interviews as well as survey items related to fear of terrorism and associated attitudinal questions. This situation gives us a rare and excellent opportunity to explore the relationship between terror attacks, fear of terrorism, and attitudinal change from a causal angle: a large number of individuals were interviewed just before the attacks took place (control group), and a large number were interviewed just after the attacks took place (treatment group). In principle, there is no reason to believe that those interviewed right before the attacks should differ systematically from those interviewed right after the attacks.[4] This is nonetheless something we can easily

[4]The procedures for assigning interview dates vary somewhat across countries, but the process is generally as follows: the interviewers are allotted a number of sampled units with whom they attempt to make appointments, either face-to-face or by phone (but phone calls to make appointments are acceptable only if the country can provide evidence that the response rate will not be damaged by appointments being made by phone

explore empirically. This means that the Mumbai terror attacks can be viewed as a natural experiment on how an exogenous shock to the expressed fear shapes policy preferences (see Krosnick and Kinder 1990 and Finseraas et al. 2011 for similar research designs).

To assess the impact of the terror attacks, we construct a variable to represent the "control" and "treatment" related to the attacks. The attacks started in the evening (local time) of November 26, and lasted until November 29. We exclude respondents interviewed on November 26 and November 27 to be reasonably sure that respondents in the treatment group knew of the attacks at the time of the interview. Then we construct a variable, *Treatment group*, which equals 1 if the respondent was interviewed up to 20 days after the attacks and zero if the respondent was interviewed up to 20 days before the attacks (we also present results with alternative time windows).

Using this approach, the difference in means of the dependent variables before and after the attacks will be the average causal effect of the attack in the 20 days after the attacks, given that assignment to treatment and control group truly is random and that other related events did not happen at the same time. In principle, it seems reasonable to assume that day of interview within the time window we have chosen is random, at least when we account for cross-national variation in sample sizes by including country fixed effects (i.e., including a full set of indicator variables for the respective countries in the sample as independent variables in the regressions). If assignment is successfully random, we should find no significant differences between the control group and the treatment group on exogenous variables. We examined whether the treatment and control group differed on a range of exogenous, potentially confounding variables—gender, age, income, education level, whether respondent is born in the respective country, whether the respondent lives in an urban area—in a series of probit regressions with treatment status as the dependent variable. We always include country fixed effects since the treatment and control group are not of the same size in all countries, and since education and income level vary slightly across West European countries. The results are reported in Table A.1. We find that respondents in the treatment group are slightly younger ($z = 1.87$, $p = 0.06$) and have slightly higher income ($z = 1.64$, $p = 0.10$) compared to those in the control group.

As reported in Table 1, the mean differences between the treatment and control group on the observed variables are small, suggesting that a simple comparison of means on the dependent variable(s) should not be too far from the average causal effect. The problem, however, is that the assumption of balanced distributions on *unobserved* variables is more difficult to defend when there are differences in means of the observed variables. Fortunately, the nature of our research design allows us to estimate the causal effect using a Regression Discontinuity (RD) design. A RD design is useful to estimate causal effects when assignment of respondents into the treatment group or the control group is known and varies discontinuously with an underlying variable (for an excellent introduction to RD designs using political data, see Hainmueller and Kern 2008). The method was originally developed by education researchers interested in estimating the causal effect of a scholarship on later achievements. In their case, students with a test score above a specific threshold were granted the scholarship, while no students with a test score below the threshold were entitled to it. Very close to the threshold, assignment to the scholarship is largely random

rather than face-to-face). At least four unsuccessful personal visits must be attempted before the sampled unit is abandoned. All interviews have to be made face-to-face. One of the most important goals is to achieve a high response rate and much of the work is tailored to obtain this goal. We find it unlikely that the procedures for obtaining a high response rate should systematically influence our results. See www.europeansocialsurvey.org for detailed fieldwork and data documentation.

Table 1 Descriptive statistics

	Obs.	Mean	Std. Dev.	Min	Max
Treatment group					
Fear of terror	2487	0.41	0.49	0	1
Prison	2471	0.79	0.41	0	1
Torture	2469	0.69	0.46	0	1
Immigrants	2465	0.61	0.49	0	1
Rightscale	2361	5.03	2.05	0	10
Days	2487	9.82	5.90	0	20
Born in country	2487	0.92	0.27	0	1
Age under 50	2487	0.55	0.50	0	1
Lives in urban area	2487	0.30	0.46	0	1
Primary education	2487	0.12	0.32	0	1
Male	2487	0.52	0.50	0	1
Income	2487	0.57	0.31	0	1
Control group					
Fear of terror	2960	0.31	0.46	0	1
Prison	2945	0.80	0.40	0	1
Torture	2932	0.70	0.46	0	1
Immigrants	2932	0.62	0.49	0	1
Rightscale	2820	5.05	2.05	0	10
Days	2960	−10.57	5.67	−20	−1
Born in country	2960	0.92	0.27	0	1
Age under 50	2960	0.53	0.50	0	1
Lives in urban area	2960	0.32	0.46	0	1
Primary education	2960	0.11	0.31	0	1
Male	2960	0.51	0.50	0	1
Income	2960	0.56	0.30	0	1

(students scoring just above the threshold will on average be very similar to students scoring just below the threshold), and by regressing treatment status on the dependent variable and controlling for the assignment mechanism (i.e., the test score in the education example, centered on the threshold score), one effectively estimates the treatment effect *at the threshold.* The simplicity and wide applicability of RD designs have caused a rapid increase in their applications within economics and political science over the last decade (e.g., Lee et al. 2004; Hainmueller and Kern 2008; Eggers and Hainmueller 2009).

In our case, assignment to treatment or control group varies discontinuously with the date of interview, as the date of interview completely determines assignment to control or treatment group. We can therefore estimate the causal effect of the attacks using the RD design by regressing treatment status on the dependent variable(s) while controlling for *days*, with days referring to the number of days before or after the attacks the respondent was interviewed; thus in our case *days* is a variable running from −20 to 20 since we chose a time window of ±20 days around the attacks. To allow the trend in responses to be different after the attacks than before, we also include an interaction term between days and treatment status. This set-up also allows for an examination of how quickly after the attacks the

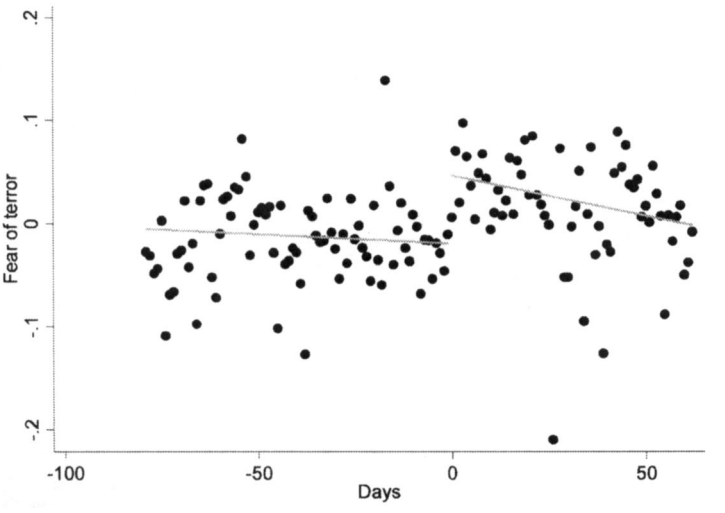

Fig. 1 Mean score on fear of terrorism by days

treatment effect disappears. Following standard practice (e.g., Angrist and Pischke 2009), we tested for different polynomial orders of days, but Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) measures of fit always supported the most parsimonious model with a linear trend. To assess the robustness of the results we vary the bandwidth (i.e., the time window around the attacks) and we conduct placebo analysis.

3 Empirical results

We begin the empirical analysis with a simple graph of the relationship between fear of terrorism and time. Fear of terrorism is measured by a survey question asking of the respondent's assessment of the likelihood of a terrorist attack in the country during the next twelve months. The respondent could answer very likely, likely, not very likely, and not at all likely. For simplicity, we recoded the variable to a binary one with 1 = very likely/likely and 0 = not at all likely/not very likely. In Fig. 1, the Y-axis represents the daily average of fear, adjusted for country-specific differences in levels, while the X-axis represents the number of days before or after the attacks centered on the November 28, 2008. In addition to the data points, the figure shows two regression lines: one line fit to the data before the attacks and one line fit to the data after the attacks. The break in the lines visualizes a jump in fear of terrorism after the attacks. Only two dates in the preattack period stand out as having a large mean, the October 3 and the November 9. We examined Wikipedia's list of world events between September 2008 and January 2009 (http://en.wikipedia.org/wiki/2008) in search of other events that might have caused a jump in the fear of terrorism. We found only one large terror attack, occurring on September 20 when 60 people were killed in a suicide truck bomb in Islamabad, Pakistan, and nothing which might cause a spike on the two pre-Mumbai attack dates that stand out in Fig. 1. We also observe one very low mean in the post-attack period, which turns out to be Christmas Eve, a date with only 14 observations and a day when the media tend to focus on "soft" news.

Table 2 reports the regression results for the ±20 days window, and represents more formal tests of whether the terror attacks increased the fear of terrorism. Column 1 is a

Table 2 Terror attacks in Mumbai, India, and fear of terror attack in own country. Marginal effects after probit

	(1) Fear of terror	(2) Fear of terror	(3) Fear of terror	(4) Fear of terror
Treatment group	0.071***	0.087***	0.088***	0.081***
	(0.013)	(0.014)	(0.026)	(0.029)
Treatment * Days			0.001	0.002
			(0.002)	(0.002)
Days			−0.001	−0.001
			(0.002)	(0.002)
Controls	No	Yes	No	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	6781	5447	6781	5447
No. of countries	12	12	12	12

Notes: *Fear of terror*: How likely terrorist attack in country during next twelve months (1 = very likely/likely, 0 = Not at all likely/not very likely). *Treatment group*: Interviewed after the terror attack in Mumbai, India. *Days*: Number of days since the terror attack. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

simple regression with the treatment indicator as the only independent variable, column 2 includes the control variables,[5] while columns 3 and 4 are regressions with a RD design. Country fixed effects are included in all models. Columns 1 and 2 examine whether the mean is higher in the treatment group compared to the control group, while columns 3 and 4 examine whether there is a break in the mean score on the dependent variable exactly when the attacks happened. We estimate probit models with robust standard errors and report the marginal effects.

The results unambiguously support the conclusion that fear of terrorism is higher in the treatment group (columns 1 and 2), and that the jump in fear happened immediately after the attacks (columns 3 and 4). The first conclusion is straightforward and based on the significant treatment coefficient in columns 1 and 2. The latter conclusion is based on the positive and significant coefficient for treatment group in columns 3 and 4, which due to the inclusion of days and its interaction with treatment in columns 3 and 4, is an estimate of the treatment effect on the first day after the attacks. Remember that those interviewed on the first day after the attacks are coded 0 on the days variable, i.e., for these respondents days = 0 and treatment * days = 0, which implies that the treatment effect for those interviewed on the first day after the attacks is simply the coefficient for treatment group. According to the estimates in Table 2, the effect of the attacks on expressed fear was substantial, as the probability of fearing terror attacks at home is around nine percentage points higher at the time of the terror attacks in India (column 3). Somewhat surprisingly, we find no significant decline in the treatment effect over the 20 days (treatment * days is insignificant), but Fig. 1 shows that a decline in fact did happen when we expand the time window. Figure 2 shows

[5] We control for gender, age, income, education level, whether the respondent is born in the respective country, and whether the respondent lives in an urban area. Strictly speaking, we do not need to include all of these controls since there is a significant difference in means between treatment and control groups only for age (see Table A.1). However, including exogenous controls might increase precision, but this is not the case here, possibly because the number of observations is reduced when we include controls.

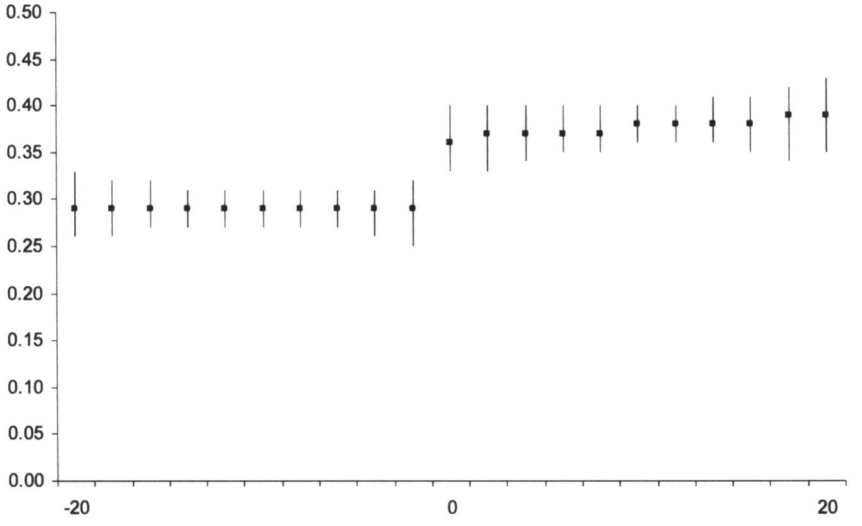

Fig. 2 Predicted fear of terrorism by days. *Dots* represent predicted probabilities, *lines* represent 95% confidence intervals. *Notes*: The predicted probabilities are based on the coefficients reported in Table 1, column 4. The control variables, including the country dummies, are set at their mean values. The probabilities are estimated using the SPost program (see Long and Freese 2006)

the predicted probabilities (and the accompanying 95% confidence intervals) of expressing fear of terrorism by *days*, visualizing how the predicted fear increases at days = 0 (28th November).

We conduct several falsification tests to improve the validity of our research design. First, we examine whether we find a treatment effect on a variable for which we should not expect to find any effect, namely on expressed fear of a burglary. Next, we conduct so-called placebo analyses where we "pretend" that the attacks happened 1 month earlier, 2 months later,[6] and 2 years earlier (using data from the third round of the ESS) than they actually did happen. We expect insignificant treatment effects in these placebo analyses since there were no large-scale terror attacks in these samples that should increase the fear of terrorism. The results, presented in Table A.2, show no treatment effects in these analyses, strengthening the validity of our design. We also estimate the treatment effect using five time windows other than the one presented in the main analysis. The results are reported in Table A.3 and show that the treatment effect is strongly significant in all windows. The coefficient is very stable, and the standard error increases only slightly from column 1 to column 5, which is to be expected due to the reduction in the number of observations. We cannot think of any reason apart from the terror attacks in India that can explain the sudden jump in fear of terrorism at that precise time.

We have provided evidence that the attacks increased the perceived threat of terrorism. Did the attacks have consequences for preferences and attitudes which are tied less directly to the terror attacks and more directly related to the potential for change in relevant policies? Since we have identified a higher level of fear in the treatment group, we ask if the terror

[6] We chose 2 months later rather than 1 month later because 1 month later is in the middle of the Christmas vacation, a period during which fewer respondents were interviewed.

Table 3 Terror attacks in Mumbai, India, and support for illiberal interrogation techniques. Marginal effects after probit

	(1) Suspect in prison until police are satisfied	(2) Suspect in prison until police are satisfied	(3) Torture never justified	(4) Torture never justified
Treatment group	−0.012	0.024	0.010	0.004
	(0.011)	(0.022)	(0.013)	(0.025)
Treatment * days		−0.0005		0.001
		(0.002)		(0.002)
Days		−0.002		−0.0003
		(0.0013)		(0.001)
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	5538	5538	5517	5517
No. of countries	12	12	12	12

Notes: *Prison*: Terrorist suspect in prison until police satisfied (1 = Agree strongly/agree, 0 = Neither agree nor disagree/disagree/Disagree strongly). *Torture*: Torture in country never justified even to prevent terror attacks (1 = Agree strongly/agree, 0 = Neither agree nor disagree/disagree/Disagree strongly). *Treatment group*: Interviewed after the terror attacks in Mumbai, India. *Days*: Number of days since the terror attacks. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

attacks led to greater support for the view that terrorist suspects should be kept in prison until the police are satisfied, or less support for the view that torture is never justified.

Table 3 presents results from the regression analysis with support for the view that "terrorist suspects should be kept in prison until police are satisfied" (columns 1 and 2), and support for the view that "torture is never justified, even to prevent terror" (columns 3 and 4). Graphs presenting the relationship between the policy variables and time are presented in the Appendix (Fig. A.1). The impression from the graphs, which are based on the widest time window possible, are that, if anything, support for illiberal interrogation policies fell at the time of the attacks. The jumps are, however, insignificant, both in the sample we use to produce the graphs and in the ±20 days window we use in Table 3. We estimated a series of models using different time windows and polynomials for *days*, but we never find a significant jump in support for illiberal interrogation techniques at the time of the attacks. Our findings suggest that even when fear is increasing, the public remains reluctant to trade-off civil rights for more security, or, perhaps they do not perceive the kind of restrictions in civil rights that we study here as effective in the fight against terrorism.

In Table 4, we explore whether the attacks had any discernible influence on preferences and attitudes that are tied less directly to the "war on terror"—that is, support for a liberal immigration policy and self-placement on the left–right scale. As mentioned, research on the Israeli electorate and on the terror attacks in Madrid, Spain, have found evidence of a shift in a less tolerant and a more conservative direction immediately afterward (Berrebi and Klor 2008; Gould and Klor 2010; Echebarria-Echabe and Fernández-Guede 2006). The graphs (Fig. A.1) show a jump in liberal immigration policy preferences, but a rightist jump on the left–right scale. However, the results in columns 1 and 2, which are based on the ±20 days window, provide no evidence of a shift in immigration policy preferences after the attacks, which is also the case if we estimate the effect using the widest time window possible. The results in columns 3 and 4 suggest a potentially large immediate shift in a conservative

Table 4 Terror attacks in Mumbai, India, and support for liberal immigration policy and self-placement on the left–right scale. Marginal effects after probit (columns 1 and 2), ordered probit (columns 3 and 4)

	(1) Allow many/some immigrants of different ethnic group	(2) Allow many/some immigrants of different ethnic group	(3) Placement on left–right scale	(4) Placement on left–right scale
Treatment group	0.008	−0.002	0.030	0.108*
	(0.014)	(0.028)	(0.030)	(0.058)
Treatment * Days		0.0045*		0.002
		(0.0024)		(0.005)
Days		−0.002		−0.005
		(0.002)		(0.003)
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	5521	5521	5276	5276
No. of countries	12	12	12	12

Notes: *Immigrants*: Allow many/some immigrants of same ethnic group as majority (1 = Allow many to come and live here/allow some, 0 = Allow a few/allow none). *Rightscale*: Placement on left–right scale (0 = Left, 10 = Right). *Treatment group*: Interviewed after the terror attacks in Mumbai, India. *Days*: Number of days since the terror attack. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

direction as a consequence of the attacks. According to a 95% confidence interval based on the estimates in column 4, the attacks led to a conservative swing of between −0.01 and 0.43 on the left–right scale, with a coefficient estimate similar to the gender gap in left–right position. The effect is, however, imprecisely estimated (as the large confidence interval suggests), with a t-value of 1.88, $p = 0.06$. Moreover, the treatment effect on the left–right scale is not significant in all time windows; for instance, it is insignificant in the widest time window. In addition, there is an important caveat regarding the analysis of the impact of the terror attacks on left–right positions. Historically, self-placement on the left–right scale has first and foremost been determined by an individual's position on economic issues (but see Fuchs and Klingemann 1990). The world witnessed the beginning of a severe recession in the fall of 2008 and one might expect some movement in the average position on left–right scale as the financial turmoil increased. Our expectation is that the recession would cause a leftward shift (for an empirical analysis, see Blekesaune 2007), which would push the estimate in the opposite direction to that we expect from the Mumbai terror attacks. If so, our estimate of the Mumbai attacks is biased downward. However, we will not stress this point without a comprehensive analysis of the impact of the recession on left–right preferences.

Finally, we examine the cross-national variation in the treatment effect to explore whether effects are different in, for instance, countries with recent experiences of terrorism (Spain and Great Britain) or countries where a larger share of the population is Muslim (for instance France and Belgium). In this analysis we rely on the widest time window to maximize the number of observations per country. Table A.4 reports descriptive data for fear of terrorism by country. The mean of fear is higher in the treatment group in all but three countries (Norway, Sweden, and Finland). Next, we examine the cross-national variation in the treatment effect at the threshold by estimating multilevel models where we allow the treatment

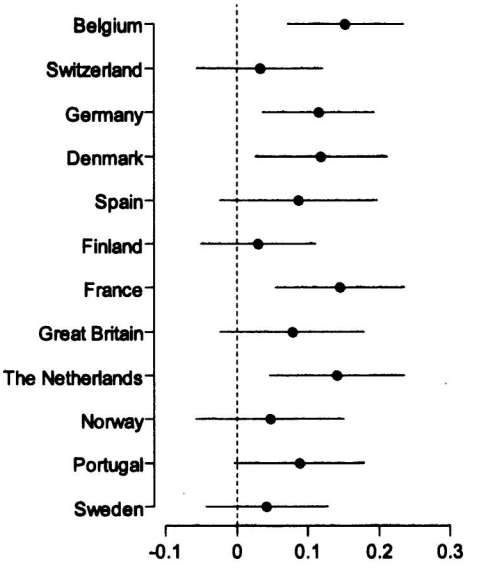

Fig. 3 Treatment effect on fear of terror by country. *Dots* are probit coefficients, the *lines* represent 95% confidence intervals. *Notes*: The estimates are based on a multilevel regression model where treatment, days, and treatment * days (see text for description) coefficients are allowed to vary across countries. The figure displays the treatment coefficient and its 95% confidence interval. The multilevel model is estimated using the lmer package (Bates et al. 2008), and the arm package (Gelman and Hill 2007) in R. R code to create the figure is from the web page accompanying Kastellec and Leoni (2007)

effect, days, and their interaction to vary across countries.[7,8] We present results only from the analysis of fear of terrorism as we find no notable country-specific treatment effects on the interrogation technique questions, the immigration policy question or placement on the left–right scale. For fear of terrorism, the multilevel estimate of the average treatment effect across countries is almost identical to the estimates in Tables 2 and A.3, as the attacks are estimated to have increased the fear of a terror attack by approximately nine percentage points (beta = 0.09, standard error = 0.02, results available upon request). Figure 3 displays the cross-national variation in the treatment effect on the fear of a terror attack. The take-home message of Fig. 3 is that the estimated treatment effect does not vary substantively across countries; however, as a consequence of the relatively small within-country sample sizes, the precision of the country-specific estimates are somewhat imprecise for several countries. Nonetheless, the variation in point estimates is not so large that we think the pooled estimate of the effect is unreasonable, particularly since all confidence intervals overlap and include the cross-national estimate. That said, we observe that the point estimate of the treatment effect is smaller in three Nordic countries that had no experience of large-scale terrorism

[7] Multilevel modeling is especially useful when one wants to examine variation across groups, as the estimates of country-specific coefficients represent a compromise between estimating separate coefficients for each country and one coefficient for all countries (see Gelman and Hill 2007 for an introduction to multilevel modeling). In practice, this implies that the country-specific estimates for those coefficients that are allowed to vary across countries represent a compromise between the mean across countries and the country-specific data. This compromise is guided by the sample size so that the estimates for countries with small samples are driven more by the cross-country relationship.

[8] We estimate linear models since we find that the estimated cross-national variation in the treatment effect estimated by the multilevel linear probability model appears more plausible in light of the descriptive data. It is well known that the multilevel logit/probit model is less accurate than the linear multilevel model when the sample size at level two is fairly small (see, for example, Hox 2002: 176). We would like to stress, however, that the multilevel probit's estimate of the average (cross-national) treatment effect is identical to the estimate from the multilevel linear model, and that all conclusions from the single-level models presented earlier in the paper are similar whether we estimate logit, probit or linear probability models. The only apparent problem with the probit model is its estimate of the degree of cross-national variation in the multilevel models.

at the time of this survey. This might suggest that the attacks in India first and foremost increased fear of terrorism because people were reminded that such attacks can take place, rather than changing the *fundamental* assessments of risk of terrorism. This interpretation of the results is consistent with a view that policy preferences are first and foremost driven by fundamental considerations, and thus will not change insofar as the fundamentals are not changed.

4 Conclusion

Several Western European countries have historical experience of terrorism, but the attacks on the Twin Towers and the London and Madrid bombings made it clear that Western countries are vulnerable to *international* terrorism on their own soil. Some research suggests that feelings of threat from terrorism will have consequences for individual political preferences. A weakness in much of this literature, however, is either the lack of ability to address these issues from a causal angle, or problems of external validity in the case of experimental studies in the laboratory. Furthermore, only a few countries have been victims of recent large-scale terror attacks, and we do not know whether public opinion reacts to terror attacks in foreign countries.

We consider the timing of the terror attacks in Mumbai in 2008 as a natural experiment which allows us to examine how the distributions of expressed fear of terrorism and related policy preferences change as a consequence of the attacks. This is possible since the fourth round of the European Social Survey was conducted around the time of the attacks and includes information on the date of interview. Thus, we can examine how average survey responses change as a result of the attacks.

We find an immediate increase in expressed fear of a terror attack due to the Mumbai attacks, but we find no significant change in support for illiberal interrogation techniques, or support for liberal immigration policies. Apparently, those expressing greater fear of terrorism due to the attacks did not change their terror policy preferences. We do find indications of a shift in the conservative direction on the left–right scale, yet this estimate is not robust across time windows. Moreover, the estimate might be biased due to the financial crisis that evolved at the same time and which possibly affected people's positioning on the left–right scale. The weak effect of the attacks on policy preferences despite the substantial increase in the fear of a terror attack raises the question of whether there is a linear causal relationship between fear and policy preferences.

Our findings are good news for those who worry that a situation with a heightened fear of terrorism will cause a form of "moral panic," where people who are liberal in times of normalcy become inclined to trade-off their civil liberties with security. However, our data set includes only two terror policy-related variables and it is possible that greater fear is related to changes in preferences for other civil liberty-restricting terror-fighting policies. Most importantly, although our treatment—the Mumbai attacks—had a strong effect on fear of terrorism, attacks closer to home would almost certainly cause an even larger change, and it is thus possible that a threshold exists above which changes in fear begin to affect policy preferences more systematically. Alternatively, one might argue that the Mumbai terror attacks did not provide new information on the likelihood of terror attacks but simply increased the saliency of terrorism and, therefore, did not change policy preferences.

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Appendix

Table A.1 Background characteristics (independent variables) and treatment status (dependent variable). Marginal effects after probit

Male	0.011	$N' = 7012$
	(0.012)	
Age	−0.0007*	$N = 6751$
	(0.0004)	
Income	0.038	$N = 5722$
	(0.023)	
Primary education	0.026	$N = 6994$
	(0.019)	
Born in country	−0.0001	$N = 7009$
	(0.022)	
Lives in urban area	−0.005	$N = 7002$
	(0.013)	

Notes: All regressions include country-fixed effects. The dependent variable is whether the respondent is assigned to the *Treatment group*: Interviewed after the terror attacks in Mumbai, India. *Age* is a continuous variable, *income* is a ten category variable, while the rest are dummy variables. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.2 Terror attack in Mumbai, India, and fear of terror attack in own country. Marginal effects after probit

	(1) Fear of burglary	(2) Fear of terror (placebo)	(3) Fear of terror (placebo)	(4) Fear of terror (placebo)	(5) Fear of terror (placebo)
Treatment group	−0.010	−0.006	−0.017	−0.007	−0.041
	(0.027)	(0.025)	(0.042)	(0.014)	(0.027)
Treatment * days	−0.002	0.003	0.0001		−0.006**
	(0.002)	(0.002)	(0.004)		(0.002)
Days	0.002	−0.001	−0.001		0.004**
	(0.002)	(0.001)	(0.003)		(0.002)
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	5583	7252	1998	5299	5299
No. of countries	12	12	12	12	12

Notes: *Fear of burglary*: How often worry about your home being burgled (1 = Never, 0 = all or most of the time/some of the time/just occasionally). *Fear of terror*: How likely terrorist attack in country during next 12 months (1 = very likely/likely, 0 = Not at all likely/not very likely). *Treatment group*: Interviewed after the terror attacks in Mumbai, India in column 1. Placebo treatment in columns 2 (the date of the attacks is set to 1 month before the actual attacks), 3 (the date of the attacks is set to one month after the actual attacks), and 4 and 5 (the date of the attacks is set to 2 years before the actual attacks). *Days*: Number of days since the terror attacks in column 1, number of days since the placebo terror attacks in columns 2–5. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.3 Terror attack in Mumbai, India, and fear of terror attack in own country. Marginal effects after probit

	Fear of terror −79/+62 days window	Fear of terror ±30 days window	Fear of terror ±25 days window	Fear of terror ±15 days window	Fear of terror ±10 days window
Treatment group	0.096***	0.091***	0.082***	0.093***	0.110**
	(0.017)	(0.024)	(0.026)	(0.033)	(0.042)
Treatment * days	−0.001*	−0.002	−0.00004	0.001	0.003
	(0.0005)	(0.002)	(0.0001)	(0.004)	(0.007)
Days	−0.0001	0.001	0.0002	−0.001	−0.004
	(0.0002)	(0.001)	(0.001)	(0.003)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Observations	17053	7419	6439	4236	2839
No. of countries	12	12	12	12	12

Notes: *Fear of terror*: How likely terrorist attack in country during next twelve months (1 = very likely/likely, 0 = Not at all likely/not very likely). *Treatment group*: Interviewed after the terror attacks in Mumbai, India. *Days*: Number of days since the terror attack. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

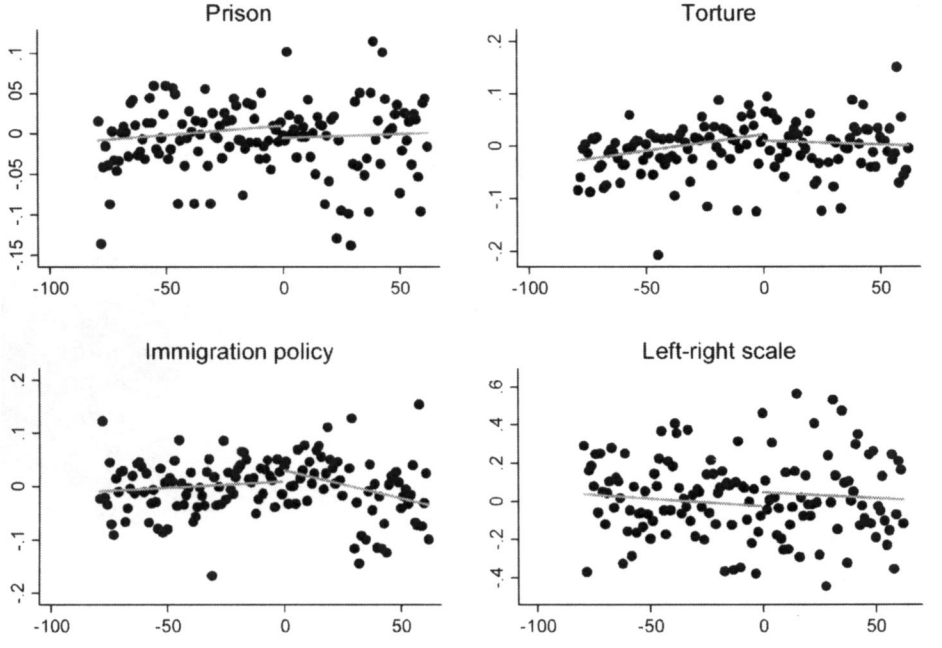

Fig. A.1 Mean scores on policy variables (*Y*-axis) by *days* (*X*-axis)

Table A.4 Descriptive statistics for fear of terror by country

Fear of terror	Treatment group		Control group	
	Obs.	Mean	Obs.	Mean
Belgium	588	0.44	244	0.28
Switzerland	164	0.21	334	0.16
Germany	579	0.60	361	0.52
Denmark	232	0.47	414	0.39
Spain	62	0.89	272	0.88
Finland	376	0.11	794	0.11
France	230	0.63	601	0.49
Great Britain	95	0.83	252	0.77
The Netherlands	66	0.42	277	0.27
Norway	77	0.13	259	0.14
Portugal	201	0.29	188	0.20
Sweden	180	0.08	569	0.11

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