Geys, B., & Qari, S. (2017). Will you still trust me tomorrow? The causal effect of terrorism on social trust. *Public Choice, 173*(3-4), 289-305
DOI: http://dx.doi.org/10.1007/s11127-017-0477-1

Will you still trust me tomorrow?
The causal effect of terrorism on social trust

Benny Geys [a] and Salmai Qari [b]

[a] Norwegian Business School BI, Department of Economics, Nydalsveien 37, N-0442 Oslo, Norway. Email: Benny.Geys@bi.no

[b] German Institute for Economic Research (DIW Berlin) & Berlin School of Economics and Law (HWR Berlin), Department of Business and Economics, Badensche Straße 52, 10825 Berlin, Germany. Email: Salmai.Qari@hwr-berlin.de

Abstract:
How do people respond to terrorist events? Exploiting the timing of the 2010 wave of the annual 'Society Opinion Media' survey in Sweden, we study the causal effect of the Stockholm bombings of 11 December 2010 on Swedish public opinion. Our main contribution is that we draw explicit attention to the link between terrorist events and individuals' social trust. While we identify a strong effect on individuals' terrorism concerns, any observed effects with respect to generalized and neighborhood trust appear to be short-lived, thus suggesting that isolated terrorist events have only limited, transitory effects on established social attitudes.

Keywords: Terrorism, Public Opinion, Trust, Threat.

1. Introduction

While scholars have not reached universal agreement about the precise definition of terrorism, it is "commonly defined as the deliberate use of violence and intimidation directed at a large audience to coerce a community (government) into conceding politically or ideologically motivated demands" (Krieger and Meierrieks 2011, p. 4; see also Sandler 2016). A rapidly expanding literature across both economics and political science has made considerable progress in our understanding of the determinants of terrorism as well as its socio-economic consequences. For instance, with respect to the drivers of terrorist activity, researchers have pointed to the roles of regime type (Gaibulloev et al. 2017), education levels (Brockhoff et al. 2015), ethnic or religious divisions (Gleditsch and Polo 2016; Filote et al. 2016), oil revenues (Piazza 2016), and banking crises (Gries and Meierrieks 2013). Interestingly, a review of this literature by Krieger and Meierrieks (2011) highlights that institutional factors generally appear to be more important than economic ones for explaining the emergence of (transnational) terrorism. Basuchoudary and Shughart (2010), however, reach the opposite conclusion, namely that economic freedoms are more important than political freedoms in reducing the emergence of terrorism. In terms of macro-level socioeconomic outcomes, periods of greater terrorist activity have been linked inter alia to trade disruptions, forgone foreign direct investment, declining tourist activity, the destruction of industrial capacity and loss of human capital (Abadie and Gardeazabal 2003, 2008; Fielding 2003; Sandler and Enders 2008). As a result, terrorism frequently can have large effects on a country's GDP and level of economic development (Crain and Crain 2006).

Research into the consequences of terrorist activity for *micro*-level attitudes and public opinion likewise has expanded strongly in recent years (e.g., Berrebi and Klor 2008; Merolla and Zechmeister 2009, 2013; Mondak and Hurwitz 2012; Getmansky and Zeitzoff 2014; Peffley et al. 2015; Hirsch-Hoefler et al. 2016). One strand of this literature shows that anti-immigrant

sentiments, support for right-wing parties and factional 'radicalization' often strengthens following terrorist events (Echebarria-Echabe and Fernández-Guede 2006; Sinclair and LoCicero 2010; Finseraas et al. 2011; Getmansky and Zeitzoff 2014; Hirsch-Hoefler et al. 2014; Gould and Klor 2015; see, however, Jakobsson and Blom 2014). Such events also make people more willing to sacrifice civil liberties and constitutional principles to suppress terrorism.[1] Bozzoli and Müller (2011), Finseraas and Listhaug (2013) and Hirsch-Hoefler et al. (2014) furthermore illustrate that terrorist attacks unsurprisingly cause heightened concerns over terrorism.

We contribute to this literature by drawing explicit attention to the link between terrorist events and individuals' level of social trust. We focus on social trust since it plays a key role in "sustain[ing] a cooperative social climate (…) [and] facilitate[s] collective behavior" (Zmerli and Newton 2008, pp. 706-707; Sønderskov 2009) and has been linked to a wide range of socially desirable outcomes – including economic growth and development (Berggren et al. 2008; Dincer and Uslaner 2010; Bjørnskov and Méon 2014), improved governmental performance as well as more effective legal and bureaucratic institutions (Putnam et al. 1993; Bjørnskov 2010; Bjørnskov and Méon 2014) and the preservation of the welfare state (Bjørnskov and Svendsen 2013). These findings make the terrorism-trust relation of prime academic and policy interest, particularly since recent work in social psychology indicates that large exogenous shocks – such as terrorist events – can induce shifts in individuals' social attitudes (Bardi and Goodwin 2011). Yet, the direction of the effect of terrorist events on people's trust in others is *a priori* unclear. On the one hand, people might 'come together' in the immediate aftermath of terrorist events to express their grief and solidarity (Gross et al.

[1] For a review of, and critical contribution to, this literature, see Mondak and Hurwitz (2012).

2004; Arvanitidis et al. 2016).[2] Such expressions of solidarity have been argued to represent part of a symbolic exchange that is central to the establishment of trust in social relations (Haas and Deseran 1981), and might thus lead one to predict a positive relation between terrorist events and social trust. On the other hand, "acts of terrorist violence can arouse fear and anxiety in a targeted population" (Huddy et al. 2005, p. 606; Hetherington and Suhay 2011), even when little actual damage is done (Friedland and Merari 1985). Both of these emotions are relevant to individuals' interpersonal attitudes because fear causes people "to be more distrustful" (Treisman 2011, p. 3), while anxiety makes them more guarded towards others (Pugh et al. 2003). This alternative line of argument therefore suggests that more widespread fear and anxiety following terrorist events might undermine the degree to which we trust one another, and lead to individuals' social dislocation (Huddy et al. 2003, 2005).

Despite the substantial potential socioeconomic implications of any shifts in trust brought about by terrorist activity, empirical evidence assessing the exact nature, strength and persistence of this terrorism-trust connection has been slow to emerge. Partial exceptions include Huddy et al. (2003), Gross et al. (2004), Wollebaek et al. (2012), and Arvanitidis et al. (2016). Yet, these studies rely on repeated cross-sectional surveys before and after the terrorist event. This makes it very hard to identify the *causal* effect of terrorist events on individuals' levels of social trust. We improve on previous studies by exploiting the fact that some respondents answered the annual Swedish 'Society Opinion Media' (SOM) survey before the 2010 Stockholm bombings, while others answered only after that event.[3] As the timing of individuals' survey responses is

[2] Consistent with this line of argument, Berrebi and Yonah (2016) report a statistically significant and substantively meaningful *positive* effect of terrorism on philanthropy.

[3] The Stockholm terrorist attack of 11 December 2010 was of limited scale. Around 5pm, two bombs were detonated in the center of Stockholm: one in a car alongside bottles of liquefied petroleum gas, and the other strapped to the body of the culprit. In the event, the bomber – an Iraqi-born Swedish citizen – was killed and two others were injured. Although the death toll was small, the Stockholm bombings widely were considered to be the first attack linked to Islamic terrorism in the Nordic countries. It also was claimed as such by the perpetrator in an email to the Swedish news agency *Tidningarnas Telegrambyrå* 12 minutes before the event. Consequently, it triggered substantial political, media and public debate.

exogenous to the event under study, a regression-discontinuity design can be implemented that permits stronger causal inferences regarding the attack's impact on individual-level trust (see also Finseraas and Listhaug 2013; Jakobsson and Blom 2014). Moreover, the SOM survey includes questions about *both* respondents' concerns about terrorist activities *and* their levels of trust. These data provide a unique opportunity to assess whether any terrorism-trust connection arises by raising levels of fear and anxiety over terrorism amongst Swedes (Huddy et al. 2005; Wollebaek et al. 2012).

2. Theoretical background and hypotheses

Research in sociology and (political) psychology increasingly suggests that "the occurrence of new environmental cues" is one important way by which individuals' values, beliefs and opinions change over time (Bardi and Goodwin 2011, p. 278; see also Peffley *et al*. 2015, Geys 2017). The reason is that personal and/or social shocks – such as divorce, unemployment, wars, natural disasters or terrorist events – can function as critical junctures that instigate a search for "new strategies for action" (Swidler 1986, p. 278). Sometimes the resultant changes in individuals' opinions and beliefs are subtle and small, whereas at other times they can be dramatic. The inherently dynamic nature of personal (and, by extension, public) opinion and belief formation reflects the fact that we update our priors based on "own experiences, … observations of others' actions and experiences, the communication with others about their beliefs and behavior, news from media sources", and so on (Acemoglu and Ozdaglar 2011, p. 4). Individuals thus will respond to structural changes in their socio-political and institutional environment by updating their beliefs and opinions.

Following this line of argument, terrorist events are likely to induce an updating process with respect to individuals' beliefs and opinions about the threat of terrorism – at least to the extent that the event provides "new information regarding the likelihood of terror attacks" (Finseraas

and Listhaug 2013, p. 214).[4] In the aftermath of a terrorist event, people reconsider their likely exposure to instances of terrorist activity and their "perception of an imminent threat to [their] security" (Finseraas and Listhaug 2013, p. 214; Hirsch-Hoefler et al. 2014). The reason is that acts of terrorist violence tend to arouse fear and anxiety in those exposed to them (Huddy et al. 2005). Both emotions are important for complementary reasons. Fear distorts individuals' subjective beliefs away from objective assessments of the actual risk involved in a given setting (Becker and Rubinstein 2011), and may lead to "exaggerate the risk that others will bring about harmful outcomes" (in this case, another terrorist event) (Treisman 2011, p. 30). This is closely related to the fact that people tend to greatly overestimate the risk associated with low-probability events (e.g., airplane crashes, being struck by lightning) that expose them to large costs if they do occur. Anxiety is often reflected in the worry that things will go wrong and is linked to "concern about potentially negative future events and experiences" (Pugh et al. 2003, p. 203). Greater fear and anxiety brought about by terrorist events thus not only induces inflated estimates of future similar events occurring (i.e., people fear new attacks), but also make individuals' preoccupation with this possibility particularly salient (i.e., people worry about future attacks). As a result, we can expect terrorism to heighten individuals' concern over terrorism.

Hypothesis 1: Terrorist events lead to higher public concern over terrorism.

[4] Sørensen (2016) argues similarly that a sudden large-scale influx of immigrants may lead to anti-immigrant anxiety within the native population, whereby people worry that immigrants pose threats to their ways of life. Empirical evidence from Norway appears to substantiate this proposition, although the effect is found to be short-lived and arises only in the initial phases of a migration shock.

Yet, the heightened fear and anxiety brought about by terrorist events is likely to have additional implications in terms of individuals' level of trust in other people. For example, as argued by Treisman (2011, p. 3), it is at least "plausible that fear might cause people to be more distrustful". Such a negative fear-trust association previously has been observed in various social settings. For instance, the constant exposure of individuals in low-income neighborhoods to "crime, concentrated physical decay and social disorder … leads to a constellation of negative psychological states which are experienced by residents: feelings of anxiety and fear, alienation from neighbors, lack of trust in others, and suspicion toward out-groups in general" (Oliver and Mendelberg 2000, p. 576). In our setting, the same reasoning would suggest that heightened levels of fear might undermine social trust after a terrorist attack. Similarly, feelings of anxiety have been linked to "the tendency for individuals to keep their guard up and resist trusting" (Pugh et al. 2003, p. 204). Again, such a negative worry-trust relation has been corroborated in empirical research on, for instance, the effects of job loss. The more people worry about the possibility of (once again) losing a job, the less trust they have in their new employer (Ashford et al. 1989; Roskies and Louis-Guerin 1990; Pugh et al. 2003). Greater fear and anxiety following terrorist events can thus be expected to undermine the degree to which we trust one another (or 'hunker down': Putnam 2007) and lead to individuals' "social and political dislocation" (Huddy et al. 2005, p. 606; see also Huddy et al. 2003; Arvanitidis et al., 2016). This is in line with some definitions of terrorism highlighting that one key terrorist goal is to create a climate of fear that goes beyond the persons targeted by the attack.

Hypothesis 2a: Terrorist events lead to lower trust between citizens.

Several scholars have asserted, however, that terrorist events might also trigger an upsurge in individuals' levels of social trust.[5] The reason is that terrorist-related tragedies may push people into 'coming together' in expressing grief and solidarity in the immediate aftermath of the event. Such greater social cohesion was observed following, for instance, the 9/11 attacks, Anders Behring Breivik's attack in Norway on 22 July 2012, and the Brussels airport bombings on 22 March 2016. Acts of bonding strengthen – or reaffirm – existing social ties and attachments (Putnam 2002). Large-scale expressions of solidarity also indicate clearly that most people care and, as such, can work towards the establishment of trust in social relations (Haas and Deseran 1981). Nonetheless, this evidence of renewed social cohesion might generate only short-lived effects on social trust as the passage of time will limit the organization of large-scale public expressions of solidarity. Hence, such solidarity-driven effects may dampen over time, allowing the negative effects on trust associated with rising fear and anxiety (see above) to dominate. This leads to an alternative hypothesis regarding the terrorism-trust relation.

Hypothesis 2b: Terrorist events lead to greater trust between citizens, albeit possibly only temporarily.

It is important to note at this point that the effects of terrorist events on both individuals' worries about terrorism (Hypothesis 1) and the extent of social trust (Hypotheses 2a and 2b) may depend on micro-level background characteristics (e.g., age, gender, education, or location) and the nature of the events themselves. Unfortunately, our data do not allow us to study such potential sources of heterogeneity in detail (we return to this below), such that we abstain from a more in-depth discussion of them at this point as well. Clearly, however, such heterogeneous effects present important avenues for further theoretical and empirical research.

[5] A substantial literature also highlights the effects of terrorist activity on institutional trust (e.g., Chanley 2002; Sinclair and LoCicero 2010; Arvanitidis et al. 2016).

3. Data

Our data derive from the Swedish 'Society Opinion Media' (SOM) study, which is "an annually repeated cross-sectional self-administered mail survey" conducted by the University of Gothenburg (Weibull et al. 2013, p. iii). Each survey is representative of the Swedish population aged 16-85. The 2010 wave included 5007 respondents, and achieved a response rate of 51%. Importantly, each survey wave confronts respondents with a wide range of attitudinal questions, including their concerns about terrorist activities, their level of trust, and so on. Each wave is fielded consistently between October and January, which implies that random respondent selection during each survey wave makes the Stockholm bombings of 11 December 2010 exogenous to the survey analysed. Consequently, we can use the responses to estimate the causal effect of the Stockholm bombings on individual-level attitudes using a regression-discontinuity design (Green et al. 2009; Bozzoli and Müller 2011; Finseraas and Listhaug 2013; Jakobsson and Blom 2014). Still, as the SOM is a postal survey and people can choose when to answer, a potential concern is that individuals answering later could be different on some dimensions from those responding earlier. We return to – and address empirically – that important issue when discussing our variables and estimation samples below.

3.1. Empirical approach

Our empirical model takes the following form:

$$Y_i = \alpha + \beta_1 PostEvent_i + \beta_2 Days + \beta_3 PostEvent_i*Days + \delta Controls_i + \varepsilon_i$$

The set of dependent variables – contained in Y_i – first of all includes a variable (*Concern about terrorism*) measuring the level of concern about terrorism expressed by individual *i* at the time of the survey (see also Bozzoli and Müller 2011; Finseraas and Listhaug 2013; Hirsch-Hoefler et al. 2016). The precise wording of the question is: "Looking at the current situation, what do you think is a concern for the future? Terrorism". Answers are recorded on a 4-point scale

ranging from 1 (“major concern”) to 4 (“no concern”), and intends to capture emotional responses related to individuals’ worries about terrorist threats (Hetherington and Suhay 2011). It thus relates directly to the hypothesized mechanism underlying the terrorism-trust relation (i.e., fear of and anxiety about such acts).

The second set of dependent variables operationalize respondents’ ‘particularized’ and ‘generalized’ social trust. The former is based on personal knowledge and, hence, restricted to a specific social unit (Freitag and Bauer 2013). In our case, the social unit is the respondent’s neighborhood (see below). In contrast, generalized trust is independent of specific persons or groups, and reflects an estimate of the trustworthiness of the average person in society (Wollebaek et al. 2012; Freitag and Bauer 2013). The wordings of the question are similar to those in the World Values Survey: “In your opinion, to what extent can people be trusted [in general] [in the area where you live]?” Both are measured on a scale from 0 (“people cannot be trusted”) to 10 (“people can be trusted”). Given the importance of measurement equivalence, we should note that recent work has confirmed the equivalence of survey-based trust measures across cultures (Freitag and Bauer 2013). Delhey et al. (2011) also analyzed individuals’ interpretation of the ‘generalized’ trust question, and substantiated its validity for western democracies.

Our main independent variable ($PostEvent_i$) is equal to 1 for respondents answering the survey *after* the Stockholm bombings occurred (0 otherwise). Its coefficient indicates the extent to which that event caused a shift in individual respondents’ attitudes. In some specifications, we also include a linear (or quadratic) time trend (*Days*) centered around the day of the bombings, and its interaction with $PostEvent_i$. These variables control for any potential pre-attack trend in individual attitudes. It furthermore assesses whether the terrorist attack induced a shift in this *trend*, rather than changing only the *levels* of our attitudinal variables. In our main specification,

Days takes values from -19 to 19. This is the maximum number of days allowing a balanced panel timeframe at both sides of the cut-off with no multi-day gap in the survey returns.[6] Still, we present a number of robustness checks to assess the validity of this window choice (see below).

The vector of control variables ($Control_i$) includes respondents' sex, age, education level (i.e., high, medium or low) and region of origin. We also experimented with additional controls for household income, unemployment status, religiosity and Swedish citizenship. While adding more controls reduces the sample size owing to missing values (especially when the income variable is entered), our results remain consistent throughout a series of robustness checks. To preserve space, we report only the results with and without the four baseline controls below.[7]

3.2. Balancing properties of the main estimation samples

As explained, our main sample comprises all observations in the period when *Days* takes values between -19 and 19. Table A.1 in the appendix provides summary statistics for this main sample. To assess the robustness of our results to the choice of the event window, we varied the bandwidth and considered samples for which *Days* ranges between -10 and 10 days, -11 and 11 days, and so on up to a range from -19 to 19 days. For each of these estimation samples, it is crucial for the validity of our empirical approach and the credibility of our results that survey respondents are balanced around the event date. While such a balancing test makes less sense for the full sample of respondents answering the 2010 SOM survey (since this is not the sample

[6] No surveys were returned around the New Year holiday break, which sets the maximum number of days after the event at 19. Note also that the density of observations around the event is balanced in the immediate vicinity before/after the event.

[7] Unfortunately, we have no information about the extent of media coverage of the terrorist event in the locations of our respondents (nor their consumption of such news coverage). As media coverage may affect individual concerns and responses to a terrorist attack, this constitutes an important avenue for further research.

that is critical to our identification strategy), individuals answering just before and just after the terrorist attack should not be different in their (un)observable characteristics.

We assess consistency with that requirement by estimating logit models with a binary dependent variable equal to 1 when individuals responded *after* the Stockholm bombings took place (0 if they responded before the event; i.e., *PostEvent*), and explanatory variables for sex, age, education level and region of origin. Table 1 summarizes the results. The Likelihood-Ratio-tests from each of these auxiliary regressions indicate that the observable characteristics of our respondents are not correlated systematically with whether responses were received before or after the event. We can therefore conclude that the individuals completing the survey either before or after the event are similar at least in terms of these background characteristics. Importantly, this conclusion holds for samples across all the event windows employed in our analysis (more detailed t-tests are provided in table OA.4 in the online appendix).

Table 1 here

3.3. Results

The results with respect to terrorism concerns – which assess the channel of anxiety and fear expected to drive the terrorism-trust relation (Hetherington and Suhay 2011) – are summarized in Table 2. We report OLS results throughout the analysis for ease of interpretation, but all findings are qualitatively and quantitatively similar using ordered logistic regressions. Column (1) includes all observations from the 2010 wave of the SOM survey, and thus reflects the regression-discontinuity specification for the whole sample. Still, as the majority of respondents submit their answers in September/October, our dataset is unbalanced in the sense that the number of observations after 11 December is limited (N=350). Columns (2) and (3) therefore

use our main analysis sample and include responses only from individuals replying in a period of 39 days around the event date (i.e., 19 days before to 19 days after 11 December 2010). As the treatment is balanced on individual background characteristics (see above), such variables are redundant unless estimation precision is increased when they are entered. Columns (4) and (5) therefore replicate the results from columns (1) and (2) while omitting all control variables.[8]

Recall that the variable *Days* refers to the day the survey arrived and was registered at SOM. Hence, a survey sent on Saturday or Sunday will tend to be registered on Monday (or even Tuesday). That is an important consideration because 11 December 2010 was a Saturday, which makes it hard to verify whether responses registered by SOM on Monday 13 December or Tuesday 14 December were completed before or after the Stockholm bombings took place at 5pm on Saturday. As surveys registered on these days could be a mixture of treated and non-treated observations, panel II of Table 2 replicates the analysis in columns (2) and (3) excluding observations from one or both of those days.

Table 2 here

The results reported in Table 2 confirm that the Stockholm bombings caused heightened concerns about terrorism – in line with analyses of other terrorist events by Bozzoli and Müller (2011), Finseraas and Listhaug (2013) and Hirsch-Hoefler et al. (2016). In terms of the estimated magnitude of the effect, the impact of the Stockholm bombings on terror concerns in Sweden is roughly equivalent to the overall effect – estimated across all European countries – of the 2008 Mumbai terror attacks observed by Finseraas and Listhaug (2013) using a similar estimation strategy and window size (i.e., plus/minus twenty days). This finding suggests that

[8] Coefficients for the control variables are not shown in Table 2 to preserve space. See Table A.2 in the appendix for these coefficients.

Sweden is not different from other European countries in the way its citizens respond to terrorist acts. Although Bozzoli and Müller (2011) employ a slightly different estimation approach, they document an immediate effect of the London bombings among respondents in the United Kingdom, which appears to be 30%-40% larger than our Swedish findings. This difference is intuitively reasonable given the different scales of both events. We will return to the potential sensitivity of our findings to the nature of the event under analysis and the specific Swedish setting in more detail in our concluding discussion.

The linear time trend (*Days*) and the interaction term in column (3) have coefficients very close to zero, and both an F-test and individual t-tests indicate that they may be omitted. Entering second-order polynomials for the running variable (i.e., *Days*) on each side of the event date likewise leaves our results unaffected qualitatively (Figure OA.1 in the Online Appendix). Columns (4) and (5) also show that the exclusion of all background characteristics has no substantive effects on our inferences,[9] whereas columns (6) to (9) in panel II indicate that the treatment effects are slightly larger when either 13 December, 14 December, or both are excluded. The latter result is in line with the idea that at least some of the surveys arriving on these days are from untreated persons (i.e., posting their surveys on Saturday *before* the 5pm bombings).

To strengthen our inferences, we implement two types of robustness checks. First, we execute placebo tests with non-event days (e.g., 11 November 2010 or 11 December 2011) and concerns about issues unrelated to terrorism (e.g., environmental degradation, global epidemics and weakened democracy). In all cases, the results remain statistically insignificant (see the Online

[9] This is unsurprising since the treatment is balanced on respondents' background characteristics (see above). Yet, as their inclusion leads to slightly more precise estimates, we retain them throughout the remainder of the analysis.

Appendix). Second, we run a series of regressions for which we vary the event window from ten days around 11 December (which, compared to Table 2, halves the number of days on both sides of threshold; see Green *et al*. 2009) up to 19 days before/after the event (as in Table 2). Obviously, standard errors will be larger with narrower event windows, so the main emphasis here is on the stability of the treatment coefficient. Figure OA.1 in the Online Appendix summarizes these additional regressions. The figure indicates statistically significant results at conventional levels once the bandwidth exceeds 12 days, and the general inference from Figure OA.1 thus is that the precise number of days considered around the event date does not affect our results qualitatively.[10]

Having established that the Stockholm bombings increased Swedish citizens' concern about terrorism, Table 3 evaluates how this effect translated into individuals' social trust (Hypothesis 2a and 2b; Huddy et al. 2005; Wollebaek et al. 2012; Arvanitidis et al. 2016). The model is equivalent to those estimated in Table 2 with the exception of the dependent variable. The first panel of Table 3 evaluates generalized trust, while the lower panel analyses trust in neighbors. Overall, the estimates are not very precise, but the coefficient on *PostEvent* becomes larger once the interaction term between *PostEvent* and *Days* is introduced. This finding differs from the results in Table 2, where inclusion of the interaction term did not change the 'main effect'. It suggests that estimating an 'average' effect for the entire period after the terrorist event may lead to biased inferences. When we omit observations from December 13 (columns 3-4) or December 13 and 14 (columns 5-6), the effects become more pronounced. Yet, the qualitative findings are unaffected. That is, the positive *PostEvent* coefficient in the specifications including interaction terms indicates an *increase* in general trust immediately following the

[10] The same also holds only individuals responding in December are included (in which case *Days* ranges from minus ten to plus nineteen). As a methodological alternative, we also considered 'non-parametric' RD-techniques to identify the optimal size of the window. However, those methods need many observations (Lee and Lemieux 2009), and thus were unfeasible given our sample size.

attack, whereas the negative interaction term indicates that individuals quickly return to their baseline levels of trust. Across all specifications, social trust returns to its baseline 13 to 16 days after the attack. Moving to the lower panel of Table 3, the same pattern emerges. Overall, we find an immediate positive effect on trust in one's neighbors immediately after the terrorist event, followed by a swift return to the baseline within a few days. This is consistent with the short-term effect of political violence found in related studies. Jaeger et al. (2012), for instance, also find that increased radicalization of the Palestinian population following political violence completely dissipates within 12 weeks.

Table 3 here

Figure A.1 in the appendix provides a graphical representation of the results in column (6), highlighting that respondents' level of trust *increases* immediately after the attack, but rapidly returns to initial levels. Using a quadratic local control function provides inferences similar to the local linear control function used in that figure. We also evaluated the robustness of these results by varying the bandwidth in one-day increments from 10 days to 19 days before and after the event. Figures OA.3 and OA.4 in the Online Appendix show that the size of the *PostEvent* coefficient is similar across these additional regressions (though they generally are estimated less precisely in the smaller samples). Overall, therefore, while the negative exogenous shock of the Stockholm bombings made Swedish citizens more concerned about terrorism (see Table 2 and Figure OA.1), the results in Table 3 and Figure A.2 reject the idea that it put them on a trajectory towards less trust in their fellow citizens. If anything, trust levels rose briefly in the immediate aftermath of the event. These results reject hypothesis 2a, but are

in line with hypothesis 2b.[11] Since trust is critical for resolving large-N collective action dilemmas (Søndersko, 2009) and is often seen "as a functional prerequisite for the possibility of society" (Lewis and Weigert 1985, p. 968), this finding is of substantial importance.

Before concluding, we discuss briefly the estimated coefficients on the control variables (see Table OA.3 in the Online Appendix for complete results), which indicate substantial heterogeneity in respondents' social trust. First, respondents with higher levels of educational attainment report significantly higher levels of trust. The difference is substantively meaningful as more highly educated individuals are on average 1.5 to 1.6 points higher on the trust scale than less-educated respondents (which equals 62% to 67% of the standard deviation of trust; see Table A.1). Second, compared to respondents under age 20, reported levels of trust increase fairly gradually amongst older age groups. Finally, gender and region of origin bear no significant relation to social trust in our setting. Our findings regarding the controls are similar to those reported by Glaeser et al. (2000). Using General Social Survey (GSS) data for the United States, they report that older cohorts exhibit higher trust levels than younger cohorts, a positive association between trust and education, and a negligible coefficient on respondents' sex. Given that these background characteristics might also affect the terrorism-trust relation (see above), we experimented with multilevel models allowing for different effects of terrorism on trust depending on individuals' background characteristics. While these models provide similar findings regarding the heterogeneity of trust across background characteristics, they do not indicate any evidence of treatment-effect-heterogeneity. Owing to the limited size of our sample, these auxiliary regressions clearly should be interpreted with caution, and further

[11] We also experimented with a mediation analysis whereby we included respondents' concern about terrorism as a control variable in the models presented in table 3. This strongly mitigates the effect of terrorism on trust, and suggests that terror concerns do indeed mediate the terrorism-trust relation.

investigation of such socio-demographic sources of potential heterogeneity remains an important avenue for future research whenever larger samples are available.

4. Concluding discussion

Trust enables people to cooperate when they have incomplete information about others or cannot know a priori how these others will behave (Fukuyama 1995; Sønderskov 2009). As such, it is often awarded a key role in "maintaining stable and effective democracy" (Zmerli and Newton 2008, p. 706). In this article, we exploited the timing of the 2010 Swedish 'Society Opinion Media' (SOM) survey and the 2010 Stockholm bombings to study the causal link between terrorism and individual-level social trust. Our main findings can be summarized as follows. First, we identify a strong effect of the 2010 Stockholm bombings on individuals' concerns about terrorism, which reflects a substantively meaningful change in their fear and anxiety about such events. Second, trust in others *increases* in the immediate aftermath of the event, but these changes in trust were found to be very transitory. The latter finding does suggest, however, that people coming together to express solidarity during the immediate aftermath of terrorist events can compensate for the elevated levels of fear and anxiety triggered by those events – at least in the short term.

Our research design allows us to improve on existing studies along two main dimensions. First, our regression-discontinuity model provides stronger causal inferences about the relation of interest than studies based on repeated cross-sectional surveys (e.g., Wollebaek et al. 2012, who study the effects of Anders Behring Breivik's attack in Norway) or comparative studies unable to include untreated respondents (e.g., Arvanitidis et al. 2016, who compare terrorist events in four countries). Second, our empirical model provides evidence that the effects of terrorist events on social trust dissipate within a few days – generally no longer than two weeks later. This finding provides a potential explanation for the inconclusive evidence reported in previous

studies, which look at the *average* effect of terror on trust over a longer period (Huddy et al. 2003; Gross et al. 2004). Based on our findings, such an approach overlooks important temporal variation in the immediate aftermath of terrorist events.

Our analysis naturally does have a number of limitations. First, the nature of our empirical research design restricts our analysis to the assessment of the *short-term* effects of a single attack. From a policy perspective, it arguably also is important to learn more about any potential longer-lasting effects as well as possible additive impacts of increasingly frequent terrorist events. This potential additive nature of terrorism's impact on micro-level attitudes and public opinion is an important avenue for further research. Second, our dataset did not contain separate measures of in-group and out-group trust. One possible alternative interpretation of our findings might be that terrorist events induce a readjustment of the interpretation of 'most people' in the social trust question. For instance, excluding Muslims from one's reference group for 'most people' following religiously inspired terrorist activity could generate increases in measured (in-group) trust – while missing possible reductions in out-group trust. This line of reasoning also suggests that the precise natures of terrorist events (e.g., perpetrated by extreme left-wing, extreme right-wing or religious fundamentalist groups) may influence the terrorism-trust relation. Hence, future research should verify our results' dependence on the background of terrorist events. Finally, since our analysis concentrates on one country, one might wonder about the sensitivity of our findings to the specific Swedish setting. Sweden might have a particular sensitivity to terrorist events owing to the high-profile murders of its Prime Minister Olof Palme in 1986 and foreign minister Anna Lindh in 2003. From this perspective, it is important to note that the sizes of the effects we observe in individuals' terror concerns are broadly in line with earlier findings for other European countries. Even so, further verification of our findings across different settings remains necessary.

Acknowledgements

We thank the editor, three anonymous referees, Gianmarco Daniele, Jon Fiva, Joshua Holm, Paul Huth and Zuzana Murdoch for helpful comments and suggestions. The first author gratefully acknowledges FWO Vlaanderen (grant number G.0022.12) for financial support. This article was conceived while the second author was visiting Norwegian Business School BI, and he is grateful for their hospitality, and to the E.On Ruhrgas Stipend programme of the Research Council of Norway for financial support.

Appendix

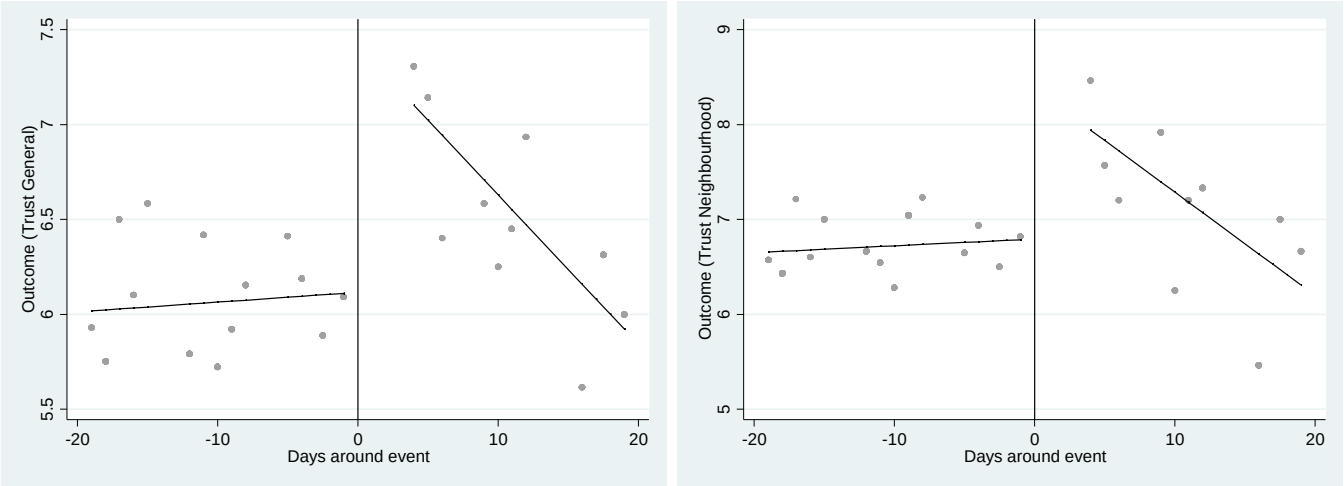
Table A1: Summary statistics for the main sample

	Obs	Mean	Std. Dev.	Min	Max
Terrorism concern	273	2.01	0.87	1	4
General trust	417	6.20	2.38	0	10
Trust in neighbors	414	6.80	2.37	0	10
Concern about global epidemics	146	2.42	0.84	1	4
Concern about weakened democracy	124	2.39	0.86	1	4
Concern about environmental degradation	274	1.62	0.69	1	4
Days	458	-2.91	11.11	-19	19
Control variables					
East Mid-Sweden	458	0.14	0.35	0	1
Smaland province and islands	458	0.09	0.29	0	1
South Sweden	458	0.19	0.40	0	1
West Sweden	458	0.21	0.41	0	1
North Mid-Sweden	458	0.08	0.27	0	1
Mid-North Sweden	458	0.02	0.15	0	1
Upper-North Sweden	458	0.06	0.23	0	1
Age 20-29	458	0.17	0.38	0	1
Age 30-39	458	0.20	0.40	0	1
Age 40-49	458	0.22	0.41	0	1
Age 50-59	458	0.17	0.37	0	1
Age 60-69	458	0.08	0.27	0	1
Age 70-79	458	0.05	0.21	0	1
Age 80-	458	0.01	0.11	0	1
Medium education	433	0.51	0.50	0	1
High education	433	0.33	0.47	0	1
Male	458	0.50	0.50	0	1

Table A2: Effect on respondents' concern about terrorism (extended table including controls)

	Panel I: Baseline findings				
	(1)	(2)	(3)	(4)	(5)
Constant	2.320***	2.528***	2.479***	1.962***	2.149***
	(0.087)	(0.240)	(0.268)	(0.016)	(0.069)
PostEvent	-0.394***	-0.463***	-0.509*	-0.195***	-0.355**
	(0.055)	(0.102)	(0.218)	(0.057)	(0.107)
Days	-	-	-0.004	-	-
			(0.012)		
*PostEvent * Days*	-	-	0.013	-	-
			(0.019)		
	Control variables				
East Mid-Sweden	-0.102*	0.309	0.318		
	(0.048)	(0.170)	(0.171)		
Smaland province	-0.146*	-0.181	-0.177		
and islands	(0.059)	(0.194)	(0.195)		
South Sweden	-0.157**	0.039	0.055		
	(0.051)	(0.160)	(0.162)		
West Sweden	-0.137**	-0.180	-0.171		
	(0.0464)	(0.155)	(0.157)		
North Mid-Sweden	-0.131*	-0.146	-0.139		
	(0.058)	(0.203)	(0.204)		
Mid-North Sweden	-0.037	0.656	0.680		
	(0.078)	(0.373)	(0.376)		
Upper-North	0.110	0.530*	0.539*		
Sweden	(0.067)	(0.232)	(0.236)		
Age 20-29	0.000	-0.298	-0.304		
	(0.081)	(0.204)	(0.205)		
Age 30-39	-0.214**	-0.284	-0.288		
	(0.0787)	(0.198)	(0.200)		
Age 40-49	-0.377***	-0.670***	-0.682***		
	(0.0755)	(0.200)	(0.201)		
Age 50-59	-0.565***	-0.685***	-0.697***		
	(0.0760)	(0.205)	(0.207)		
Age 60-69	-0.740***	-0.660**	-0.664**		
	(0.0761)	(0.244)	(0.245)		
Age 70-79	-0.844***	-1.276***	-1.283***		
	(0.0808)	(0.329)	(0.331)		
Age 80-	-0.916***	-1.837*	-1.862*		
	(0.107)	(0.832)	(0.838)		
Medium education	0.001	-0.061	-0.053		
	(0.042)	(0.157)	(0.158)		
High education	0.163***	-0.116	-0.111		
	(0.0440)	(0.167)	(0.168)		
Male	0.335***	0.362***	0.361***		
	(0.0293)	(0.102)	(0.102)		
Observations	3130	261	261	3130	261

Figure A.1: Graphical representation of effect on trust variables



Note: Dependent variable is the level of trust in people in general (left panel) and one's neighbours (right panel). In both panels, we analyse 11 December 2010 as the event date and an event window of nineteen days before/after the Stockholm bombings (excluding observations from 13-14 December due to uncertainty regarding the moment of these surveys' completion). Using a quadratic local control functions provides similar inferences than the local linear control function shown here.

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Table 1: Balancing tests for the main samples

Bandwidth	10	11	12	13	14
N	154	184	232	273	273
chi^2	20.15	20.65	14.96	14.69	14.69
P	0.166	0.149	0.527	0.547	0.547
Bandwidth	15	16	17	18	19
N	273	285	313	338	376
chi^2	14.69	13.43	14.48	16.81	14.75
P	0.547	0.641	0.633	0.467	0.614

Note: Dependent variable is a dummy variable that is equal to 1 if the individual responded after December 11 and 0 if the individual responded before December 11. The first model is run for a sample where observations on *Days* ranging from -10 and 10 are included, while the last model is run on our main sample where *Days* runs between -19 and 19. In all models, the null hypothesis is that no correlation exists between answering the survey before/after December 11 and observables.

Table 2: Effect on respondents' concerns about terrorism

	Panel I: Baseline findings				
	(1)	(2)	(3)	(4)	(5)
PostEvent	-0.394***	-0.463***	-0.509*	-0.195***	-0.355**
	(0.055)	(0.102)	(0.218)	(0.057)	(0.107)
Days			-0.004		
			(0.012)		
*PostEvent * Days*			0.013		
			(0.019)		
Constant	2.320***	2.528***	2.479***	1.962***	2.149***
	(0.087)	(0.240)	(0.268)	(0.016)	(0.069)
Observations	3130	261	261	3130	261

	Panel II: Excluding 13-14 December			
	(6)	(7)	(8)	(9)
PostEvent	-0.478***	-0.570*	-0.470***	-0.544*
	(0.103)	(0.234)	(0.107)	(0.273)
Days		-0.006		-0.006
		(0.012)		(0.012)
*PostEvent * Days*		0.020		0.018
		(0.019)		(0.022)
Constant	2.575***	2.503***	2.551***	2.489***
	(0.240)	(0.267)	(0.243)	(0.271)
Observations	252	252	242	242

Note: Dependent variable is the level of concern over terrorism on scale ranging from 1 ("major concern") to 4 ("no concern"). In all models, we analyse 11 December 2010 as the event date. Column (1) uses the entire available sample, while columns (2) and (3) look at an event window of nineteen days before/after the Stockholm bombings. Controls for sex, age, education level and region of origin are included in columns (1) to (3) and (6) to (9), while columns (4) and (5) replicate the analysis in columns (1) and (2) when omitting any control variables. Panel II replicates columns (2) and (3) while excluding observations from 13 December (columns (6) and (7)) or from 13 and 14 December (columns (8) and (9)). Standard errors in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table 3: Effect on respondents' level of trust

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel I: Trust in most people			
PostEvent	0.337	0.515	0.405^{+}	0.893^{+}	0.405	1.200^{+}
	(0.239)	(0.492)	(0.245)	(0.534)	(0.256)	(0.618)
Days		0.015		0.013		0.0131
		(0.026)		(0.026)		(0.0262)
*PostEvent * Days*		-0.049		-0.073		-0.0947^{+}
		(0.043)		(0.045)		(0.0500
Constant	4.498***	4.666***	4.421***	4.600***	4.458***	4.653***
	(0.580)	(0.637)	(0.586)	(0.641)	(0.601)	(0.655)
Observations	408	408	395	395	380	380
			Panel II: Trust in neighbours			
PostEvent	0.213	0.214	0.330	0.788	0.364	1.285*
	(0.237)	(0.487)	(0.239)	(0.520)	(0.248)	(0.598)
Days		0.0227		0.0216		0.0209
		(0.0261)		(0.0256)		(0.0255)
*PostEvent * Days*		-0.0471		-0.0868^{+}		-0.121*
		(0.0429)		(0.0444)		(0.0484)
Constant	4.934***	5.177***	4.870***	5.141***	4.853***	5.144***
	(0.575)	(0.633)	(0.573)	(0.627)	(0.585)	(0.636)
Observations	405	405	392	392	377	377

Note: Dependent variable is the level of trust in people in general (Panel I) and one's neighbours (Panel II). In all cases, higher numbers thereby refer to higher levels of trust. In all columns, we analyse 11 December 2010 as the event date and look at an event window of nineteen days before/after the Stockholm bombings. Columns (1) and (2) include all observations within the observation window, while Columns (3) and (4) exclude observations from 13 December and columns (5) and (6) exclude observations from 13 and 14 December due to some uncertainty regarding the moment of these surveys' postage. Standard errors in parentheses; $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001; Controls for sex, age, education level and region of origin included in all models.

Online Appendix to

Will You Still Trust Me Tomorrow?
The Causal Effect of Terrorism on Social Trust

Robustness analysis

To credibly attribute the attitudinal shifts documented in tables 2 and 3 to the Stockholm bombings, we should only observe strong effects for concerns about issues that undergo a sudden shift due to this event (such as concern over terrorism). Concerns about issues unrelated to the Stockholm bombings should *not* be similarly affected. In table OA.1, we therefore present results analysing individuals' concerns about, respectively, environmental degradation (panel I), global epidemics (panel II) and weakened democracy (panel III) as the dependent variables (again on a 4-point scale with higher values reflecting lower concern). Since none of these results reveal significant coefficient estimates for the *PostEvent* dummy when assessing a narrow 39-day time period around the event, this rules out that the Stockholm bombings affected Swedes' concern about *everything* in the aftermath of this event (for a graphical representation, see figure OA.2).[12]

Table OA.2 implements a further set of placebo tests in which we code the central independent variable as reflecting non-existing events, which allows us to assess whether the effects observed in table 2 in the main text are really driven by the Stockholm bombings rather than representing a statistical artefact. In columns (1) and (2), we take 11 November 2010 rather than 11 December 2010 as the event date. Furthermore, as our post-event period lies close to the Christmas holidays, one might worry that the Christmas holidays influence the treatment effect due to a change in media coverage in the days close to Christmas. Columns (3) and (4) in table OA.2 therefore take 11 December 2009 as the event date, while columns (5) and (6) look at 11 December 2011. In both cases, implementing a regression discontinuity around a non-existing event date generates insignificant findings, which provides further corroborating evidence for the idea that the effects observed in table 2 derive from the Stockholm bombings. Note that we show 2009 and 2011 merely for illustrative purposes. Similar non-significant results are obtained when using 11 December in any year between 2001 and 2011 as the event date (except, of course, 11 December 2010).

[12] One might argue that we test for treatment effects on a range of outcomes using the same data, which requires a correction of the critical values employed for the evaluation of statistical significance using, for instance, Bonferroni-corrections (Gerber and Green, 2012). Still, such corrections typically induce an upward adjustment of the estimated standard errors, and thus would not affect the *positive* nature of the observed effects for our measures of social trust in the main text. Furthermore, the effect of the Stockholm bombings on terrorism concerns in table 1 in the main text is statistically significant beyond the 99.9% confidence level, and would retain statistical significance at conventional levels also after an upward adjustment of the standard errors.

Table OA.4 revisits the balance tests reported in the main text. It presents a series of difference in means t-tests comparing population subsets before and after the event. The small sample size requires combining individuals aged 70 and over in one group, as well as all respondents from the northern parts of Sweden (i.e. North Mid-Sweden, Mid-North Sweden and Upper-North Sweden). The results highlight the balanced nature of our sample on the four reported background characteristics. Indeed, the few indications of lack of balance when using the very narrow 10-day event window are due to the low number of observations in this sample (making population shares very sensitive to one more or less respondent in a given subgroup), and disappear when using the larger 19-day window.

Finally, the choice of the event window may have an important impact on the results in regression discontinuity designs. Figures OA.3, OA.4 and OA.5 therefor verify that our results are not driven by the specific 19-day window employed throughout the analysis in the main text. These figures illustrate that our central inferences are robust to different event windows between 10 and 19 days.

References

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Table OA.1: Placebo checks on concerns for the environment, global epidemics and democracy.

	(1)	(2)	(3)	(4)
	Panel I: Concern about environmental degradation			
PostEvent	-0.063	-0.037	-0.041	0.079
	(0.088)	(0.186)	(0.094)	(0.236)
Days		-0.003		-0.004
		(0.010)		(0.010)
*PostEvent * Days*		0.004		-0.002
		(0.016)		(0.019)
Constant	1.752***	1.721***	1.825***	1.786***
	(0.210)	(0.234)	(0.214)	(0.238)
Observations	264	264	243	243
	Panel II: Concern about global epidemics			
PostEvent	-0.187	0.077	-0.167	0.277
	(0.150)	(0.325)	(0.165)	(0.438)
Days		-0.023		-0.022
		(0.020)		(0.019)
*PostEvent * Days*		0.019		0.003
		(0.030)		(0.037)
Constant	2.427***	2.174***	2.413***	2.154***
	(0.330)	(0.394)	(0.352)	(0.416)
Observations	141	141	128	128
	Panel III: Concern about weakened democracy			
PostEvent	0.216	0.433	0.140	0.167
	(0.175)	(0.365)	(0.177)	(0.432)
Days		-0.002		-0.003
		(0.018)		(0.018)
*PostEvent * Days*		-0.016		0.004
		(0.028)		(0.032)
Constant	2.911***	2.928***	2.880***	2.848***
	(0.448)	(0.491)	(0.438)	(0.482)
Observations	119	119	111	111

Note: Dependent variable is the level of concern about environmental degradation (Panel I), concern about global epidemics (Panel II) and concern about weakened democracy (Panel III) ranging from 1 ("major concern") to 4 ("no concern"). In all columns, we analyse 11 December 2010 as the event date and look at an event window of nineteen days before/after the Stockholm bombings. Columns (1) and (2) include all observations within the observation window, while Columns (3) and (4) exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' postage. Standard errors in parentheses; * p<0.05, ** p<0.01, *** p<0.001; Controls for sex, age, education level and region of origin included in all models.

Table OA.2: Placebo checks using non-existing event dates

	(1) Concern about terrorism	(2) Concern about terrorism	(3) Concern about terrorism	(4) Concern about terrorism	(5) Concern about terrorism	(6) Concern about terrorism
PostEvent	0.079	0.130	0.166	0.046	-0.140	0.377
	(0.079)	(0.155)	(0.139)	(0.297)	(0.142)	(0.295)
Days		-0.008		-0.005		-0.019
		(0.006)		(0.014)		(0.015)
*PostEvent * Days*		0.010		0.030		-0.015
		(0.013)		(0.031)		(0.025)
Constant	2.193***	2.105***	2.888***	2.834***	2.416***	2.135***
	(0.171)	(0.187)	(0.265)	(0.310)	(0.317)	(0.391)
Observations	588	588	216	216	180	180

Note: Dependent variable is the level of concern over terrorism on scale ranging from 1 ("major concern") to 4 ("no concern"). In columns (1) and (2), we analyse 11 November 2010 as the event date, while in columns (3) and (4) we analyse 11 December 2009 as the event date. In columns (5) and (6), we analyse 11 December 2011 as the event date. Standard errors in parentheses; * p<0.05, ** p<0.01, *** p<0.001; Controls for sex, age, education level and region of origin included in all models.

Table OA.3: Results for generalized trust (extended table including controls)

	(1)	(2)	(3)	(4)	(5)	(6)
PostEvent	0.337	0.515	0.405^{+}	0.893^{+}	0.405	1.200^{+}
	(0.239)	(0.492)	(0.245)	(0.534)	(0.256)	(0.618)
Days		0.0147		0.0131		0.0131
		(0.0262)		(0.0261)		(0.0262)
*PostEvent * Days*		-0.0487		-0.0727		-0.0947^{+}
		(0.0432)		(0.0455)		(0.0500)
Constant	4.498***	4.666***	4.421***	4.600***	4.458***	4.653***
	(0.580)	(0.637)	(0.586)	(0.641)	(0.601)	(0.655)
East Mid-Sweden	0.0998	0.0726	0.0590	0.0118	-0.00889	-0.0688
	(0.394)	(0.396)	(0.399)	(0.400)	(0.408)	(0.408)
Smaland province and islands	0.00200	0.00516	-0.0721	-0.0762	-0.128	-0.132
	(0.477)	(0.477)	(0.482)	(0.482)	(0.493)	(0.492)
South Sweden	-0.556	-0.590	-0.460	-0.516	-0.561	-0.588
	(0.369)	(0.370)	(0.372)	(0.373)	(0.387)	(0.387)
West Sweden	-0.208	-0.222	-0.238	-0.270	-0.271	-0.325
	(0.365)	(0.366)	(0.367)	(0.367)	(0.373)	(0.373)
North Mid-Sweden	0.175	0.142	0.294	0.252	0.217	0.181
	(0.497)	(0.498)	(0.501)	(0.501)	(0.514)	(0.513)
Mid-North Sweden	0.945	0.852	0.886	0.781	0.777	0.709
	(0.819)	(0.824)	(0.858)	(0.860)	(0.920)	(0.920)
Upper-North Sweden	-0.207	-0.224	-0.197	-0.225	-0.245	-0.316
	(0.533)	(0.538)	(0.550)	(0.553)	(0.555)	(0.558)
Age 20-29	-0.107	-0.0884	-0.150	-0.129	-0.0593	-0.0548
	(0.474)	(0.476)	(0.484)	(0.484)	(0.502)	(0.502)
Age 30-39	0.600	0.595	0.570	0.566	0.588	0.563
	(0.473)	(0.473)	(0.485)	(0.485)	(0.504)	(0.503)
Age 40-49	0.760	0.789^{+}	0.734	0.765	0.759	0.779
	(0.465)	(0.466)	(0.471)	(0.471)	(0.488)	(0.487)
Age 50-59	1.232**	1.251**	1.162*	1.161*	1.095*	1.081*
	(0.472)	(0.473)	(0.477)	(0.478)	(0.495)	(0.495)
Age 60-69	0.572	0.569	0.489	0.475	0.505	0.449
	(0.595)	(0.596)	(0.606)	(0.606)	(0.618)	(0.618)
Age 70-79	1.428*	1.533*	1.386*	1.508*	1.413*	1.525*
	(0.694)	(0.703)	(0.696)	(0.703)	(0.709)	(0.714)
Age 80-	0.605	0.665	0.609	0.684	0.617	0.685
	(1.123)	(1.125)	(1.120)	(1.120)	(1.132)	(1.129)
Medium education	1.001**	0.979**	1.124**	1.099**	1.135**	1.112**
	(0.362)	(0.363)	(0.365)	(0.365)	(0.371)	(0.370)
High education	1.534***	1.530***	1.596***	1.580***	1.636***	1.621***
	(0.386)	(0.387)	(0.390)	(0.390)	(0.397)	(0.397)
Male	0.138	0.141	0.181	0.170	0.145	0.133
	(0.234)	(0.234)	(0.237)	(0.237)	(0.243)	(0.243)
Observations	408	408	395	395	380	380

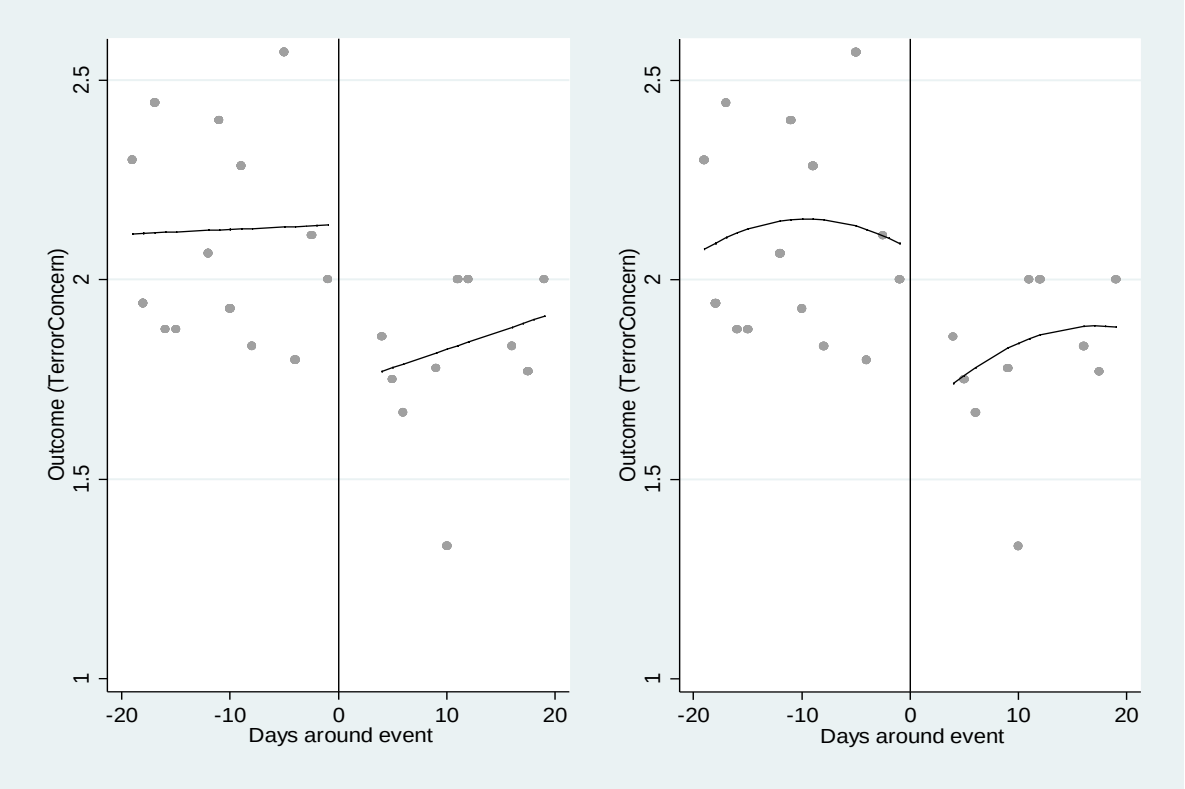
Note: This table is and extended version of the upper panel (Panel I) of Table 3 in the paper and shows all coefficients.

Table OA.4: Balancing t-tests for two main samples

	Bandwidth 10 Days			**Bandwidth 19 Days**		
	Before	After	t-test on difference	Before	After	t-test on difference
Female	0.536	0.455	1.038	0.519	0.459	1.146
Age 16-19	0.099	0.091	0.180	0.093	0.081	0.400
Age 20-29	0.146	0.200	0.938	0.159	0.200	1.036
Age 30-39	0.225	0.255	0.439	0.190	0.207	0.412
Age 40-49	0.232	0.127	1.650	0.228	0.215	0.311
Age 50-59	0.159	0.218	0.988	0.163	0.185	0.575
Age 60-69	0.093	0.091	0.039	0.097	0.052	1.571
Age 70-	0.046	0.018	0.923	0.069	0.059	0.383
Low education	0.159	0.096	1.111	0.168	0.138	0.770
Medium education	0.551	0.500	0.623	0.513	0.485	0.528
High education	0.290	0.404	1.502	0.319	0.377	1.155
Stockholm	0.212	0.164	0.765	0.204	0.230	0.597
East Mid-Sweden	0.132	0.218	1.503	0.135	0.162	0.764
Smaland province and islands	0.079	0.109	0.664	0.083	0.104	0.693
South Sweden	0.212	0.109	1.686 *	0.197	0.156	1.031
West Sweden	0.185	0.309	1.908 *	0.208	0.230	0.513
North Sweden	0.179	0.091	1.542	0.173	0.119	1.442
N	52-55	138-151		130-135	273-289	

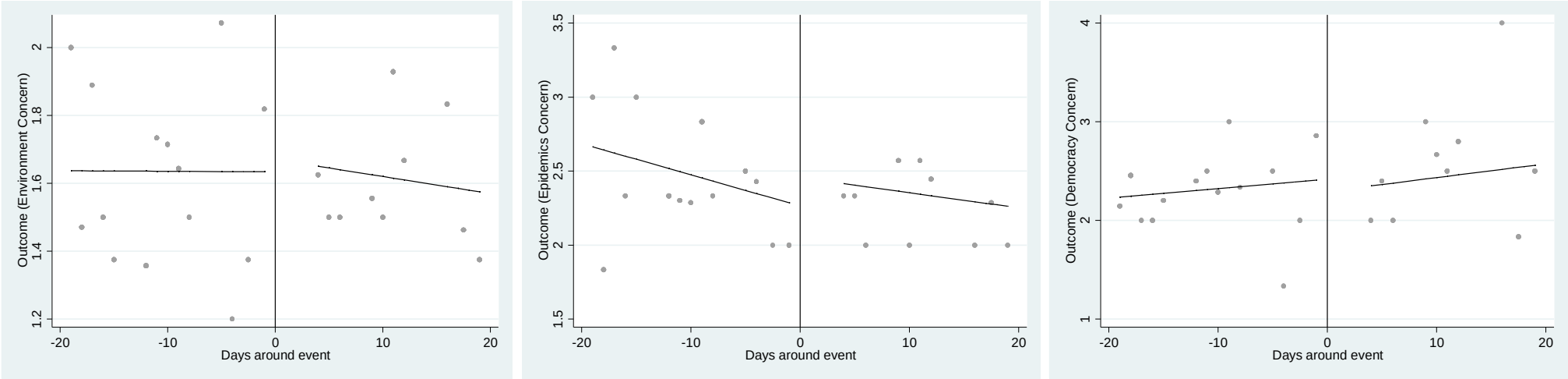
Note: 'Before' and 'After' refer to whether an individual responded before or after December 11. The first three columns look observations where *Days* ranges from -10 and 10, while the last three columns include our main sample where *Days* runs between -19 and 19. Columns (1), (2), (4) and (5) report the share of specific subsets of respondents in the data, while columns (3) and (6) present t-tests for the difference between the shares reported in the previous two columns. To avoid issues with very small sample sizes in certain population subsets, we combine individuals aged 70 and over in one group, as well as all respondents from northern parts of Sweden (i.e. North Mid-Sweden, Mid-North Sweden and Upper-North Sweden). N varies slightly due to missing values for some respondents' education levels.

Figure OA.1: Main effect on concern about terrorism for different polynomials



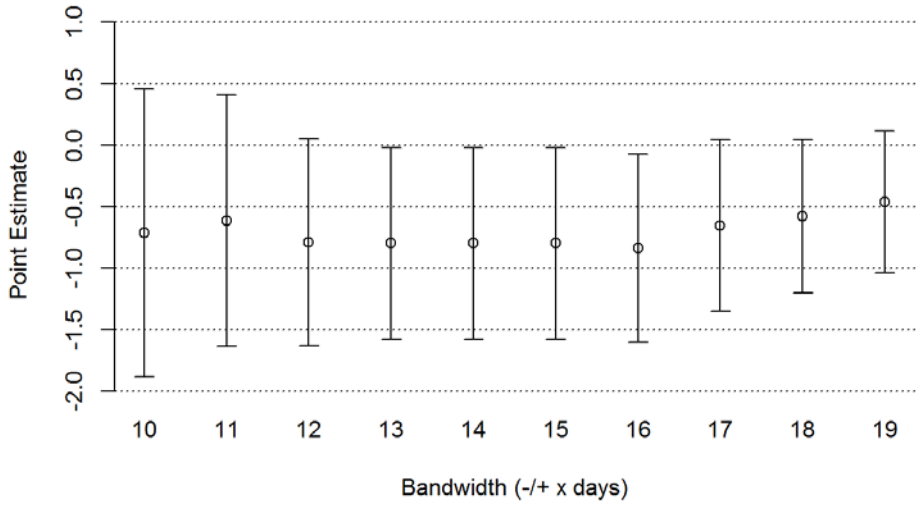
Note: Dependent variable is the level of concern over terrorism on scale ranging from 1 ("major concern") to 4 ("no concern"). In all panels, we analyse 11 December 2010 as the event date and an event window of nineteen days before/after the Stockholm bombings. In all cases, we exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' completion. The panels differ only in terms of the polynomial order of the running variable (*Days*), with a polynomial of order 1 on the left-hand side and order 2 on the right-hand side.

Figure OA.2: Effect on concern about environment, global epidemics and weakened democracy



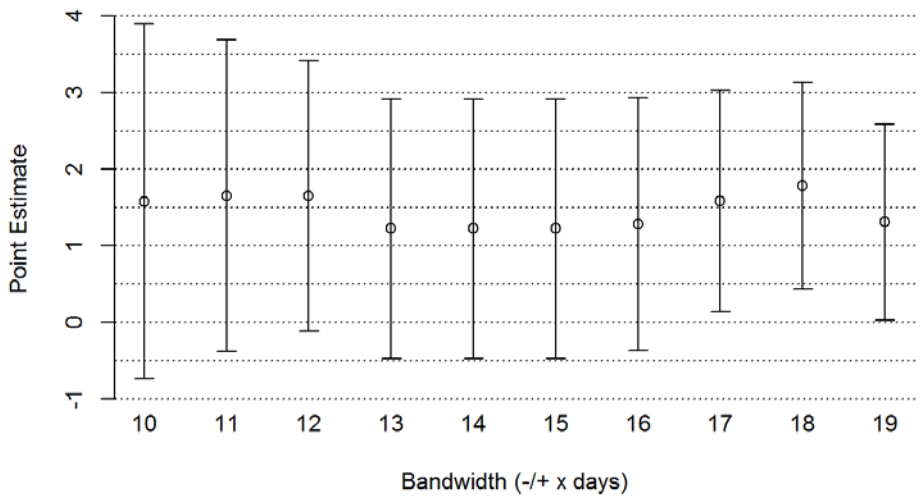
Note: Dependent variable is the level of concern about environmental degradation (left panel), concern about global epidemics (centre panel) and concern about weakened democracy (right panel) ranging from 1 ("major concern") to 4 ("no concern"). In all panels, we analyse 11 December 2010 as the event date and an event window of nineteen days before/after the Stockholm bombings. In all cases, we exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' completion. Using a quadratic local control functions provides similar inferences than the local linear control function shown here.

Figure OA.3: Robustness to event windows: Concern over terrorism



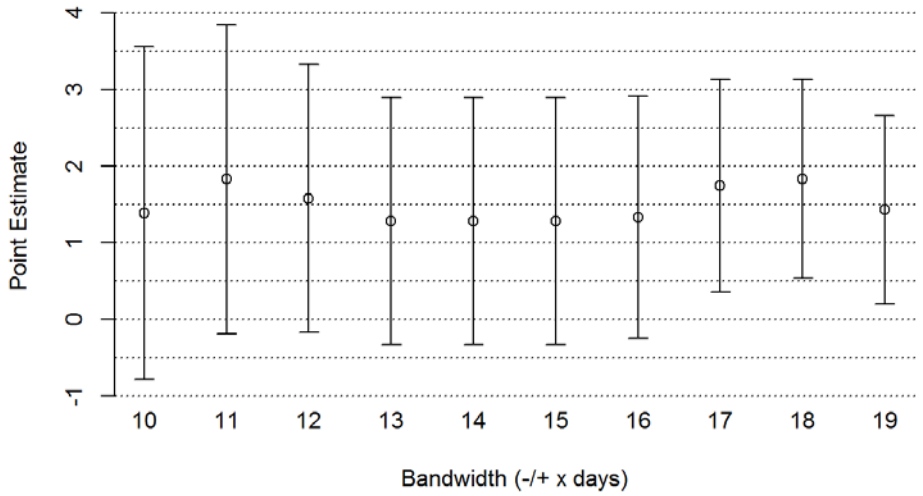
Note: Dependent variable is the level of concern over terrorism on scale ranging from 1 ("major concern") to 4 ("no concern"). In all regressions, we analyse 11 December 2010 as the event date, and only vary the event window from ten days before/after the Stockholm bombings (i.e. half the window size employed in table 2) up to nineteen days before/after the attack (as in table 2). In all cases, we exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' completion. Controls for sex, age, education level and region of origin included in all models. The bars below and above the point estimates show 95 percent confidence intervals.

Figure OA.4: Robustness to event windows: General Trust



Note: Dependent variable is the level of general trust ranging from 0 ("people cannot generally be trusted") to 10 ("people can generally be trusted"). In all regressions, we analyse 11 December 2010 as the event date, and only vary the event window from ten days before/after the Stockholm bombings (i.e. half the window size employed in table 1) up to nineteen days before/after the attack (as in table 1). In all cases, we exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' completion. Controls for sex, age, education level and region of origin included in all models. The bars below and above the point estimates show 95 percent confidence intervals.

Figure OA.5: Robustness to event windows: Trust in neighbours



Note: Dependent variable is the level of trust in neighbours ranging from 0 ("people cannot generally be trusted") to 10 ("people can generally be trusted"). In all regressions, we analyse 11 December 2010 as the event date, and only vary the event window from ten days before/after the Stockholm bombings (i.e. half the window size employed in table 1) up to nineteen days before/after the attack (as in table 1). In all cases, we exclude observations from 13-14 December due to some uncertainty regarding the moment of these surveys' completion. Controls for sex, age, education level and region of origin included in all models. The bars below and above the point estimates show 95 percent confidence intervals.