Terrorist Events and Attitudes toward Immigrants: A Natural Experiment[1]

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Using a quasi-experimental research design, this study examines the effect of terrorist events on the perception of immigrants across 65 regions in nine European countries. It first elaborates a theoretical argument that explains the effect of events and points to economic conditions, the size of the immigrant population, and personal contact as mediating factors. This argument is evaluated using the fact that the terror attack in Bali on October 12, 2002, occurred during the fieldwork period of the European Social Survey. The findings from this natural experiment reveal considerable cross-national and regional variation in the effect of the event and its temporal duration. The analysis on the regional level supports the argument about contextual variations in the response to the event and a second analysis based on the 2004 Madrid bombing confirms the study's conclusions. Implications of the findings for societal responses to terror attacks, the literature on attitudes toward immigrants, and survey research are discussed.

INTRODUCTION

On October 12, 2002, Indonesia and the world witnessed one of the deadliest terror attacks on civilian life in recent decades. Around 11:00 p.m., a

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terrorist entered the nightclub Paddy's Pub in Kuta on the Indonesian island of Bali and ignited his explosive-laden backpack, triggering a wave of fear and causing guests to pour out the onto street. The safety of the open pavement would only last a few seconds, however; a powerful car bomb was soon detonated by another suicide bomber on the crowded street in front of the nightclub.

With a death toll of 202—many of the casualties European tourists—the terror attack in Bali appeared on the front pages of newspapers around the world. Since the attacks of September 11, 2001, acts of international terrorism committed by groups purporting to speak in the name of Islam have been important in public discourse and have precipitated a wave of discriminatory acts against Muslims in Western societies (Allen and Nielsen 2002; Human Rights Watch 2002). At the same time, research across European countries consistently finds negative sentiments toward immigrants in general and reveals that immigrants are often "viewed as a threat to economic success, to national identity, and to the social order" (Semyonov, Raijman, and Gorodzeisky 2006, p. 432; also see Ceobanu and Escandell 2010). Yet, the link between specific events and public attitudes toward immigrants is mostly anecdotal. Current research remains relatively silent on why, how, and under which circumstances an event may affect the perception of an out-group. No thorough empirical investigation has been undertaken to estimate the causal effect of events on attitudes toward immigrants across social contexts. The fact that the terror attack in Bali coincided with the fieldwork period of nine countries in the European Social Survey (ESS) provides a unique opportunity to fill this gap.

To that end, this article begins with an elaboration of a theoretical mechanism that shows why and how events affect attitudes toward an out-group and elucidates the conditions under which this is likely to occur. In particular, I draw on group-threat and intergroup contact theory to argue that events such as terror attacks negatively affect attitudes toward immigrants. They foster the perception of an out-group as threatening and direct attention toward potential sources of intergroup conflict, such as the relative size of the out-group and economic conditions. The experiences and information from direct contact with members of the out-group, however, mitigate the role of the out-group size and reduce the effect of the event itself. Accordingly, the response to events should be more pronounced in regions with an increasing unemployment rate, while out-group size and direct contact with immigrants interact in shaping the response to events. To evaluate this argument, I take advantage of the timing of the ESS interviews of 2002 to design a quasi experiment that allows me to study the impact of a single event on attitudes toward immigrants in 65 regions from nine countries. Specifically, this study uses the fact that the terror attack in Bali coincided with the interview period of the

ESS in Belgium, Finland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and Great Britain. Using the event as a source of exogenous variation, I define those respondents interviewed before the attack as the control group, and those interviewed after the attack serve as the treatment group. In this fashion, I aim to estimate the Bali attack's causal effect on anti-immigrant sentiments across 65 regions from nine countries. The findings from this natural experiment reveal considerable cross-national and regional variation in both the magnitude of the causal effect and its temporal duration. In two or—less clearly—three out of the nine countries for which the fieldwork period coincided with the terror attack in Bali (Portugal, Poland, and Finland), the estimated causal effect is highly significant and substantial. Supporting the argument about contextual variations, the analysis on the regional level reveals that the variations in the response to the event are driven by the local unemployment rate, the size of the out-group population, and personal contact with immigrants. A second case study based on the March 2004 terror attack in Madrid and Eurobarometer data replicates important aspects of these findings and thereby reaffirms my conclusions (this second case study is part of app. A, but the results are discussed throughout the article).

The current study uses this unique opportunity to contribute to several areas of research: First, in examining the impact of the attacks on the relation between the majority population and immigrants in European countries, the study contributes to the empirical understanding of terrorism's effects and specifies the circumstances under which events such as terrorist attacks increase anti-immigrant sentiment (Spilerman and Stecklov 2009). Second, it makes a theoretical contribution to the literature on attitudes toward immigrants and group-threat theory by elaborating a mechanism for short-term and potentially long-term changes in the perception of an out-group. Finally, from a methodological perspective, the exploitation of exogenous events together with the variation in the timing of the interviews in the ESS demonstrates how the temporal embeddedness of events and survey responses can provide researchers with analytic leverage. It also reveals a potential source of bias in survey research.

THE PERCEPTION OF IMMIGRANTS AND TERRORIST EVENTS: THEORY AND RESEARCH

Public attitudes toward immigrants have been an important and extensively studied area of research. This research consistently finds negative sentiments toward immigrants across Europe, the United States, and other countries (Ceobanu and Escandell 2010). Contact and group-threat theory, the two most prominent approaches in this literature, focus on individual-level factors such as contact with out-group members or contextual-level factors such as out-group size and economic conditions (Quillian 1995; Ceobanu and Escandell 2010). In the broader literature, however, events such as riots, attacks, or homicides play an important role, and many recognize that they have the potential to shape attitudes toward out-groups. Along these lines, a number of studies explore how race riots in the second half of the 20th century have shaped race relations and public opinion. Bobo et al. (1994), for example, rely on a Los Angeles survey with a fieldwork period that coincides with the verdict in the Rodney King beating and the following 1992 Los Angeles riot to explore changes in public opinion among different racial groups. The most pronounced findings are increased negative attitudes among African-Americans about the future of race equality in the United States as well as a shift among Asians toward more negative stereotypes about blacks (for another study on race riots, see Bellisfield [1972]). Other studies have focused on different events such as military interventions or ad campaigns. Using data on complaints filed in Belgium and a simple time-series design, Jacobs et al. (2011) study whether the conflict in Gaza influenced anti-Semitism. Their results show that complaints about anti-Semitism increased during the Israeli military operation Cast Lead but abated several weeks after the operation. A number of small-scale studies based on specific subpopulations (high school or college students) also examine the effect of terror attacks on attitudes toward immigrants and at least partially confirm such an effect.[2] The most convincing evidence can be found in Hopkins's (2010) recent work. He relies on a panel survey conducted in fall 2000, October 2001, and March 2002 to show that 9/11 had a profound short-term impact on attitudes toward immigrants, which abated by March 2002.

This research shows that events have the potential to shape attitudes toward out-groups. Yet, little is known about why, how, and under which circumstances such events matter. In this study, I use a quasi-experimental research design based on large-scale surveys from nine countries and their subregions to extend this line of research and examine the circumstances under which events such as terrorist attacks increase anti-immigrant sentiment. For this purpose, I draw on group-threat and intergroup contact theory

[2] Boomgaarden and de Vreese (2007) use a small online survey of college students studying social sciences as quasi-experimental data to investigate the impact of Theo van Gogh's assassination on November 2, 2004, in the Netherlands on attitudes toward immigrants. Bar-Tal and Labin (2001) and Echebarria-Echabe and Fernández-Guede (2006) conduct similar small-scale studies. The latter looks at the effect of the March 11, 2004, terror attack in Madrid on authoritarianism, anti-Semite, and anti-Arab attitudes in Spain. The former considers the effect of a terror attack, carried out by Palestinian extremists, on stereotypes toward Palestinians, Jordanians, and Arabs among adolescents in Israel. A number of other studies also examine the effect of events using only postevent data and occasionally compare their results with other data sources from prior studies (Noelle-Neumann 2002; Traugott et al. 2002; Hitlan et al. 2007).

to describe a mechanism that shows why and under which conditions events such as terror attacks might affect attitudes toward immigrants (or more generally, an out-group).

Group-Threat and Intergroup Contact Theory

Group-threat and intergroup contact theory have been two of the most prominent approaches in the literature on both racial and anti-immigrant attitudes. Group-threat theory postulates that negative attitudes toward an out-group arise when outsiders are perceived as a threat to the privileges of the dominant group (Quillian 1995; for a classical formulation of the theory, see Blumer [1958]). Blalock (1967) explicates this framework and argues that perceived threats emerge from competition over scarce resources and foster negative attitudes toward the out-group. As outlined by Schlueter and Scheepers (2010), this argument involves two steps: First, actual or, as some argue, imagined competition between groups over scarce resources provokes a perception of the out-group as a threat to economic and cultural privileges. Second, the perception of threat, in turn, feeds negative sentiments toward the out-group. Accordingly, the perception of threat mediates the relationship between intergroup conflict over scarce resources and anti-out-group attitudes. The argument applies both to economic interests such as jobs in the labor market or access to the housing market as well as to nonmaterial issues such as "fears that immigrants could alter the prevailing way of life or the foundation of national identity" (Ceobanu and Escandell 2010, p. 318; also see Blumer 1958; Bobo 1999).

The most commonly used indicators of intergroup conflict or sources of perceived threat are the relative size of the subordinate group and the economic situation (Blalock 1967; Quillian 1995; Oliver and Mendelberg 2000; Meuleman, Davidov, and Billiet 2009, p. 589). Recent strands of group-threat theory and research from other areas also suggest a dynamic formulation of this theoretical framework. According to recent work by Meuleman et al. (2009), Hopkins (2010), and others, it is not so much the current size of an out-group population or the current economic situation that matters but, rather, changes in these two factors.

Contact theory, in contrast, generally posits that intergroup contact facilitates intergroup relations by improving attitudes toward the out-group and by reducing stereotypes (Pettigrew 1998). Allport's (1954) classical formulation of the theory and many subsequent studies focus on situational factors such as equal status, common goals, and cooperation as conditions for the positive effect of intergroup interactions. More recent empirical work, however, reports a positive effect of direct contact with out-group members, even in the absence of these conditions (Pettigrew 1998, p. 68). Accordingly, the core premise of the theory that direct contact through close friends, coworkers, or even everyday encounters mitigates negative sentiments and stereotypes toward an out-group is generally supported by empirical work. This individual-level finding is often used to derive a contextual-level hypothesis that postulates a positive effect of the relative size of the out-group, which seemingly contradicts the argument based on group-threat theory. Dixon (2006) and, more explicitly, Schlueter and Scheepers (2010; also see Savelkoul et al. 2011) reconcile these allegedly contradicting arguments about the relative size of the out-group derived from group-threat and intergroup contact theory. According to their argument, the relative size of the out-group, on the one hand, fosters the perceived threat of the out-group and, on the other hand, increases the chances of intergroup contact. As a result, the relative size of the out-group has both a direct negative effect on the perception of the out-group and an indirect positive effect through personal contact with members of the out-group.

The Effect of Events on Attitudes toward Immigrants

While group-threat and intergroup contact theory have been the most prominent approaches in the literature on attitudes toward racial minority and immigrant groups, they do not explicate how events might affect attitudes toward these groups. In this article, I complement the existing theories and argue that events might alter the perception of an out-group in important ways. In particular, I first draw on group-threat theory to argue why and under which conditions events might affect attitudes toward immigrants and then discuss intergroup contact as a factor that mitigates this effect.

Even distant events such as the terror attack in Bali can foster the perception of immigrants and particularly Muslim communities as threatening. They might draw attention toward potential sources of intergroup conflict such as the local immigrant population or the alleged economic threats posed by immigrants (Hopkins 2010). This argument is partly supported by research on the psychological consequences of terror, which shows that terror not only is destructive in the material sense but also evokes fear and anxiety in the population (Spilerman and Stecklov 2009). Considering that groups purporting to speak in the name of Islam carried out the attacks in question, it is plausible that such fears evoked by terrorism are directed toward Muslim communities. It remains unclear, however, whether the same is true for immigrants in general. Psychological research on stereotypes shows that people tend to simplify and generalize their stereotypes (Bodenhausen 1993), which can lead to undifferentiated reactions to events (Bar-Tal and Labin 2001, p. 276). In addition, some have argued that non-European immigrants are most noticeable and shape the perception of immigrants in general (Semyonov, Raijman, and Gorodzeisky 2008, p. 22). Accordingly, it seems reasonable that terror attacks by groups speaking in the name of Islam foster fears toward not only Muslim communities but also the overall immigrant population as an out-group. Along these lines, Islamic terrorism precipitated a debate about immigrants and immigration in general. From this perspective, events are a source of perceived out-group threat. They direct attention toward existing fears related to potential intergroup conflicts such as those that result from a sizable immigrant population or changing economic conditions. Accordingly, the effect of events such as terror attacks should be related to the regional economic conditions such as the unemployment rate and the relative size of the immigrant population as well as changes in these two factors.

The role of the relative size of the out-group population, however, might be offset by personal contact with out-group members. As outlined by Pettigrew (1998, p. 70), one of the key mechanisms explaining the positive effect of intergroup contact is simply "learning about the out-group." According to this argument, direct interactions provide a source of information about the out-group that influences actors' perceptions and potentially replaces common stereotypes. The lack of such firsthand information, in contrast, renders other information sources (e.g., common stereotypes or media reports) more important (Sigelman and Welch 1993, p. 793). Stein, Post, and Rinden (2000), for example, argue that, "in a racially and ethnically homogeneous context, the mass media, school, and family socialization shape Anglo attitudes and opinions about minority groups," whereas in an ethnically heterogeneous context, "Anglos have more opportunities to form their opinions about minority groups . . . using direct contact" (p. 290). Accordingly, firsthand experience—whether through a close friend or a coworker—mitigates the role of other information sources, such as media reports about events. In addition, the proportion of immigrants in a particular region provides opportunities for intergroup interactions, which in turn directly affects the perception of the out-group and weakens the extent to which individuals perceive a large out-group as threatening. This argument implies that the experiences and information from interactions with members of the out-group mitigate the role of out-group size so that out-group size and contact with immigrants interact in shaping the response to the event. It also suggests that direct contact with out-group members can reduce the effect of the event itself.

This theoretical argument describes the mechanism by which events such as terror attacks might affect attitudes toward immigrants (or more generally, an out-group). It also points at the conditions under which such an effect is likely to occur and allows us to formulate concrete expectations about regional variation in the response to the Bali and the Madrid attacks. In particular, the argument suggests that events foster the perception of an out-group as threatening and direct attention toward potential sources of intergroup conflict such as the size of the immigrant population, economic conditions, or changes in these two factors. It also suggests that the experience provided by direct contact with immigrants mitigates the role of the out-group size and reduces the size of the effect itself. In brief, the argument can be summarized in three concrete expectations. First, the response to the event is more pronounced in regions with worsening economic conditions such as an increasing unemployment rate. Second, the effect of the event is larger in regions with a sizable out-group population but only for those who have no direct contact with immigrants. Third, direct contact with immigrants itself reduces the effect of the event on attitudes toward immigrants.

The Bali and Madrid Bombings and the Perception of Immigrants

How does this theoretical argument about the effect of events on the perception of immigrants illuminate the cases at hand? To answer this question, it is important to put the Bali and Madrid bombings into theoretical and historical perspective. Since 9/11, acts of international terrorism carried out by groups acting in the name of Islam have been central to the public discourse on Muslim communities and on immigration in general. The attacks have fed a narrative around immigrants as hostile to the fundamental values of the Western world. This has precipitated a wave of discriminatory acts against Muslims in Western societies (Allen and Nielsen 2002; Human Rights Watch 2002). Across the board, these terror attacks have also bolstered legislative initiatives for stricter immigration regulations. However, both the Bali and the Madrid bombings raise the question of whether they had similar consequences. Both attacks occurred after 9/11, which shaped the discourse on immigrants in important ways and potentially mitigated the effect of subsequent events. The Bali attack took place in a geographically distant place. After the Madrid bombing, at least some initially blamed the nationalist terrorist group Euskadi Ta Askatasuna (ETA; see app. A). Despite these facts, it is reasonable to argue that both attacks invoked immigration issues. First, with a death toll of 202 in Bali (many of the casualties European tourists) and 192 in Madrid, both attacks ranked among the deadliest on civilian life in recent decades. As such, the events themselves were devastating and traumatic, appearing on front pages of newspapers around the world. Second, previous research such as Jacobs et al.'s (2011) study about the impact of Israeli military operations in Gaza on anti-Semitism in Belgium has shown that even distant events can influence group relations. Third, the Bali attack was at least partly targeted at European citizens—directly confronting Europe with the threat of Islamic terrorism. Historically, Europe had mostly faced nationalist terrorism (e.g., IRA and ETA) and terrorism from the extreme right and left. From this perspective, the 2002 Bali bombing constituted a new situation in the European context.

At the same time, this historical background fueled the initial controversy about a potential ETA involvement in the Madrid bombing. Given the ubiquitous media coverage of both events and their respective role in the European context, it is reasonable to argue that the Bali and Madrid bombings invoked immigration issues in the public discourse as well as the general public.

DATA AND METHOD

The following analyses are based on data from nine countries in the first round of the ESS in 2002 (Belgium, Finland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and Great Britain). The ESS is a large-scale, cross-national project that conducts biennial surveys based on representative samples and face-to-face interviews in over 20 countries (Jowell 2003). During the fieldwork period for nine countries in the 2002 survey, a major terror attack occurred: the October 12, 2002, suicide bombing in Bali. Since the ESS 2002 includes questions on the perception of immigrants as well as information on European subregions, this coincidence provides a unique opportunity for a natural experiment that examines the impact events may have on the perception of immigrants across different contexts. For the main analyses presented in this article, I restrict the sample of the nine countries to the 5,236 respondents in 65 regions who were interviewed in a certain time interval before and after the event.[3] The replication analysis based on the Madrid bombing and the Eurobarometer uses a research design that closely resembles the approach described here and is discussed in appendix A.

Estimation Strategy

In the following analysis, I use the terror attack in Bali as an exogenous source of variation, together with the timing of the interviews in the ESS 2002, to define the experiment's treatment and control groups (for a similar method, see Van der Brug 2001; Boomgaarden and de Vreese 2007; Perrin and Smolek 2009). Respondents interviewed in a certain time interval before the Bali event can be designated as the control group (i.e., respondents who were not exposed to the treatment condition), and respondents

[3] To measure attitudes toward immigrants, I exclude respondents who were not born in the respective countries and control for the migration background of respondents' parents. In addition, five regions had to be excluded from the analysis because the European Union (EU) revised the regional classification system NUTS (nomenclature of statistical territorial units; for details, see below) used by the ESS. As a consequence, the regional data provided by Eurostat and the different national statistical agencies do not contain information for these changed or terminated regions.

who were interviewed in a discrete time interval after the event can be designated as the treatment group. Figure 1 illustrates this identification strategy using the ESS 2002 data from Portugal. Formally, the treatment indicator can be defined as

$$T_{ij} = \begin{cases} 0 & \text{if observation } i \text{ in country } j \text{ received the control,} \\ & \text{i.e., was interviewed in the time interval } t_0 \text{ before the event;} \\ 1 & \text{if observation } i \text{ in country } j \text{ received the treatment,} \\ & \text{i.e., was interviewed in the time interval } t_1 \text{ after the event.} \end{cases} \quad (1)$$

In many ways, this identification strategy resembles a regression discontinuity design (Imbens and Lemieux 2008). In both cases, an exactly defined cutoff point (the Bali attack) in a continuous covariate x(timing of interview) is used to define the treatment and the control groups. Such a design relies on two core assumptions to guarantee the ignorability of the treatment assignment (ignorability assumption).

First, the timing of the interviews across the fieldwork period or at least small differences around the cutoff point must occur by chance (i.e., the timing of the interview must be exogenous). There are, however, two potential biases that stand to subvert the assumption of complete randomization. (a) The literature on survey research has documented systematic differences in how easy or difficult it is to contact individuals. These well-documented differences create a potential *reachability bias* since respondents who are easier to contact tend to be interviewed earlier during the survey period—a factor that might induce systematic differences between the control and the treatment groups. This selectivity, however, is well documented and observable. It can be handled statistically by controlling for the number of times a respondent was contacted before being interviewed or alternatively for covariates that influence the reachability of respondents (mainly age and employment status). (b) Large-scale cross-sectional surveys like the ESS usually rely on a multistage sampling procedure. The random selection of regionally confined sampling points during the first stage may induce a *regional sampling bias*, if the fieldwork starts later at certain sampling points for logistical reasons. Although there is no reason to believe that such a bias would be systematically related to the outcome under investigation here, I evaluate this potential bias in the next section.

Second, the identification strategy relies on the assumption that there are no other time-varying variables that are causally before the event and systematically related to the outcome conditional on the event (*temporal stability assumption*). This assumption is essential and implies no trend in the average outcome in the absence of the treatment. It is a consequence of the fact that not the treatment itself but instead the covariate x(timing of interview) is randomized. The small differences in time and the absence of other notable events, to my knowledge, support the plausibility of the

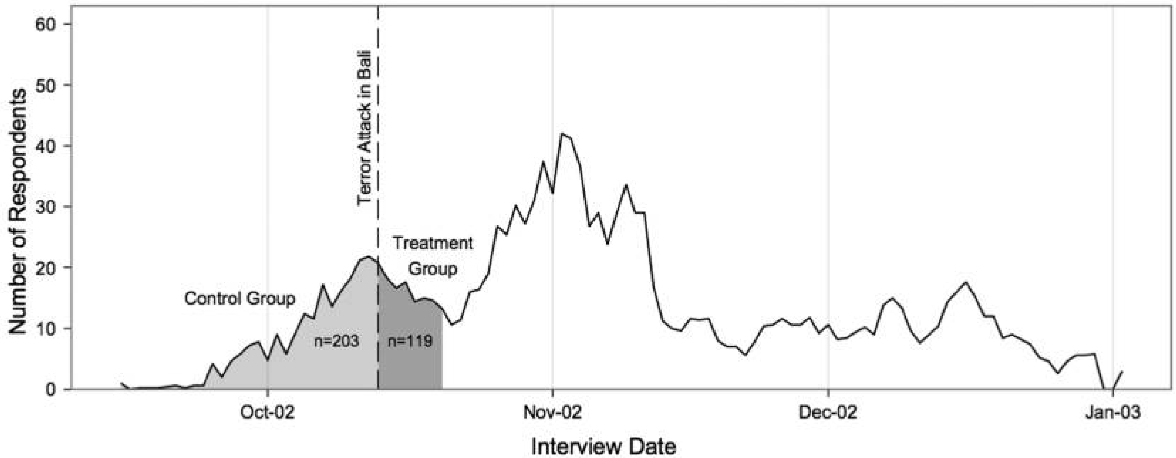

FIG. 1.—Treatment and control groups for Portugal. The figure shows the number of observations per day throughout the fieldwork period in terms of a four-day moving average. Control group (*light gray*) includes the respondents who were interviewed in the 30 days before the event. Treatment group (*dark gray*) includes the respondents who were interviewed in the week after the event.

assumption. The plausibility can also be assessed by comparing the treatment and the control groups with regard to other measures that should not be affected by the treatment. In addition, my analysis includes a simulation of fictitious events that partly evaluates whether time-varying variables that trend over longer time periods or regular temporal patterns produce results that are similar to those observed for the Bali terror attack. Yet, the prepost design makes it impossible to avoid remaining bias introduced by any variable that trends over the same time period, and no randomization can solve this problem.

Given these assumptions, the average causal effect of the event can be estimated with regression models and a dichotomous predictor for the treatment status:

$$y_{ijs} = \alpha_{js} + T_{ijs}\theta_{js} + X_{ijs}\beta_j + \epsilon_{ijs}.$$

Here, i, j, and s are the indexes for respondents, countries, and regions, respectively. The coefficient θ_{js} (for the treatment indicator T_{ijs}) is the crucial statistic and represents the difference in means between control and treatment groups, conditional on the covariates in $\boldsymbol{X}$. Under the assumptions discussed above, this difference in means can be interpreted causally as the average causal effect of the treatment on the outcome. In the first step of the analysis, I omit the regional level from this regression model and run a set of country-specific regressions that show the effect of the event separately for each country. In the second step, I use the pooled sample across all nine countries and multilevel models with a random intercept α_{js} and a random slope for the treatment effect θ_{js} on both the country and the regional levels.[4] To evaluate my main argument about variations in the response to the event across contexts, I extend these multilevel models with a set of two- and three-way interaction terms $\delta(T_{ijs} \times x_{js})$.

Across all models, $\boldsymbol{X}$ represents a matrix of control variables on the individual and the regional levels, and β, a vector of corresponding coefficients, which are of secondary interest and cannot be interpreted causally. Matching procedures are a further technique to condition on the set of observable covariates in $\boldsymbol{X}$. While the practical benefits of matching remain in dispute (Shadish, Clark, and Steiner 2008), matching offers potential advantages that might increase the balance between the treatment and the con-

[4] The multilevel models not only adjust the standard error for clustering on the regional and the country levels but also address the potential problem of the small sample size in some of the regions. The so-called empirical Bayes estimates are a weighted average of the estimates from a certain region and the overall estimate for the larger population (which is the prior information from a Bayesian perspective) in which the weighing depends on the available information for the respective region. Because of this, multilevel models provide the best estimates for all regions and are suited perfectly for applications in which the number of cases is small for some regions (for a discussion of this, see Gelman and Hill [2007]).

trol groups. For this reason, I supplement the results obtained from the regressions with estimates based on the matched sample. Further details about the matching procedure are presented in appendix B.

Plausibility of Ignorability Assumption and Imbalance between Control and Treatment Groups

The estimation of the causal effect as described in the last section depends directly on the plausibility of the ignorability assumption and the balance between the control and the treatment groups. In order to evaluate this assumption, figure 2 presents the imbalance between the treatment and the control groups in terms of the standardized difference in means (X-axis) and the variance ratio (Y-axis) in the raw data and the matched sample for 22 covariates in Portugal (the imbalance for the other countries is presented in table B2).[5] The figure includes a number of "pretreatment" variables as well as the propensity score as the predicted probability of receiving the treatment from a logit model.[6] As indicated by a rectangle in the figure, Rubin (2001) suggests that the absolute standardized differences in means should not be greater than 0.25, and the variance ratio should be between 0.5 and 2. Generally, balance should be reduced without limit (Imai, King, and Stuart 2008, p. 498). For most variables, the balance is within these limits, even for the raw data. Some, however, fall outside this threshold, which suggests a small but present imbalance between the two groups. To improve covariance balance, the final analysis conditions on a number of control variables, using both standard regression techniques and matching procedures. Figure 2*b* shows that the imbalance between the groups is considerably reduced in the matched sample. Table B2, however, also reveals that imbalance slightly increases for some covariates. This usually occurs when the balance in the raw data is already very high (Stuart 2010, p. 12).

In addition to providing information about the overall balance, the differences in means can be used to evaluate the two assumptions discussed above. The pattern observed in Finland bears out our expectations about the reachability bias (table B2). The treatment group is on average slightly younger, and the proportion of people who are retired and who work from home is lower. Portugal, by contrast, does not conform with this expectation (indicated in fig. 2). In the remaining countries, the treatment group is generally slightly younger, but the pattern varies in the case of

[5] Note that a comparison of means and variances does not necessarily imply that the groups are balanced since balance requires that the multivariate distribution for all covariates is the same for the treatment and the control groups.

[6] The covariates are age, age^2, female, education (years), education (categorical), working status (categorical), number of household members, voted during last election, Christian, urban area, time lived in area, and a number of interaction terms.

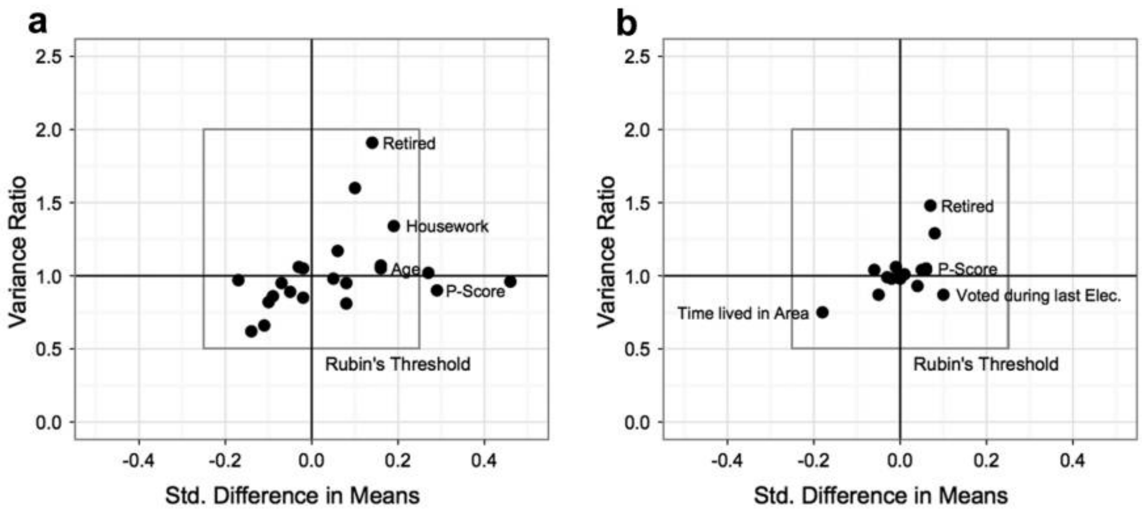

FIG. 2.—Imbalance between treatment and control groups for Portugal in terms of the standardized difference in means and the variance ratio between the treatment and the control groups in the raw data and the matched sample for 22 covariates. *a*, Balance in raw data, highlights variables that are relevant for potential selection processes; *b*, balance in matched sample, highlights covariates that are less well balanced. Imbalance measures for the other countries are presented in table B2.

working status. Such variation between the countries, in terms of the characteristics potentially affected by the reachability bias and the near lack of notable differences, suggests that even these differences are likely to result from a random process. This result indicates that the potential selection bias due to reachability exerts only a minor influence on the assignment of control and treatment conditions.

I also evaluate the potential regional selection bias created by the multistage sampling procedure. Figure 2 includes some measures that are regionally clustered, such as education, time lived in area, a measure of religion, the number of household members, and the urban location. Since a regional selection bias should be reflected in these measures, the absence of any significant differences—along with the generally small imbalance—indicates that the regional bias created through the time difference between the two groups is ignorable.

Overall, three conclusions can be drawn from these findings: First, the results indicate that the two potential selection biases (reachability bias and regional sampling bias) exert only a minor influence on the assignment of the treatment condition. Second, the generally small imbalance and insignificant differences between the treatment and the control groups in the raw data in terms of pretreatment variables support the plausibility of the time-invariance assumption. Third, conditioning on observable variables (illustrated with the matched sample) further increases the balance for most variables but not all. Using both regression and matching techniques helps to increase the confidence in the results by showing the robustness of the findings to different model specifications.

NUTS Regions, Variables, and Missing Data

The respondents of the ESS are nested in both countries and regions. The subregions are defined by the NUTS classification, which is a hierarchical geocode standard that divides the territory of the EU as well as some non-EU countries into three levels of subregions (NUTS 1, 2, and 3). The regions are based on socioeconomic, cultural, and historical characteristics to represent relative homogenous areas. The classification system distinguishes between 97 major socioeconomic regions with a population of 3–7 million (NUTS 1), 271 basic regions with a population of 0.8–3 million (NUTS 2), and 1,303 small regions with a population of 0.15–0.8 million (NUTS 3). The regional level provided by the ESS depends on the respective country, so the following analyses are based on NUTS 1 regions for Belgium and Great Britain and NUTS 2 for the remaining countries.[7]

[7] The regional level provided by the ESS ranges from NUTS 1 for Belgium and Great Britain to NUTS 3 for Netherlands. Some of the covariates, however, are not available for NUTS 3 regions, so the data for Netherlands are aggregated to the NUTS 2 level.

The ESS 2002 module on immigration provides a range of indicators on attitudes toward immigrants. The dependent variable used in the following analysis is constructed from six of these items, each of which is measured on an 11-point scale ranging from 0 to 10. The same items have previously been used in research on attitudes toward immigrants (Semyonov et al. 2008), so the measure relates directly to the literature. The questions focus on the impact foreigners have on different aspects of society and the extent to which they are perceived as a threat. The perception of impact and threat is an important aspect of the public discourse on immigration issues and directly related to group-threat theory and the overall theoretical argument. In particular, the questions refer to the impact foreigners, or more precisely "people who come to live here from other countries," have on the job situation, the welfare system, the economy, cultural life, general living conditions, and crime. Table 1 includes the exact wording of each question, as well as some summary statistics for the pooled sample of the nine countries in the analysis.

I use exploratory maximum likelihood factor analysis to construct the dependent variable from the six items. Previous research (Semyonov et al. 2008, 11) and my own results suggest that the six items belong to the same factor.[8] This factor can be understood as an index of how the local population perceives immigrants' impact on their society. The final analysis presented below is based on the factor score from this exploratory maximum likelihood factor analysis. Factor loadings and uniqueness are shown in table 1. Higher values of this variable can be interpreted as an increase in anti-immigrant attitudes, and the factor score variable was standardized by the standard deviation of the control group.[9]

The crucial independent variable—the treatment indicator T_{ij}—has been specified generally in equation (1). The control group includes all respondents who were interviewed 30 days before the event. The treatment group includes the respondents who were interviewed in the week after the event, so that the interval ranges from October 14–20, 2002 (for an illustration, see fig. 1).[10] This time interval should be as small as possible, while still con-

[8] Only one of the factors has an eigenvalue above 1 (Kaiser criterion), and all the variables have factor loadings above 0.5 (with most above 0.6).

[9] An additional issue concerns the measurement equivalence of the factor across countries—something that is important for analyzing how psychological constructs change across countries. Using a similar but not identical latent variable to measure attitudes toward immigrants in the ESS, Meuleman et al. (2009, pp. 357–59) have shown that there is indeed metric invariance. Such invariance is a prerequisite for the meaningful interpretation of cross-national variations in the effect of the event (Vandenberg and Lance 2000).

[10] Respondents who were interviewed on the day of the event are not included in the analysis because it is unclear whether they were already informed about the event.

TABLE 1
Description of Dependent Variables and Factor Analysis

				Factor Analysis	
Variable	Mean	SD	Distribution	Factor Loading	Uniqueness
"Would you say that people who come to live here generally take jobs away from workers in [country], or generally help to create new jobs?"	5.20	2.01		.61	.63
"Most people who come to live here work and pay taxes. They also use health and welfare services. On balance, do you think people who come here take out more than they put in or put in more than they take out?"	5.73	2.13		.60	.64
"Would you say it is generally bad or good for [country]'s economy that people come to live here from other countries?"	5.07	2.16		.77	.40
"And . . . would you say that [country]'s cultural life is generally undermined or enriched by people coming to live here from other countries?"	3.97	2.22		.69	.52
"Is [country] made a worse or a better place to live by people coming to live here from other countries?"	5.21	2.05		.76	.42
"Are [country]'s crime problems made worse or better by people coming to live here from other countries?"	6.93	1.93		.52	.73

Note.—N = 4,315. Mean, SD, and distribution refer to the pooled sample for the control and the treatment groups across all nine countries. Scale = 0–10 for all variables.

taining a sufficient number of cases.[11] In the case of the 2002 ESS, seven days turned out to be a fruitful compromise between these two criteria. Similar results were obtained with other intervals (available from the author). In a later step of the analysis, the interval for the treatment group was gradually shifted, in order to examine the decline of the treatment effect over time.

In addition to the treatment indicator, the second step of the analysis includes a number of important covariates that are part of interaction terms to evaluate the argument about variations in the response to the event. First, my argument suggests that the response to the event should be more pronounced in regions with a high or increasing unemployment rate. The corresponding variables are collected from Eurostat and defined as the regional unemployment rate in 2002 and the change in the rate between 2001 and 2002 (the results reported in this article are based on the change in the unemployment rate, but the findings for the two variables are essentially the same). Second, I have argued that the effect is larger in regions with a high proportion of immigrants for respondents who have no direct contact with immigrants. To evaluate this argument, I measure the relative size of the immigrant population in terms of the proportion of the population that is nonnational. This measure is based on the 2001 census and administrative data provided by Belgium's statistical agency Statbel.[12] There is no reliable and comparable regional measure for earlier periods, so it is only possible to examine the argument for the size of the immigrant population in 2001 and not for changes over time. Third, I expect that direct contact with immigrants mediates the relation between the relative size of the out-group population and the response to the event and reduces the effect of the event itself. Direct contact with immigrants is

[11] The time interval should be as small as possible because a smaller time interval reduces the potential for bias. Similar to a regression discontinuity design (Green et al. 2009), including observations that are more distant from the event increases the likelihood that the potential sources of bias (reachability bias and regional sampling bias) exert a greater influence on the assignment of the treatment condition. A larger time interval also increases the likelihood that there are other time-varying variables that are causally before the event and systematically related to the outcome conditional on the event (temporal stability assumption). A smaller time interval around the cutoff point, however, decreases the number of cases and therefore the precision of the estimates. To address this trade-off, I have selected a time interval that balances these two criteria, and I examined whether the results are sensitive to the size of the time interval.

[12] A drawback of this measure is the fact that it defines immigrants as nonnationals. In contrast to immigrants, nonnationals do not include naturalized immigrants, which might be problematic considering differences between countries in terms of naturalization policies. To my knowledge, these are the only available high-quality regional data across all nine countries that can be used as a proxy for the relative size of the immigrant population. The measure is closely related to other variables that are available for a subset of the countries, such as the proportion of the population that was born in a different country (correlation: 0.92). In addition, my regional analysis includes a country-level fixed-effect model, which controls for all country-level factors such as differences in immigration policies.

measured using two variables in the ESS survey that ask respondents whether they have any "friends" or "colleagues at work" "who have come to live in [country] from another country?" On the basis of these two variables, I have created a dummy variable that is coded 0 for respondents who have neither friends nor coworkers with an immigration background and 1 for those who have either friends or coworkers.

Finally, the models include a number of control variables on the individual and regional level, which improve the balance between the control and the treatment groups. They are selected on the basis of previous research, the potential selection biases discussed above, and the question of whether they are clearly pretreatment. An additional interaction term is included between two of the most important covariates because the inclusion of this term increases the balance between the treatment and the control groups. The variables are age, sex, education, working status, urban/rural location, the number of household members, an indicator noting whether a respondent's parents were born in the country in question, and an interaction term between education and location on the individual level. A separate sensitivity analysis adds the number of times a respondent was contacted before being interviewed as an additional control variable (the variable is not part of the main analysis because it is only available for eight out of the nine countries). The multilevel models also include a number of regional control variables. These variables include population size, population density, the unemployment rate in 2001, and the proportion of residents with no more than primary education in 2001. All variables are described in table C2.

The data include 12.2% of cases with missing values on any of the variables used in the analysis. These missing values are mainly on the dependent variable (11.3%), so multiple imputation would add little information to the regression (e.g., Little 1992). Accordingly, the main analysis presented in this article is based on case-wise deletion, but essentially the same results were obtained on the basis of multiple imputation.[13] A potential problem, however, is that the event changes the response behavior insofar as certain respondents are less willing to answer questions about immigration after the event. To explore this possibility, I use country-specific logistic regressions to model "missing on dependent variable" as a function of the treatment indicator, the control variables, and interaction terms between the treatment indicator and each of the control variables. The results show that the response behavior is not influenced by the event. From the 135 relevant re-

[13] The imputation was performed separately for each country using both the multivariate normal model (reported here) and the chained equations approach (Gelman and Hill 2007, chap. 25). Additional questions about immigrants and some other important measures (e.g., health and religiosity) were included as auxiliary variables to improve the imputation model. The results for the two approaches are nearly identical.

gression coefficients (treatment indicator plus interactions for each country), only 2% have a P-value below .05 and none are below .01. In addition, the direction of effects does not reveal any systematic pattern. This finding indicates that the response behavior was not influenced by the event and therefore most likely does not affect my analysis.

RESULTS

The Causal Effect of the Terror Attack in Bali: Cross-National Variations and Temporal Duration

I begin my analysis with a set of country-specific regressions that show the effect of the event on attitudes toward immigrants for each of the nine countries. The point estimates and 95% confidence intervals of the standardized treatment effect (θ_j) for the terror attack in Bali on October 12, 2002, are presented in figure 3*a* and table B1. The results show that the treatment effect is significant in Portugal, Poland, and Finland. The size of the effects is substantial. For all three countries, it is larger than the difference between male and female respondents and comparable to the effect of 7.3 years of education in Portugal, 5.1 in Poland, and 3.2 in Finland.[14] These effects are impressive, especially considering that education has been a major determinant for attitudes toward immigrants in the literature (Ceobanu and Escandell 2010, p. 319). In the other six countries, the effect is smaller and statistically insignificant with a negative point estimate for three out of the nine countries.[15] Similar results were obtained by using the matched sample (see table B1). In particular, the estimated effects for Portugal, Poland, and Finland, based on the matched sample, are substantial and statistically significant: 0.46, 0.31, and 0.16 standard deviations, respectively (or 7.9, 5.5, and 2.8 years of education). The P-values are slightly

[14] The effect of one additional year of education was estimated as −0.06 standard deviations in a simple binary regression using the pooled sample across all nine countries without any additional covariates.

[15] Given the multiple comparisons between the control and the treatment groups and the fact that the point estimates are not consistently positive, it is important to consider the possibility that chance alone produces the appearance of a significant effect in three out of nine countries. To address this problem, I have calculated the adjusted test statistics on the basis of the Bonferroni correction, which corresponds to a significance level of $\alpha/9$ (nine for the number of comparisons) and is generally considered a conservative adjustment. After this adjustment, the treatment effects for Portugal and Poland remain marginally significant at $P < .1$. The effect for Finland, however, is not significant after the Bonferroni correction is taken into account. These findings indicate that the observed effects for Portugal and Poland are unlikely to occur by chance, even after taking the multiple comparisons into account, while the effect for Finland might well be the result of chance alone. The second step of the analysis based on regional data and the pooled sample of all nine countries circumvents the problem of multiple comparisons and therefore further extends the tests performed here.

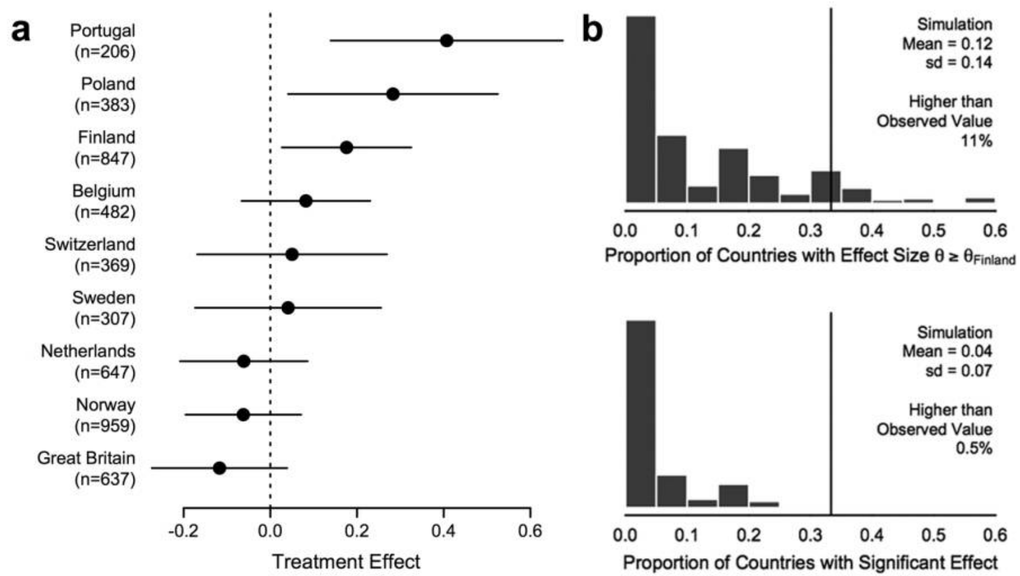

FIG. 3.—Estimated treatment effect for the terror attack in Bali. *a*, Estimated treatment effect; *b*, fictitious event simulation

higher, especially for Poland, but are below the 0.05 threshold in all three cases. Adding the number of times a respondent was contacted before being interviewed as an additional control variable to the regressions for eight of the nine countries leads to almost identical results.

The same analysis for the Madrid bombing based on Eurobarometer data presented in appendix A similarly shows substantial cross-national variations in the response to the event, with a clear effect only in a subset of the countries. The size of the effect in Spain indicates that national events can be far more influential, compared to distant terror attacks such as the Bali bombing. In particular, the effect in Spain corresponds to 25 years of education (the original scale is not comparable). The proportion of the Spanish population who considers immigration as one of the most important issues facing their country jumped by 12.9%, from 8.3% to 21.2%, immediately after the attack (average predicted difference).

Fictitious Event Simulation

It is important to consider the possibility that temporal variation in the sample or other time-varying factors apart from the event such as regular temporal patterns produce a similar result. To address this question, I use a simulation-based approach to calculate the probability of observing a similar result for fictitious events. This simulation of random events allows me to partly evaluate the two core assumptions of the estimation strategy discussed above (the assumption of exogenous interview timing and temporal stability). If either of these two assumptions were violated, we would expect to observe similar results for randomly picked (i.e., fictitious) events.[16]

To conduct this simulation, I randomly select days from the ESS fieldwork period, excluding the days around the actual terror attack. I then use respondents who were interviewed 30 days before these fictitious events as the control group and respondents who were interviewed in the week afterward as the treatment group.[17] Since the number of countries in the simulation varies, the two statistics used for the simulation are based on the proportions of countries that (*a*) have a standardized effect size at least

[16] Note that this is only true for time-varying variables that trend over longer time periods or regular temporal patterns such as differences between weekdays and weekends. It might still be the case that a time-varying variable trends over the same time period as the Bali terror attack but not during other periods of the ESS fieldwork.

[17] I completed 1,000 simulations, using all countries for which there were at least 20 respondents in both the control and the treatment groups. The distributions presented in fig. 3*b* are based on simulations including at least three countries. This reduced the number of simulations to slightly below 400.

as big as in Finland and (*b*) demonstrate a significant treatment effect. Figure 3*b* presents the distribution of these proportions from the simulations (i.e., the simulated sampling distribution) for the effect size (top) and the significant effects (bottom), as well as the observed proportion of three of nine countries (vertical lines). The figures show that the mean from the simulations is much smaller than the observed proportion and that the probability of obtaining at least the same proportion as observed (i.e., the P-value) is .1 for effect size and .01 for significant effects. Although the sample size is relatively small for some of the countries under analysis, these estimates are conservative since they do not take into account the much larger effect sizes and smaller P-values for Portugal and Poland. In sum, the results of the fictitious event simulation show that the effects observed for the three countries are highly unlikely to occur for randomly picked events. This finding indicates that temporal variations in the sample based on differences in the timing of interviews or other time-varying factors apart from the event, such as regular temporal patterns, are unlikely to produce the observed result for the terror attack in Bali.

Decay of the Effect over Time

The next segment of the analysis looks at the temporal duration of the effect using a moving window approach. More specifically, I estimate a series of regression models by “moving” the time interval for the treatment group, as shown in figure 1, away from the event. Accordingly, the control group remains the same, while the time interval for the treatment group changes by one day. The result is a series of estimates for the causal effect from the incrementally changed and overlapping time intervals for the treatment group, which permits us to see the temporal duration of the effect.

Figure 4 presents 25 estimates obtained by this method in chronological order for Portugal, Poland, and Finland. The overall estimates from the pooled sample (all nine countries) with an additional term for country fixed effects are included for comparison. The graph clearly shows that the effect of the event was substantially larger in Portugal and Poland than in Finland. The graph also shows that this larger effect in Portugal and Poland declined after some time but still remained at a relatively high and, for Portugal, statistically significant level several weeks after the event occurred. Finland, by contrast, shows an immediate drop after the first week, and the effect remains at this low level until the end of the period covered in the graph; a finding that might further question a short-term effect in Finland. Note that these differences in the treatment effects’ decline should be interpreted with caution since the difference in the estimated effects is not significant for some of the estimates and the sample size fluc-

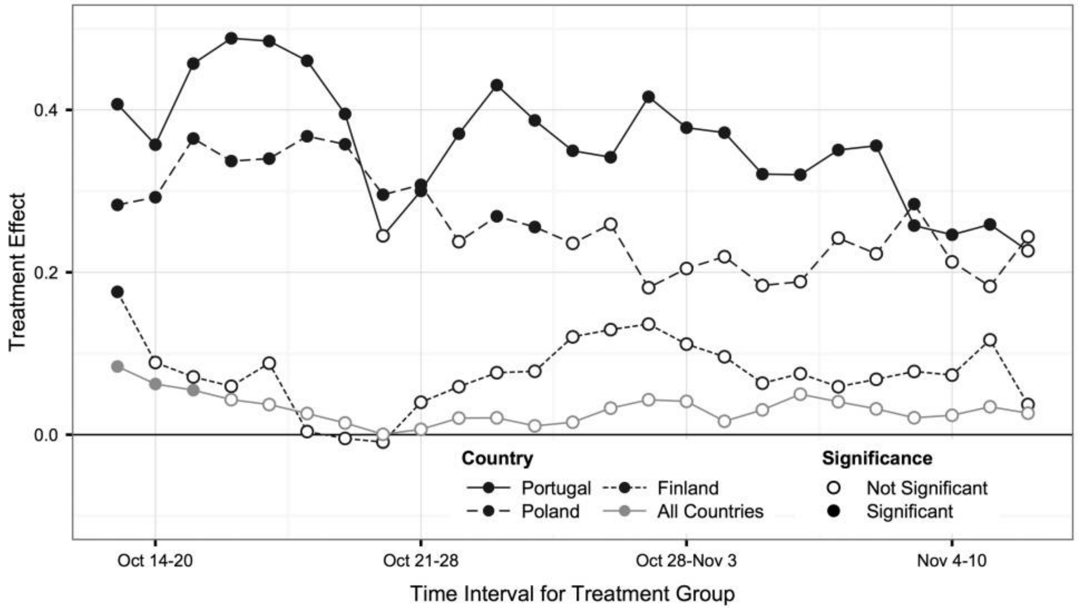

FIG. 4.—Decay of treatment effect (terror attack in Bali) over time for Portugal, Poland, Finland, and the pooled sample of all nine countries. Estimates are based on a series of regression models with incrementally changed and overlapping time intervals for the treatment group.

tuates substantially, particularly for Portugal.[18] Still, the results are suggestive: in Portugal and Poland, the Bali attack appears to have had a profound short-term effect on attitudes toward immigrants that slightly abates over time but is still observable several weeks after the event.

Regional Variations in the Causal Effect of the Terror Attack in Bali

The findings on the country level show substantial cross-national variations in the size of the causal effect, but the small number of cases limits the opportunities to examine the variations of the response to the event across contexts. To circumvent this problem and evaluate my argument about contextual variations in the response to the event, I now examine the variations of the treatment effect across 65 subregions in the nine European countries. The theoretical argument presented in this article suggests that, first, the response to the event should be more pronounced in regions with an increasing unemployment rate. Second, the effect of the event should be larger in regions with a sizable out-group population for those who have no direct contact with immigrants. Third, direct contact with immigrants should reduce the effect of the event itself on attitudes toward immigrants. To evaluate these hypotheses, I run a set of multilevel models with a random intercept and a random slope on the country and regional level based on the pooled sample with all nine countries and a set of interaction terms between the treatment effect and the three relevant covariates (the models also include additional control variables on the regional level).

Table 2 presents the results from these multilevel regressions. Model 1 first shows that the effect of the Bali terror attack across all nine countries is small and insignificant. Models 2 and 3 add a number of interaction terms to evaluate the argument about the conditions under which the effect of the terror attack is more pronounced. The findings relate to the three hypotheses formulated above. First, the results from model 2 indicate that the effect of the event is stronger in regions that have experienced a recent increase in the unemployment rate. This common measure in the literature on group-threat theory shows a strong and statistically significant relation to the size of the treatment effect (the same pattern emerges with a measure for the level of, and not the change in, the unemployment rate). In regions with an average increase in the unemployment rate, the treatment effect is relatively small and still statistically insignificant (note that the size and significance of the main effect for the treatment indicator depends on the scaling of the other variables). In places where the unemployment rate has

[18] This fluctuation in sample size is also reflected in the stronger variation in the treatment effect over time for Portugal. The reason for this variation is that the number of cases varies from day to day, so moving the time interval by one day might result in relative large differences in sample size.

TABLE 2
Treatment Effect of the Terror Attack in Bali across European Regions

	Model 1		Model 2		Model 3		Model 4		Model 5	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
T − treatment effect of Bali attack	.02	.06	.05	.05	.06	.06	.00	.39	.03	.11
Interaction term:										
T × increase in unemployment rate			.11**	.04	.11**	.04	.10*	.05	.09	.06
T × contact with immigrants			−.08	.08	−.06	.08	−.05	.08	−.04	.08
T × proportion of immigrants			.07	.05	.14*	.06	.14*	.07	.24**	.09
T × proportion of immigrants × contact with immigrants					−.17*	.09	−.18*	.09	−.17*	.09
Individual-level covariates	✓		✓		✓		✓		✓	
Regional-level covariates	✓		✓		✓		✓		✓	
Country-level covariates							✓			
Country-level fixed effects[a]									✓	
Random effects (regions):										
SD for intercept	.14		.15		.15		.15		.13	
SD for treatment effect	.09		.06		.06		.07		.07	
Random effects (countries):										
SD for intercept	.29		.27		.28		.23			
SD for treatment effect	.13		.12		.13		.17			

Note.—$N = 4{,}315$; $N(\text{regions}) = 65$. All continuous variables are standardized with a mean of zero, SD = 1. Lower-level interaction terms are omitted. A detailed description of all control variables is in table C2.

[a] Includes a set of interaction terms for country-specific treatment effects.

* $P < .05$.

** $P < .01$.

increased at a higher rate, however, the response to the event is more pronounced. This finding is in line with the theoretical argument that events such as the terror attack in Bali might direct attention toward potential sources of intergroup conflict. Figure 5 illustrates this relation and shows the empirical Bayes estimates from both the fixed- and the random-effects parts in model 2 for the size of the treatment effect in the 65 regions across the nine countries (the size of the points refers to the population in the respective region). The regions from the two countries with a large effect of the event (Portugal and Poland) are also those with a high increase in the unemployment rate from 2001 to 2002. Particularly interesting are the four regions from these countries with a modest increase in the unemployment rate of about 0.5% (black circles at around 0.5 on the X-axis and 0.1 on the Y-axis). In line with the overall pattern, these four regions show no or a very modest response to the event. The four Finnish regions, however, are some of the outliers with a low increase in unemployment but a treatment effect as large as 0.2 standard deviations.

The same pattern emerges in my analysis of the 2004 terror attack in Madrid. As shown in figure A1*b*, the effect of the Madrid bombing on the extent to which respondents consider immigration an important issue for their country is larger in regions with an increasing unemployment rate. This replication based on a second case reaffirms my results from the Bali analysis and further supports the argument that the response to terror attacks is shaped by the local unemployment rate as a potential source of intergroup conflict.

Second, the interaction between the treatment effect and contact with immigrants in model 2 shows that the treatment effect is larger for those without immigrant friends or coworkers. The interaction effect, however, is not significant. Accordingly, the evidence for my argument that direct contact with immigrants reduces the response to the event itself is not conclusive.

Third, model 2 shows that the point estimate for the interaction between the treatment effect and the proportion of immigrants is small and statistically insignificant. Only adding the three-way interaction term between the treatment effect, the regional proportion of immigrants, and contact with immigrants in model 3 reveals how the size of the immigrant population and contact with the out-group interact in shaping the response to the event. As expected and illustrated in figure 6, the proportion of immigrants in the local context only increases the response to the event for those who do not have direct contact with immigrants. For those who have either friends or coworkers with an immigration background, however, the size of the treatment effect does not depend on the size of the immigrant population in the local environment.

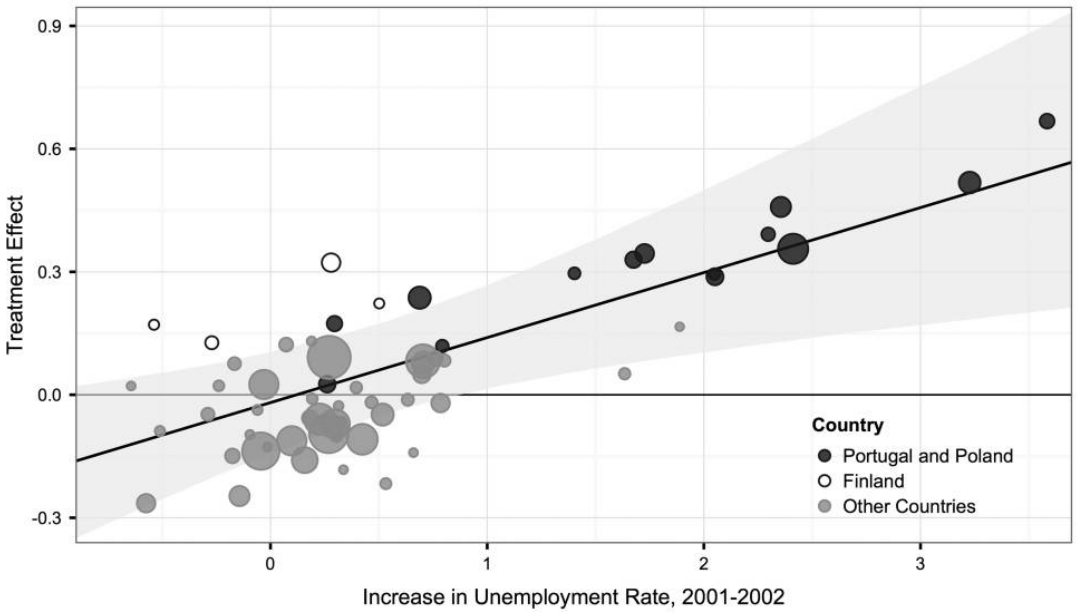

FIG. 5.—Relation between changes in the unemployment rate and the response to the terror attack in Bali across 65 European regions. Circle size represents the size of the population in the respective region.

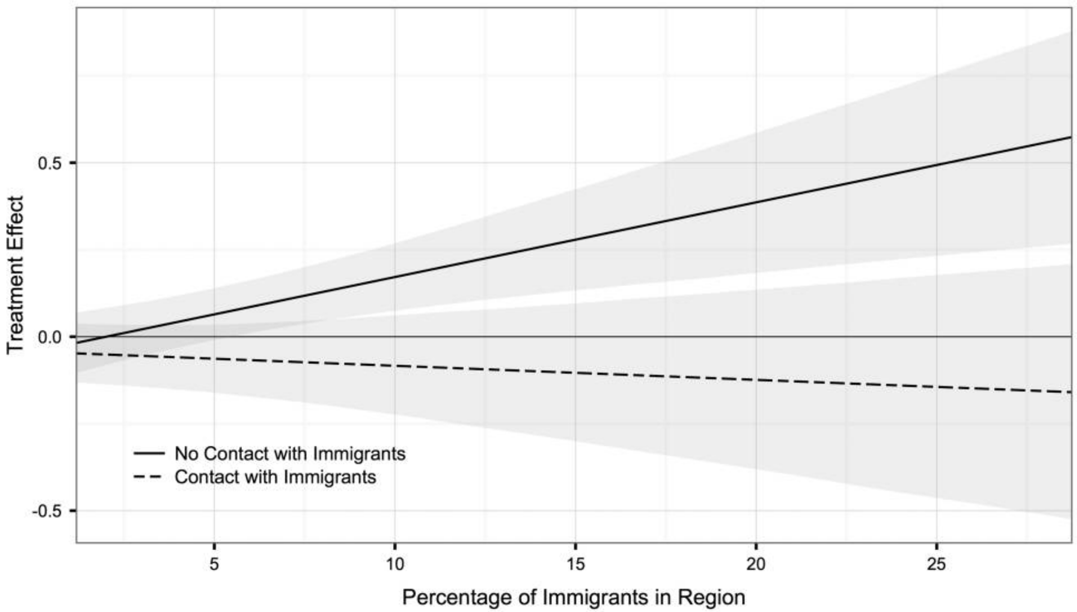

FIG. 6.—Relation between the relative size of the immigrant population and the response to the terror attack in Bali for those who have friends or coworkers with an immigration background and for those who have no direct contact with immigrants.

Overall, the results support the argument presented above. They indicate that the response to the event is more pronounced in regions with an increasing unemployment rate and show that the size of the out-group only plays a role for respondents who do not have direct contact with immigrants. Such direct contact with out-group members not only mitigates the role of the relative out-group size, but there is also partial evidence that direct contact reduces the effect of the event itself. As most clearly shown in figure 5, these findings also offer an explanation of the cross-national variations in the response to the event documented in the last section.

Alternative Explanations and Robustness Checks

The findings presented in the last section offer a convincing explanation of the cross-national and contextual variations in the treatment effect. It might nonetheless be the case that certain country-level factors explain the cross-national variations in the response to the event and not the contextual argument evaluated in the last section.

First and most obviously, variation in the national significance of the event could explain the differences in the effect of the attack on attitudes toward immigrants. The number of fatalities experienced by citizens of a particular country may be conceived as an index of the "importance" of the event to that country. Second, countries such as Great Britain have had significant (and recent) experience with domestic terror attacks. Hence, Britons may have experienced the distant terror attack in Bali as less immediate, compared to residents of countries that have no prior experience with domestic terrorism. This expectation is consistent with the argument put forward by other researchers, to make sense of the relatively mild psychological response to the London bombing of July 2005 (Rubin et al. 2005) and the Madrid bombing of March 2004 (Lopez-Rousseau 2005). In both cases, the authors suggested that experience with terrorism—IRA violence for Great Britain and the ETA attacks for Spain—played a role in mitigating citizens' reactions to the attack. Third, differences in the amount of media coverage of the event might play an important role in shaping the response to the event. The amount of reporting on the attacks might be seen as a reflection of the event's national importance and also as a source of variation in its own right.

To evaluate these competing accounts, I extend the multilevel models with country-level data on the number of casualties, the number of prior terror attacks in the respective country, and the extent of media coverage.[19]

[19] The data on the number of terror attacks were collected from the Global Terrorism Database and refer to a national terror attack with at least one casualty between 1970 and October 2002 (http://www.start.umd.edu/gtd/). I have experimented with different time periods and without the restriction to attacks with at least one casualty. The results

The extended regression model (model 4 in table 2) indicates that the results reported in the last section are not sensitive to these additional country-level factors, and the bivariate scatter plots shown in figure 7 suggest that none of the three measures is clearly related to the size of the effect in the respective country. This finding, of course, indicates nothing about the content of media coverage—an issue I take up in the conclusion.

As a final robustness check, I replace the random effects on the country level with country fixed effects together with interaction terms for country-specific treatment effects. These models are conservative, considering the relatively small number of regions for each country. But they are also a powerful way to rule out any alternative explanation on the country level such as the strength of national right-wing parties, the content of national media coverage, immigration policies, and many others. The results are presented in model 5 and confirm the findings reported above. The only considerable difference is that the two-way interaction term between the treatment effect and the change in the unemployment rate is only marginally significant in model 5, even though the size of the effect remains essentially the same compared to model 3. The increase in the standard error is not surprising, considering that the robustness check is conservative, while the fact that the size of the estimate remains essentially the same even after controlling for country fixed effects reaffirms my findings.

CONCLUSION

Since the attacks of September 11, 2001, acts of international terrorism committed by groups purporting to speak in the name of Islam have been a major theme in public discourse. At the same time, cross-national research consistently finds negative sentiments toward the immigrant population. Yet, the link between specific events and the perception of immigrants is mostly anecdotal. The fact that the Bali terror attack occurred during the fieldwork period of nine countries in the ESS afforded a unique opportunity to study this link and examine the impact events have on the perceptions of immigrants across different countries and their subregions. The findings of this natural experiment show considerable variations in both the magnitude of the effect and its temporal duration. In two or—to a lesser extent—three out of the nine countries—Portugal, Poland, and Finland—

are essentially the same. The extent of media coverage measure is based on the average proportion of days from October 14–20 on which the Bali terror attack was covered in newspapers for each country. App. C contains a detailed discussion of the selection of newspapers, the coding procedure, and a list of the newspapers. The results presented here are only based on one of the variables derived from the actual coding of the newspapers, all of which lead to similar results.

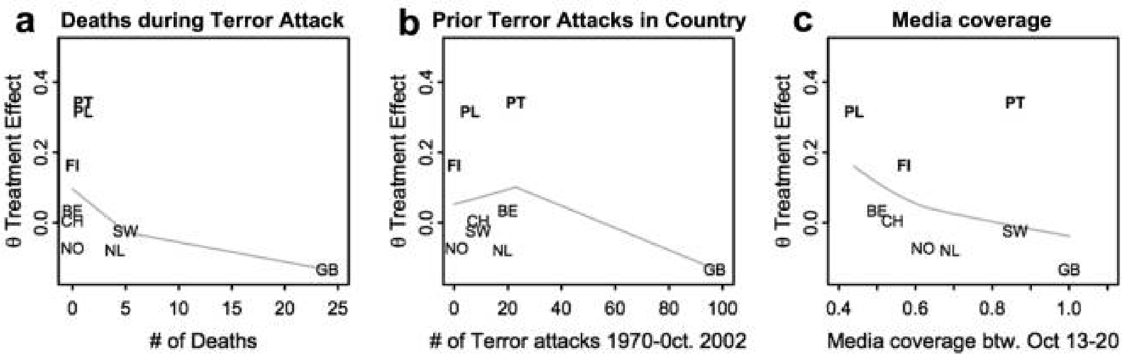

Fig. 7.—Cross-national variations of the treatment effect (terror attack in Bali); BE = Belgium, FI = Finland, NL = Netherlands, NO = Norway, PL = Poland, PT = Portugal, SW = Sweden, CH = Switzerland, GB = Great Britain.

the estimated causal effect is highly significant and substantial, ranging from the equivalent effect of 7.3 (Portugal) to 3.2 (Finland) years of education. In Finland, however, the effect disappears after the first week. The Madrid analysis in appendix A reveals similar cross-national variations in the response to the event. It also indicates that national events, such as the Madrid bombing in Spain itself, have a more profound effect on anti-immigrant sentiments compared to distant events such as the terror attack in Bali. Providing leverage to explain these cross-national variations, the Bali analysis on the regional level shows that the effect of the event is larger in regions with an increasing unemployment rate and that the relative size of the out-group and direct contact with immigrants interact in shaping the response to the event. These regional factors appear to explain the country-level findings, insofar as the Bali effect in Portugal and Poland seems to be driven by the change in the unemployment rate in many of the regions of those countries. The regional Madrid analysis reveals a similar pattern and confirms the finding that the response to events such as terror attacks is shaped by changes in the regional unemployment rate (data limitations make it impossible to replicate the other findings). Alternative country-level explanations such as the number of victims, prior experience with terror attacks, and the extent of media coverage were considered but found empirically lacking.

The implications of these findings fall into three broad categories. First, the study makes an empirical contribution to research on societal responses to terrorist attacks. While concerns about post-9/11 Islamophobia have received attention in public debates, empirical research on the responses to terrorist events has focused on psychological effects and changes in safety behavior (for an overview, see Spilerman and Stecklov 2009). The current study shows that terrorist attacks can have a profound short-term effect on citizens' perception of immigrants in some cases and under certain conditions. It elaborates the mechanisms that explain contextual variations in the response to such events and highlights the circumstances under which events such as terrorist attacks increase anti-immigrant sentiment. To generalize about the conditions under which such a mechanism appears to function, it is important to consider some characteristics of the attack in Bali as well as the specific survey questions used as a dependent variable. First, the event took place in a location that was geographically distant from the countries in the study. Second, the attack in Bali occurred roughly a year after the 9/11 attacks in the United States. September 11 probably had a profound impact on respondents' perceptions of immigrants and may in turn have mitigated the effect of the event in Bali. Third, the actual wording of the survey questions focuses on all immigrants and ignores that the immigrant populations in Europe are diverse in terms of ethnic origin and religion. These characteristics of the Bali attack strengthen rather than weaken our results. The fact that a distant event, shortly after 9/11, had any effect on attitudes toward immigrants in general may be considered a conservative test of the effect of events on attitudes toward immigrants. The findings from the Madrid case confirm this argument, insofar as the effect in Spain is much larger. Fourth, while devastating and horrific, the Bali attacks were significantly less salient compared to 9/11, considering not only the human and economic damage but also the public discourse that followed the event. The attacks were not historically unique events; on the contrary, there are many parallel events on a similar scale such as the attacks in London (July 2005), Mumbai (November 2008), and most recently in Moscow (March 2010). While it is notoriously difficult to predict the impact of events like these on attitudes toward immigrants, purely on theoretic grounds, the mechanism elaborated in this article offers a useful starting point for understanding the conditions under which an event has an effect. The second case study based on the Madrid bombings reaffirms the conclusions from the Bali case. In particular, the argument suggests that for the extension to other cases, potential sources of group-threat—such as the economic conditions and the size of the out-group population in the local environment as well as intergroup interactions—play a crucial role.

Second, this study makes a theoretical contribution to group-threat and intergroup contact theory as well as the literature on attitudes toward immigrants. It elaborates a mechanism for short-term (and potentially long-term) changes in the perception of an out-group. In particular, I have argued that events such as terror attacks negatively affect attitudes toward immigrants by fostering the perception of an out-group as threatening and by directing attention toward potential sources of intergroup conflict. The experiences and information from direct contact with members of the out-group, however, might mitigate the role of the out-group size and reduce the effect of the event itself. This mechanism extends group-threat and contact theory in important ways. It shows that intergroup conflict as well as the contact between groups not only shape attitudes toward immigrants independent of the particular time and period but also amplify or mitigate the response to particular events, which adds a temporal component to the classical formulation of the two theories. In brief, the findings presented in this article contribute to our understanding of intergroup relations and, in turn, may have implications for the study of discrimination and other more enduring relationships between groups.

Finally, the exploitation of exogenous events together with the variation in the timing of the ESS survey demonstrates how the temporal embeddedness of both events and survey responses can be used to gain analytic leverage. At the same time, temporal embeddedness constitutes a potential source of bias in survey research. While regional variations have traditionally figured prominently in the design of surveys, and have become a mainstream topic in sociological research, temporal variations have received little attention and are seldom addressed as a potential source of bias. In cross-national research, for example, temporal bias might play an important role when cross-national variations are the artifact of national events that occur during the survey period. While it is important to remain alert to the potential bias posed by the temporal embeddedness of survey interviews, the implicit contention of this study has been that temporality should be thought of as part of the opinion formation process and, therefore, as an analytic area yet to explore (for a perspective on opinion formation that supports this claim, see Zaller [1992]). From a practical perspective, the ESS is exemplary in regard to this program: it supplements its large-scale cross-national survey with an event catalog. Although rudimentary compared to the event catalogs used in research on collective action (Earl et al. 2004), this feature allows researchers to gain key analytical leverage based on the temporality of events and the timing of interviews.

The findings from the study are also limited in several regards. First, the empirical analysis is limited to two specific historical cases, and only one allowed me to fully explore the theoretical argument. Given this limitation, future studies should be undertaken to establish the extent to which the conclusions from this study hold in other cases. Second, the study falls short in exploring the role of media content in mediating the effect of events. Studies such as Boomgaarden and Vliegenthart (2009) or Ladd and Lenz (2009) illustrate that the actual content of media reports can have an influence on public opinion. For the substantive case at hand, the representation of the event in the media might mediate the response to the event. Such a content analysis was beyond the scope of this study and stands as a promising topic for future research. It should also be noted that a connection between media content and the response to the event would not be at odds with the explanation proposed in this article. On the contrary, an analysis of media content would provide a way of further exploring the argument that events direct attention toward potential sources of intergroup conflict. Finally, this study is limited to the cognitive dimension and does not explore how the effect of events on attitudes may propagate to actual behavioral outcomes (e.g., discriminatory practices or collective violence). The findings suggest, however, that research on intergroup relations and categorical inequality should take temporal variations seriously and consider the ways in which events can induce profound short-term and potentially also long-term changes in the perception of an out-group.

APPENDIX A

Madrid Bombings and Attitudes toward Immigrants

The analysis presented in this article is based on a unique opportunity for a natural experiment. It makes use of the fact that the terror attack in Bali coincided with the fieldwork period of a large-scale, cross-national survey that includes questions on immigrants and immigration. A major concern about the findings, however, is the focus on a single case, the Bali terror attack in 2002. Here, I present partial evidence from an additional case that reaffirms some of the major findings from my analysis. Using Eurobarometer data, I examine the effect of the 2004 Madrid bombing on attitudes toward immigrants across different European countries and their subregions. The analysis is limited in several regards but closely resembles the ESS case and replicates some of the most important aspects of my findings. At the same time, the limitations also show that my main analysis of the ESS data stands out as a unique opportunity, which provides a more nuanced picture of the conditions under which events affect attitudes toward immigrants.

Around 7:40 a.m. on March 11, 2004, 10 explosions shattered four rush hour trains in Madrid, Spain, killing 192 and injuring over 1,800 commuters. The Madrid bombing is widely considered as the most devastating terror attack in Europe and the first major attack committed by groups purporting to speak in the name of Islam. To estimate the effect of the 2004 Madrid bombing on attitudes toward immigrants, I use the Eurobarometer 61.0 from 2004 (European Commission 2004). The Eurobarometer is a series of cross-sectional surveys conducted in EU countries with the goal to track public opinion on issues that are relevant for the EU. The fieldwork period of the spring 2004 survey coincided with the Madrid terror attack. As such, the Madrid bombings offer a rare opportunity to replicate my findings from the Bali case using a similar cross-national survey from multiple European countries.

Compared to my analysis based on the ESS and the terror attack in Bali, this second case is limited in several regards. First, the Spanish government and many others initially focused on separatist terrorism and speculated about an ETA involvement. Along these lines, the public discourse at first evolved around both separatist terrorism (ETA) as well as possible connections to Islamic terror groups. Only several days after the event, the attack was attributed to an al-Qaeda-inspired terrorist cell. This controversy in the days after the event might have shifted the focus away from immigration issues and toward long-standing national problems. Second, the Eurobarometer 61.0 was not focused on attitudes about immigrants and immigration. The survey only includes a single question to evaluate the effect of the Madrid bombing on attitudes toward immigrants. This restriction to a single indicator decreases the quality of the outcome measure compared to the ESS analysis and reduces the possibilities to examine the conditions under which events are influential. In particular, the lack of a measure for contact with immigrants makes it impossible to examine the argument about the role of out-group size and contact with immigrants. Accordingly, my analysis of the Eurobarometer data focuses on the unemployment rate as one of the most important aspects of my argument. A third limitation of the Eurobarometer data is that the number of respondents interviewed after the event is relatively small in some of the countries. As a consequence, the confidence intervals for some of the estimated effects are large, and the results are less stable compared to the ESS analysis. Overall, these limitations highlight the unique opportunity provided by the ESS. Nonetheless, the additional case allows me to reaffirm some of the most important aspects of my argument, and as such it alleviates the concerns arising from the focus on a single case.

To estimate the causal effect of the terror attack in Madrid on attitudes toward immigrants across 13 countries and their subregions, I use the same research design with regression models that resemble my ESS analysis.[20] The outcome variable is based on a single question that measures attitudes toward immigration. This question asks respondents about the most important issues facing their country today, with the instruction to select no more than two from a list of 15 issues.[21] The following analysis is based on a binary indicator constructed from this question coded 1 if the respondent selected "immigration" from this list.

Figure A1 shows the main findings from my analysis. First, figure A1*a* shows the size of the causal effect together with 95% confidence intervals for each country in terms of log odds based on a set of country-specific logistic regressions.[22] With an effect of about 3.2 odds ratios, or 25 years of

[20] Note that the analysis also differs from the ESS in important ways that are mainly related to differences between the two data sets. The Eurobarometer analysis, e.g., does not include measures for the number of household members, contact with immigrants, and parents' place of birth.

[21] The exact wording of the question is "What do you think are the two most important issues facing [our country] at the moment?" The list includes crime, public transport, economic situation, prices/inflation, taxation, unemployment, terrorism, defense/foreign affairs, housing, immigration, health care system, educational system pensions, environmental protection, and others.

[22] The country-specific analysis excludes Great Britain, Ireland, and Italy, which have a particularly small sample size in the treatment group. The estimates for these countries have a large confidence interval and are extremely sensitive to the model specification. These three countries, however, are part of the multilevel models used for the regional analysis, which mitigates the problem of small sample sizes in some countries and regions.

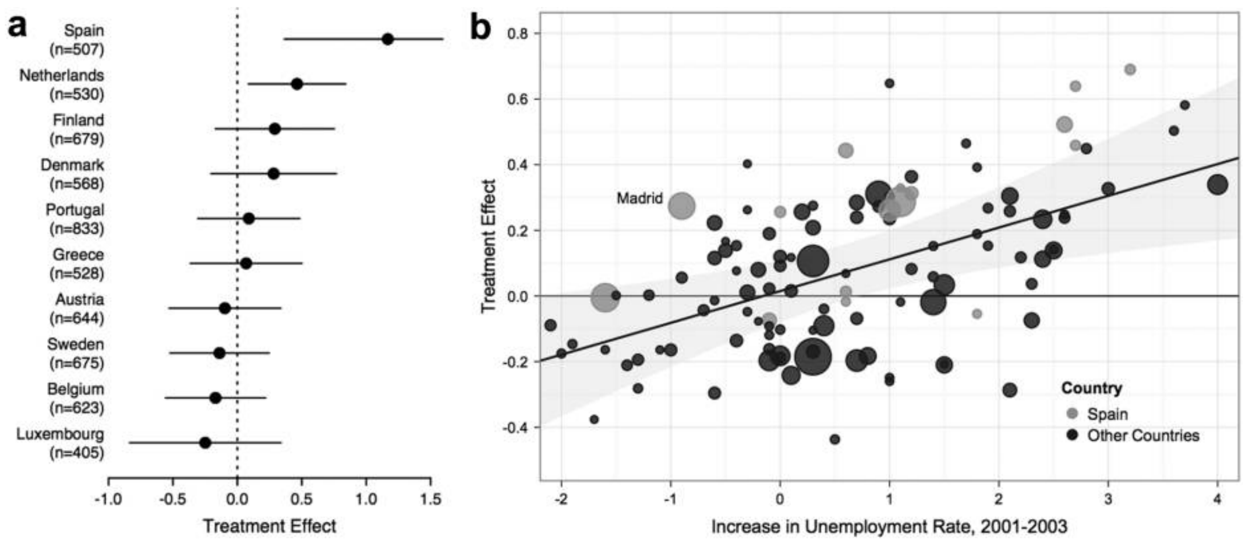

FIG. A1.—Estimated treatment effect of the terror attack in Madrid, March 2004, in log odds on the extent to which respondents consider immigration one of the most important issues facing their country today. *a*, Effect of Madrid bombing; *b*, changing unemployment rate and effect of Madrid bombing. Country-specific estimates (with 95% confidence interval) in *a* reveal substantial variation in the size of the effect across countries, with a large effect in Spain itself; *b* shows the relation between changes in the regional unemployment rate and the response to the event in 129 European regions and indicates that the response to the event was larger in regions with an increasing unemployment rate.

education, the estimated effect of the 2004 Madrid bombings on attitudes toward immigrants is extremely large in Spain itself. The average predicted difference indicates that the proportion of the population that considers immigration as one of the most important issues facing the country increased by 12.9%, from 8.3% to 21.2%, immediately after the attack (the 95% confidence interval for the increase ranges from 3.9% to 25.9%). While this finding is consistent across different model specifications, the estimates for the other countries are less stable. In general, the effect tends to be relatively large in the Netherlands, Finland, and Denmark but not consistently significant. For the remaining countries, the findings generally indicate small and statistically insignificant differences between the treatment and the control groups, indicating that the terror attack in Madrid had no short-term effect on respondents' perception of immigrants. These small and specification-sensitive effects might be explained by the initial focus on separatist terrorism. Similar to my analysis of the ESS data, these findings indicate considerable cross-national and (as shown later) regional variation in the response to the event. They also reveal that the effect of national events can be much larger compared to distant terror attacks such as the Bali bombing. This finding confirms my argument that the terror attack in Bali is a conservative test of events' effect on attitudes toward out-groups.

Second, figure A1*b* shows the relation between the response to the event and the regional increase in the unemployment rate from a logistic multilevel model with 129 regions. The graph indicates that the effect of the terror attack is larger in regions with an increasing unemployment rate. This finding reaffirms my results from the Bali case and supports my theoretical argument about the conditions under which events such as terror attacks are influential. The graph also highlights Madrid and other Spanish regions as outliers with a larger response to the event than predicted from the model alone.[23]

Overall, the findings from the Madrid case reaffirm my main results in important ways. They show considerable variations in the effect of events on attitudes toward immigrants across countries and subregions. Extending the previous findings, the Madrid case reveals that the response to national events can be much larger, compared to distant terror attacks, and also reaffirms the role of the regional unemployment rate for the response to events. This partial replication of my main findings addresses concerns related to the focus on a single case.

[23] Note that in all of the Spanish regions, only a small number of interviews were conducted after the event, so the regional estimates are pulled toward the overall mean, which explains the difference in effect size between the country-specific and regional estimates.

APPENDIX B

Details about Matching Procedure

Although the practical benefits of matching remain in dispute (Shadish et al. 2008), I supplement the results obtained from the regressions with estimates based on matched samples. In particular, I use a matching procedure called genetic matching (GenMatch), which automatically finds matches with optimal balance (Sekhon 2011; Diamond and Sekhon, in press). In contrast to the more widely used technique of propensity score matching, GenMatch circumvents the problem of finding the best propensity score model by using a genetic search algorithm to determine the weight given to each covariate in the multivariate matching process, thereby optimizing balance. The actual variables used for the matching are those described in table C2, together with some additional interaction terms. These variables were selected in order to maximize balance and outperformed alternative specification in this regard. The estimates from the matched sample are based on the same regression model as the one described above. Table B1 presents the regression estimates from the raw data and the matched sample. The results are based on multiple imputation, so they also allow a comparison between the estimates based on case-wise deletion (fig. 3*a*) and the imputed data set. Table B2 complements figure 2 and presents the standardized differences in means for all nine countries.

TABLE B1
Estimated Treatment Effect

		Regression Estimate		Matching Estimate	
	n	Coefficient	SE	Coefficient	SE
Portugal	299	.29**	.11	.46**	.12
Poland	495	.27*	.11	.31**	.11
Finland	911	.16*	.08	.16*	.08
Belgium	603	.08	.07	.03	.08
Switzerland	364	.03	.10	−.07	.11
Sweden	405	−.02	.11	−.11	.12
Netherlands	999	−.05	.07	−.06	.07
Norway	727	−.09	.07	−.09	.08
United Kingdom	698	−.12	.08	−.11	.08

Note.—Control variables for the regression estimates are described in table C2. Both estimates are based on the imputed data set so the sample size differs from fig. 3*a* above.

* $P < .05$.

** $P < .01$.

TABLE B2

Imbalance between Treatment and Control Groups

Variable	Raw Sample	Matched Sample	Raw Sample	Matched Sample	Raw Sample	Matched Sample	Raw Sample	Matched Sample
	Belgium		Switzerland		Great Britain		Netherlands	
P-score	.30	.03	−.11	−.15	.39	.02	.26	.00
Age	−.12	.02	−.05	−.14	−.19	−.03	.02	−.09
Age^2	−.12	.02	−.10	−.16	−.20	−.02	−.02	−.11
Female	−.08	.01	−.05	.03	.13	.01	−.10	−.01
Education (years)	−.04	−.10	−.07	−.06	.00	−.12	.04	.00
Education:								
None or primary	.08	.01			−.10	.00	−.11	.00
Lower secondary	−.04	−.03	−.08	.00	−.11	.01	.02	.00
Upper secondary	−.17	.01	.11	.00	.11	−.01	.12	.00
Tertiary	.16	.01	.06	.00	.05	−.01	−.10	.00
Working status:								
Working	.18	−.01	.04	.00	.13	.00	.00	.00
Unemployed	−.11	.00	.06	.00	.00	.00	−.02	.00
Retired	−.02	.02	.07	.00	−.09	.00	−.05	.00
Housework	−.08	.00	−.21	.00	−.10	.00	.04	.00
No. household members	.01	−.02	−.28	−.03	.06	.03	−.03	.01
Voted during last election	−.01	−.02	−.16	−.16	−.01	.02	.16	.16
Christian	−.12	−.15	−.11	−.18	.08	.06	.09	.04
Urban area	.07	−.02	.15	.00	−.04	.00	−.11	.00
Time lived in area	−.13	−.04	−.05	−.06	−.04	.09	.13	.10
Interactions:								
Education × female	−.07	−.01	−.01	.06	.12	−.01	−.07	−.01
Education × urban area	.07	−.03	.11	−.02	−.01	.02	−.12	.02
Education × Christian	−.07	−.10	−.14	−.46	.1	.15	.07	−.01
Education × age	−.08	−.02	−.16	−.25	−.17	−.11	.04	−.09

TABLE B2 (*Continued*)

Variable	Raw Sample	Matched Sample	Raw Sample	Matched Sample	Raw Sample	Matched Sample	Raw Sample	Matched Sample
	Norway		Poland		Sweden		Finland	
P-score	.25	.00	.28	.10	.29	.04	.22	.07
Age	−.05	−.06	−.32	.01	−.19	−.08	−.09	−.01
Age^2	−.08	−.07	−.37	.00	−.21	−.09	−.09	−.01
Female	.02	.01	−.07	−.15	.06	.01	.02	−.01
Education (years)	.03	−.03	.09	−.04	.05	.01	−.01	.04
Education:								
None or primary	−.15	.00	.01	−.07	−.05	.01	−.02	−.02
Lower secondary	.07	.00	.02	.01	0	−.01	−.01	−.02
Upper secondary	.09	.00	−.08	.04	.06	.00	.02	.02
Tertiary	−.19	.00	.04	.02	−.1	.00	−.01	.00
Working status:								
Working	.05	.00	.03	−.06	.22	.00	−.01	.01
Unemployed	−.02	.00	.12	−.03	−.17	.01	.03	−.02
Retired	.01	.00	−.06	.07	.04	−.02	.1	.00
Housework	−.05	.00	−.14	.05	−.13	.00	−.08	.00
No. household members	−.01	.00	.08	−.06	−.04	.08	.11	.03
Voted during last election	.06	.01	−.15	−.15	−.17	−.19	.00	.09
Christian	.01	.07	.12	.07	−.02	−.09	.13	.18
Urban area	.10	.00	−.02	.09	.06	.01	.01	.02
Time lived in area	.07	.00	−.17	.07	−.23	−.18	−.08	.00
Interactions:								
Education × female	−.01	−.01	−.04	−.16	.04	−.02	.03	.01
Education × urban area	.05	−.03	−.05	.07	.08	.02	.02	.00
Education × Christian	.00	.03	.16	.02	−.01	−.09	.12	.18
Education × age	−.01	−.08	−.15	.02	−.19	−.11	−.09	.01

NOTE.—Standardized differences in means for the raw data (raw) and the matched sample (matched) calculated as the mean for the treatment group minus the mean for the control group divided by the SD in the original treatment group.

APPENDIX C

Description of Independent Variables and Measure of Media Coverage

Table C2 contains a description of the independent variables used throughout the article. The measure of media coverage is based on newspaper reporting in the weeks after the event. The selection of newspapers in order to measure the media coverage of the event was based on circulation and diversity but was also constrained by availability. For each country, the print version of at least one newspaper was examined, and whenever possible, the full-text online versions of additional newspapers were also examined with a simplified coding schema. Multiple newspapers were selected in order to avoid potential biases. This was not possible in Belgium, Finland, and Norway. Table C1 contains a list of the newspapers used for each country, together with the circulation. The actual coding covered October 13–31, 2002, and assigned one of four categories to each day the newspaper was published during this period: the Bali terror attack was covered (1) on the front page with picture, (2) on the front page without picture, (3) on the following pages, and (4) not at all. For the coding of the full-text online version, this schema was reduced to (1) covered in the issue and (0) not covered because the page numbers from the printed edition were not always available. For both codings, only articles that explicitly focused on the terror attack were counted.

TABLE C1
List of Newspapers

Newspaper	Circulation
Belgium:	
De Morgen	73,784
Finland:	
Helsingin Sanomat	412,421
Netherlands:	
Trouw	107,000
De Volkskrant	282,000
NRC-Handelsblad	240,000
Norway:	
Dagbladet	186,136
Poland:	
Dziennik Baltycki	154,000
Gazeta wyborcza	417,000
Zycie Warszawy	250,000
Portugal:	
Público	33,159
*Diário Económico**	15,100

TABLE C1 (*Continued*)

Newspaper	Circulation
Sweden:	
Dagbladet	193,637
Dagens nyheter	344,000
*Svenska Dagbladet**	195,200
Switzerland:	
Neue Züricher Zeitung	134,526
Aargauer Zeitung (regional)*	200,000
Berner Zeitung (regional)*	165,700
Basler Zeitung (regional)*	194,358
United Kingdom:	
Daily Express	668,273
Daily Telegraph	686,679
*The Sun**	3,005,308

* Simplified coding based on full-text online version of printed newspaper.

TABLE C2

DESCRIPTION OF INDEPENDENT VARIABLES

Independent Variable	Description
Individual level:	
Contact with immigrants	0 = no friends or coworkers with immigration background; 1 = either friends or coworkers with immigration background
Age	Categorical age variable: 1 = under 35; 2 = 35–59; 3 = over 60
Sex	0 = male; 1 = female
Education	Categorical variable based on the CASMIN classification: 1 = no or primary education; 2 = lower secondary education; 3 = upper secondary education; 4 = post-secondary/first stage of tertiary education; 5 = second stage of tertiary education
Household members	Number of household members (categorical): 1 = one-person household; 2 = two-person household; 3 = three or more person household
Working status	Categorical variable based on the major activity during the last seven days: 1 = employee or self-employed; 2 = in education or military service; 3 = unemployed; 4 = retired, permanently sick, or disabled; 5 = housework
Location	Self-reported location: 0 = rural; 1 = urban
Parents' place of birth	0 = both born in [country]; 1 = at least one not born in [country]
Interaction term	Interactions between education and location
Regional level (NUTS 1, 2):	
Population size, 2002	Regional size of population, 2002 (source: Eurostat)
Population density, 2002	Regional population density, 2002 (source: Eurostat)
Unemployment rate, 2001	Regional unemployment rate for population above 15, 2001 (source: Eurostat and national statistical agencies for Switzerland and Norway)

TABLE C2 (*Continued*)

Independent Variable	Description
Change in unemployment rate, 2001–2	Change in regional unemployment rate, 2001–2 (source: Eurostat and national statistical agencies for Switzerland and Norway)
Proportion of immigrants . . .	Proportion of population that is nonnationals, 2001 (source: census data from Eurostat and national statistical agencies for Belgium)
Educational composition	Proportion of population that has no more then primary education, 2001 (source: census data from Eurostat and national statistical agencies for Belgium)

NOTE.—NUTS = nomenclature of statistical territorial units; CASMIN = comparative analysis of social mobility in industrial nations.

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