Supplementary Information for

Using a Natural Experiment to Estimate the Electoral Consequences of Terrorist Attacks

This PDF file includes:

Supplementary text
Figs. S1 to S18
Tables S1 to S11
References for SI reference citations

Supporting Information Text

Information on the Attacks

We provide information on each of the attacks in Table S1. For each attack, we show the date when it occurred, the province where it took place, the name of the victim, how the attack was perpetrated (e.g., shooting, car bomb), whether the victim died or was injured, the victim's status (e.g., civilian, or member of the military or the police), and the political party that was in office. We also show when the attack took place in relation to the timing of the interview. National media outlets covered all the ETA attacks immediately after they occurred, so we assume that the individuals in our sample were aware of them the same day or, at the latest, the day after they took place. For additional context, in Table S7, we show when each attack occurred in relation to the most recent and next general election.

Table S1. Timing of ETA Attacks and CIS Surveys

	Date	Province	Victim	Status	Days after CIS survey started	Days before CIS survey ended	Incumbent	CIS Survey ID
Attack 1	09/12/1989	Vizcaya	Luis Reina Mesonero (car bomb; killed)	Civilian	5	2	PSOE	1836
	09/12/1989	Madrid	Carmen Tagle González (shot; killed)	Civilian (Judge)	5	2	PSOE	1836
Attack 2	06/03/1990	Navarra	Francisco Almagro Carmona (shot; killed)	Civilian	2	6	PSOE	1873
Attack 3	06/05/1991	Madrid	Enrique Aguilar (car bomb; killed)	Military	1	7	PSOE	1967
Attack 4	01/13/1995	Vizcaya	Domingo Durán (shot; severely injured)	Police	1	4	PSOE	2130
	01/13/1995	Vizcaya	Rafael Leiva (shot; killed)	Police	1	4	PSOE	2130
Attack 5	01/23/1995	Guipúzcoa	Gregorio Ordónez (shot; killed)	Politician	2	11	PSOE	2131
Attack 6	04/10/1995	Guipúzcoa	Mariano De Juan Santa María (shot; killed)	Military	9	4	PSOE	2152
Attack 7	04/19/1995	Madrid	Jose María Aznar (car bomb; uninjured)	Politician	5	11	PSOE	2152
Attack 8	07/13/1997	Vizcaya	Miguel Angel Blanco (kidnapped and shot; killed)	Politician	2	5	PP	2254

Attacks 6 and 7 overlap with the same survey (CIS 2152). We split that survey in two non-overlapping parts and use each part for separate attacks.

Table S2. Timing of Attacks and General Elections in Spain

	Date of the attack	Days after last general election	Days before next general election
Attack 1	9/12/1989	1209	47
Attack 2	6/3/1990	217	1068
Attack 3	6/5/1991	584	7001
Attack 4	1/13/1995	617	415
Attack 5	1/23/1995	627	405
Attack 6	4/10/1995	704	328
Attack 7	4/19/1995	713	319
Attack 8	7/13/1997	436	973

Additional Information on the ETA Conflict

In Figure S1, we show the evolution of the conflict over time in terms of the number of ETA attacks between 1960 and 2006 (1). It shows that the late 1970s were the most violent years of this conflict and that ETA was quite active in the 1980s and 1990s. Our study focuses on attacks that ETA perpetrated between 1989 and 1997, a period in which the terrorist organization perpetrated attacks frequently. Figure S2 and Table S3 show the distribution of ETA's attacks across the different provinces of Spain. Álava, Guipúzcoa, and Vizcaya, the three provinces composing the Basque Country, in the central northern part of Spain, were the most severely affected by the conflict. Between 1960 and 2006, Guipúzcoa experienced 255 attacks (with 309 casualties), Vizcaya 172 attacks (with 209 casualties), and Alava 34 attacks (with 42 casualties). Navarra, which Basque nationalists consider part of *Euskal Herria*, suffered 35 attacks and 42 casualties. Among the 52 Spain's provinces, 30 escaped the direct consequences of the conflict and did not experience any attacks.

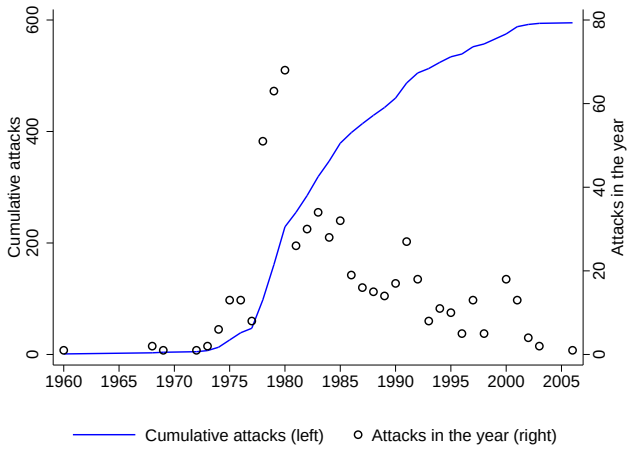


Fig. S1. Number of Terrorist Attacks by Year

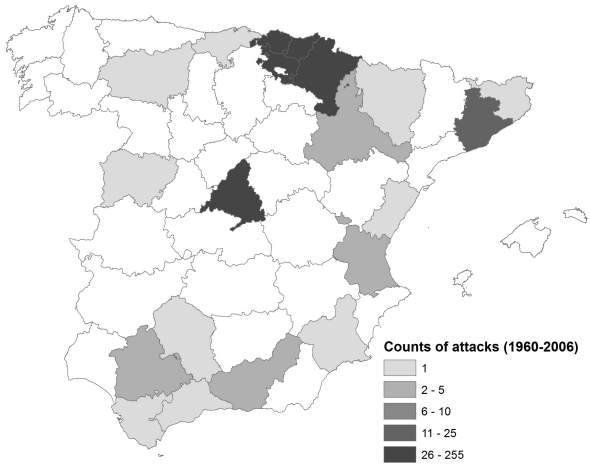


Fig. S2. Attacks by Province (1960-2006)

Table S3. Attacks and Victims by Province, 1960-2006

Province	Attacks	Victims
Guipúzcoa	255	309
Vizcaya	172	209
Madrid	53	123
Navarra	35	42
Álava	34	43
Barcelona	18	53
Sevilla	3	7
Granada	3	3
Valencia	3	3
Zaragoza	3	14
Alicante	2	5
Logroño	2	4
Salamanca	1	1
Cantabria	1	3
Cádiz	1	1
Murcia	1	1
León	1	1
Gerona	1	1
Málaga	1	1
Castellón	1	1
Huesca	1	2
Córdoba	1	1
Total	593	828

Note: The counts exclude the attacks that ETA perpetrated in France.

CIS Fieldwork Methodology

The CIS has a very decentralized fieldwork structure. Forty province coordinators receive simultaneous instructions and survey questionnaires from the head of fieldwork from Madrid (2). These province coordinators have several local enumerators working for them, to whom they distribute the materials and the assigned localities and sections within localities (i.e., a few streets within the locality) where they have to conduct the door-to-door interviews. Local enumerators do not have a particular order assigned for the interviews; they choose where to start and where to end the fieldwork depending on their individual logistical needs and preferences. Enumerators may go to different localities and sections of localities, but it is important to keep in mind that they are assigned specific localities and sections of localities, which they cannot skip. Within their assigned localities and sections, enumerators follow a "random route" system (what is called "sistema de rutas aleatorias") to find individuals. And while they need to fulfill gender and age quotas determined by the sampling, they have a lot of leeway on how to do it. For example, when they ring the bell of an apartment, they can choose who to interview in the apartment in order to fill their assigned quotas (they can only interview one person in each selected household, though). The activities of the different local and provincial coordinators take place simultaneously; there are many local enumerators working for the same provincial coordinator (Díaz de Rada (2015) refers to over 300 enumerators in total), so many of them do the interviews in different localities around the same time.

We have interviewed two individuals who held senior positions at the CIS during years in which ETA was active (the Head of the Research Department from 2005 to 2008 and the Head of the Research Department from July 2008 to November 2010), and they emphasized the fact that province coordinators and local enumerators have a lot of leeway to carry out the survey's interviews within the time frame they have to implement the fieldwork (usually, around one week). Importantly, the CIS employees we interviewed have confirmed that, as far as they are concerned, fieldwork plans were never adjusted in response to ETA attacks. Similarly, Díaz de Rada and Nuñez (3) collected experiences of enumerators with many years of work experience in the CIS, who were inquired about their fieldwork incidents. There are no mentions to ETA terrorist attacks as a cause for fieldwork disruption. Overall, there are no systematic patterns in the CIS fieldwork implementation that could challenge our research design.

Threats to Identification

We rely on the exogeneity of the timing of the attacks relative to that of the interviews as the basis for the identification of the causal effect of terrorism on electoral participation and support for the incumbent party. While it is implausible to believe that

the CIS and ETA would have coordinated their actions (see the CIS Fieldwork Methodology section), there are a number of potential biases that can pose a threat to our identification strategy. First, individuals of specific characteristics may be more likely to be reached at home to respond to the survey than others. If these characteristics that predict reachability are also predictive of the outcome, they would cast doubt on our estimation strategy. In Table S4, we show that there are not systematic differences in demographic attributes such as age, employment status, or educational attainment across individuals interviewed before and after the attacks. Additionally, for three of the surveys, we have information on the number of attempts that the interviewers made to reach a particular respondent; there are no significant differences before and after the attacks.

We also examine patterns in missing data and in non-response. We find that 3.27 percent of interviewees have missing data on the question that asks about their vote in a hypothetical election. In most cases, these individuals have also missing data on all or the majority of other questions (possibly because of the interview ended before it was completed). We examine whether missing data in the outcome variable are more likely among those interviewed after the attacks and find no evidence for this pattern. Similarly, 16.05 percent of interviewees declined to respond to the survey question that we use to measure our outcomes. We examine whether refusing to respond was more likely after the attacks and find no evidence that this was the case. In our analyses, we drop all individuals who refused to answer the question about voting.

An additional source of bias could arise from the non-random selection of provinces that were surveyed first. This is not a major concern because the CIS tried to conduct fieldwork simultaneously across the territory of Spain (see the CIS Fieldwork Methodology section). However, it could still be that the timing of fieldwork in some provinces was different from others, so we include a set of province-by-survey fixed effects, Z'_{ps}. This strategy ensures that we carry out the pre and post comparisons within surveys *and* within provinces (see section Assessing the Contribution of Provinces and Attacks to the Fixed Effects Estimation below). In addition, the vector of controls X'_{ips} includes dummies for the different categories of municipality size, which will address any concerns about when people living in small and large towns were surveyed in relation to the timing of the attacks.

Table S4 shows descriptive statistics for the demographic attributes that we observe in the surveys and that we include as controls: vote in past election, gender, age, education, employment status, and size of the municipality. Each of these attributes is coded from the surveys in categorical form, which enter the models as sets of dummy indicators. For each category, we show the mean for those interviewed before and after the attack and compute the difference in means across the two groups, conditioning on living in the same province. We report p-values for each of the differences in means. None of the p-values is smaller than .01 and only two are below .05. Because the t-tests for differences in means that we carry out within each of the attributes are not independent of each other, we also report results from F-tests of joint significance. To do so, we regress the treatment variable (i.e., an indicator for being interviewed after the attack) on the set of dummies that configure all categories of a given attribute (e.g., all indicators for educational attainment) and test for the joint significance of the set of dummies. The p-values from the F-tests of joint significance show that none of the demographic attributes of the interviewees predicts treatment status. Again, the F-tests include the set of province-by-survey fixed effects.

In a final test of covariate balance, we regress the treatment variable on *all* predictors shown in Table S4 (arbitrarily dropping one for each set of attributes) and the set of province-by-survey fixed effects. As reported at the bottom of the table, the p-value from the joint test of significance of all predictors is .18, suggesting that individuals interviewed before and after the attacks are statistically equivalent in terms of the attributes that we observe. All tests of significance shown in Table S4 are conducted within +/- 3 days of the day of the attacks. Examining covariate balance within 1 day and 5 days from the attack yields similar results in all tests of significance.

Overall, Table S4 shows a strong balance across the two groups along the majority of measured pre-treatment attributes. There are three characteristics, however, that show statistically significant differences across treatment and control units. Individuals interviewed after the attacks are 2 percentage points more likely to have voted in the previous election, 3 percentage points less likely to be uneducated, and 4 percentage points less likely to live in a city with at least 1,000,000 residents. Of these unbalanced attributes, the most problematic for our research design is the difference in reported participation in the past election. Although this difference is statistically significant, we believe that it is not substantively significant given the pre-attack mean in reported participation, 79%. Furthermore, turnout in the prior election is measured retroactively post-treatment, which raises the possibility of this difference being a result of reporting bias. To assess the implications of these pre-treatment differences, we report estimates with and without pre-treatment controls in all our models, and we test for unobservable selection into the treatment using the coefficient stability approach proposed by Oster (4).

Table S4. Covariate Balance Across Control and Treated Units

	Pre Attack	Post Attack	Diff. means	P-val
	Vote in previous election (F-test p-val =0.15)			
Voted PP	0.16	0.16	0.01	0.60
Voted PSOE	0.35	0.36	0.01	0.57
Voted PP	0.35	0.36	0.01	0.57
Voted IU	0.09	0.08	-0.01	0.09
Voted Basque	0.05	0.05	0.00	0.76
Voted Catalan	0.03	0.03	0.00	0.41
Voted Other	0.04	0.04	-0.00	1.00
Voted None	0.21	0.19	-0.02	0.03
Voted White	0.02	0.02	0.01	0.16
	Gender (F-test p-val = 0.72)			
Female	0.51	0.50	-0.01	0.19
	Age (F-test p-val = 0.81)			
Aged 18-34	0.39	0.39	0.01	0.40
Aged 35-64	0.46	0.46	-0.00	0.85
Aged 65-99	0.15	0.15	-0.01	0.38
	Education (F-test p-val = 0.11)			
No education	0.21	0.18	-0.03	0.03
Secondary education	0.68	0.71	0.02	0.06
College education	0.11	0.11	0.01	0.42
	Employment status (F-test p-val = 0.87)			
Employed	0.40	0.42	0.01	0.26
Retired	0.15	0.15	-0.00	0.57
Unemployed	0.12	0.11	-0.01	0.15
Student	0.09	0.09	0.00	0.91
Housewife	0.23	0.23	0.00	0.98
	Size of the municipality (F-test p-val = 0.32)			
Less 2,000	0.07	0.07	-0.00	0.94
2,000 to 10,000	0.14	0.12	-0.02	0.51
10,000 to 50,000	0.21	0.17	-0.04	0.18
50,000 to 100,000	0.08	0.11	0.03	0.21
100,000 to 400,000	0.18	0.23	0.05	0.18
400,000 to 1,000,000	0.09	0.11	0.02	0.19
More 1,000,000	0.12	0.08	-0.04	0.04
	All predictors combined (F-test p-val = 0.18)			

The sample is restricted to observations within + 3 days of the attack. The balance in covariates is robust to focusing on observations within + 1 and + 5 days of the attack.
Differences in means are computed using a regression of each covariate on the treatment variable *post* and the set of attack-by-province fixed effect. Tests of joint significance are carried by regressing the treatment variable on the corresponding set of predictors and attack-by-province fixed effect and then testing for the the joint significance of the predictors using a Wald test (F-test). All tests of individual and joint significance account for clustering within the primary sampling units of each study.

Results when Controlling for Unbalanced Pre-Treatment Covariates

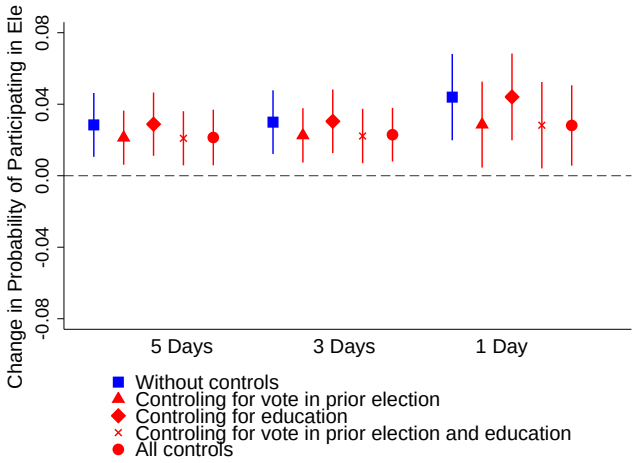


Fig. S3. Impact on Electoral Participation when Controlling for Unbalanced Pre-Treatment Covariates

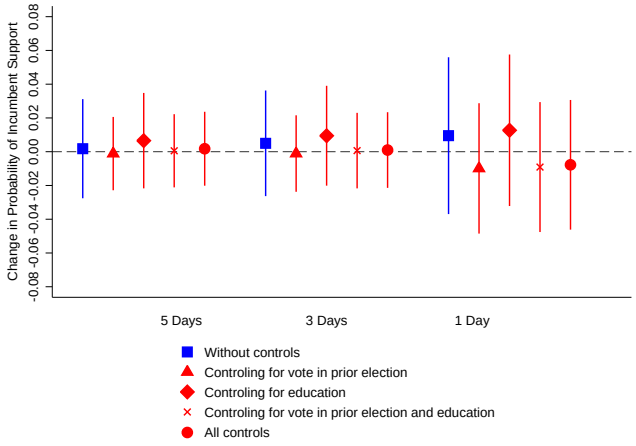


Fig. S4. Impact on Incumbent Support when Controlling for Unbalanced Pre-Treatment Covariates

Difference-in-Differences Estimation

In Table S5 we present results from a difference-in-difference estimation strategy similar to the one used in other studies of terrorism and electoral outcomes (5, 6). To do so, we aggregate the individual-level data up to the province level. For each survey and province, we compute the percentage of respondents who answered that they would participate in the election before and after the attacks. We also compute, for each survey and province, the percentage of respondents who answered that they did not vote in the prior election—also before and after the attacks. Each observation in the data set represents a pre-/post-attack measure of participation (which we index as i) for each province (indexed as p) and survey (indexed as s). We then estimate the following model:

$$(\%\ \text{Participate})_{ips} = \delta\ Post_{ips} + \boldsymbol{W_p'}\boldsymbol{\gamma} + \boldsymbol{Z_s'}\boldsymbol{\theta} + e_{ips} \quad [1]$$

$(\%\ \text{Participate})_{ips}$ is the percentage of individuals reporting participation measured in a given pre/post attack period (i.e., measured before attacks when $i = 0$ and measured after when $i = 1$) in province p and survey s. $Post_{ips}$ is an indicator for whether the outcome is measured before or after the attacks. $\boldsymbol{W_p'}$ are province fixed effects, and $\boldsymbol{Z_s'}$ are survey fixed effects.

The model can include a control for the percentage of individuals that did not vote in the previous election measured in a given pre-/post-attack period i in province p and survey s, $(\%\ \text{Absenteeism})_{ips}$.

We run three specifications for each outcome: Models 1 and 4 regress the outcome on a post-attack indicator, province fixed effects, and survey fixed effects; Models 2 and 5 add the percentage of respondents who answered that they did not vote in the prior election as a control; and Models 3 and 6 add the full set of controls that are listed in Table S4 (also measured as percentages). Comparing the estimates from S5 to those from Figs. 1 and 3, we find that our results are robust to using this alternate model specification.

Table S5. Difference-in-Differences Estimation Using Aggregated Data

	% Will Participate in Election			% Incumbent Support		
	(1)	(2)	(3)	(4)	(5)	(6)
Post attacks (0,1)	2.266**	1.709**	1.886**	0.520	0.171	-0.460
	(0.917)	(0.722)	(0.899)	(1.581)	(1.472)	(1.610)
% Did not vote last election		-0.503***	-0.342***		-0.315***	-0.000
		(0.040)	(0.123)		(0.088)	(0.219)
Observations	330	330	330	330	330	330
Adj-R2	0.337	0.582	0.575	0.572	0.594	0.663
Controls	No	No	Yes	No	No	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Attack fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors by province in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. When included, the set of controls are the same ones than those in Table S4 (in % form). All models are weighted by the number of respondents.

Additional Plots of Effect Heterogeneity

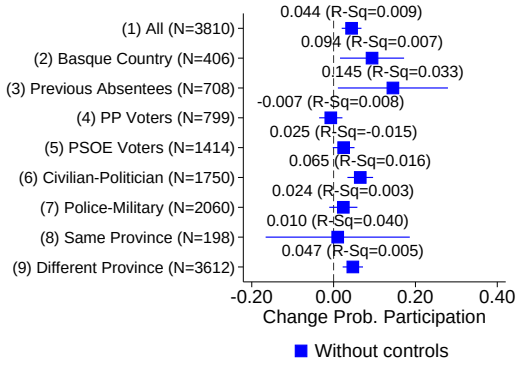


Fig. S5. Heterogeneity in Activation Effect, Within 1 Day, Without Controls

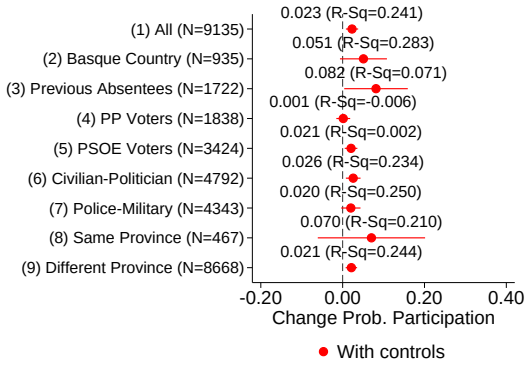


Fig. S6. Heterogeneity in Activation Effect, Within 3 Days, With Controls

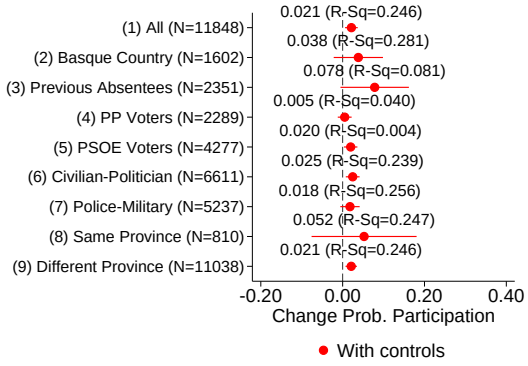


Fig. S7. Heterogeneity in Activation Effect, Within 5 Days

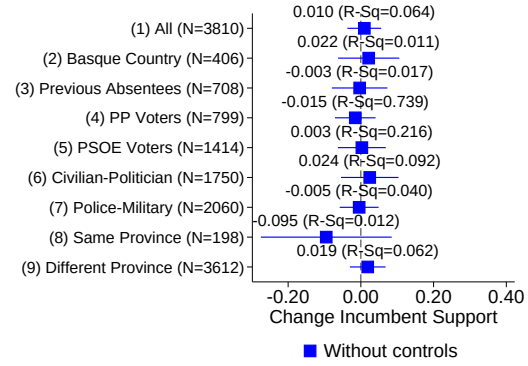


Fig. S8. Heterogeneity in Incumbent Effect, Within 1 Day, Without Controls

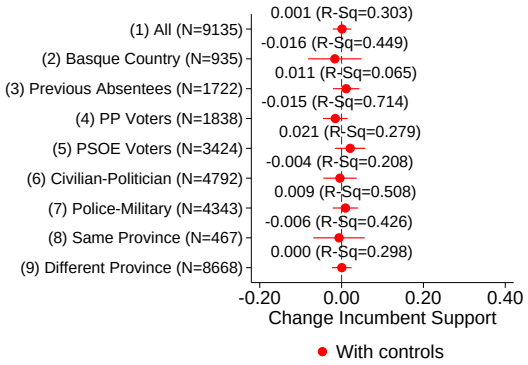


Fig. S9. Heterogeneity in Incumbent Effect, Within 3 Days, With Controls

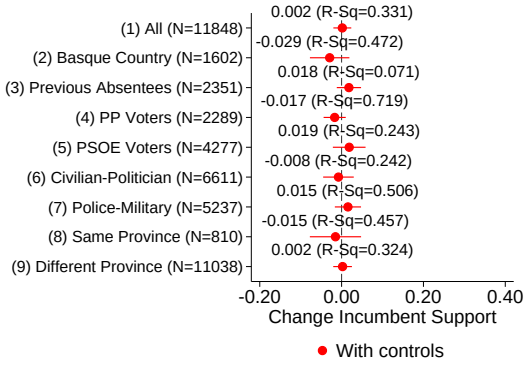


Fig. S10. Heterogeneity in Incumbent Effect, Within 5 Days, With Controls

Table S6. Interaction by Type of Victim of Effect of Attacks on Participation, Within 1 Day

	(1)	(2)
Post Attacks	0.028**	0.046***
	(0.011)	(0.014)
Post Attacks x Victim is Police/Military		-0.034*
		(0.020)
Observations	3,810	3,810
Adj-R2	0.217	0.217
Controls	Yes	Yes
Province fixed effects	Yes	Yes
Attack fixed effects	Yes	Yes

Clustered standard errors by province and municipality size in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. When included, the set of controls are the same ones than those in Table S4.

Effect on Vote Switches

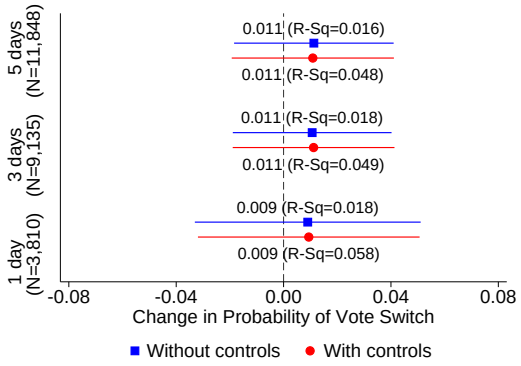


Fig. S11. Effect on the Probability of Changing Vote

Sensitivity Analyses

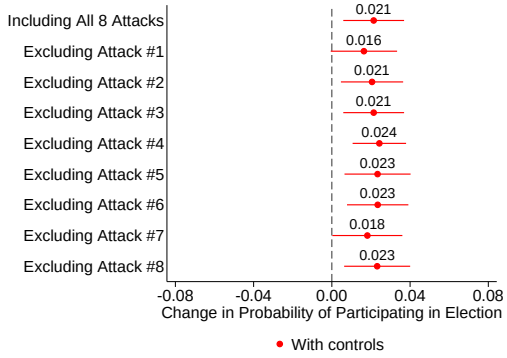
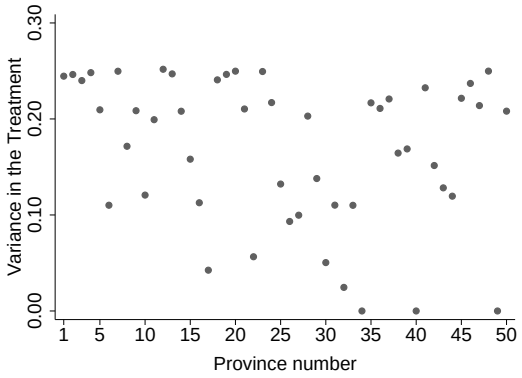


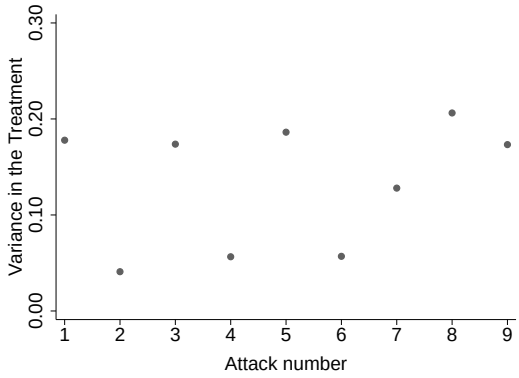
Fig. S12. Impact on Electoral Participation when Excluding Single Attacks

Assessing the Contribution of Provinces and Attacks to the Fixed Effects Estimation

A well-known property of the OLS fixed effects estimator is that groups with more variation in treatment assignment will have more weight in the estimation (7). In this case, more weight will be given to provinces where there are balanced numbers of before- and after-attack interviewees, as opposed to provinces where most interviews falling before or after the date of attack. Similarly, the estimator will give more weight to surveys in which there is a more balanced number of before- and after-attack interviewees. In Fig. S13, we show that, although there is variation in the degree to which the treatment varies within provinces and surveys, no outliers appear to be driving the estimation.



(a) Treatment Variation within Provinces



(b) Treatment Variation within Attacks

Fig. S13. Treatment Variation within Fixed Effects Units

Robustness to Alternative Clustering Choices

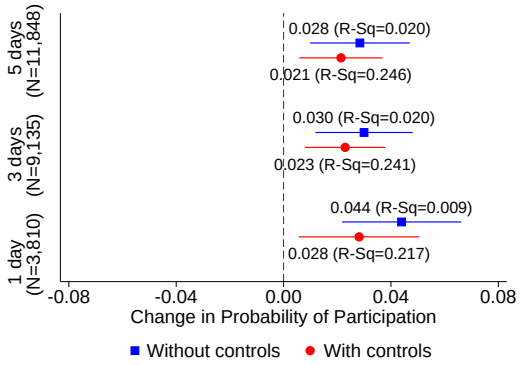


Fig. S14. Impact on Electoral Participation when Clustering Standard Errors by Province

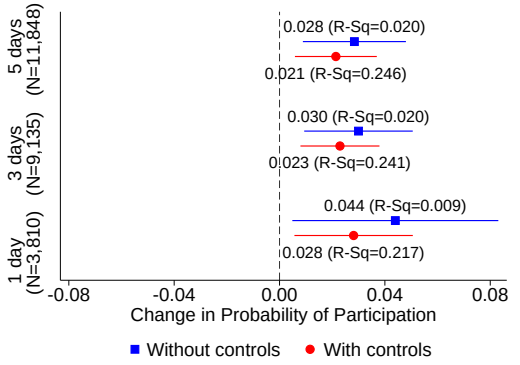


Fig. S15. Impact on Electoral Participation when Clustering Standard Errors by Survey

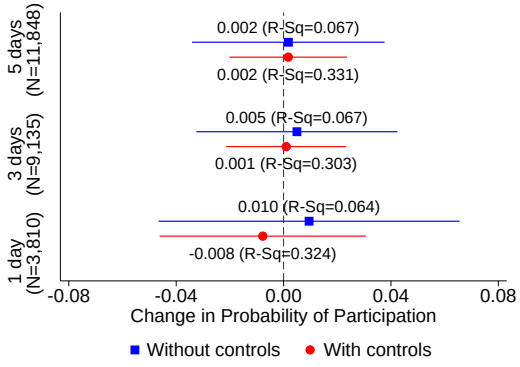


Fig. S16. Impact on Incumbent Support when Clustering Standard Errors by Province

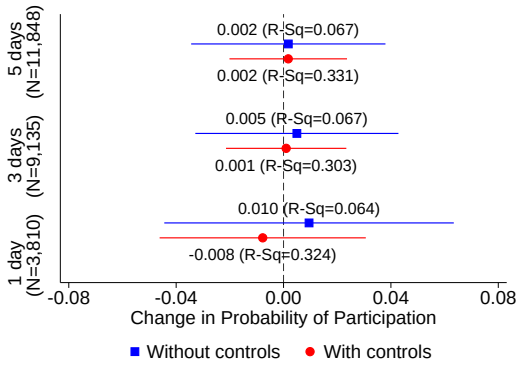


Fig. S17. Impact on Incumbent Support when Clustering Standard Errors by Survey

Exact Wording of Questions on Views on Public Safety and Criminal Justice Policies (Fig. 4)

- (1) Concerned about Safety: “The main concern is safety”
- (2) Against Shortening Prison Sentences: “Depending on the type of crime and on inmates’ behavior, it would be good to shorten sentences to facilitate the reintegration of inmates” – Reverse coded
- (3) Concerned about Lack of Civility: “The main concern is lack of civility in society”
- (4) Individual Liberty is Most Important Value: “The most important value in a society is individual liberty” – Reverse coded
- (5) Prisons Should Punish Criminals: “The main function of prisons should be to punish criminals”
- (6) Against Furlough Privileges: “Are you supportive of granting inmates furlough privileges?”– Reverse coded
- (7) Concerned about Drugs: “The main concern is drug use in society”
- (8) Prisons Should Deter Criminals: “The main function of prisons should be to deter potential criminals”
- (9) Against Transitional Leave Programs: “Are you supportive of short-term transitional leave programs to facilitate inmates’ reintegration into the community?”– Reverse coded
- (10) Public Safety is Most Important Value: “The most important value in a society is public safety”
- (11) Prisons Shouldn’t Rehabilitate Criminals: “The main function of prisons should be to rehabilitate criminals”– Reverse coded
- (12) Concerned about Terrorism: “The main concern is terrorism”
- (13) We Need Law and Order: “In a scale from 1 to 10, position yourself between these two statements: ‘To reduce delinquency we need more education and jobs’ (0) and ‘To reduce delinquency we need more law and order’ (10)”
- (14) Criminals Will Reoffend: “In a scale from 1 to 10, position yourself between these two statements: ‘Criminals can be rehabilitated and reintegrated into society’ (0) and ‘Criminals have a tendency to re-offend’ (10)”
- (15) Prisons Should Protect Society: “The main function of prisons should be to protect of society”

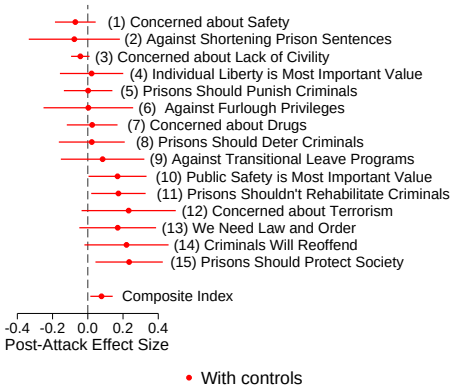


Fig. S18. Effect of Attack against J.M. Aznar on Views on Public Safety and Criminal Justice Policies, With Controls

Reported Turnout and Actual Turnout

One of the limitations of using survey data is that turnout may be overreported in surveys (8). We look at actual turnout in the national elections that took place after the attacks and compare it to the turnout estimated in the CIS surveys we are analyzing; of course, there is variation in the timing between the attacks and the national elections. As we have shown in Table S2 above, some elections took place a few months after the attacks while others took place after a few years.

In Table S7 below, we show that all the surveys in our study tend to overestimate actual turnout in Spanish national elections that happened after the surveys were fielded. However, they tend to overestimate turnout less than other CIS surveys. For example, for the year 1996 elections, in which turnout was 77.38% the CIS pre-electoral survey predicted a turnout of 91.3%. The different surveys that we use for the period preceding the 1996 elections predict lower turnout (between 81.9% and 87.9%). The same thing happened in 1993. For the year 2000, the turnout estimated in our survey is very similar to the one estimated in the CIS survey that was fielded right before the national elections. For the 1989 election, our survey overestimates turnout more than the CIS pre-electoral survey, but this might be driven by the fact that our survey is in this case regional (of Castilla La-Mancha) and it is not nationally representative. Overall, it is clear that reported turnout does not exactly correspond to actual turnout, which generally tends to be lower, but that this overreporting is affecting our surveys less than CIS pre-electoral surveys, which tend to have more overreporting perhaps due to closeness to the election and to the electoral campaign.

Table S7. Estimated Participation and Actual Participation in the Relevant General Elections

1989 Election	
Actual turnout	69.74%
Turnout in CIS pre-electoral survey	86.00%
Turnout in survey used in Attack #1	90.56%
1993 Election	
Actual turnout	76.44%
Turnout in CIS pre-electoral survey	91.20%
Turnout in survey used in Attack #2	86.30%
Turnout in survey used in Attack #3	86.46%
1996 Election	
Actual turnout	77.38%
Turnout in CIS pre-electoral survey	91.30%
Turnout in survey used in Attack #4	87.97%
Turnout in survey used in Attack #5	83.68%
Turnout in survey used in Attack #6	81.92%
Turnout in survey used in Attack #7	81.92%
2000 Election	
Actual turnout	68.71%
Turnout in CIS pre-electoral survey	88.59%
Turnout in survey used in Attack #8	88.17%

The CIS survey ID for Attack #1 is 1836, the CIS survey ID for Attack #2 is 1873, the CIS survey ID for Attack #3 is 1967, the CIS survey ID for Attack #4 is 2130, the CIS survey ID for Attack #5 is 2131, the CIS survey ID for Attack #6 is 2152 (first half), the CIS survey ID for Attack #7 is 2152 (second half), and the CIS survey ID for Attack #8 is 2245.
The survey ID for the CIS pre-electoral surveys are 1838 (1989 Election), 2060 (1993 Election), 2207 (1996 Election), and 2383 (2000 Election).

Coverage of the Attacks in the Spanish Media Outlets

One of the key assumptions of our research design is that, regardless of where each attack occurred, individuals from all provinces in Spain were potentially exposed to the attacks through their coverage in national media outlets. To examine the extent to which this was true, we analyze the media coverage of each attack in the five newspapers with the highest circulation in the country, the Spanish public radio (RNE), and the main public television channel (RTVE). We use standard methodology in media analysis (9) to examine the coverage that each attack received. In Tables S8 to S10 and Figs. **??** to **??**, we show the minutes of footage that each attack received on the national public radio and TV news programs as well as their coverage in the major national newspapers. In Table S11, we provide links to the front covers of the major newspapers in the day after the

attacks.

Except for one (the assassination of Francisco Almagro Carmona on June 3, 1990), all attacks in our study appeared on the front pages of all five newspapers. In addition, most of the attacks were covered by the newspapers for several consecutive days, and the most prominent ones, those of politicians José María Aznar, Miguel Angel Blanco, and Gregorio Ordoñez, were covered in the newspapers over 10 consecutive days. In addition, all the attacks were covered by radio news, and except for one, all received substantial coverage in the television news summaries.

Coverage of the Attacks in National Media Outlets

Table S8. Order of Appearance and Minutes of Footage in the National Public Television (TVE) News

	2:00pm news		9:00pm news		Consecutive days
	Position	Minutes	Position	Minutes	appearing[1]
Attack 1	3	3	4	3.1	0
Attack 2	0	0	0	0	0
Attack 3	1	6	0	0	0
Attack 4	1	4	1	2	1
Attack 5	1	25	1	17	0
Attack 6	1	7	1	5.2	1
Attack 7	1	19	1	14	7
Attack 8	1	36	1	77.6	9

[1] We examine appearance in the consecutive days by looking at the coverage that the attack received in the 2:00pm news summary, which is the one with the largest audience.

Table S9. Order of Appearance and Minutes of Footage in the National Public Radio (RNE) News

	8:00am news		2:00pm news		8:00pm news		Midnight news		Consecutive days
	Position	Minutes	Position	Minutes	Position	Minutes	Position	Minutes	appearing[1]
Attack 1	2	11	3	3.5	2	5.9	3	3	1
Attack 2	8	2	7	2	12	0.1	0	0	0
Attack 3	2	7	2	1.6	5	1	13	0.2	0
Attack 4	1	4	1	6	1	5.5	1	4	2
Attack 5	1	19.5	1	20.5	1	13	1	12	8
Attack 6	1	6	2	5	2	3.5	2	4	2
Attack 7	1	15	1	10.5	1	10	1	14.5	7
Attack 8	1	39.3	1	70	1	30	1	120	16

[1] We examine appearance in the consecutive days by looking at the coverage that the attack received in the 2:00pm news summary, which is the one with the largest audience.

Table S10. Coverage of the Attacks in National Newspapers

	El País				El Mundo				ABC				La Vanguardia				El Periódico			
	Front page	Position	Pages	Consec. days	Front page	Position	Pages Pages	Consec. days	Front page	Position	Pages	Consec. days	Front page	Position	Pages	Consec. days	Front page	Position	Pages	Consec. days
Attack 1	1	1	2	2					1	1	4	1	1	1	1	2	1	1	2	1
Attack 2	0	1	1	1	1	1	2	2	0	0	1	2	0	0	1	1	0	0	1	1
Attack 3	1	2	1	2	1	1	2	2	1	1	3	1	1	2	1	2	1	2	2	2
Attack 4	1	3	1	3	1	2	2	3	0	0	2	3	1	1	1	3	1	1	2	4
Attack 5	1	1	4	11	1	1	5	12	1	1	8	23	1	1	2	5	1	1	3	10
Attack 6	1	2	1	2	1	1	2	3	1	2	3	5	1	1	1	4	1	1	2	4
Attack 7	1	1	9	15	1	1	10	4	1	1	13	17	1	1	6	9	1	1	6	4
Attack 8	1	1	11	17	1	1	12	34	1	1	20	32	1	1	7	8	1	1	9	12

Table S11. Links to Front Covers of Major Newspapers in the Day After the Attacks

Attack 1
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1989/09/13/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1989/09/13/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1989/09/13/pagina-1/33075051/pdf.html
Attack 2
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1990/06/04/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1990/06/04/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1990/06/04/pagina-1/33021243/pdf.html
Attack 3
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1991/06/06/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1991/06/06/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1991/06/06/pagina-1/33472300/pdf.html
Attack 4
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1995/01/14/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1995/01/14
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1995/01/14/pagina-1/34428654/pdf.html
Attack 5
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1995/01/24/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1995/01/24
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1995/01/24/pagina-1/33790539/pdf.html
Attack 6
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1995/04/11/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1995/04/11/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1995/04/11/pagina-1/33791033/pdf.html
Attack 7
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1995/04/20/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1995/04/20/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1995/04/20/pagina-1/34434340/pdf.html
Attack 8
ABC: http://hemeroteca.abc.es/nav/Navigate.exe/hemeroteca/madrid/abc/1997/07/13/001.html
El País: https://elpais.com/hemeroteca/elpais/portadas/1997/07/13/
El Mundo: http://www.elmundo.es/1997/07/13/
La Vanguardia: http://hemeroteca.lavanguardia.com/preview/1997/07/13/pagina-1/34636080/pdf.html

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