Voting after the bombings : A natural experiment on the effect of terrorist attacks on democratif elections.
José G. Montalvo (2011)

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1 Introduction

On Saturday, April 26, 2025, a motorist crashed into a Filipino community gathering in Vancouver. This tragedy took place on the eve of the country's general election. Conservative candidate Pierre Poilievre, campaigning against Liberal candidate and Canadian Prime Minister Marc Carney, had made safety one of his main themes. In the end, the Liberal Party won the majority of seats. However, this majority was tenuous – 43.76% against 41.31% for the Conservative Party[1]. One may wonder whether the ram-raid attack in Vancouver had a last-minute effect on Canadians' votes.
Economists and political scientists have a long-standing interest in the effect of extraordinary events such as mass attacks, and particularly terrorist ones. For instance, a strong effect of the September 11 terrorist attack is suspected on the outcome of the 2004 US presidential elections.

In his article published in the Review of Economics and Statistics in November 2011, José G. Montalvo (2011) looks at the effect of the terrorist attacks in Madrid on March 11, 2004. At the time, bombs planted by Islamists killed almost 200 people and injured nearly 1,900. These attacks took place 3 days before the national general elections, for which the two major competing parties were the PP (*Partido Popular*, a conservative right-wing Christian Democrat party) and the PSOE (*Partido Socialista Obrero Español*, a progressive center-left Social Democrat party).
Eventually, the PP came second - with 37.71% of the vote to the PSOE's 42.59% - even though the polls had been in its favor in the weeks before the elections (Table 1)[2]. The popular press argued that it could have been because the PP was in favor of the war in Iraq, while the PSOE was not.

	% Conservative	% Socialist	Difference
January	42.92	36.32	6.60
February	42.90	36.35	6.55
Before March 7	42.36	37.60	4.76
Election March 14	37.71	42.59	-4.88

Table 1: Monthly average percentages of votes for different polls

Two research studies have already addressed this issue: Lago and Montero (2005); Bali (2007). Both use data from postelectoral surveys, but have different conclusions. The use of this type of data is indeed mainly limited for two reasons:

- People who voted for the winning party may be over-represented;
- Respondents may display cognitive dissonance when questions are asked about their past actions.

It is therefore important to propose an alternative method, which does not use postelectoral data. This is what Joé G. Montalvo does in his 2011 article, and we will attempt to replicate his work.

[1] https://www.radiofrance.fr/franceinter/podcasts/l-info-de-france-inter/l-info-de-france-inter-7888751
[2] Data compiled by José G. Montalvo from studies by the following statistical institutes: Sigma Dos, Noxa, Opina, Vox Publica, Gallup, Celeste Tel, Metra Seis, Citigate Sanchis, Demoscopia, CIS, and Ipsos-Eco Consulting.

2 Data

The data chosen by José G. Montalvo are the general election results themselves. Indeed, the timing of the terrorist attack – very close to the election – makes this event a natural experiment. While Spanish residents were aware of the attack on voting day, this was not the case for Spaniards living abroad, who voted before the attack. Thus, the voting data can be separated into two groups, the "treated" group and the "control" group. The votes of these two groups can be compared to estimate the effect of the attack on electoral results. It's important to bear in mind that this effect includes not only the attack itself, but also the management of this crisis by the Conservative government at the time.

Our data come from the database provided on the website of the Spanish Ministry of Interior [3]. They include Spanish general election results for 5 years (1989, 1993, 1996, 2000 and 2004) and for the 52 Spanish regions. The data also enable us to distinguish, for each region, between voters resident and non-resident in Spain. Voters living abroad are identified by their registration with a Spanish consulate.

In terms of identifying socialist and conservative parties, we decided to adopt a different strategy from José G. Montalvo's, to see if this had any influence on the results. Indeed, we thought it would be interesting to see whether the terrorist attack had had an effect on the conservative vote and socialist and/or progressive vote considered as a whole, rather than considering only PSOE and PP voters.
We therefore constructed a party classification to group together the main social-democrat parties, and did the same for parties with a centrist-conservative orientation.

3 Methodological Framework: Principles and Identification Strategy

Empirical Objective and Justification of the DiD

This study seeks to measure the causal effect of the March 11, 2004 Madrid terrorist attacks on voting behavior in Spain's general election. The attacks occurred just three days before the election, providing an exogenous shock to public sentiment. Our identification strategy leverages the fact that two electorates were exposed to different information sets:

- **Treatment group**: Spanish residents who voted *after* the attacks (on March 14).
- **Control group**: Spanish citizens living abroad who cast their votes *before* the attacks (between March 2 and 7).

This quasi-experimental setting enables a Difference-in-Differences (DiD) approach that compares changes in the vote ratio between the conservative party (PP) and the socialist party (PSOE) over time between the two groups (Imai et al., 2012). The average treatment effect on the treated (ATT) captures how the attacks influenced domestic voter behavior compared to counterfactual evolution inferred from expatriate voting patterns Angrist and Pischke (2009).

Econometric Model and Key Assumptions

The estimated baseline DiD specification is:

$$\text{ratio}_{it} = \alpha + \beta \cdot \text{treated}_i + \gamma \cdot \text{post}_t + \delta \cdot (\text{treated}_i \times \text{post}_t) + \varepsilon_{it} \tag{1}$$

where:

- ratio_{it}: Conservative-to-socialist vote ratio in province i and year t
- treated_i: Dummy equal to 1 if observation i is a resident voter
- post_t: Dummy equal to 1 for election year 2004 (post-attacks)
- δ: DiD coefficient estimating the treatment effect

To improve identification and address omitted variable bias, we extend this model by including two-way fixed effects:

$$\text{ratio}_{it} = \delta \cdot \text{did}_{it} + \lambda_i + \theta_t + \varepsilon_{it} \tag{2}$$

where λ_i and θ_t are fixed effects for provinces and years, respectively.

This specification is aligned with the canonical DiD framework (Angrist and Pischke, 2009; Wooldridge, 2021).

[3] https://infoelectoral.interior.gob.es/es/elecciones-celebradas/resultados-electorales/

Interpretation: δ captures the differential change in the vote ratio for treated units (domestic voters) in the post-treatment period (2004), relative to the control group (expatriates), net of province-specific and time-specific effects.

Identification Conditions and Validity Checks

The validity of the DiD strategy is based on the following identification assumptions and based on (Callaway and Sant'Anna, 2021; Mora and Reggio, 2012) researches :

1. **Parallel Trends Assumption**: In the absence of treatment (i.e., the terrorist attack), the treated and control groups would have experienced the same time trend in vote shares. This is crucial for δ to be interpreted as a causal effect.

2. **No Anticipation or Spillovers**: Expatriates must not have been influenced by pre-election rumors or early information about the attack.

3. **Stable Composition**: The groups must be compositionally stable across periods. Any systematic change in turnout or group characteristics could bias the estimate.

Systematic differences in education, media exposure, or political engagement between residents and expatriates could bias estimates (Lago and Montero, 2006; Mora and Reggio, 2012). However, this result warrants caution, as even minor pre-trend violations can invalidate DiD estimates (Roth, 2022).

We test these assumptions via:

- **Pre-trend Visual Test**: Figure 1 shows the evolution of vote ratios prior to 2004. The trends for both groups are visibly parallel, which supports the identification strategy.

- **Placebo Test (Year 2000)**: A falsification DiD using 2000 as a pseudo-treatment year shows a small and marginally significant effect (+0.14*), suggesting a limited risk of spurious identification.

R Code for DiD Model Estimation (verbatim)

```
library(fixest)
# Main DiD estimation with two-way fixed effects
mod_fe <- feols(ratio ~ did | lieu + annee, data = votes_did)
summary(mod_fe)

# Cluster-robust standard errors
mod_fe_cluster <- feols(ratio ~ did | lieu + annee,
cluster = ~lieu, data = votes_did)

# Log-transformed outcome
mod_log <- feols(log_ratio ~ did | lieu + annee,
data = votes_did)

# Placebo test (pseudo-treatment year = 2000)
mod_placebo <- feols(ratio ~ did_placebo | lieu + annee,
data = votes_did)
```

In conclusion, the DiD design used in this replication leverages the unique timing of the 2004 Spanish elections to cleanly identify the electoral consequences of the Madrid bombings. By ensuring the parallel trends assumption, accounting for province-level heterogeneity, and conducting placebo checks, we are able to replicate Montalvo's original findings with high credibility.

4 Replication of the Difference-in-Differences Strategy

This section documents the replication of Montalvo's (2011) core empirical strategy using a Difference-in-Differences (DiD) design. We describe the specification and implementation of the model, discuss the econometric assumptions and their empirical support, and compare our estimated effects with those reported in the original article. The analysis is structured to reflect both methodological rigor and applied econometric logic. Replication code was written in `R`, and key steps are presented inline for transparency.

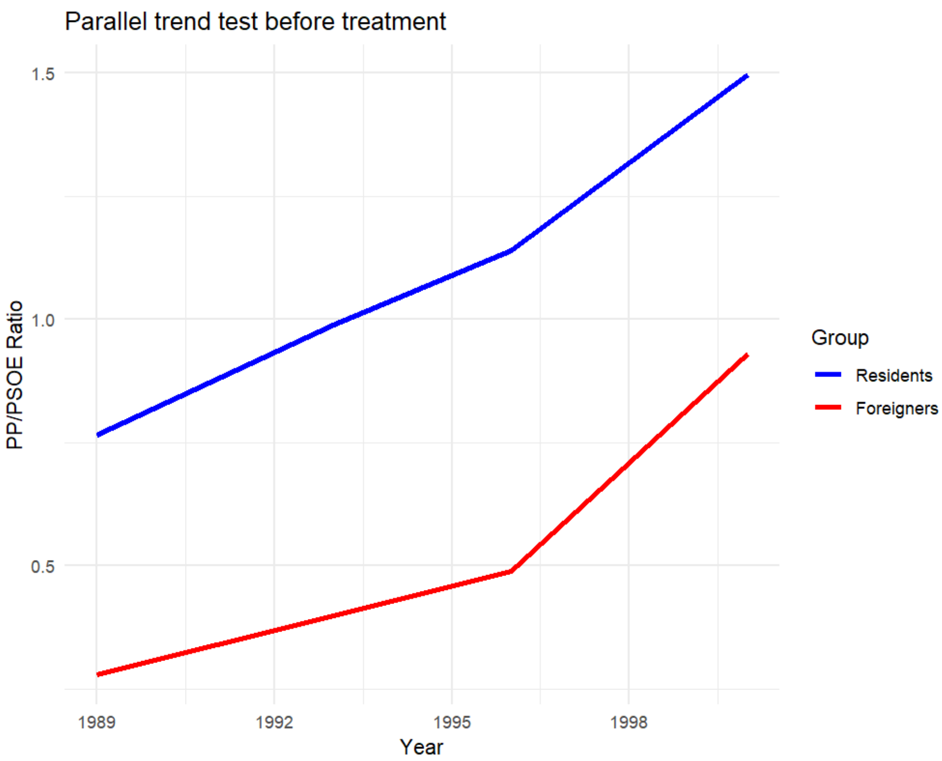

Figure 1: Pre-treatment Trends in Vote Ratios (1993–2000)

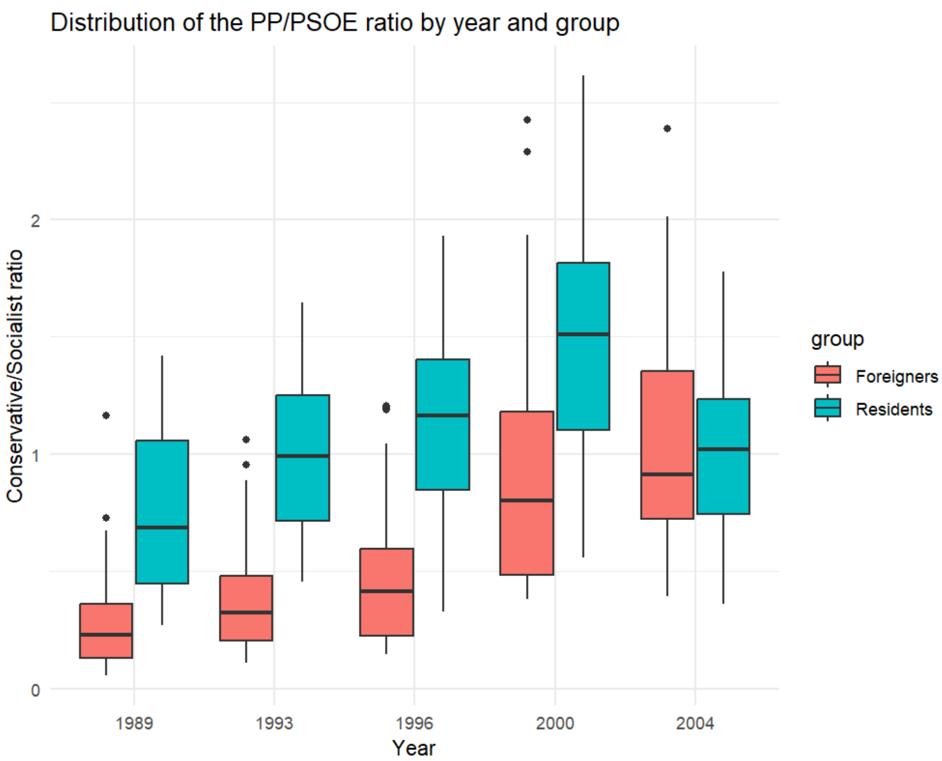

Figure 2: Vote Ratio Distributions by Group and Year

4.1 Data Preparation and Variable Construction

The raw electoral data consist of 7,564 rows across multiple elections (1989–2004), aggregated at the province-year level. Although data cleaning was initially carried out by a teammate, we adapted it specifically for the DiD setting by restructuring the outcome variable and treatment indicators. Our main variable of interest is the ratio of conservative to socialist votes:

$$ratio_{it} = \frac{votes_{PP}}{votes_{PSOE}} \tag{3}$$

This measure reflects the relative balance of support between the two major Spanish parties in a given province i and year t. Using ratios (rather than levels or shares) offers a scale-invariant measure robust to variations in turnout and province size.

Key variables constructed include:

- `treated`: equals 1 for resident voters (who voted on March 14, post-attack), 0 for expatriates (who voted before March 11);
- `post`: equals 1 for year 2004, 0 otherwise;
- `did`: interaction term `treated * post` capturing the DiD effect;
- `log_ratio`: logarithmic transformation of the ratio to address skewness and heteroskedasticity.

Code reference:

```
votes_did <- votes_agg %>%
mutate(
treated = ifelse(etranger == 0, 1, 0),
post = ifelse(annee == 2004, 1, 0),
did = treated * post,
log_ratio = log(ratio)
)
```

The histogram of `ratio` revealed a long right tail, with values ranging from approximately 0.05 to 2.65.

The logarithmic transformation of the outcome produced a distribution closer to normality (Figure 4), consistent with best practices in applied work (Wooldridge, 2010).

4.2 Testing the DiD Assumptions

We test the key identification assumption of DiD: *parallel pre-treatment trends*. Using group-level averages, we visualize the evolution of the `ratio` over elections prior to 2004 (Figure **??**). Visual evidence supports a parallel evolution between expatriate and resident voters from 1989 to 2000.

Code reference:

```
votes_did %>%
filter(annee < 2004) %>%
group_by(annee, etranger) %>%
summarise(ratio_moy = mean(ratio)) %>%
ggplot(...) + ...
```

To reinforce this result, we estimate a placebo DiD using 2000 as a pseudo-treatment year:

```
mod_placebo <- feols(ratio ~ did_placebo | lieu + annee, cluster = ~lieu, data = votes_did)
```

The estimated placebo effect is `0.143` with a p-value of `0.016`, which, while statistically significant, is smaller in magnitude and directionally opposite to the 2004 effect. This finding is important, it may indicate a partial violation of the parallel trends assumption, which is a substantial limitation in DiD designs with non-randomized groups (Angrist and Pischke, 2009; Mora and Reggio, 2012). Such results echo concerns raised in the literature about group comparability and unobserved heterogeneity.

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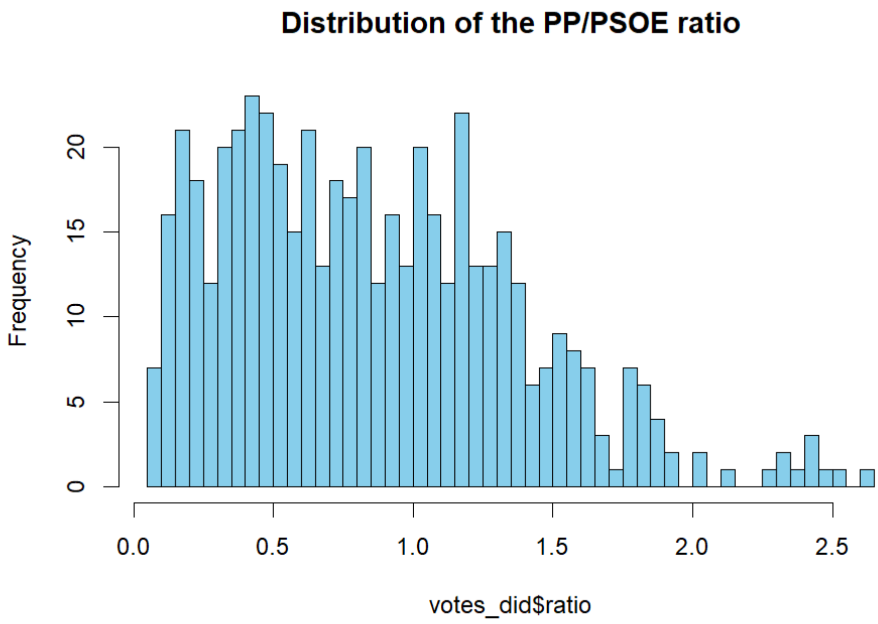

Figure 3: Distribution of the PP/PSOE ratio

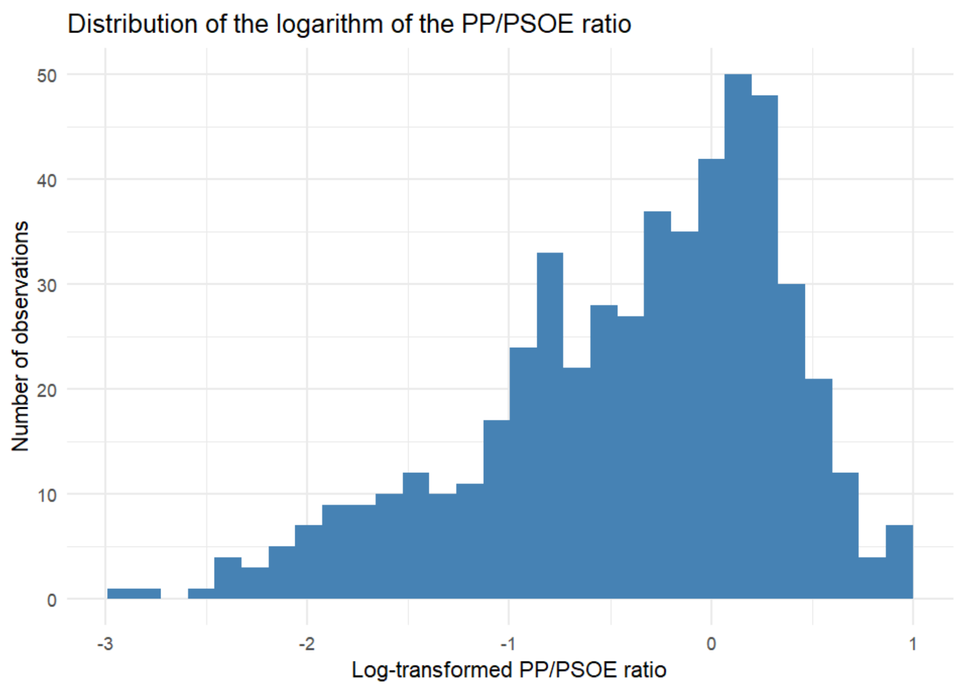

Figure 4: Distribution of the PP/PSOE ratio

4.3 Model Estimation and Specification

We estimate the DiD effect under four model specifications:

1. Simple OLS:

```
lm(ratio ~ treated + post + did, data = votes_did)
```

2. Fixed Effects (province and year):

```
feols(ratio ~ did | lieu + annee, data = votes_did)
```

3. Clustered standard errors:

```
feols(ratio ~ did | lieu + annee, cluster = ~lieu, data = votes_did)
```

4. Log-transformed dependent variable:

```
feols(log_ratio ~ did | lieu + annee, cluster = ~lieu, data = votes_did)
```

Main result:

```
did = -0.605 (SE = 0.037, p < 0.001)
log_did = -0.920 (SE = 0.040, p < 0.001)
```

All specifications consistently point to a significant and substantial decline in PP/PSOE ratio among residents in 2004. The log-specification indicates a nearly 92

4.4 Electorally Weighted Estimation

To capture heterogeneity in province size, we weight the regression by the total number of votes cast for PP and PSOE:

```
votes_did <- votes_did %>%
mutate(weight = conservateur + socialiste)
feols(ratio ~ did | lieu + annee, weights = ~weight, cluster = ~lieu, data = votes_did)
```

This yields a more negative effect (`did = -0.681, SE = 0.076, p < 0.001`), suggesting that the effect of the attack was more pronounced in populous constituencies. Such weighting is consistent with practices in vote-share analyses (Gelman and Hill, 2007).

Table 2: Weighted DiD Model Results

Variable	Estimate	Std. Error	p-value
`did` (treatment effect)	−0.681	0.076	$< 0.001^{***}$
Model diagnostics			
Observations	520		
Fixed Effects	Province (104) and Year (5)		
Weights	Total votes for PP and PSOE per province-year		
Standard Errors	Clustered at province level		
RMSE	55.9		
Adjusted R^2	0.878		
Within R^2	0.043		

Note: *** significant at 1%. Outcome: vote ratio PP/PSOE.

4.5 Comparative Effect Estimates: Montalvo (2011) vs. Our Replication

Montalvo reports a DiD estimate of approximately −0.60, with a 95

These findings are in line with previous research showing a strong negative impact of the 2004 Madrid bombings on the incumbent party, but the magnitude and direction of such effects can vary across countries and electoral contexts (Bali, 2007; Lago and Montero, 2006; Baccini et al., 2021). The natural experiment design, leveraging the exogenous timing of absentee voting, is particularly robust, but the partial significance of the placebo test suggests that parallel trends may not be fully satisfied, echoing concerns raised in the literature about group comparability and unobserved heterogeneity.

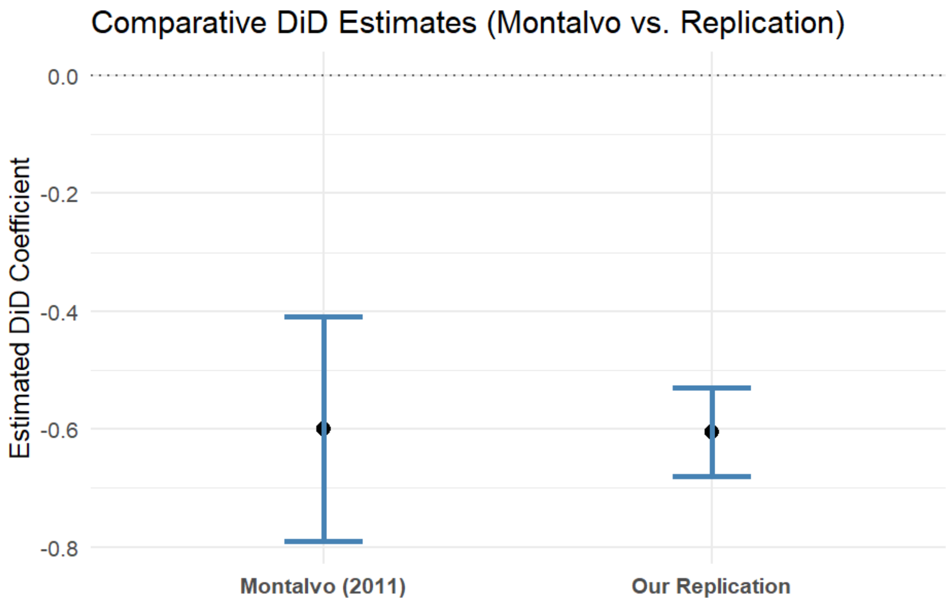

Figure 5: Comparative DiD Estimates: Montalvo (2011) vs. Replication

4.6 Discussion: Composition Bias, Treatment Nature, and External Validity

As Montalvo notes, expatriate voters may differ systematically from residents in ideology, education level, socioeconomic background, and exposure to Spanish media (Lago and Montero, 2006; Mora and Reggio, 2012). These structural divergences are documented in the literature and can affect the internal validity of the DiD approach (Lago and Montero, 2006; Mora and Reggio, 2012). Although the DiD design accounts for time-invariant differences, our dataset lacks individual-level covariates. We recommend future work incorporate demographic controls, census-based matching, or synthetic control methods to ensure stronger group comparability (Callaway and Sant'Anna, 2021; Abadie et al., 2010).

It is also important to note that the 'treatment'—exposure to the March 11 attacks—was not only an exogenous shock but also an event whose political management, especially government communication in the aftermath, likely played a role in shaping the electoral response (Bali, 2007; Lago and Montero, 2006). This highlights the complexity of interpreting the DiD estimate as a pure treatment effect.

Finally, while the Spanish case provides a compelling natural experiment, the observed effects may not be generalizable to other countries or crises, as shown by studies finding different or ambiguous electoral responses to terrorist attacks in other contexts (Baccini et al., 2021). This underlines the value of replication and comparative research.

5 Discussion and Limitations of the Difference-in-Differences Approach

The replication of Montalvo's (2011) study confirms that the March 11, 2004 Madrid bombings had a significant and negative effect on the electoral support for the conservative party (PP) among resident voters. This section critically discusses the methodological strengths of the DiD strategy, evaluates the internal validity of the design, and explores the potential limitations and avenues for improvement.

5.1 Strengths of the DiD Design

The most compelling aspect of Montalvo's identification strategy is the quasi-experimental nature of the treatment assignment. The exogeneity of the terrorist attacks and their precise timing just before the national elections allow for a credible causal inference framework. The use of expatriate voters as a control group is especially insightful, since they voted prior to the attacks and were presumably unaffected by the event when casting their ballots.

Our replication confirms the robustness of the main estimates. The coefficient on the DiD interaction term remains stable across OLS, fixed-effects, and log-transformed models, consistently suggesting a strong drop in support for the PP among residents. The results also remain significant when clustering standard errors at the province level, addressing concerns about intra-group correlation (Bertrand et al., 2004; Cameron and Miller, 2015).

5.2 Internal Validity and Robustness Checks

The identifying assumption behind the DiD model is that, absent the treatment, the outcome trends in the treated and control groups would have evolved in parallel. (Angrist and Pischke, 2009; Callaway and Sant'Anna, 2021) We evaluated this assumption by visually inspecting pre-2004 vote ratios between groups and implementing a placebo test using data from the year 2000.

Although the graphical pre-trends appear relatively parallel, the placebo DiD for 2000 yields a small but statistically significant coefficient. This finding is important: it may indicate a partial violation of the parallel trends assumption, a substantial limitation in DiD designs with non-randomized groups (Angrist and Pischke, 2009; Mora and Reggio, 2012). To further strengthen validity, future replications should formally test for pre-treatment trends by estimating interactions between group and year over multiple pre-treatment periods (Autor, 2003; Callaway and Sant'Anna, 2021).

In line with Montalvo's approach, we also log-transformed the outcome variable to stabilize variance and reduce the influence of outliers. The transformation yielded consistent and significant results, indicating that the DiD findings are not driven by skewed distributions.

5.3 Limitations and Potential Biases

Despite the strengths of the DiD strategy, several limitations merit attention:

(1) Group Composition Bias. The most significant concern is the potential compositional difference between expatriate and resident voters. Expatriates may differ systematically from residents in terms of education level, socioeconomic status, political engagement, and exposure to Spanish media (Lago and Montero, 2006; Mora and Reggio, 2012). These differences could affect voting behavior independently of the attacks. While fixed effects help mitigate this issue, a more detailed comparison of group characteristics —or the use of matching or reweighting methods —would reinforce the credibility of the parallel trends assumption.

(2) Information Contamination. The assumption that expatriates were uninformed of the attacks at the time of voting is difficult to verify. With the increasing speed of information flows, it is plausible that some expatriates learned about the bombings before voting, especially those in nearby European countries or with strong connections to Spain. If a fraction of them received news of the bombings before voting, the control group would be partially treated, attenuating the estimated effect (Bali, 2007; Baccini et al., 2021). Unfortunately, voter-level timestamped data are not available to directly test for contamination.

(3) No Mechanism Decomposition. The DiD estimate captures a reduced-form effect that conflates different channels, including emotional reactions, media framing, and perceptions of government responsibility. Montalvo acknowledges this limitation, emphasizing that the main objective is to estimate the net electoral response rather than isolate mechanisms.

(4) Limited External Validity. The study focuses on a specific institutional setting (Spain, 2004) and a highly salient event. Comparative research shows that the political effects of terrorist attacks can vary substantially depending on context, institutional arrangements, and the nature of the attack (Bali, 2007; Lago and Montero, 2006; Baccini et al., 2021). Generalizing to other countries or crises therefore requires caution. However, the replication reinforces the DiD design's usefulness for assessing high-frequency shocks on political outcomes.

(5) Average Treatment Effect Only. The DiD framework identifies an average treatment effect for the treated group but does not capture potential heterogeneity in effects across regions or voter profiles unless explicitly modeled (e.g., via interactions or triple-difference specifications (Angrist and Pischke, 2009; Callaway and Sant'Anna, 2021)).

5.4 Opportunities for Extension

Our analysis suggests three main directions for methodological improvement:

- **Weighting by Electorate Size.** Montalvo (2011) does not explicitly weight by the number of voters per province. Incorporating population weights could refine effect estimates and better reflect electoral impact.
- **Triple-Differences (DiDiD) Specification.** Adding a third dimension (e.g., regional intensity of attack exposure or ideology) could help separate local treatment heterogeneity from national shock effects.
- **Synthetic Control Comparison.** As Montalvo (2011) does, using a synthetic control method (Abadie et al., 2010) can provide an additional benchmark for the robustness of the DiD findings, and is now a standard for evaluating aggregate shocks in comparative case studies.

5.5 Conclusion

Overall, our DiD replication validates the core insights of Montalvo (2011) and demonstrates the power of difference-in-differences to isolate causal effects in political economy. While the estimation is econometrically sound and statistically robust, it would benefit from additional diagnostics on group comparability, formal pre-trend tests, and exploration of potential confounding channels. Nevertheless, the DiD framework remains well-suited for analyzing electoral shocks under natural experimental settings.

6 Counterfactual with a Synthetic Control Group

The difference-in-difference approach is not without its shortcomings. Numerous sources of uncertainty can affect the reliability of results:

- Bertrand et al. (2004) warn that when using time series, the autocorrelation of residuals leads to an underestimation of standard deviations, and therefore to false positives;
- Lang and Donald (2007) highlight the fact that when the number of groups considered in panel data is reduced – which is the case with difference-in-difference approaches – conventional statistical tests can no longer be used, leading to false positives;
- Finally, Hansen (2007) draws attention to the fact that in this type of data, the number of periods is sometimes much greater than the number of individuals observed, which biases t- and F-statistics.

It is also possible that the control group may not be able to replicate the behavior of the treatment group well in the absence of treatment (Abadie et al., 2007). For all these reasons, José G. Montalvo tests the robustness of his results using another method: the creation of a synthetic control group.

Unlike other studies, which construct synthetic control groups based on a few similarities (as in Card 1990), José G. Montalvo draws on Abadie and Gardeazabal (2003) to construct a synthetic control group in a rigorous and transparent fashion.
A synthetic control group is a combination of individuals not subject to treatment (here nonresident voters), constructed in such a way that the pre-treatment characteristics of this group are as close as possible to the pre-treatment characteristics of a treated individual (here resident voters). Let X_1 be a vector of K pre-treatment characteristics of a treated individual, and X_0 a matrix of the same K characteristics for J individuals in the control group. The synthetic control group method searches for a vector W of J weights such that $X_0 \times W$ is as close as possible to X_1. The weights $w_1 \dots w_J$ must be positive or zero, and their sum must equal 1.

$$\underset{(K\times1)}{\overset{X_1}{\begin{bmatrix} a_1 \\ \vdots \\ a_K \end{bmatrix}}} \approx \underset{(K\times J)}{\overset{X_0}{\begin{bmatrix} b_{11} & \dots & b_{1J} \\ \vdots & \ddots & \vdots \\ b_{K1} & \dots & b_{KJ} \end{bmatrix}}} \times \left[\; w_1 \quad \underset{(J\times1)}{\overset{W}{\dots}} \quad w_J \;\right], \text{ with } \begin{cases} w_j \geq 0 & \forall j \in \{1, ..., J\} \\ \sum_{j=1}^{J} w_j = 1 \end{cases}$$

The aim is to choose W in such a way as to minimize the following distance function (with V a symmetric positive semidefinite matrix):

$$||X_1 - X_0W|| = \sqrt{(X_1 - X_W)'V(X_1 - X_0W)}$$

For the treated individual (X_0), we choose to aggregate all electoral results, for the resident voters, across all Spanish regions. José G. Montalvo selects 4 features when minimizing the distance function: the percentage of conservative votes in 1989 and 1993, and the ratio of conservative to socialist votes in 1996 and 2000. In the end, in this study, $K = 4$ and $J = 52$.

Once the W vector is estimated, we construct the synthetic control group and calculate the percentage of votes for the Conservative party and the ratio between Conservative and Socialist votes for each Spanish general election from 1989 to 2004 (5 general elections in total). We see in Table 3 that we achieved perfectly similar results for each characteristic used during the computation of W, for both treated and synthetic control group. Our results are slightly different from Montalvo's. This is due to the way we aggregated voting data considering not only parties, but overall political sensitivity.
We then plot a graph representing the evolution over time, for the treated group and the synthetic control group, of the ratio of votes for the conservative party to votes for the socialist party during the Spanish general election (Figure 6).

	Treated	Synthetic
Percentage of votes for the conservative party in 1989	25.99	25.99
Percentage of votes for the conservative party in 1993	35.05	35.05
Ratio of voters of the conservative party over the socialist party in 1996	1.03	1.03
Ratio of voters of the conservative party over the socialist party in 2000	1.30	1.30

Table 3: Comparison groups

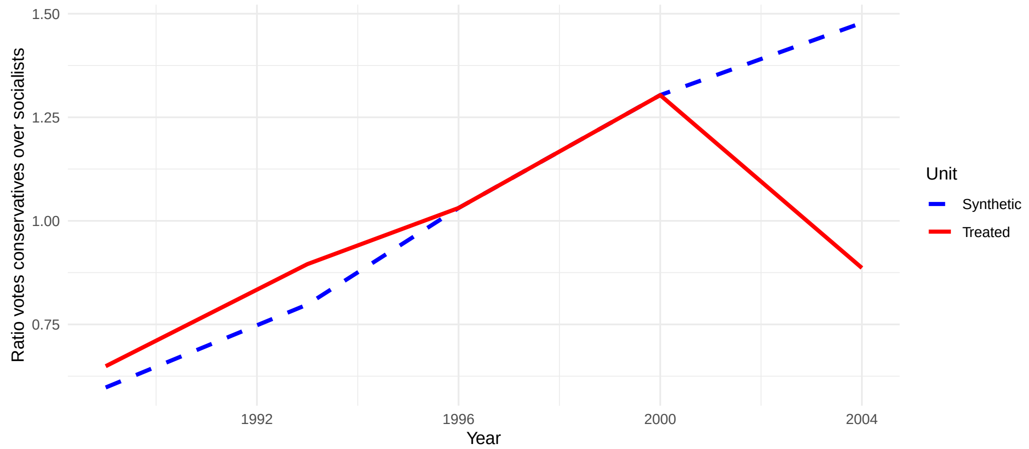

Figure 6: Trends in the ratio of conservative to socialist votes :
Treated aggregate group versus synthetic control group

Despite the small number of characteristics used to build the synthetic control group, it is very clear with a quick visual analysis that the two trends are very close, before diverging sharply for the treatment year. In 2004, they display a difference of 0.59 points, which is slightly bigger than what was found by José G. Montalvo (0.55). Once again, this might be due to the way we aggregated the data. It is possible that the conservative wing as a whole suffered from the terrorist attack, and not just the PP.
We conclude by computing the root mean square prediction error, in order to estimate the quality of the fit for pre-treatment years. Root mean square error can be expressed as :

$$RMSE = \sqrt{\frac{\sum_{i=1}^{N} ||y(i)-\hat{y}(i)||^2}{N}}, \text{ with } \begin{cases} y(i) & \text{the value of the variable for the treated group in year i} \\ \hat{y}(i) & \text{the value predicted by the synthetic control in year i} \\ N & \text{the number of years before treatment } (N=4) \end{cases}$$

A low RMSPE means that the synthetic control corresponds well to the data from the pre-treatment group. The root mean square prediction error is 0.05510084, which is very low. This indicates that our results are probably trustworthy, since they largely align with the results of the previous section adopting a difference-in-differences approach.

7 Conclusion

Other articles, following José G. Montalvo, look at the impact of attacks on election results. Balcells and Torrats-Espinosa (2018) look at Basque pro-independence terrorism in Spain. They study the effect of 8 attacks, between 1989 and 1997, by the ETA (Euskadi Ta Askatasuna) group, and find that both lethal and non-lethal terrorist attacks led voters to participate more in democratic elections. However, both researchers were more mixed on the effect of this type of attack on public support for theincumbent party.

José G. Montalvo concluded his article by noting that recent research from this period (such as Pape 2003 (Pape, 2003)) shows that the dates of terrorist attacks are probably not random, but part of well-thought-out strategies with a political aim. Fourteen years after José G. Montalvo and twenty-two years after Pape, one can only wonder whether these leads have been followed up by contemporary research.
In 2014, Lindsay Shorr Newman (Newman, 2014)studied a set of terrorist attacks, over the period 2000-2005, on a set of several countries. She finds "*strong support for the hypothesis that terrorist violence increases as we move closer to an election date. In fact, terrorist violence approximates a normal distribution centered on the election date*".
It is possible to extend this assertion to different contexts and add some nuance thanks to Deniz Aksoy's article published in 2014 (Aksoy, 2014). Using data on terrorist attacks and electoral outcomes in Western democracies over the period 1950-2004, he shows that pre-election periods are indeed associated with an increase in the

number of terrorist attacks in democracies with the least permissive electoral systems, but not in democracies with permissive electoral systems.

If we look at the evolution of the social context in Western democracies between 2004 and today, it is interesting to note the growing role of social networks in the perception of political events. While the studies cited above use relatively old data, one avenue of research would be to look at the effect of terrorist attacks on election results in a context where the use of social networks is more widespread than ever before. In 2017, Terziyska, Setu and Xiao (Terziyska et al., 2017) compared trends identified in a public dataset (the Global Terrorism Database) with trends reflected in data obtained using Twitter's API. The results showed discrepancies in trends related to terrorist events between these two data sources, suggesting a certain bias in media and public perception of terrorism.

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