Essays on Political Economy

Andreu Arenas Jal

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

Florence, 22 June 2016

European University Institute
Department of Economics

Essays on Political Economy

Andreu Arenas Jal

Thesis submitted for assessment with a view to obtaining the degree of
Doctor of Economics of the European University Institute

Examining Board

Prof. Andrea Ichino, EUI, Supervisor
Prof. Andrea Mattozzi, EUI
Prof. Manuel Bagues, Aalto University
Prof. Ruben Durante, Sciences Po

European University Institute

DEPARTMENT OF ECONOMICS

Researcher declaration to accompany the submission of written work

I, Andreu Arenas, certify that I am the author of the work *Essays on empirical political economy* I have presented for examination for the PhD thesis at the European University Institute. I also certify that this is solely my own original work, other than where I have clearly indicated, in this declaration and in the thesis, that it is the work of others.

I warrant that I have obtained all the permissions required for using any material from other copyrighted publications.

I certify that this work complies with the *Code of Ethics in Academic Research* issued by the European University Institute (IUE 332/2/10 (CA 297).

Signature and Date:

Andreu ARENAS, 02/05/2016

Abstract

In this thesis I use the ban of a political party -Batasuna- as a natural experiment to answer three questions in political economics. The first chapter deals with instrumental and expressive voting. After its ban, Batasuna called for a null vote, which has expressive but no instrumental value. I exploit the heterogeneity in the pre-ban presence of Batasuna and in the enforcement of the ban across municipalities and elections in an effort to show that null voters are two-thirds of the potential Batasuna voters. Null votes lower the electoral threshold, subsequently raising the instrumental incentives to vote for smaller parties, but the behavioral effect of the ban on city councils' fragmentation is close to zero. In a region with salient identity politics, the results are consistent with prevalent expressive voting.

The second chapter is centred on the effects of government fragmentation on government spending. Since the effects the ban are mostly mechanical because a large fraction of Batasuna voters cast null votes, the ban triggers a proportional reshuffling of seats in city councils; in some cases, this changes the majority status of the local government. I compare policy changes in municipalities where the ban triggered a change in government fragmentation with policy changes in municipalities where the ban did not change the majority status of the local government, within municipalities where Batasuna used to be equally important. I find that absolute majorities reduce current spending significantly but increase capital expenditures, consistent with common pool and veto player models.

In the third chapter, I evaluate the effects of the ban after its end, exploiting its heterogeneous length across municipalities. I find a negative effect of a longer ban on electoral support for Batasuna in local elections, driven both by a lower number of candidatures and less votes received. These effects are persistent for at least two elections and have spill-overs to regional elections. The extension of the ban triggered a spike in street terrorism for one month; overall, however, it had no effects since it slightly reduced street terrorism during the year after.

Als pares, i als avis

Acknowledgements

I am very grateful to my supervisor Andrea Ichino, from whom I learnt a lot about being an applied economist throughout the years. I would also like to thank Andrea Mattozzi for his advice and support and Ruben Durante for being part of my committee. A special thanks to Manuel Bagues for his time and comments, and for giving me the opportunity to visit Aalto University, hosting me with great hospitality. I am grateful as well to Jérôme Adda, who advised me at the early stages of this thesis, and to Juan Dolado for his support and proximity.

I warmly thank Lori and Sonia, who greatly contributed to my health during the PhD, giving me an invaluable dose of good mood and proximity on a daily basis. I am also grateful to Lucia Vigna for her help in preparing the thesis defense.

It's been five years in Florence, and I have met many special people with whom I shared good moments. I can't help to mention my classmates Clément, Dominik, Vincent, Rana, John, Nacho, Brais, Andreas and Guilherme; EUI colleagues such as Mireia, Antoni, Agustín, Abián, Jeni, Pedro, Quique and Albert; and Axelle.

I cannot overstate the role of my friends at the other side of the Mediterranean, sempre a punt pel mamoneo corresponent per la desconnexió. Gràcies al Rai, Max, Enric, Pols, Keke, Hug, Estruch; i al Xavi, Pau, Bobi, Edgar, Goday i Pegot. Another special thanks goes to many of my flatmates in Florence during these years.

Finalment, l'agraïment més immens és per als meus pares i la meva família, que m'han ofert un suport absolutament incondicional. Incomptables els trajectes d'anada i tornada a l'aeroport; sempre amb els ànims i la comprensió que fes falta. L'única tristesa, no poder compartir un moment així amb els avis.

Firenze, 15/6/2016

Contents

Introduction and institutional framework **9**

1 Sticky votes: expressive voting after a political ban **15**

1.1 The call for a null vote 18

1.1.1 Implications 23

1.2 Mechanical and Behavioral effects of the ban 25

1.2.1 Estimation 28

1.2.2 Results 31

1.3 Conclusions 35

2 Government fragmentation and government spending: evidence from the ban of Batasuna **43**

2.1 Identification and estimation 47

2.1.1 Results and Interpretation 54

2.1.2 Channels and robustness 57

2.1.3 Absolute majorities or single-party governments? 60

2.2 Conclusions 63

3 Is it effective to ban a political party? **73**

3.1 Identification and estimation 76

3.1.1 Identification threats 81

3.1.2 Regional elections 87

3.1.3 Street terrorism 90

3.2 Interpretation and limitations 95

3.3 Conclusions 98

Introduction and institutional framework

In this thesis I use the ban of Batasuna, a political party in the Spanish region of the Basque Country, which was banned due to its links to terrorism, as a natural experiment with the objective to answer three questions in political economics, structured around three chapters.

The first chapter deals with instrumental and expressive voting. Voting can be viewed both as an investment alternative that might yield utility if the individual voter happens to be decisive, and as a consumption good, with the individual deriving utility from the act of voting for a certain candidate, affirming her identity or moral. Whether vote choices are instrumental or expressive is relevant when striving to understand the preferences and the information aggregated by elections. However, empirically identifying either motivation in actual elections is difficult since changes in the pivotal probability of individual voters are typically associated with changes in the stakes of the election, with the overall effect on the instrumental value of a single vote being ambiguous. In this chapter, I study the effects of the ban of Batasuna, which, after its ban, called for a null vote, which has expressive but no instrumental value. I exploit the heterogeneity in the pre-ban presence of Batasuna and in the enforcement of the ban across municipalities and elections to show that null voters are two-thirds of the potential Batasuna voters in a regular election, meaning that expressive motivations alone explain two-thirds of the votes for this party. Null votes lower the electoral threshold, raising the instrumental incentives for the remaining voters to vote for smaller parties; however, the behavioral effect of the ban on city councils' fragmentation is close to zero. In a region with salient identity politics, the results are consistent with prevalent expressive voting, in contrast with the common assumption of purely instrumental voters.

The second chapter is focused on the effects of government fragmentation on the level or the composition of public spending. Differences in electoral rules explain a considerable amount of variation in countries' government spending, and there are a number of channels which could potentially explain this relationship, such as differences in turnout, government fragmentation, the representation of special-interest parties, the quality of politicians, or corruption. The effect of electoral rules on each of these variables is heterogeneous and context-dependent, and hence it is important to understand the role played by each of these mechanisms. In this chapter, I draw from the findings in the first chapter, which show that the effects the ban were mostly mechanical. The ban triggers a reshuffling of seats in city councils, with every party gaining seats rather proportionally and, in some cases, changing the majority status of the local government. In an effort to identify the effect of government fragmentation on spending, I look at municipalities where Batasuna used to be equally important and compare policy changes in municipalities where the ban triggers a change in government fragmentation with policy changes in municipalities where the ban does not change the majority status of the local government. I find that absolute majorities reduce current spending significantly, by spending less on public goods and public services. On the other hand, absolute majorities increase capital expenditures by a similar amount, although this effect is imprecisely estimated.

In the third paper, I evaluate the effects of the ban after its end, exploiting its heterogeneous length across municipalities. The question of interest is whether preventing the political representation of political parties weakens the electoral base of the targeted political movement, affects voters' preferences or has side effects such as violence and further conflict. Beliefs surrounding the answers to these questions, which are not straightforward, often shape the public debate about the convenience of political bans, complementing legal and philosophical arguments. However, it is difficult to establish suitable control groups and comparable measures of support in mind of empirically evaluating the effects of political bans. In this chapter, I seek to overcome this by exploiting the finite length of the ban of Batasuna, which allows changes in electoral support to be observed, and its heterogeneous enforcement in local elections, which allows for a comparison of changes across municipalities. I find a negative effect of a longer ban on electoral support in local elections, driven both

by a lower number of candidatures after a longer ban and less votes received. These effects persist for at least two elections after the ban and have spill-overs to support for Batasuna in regional elections. The extension of the ban, which in a first electoral term applied to all municipalities, but in a second term was enforced only in a subset of them, triggered a spike in street terrorism for one month, but overall it had no significant effects since it slightly reduced street terrorism during the year after.

All three chapters are empirical applications which draw upon the same institutional framework, which I summarise in the following lines.

Institutional framework

The Basque Country is a region in the north of Spain with more than two million inhabitants and a GDP per capita of more than €30.000, being one of the richest regions of Spain.[1] Basque politics feature a multi-dimensional party system such that every party is characterized by a policy position in the left-right and in the regional autonomy dimensions, with positions ranging from total centralization in Spain to Basque independence. On top of policy positions, the electorate is divided by cultural identity, which correlates with preferences for decentralization.[2]

The main parties contesting the Basque regional and local elections can be divided into Federal parties (with candidatures everywhere in Spain) and Basque Nationalist parties (with candidatures only in the Basque Country). Among the Federal, the main parties are the Popular Party - PP, which is the main federal conservative party and is in favor of a rather centralized organization of Spain; the Socialist Party - PSOE, which is the main federal social democratic party, in favor of a more decentralized organization of Spain; and United Left

[1] The Basque Country is a region with a privileged fiscal status. This is due to the fact that in the middle ages, as the Kingdom of Castile expanded and incorporated other territories into the Crown of Castile, the monarchy granted some of them certain privileges which were known as fueros, or "charters". While these privileges had been abolished for long periods of time, the Spanish Constitution of 1978 recognized them again for the Basque Country, which as a result has its own autonomous treasury and fiscal autonomy. It can establish and regulate its own tax system and collect and manage all Federal taxes, with the exception of the VAT, and it just has to pay a certain amount of money to the Central Government for the management of Federal Competences

[2] Ansolabehere and Puy (2015) provide a complete description and a model of the Basque party system, jointly considering ideology, nationalism and identity for vote choices in regional elections

- IU, which is the main federal leftist party, in favor of a rather decentralized organization of Spain and which recognizes the right of self-determination for the nations or historical regions of Spain. Among the Basque Nationalists, the Basque Nationalist Party - PNV has held the regional government almost every term since the end of Franco's dictatorship in 1975 and is the main conservative (Christian Democratic) party in the region, in favor of greater autonomy for the Basque Country. Eusko Alkartasuna - EA is an independentist and centrist – social democratic party which split from the PNV in the 1980's. In local elections, in some municipalities the PNV and EA have contested elections together in a single electoral list, especially in the 1990's, and also in some regional election.[3] Last but not least, Batasuna is the main leftist and independentist party in the region. This party had different names and electoral brands over the last decades, such as Herri Batasuna, Euskal Herritarrok, or Eusko Abertzale Ekintza-Acción Nacionalista Vasca. For simplicity, I refer to these parties, and in general to the political representation of the "izquierda abertzale" (leftist-independentism), as Batasuna.

Politics in the Basque Country have been heavily influenced by the existence of Euskadi Ta Askatasuna (ETA), a terrorist organization in favor of the independence of the Basque Country. ETA was created in 1958 (during Franco's dictatorship in Spain) and was active until October 2011, when it announced a permanent end of its armed activities.[4] Over this period, ETA killed more than 800 people, mostly between the end of the 1970's and the 1980's. Besides its direct victims, ETA's terrorism has had important economic consequences for the region, and also has influenced the political environment.[5] Politically, one of the most relevant consequences of the existence of ETA has been the ban of Batasuna, which used to represent its ideological (leftist-independentism) and political space and was never willing to reject ETA's terrorism.

In June of 2002, the Spanish Parliament passed a law of Political Parties with the support of more than 90% of its members.[6] The aim of the law reads as follows:

[3]In other elections, EA has also formed pre-electoral coalitions with leftist parties

[4]During this period, ETA held a number of cease-fires. The last ceasefire started in September of 2010. In January of 2011, ETA announced that that ceasefire would be permanent and verifiable by international observers. On October of 2011, ETA announced a definitive cessation of its armed activities.

[5]Abadie and Gardeazabal (2003) estimate that after the outbreak of terrorism in the late 1960's, per capita GDP in the Basque Country declined 10% relative to a synthetic control region without terrorism

[6]Organic Law 6/2002 of Political Parties. Boletín Oficial del Estado, 28/06/2002. Voted in favor of the

"The aim is to guarantee the operation of the democratic system and the citizens' essential freedoms, avoiding the possibility that a political party could, in a reiterate and grave way, attempt against this democratic regime of freedom, justify racism and xenophobia or politically support violence and the activities of terrorist groups. (...) it becomes indispensable to identify and to distinguish with all clarity those organizations which defend and promote their ideas and programs, whichever they are, even those which expect to revise the constitutional framework, with a scrupulous respect for the democratic methods and principles, from those which base their political action on the connivance with violence, terror, discrimination, the exclusion and the violation of rights and freedoms"

A few weeks later, the Council of Ministers asked the Supreme Court of Spain for the ban of Batasuna. After a deliberation process, in March of 2003, the Supreme Court of Spain banned Batasuna due to its links to ETA and its non-rejection of terrorism.[7] The ban was not a result of bargaining among the existing political agents in the Basque Country, which mostly opposed it, but rather a process led by the Spanish Parliament and enforced by the Courts. The law of parties was passed in a period of increasing international concerns about terrorism, especially after the 9/11 attacks, and under the absolute majority in the Spanish Parliament of the Popular Party (PP), the party with the largest number of victims of ETA.[8] Before its ban, Batasuna used to obtain between 20 and 25% of the vote in Basque local elections.

Following its ban, Batasuna adopted different strategies to be present in the elections but these were mostly successfully blocked by the Courts. In 2003, they created a new party ("Autodeterminaziorako Bilgunea" - AuB) with the aim of contesting the 2003 local elections, but the Supreme Court invalidated AuB's lists due to its links with the previously banned organizations. With the aim of participating in the 2007 local elections, they created another party ("Abertzale Sozialisten Batasuna") which again was outlawed by the courts, and they also revived an old party, "Eusko Abertzale Ekintza-Acción Nacionalista Vasca" (EAE-ANV). In the case of EAE-ANV, the Courts could only invalidate a share of its municipal electoral lists (around 50%) due to its links to the previously banned organizations,

law: PP, PSOE, CiU, CC, PA. Voted against the law: PNV, EA, BNG, ERC, ICV, CHA

[7]The political organizations which were outlawed by that judicial sentence were Herri Batasuna, Euskal Herritarrok and Batasuna.

[8]See Bourne (2015) for a further analysis of the political mechanisms which led to the ban of Batasuna.

as they failed to find enough evidence of links to invalidate all their lists before the election. Although to avoid its ban, Batasuna denied any connection to EAE-ANV before the Courts made public their veredict, afterwards they called for the vote for EAE-ANV.[9] Eventually, EAE-ANV was fully banned in 2008, but this was after the 2007 election, when they obtained representation in a number of municipalities.

With the aim of participating in the 2011 local elections, Batasuna created another party, "Sortu", which was meant to lead a larger candidature named "Bildu", which had been created by EA (centrist independentists) and Alternatiba, a Basque Split from IU (federal leftists). Sortu was the first party of the "Izquierda Abertzale" to explicitly reject ETA's violence. The 1st of May of 2011, the Supreme Court of Spain invalidated the electoral lists of Bildu and forbade the inscription of Sortu into the registry of political parties because of its ties with Batasuna. However, a few days later, in May 5th, the Constitutional Court of Spain revoked the Supreme Court decision and allowed Bildu to contest the 2011 elections to be held on May 22nd, in a close decision (6 judges voted in favor, 5 against). Another equally close decision legalized Sortu in 2012, which *de jure* became again part of Bildu, although *de facto* it was already part of (and leading) it.

As a result, to sum up, Batasuna was banned for two local elections (2003 and 2007), although in 2007 it escaped the ban in a number of municipalities.

[9] El País, May 2007

Chapter 1

Sticky votes: expressive voting after a political ban

Voting can be viewed both as an investment alternative moved by preferences over the election outcome and which might yield utility if the individual voter happens to be decisive, and as a consumption good allowing the voter to express her identity or moral, moved by preferences over the act of voting by itself. Instrumental and expressive motivations might lead to the same vote choices, but when they do not and expressive motivations dominate, this can prevent information aggregation in elections (Morgan and Várdy (2012)) or lead to expressive policy traps, where a policy is implemented thanks to the favorable votes of individuals who, if pivotal, would vote against it (Hillman (2010)).[1] It is important, thus, to understand what is the extent of expressive voting and under what circumstances it is prevalent.

Identifying whether vote choices are expressive or instrumental in actual elections is difficult because, typically, changes in the pivotal probability of individual voters are associated with changes in the stakes of the election outcome - the overall effect on the instrumental value of a single vote being ambiguous. In this chapter, I study the effect of the ban of Batasuna, a leftist-indepedentist political party in the Spanish region of the Basque Country which was banned due to its links to terrorism. At the moment of its ban, in 2003, Batasuna

[1]Bandwagon voting behavior due to a purely a psychological desire to be on the winning side is an example of expressive voting that might lead to expressive policy traps. For bandwagon behavior due to other-regarding preferences, instrumental and expressive choices coincide (Morton and Ou, 2015)

was an important party at the local level, obtaining between 20 and 25% of the vote in local elections. After its ban, Batasuna asked the voters to still vote for them and distributed ballots of its banned candidature: these votes would be counted just as null votes, which have expressive but no instrumental value. I show that this call was quite successful: in municipalities where Batasuna used to exist, null votes jump from close to zero to two-thirds of the pre-ban Batasuna vote share, while remaining close to zero in the remaining municipalities. In the second election after the ban, in 2007, Batasuna managed to escape the ban in around half of the municipalities, giving rise to a unique setting: within municipalities where Batasuna had very similar pre-ban levels and trends in support, in half of them voting for Batasuna has a purely expressive value while in the other half it has both expressive and instrumental value. The results show that null voters are two-thirds of the potential Batasuna voters in a regular election, thus meaning that expressive motivations alone explain two-thirds of the vote for this political party. This is an important result when considering it shows that a large fraction of voters obtain expressive utility from voting for this party and that the vote choice that maximizes their expressive utility is their actual vote choice.

I also investigate the consequences for the remaining voters of such a large fraction of the electorate suddenly casting a null vote. Since null votes are not taken into account in computing electoral thresholds, the reaction by Batasuna voters' effectively lowers the barriers to city council entry by lowering the votes needed to obtain a seat, raising the instrumental incentives for the remaining voters to vote for smaller parties, which within proportional systems like the one in Spain (d'Hondt) can have significant effects on policy (Folke (2014), Freier and Odendahl (2015)). To estimate the response of the remaining voters to this change, I estimate the effect of the ban on a number of political outcomes by differences-in-differences and decompose this into a mechanical component that arises just because the votes for Batasuna no longer translate into seats, and a behavioral component, which captures the effect of the ban on voting behavior.

I find that the effect of the ban on Basque city councils is mostly mechanical: city councils following the ban are, on average, very close to those before the ban but just excluding Batasuna. The behavioral effect of the ban on measures of fragmentation such as the Effective Number of Parties is close to zero and not statistically significant. The absence

of behavioral effects is not driven by changes in voting behavior across ideological dimensions or political parties, since the behavioral effect on them is also close to zero and not significant. The results are not driven by the ban leading to absolute majorities more often (and hence by smaller parties becoming less able to influence policy), as they persist in municipalities for which the ban does not affect the majority status of the government. While other voters might just have not expected the success of the call for a null vote in the first election after the ban, the absence of a behavioral effect persists in the second election after the ban, in 2007, when Batasuna managed to escape the ban in a subset of municipalities.

These results show small behavioral responses to important changes in instrumental incentives. If Basque voters were instrumental (Downs (1957)), they would be elastic with respect to changes in electoral rules, and behavioral effects would be large. Using survey data on identity and voter choices in regional elections in the Basque Country, Ansolabehere and Puy (2015) show that "*Euskera speakers and others who identify with the culture vote in line with that identity, above and beyond their preferences about regional autonomy, education policy, and other policies that reflect Nationalism and quite apart from the usual Left-Right divisions common to most European political systems*". Hence, identity voting, which is prevalent in the region and is a form of expressive voting, could be behind this small behavioral response, making voters inelastic to changes in instrumental incentives.[2] Existing experimental evidence shows that even minimal group identities may have an important effect on voter choices in elections where incentives or information are low (Bassi *et al.*, 2011). Moreover, the perception of the ban as an unfair measure by Batasuna voters might have triggered further expressive voting due to moral reasons, contributing to the success of the call for a null vote. The absence of significant effects of the ban on voting behavior across the left-right blocks -in spite of Batasuna being a leftist party- is also consistent with Basque voters voting expressively rather than instrumentally.

This chapter contributes to the literature on expressive behavior in economics and politics (Hillman (2010), Hamlin and Jennings (2011)) by providing direct -Batasuna voters casting null votes- and indirect -no behavioral reaction on fragmentation nor ideology- evidence of

[2]In a related paper, Hillman *et al.* (2015) investigate the link between group identity and turnout and accountability, considering the role of group decisiveness

expressive voting. To the best of my knowledge, this is the first paper identifying expressive voting in the field rather than in the lab, as Feddersen *et al.* (2009), Shayo and Harel (2012), Kamenica and Egan Brad (2014) or Morton and Ou (2015), which exogenously manipulate pivotal probabilities in experiments and estimate its effects on vote choices.[3]

This chapter also extends the empirical literature on the mechanical and psychological or behavioral effects of electoral rules by estimating the mechanical and behavioral effects of a political ban. Dissimilar to previous empirical contributions, which find sizable psychological or behavioral effects, such as Fiva and Folke (2014), Pellicer and Wegner (2014) or Van der Straeten *et al.* (2013), I find no significant behavioral responses to the ban of Batasuna. This is also related to the literature on strategic or Duvergerian voting: while Gerring (2005), Gaines (1999) or Pradeep Chhibber (1998) have studied the existence and the extent of voting for non-competitive candidates in majoritarian systems, this chapter focuses on the intensive margin; i.e. how changes in small parties' competitiveness affect voting behavior. Fujiwara (2011) estimates the effect of single-ballot vs. dual-ballot (runoff) plurality systems on voting behavior in a Regression Discontinuity Design and finds large strategic responses; in this chapter I find very little strategic responses to a decrease in the barriers to entry of small parties within a proportional system.[4]

1.1 The call for a null vote

Whenever and wherever Batasuna was banned, it distributed ballots of its banned candidature and asked the voters to still vote for them: these votes would be counted just as null votes, and following the elections, Batasuna would claim those votes as votes for them.[5] Figure 1.1 shows turnout, the vote share for Batasuna and null votes in Basque local elections between 1987 and 2007, for municipalities above 250 inhabitants, which feature a closed

[3]The notion of expressive voting was introduced by Buchanan (1954), Fiorina (1976) and Brennan and Lomasky (1997), among others. Expressive behavior is not limited to politics, see for instance Johansson-Stenman and Martinsson (2006) on the motivations for buying a BMW

[4]In a related paper, Kawai and Watanabe (2013) propose a model of strategic voting and quantify its impact on election outcomes by adopting an inequality-based estimator, finding a large fraction of strategic voters but a low fraction of misaligned voters in general (plurality) elections in Japan.

[5]El País, May 14th, 2003, El Diario Vasco, May 27th, 2003

list proportional system (d'Hondt with a 5% electoral threshold).[6] While turnout remained rather stable throughout the period, in 2003 the Batasuna vote share drops to zero due to the ban while null votes jump to more than 15%, which is around two-thirds of the pre-ban vote share of Batasuna. In 2007, null votes decline since Batasuna escaped the ban in some municipalities and calls for a null vote only wherever they are banned.

Figure 1.1: Batasuna vote share, turnout and null vote in Basque local elections

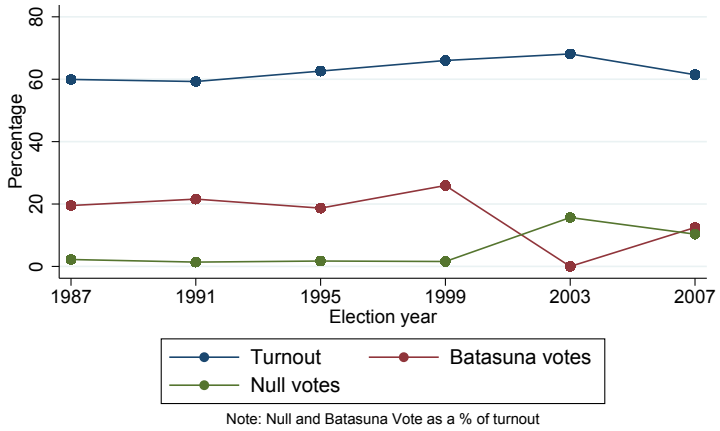
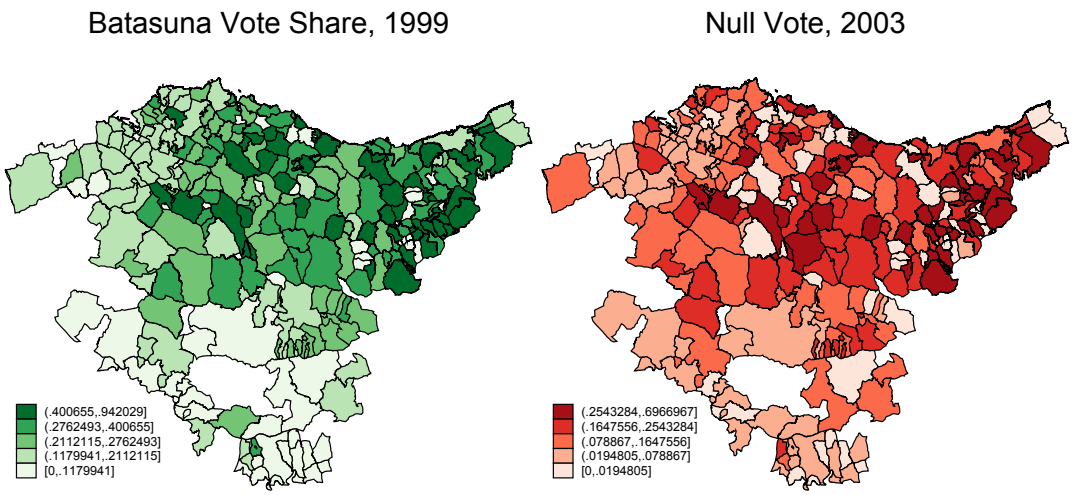


Figure 1.2 shows the geographical distribution of the vote share for Batasuna in the 1999 local election and of null votes in the 2003 local election. Both maps show very similar patterns, suggesting that the former or potential voters of Batasuna are those casting null votes after the ban. The left-hand side graph in figure 1.3 shows null votes for two groups of municipalities: those where Batasuna used to exist before the ban, and those where it did not exist.[7] The graph shows how the increase in null votes after the ban is only driven by those municipalities where Batasuna used to exist before the ban. The right panel in figure 1.3 displays the evolution of null votes over time within municipalities where Batasuna used to exist before the ban, for two subgroups: those municipalities where Batasuna escaped the ban in 2007, and those where it remained banned. Wherever Batasuna becomes legal again, null votes go back to close to zero, while wherever it was still banned, null votes remain stable around 18% of turnout.

[6]In municipalities with less than 250 inhabitants there are two different types of majoritarian systems, which are excluded from the analysis

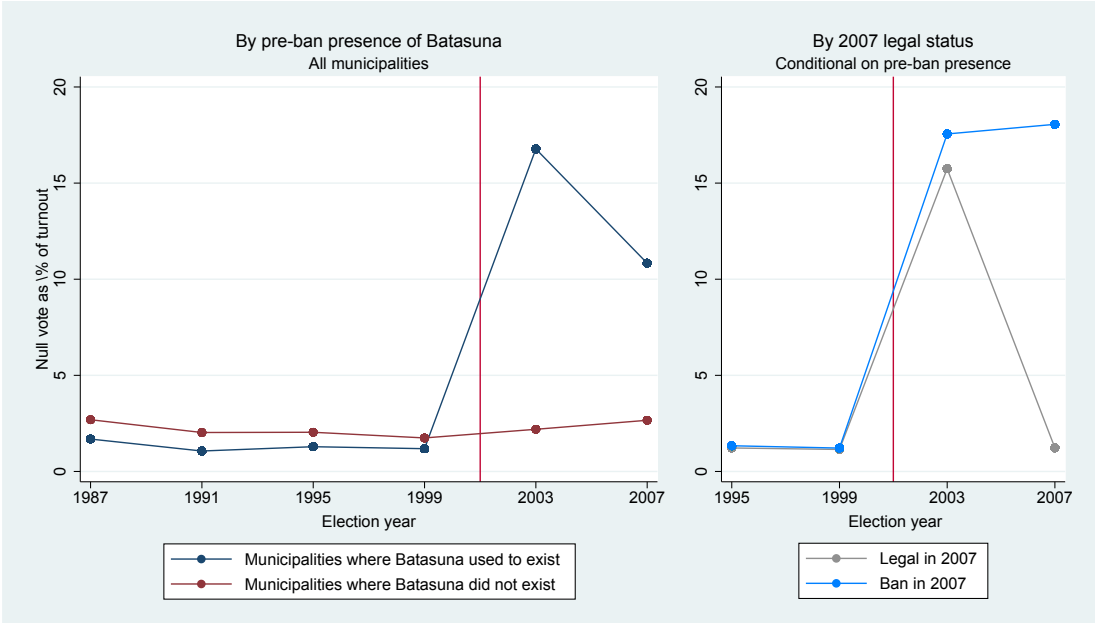
[7]Used to exist is defined as Batasuna having presented a list in 1995 and/or 1999

Figure 1.2: Batasuna vote and Null vote



Note: color categories are quintiles of Batasuna and Null vote (both expressed as % of turnout)

Figure 1.3: Null vote



According to instrumental motives for voting (i.e., motivations over policy or material outcomes), there would be no reason to cast a null vote after the ban, since such votes do not translate into seats. We would like to quantify the percentage of null votes as a fraction of the voters who would have voted for Batasuna in an election where Batasuna would have been legal, to disentangle what fraction of their electorate votes for them purely for expressive reasons and regardless of instrumental considerations. First, we can compare the effect on null vote with the pre-ban vote share for Batasuna. Column (1) in table 1.1 shows differences-in-differences estimates of the effect of the ban on null votes. Ban is a dummy equal to one for treated municipalities after the ban: this compares changes in null votes before (1995, 1999) and after the ban (2003), in treated (where Batasuna used to exist before the ban) versus control municipalities (where Batasuna did not exist before the ban). This is the point estimate for the effect in figure 1.3 in 2003, and it shows that the ban raised null votes by 15.3 percentage points, which is around 63% of the pre-ban vote share of Batasuna in those municipalities, as displayed in the descriptives in table 1.5 in the Appendix A. However, we cannot know what would have been the vote share of Batasuna in those municipalities in 2003 in absence of the ban. Because the control group is such that Batasuna did not exist before the ban, it might not be appropriate to assume that either group of municipalities would have followed the same trend in support for Batasuna in absence of the ban, particularly since figure 1.1 shows that Batasuna was slightly trending up before the ban.

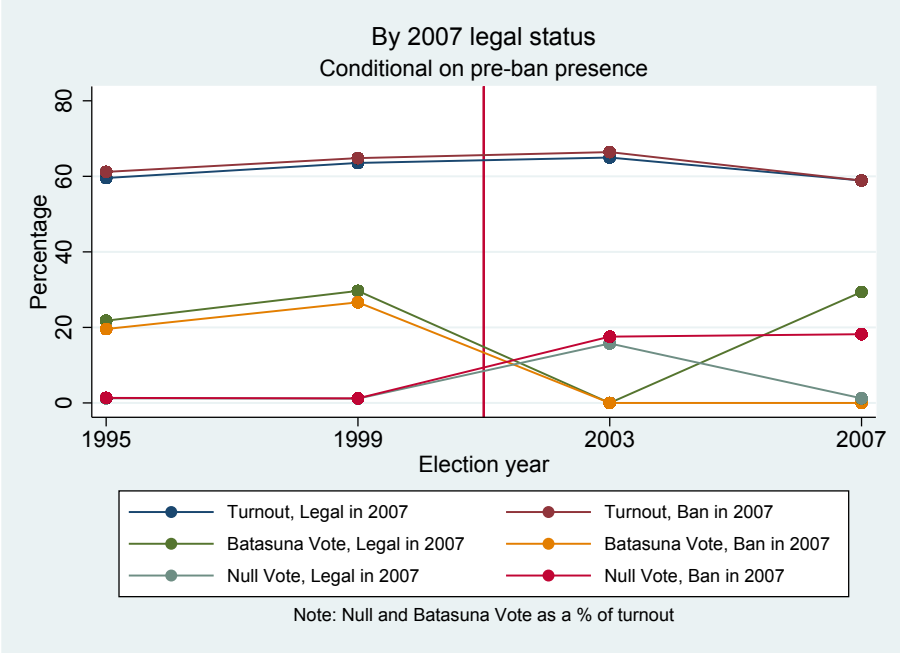
However, we can exploit the heterogeneous enforcement of the ban in the 2007 local election and look at the effect of the ban within the group of municipalities where Batasuna used to exist before the ban. As figure 1.4 shows, these groups were following similar trends in support for Batasuna before the ban. Exploiting this variation, column (2) in table 1.1 shows the difference-in-differences estimate of the effect of the ban on null votes and on the vote share for Batasuna. This compares changes in outcomes before (1995, 1999) and after the ban (2007), in treated (where Batasuna used to exist before the ban and was banned in 2007) versus control municipalities (where Batasuna used to exist before the ban and was legal in 2007). The results show a similar figure: null voters are estimated to be 65% of potential Batasuna voters. This setting is particularly interesting because for a subset of

Table 1.1: Effects of the ban on null votes

	2003 Ban	2007 Ban	
	Dependent Variable	Dependent Variable	
	Null Vote	Null Vote	VS, Batasuna
	(1)	(2)	(3)
Ban	0.153***	0.171***	-0.263***
	(0.01)	(0.011)	(0.014)
Municipality F.E.	✓	✓	✓
Year FE	✓	✓	✓
R^2	0.4878	0.6	0.44
N	653	586	586
Municipalities	222	189	189
$\frac{\widehat{NullVoters}}{BatasunaVoters}$	0.638	.649	

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Both dependent variables as a percentage of turnout. For the 2003, ban, $\frac{\widehat{NullVoters}}{BatasunaVoters}$ obtained dividing by the pre-ban Vote Share for Batasuna in the treated municipalities. 2003 ban: 1995, 1999, 2003 elections. 2007 ban: 1995, 1999, 2007 elections. Ban is a dummy variable equal to one for the treated after the ban.

Figure 1.4: Turnout, Null vote and Vote for Batasuna



municipalities, voting for Batasuna has solely expressive value; for the remaining subset, on the other hand, it has both expressive and instrumental value. Hence, this point estimate means that expressive motivations alone explain two-thirds of the votes for Batasuna.

This is further illustrated in figure 1.4. To see what fraction of potential Batasuna voters are casting a null vote we want to compare the red line (Null vote, ban in 2007) with the green line (Batasuna vote, legal in 2007) in 2007, accounting for slight pre-ban differences in levels of support. This distance is roughly the difference between the point estimates in columns (2) and (3) of table 1.1. Moreover, figure 1.4 shows that these differences cannot be attributed to changes in turnout.

The source of variation in 2007, however, is less likely to be exogenous, because Batasuna managed to escape the 2007 ban in municipalities where they were able to find a large number of candidates without links with Batasuna in the past.[8] Figure 1.4 shows how wherever Batasuna managed to escape the ban in 2007, it had slightly higher levels of pre-ban support. If the ability to find *clean* candidates is related to the level of support, the estimate in column (3) of table 1.1 is unbiased as municipality-specific unobserved heterogeneity is absorbed by municipality fixed effects. If instead it is related to unobserved increasing trends in support, the estimate in column (3) would be downward biased and the fraction of potential Batasuna voters casting a null vote would be even larger than 65%.

1.1.1 Implications

Consider a simple framework with two candidates, namely A and B. Voter i casts a vote $v_i = A$ or $v_i = B$ to maximize her utility given by the sum of an instrumental component, which depends on the policy implemented by the winning candidate -$u(A)$ or $u(B)$- and an expressive component which depends on the expressive value of voting for either candidate, $I(v_i)$. The vote choice enters the utility function trough the probability of affecting the election outcome and through its expressive component.[9]

$$u^{Total}(v_i) = \left(p^A(v_i)u(A) + (1 - p^A(v_i))u(B)\right) + I(v_i) = u^{Inst}(v_i) + u^{Expr}(v_i)$$

[8]The number of linked candidates which would lead to a ban in 2007 was known only ex-post (i.e. after Batasuna/EAE-ANV presented its lists)

[9]This framework draws from Hillman (2010)

Denote by v_i^* the utility maximizing vote choice for voter i and by $v_i^{Inst^*}$ the vote choice which maximizes the instrumental utility component (i.e. $v_i^{Inst^*} = A$ whenever $(p^A(A) - p^A(B))u(A) + (p^A(B) - p^A(A))u(B) > 0$). $(p^A(A) - p^A(B))$ is in general very small and arbitrarily close to zero, and $v_i^{Inst^*}$ is driven by differences between $u(A)$ and $u(B)$. On the other hand, $v_i^{Exp^*}$, the vote choice which maximizes the expressive utility component, is driven by differences in the non-consequentialist payoff from either alternative, $I(A)$ vs. $I(B)$. In a regular election, it is impossible to know what is the driver of v_i^*, as we do not observe $u(A)$ nor $u(B)$, $I(A)$ or $I(B)$. Under the ban, voting for Batasuna has no chance of affecting the election outcome but still has expressive value. This means that for the null voters, which we have identified as Batasuna voters, $u^{Exp}(Batasuna) > 0$ and $v_i^{Total^*} = v_i^{Exp^*}$. An expressive policy trap (Hillman (2010)) might arise if the fraction of voters such that:

1. $u^{Exp}(v_i) > 0$, for some v (i.e., deriving expressive utility from voting for party v, regardless of the election outcome)

2. $v_i^* = v_i^{Exp^*}$ (i.e., voting for the party which maximizes their expressive utility)

3. $v_i^{Exp^*} \neq v_i^{Inst^*}$ (i.e. not voting for the party which maximizes their instrumental utility)

is large enough so that the election outcome differs from the election outcome which would have arisen with purely instrumental voters. The results in this chapter show that (1) and (2) are satisfied for at least 18% of Basque voters (which are 65% of Batasuna voters). However, it is important to note that an expressive policy trap could not arise in this setting, since voting for Batasuna under the ban cannot affect the election outcome, and it is for this precise reason that we can infer that these votes are expressive and not instrumental.

There are two important points to keep in mind when interpreting the results in this chapter. First, that this setting is likely to be exceptionally conductive to expressive voting, as it is motivated by a political ban and it is in a region with salient identity politics, which means the large estimate of the fraction of expressive voters should be seen as closer to an upper bound than to an average value. Second, that there is no reason why we would expect a large fraction of these expressive voters to be conflicted (i.e., to prefer to vote for another party in case they were pivotal), which is a crucial parameter for the welfare implications

of expressive voting. However, Morgan and Várdy (2012) show how for a Condorcet jury, "*unless conflict between expressive and instrumental preferences is very low, information does not aggregate in the limit, and large voting bodies perform no better than a coin flip in selecting the correct outcome*". Hence, given the potentially large fraction of voters for which (1) and (2) are satisfied according to our results, even a small fraction of conflicted voters could have consequences.

1.2 Mechanical and Behavioral effects of the ban

The ban and the success of the call for a null vote have important consequences for the remaining voters and parties. The electoral rule for Spanish local elections is proportional (d'Hondt) with a legal threshold of 5% of the valid votes. The legal threshold sets a minimum fraction of votes necessary to obtain a seat in the city council in terms of valid votes (VV), which are typically very close to total votes (TV) since there are very few null votes.[10] However, after the ban, this is no longer the case. We can rewrite the necessary condition as a function of total votes, which is a better measure of how costly it is for a party to obtain representation:

$$Necessary\ Votes_{TV} = 0.05\frac{VV}{TV}$$

The ban mechanically makes the necessary condition to enter the city council easier to achieve for smaller parties, since the votes for Batasuna mechanically become null votes instead of valid votes.[11] Because the size of city councils in the Basque Country ranges from 7 to 29, reaching the 5% threshold is a necessary but not a sufficient condition to obtain a seat and in many municipalities a vote share close to 10% is needed in order to obtain a seat. Denoting by VS_i the vote share of party i as a fraction of valid votes (VV), the fraction of valid votes sufficient for a party to be present in the city council is given by:

$$Sufficient\ Votes_{VV} = max\left\{0.05, \frac{\sum_{i;VS_i \geq 0.05} VS_i}{(1 + Council\ \ Size)}\right\}$$

[10] The difference between valid and total votes is given by null votes. Valid votes include both blank votes and votes for parties

[11] Mechanically being defined as holding voting behavior constant

Which we can rewrite as a fraction of total votes:

$$Sufficient\ Votes_{TV} = max\left\{0.05\frac{VV}{TV}, \frac{\sum_{i;VS_i\geq 0.05} VS_i}{(1+Council\ \ Size)}\frac{VV}{TV}\right\}$$

It is important to notice that $\sum_{i;VS_i\geq 0.05} VS_i$ only adds up the VS of parties which obtain more than 5% of the valid votes. The left term in the bracket mechanically strictly decreases with the ban, as votes for Batasuna are not valid anymore and are counted as null votes instead, since this corresponds to the case in which the council size is large enough that the necessary condition to enter the council becomes sufficient as well. For the right term, we can distinguish a number of cases, depending on the existence of parties below the 5% threshold and on the size of Batasuna.

1. Whenever there are no parties below the 5% threshold, the ban mechanically strictly decreases the sufficient threshold, since VV decrease and the remaining terms are not affected.

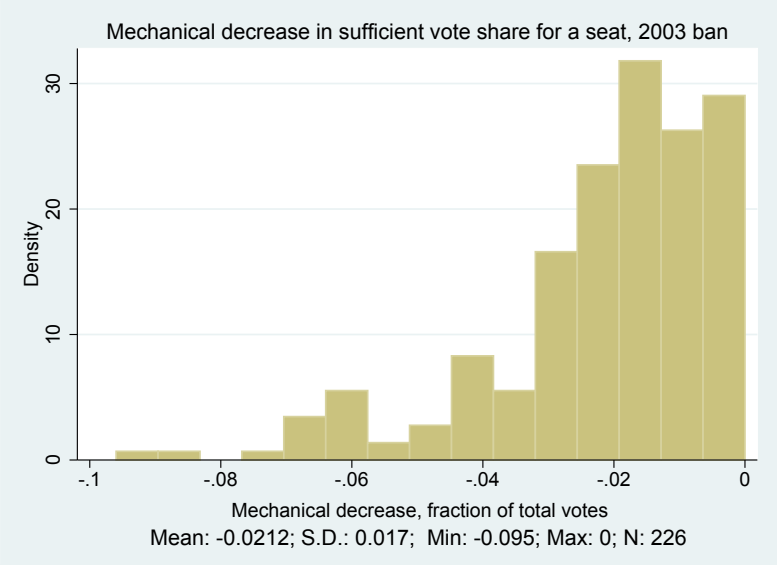
2. Whenever there are parties below the 5% threshold, in all cases VV decrease due to the ban. Regarding $\sum_{i;VS_i\geq 0.05} VS_i$, if Batasuna VS (BVS)$\in (0, 0.05)$, it could increase if there exist parties $\in (0.0475, 0.05)$ of the valid votes which then would add to $\sum_{i;VS_i\geq 0.05} VS_i$. If BVS$\in [0.05, 1)$, then $\sum_{i;VS_i\geq 0.05} VS_i$ decreases by BVS, and increases if there exist parties $\in (0.05 \times (1 - BVS), 0.05)$ which then would add to $\sum_{i;VS_i\geq 0.05} VS_i$, the overall effect being ambiguous.

Even if ex-ante there could exist particular cases in which the sufficient votes needed would increase, figure 1.5 displays the histogram of the mechanical effect of the ban on $Sufficient\ Votes_{TV}$, showing that in our setting the ban always mechanically decreases (or leaves unaffected) the sufficient number of votes for a party to secure a seat in the city council.

Hence, the ban mechanically decreases both the necessary and sufficient conditions for small parties to obtain representation, lowering the barriers to entry and the uncertainty about the representation of small parties. Within proportional systems like the Spanish one, small parties, possibly focused on specific policies, can have significant influence on policy, as Folke (2014) and Freier and Odendahl (2015) show using close seats in local elections in

Sweden and Germany, respectively. With a lower threshold, voters have higher instrumental incentives to vote for smaller parties, in line with the findings of Pellicer and Wegner (2014) which document a decrease in the vote for small parties after a change in the legal threshold in local elections in Morocco, or of Fiva and Folke (2014) after a change from D'Hondt to Modified Sainte-Laguë in Norway.

Figure 1.5: Histogram



It is worth noting that the appreciation that null votes lower the electoral threshold might seem contradictory with null votes having no instrumental value. However, this seems to be a second-order unintended consequence. To test for a hidden motivation by Batasuna leaders or supporters for boosting the entrance of small parties in city councils, I regress null votes on a continuous measure of treatment -an interaction between the pre-ban vote share of Batasuna and a post 2003 dummy- and on the mechanical decrease in $Sufficient\ Votes_{TV}$ (interacted with a post 2003 dummy), where the latter is meant to measure the instrumental value of null votes in terms of decreasing the barriers to entry to the city council. The results, in table 1.7 in the Appendix A, show that the mechanical decrease in $Sufficient\ Votes_{TV}$ is not significantly related to null votes, with the sign of the point estimate being inconsistent with the aforementioned hypothesis. Notice also that the results in the first column, with a continuous treatment, are similar to the previous estimates regarding null voters as a fraction of Batasuna voters in a regular election.

1.2.1 Estimation

I estimate the behavioral responses to the ban by decomposing the effects of the ban into its mechanical and behavioral counterparts. Mechanical effects arise because the votes for Batasuna no longer translate into seats and behavioral effects arise due to changes in electoral behavior in anticipation of the mechanical effects. The decomposition of the effects of electoral rules into mechanical and behavioral effects goes back to Duverger (1954) and has been empirically advanced by Fiva and Folke (2014), Pellicer and Wegner (2014), Van der Straeten *et al.* (2013), Blais *et al.* (2012) and Blais *et al.* (2011). I follow the approach of Fiva and Folke (2014) and accordingly use the formulaic structure of electoral rules to generate counterfactual election outcomes to decompose the effect of the ban.

For a given number of parties k, let an electoral rule f be a function from a vector of votes into a vector of seats, f: $\mathbb{R}^k \rightarrow \mathbb{R}^k$. Let an electoral outcome h (i.e. share of leftist parties, Herfindahl Index of seat share concentration) be a function from a vector of seats into the real numbers, h: $\mathbb{R}^k \rightarrow \mathbb{R}$. Consider a pair of elections, election 1 and election 2, such that everything is identical but the electoral rule. Election 1 takes place under electoral rule f_1, and the voting result is v_1; election 2 takes place under electoral rule f_2, and the voting result is v_2. For a generic outcome h, the total effect of switching from electoral rule 1 to electoral rule 2 is:

$$TE = h(f_2(v_2)) - h(f_1(v_1))$$

The mechanical effect of switching from electoral rule 1 to electoral rule 2 is defined as:

$$ME = h(f_2(v_1)) - h(f_1(v_1))$$

The behavioral effect of switching from electoral rule 1 to electoral rule 2 is defined as:

$$BE = h(f_2(v_2)) - h(f_2(v_1))$$

Note that the sum of the mechanical and behavioral effects is equal to the total effect:

$$ME + BE = h(f_2(v_1)) - h(f_1(v_1)) + h(f_2(v_2)) - h(f_2(v_1)) = h(f_2(v_2)) - h(f_1(v_1)) = TE$$

Therefore, we can decompose the total effect into a component which captures the effect of the rule for a given voting result (the mechanical effect) and a component which is only driven by changes in electoral behavior due to the rule change (the behavioral effect). Note that, among the above, only $f_2(v_1)$ is not observed: it is constructed by using the formulaic structure of electoral rules to obtain a counterfactual vector of seats (a counterfactual city council) applying rule 2 to v_1. The ban of a political party can be seen as a change in the electoral rule such that the votes for that party simply no longer translate into seats and are counted as null votes instead, and hence for each municipality I create a counterfactual city council (i.e. $f_2(v_1)$) by taking the pre-ban election results and allocating seats according to d'Hondt, computing the votes for Batasuna as null votes. This is illustrated for a given municipality in figure 1.6.

I estimate these effects using differences-in-differences, where $y = h(v)$. To estimate the Total Effect of the ban on a political outcome, I use data on the actual outcome from the 1995 and 1999 elections (pre-ban) and the 2003 election (post-ban) and estimate the following equation by OLS:

$$y_{mt} = \alpha_m + \delta_t + \beta^{TE} Ban_{mt} + \epsilon_{mt}$$

Where m stands for municipality, t for time, $Ban_{mt} = 1$ for the post-ban observations if Batasuna used to be present in that municipality before the ban and zero otherwise. Table 1.5 shows descriptives for either group, before and after the ban. I use the heterogeneity in the pre-ban presence of Batasuna to separately estimate the election effect and the effect of the ban, so that the election effect captures time-specific changes in outcomes which are not related to the ban. The differences-in-differences (DiD) estimator of the Total Effect of the ban (TE) is given by:

$$\hat{\beta}^{DiD,TE} = [\overline{y}^{\text{Ban}}_{\text{Post}} - \overline{y}^{\text{Ban}}_{\text{Pre}}] - [\overline{y}^{\text{Control}}_{\text{Post}} - \overline{y}^{\text{Control}}_{\text{Pre}}]$$

Its expectation, $E[\hat{\beta}^{DiD,TE}]$, is given by:

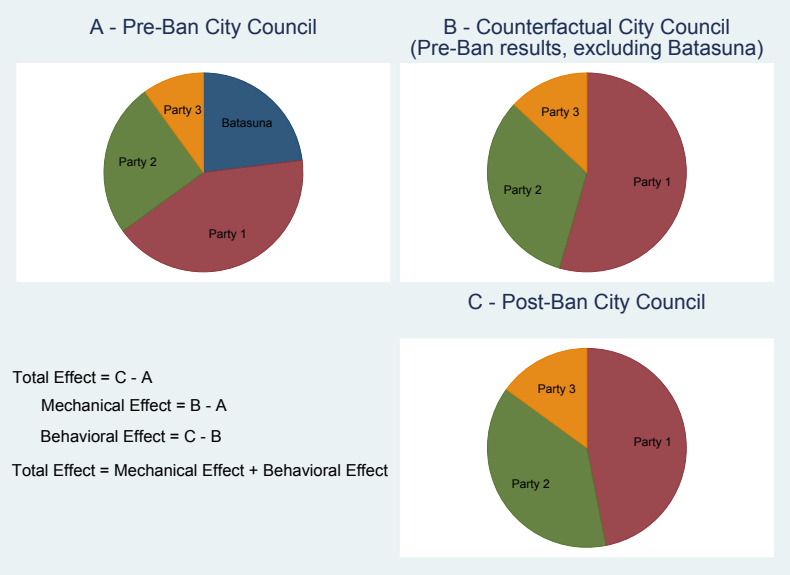
$$E(\beta_m|\text{Ban}) + [E(\epsilon_{mt_{\text{Post}}} - \epsilon_{mt_{\text{Pre}}}|\text{Ban})] - [E(\epsilon_{mt_{\text{Post}}} - \epsilon_{mt_{\text{Pre}}}|\text{Control})]$$

Which means that we will recover a causal effect whenever the unobservable characteristics driving political outcomes in either group follow parallel trends (i.e., $[E(\epsilon_{mt_{\text{Post}}} - \epsilon_{mt_{\text{Pre}}}|\text{Ban})] = [E(\epsilon_{mt_{\text{Post}}} - \epsilon_{mt_{\text{Pre}}}|\text{Control})]$.

Assuming parallel trends means assuming that all sources of unobserved heterogeneity in political outcomes are either (1) municipality-specific and constant over time, or (2) time-specific but constant across municipalities, so that we can construct a counterfactual for the effect of the ban by relying on group and time averages. In that case, $E[\hat{\beta}^{DiD,TE}] = E(\beta_m^{TE}|\text{Ban in 2007})$, and in an heterogeneous treatment effects framework, we would estimate the Average Treatment Effect on the Treated. This is the treatment effect in which we hold most interest since it is the treatment effect for the municipalities where Batasuna used to exist.

This effect is in turn decomposed into its mechanical and behavioral counterparts, by creating a sample of counterfactual city councils (B's, in figure 1.6) and estimating (1) differences-in-differences between the pre-ban city councils and the counterfactual city councils obtained by excluding Batasuna from the pre-ban city council and (2) differences-in-differences between the counterfactual city councils and the post-ban city councils. The sum of the estimated mechanical and behavioral effects is the estimated total effect $\hat{\beta}^{DiD,TE}$.

Figure 1.6: Illustration of mechanical and behavioral decomposition



1.2.2 Results

Table 1.2 shows the estimates of the total, mechanical and behavioral effect of the 2003 ban on a number of measures of fragmentation. Every cell shows the estimate of a separate regression: columns denote the outcome of interest and rows denote the estimated effect. According to instrumental motives, we would expect the ban to have a positive behavioral effect on fragmentation. However, the behavioral effect on the Effective Number of Parties (ENP), which is a measure of fragmentation given by the reciprocal of the Herfindahl index of seat share concentration, is negative and not statistically significant. A similar pattern is observed for the Herfindahl index and for the fraction of Absolute Majorities, although in all cases the relatively small sample does not allow for very precise estimates.

Figure 1.7 illustrates these estimates and decomposition. The red line displays the average outcome for treated municipalities (i.e. those where Batasuna used to exist before the ban), the green line displays the average outcome for control municipalities and the blue line displays the average outcome for the counterfactual city councils (pre-ban election results, excluding Batasuna from the city council) of the treated municipalities. The figures illustrate how for every outcome the counterfactual city council is quite very close to the post-ban city council of treated municipalities. Figure 1.7 also shows that treated and control municipalities were following similar trends before the 2003 ban.

Table 1.3 shows the total, mechanical and behavioral effect of the 2003 ban on the ideological composition of city councils. Every cell shows the estimate of a separate regression: columns denote the outcome of interest and rows denote the estimated effect. In all cases the behavioral effect is smaller than the mechanical effect, close to zero and not statistically significant (although with large standard errors). The results suggest that the absence of a significant behavioral effect on fragmentation is not due to voters casting a vote for Batasuna substitutes, which would be another form of instrumental response to the ban, as voters do not significantly turn towards any particular ideological block (i.e. not by voting for leftist parties to compensate for the absence of a leftist party like Batasuna in the city council). Table 1.8 in the Appendix A shows a similar pattern for the seat shares of the main political parties.

Table 1.2: Effects of the 2003 ban - fragmentation

	ENP	Herfindahl	Abs. Maj
	(1)	(2)	(3)
TE	-0.762***	0.196***	0.287***
	(0.090)	(0.032)	(0.058)
ME	-0.653***	0.166***	0.255***
	(0.021)	(0.009)	(0.027)
BE	-0.111	0.031	0.034
	(0.088)	(0.031)	(0.052)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=653; for Mechanical Effect Regressions, N=866.

Figure 1.7: Mechanical and behavioral effects of the ban

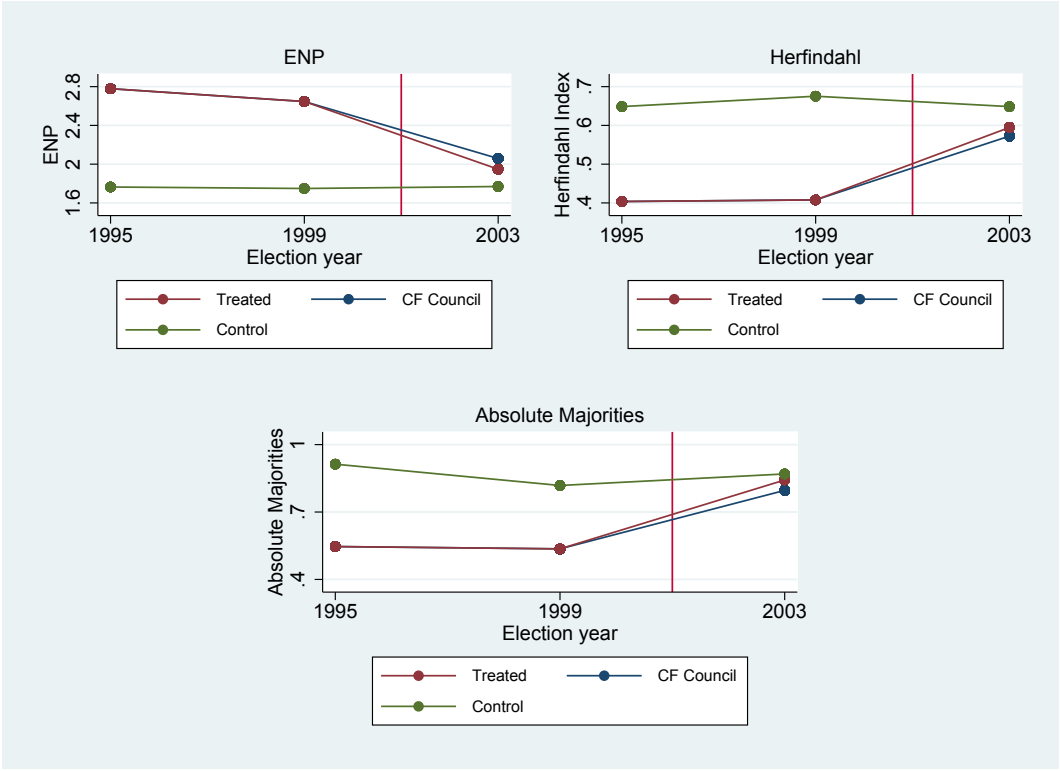


Table 1.3: Effects of the 2003 ban - council ideological composition

	Share Left	Share Right	Share Basque	Share Federal
	(1)	(2)	(3)	(4)
TE	-0.217***	0.199***	-0.130**	0.048**
	(0.017)	(0.051)	(0.054)	(0.021)
ME	-0.224***	0.166***	-0.067***	0.039***
	(0.011)	(0.008)	(0.008)	(0.004)
BE	0.005	0.035	-0.063	0.009
	(0.014)	(0.051)	(0.054)	(0.021)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=653; for Mechanical Effect Regressions, N=866.

Table 1.4: Effects of the 2003 ban - excluding mechanical absolute majorities

	ENP	Herfindahl	Abs. Maj
	(1)	(2)	(3)
TE	-0.575***	0.179***	0.026
	(0.094)	(0.033)	(0.056)
ME	-0.558***	0.180***	-0.008
	(0.027)	(0.014)	(0.006)
BE	-0.019	-0.000	0.035
	(0.092)	(0.033)	(0.055)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=431; for Mechanical Effect Regressions, N=570. Sample excludes those municipalities for which the ban mechanically leads from a coalition to an absolute majority

A relevant feature of the results in table 1.2 is that the ban had a large effect on the existence of absolute majorities. One possible explanation for the absence of behavioral effects, thus, could be a discouragement effect: voters might perceive voting for smaller parties as irrelevant, since their policy influence under an absolute majority in the city council might be limited. To test whether this is driving the results, in table 1.4 I exclude the treated municipalities where the ban is mechanically changing the majority status of the government (i.e. treated municipalities without an absolute majority in the pre-ban city council but with an absolute majority in the counterfactual city council) from the sample. The results are very similar to those in table 1.2, suggesting that this is not the case.

Another possible explanation for the absence of behavioral effects could be that voters just did not expect the call for a null vote to be successful, and thus did not expect the decrease in barriers to entry for small parties. To test whether this is driving the results, I reestimate the effects of the ban by focusing on the 2007 ban, which moreover offers the possibility of exploiting differences in the legal status of Batasuna across municipalities where Batasuna used to exist before the ban (i.e. within the treated in 2003). This has the added value that the size and characteristics of treatment and control groups, which are defined by whether Batasuna was banned in 2007, are more similar, as shown in table 1.6 in the Appendix A which displays descriptives for either group. Table 1.9 in the Appendix A shows the effect of the 2007 ban on measures of fragmentation, showing very similar point estimates and suggesting that the absence of behavioral effects is not due to an unexpected success of the call for a null vote. The similarity of the results is reassuring given that we are estimating the same effects for different treatment and control groups and different periods. Tables 1.10 and 1.11 in the Appendix A show the effects of the 2007 ban on the ideological composition of the city council and the seat shares of the main parties, which are also similar to the effects of the 2003 ban. Table 1.12 in the Appendix A reports results excluding mechanical absolute majorities in counterfactual city councils, with similar results.

1.3 Conclusions

I study the effects of the ban of Batasuna, a political party which after its ban calls for a null vote. In contrast with voting for Batasuna whenever they are legal, which has both expressive and instrumental value, null votes only have expressive value. I exploit the heterogeneity in the pre-ban presence of Batasuna and in the enforcement of the ban across municipalities and elections to show that null voters are two-thirds of the potential Batasuna voters in a regular election, which means that expressive motivations alone explain 65% of the vote for this political party, which before the ban used to obtain between 20 and 25% of the votes in local elections in the Basque Country. The results are similar across two local elections, with different groups of municipalities being exposed to the ban in either election. This result is important because it shows that an important fraction of voters obtain expressive utility from voting for this party and that the vote choice that maximizes their expressive utility is their actual vote choice. The fraction of expressive voters is a relevant parameter to understand to what extent elections aggregate preferences over policy outcomes or non-consequentialist preferences.

I also investigate the consequences for the remaining voters of such a large fraction of the electorate suddenly casting a null vote. Since null votes are not taken into account to compute electoral thresholds, the reaction by Batasuna voters' effectively lowers the barriers to city council entry for small parties, which within proportional systems have potential for policy influence. However, city councils after the ban are, on average, very close to those before the ban but just excluding Batasuna from the city council. The behavioral effect of the ban on measures of fragmentation such as the Effective Number of Parties is close to zero and not statistically significant, and this is not driven by changes in voting behavior across ideological dimensions or political parties. I interpret these results as being consistent with Basque voters voting expressively rather than instrumentally.

I hypothesize that the results might be driven by the strong presence of identity voting in the Basque Country. For instance, Ansolabehere and Puy (2015) use survey data to estimate a spatial model of voting in which voters are not only characterized by their ideal policy, but also by their identity groups, and find that identity, measured by whether the individual

speaks the Basque language, is one of the main predictors of vote choice in Basque regional elections. In local elections, where information and incentives are lower, and decentralization is not a policy issue, identity voting could be even more important. Hence, the results in this chapter are especially relevant for contexts with salient identity politics (i.e. due to ethnicity or religion).

While a number of papers had provided evidence of expressive voting in the lab, to the best of my knowledge this chapter offers a first attempt at identifying expressive voting in an actual election. The next step should be to investigate how often and under what conditions expressive and instrumental choices are in conflict, which is what ultimately would affect the interpretation of the preferences aggregated by elections.

Appendix A

Table 1.5: Descriptive statistics, 2003 ban

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Batasuna Vote Share	0.245	0	0	0	0.220	0
	(0.136)	(0)	(0)	(0)	(0.149)	(0)
Batasuna Seat Share	0.252	0	0	0	0.227	0
	(0.157)	(0)	(0)	(0)	(0.167)	(0)
ENoP	2.711	1.948	1.752	1.770	2.614	1.930
	(0.830)	(0.759)	(0.665)	(0.642)	(0.865)	(0.748)
Absolute majorities	0.542	0.843	0.864	0.870	0.575	0.845
	(0.499)	(0.365)	(0.347)	(0.344)	(0.495)	(0.362)
[1em] Turnout	0.624	0.658	0.666	0.720	0.628	0.664
	(0.0832)	(0.0696)	(0.122)	(0.0780)	(0.0887)	(0.0728)
$\frac{NullVotes}{Population}$	0.00777	0.110	0.0118	0.0161	0.00818	0.0998
	(0.00628)	(0.0868)	(0.0109)	(0.0152)	(0.00697)	(0.0871)
$\frac{NullVotes}{Turnout}$	0.0124	0.168	0.0189	0.0220	0.0130	0.153
	(0.00978)	(0.131)	(0.0174)	(0.0196)	(0.0110)	(0.132)
Seat Share Left	0.340	0.130	0.0455	0.0559	0.310	0.123
	(0.177)	(0.173)	(0.110)	(0.127)	(0.193)	(0.170)
Seat Share Right	0.502	0.719	0.468	0.503	0.499	0.696
	(0.222)	(0.246)	(0.426)	(0.406)	(0.250)	(0.274)
Seat Share Basque	0.784	0.705	0.390	0.453	0.744	0.679
	(0.226)	(0.258)	(0.327)	(0.362)	(0.266)	(0.280)
Seat Share Federal	0.145	0.167	0.201	0.180	0.150	0.169
	(0.172)	(0.185)	(0.239)	(0.244)	(0.181)	(0.191)
Population	10782.4	10612.6	442.9	455.5	9731.7	9550.7
	(34511.9)	(33905.8)	(219.4)	(231.9)	(32856.5)	(32226.9)
#Clusters	198		24		222	

Treated: municipalities where Batasuna used to exist in 1995 and/or 1999. Control: municipalities where Batasuna did not exist in 1995 nor 1999. Pre values are averages of 1995 and 1999 election results. Post values correspond to 2003 election results.

Table 1.6: Descriptive statistics, 2007 ban

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Batasuna Vote Share	0.234	0	0.260	0.298	0.220	0.123
	(0.137)	(0)	(0.135)	(0.134)	(0.149)	(0.177)
Batasuna Seat Share	0.240	0	0.268	0.323	0.227	0.133
	(0.157)	(0)	(0.156)	(0.170)	(0.167)	(0.199)
ENoP	2.724	2.254	2.694	2.918	2.614	2.465
	(0.880)	(0.913)	(0.765)	(0.983)	(0.865)	(0.986)
Absolute majorities	0.539	0.679	0.547	0.435	0.575	0.593
	(0.500)	(0.469)	(0.499)	(0.499)	(0.495)	(0.492)
Turnout	0.630	0.589	0.616	0.589	0.628	0.598
	(0.0872)	(0.0918)	(0.0773)	(0.0825)	(0.0887)	(0.0938)
$\frac{NullVotes}{Population}$	0.00804	0.106	0.00741	0.00737	0.00818	0.0586
	(0.00647)	(0.0773)	(0.00602)	(0.00555)	(0.00697)	(0.0736)
$\frac{NullVotes}{Turnout}$	0.0128	0.182	0.0119	0.0123	0.0130	0.0999
	(0.0104)	(0.123)	(0.00888)	(0.00860)	(0.0110)	(0.121)
Seat Share Left	0.327	0.175	0.356	0.451	0.310	0.280
	(0.172)	(0.202)	(0.183)	(0.185)	(0.193)	(0.242)
Seat Share Right	0.530	0.588	0.467	0.377	0.499	0.490
	(0.223)	(0.279)	(0.217)	(0.215)	(0.250)	(0.288)
Seat Share Basque	0.778	0.651	0.791	0.755	0.744	0.667
	(0.222)	(0.289)	(0.232)	(0.231)	(0.266)	(0.288)
Seat Share Federal	0.156	0.180	0.130	0.119	0.150	0.159
	(0.179)	(0.195)	(0.163)	(0.165)	(0.181)	(0.193)
Population	13648.5	13389.0	7090.3	7227.4	9731.7	9616.6
	(44257.0)	(43649.7)	(13549.5)	(13285.7)	(32856.5)	(32361.7)
#Clusters	114		85		199	

Treated: municipalities where Batasuna was banned in 2007 and used to exist in 1995 and/or 1999. Control: municipalities where Batasuna was legal in 2007 and used to exist in 1995 and/or 1999. Pre values are averages of 1995 and 1999 election results. Post values correspond to 2007 election results.

Table 1.7: Effects of the ban on null votes and change in the electoral threshold

	2003 Ban	
	Dependent Variable	
	Null Vote	Null Vote
	(1)	(2)
$Pre-ban\ Batasuna\ Vote\ Share \times Post2003$	0.698***	0.766***
	(0.064)	(0.185)
$Mech.\ decrease\ in\ SufficientVotes_{TV} \times Post2003$		-0.647
		(2.11)
Municipality F.E.	✓	✓
Council Size × Year FE	✓	✓
R^2	0.557	0.5725
N	650	650
Municipalities	219	219

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. All variables (null votes, pre-ban Batasuna vote share and mechanical decrease in sufficient votes) are defined as a fraction of turnout. Pre-ban: 1995, 1999 elections. Post-ban: 2003 election.

Table 1.8: Effects of the 2003 ban - main political parties seat shares

	PNV	EA	PNV-EA	PNV+EA +PNV-EA	PP	PSOE	IU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TE	-0.005	-0.032	0.164***	0.126**	-0.004	0.007	0.010***
	(0.037)	(0.025)	(0.058)	(0.051)	(0.027)	(0.013)	(0.002)
ME	0.113***	0.036***	0.035***	0.184***	0.017***	0.022***	0.006***
	(0.008)	(0.004)	(0.005)	(0.01)	(0.002)	(0.003)	(0.001)
BE	-0.118***	-0.068***	0.130**	-0.057	-0.021	-0.014	0.004*
	(0.036)	(0.026)	(0.058)	(0.050)	(0.027)	(0.013)	(0.002)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=653; for Mechanical Effect Regressions, N=866.

Table 1.9: Effects of the 2007 ban - fragmentation

	ENP	Herfindahl	Abs. Maj
	(1)	(2)	(3)
TE	-0.673***	0.135***	0.246***
	(0.105)	(0.020)	(0.066)
ME	-0.667***	0.168***	0.237***
	(0.03)	(0.014)	(0.033)
BE	-0.01	-0.028	0.005
	(0.106)	(0.02)	(0.063)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=586; for Mechanical Effect Regressions, N=778.

Table 1.10: Effects of the 2007 ban - council ideological composition

	Share Left	Share Right	Share Basque	Share Federal
	(1)	(2)	(3)	(4)
TE	-0.237***	0.144***	-0.086***	0.036**
	(0.021)	(0.024)	(0.030)	(0.012)
ME	-0.213***	0.161***	-0.063***	0.039***
	(0.013)	(0.011)	(0.01)	(0.006)
BE	0.03*	-0.014	-0.026	-0.003
	(0.018)	(0.023)	(0.031)	(0.012)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=586; for Mechanical Effect Regressions, N=778.

Table 1.11: Effects of the 2007 Ban - main political parties' seat shares

	PNV	EA	PNV-EA	PNV+EA +PNV-EA	PP	PSOE	IU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TE	0.117***	0.057***	0.024	0.199***	0.007	0.034***	-0.001
	(0.024)	(0.016)	(0.015)	(0.029)	(0.006)	(0.008)	(0.005)
ME	0.118***	0.033***	0.026***	0.177***	0.017***	0.0212***	0.005***
	(0.012)	(0.006)	(0.006)	(0.013)	(0.003)	(0.004)	(0.001)
BE	0.003	0.024	-0.003	0.024	-0.011*	0.012	-0.006
	(0.024)	(0.017)	(0.019)	(0.03)	(0.006)	(0.009)	(0.005)

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=586; for Mechanical Effect Regressions, N=778.

Table 1.12: Effects of the 2007 ban - excluding absolute majorities in counterfactual councils

	ENP	Herfindahl	Abs. Maj
	(1)	(2)	(3)
TE	-0.930	0.065	0.076
	(0.575)	(0.055)	(0.183)
ME	-0.674***	0.052***	
	(0.061)	(0.004)	
BE	-0.257	0.013	
	(0.571)	(0.055)	

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Each cell corresponds to the estimate of a separate regression. For Total and Behavioral Effect regressions, N=100; for Mechanical Effect Regressions, N=75. Sample excludes those municipalities for which the counterfactual city council using pre-ban election results but excluding Batasuna from the council features an absolute majority

Data sources and definitions

- Interior Ministry, Spain - 1995, 1999, 2003 and 2007 local elections http://www.infoelectoral.interior.es/min/areaDescarga.html?method=inicio
- Basque municipalities with a proportional electoral system (i.e. with 250 or more inhabitants in the election year)
- Pre-ban presence of Batasuna is defined as Batasuna having obtained votes in the 1995 and/or 1999 local election
- Municipalities where Batasuna used to have 100% of the votes out of votes for candidatures before the ban are excluded from the sample since the counterfactual city council is not defined
- Batasuna: Herri Batasuna - HB (1995), Euskal Herritarrok - EH (1999), Eusko Abertzale Ekintza/Acción Nacionalista Vasca- EAE/ANV (2007)

Parties ideological classification

- Basque: Aralar, Batasuna, EA, Herri-Hats, Iniciativa Ciudadana Vasca, PNV, PNV-EA, Zutik, Parties with abertzale or nacionalista in their names.
- Federal: CDL, CDS, Falange Española, PP, PSOE, Unidad Alavesa, Union Centrista y Liberal
- Left: Aralar, Batasuna, Carlistas, Herri-Hats, Partido Humanista, IU, IU-Aralar, IU-Verdes, IU-Verdes-Aralar, PSOE, Verdes, Zutik, Parties with izquierda, obrero, socialista or progresista their names.
- Right: Falange Española, PNV, PNV-EA, PP, Unidad Alavesa.

Chapter 2

Government fragmentation and government spending: evidence from the ban of Batasuna

Differences in electoral rules explain a considerable amount of variation in countries' government spending.[1] How do electoral rules influence government spending? Possible channels include differences in turnout, government fragmentation, the representation of special-interest parties, political extremism, the quality of politicians or corruption.[2] The effect of electoral rules on each of these channels is heterogeneous, and hence it is important to understand what is the role of each of these mechanisms.

This chapter focuses on government fragmentation. Especially for local elections, typically featuring a single electoral district, one of the main differences between electoral rules is the incidence of coalition governments.[3] Proportional electoral rules lead to multi-party systems and frequent coalition governments, as opposed to plurality or majoritarian elec-

[1] Across countries, Milesi-Ferretti *et al.* (2002), Persson and Tabellini (2004), Persson *et al.* (2007), within countries (local elections) Bordignon *et al.* (2014), Ade (2013), Coate and Knight (2011)

[2] For proportionality and turnout, see Eggers (2015); for proportionality and the quality of politicians, Mattozzi and Merlo (2015); for special-interest parties Lizzeri and Persico (2005) and Folke (2014); for political extremism Bordignon *et al.* (2014); for corruption Myerson (1993) or Persson *et al.* (2003)

[3] A number of papers analyze the trade-off arising from electoral rules differences in districting (which typically exist in national elections), such as Persico and Lizzeri (2001), Gagliarducci *et al.* (2011), Funk and Gathmann (2013) or Beath *et al.* (2014)

toral rules, which lead to two party systems.[4] Whenever a coalition party has some degree of autonomy and spending discretion, it has incentives to overspend since it partially shares the costs of spending with the coalition partner.[5] On the other hand, a number of lumpy spending projects require coalition parties to reach agreements. Failure to reach such agreements due to the veto power of coalition parties could lead coalition governments to spend as much or even less than single-party governments due to legislative gridlock.[6]

The existence and the magnitude of the effect of government fragmentation on government spending is an empirical question, but coalitions and single party governments do not only differ in terms of government fragmentation. Within proportional electoral systems, absolute majorities are driven by electoral success, which is not randomly assigned with respect to policy outcomes. For instance, candidates or platforms managing to obtain an absolute majority are likely be different in unobservables related to policy outcomes. If voters prefer educated politicians, these will be more likely to hold absolute majorities and they might reduce spending by improving efficiency or because they have weaker preferences for redistribution and public services.[7] Moreover, if political parties expect a large budget in the following electoral term, parties might put more effort into winning the election, making it more difficult for a single party to hold an absolute majority.

In order to address these identification problems, in this chapter I use variation induced by the ban of Batasuna. The ban was an important shock for Basque local politics, since, at the time of the ban, Batasuna used to hold more than 20% of the seats in Basque city councils.[8] As we saw in chapter 1, after its ban, Batasuna called for a null vote, which was quite successful, and the effects of the ban on political outcomes were mostly mechanical, with counterfactual city councils constructed using pre-ban election results but excluding Batasuna from the city council explaining most of the changes due to the ban. This means that the ban triggered a reshuffling of seats in city councils, such that the remaining parties

[4]Under plurality or majoritarian rules, ideologically close parties have incentives to merge since what matters is being the most voted party, and voters have incentives to vote for larger parties. This effect leads to two party competition - Duverger (1954), Lijphart and Aitkin (1994)).

[5]Bawn and Rosenbluth (2006) and Persson *et al.* (2007), Primo and Snyder (2008)

[6]Alesina and Rosenthal (1995), Tsebelis (2002), Blais *et al.* (2010), Freier and Odendahl (2012).

[7]Gagliarducci and Nannicini (2013), Alesina and Giuliano (2009)

[8]As in the rest of Spain, in the Basque Country the seats of municipal city councils are allocated using the d'Hondt Method, a proportional rule, with a 5% threshold and blocked lists

increased their vote shares and seats rather proportionally, which in some cases changed the majority status of the local government (i.e. whenever the first party gained enough seats due to the ban to hold an absolute majority). In some others, it did not (i.e. whenever there was already an absolute majority or when the remaining parties were fragmented enough). Using data on municipal fiscal and political outcomes over four electoral terms, I compare spending changes in municipalities where the ban triggers a change in government fragmentation with spending changes in municipalities where the ban does not change the majority status of the government, within municipalities where Batasuna used to be iqually emportant. The variation induced by the ban is interesting as it allows a comparison to be drawn between absolute majorities and coalitions which were quite fragmented or balanced, as opposed to the variation in close absolute majorities, in which a party holds almost 50% of the seats and where discretion by either coalition party, which is the main driver of common pool problems, might not be operating.

I find that absolute majorities reduce current spending significantly but increase capital spending by a similar amount, although the latter is imprecisely estimated. According to instrumental variables estimates, absolute majorities have a negative and statistically significant effect on current spending of €80 per-capita. This is driven by a significant negative effect of €60 per-capita on spending on public goods and services, which includes the most targetable types of spending at the local level, such as health care, care for the elderly, and cultural or sports activities. Absolute majorities increase capital expenditures by a similar amount; however, this effect is not statistically significant, and it implies that the estimated effect of absolute majorities on total spending is close to zero but with a wide confidence interval. On average, the fraction of total spending which absolute majorities devote to current expenditures is around 5 percentage points lower. These results are consistent with common pool models, since coalitions spend more on current spending, which is easier to split and to target, and it is the type of spending which could be affected by ministerial discretion by coalition parties, which is the driver of the common pool mechanism. These results are also consistent with veto player models, as coalitions spend relatively less on capital expenditures, which are lumpy and might entail higher bargaining costs. These results are not driven by changes in the ideological composition of the city councils and suggest that

coalitions adjust between types of spending rather than the level of spending.

This chapter extends the existing empirical literature on the effects of government fragmentation on public spending. Across countries, Bawn and Rosenbluth (2006) find that government fragmentation is positively correlated with total government outlays as a percent of GDP, and Persson *et al.* (2007) use the electoral rule (plurality vs. proportional) as an instrument for the incidence of coalition governments, following their theoretical model which suggests that electoral rules affect spending only through this channel, and also find support for the common pool hypothesis.[9]

Other papers have relied on within-country variation, with mixed support for the common pool hypothesis.[10] Solé-Ollé (2006b) uses data on Spanish municipalities and finds that coalition governments are correlated with higher levels of government spending. Schaltegger and Feld (2009) use panel data on Swiss cantons and do not find any significant relationship between the incidence of coalition governments and government size. Baskaran (2013) performs a similar analysis on German States, reaching similar conclusions. In addition to fixed effects regressions he provides instrumental variables estimates using the number of parties in the state parliament as an instrument for coalition governments.

Some recent working papers combine within-country variation with exogenous sources of variation. Freier and Odendahl (2012), Garmann (2012) and Artés and Jurado (2014) use close absolute majorities in Regression Discontinuity Designs (RDDs) and do not find that coalitions spend more than single party governments (in some cases, in fact, they find that they spend less). A contribution of this chapter is that it identifies the effect of absolute majorities over a subpopulation which is different from that in an RDD with close absolute majorities.[11] This is because the municipalities which are mechanically pushed by the ban of Batasuna towards an absolute majority would have had more balanced coalitions (i.e. with the largest party not necessarily holding almost 50% of the seats, as in a close election).[12]

[9] Other cross-country studies include Woo (2003) or Kontopoulos and Perotti (1999)

[10] A related strand of the literature (Egger and Koethenbuerger (2010), Pettersson-Lidbom (2012), Aidt and Shvets (2012) or Saarimaa and Tukiainen (2015)) has investigated the existence of common pool problems related to cabinet size, term limits, or municipal mergers.

[11] Conditional on a number of assumptions, IV estimate the Local Average Treatment Effect (LATE), the average treatment effect for the compliers (Imbens and Angrist, 1994)

[12] One could also think of absolute majorities as a censored measure of single-party governments. Under non-classical measurement error, OLS would be biased additively by omitted variables and attenuated

This is relevant since the common pool mechanism is based on every coalition party having some spending discretion, and thus one could expect more balanced coalitions to have larger common pool problems compared to the case of close absolute majorities, in which one party holds almost 50% of the seats and just needs a small support, being likely to be able to form a minority government in which only one party has spending discretion and reaches punctual agreements with the opposition. On the other hand, the veto player mechanism might still operating for close absolute majorities, which could explain why some of the existing evidence exploiting close absolute majorities finds a positive effect of absolute majorities on spending.

2.1 Identification and estimation

I use data from the 1995, 1999, 2003 and 2007 municipal elections in the Basque Country, collected by the Spanish Interior Ministry.[13] These data include turnout, spoilt –"null"– votes, votes and seats for each party in every municipality and election.

Table 2.5 in the Appendix B shows descriptive statistics of the main political outcomes before and after the ban averaged over the corresponding periods. While turnout remains stable, null votes (as a fraction of turnout) sharply increase from less than 1% before the ban to more than 10% after the ban, since whenever Batasuna could not contest the elections it was asking for a null vote. While almost 35% of municipalities had an absolute majority before the ban, after the ban this percentage increased up to 52%. Similarly, the Effective Number of Parties (ENP), a measure of legislative fragmentation given by the reciprocal of the Herfindahl index of seat share concentration, declines after the ban from around 3 to 2.6.[14]. The average seat share of the sum of all leftist parties decreased by 8% and the average seat share of right wing parties increased accordingly.[15] The share of Nationalist declined by 8% but the share of Federal parties increased by 3%. Overall, the ban increases

because of measurement error, RDD would be attenuated because of measurement error, and IV would be expanded because of measurement error, providing an upper bound for the effect of single-party governments. Under this interpretation, the intuition and the reason why the identification strategy in this chapter is of added value are very similar

[13]Pre-ban: 1995 and 1999 elections. Post-ban: 2003 and 2007 elections. These data can be freely downloaded from `http://www.infoelectoral.mir.es/min/`, in the "Área de Descargas"

[14]The ENP was introduced by Laakso and Taagepera (1979)

[15]Parties are classified as leftists (mainly Batasuna, the PSOE and IU), centrists (mainly EA) and right-wing (mainly the PNV, the joint lists PNV-EA and the PP)

the average left-right gap in Basque city councils, while reducing the Basque-Federal gap. Regarding Batasuna, table 2.5 shows that before the ban it used to hold 21.5% of the seats in the city councils in the region.[16] After the ban, this fell until 6% (instead of 0%) because in 2007 the courts could not block all the candidatures of EAE-ANV. To sum up, these descriptive statistics show that the political environment changed substantially after the ban of Batasuna. However, chapter 1 shows that, partially due to the success of the call for a null vote, the effects of the ban on Basque city councils are mostly mechanical effects: if we construct a counterfactual city council using the pre-ban election results but excluding Batasuna from the city council, this explains most of the effect of the ban in terms of fragmentation and ideological and party composition: the ban did not significantly change electoral behavior towards any specific direction.

Basque local governments have spending responsibilities in a number of areas, similarly to most European local governments. All municipalities must provide and mantain street lighting, waste collection, cemeteries, street cleaning, drinking water, sanitary sewer, road paving, and most of them also public parks, libraries, civil protection, primary health care, care for the elderly, public sports facilities, environmental protection, urban planning or public transportation. I use panel data on yearly fiscal variables (spending and budget balance) over 15 years (from 1997 to 2011), which correspond to four electoral terms (1996-1999, 2000-2003, 2004-2007 and 2008-2011).[17]

Table 2.6 in the Appendix B displays descriptive statistics of yearly local spending and revenues in per-capita €2011.[18] Regarding spending, around 60% is devoted to current expenditures, with personnel expenditures and goods and services expenditures being its largest components. Regarding capital spending, it mainly consists of investments. A small share is devoted to capital transfers and debt service. The main sources of revenues of Basque municipalities are "own" revenues (mostly local taxes and fees), grants, and debt. On average, grants are the larger component of revenues (almost 60%) whereas current grants are twice as large as capital grants. Own revenues represent almost 40% of total revenues,

[16] Before the ban, Batasuna was named Herri Batasuna and Euskal Herritarrok. After the ban, EAE-ANV

[17] The municipalities of Markina-Xemein and Ziortza-Bolibar are excluded form the sample as they used to be a single municipality until they split in 2003.

[18] Data on revenues and spending have been obtained from EUSTAT, the regional statistics service of the Basque Country

and the share of revenues that is obtained from debt is lower (around 4%). Typically current spending is mostly funded out of own revenues and unconditional grants and capital spending is largely funded out of conditional capital grants. On average, Basque municipalities have some budget imbalances, but these are not very large: 1.5% of their total revenues.

I use the dataset which includes the municipalities' yearly spending variables from 1997 to 2011 merged with the political variables corresponding to the 1995, 1999, 2003 and 2007 elections to investigate the effect of government fragmentation on government spending by estimating the following regression:

$$\text{Government spending}_{mt} = \alpha_m + \delta_{p(m),t} + \beta \text{Absolute Majority}_{mt} + \phi' \text{X}_{mt} + \epsilon_{mt} \tag{2.1}$$

The fiscal policy of Municipality m in year t is regressed on an indicator variable which is equal to one if there is an absolute majority in the city council (a party holding more than 50% of the seats).[19] So as to control for time-invariant municipality-specific unobserved heterogeneity, I include municipality fixed effects, and to control for time-specific municipality-invariant unobserved heterogeneity I include province-specific year fixed effects, which are denoted by $\delta_{p(m),t}$.[20] X is a vector of municipality-specific time-variant demographic controls.

We are interested in estimating the effect of Absolute Majorities on public spending (β). However, coalitions and single party government might be different in dimensions related to policy other than government fragmentation. Specifically, within a proportional system, absolute majorities are more successful electorally and on average have large electoral advantages, and we cannot treat this as being randomly assigned. For example, if parties have a preference for managing large budgets, if they anticipate that important spending projects will have to be pursued after the elections, they will exert more effort in the elections, making it more difficult for a single party to hold an absolute majority. This could lead to a correlation between government fragmentation and spending even in the absence of a causal effect of government fragmentation on spending. Besides this reverse causality problem,

[19] 1997-1999 fiscal variables - 1995 election results, 2000-2003 fiscal variables - 1999 election results, 2004-2007 fiscal variables - 2003 election results and 2008-2011 fiscal variables - 2007 election results

[20] There are three provinces in the Basque Country: Álava (Vitoria), Bizkaia (Bilbao) and Gipuzkoa (San Sebastián). Time effects are province-specific since a fraction of the local budget is funded by provincial authorities

one would expect candidates or platforms managing to obtain an absolute majority to be different in unobservables from candidates or platforms who do not manage to obtain such a majority. Some unobservables potentially related to electoral success, such as politicians' quality, education or preferences for spending, are likely to be related to government spending as well.

In an effort to address these identification problems, I use the ban of Batasuna to construct an instrument for absolute majorities. First, I construct a sample of counterfactual city councils, taking the pre-ban election results and excluding Batasuna from the city council. Building on the findings in chapter 1 that these counterfactual city councils explain most of the effect of the ban on city councils in terms of fragmentation, ideological blocks and party shares, I look at municipalities where the ban mechanically changes the majority status of the local government (leading to an absolute majority), holding constant the pre-ban importance (i.e. the seat share) of Batasuna. The first stage regression is given by:

$$\begin{aligned} \text{AM}_{mt} = \pi_m + \rho_{p(m),t} + \gamma(\text{Mechanical Change in AM})_{mt} \\ + f(\text{Mechanical Change in Batasuna's seat share})_{mt} + \theta' \text{X}_{mt} + \upsilon_{mt} \end{aligned} \tag{2.2}$$

Where:

Mech. Change in AM_{mt}=0; $t \in [1997, 2003]$

Mech. Change in AM_{mt}=(Pre-Ban AM | Counterfactual City Council)$_m$−(Pre-Ban AM)$_m$; $t \in [2004, 2011]$

Mech. Change in Batasuna's seat share$_{mt}$=0; $t \in [1997, 2003]$

Mech. Change in Batasuna's seat share$_{mt}$=(Pre-Ban Batasuna's seat share)$_m$; $t \in [2004, 2011]$

It is important to be precise about the variation that I exploit for identification. I account for systematic differences between municipalities by including municipality fixed effects, which means that I exploit the changes due to the ban within each municipality. At the same time, I include province-specific year dummies, to account for any time-variant changes in spending needs or capacity that are common to all municipalities in a province. Since the ban could have different effects on spending depending on how important Batasuna used to be, I account for such differences by controlling for a flexible function (i.e. a

polynomial) in the mechanical change in the seat share of Batasuna, which is equal to zero for all municipalities before the ban, and equal to the pre-ban seat share of Batasuna after the ban.[21]

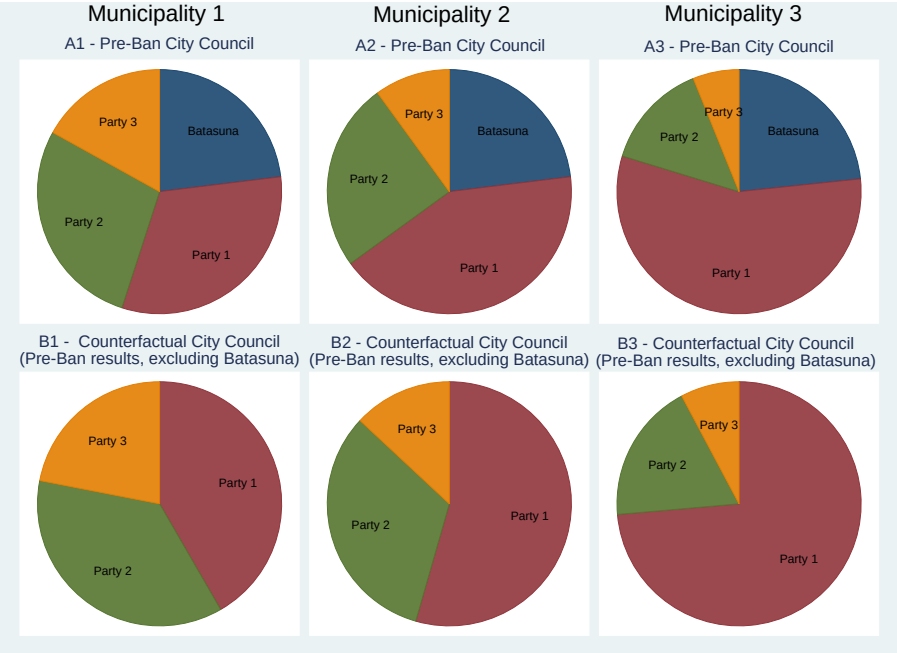
The instrument for the existence of an absolute majority in the city council is the Mechanical Change in Absolute Majority due to the Ban of Batasuna. The instrument is equal to zero for the pre-ban period, since before the ban there are no mechanical changes due to the ban. After the ban (2003), it is equal to the municipality specific difference in absolute majority between the pre-ban actual city councils and the pre-ban counterfactual city councils.[22] The counterfactual city council is obtained by applying the post-ban electoral rule (i.e. the votes of Batasuna do no longer translate into seats) to the pre-ban voting results. For a given pre-ban seat share of Batasuna, there are three types of municipalities, illustrated in Table 2.1. Municipalities like municipality 1, where the remaining parties are fragmented enough so that the ban does not mechanically change the majority status of the government; municipalities like municipality 2, where the first party mechanically gains enough seats due to the ban to reach an absolute majority; municipalites like municipality 3, where the first party already had an absolute majority and therefore the ban does not mechanically change government fragmentation. Notice that these three cases can hold, as in the example, for a given pre-ban seat share of Batasuna. I compare policy changes in municipalities like municipality 2 against policy changes against municipalities like municipality 1 and 3, where the ban does not mechanically change government fragmentation: the first-stage is essentially a differences-in-differences regression which controls for other changes occurring at the same time (i.e., Batasuna disappearing, and with what intensity). Note that the comparison is in terms of mechanical changes and not actual changes (which are endogenous). In order to obtain a treatment effect estimate for absolute majorities we will scale the regression of the outcomes on the mechanical change by the first stage.

It must be emphasized that my identification assumption is not that the instrument is

[21] In practice the polynomial is a quadratic term, but higher order polynomials in the mechanical change of the seat share of Batasuna give similar results

[22] In practice, it is the average mechanical change of the mechanical change using the 1995 election results and the mechanical change using the 1999 results to construct the counterfactual city council. This means that if a municipality is mechanically pushed from a coalition towards an absolute majority in both counterfactual city councils, the instrument is equal to 1 after the ban; it its only pushed towards an absolute majority for one of the counterfactual city councils, the instrument is equal to 0.5 after the ban

Table 2.1: Heterogeneity in Mechanical Effects, conditional on Batasuna's weight



valid unconditionally but conditionally, because the disappearance of Batasuna could have effects on policy *per se* and the mechanical changes in Absolute Majorities could be correlated with the pre-ban weight of Batasuna. After conditioning on the mechanical change in the seat share of Batasuna (i.e. fixing the pre-ban importance of Batasuna) there is still variation in the mechanical changes in Absolute Majorities coming from differences in the pre-ban level of fragmentation of the remaining parties, as shown in Table 2.1. The exclusion restriction, therefore, is that for a given pre-ban seat share of Batasuna, the ban has a different effect on the municipalities where the remaining parties had intermediate levels of fragmentation before the ban (i.e., municipalities like municipality 2), only because it triggers a change in government fragmentation. The variation that I exploit is net of any effects of the ban which would be proportional to the importance that Batasuna used to have, (i.e. due to Batasuna specific policy preferences). Moreover, I allow the effect of the mechanical change in the seat share of Batasuna to change with a measure of fragmentation of the remaining parties (the pre-ban seat share of the first party other than Batasuna). In the example in table 2.1, this means that I allow the effect of the disappearance of Batasuna to be decreasing between

municipality 1, 2 and 3.

In addition to these identification assumptions, the correspondence between the mechanical changes in Absolute Majorities and the post-ban Absolute Majorities, which is not perfect, has to be strong enough. Table 2.2 details the results of the first-stage regression. The mechanical change in absolute majority is a significant predictor of having an absolute majority after the ban. The heteroskedasticity-robust F statistic is equal to 69.82, which is significantly larger than the critical value for weak instruments, as tabulated by Stock and Yogo (2002). To improve on precision in the second stage, I include controls for time-variant and municipality-specific demographic characteristics: a third order polynomial in the log of the municipality's total population and dummies for population thresholds at which funding from national or provincial authorities change.

Table 2.2: First Stage

	Dependent Variable
	Absolute Majority
	(1)
Mech. Change in Absolute Majority	0.677***
	(0.081)
f(Mech.Change, Batasuna Seat Share)	✓
Demographic Controls	✓
Municipality F.E.	✓
Province-Year dummies	✓
Kleibergen-Paap F	69.82
R^2	0.1545
N	3320

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Estimates weighted by municipality population.

In a constant treatment effects framework, the relevance of the first stage together with the validity of the previously stated identification assumptions would be sufficient for the IV estimator to consistently estimate the Average Treatment Effect of Absolute Majorities. However if the gain from treatment is not constant, in order for the IV estimator of equation

to consistently estimate a causal effect we need a further assumption: monotonicity. In this context, monotonicity implies that there should be no municipalities which would be less likely to have an absolute majority after a mechanical increase in Absolute Majority due to the ban. If monotonicity is satisfied, according to the Angrist-Imbens-Rubin framework of heterogeneous treatment effects (Angrist *et al.*, 1996), the IV estimator of equation 2.1 will consistently estimate the Local Average Treatment Effect (LATE) of Absolute Majorities on Policy, which is the average treatment effect for the subpopulation of compliers: those municipalities such that their government fragmentation is affected by the mechanical changes due to the ban of Batasuna.

The monotonicity assumption is fundamentally untestable as we cannot observe counterfactuals for a particular municipality. In the context of the ban of Batasuna there is a particular mechanism that could be at work, which if sufficiently strong could lead to a violation of monotonicity. This mechanism would be the expected decrease in the electoral threshold due to the expectation that a non-negligible share of the former voters of Batasuna would cast a null vote. This raises the incentives to vote for smaller parties, and therefore it could be that a mechanical increase in absolute majorities comes along with a decrease in absolute majorities. However, I believe that monotonicity is unlikely to be violated, for the following reasons. The results in chapter 1 show (1) that the changes in government fragmentation due to the ban are mostly driven mechanically and (2) that the sign of the behavioral effect is the same as the mechanical effect. Moreover, the expected increase in proportionality would be proportional to the mechanical change in the seat share of Batasuna, but the variation that I exploit is conditional on the mechanical change in the seat share of Batasuna. Furthermore, table 2.12 in the Appendix B shows how the fraction of null vote, which is the main driver of the absence of strategic behavior, is uncorrelated to the mechanical changes in absolute majority (the regression is the same specification as the first stage but with null votes as the dependent variable).

2.1.1 Results and Interpretation

Table 2.3 reports the OLS and IV results. The dependent variables are total expenditures and its two main components, current and capital expenditures; and the budget balance as

a fraction of revenues. According to the IV estimates, absolute majorities reduce current expenditures by €87 per capita. The magnitude of this estimate is important, as the effect of absolute majorities on Current Expenditures is of around 9% of its sample mean. Regarding the effect of absolute majorities on capital expenditures, the estimate is imprecise, and it has opposite sign and a similar magnitude (€63 per capita) as the effect on current expenditures, and thus the net effect on total spending is negative, close to zero but imprecisely estimated due to the imprecision of the effect on capital spending. In relative terms, current expenditures as a fraction of total expenditures decline by 5 percentage points. Finally, while absolute majorities have an estimated negative effect on the budget balance, it is imprecisely estimated and not statistically significant. These results are consistent with veto player models, as coalitions spend relatively less on capital expenditures, which are lumpy and more difficult to split between coalition parties, but also with common pool models, since coalitions spend more on current expenditures which are easier to split and to be targeted. However, the results suggest that the margin of adjustment is between different types of spending rather than total spending.

Considering an heterogeneous treatment effects framework (under which IV estimates the LATE) to interpret the results, it is interesting to compare the results to those of other papers which have estimated causal effects of absolute majorities using alternative identification strategies. Concretely, some recent working papers have used close absolute majorities in a Regression Discontinuity Design (RDD) as an alternative identification strategy, which delivers a different local treatment effect. This is because the common-pool problem for coalition governments arises from the fact that each coalition party has some degree of discretion, unilateral or agenda setting powers in a certain area, an area which their voters value and for which they are hold accountable. It seems likely that this problem would be more significant in cases where parties in the coalition have more balanced weights, such that the small party (or parties) in the coalition is larger relative to the largest party and thus more likely to have more bargaining power to decide unilaterally or set the agenda in a certain area. Regarding the effective veto power of coalition parties, it need not be smaller in unbalanced coalitions, at least not as much as for common pool problems. Therefore, the IV estimates in table 2.3 measure the effect of a change in government fragmentation in

Table 2.3: Instrumental Variables

	Dependent Variable							
	Total Exp.		Current Exp.		Capital Exp.		Budget Balance	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Absolute Majority	13.12	-23.87	-7.468	-86.96**	20.58	63.09	-0.001	-0.035
	(27.84)	(69.23)	(7.936)	(33.96)	(26.11)	(59.44)	(0.01)	(0.029)
f(Mech.Change, Batasuna Seat Share)	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓	✓	✓	✓	✓
Kleibergen-Paap F		69.82		69.82		69.82		69.82
R^2	0.16		0.0834		0.1265		0.000	
N	3320	3320	3320	3320	3320	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. All estimates weighted by municipality population.

Table 2.4: Reduced Form and Placebo Test

	Dependent Variable				
	Abs. Majority	Total Exp.	Current Exp.	Capital Exp.	Budget Balance
	(1)	(2)	(3)	(4)	(5)
Mech. Change in AM	0.697***	-15.36	-57.86**	42.71	-0.02
	(0.086)	(47.14)	(23.03)	(41.39)	(0.02)
Placebo Mech. Change in AM	-0.048	-1.901	-2.394	0.493	-0.008
	(0.046)	(50.66)	(14.29)	(44.16)	(0.023)
f(Mech.Change, Batasuna Seat Share)	✓	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓	✓
R^2	0.1580	0.1607	0.0868	0.124	0.000
N	3320	3320	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues and absolute majority which is an indicator variable. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The placebo mechanical change in absolute majority is the mechanical change as if the ban had happened in 1999 (i.e., a lead of the mechanical change in absolute majority). The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. All estimates weighted by municipality population.

municipalities where coalitions were relatively more balanced in terms of parties' seat shares, compared to a case in which the first party holds almost 50% of the seats, as it is the case for compliers in an RDD with close absolute majorities. This can explain the difference between these results and the findings of some recent RDD studies, such as Freier and Odendahl (2012), Garmann (2012) or Artés and Jurado (2014), which find that coalitions they spend less than absolute majorities or the same, as the variation in close absolute majorities is more unlikely to pick up common pool effects.[23] The size of the effects of absolute majorities in this chapter, which is large, is comparable to the size of the effects of absolute majorities in other papers such as Freier and Odendahl (2012), Garmann (2012), or Merilainen (2013). However, since the identification strategy in this chapter arises from a very large shock, it is difficult to completely rule out violations of the exclusion restriction. Nevertheless, in the following section I try to address a number of testable identification threats.

2.1.2 Channels and robustness

A concern about the validity of these estimates, which are differences in differences, is that the results might be driven by differential time trends. I investigate this concern by testing whether the municipalities which are mechanically pushed by the ban towards an absolute majority were trending differently before the ban. Table 2.4 shows the results of the reduced form -the regression of the outcomes of interest on the instrument-, and a placebo instrument, as if the ban had taken place in 1999 (i.e., a lead of the instrument). The coefficients on the placebo instrument are very close to zero and never statistically significant. Even though the estimates for the placebo instrument are not very precise, the difference in magnitude between the true reduced form and the placebo reduce formed corresponding to the first stage and the regressions on current expenditures and capital expenditures are large. Overall, the results suggest that the IV results are not driven by differential time trends across treated and control municipalities.

The ban of Batasuna was an important shock, and it did not affect only government fragmentation, but also the composition of the city council. Pettersson-Lidbom (2008) and

[23] An exception is Merilainen (2013), using the same identification strategy and using data on Finnish municipalities, finds that absolute majorities reduce spending initally, but the effect decreases over time

Folke (2014) show that this can have significant effects on policy outcomes. Table 2.13 in the Appendix B reports the IV results including controls for the mechanical change in the seat share of leftist parties, the mechanical change in the seat share of basque nationalist parties and the mechanical change in the incumbent, which are exogenous controls. The mechanical change in the incumbent controls for differences in the municipalities in which the ban mechanically changed the Mayor.[24] The aim of controlling for the mechanical change in the incumbent is to address concerns due to the fact that the change in the party in office could have effects on policy.[25] This could arise because of learning, Batasuna-specific policy preferences or changes in transfers due to a change in the partisan alignment of the local government with respect to the regional or central government, a mechanism which Curto-Grau *et al.* (2012) and Solé-Ollé and Sorribas-Navarro (2008) have shown that can be important for a sample of Spanish municipalities. The change in the incumbent and in the ideological composition of the council is a direct effect of the ban, which according to the identification assumption should be proportional to the mechanical change in the seat share of Batasuna and therefore should not change the estimates. Table 2.13 show very similar estimates, suggesting that these changes are not driving the results and that the flexible function in the mechanical change in the seat share of Batasuna is doing a good job in capturing the direct effects of the ban.

To further understand the channels through which absolute majorities and coalitions spend differently, table 2.8 in the Appendix B provides estimates of the effect of absolute majorities on the main components of current expenditures (public goods and public services) and capital expenditures (investment). These are both the main components and the main drivers of the effects of absolute majorities on spending. Absolute majorities reduce spending in public goods and services by €60 per capita, and this effect is significant, while they increase investment by €32 per capita, although this effect is not precisely estimated. Moreover, table 2.8 also shows the estimated effect of absolute majorities on spending on civil security and public safety. This spending category is not a subset of current or capital

[24]This variable is equal to zero before the ban, for all municipalities. After the ban, it is equal to one if the Mayor used to be a Batasuna candidate in both pre-ban electoral terms, equal to 0.5 if it used to be a Batasuna candidate only in one of the pre-ban electoral terms, and zero otherwise

[25]Batasuna used to be in office for at least one of the pre-ban electoral terms in 16.5% of the municipalities

expenditures but corresponds to an alternative functional classification. It is interesting to look at the effect on this type of spending since the ban could have led to some protests and riots, and it is reassuring with regards to the exclusion restriction to observe that the estimated effect is close to zero and not statistically significant.

Tables 2.9, 2.10 and 2.11 in the Appendix B displays further IV results for a number of subsamples and robustness checks. Table 2.9 shows results using only treated municipalities where the party that is mechanically being pushed towards an absolute majority is center or left. In most of the treated municipalies in the sample (80% of the treated), the party being mechanically pushed towards an absolute majority is conservative (Christian-democratic), and therefore, one concern about the results could be that they only represent the effect for right-wing absolute majorities vs. coalitions (recall that the variation that I exploit holds constant the ideological composition of the council). The results in table 2.9 are very similar to the baseline results, which suggests that the estimated effects are not driven by the fact that most majorities arising due to the ban are right-wing.

Table 2.10 shows results using only pre-ban coalitions as controls (i.e. exploiting only the variation between municipalities like municipality 1 and municipality 2 in table 2.1. Most of the control municipalies (75% of the controls) in the sample had an absolute majority before the ban (i.e. are like municipality 3 in table 2.1), and therefore, one concern about the results could be that the effect for the ban is stronger in the treated municipalities than in control municipalities and that this is driving the results rather than government fragmentation. This could be the case, for example, if Batasuna were often participating in governing coalitions. If this were the case, the results using variation only between the treated municipalities and the pre-ban coalition control municipalities would have the opposite sign as the baseline results using both the pre-ban coalitions control municipalities and pre-ban absolute majorities control municipalities. Another possibility could be that in treatment municipalities, other parties have more incentives to capture disenfranchised voters and change policy accordingly. However, the point estimates in table 2.10 are quite similar to the baseline results (although given the lower number of municipalities, precision is lost), suggesting that this is not biasing the IV estimates.

Table 2.11 displays further IV results for a number of subsamples: election and non-

election years, excluding the year of the ban (2003) , and excluding the years of the crisis (using only 1997-2008 observations). The results show that the effect on current spending is slightly larger in election years, which is consistent with the electoral motive behind common pool mechanism. The point estimates are smaller excluding the crisis period (the effect on current and capital spending is of €60 per-capita rather than €80 per-capita), but overall the results seem to be qualitatively similar across these samples: in all cases, there seems to be a substitution from capital towards current spending by coalition governments, leaving total spending rather unaffected (although with a large confidence interval).

A final concern about the validity of these estimates is the existence of general equilibrium effects. The existence of such effects, which could arise in the form of spending spillovers (i.e. fiscal decisions of one jurisdiction influencing the fiscal decisions of its neighbors), would violate the Stable Unit Treatment Value Assumption (SUTVA) which is needed to causally interpret the estimates in this chapter. This assumption is not testable and estimating whether spending spillovers exist is challenging. While Solé-Ollé (2006a) provides estimates of such spillovers for a sample of Spanish municipalities, finding positive effects mostly in urban areas, recent causal evidence using US data (Isen, 2014) does not find any spending externalities at the local level. In the case that such externalities were important for the Basque Country, this would lead to underestimate the effects of absolute majorities, as municipalities without absolute majorities would reduce their spending in response to the reduction in spending by neighboring municipalities which face an absolute majority due to the ban. However, the fact that such externalities are more likely to arise in urban environments and that the largest municipalities in the Basque Country i.e. with more than 50000 inhabitants) in the sample are not affected by mechanical changes in absolute majorities suggests that a potential violation of SUTVA is not likely to have a large effect on the estimates presented in this chapter.

2.1.3 Absolute majorities or single-party governments?

It must be emphasized that I only observe whether there is an absolute majority in the city council or not, and that although I refer to the governments which do not hold an absolute majority as coalitions, some of them could actually be single-party minority governments.

This partition is common in the literature because very often it is only possible to observe whether there is an absolute majority in the city council but not whether there is a proper coalition or a single party government whenever there is no absolute majority. This is the case especially for local governments, for which larger and more homogeneous samples are available.

If we are interested in the effect of absolute majorities, this is not a problem. However, if some minority single party governments do have *de facto* as much power as absolute majorities, possibly because there is no viable alternative coalition willing to collude to turn down the mayor's proposals, the actual effect of interest is that of single party governments. In fact, in Spain, the electoral rule is such that if no viable coalition has an absolute majority, the head of the most voted party automatically becomes the mayor, a mayor which has strong powers, and the approval of the budget requires only a simple majority in the city council (i.e. more votes in favor than against), instead of an absolute majority (i.e. more than 50% of votes in favor). Local laws in many countries have similar features, with the aim of avoiding policy gridlocks in a potentially large number of municipalities. This has important econometric implications which have been overlooked by the literature. Absolute majorities are single-party governments measured with (non-classical) error due to its discrete nature, and the parameters of their population regression functions are different, although they can be linked.[26]

Denoting by β^{AM} and β^{SPG} the population regression function parameters of the regression of a fiscal outcome y_i on an indicator for absolute majorities and of the regression of a fiscal outcome on an indicator for the existence of a single party government, respectively, we have that:

$$\beta^{AM}_{OLS} = E[y_i|AM_i = 1] - E[y_i|AM_i = 0] = \beta^{SPG}_{OLS}(1 - E[SPG_i = 1|AM_i = 0])$$

[26]Similar problems in the literature on the returns to schooling or on health insurance have been addressed in Black *et al.* (2000) or Kane *et al.* (1999). Alternatively, one could frame it as a problem of censoring, with similar results (Rigobon and Stoker, 2009)

Since moreover, government fragmentation is not randomly assigned, and assuming constant treatment effects for simplicity:

$$\beta_{OLS}^{AM} = \beta_{OLS}^{SPG}(1 - E[SPG_i = 1|AM_i = 0]) = (TE^{SPG} + SB)(1 - E[SPG_i = 1|AM_i = 0])$$

Where TE stands for treatment effect, and SB stands for selection bias. Intuitively, omitted variables additively bias the estimate of the treatment effect, and non-classical measurement error attenuates it, since $0 < (1 - E[SPG_i = 1|AM_i = 0]) < 1$. The attenuation factor here will be important if the fraction of single party governments within the non-absolute majorities is large. Moreover, with panel data, the effect of the measurement error is likely to be even larger.[27] On the other hand, IV estimates are also biased under non-classical measurement error (Rigobon and Stoker, 2009):

$$\beta_{IV}^{AM} = \frac{TE^{SPG}}{E[AM|SPG = 1]} > TE^{SPG}, \quad (\text{since } \ 0 < E[AM|SPG = 1] < 1)$$

This means that instrumental variables estimates of the effect of single party governments when we only observe absolute majorities will suffer from the opposite problem, namely an expansion bias. The expansion bias here will be important whenever the fraction of single party governments with no absolute majorities is large. These results are relevant if we want to interpret the IV estimates as the estimates of the effect of a single party government: these will be expanded, correct in sign, but too large in magnitude: they will be upper. For instance, if we take as a reference the mismatch between SPG and AM in Spanish province capitals, for which information is available, our IV estimate would be biased, expanded by $\frac{4}{3}$, meaning that the effect on current spending would drop from between €60 and €80 per capita to between €45 and €60 per capita.

[27] Note that this also affects the estimates based on close absolute majorities, which eliminate the selection bias but not the attenuation: $\beta_{RDD}^{AM} = (TE^{SPG})(1-E[SPG_i = 1|AM_i = 0])$. Under heterogeneous treatment effects, one should further consider the fact that RDD is a local estimator and estimates the ATT at the threshold

2.2 Conclusions

This chapter investigates the effects of government fragmentation on government spending using panel data on political and fiscal outcomes of municipalities in the Spanish region of the Basque Country. So as to identify a causal effect, I used exogenous variation arising from the ban of Batasuna, a political party which was banned due to its links to the Basque terrorist organization ETA. After its ban, Batasuna called for a null vote, which was followed by most of its former voters. As a result, the effects of the ban become mostly mechanical and the remaining parties gain vote shares and seats rather proportionally to their pre-ban vote shares and seats. Thus, the ban triggers a reshuffling of seats in city councils, which in some municipalities gives enough seats to the first party to secure an absolute majority which would not have had in absence of the ban.

I compare spending changes between municipalities where the reshuffling of seats triggers a change in government fragmentation and municipalities where the reshuffling of seats does not change the majority status of the government, within municipalities where Batasuna used to be equally important. The results show that absolute majorities decrease current spending, an effect which is large and driven by spending on public goods and services. The estimated effect on capital spending is of the opposite sign and similar magnitude and driven by an increase in investment, but it is imprecisely estimated and not statistically significant. Adding up both effects, the effect on total spending becomes close to zero, but it is imprecisely estimated, due to the imprecision of the effect on capital spending. I use pre-ban data and conduct a placebo test to show that the results are not driven by pre-ban differential trends, and I show that the results are robust to the inclusion of controls which account for the changes in the ideological composition of city councils induced by the ban, and that they hold across a number of sub-samples. I also show that the results are not driven by any changes on spending on civil protection and security. In all cases, the results suggest that coalitions substitute capital for current spending, leaving total spending rather unaffected.

The contribution of this paper is to compare absolute majorities to coalitions that were more fragmented or balanced that those in previous papers using close absolute majorities

as an exogenous source of variation. This is relevant because one of the main mechanisms which could lead coalitions to spend differently, which is the common pool mechanism, requires that more than one political party has some degree of discretion over spending. The results are consistent with this, since I find that absolute majorities reduce current spending significantly, while most RDD evidence suggests that absolute majorities increase or do not spend more than coalitions. These results are consistent with the common pool hypothesis, which suggests spending discretion by coalition parties would lead to overspending, but also with veto player models, which suggest that legislative gridlock would lead coalitions to spend less, as this is especially relevant for lumpy projects. However, the margin of adjustment is between different types of spending rather than total spending, although the large standard errors for the effect on total spending do not allow me to rule out a range of important effects.

These results suggest that electoral rules which deliver more frequent single-party governments will lead to compositional changes in government spending. These results are specially relevant for local governments where the alternative to a proportional rule is likely to be a plurality rule with a single district. For larger jurisdictions, where the set of alternatives to a single-district proportional rule is larger and also involves districting, other trade-offs arise and should be considered together with this evidence.

Appendix B

Table 2.5: Descriptive statistics, political outcomes

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Turnout	0.585	0.579	0.580	0.590	0.582	0.586
	(0.0620)	(0.0730)	(0.0832)	(0.0857)	(0.0764)	(0.0815)
Null votes (% of turnout)	0.0103	0.137	0.0110	0.105	0.0107	0.116
	(0.00474)	(0.116)	(0.00963)	(0.113)	(0.00824)	(0.115)
Absolute Majority	0.111	0.600	0.462	0.475	0.338	0.519
	(0.316)	(0.492)	(0.499)	(0.500)	(0.474)	(0.500)
ENP	3.195	2.692	3.199	2.610	3.198	2.639
	(0.587)	(0.983)	(1.092)	(0.887)	(0.944)	(0.922)
Batasuna Seat Share	0.259	0.0694	0.191	0.0541	0.215	0.0595
	(0.104)	(0.140)	(0.156)	(0.142)	(0.143)	(0.142)
Left Seat Share	0.378	0.276	0.379	0.305	0.379	0.295
	(0.140)	(0.213)	(0.193)	(0.222)	(0.176)	(0.219)
Right Seat Share	0.454	0.562	0.496	0.572	0.482	0.568
	(0.152)	(0.230)	(0.216)	(0.251)	(0.197)	(0.244)
Basque Seat Share	0.777	0.678	0.628	0.553	0.681	0.597
	(0.173)	(0.206)	(0.253)	(0.266)	(0.239)	(0.254)
Federal Seat Share	0.176	0.203	0.281	0.314	0.244	0.275
	(0.152)	(0.166)	(0.215)	(0.239)	(0.201)	(0.223)
Population	10720.1	10816.8	58286.0	56961.7	41466.8	40812.4
	(9709.9)	(9429.7)	(98708.6)	(96953.6)	(82718.3)	(81364.6)
#Clusters	73		155		228	

Treated: municipalities where Batasuna the ban mechanically leads towards an absolute majority. Pre values are averages of 1995 and 1999 election results. Post values correspond to averages of 2003 and 2007 election results. Weighted by square root of population size.

Table 2.6: Descriptive statistics, government spending

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Total Spending	1132.5	1514.3	1252.6	1682.6	1210.1	1623.6
	(371.9)	(458.2)	(481.3)	(663.2)	(449.2)	(604.6)
Current Spending	715.3	951.3	795.8	1061.8	767.3	1023.1
	(142.5)	(166.8)	(198.1)	(263.9)	(184.4)	(240.3)
Capital Spending	417.2	563.0	456.8	620.8	442.8	600.6
	(296.4)	(393.9)	(371.3)	(550.1)	(347.0)	(501.6)
Budget Balance	-0.0552	-0.00727	-0.0525	-0.000653	-0.0535	-0.00297
	(0.140)	(0.148)	(0.131)	(0.155)	(0.134)	(0.153)
Goods and services	297.1	436.4	335.6	478.3	322.0	463.6
	(100.1)	(127.1)	(135.9)	(173.9)	(125.8)	(160.3)
Investment	323.1	468.8	355.4	533.8	344.0	511.0
	(279.0)	(365.4)	(361.3)	(510.4)	(334.8)	(465.7)
Security and civil protection	39.83	56.36	52.96	72.00	48.32	66.52
	(31.56)	(43.51)	(49.58)	(69.77)	(44.49)	(62.29)
#Clusters	73		155		228	

Treated: municipalities where Batasuna the ban mechanically leads towards an absolute majority. Pre values are averages of 1995 and 1999 election results. Post values correspond to averages of 2003 and 2007 election results. Weighted by square root of population size.

Table 2.7: Instrumental Variables - Controlling for ideological heterogeneity

	Dependent Variable			
	Total Exp. (1)	Current Exp. (2)	Capital Exp. (3)	Budget Balance (4)
Absolute Majority	-12.60	-80.94**	68.34	-0.035
	(73.70)	(34.41)	(62.85)	(0.032)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓
Kleibergen-Paap F	56.617	56.617	56.617	56.617
N	3320	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Ideological controls include the mechanical change in the seat share of leftist parties, basque parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). All estimates weighted by municipality population.

Table 2.8: Instrumental Variables - Channels

	Dependent Variable		
	Public Goods and Services (1)	Investment (2)	Civil Protection and Public Safety (3)
Absolute Majority	-62.24**	32.24	-5.876
	(30.00)	(60.84)	(6.146)
f(MC in Batasuna's Seat Share)	✓	✓	✓
Ideology Controls	✓	✓	✓
Demographic Controls	✓	✓	✓
Municipality F.E.	✓	✓	✓
Province-Year dummies	✓	✓	✓
Kleibergen-Paap F	56.617	56.617	56.617
N	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms. Spending in public goods and services is a subset of current spending. Investment is a subset of capital spending. Security spending corresponds to an alternative classification of expenditures. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Ideological controls include the mechanical change in the seat share of leftist parties, basque parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). All estimates weighted by municipality population.

Table 2.9: Instrumental Variables - Excluding right-wing majorities

	Dependent Variable			
	Total Exp. (1)	Current Exp. (2)	Capital Exp. (3)	Budget Balance (4)
Absolute Majority	-7.194	-85.90**	78.70	-0.068
	(120.0)	(38.60)	(104.8)	(0.045)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓
Kleibergen-Paap F	24.316	24.316	24.316	24.316
N	2420	2420	2420	2420

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Ideological controls include the mechanical change in the seat share of leftist parties, basque parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). All estimates weighted by municipality population.

Table 2.10: Instrumental Variables - Excluding pre-ban absolute majorities as controls

	Dependent Variable			
	Total Exp. (1)	Current Exp. (2)	Capital Exp. (3)	Budget Balance (4)
Absolute Majority	-11.64	-88.97	77.33	-0.049
	(73.70)	(55.81)	(94.04)	(0.047)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓
Kleibergen-Paap F	27.285	27.285	27.285	27.285
N	3320	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Ideological controls include the mechanical change in the seat share of leftist parties, basque parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). A dummy equal to one after the ban if the municipality had absolute majorities in the two elections before the ban is included as a control. All estimates weighted by municipality population.

Table 2.11: Instrumental Variables - subsamples

	Dependent Variable							
	Sample: non-election years				Sample: election years			
	Total Exp. (1)	Current Exp. (2)	Capital Exp. (3)	Budget Balance (4)	Total Exp. (5)	Current Exp. (6)	Capital Exp. (7)	Budget Balance (8)
Absolute Majority	-17.01	-72.57**	55.56	-0.047	-8.819	-108.8***	100.0	-0.004
	(85.44)	(34.32)	(72.91)	(0.034)	(91.15)	(41.04)	(86.78)	(0.07)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓	✓	✓	✓	✓
Kleibergen-Paap F	56.371	56.371	56.371	56.371	55.522	55.522	55.522	55.522
N	2435	2435	2435	2435	882	882	882	882

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Every regression includes controls for the mechanical change in the seat share of Batasuna and its square. Demographic controls include a third order polynomial in the log of the municipalities' population, the log of the share of young inhabitants and the log of the share of old inhabitants. Ideological controls include the mechanical change in the seat share of leftist parties, right wing parties, basque parties and federal parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). All estimates weighted by municipality population.

	Dependent Variable							
	Sample: excluding 2003				Sample: pre-crisis [1997-2008]			
	Total Exp. (1)	Current Exp. (2)	Capital Exp. (3)	Budget Balance (4)	Total Exp. (5)	Current Exp. (6)	Capital Exp. (7)	Budget Balance (8)
Absolute Majority	-27.24	-82.00**	54.76	-0.029	-0.197	-60.10**	59.90	-0.022
	(79.42)	(35.75)	(66.46)	(0.03)	(77.92)	(27.09)	(71.04)	(0.03)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓	✓	✓	✓	✓
Kleibergen-Paap F	57.309	57.309	57.309	57.309	48.946	48.946	48.946	48.946
N	3104	3104	3104	3104	2644	2644	2644	2644

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. Every regression includes controls for the mechanical change in the seat share of Batasuna and its square. Demographic controls include a third order polynomial in the log of the municipalities' population, the log of the share of young inhabitants and the log of the share of old inhabitants. Ideological controls include the mechanical change in the seat share of leftist parties, right wing parties, basque parties and federal parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban). All estimates weighted by municipality population.

Table 2.12: Change in fragmentation and strategic behavior

	Dependent Variable
	$\frac{NullVotes}{Turnout}$
	(1)
Mech. Change in Absolute Majority	-0.004
	(0.016)
f(Mech.Change, Batasuna Seat Share)	✓
Demographic Controls	✓
Municipality F.E.	✓
Province-Year dummies	✓
R^2	0.2004
N	3320

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Estimates weighted by municipality population.

Table 2.13: Instrumental Variables - Unweighted results

	Dependent Variable			
	Total Exp.	Current Exp.	Capital Exp.	Budget Balance
	(1)	(2)	(3)	(4)
Absolute Majority	13.16	-76.48**	89.64	-0.008
	(105.6)	(36.12)	(93.53)	(0.03)
f(MC in Batasuna's Seat Share)	✓	✓	✓	✓
Ideology Controls	✓	✓	✓	✓
Demographic Controls	✓	✓	✓	✓
Municipality F.E.	✓	✓	✓	✓
Province-Year dummies	✓	✓	✓	✓
Kleibergen-Paap F	83.342	83.342	83.342	83.342
N	3320	3320	3320	3320

All dependent variables are in €2011 and in per-capita terms, except for the budget balance which is a % of total revenues. Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. The mechanical change in the seat share of Batasuna and its square and its interaction with the pre-ban seat share of the first party and a post-ban dummy for whether Batasuna used to exist before the ban are included as controls. Demographic controls include a third order polynomial in the log of the municipalities' population and dummies for population thresholds at which funding from national or provincial authorities changes. Ideological controls include the mechanical change in the seat share of leftist parties, basque parties, and the mechanical change in the party of the mayor (i.e., a variable indicating whether Batasuna used to be in office before the ban)

Data sources and definitions

- Interior Ministry, Spain - 1995, 1999, 2003 and 2007 local elections http://www.infoelectoral.interior.es/min/areaDescarga.html?method=inicio
- Basque municipalities with a proportional electoral system (i.e. with 250 or more inhabitants in the election year)
- Statistics Service, Basque Government - 1997-2011 Spending variables http://www.eustat.eus/estadisticas/tema_197/opt_0/temas.html
- All mechanical changes averaged over its 1995 and 1999 values
- Batasuna: Herri Batasuna - HB (1995), Euskal Herritarrok - EH (1999)

Parties ideological classification

- Basque: Aralar, Batasuna, EA, Herri-Hats, Iniciativa Ciudadana Vasca, PNV, PNV-EA, Zutik, Parties with abertzale or nacionalista in their names.
- Federal: CDL, CDS, Falange Española, PP, PSOE, Unidad Alavesa, Union Centrista y Liberal
- Left: Aralar, Batasuna, Carlistas, Herri-Hats, Partido Humanista, IU, IU-Aralar, IU-Verdes, IU-Verdes-Aralar, PSOE, Verdes, Zutik, Parties with izquierda, obrero, socialista or progresista their names.
- Right: Falange Española, PNV, PNV-EA, PP, Unidad Alavesa.

Chapter 3

Is it effective to ban a political party?

The existence of political parties or organizations which question the legitimacy of the existing political regime -anti-system parties, as defined in Sartori (1976)-, typically by supporting or justifying violence, provokes no uniform response on the part of democratic states (Cram, 2008). While a number of countries resort to political bans, such as Belgium, Spain, Lithuania, Bulgaria or Turkey, other countries tolerate such parties, which in some cases are able to obtain significant electoral support, such as Golden Dawn, the Greek neo-nazi party which became the third most voted party in the 2015 national election, after European officials had unsuccessfully called for its ban.[1][2] From a legal or political philosophy point of view, the question of political bans trades off principles associated with procedural views of democracy (the right of individuals to freedom of association and speech) against principles associated with substantive views of democracy (the commitment to core liberal democratic norms such as respect for law and justice). However, there is little empirical evidence regarding whether, besides preventing its institutional representation, political bans reduce the support for these parties or have any side effects.

This chapter studies the effects of the ban of Batasuna, a political party in the Spanish region of the Basque Country which was banned due to its links to terrorism, focusing on the effects of the ban on electoral support for this party and on street terrorism by its supporters.

[1]Examples of political bans include the islamist Welfare Party (Turkey), the far-right and secessionist Vlaams Blok (Belgium), the Communist Party of Lithuania, the secessionist United Macedonian Organization Ilinden-Pirin (Bulgaria), and the far-left and independentist Batasuna in Spain

[2]Financial times, April 16, 2013

The question of interest is whether preventing the political representation of political parties weakens the electoral base of the targeted political movement, affects voters' preferences or has side effects such as violence and further conflict. Beliefs concerning the answers to these questions, which are not straightforward, often shape the public debate about the convenience of political bans, complementing legal and philosophical arguments. While political bans reduce the resources and the institutional visibility of the targeted movement, at the same time they might be used to gain notoriety and legitimacy, and expressive utility from supporting the banned party might increase, possibly attracting voters of other parties which oppose the ban. Political bans might be the type of policy terrorist groups aim for, since they inflict damage to a broader population than the terrorist group, potentially leading them to conclude that the government is not concerned with their welfare and increasing the appeal of supporting violent means (Bueno de Mesquita and Dickson, 2007).

Empirically evaluating the effects of political bans is challenging because it is difficult to find suitable control groups and comparable measures of support. For this reason, papers analysing political bans, such as Bale (2007), tend to use a narrative and qualitative approach. This chapter seeks to overcome these difficulties by exploiting the finite length of the ban of Batasuna, which allows to observe changes in electoral support before and after the ban, and its heterogeneous enforcement in local elections, which allows for a comparison of changes across municipalities: in some of them, the ban did last for two electoral terms; in some others, it only lasted for one electoral term.

I estimate the effect of a longer ban by differences in differences, finding a negative effect on electoral support in local elections, driven both by a lower number of candidatures and less votes obtained. These effects persist for at least two elections after the end of the ban and have spill-overs to support for Batasuna in regional elections. These spillovers, which could be due to an incumbency externality effect or to an aversion to divided government, persist even after accounting for the effects of the ban on local support, suggesting that the ban might have affected voters' preferences as well. The extension of the ban, which in a first electoral term applied to all municipalities, but in a second term was enforced only in a subset of them, triggered a spike in street terrorism for one month, but overall it had no significant effects since it slightly reduced street terrorism during the year after.

The heterogenous length of the ban across municipalities, however, is not randomly assigned: it is related to whether, in a particular election, the party was able to find candidates without links to Batasuna in the past. If this ability is related to time-invariant unobservables, the results are not biased, but if this ability is related to time-varying unobservables such as differential trends in support at the moment of the ban, however, one then might be concerned that Batasuna would have obtained better results after the ban in those municipalities where the ban was shorter in any case. To deal with this, I exploit the rule set by the public prosecutor to denounce electoral lists and which influenced the decision of the Courts, which ultimately gave rise to the heterogeneous length of the ban. The public prosecutor denounced the lists of Batasuna in those municipalities where three or more candidates were former candidates of Batasuna in the past, with at least one of them having been elected as a city councilor. I use this rule, which is based on observables and was set after the electoral lists were presented, to instrument for the differential length of the ban, relying on a weaker identification assumption. Instrumental variables estimates are less precise but qualitatively similar, suggesting that the results capture a causal effect.

The effect of the differential length of the ban on electoral support is not necessarily the same as the overall effect of the ban, but nevertheless it is a policy relevant effect: while an initial ban is straightforward to implement, it is likely that the banned movement will try to escape the law and re-emerge with another name or in another form. The effect of the differential length of the ban of Batasuna, thus, should be understood as the effect of banning a spin-off of the banned party after its initial ban.

Although some important effects of the ban might go beyond the treated municipalities and general equilibrium effects are likely to matter, to the best of my knowledge this is the first paper which identifies and estimates some of its most relevant partial equilibrium effects, complementing existing qualitative legal and political studies such as Bale (2007) and Downs (2012). The findings in this paper intersect with a number of literatures, namely the literature on incumbency advantage and incumbency spillovers or coattail effects (Ade and Freier (2013), Meredith (2013), Liang (2011), Broockman (2009), Dinas *et al.* (2015)), the literature on the effect of government intervention on preferences (Fuchs-Schündeln and Schündeln (2015), Clots-Figueras and Masella (2013), Alesina and Fuchs-Schündeln (2007))

and the literature on counter-terrorist policies and political regimes and terrorism (Abadie (2006), Bueno de Mesquita and Dickson (2007), Bueno de Mesquita (2005), Sánchez-Cuenca and de la Calle (2009), Dragu (2016)).[3] It also adds to a number of papers which have addressed related questions regarding determinants of terrorism in the Basque Country. Barros *et al.* (2009) use an auto-regressive model and find that on average, violence decreases between the first ban of Batasuna in 2003 and 2005, although it is difficult to know whether that decrease would have happened regardless of the ban or not.[4] de la Calle and Sánchez-Cuenca (2013) study the relationship between the typology of ETA's targets and Batasuna's strength at the local level and viceversa; de la Calle (2007) provides an in-depth analysis of the determinants of street terrorism in the Basque Country; and Gardeazabal (2011) studies the effects of linguistic and political polarization on conflict.

3.1 Identification and estimation

Figure 3.1 displays turnout, the votes received by Batasuna and the share of null votes in every local election in the Spanish region of the Basque Country from 1987 until 2015.[5] While turnout remained rather stable around 60% throughout the period, there are sharp changes in the fraction of null votes and in the vote share for Batasuna in the years of the ban, 2003 and 2007. In 2003, when Batasuna was banned everywhere (and therefore it obtained 0% of the vote), null vote as a fraction of turnout jumps from very close to zero to to more than 18%, which is around 75% of the average pre-ban vote share for Batasuna.[6] In 2007, Batasuna managed to be legal in 98 out of the 251 Basque municipalities, and therefore in those municipalities they obtain votes, calling for a null vote only in the remaining ones.[7] As a result, in 2007 the aggregate shift between Batasuna votes and null votes in figure 3.1 is attenuated. In 2011 and 2015, Batasuna could be present in the local elections through Bildu, which obtained more than 30% of the votes, and null votes went back to close to zero.

[3]See Bueno de Mesquita (2008) for a review of the most recent literature

[4]See also Gil-Alaña and Barros (2010) for a related note

[5]Local elections take place every four years

[6]This is slightly different than in chapter 1 because here I also include municipalities below 250 inhabitiants

[7]In 1995 and 1999, Batasuna was present in local elections in around 200 municipalities

Figure 3.2 shows the same variables, split by their legal status in 2007. The figure shows how wherever Batasuna was legal in 2007, null votes go back to zero and the vote share for Batasuna increases again; while wherever Batasuna was still banned in 2007, null votes remain high. The figure shows how the pre-ban gap in support between either group becomes larger after the ban, suggesting that a longer ban led to relatively less support after the end of the ban. Looking at figure 3.1 alone, one would think that in spite of these differences, overall the ban backfired and that whenever Batasuna went back to the electoral arena under regular conditions, it obtained more support than ever. However, the larger vote shares obtained by Bildu can be explained by the presence of other political actors in this coalition, such as the centrist and independentist Eusko Alkartasuna (EA). Although the electoral success of EA had been decreasing over time, before the ban of Batasuna it used to obtain a vote share of between 15% (in 1987) and 6% (in 1999). Figure 3.6 in the Appendix C displays the same figure, adding up the vote share of Batasuna and EA, and table 3.8 in the Appendix C shows further descriptives before and after 2007, by treatment status.

It is difficult to identify the effect of the overall ban on support for Batasuna because there is no suitable control group which we would expect to exhibit the same trends in electoral support but not affected by the 2003 ban (for instance, municipalities where Batasuna did not exist before the ban are not a good counterfactual to estimate the effect of the ban on support for Batasuna). However, municipalities where the ban did last for one election are similar to those where the ban lasted for two elections. Since we observe support for Batasuna before and after the ban in all municipalities, and the length of the ban changes across municipalities, conditional on a parallel trends assumption we will recover a causal effect of the differential length of the ban on the electoral success of Batasuna. Consider the following causal model for support for Batasuna in municipality m in year t, where we allow the causal effect of the 2007 ban to differ across municipalities.

$$Support_{mt} = \alpha_m + \delta_t + \beta_m \Big(I(\text{Post 2007})_t \times I(\text{Ban in 2007})_m \Big) + \epsilon_{mt}$$

Figure 3.1: Turnout, Batasuna Vote Share and Null Vote

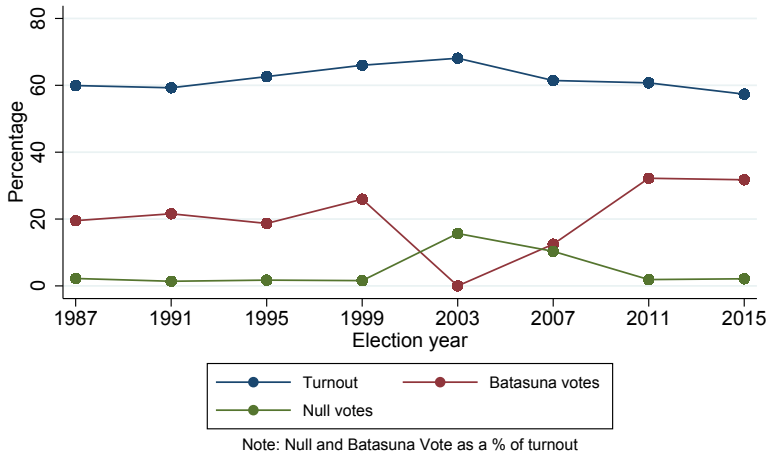
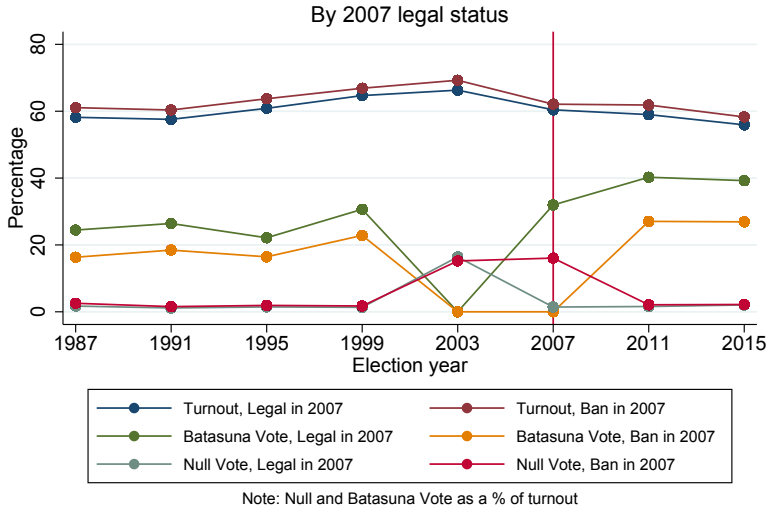


Figure 3.2: Turnout, Batasuna Vote Share and Null Vote, by 2007 legal status



We will estimate the effect of the 2007 ban using differences-in-differences:

$$\hat{\beta}^{DiD} = [\overline{Supp}^{\text{Ban in 2007}}_{\text{Post 2007}} - \overline{Supp}^{\text{Ban in 2007}}_{\text{Pre 2007}}] - [\overline{Supp}^{\text{Legal in 2007}}_{\text{Post 2007}} - \overline{Supp}^{\text{Legal in 2007}}_{\text{Pre 2007}}]$$

Its expectation, $E[\hat{\beta}^{DiD}]$, is given by:

$$E(\beta_m|\text{Ban in 2007}) + [E(\epsilon_{m_{\text{Post 2007}}} - \epsilon_{m_{\text{Pre 2007}}}|\text{Ban in 2007})] - [E(\epsilon_{m_{\text{Post 2007}}} - \epsilon_{m_{\text{Pre 2007}}}|\text{Legal in 2007})]$$

Which means that we will recover a causal effect whenever the unobservable characteristics which determine support for Batasuna in either group follow parallel trends (i.e. , $[E(\epsilon_{m_{\text{Post 2007}}} - \epsilon_{m_{\text{Pre 2007}}}|\text{Ban in 2007})] = [E(\epsilon_{m_{\text{Post 2007}}} - \epsilon_{m_{\text{Pre 2007}}}|\text{Legal in 2007})]$. In that case, $E[\hat{\beta}^{DiD}] = E(\beta_m|\text{Ban in 2007})$, and in an heterogeneous treatment effects framework, we would estimate the Average Treatment Effect on the Treated (ATT). This is an interesting Treatment Effect, as it is the Treatment Effect for a subset of municipalities with a considerable level of support for Batasuna.

To compare municipalities where the party had a similar local infrastructure before the ban, I group municipalities according to whether Batasuna presented lists in each of the local elections before to the ban and include year fixed effects for each of these groups.[8] The largest group is the group such that Batasuna presented lists in every election from 1987 to 1999. In all regressions, 2007 is excluded from the sample, since support is not comparable because of the different legal status (which is precisely the treatment of interest). In 2003, I measure support for Batasuna as the share of null votes, following the findings in chapter 1, and to be consistent across years, the dependent variable -support for Batasuna- is defined as $\frac{Batasuna\ Votes + Null\ Votes}{Total\ Votes}$.

Column 1 in table 3.1 shows the difference in differences estimate of the 2007 ban on support for Batasuna. The point estimate shows that wherever Batasuna could be legal in 2007, it obtained 6 percentage points (pp) more of votes after the ban. This is a substantial effect, of between 15 and 20% of the average post-ban support, or almost 0.3 standard

[8]Groups are defined for all possible combinations of being present between 1987 and 1999. For instance, municipalities with no lists until 1991 but with lists in 1995 and 1999 are grouped together, municipalities with lists only in 1991 are grouped together, municipalities present in all elections are grouped together, and so on.

Table 3.1: Effects of the 2007 ban on support for Batasuna in local elections

	Dependent Variable		
	Batasuna Support	I(Batasuna list)	Batasuna Support
	(1)	(2)	(3)
$I(\text{Post 2007}) \times I(\text{Ban in 2007})$	-0.0636***	-0.0703***	-0.0361***
	(0.0183)	(0.0263)	(0.0141)
$I(\text{Post 2007}) \times I(\text{Batasuna list})$			✓
Municipality F.E.	✓	✓	✓
Pre-ban presence × election FE	✓	✓	✓
N	1735	1485	1735
Municipalities	251	251	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample. All regressions control for a cubic in log population. All regressions control for a cubic in municipality size

deviations in post-ban support. Column 2 in table 3.1 shows the difference in differences estimate of the effect of a longer ban on an indicator for Batasuna presenting a list.[9] The point estimate shows that escaping the ban in 2007 increases the subsequent probability of presenting a list by 7pp (8.5% of the post-ban mean). This is a large effect, suggesting that part of the effect of a longer ban is to harm the party's local infrastructure. Column 3 displays the difference in differences estimate of the 2007 ban on support for Batasuna, controlling for whether Batasuna presented a list after the ban. This is an endogenous or bad control, since it is an outcome of the ban. The ban is no longer exogenous once we control for presenting lists afterwards, since those municipalities where Batasuna was banned in 2007 but presented lists afterwards are likely to have unobservables positively linked to electoral support, which means that the point estimate in column 3 will be biased towards zero or towards being positive. Nevertheless, the effect of a longer ban on support is still large, negative and significant, of around 3.5pp. Overall, the results in table 3.1 show how a longer ban affected support for Batasuna both through the extensive and intensive margins of electoral support at the local level. However, since the electoral result is an equilibrium outcome, it is not

[9]This specification excludes 2003 from the sample, since in 2003 they could not present a list anywhere

possible to know to what extent the decrease in votes is due to a lower demand because of a change in voters preferences or to a lower demand due to unobservable weaknesses in the local party infrastructure which could go beyond presenting a candidature. Table 3.10 in the Appendix C reports estimates of the same regressions but including group-specific trends, displaying point estimates which are even larger in magnitude, suggesting that the estimates in table 3.1 are not driven by differential pre-ban trends, which is consistent with figure 3.2, which suggests that the pre-ban levels of support of either group were slightly converging.

3.1.1 Identification threats

The estimates above are causal under the assumption that either group would have followed parallel trends in absence of the 2007 ban. As figure 3.2 shows, municipalities which manage to escape the ban in 2007 had stronger levels of support before the ban. Hence, the previous estimates could be biased if the effect of the 2003 ban or of any other political development throughout the period which could have an effect on support for Batasuna were proportional to the underlying level of support. While pre-ban trends suggest that either group responds similarly to global shocks, I formally deal with this by using an Oaxaca-Blinder estimator (Kline (2011), Kline and Moretti (2013), Busso *et al.* (2013)). I first fit regression models to the municipalities where Batasuna was legal in 2007 of the form:

$$\Delta Support_m = \alpha + \gamma X_m + \Delta\epsilon_m$$

where $\Delta Support_m$ is the change in support between the average of the post-ban period (elections in 2011, 2015) and the average of the pre-ban period (1987-2003) for municipality m and X_m is a vector of pre-ban characteristics. I then use $\hat{\gamma}$ to predict the counterfactual mean for the treated municipalities (banned in 2007), and compare this predicted mean with the actual mean of the treated municipalities, the difference being the estimate of the treatment effect. Kline (2011) shows that this Oaxaca-Blinder estimator is equivalent to a propensity score reweighting estimator modeling the odds of treatment as a linear function of the covariates. This estimator imposes exact covariate balance between treated and control units, which I specify to be a vector of pre-ban measures of support. More specifically, X

is a vector of all the pre-ban vote shares (from 1987 to 2003), and of dummies indicating pre-ban presence (from 1987 to 1999).

Figure 3.3: Oaxaca-Blinder D-i-D, matching on pre-ban support and presence

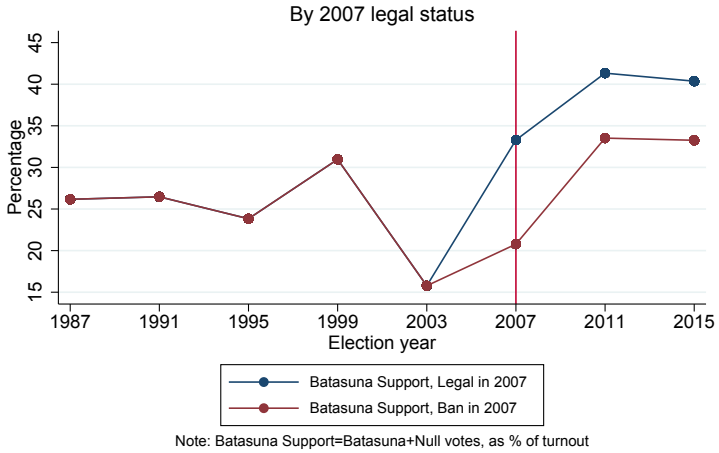


Table 3.2: Effects of the 2007 ban on support for Batasuna, O-B

	Dependent Variable		
	Batasuna Support	I(Batasuna list)	Batasuna Support
	(1)	(2)	(3)
I(Post 2007) × I(Ban in 2007)	-0.0746***	-0.059	-0.0538***
	(0.0218)	(0.0371)	(0.0195)
I(Post 2007) × I(Batasuna list)			✓
Pre-ban presence	✓	✓	✓
Pre-ban support	✓	✓	✓
Municipalities	237	237	237

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample. All regressions control for a cubic in municipality size

Figure 3.3 illustrates this estimate using the implicit weights assigned by the Oaxaca-Blinder estimator. The point estimate is displayed in column 1 of table 3.2. Although the point estimate for the presence of Batasuna is less precise, the results are qualitatively very similar to those in table 3.1, and suggest that the previous estimates are not driven by an heterogeneous response to the ban or to global shocks by level of support, nor by differences

in pre-ban trends.[10] The estimates in figure 3.7 and table 3.11 in the Appendix C also match on pre-ban support for EA, the post-ban coalition partner, with similar results.

The most relevant identification threat, however, is due to the legal status in 2007 being related to the ability of the local party to find candidates without links with Batasuna in the past. If this differential ability is related to the level of support for Batasuna, this is absorved by the municipality FE, and D-i-D will deliver the causal effect of interest. But if the differential ability is related to an unobservable change in support at the moment of the ban, then $E(\epsilon_{mt_{\text{Post 2007}}} - \epsilon_{mt_{\text{Pre 2007}}} | \text{Ban in 2007}) < E(\epsilon_{mt_{\text{Post 2007}}} - \epsilon_{mt_{\text{Pre 2007}}} | \text{Legal in 2007})$ and D-i-D would overestimate the negative effects of the ban on political support. To see whether this is the case, I use the rule set by the public prosecutor to denounce the Batasuna (EAE-ANV) lists to construct an instrument which relies on a weaker identification assumption.

In May 2007, the public prosecutor, based on police reports about the links between the EAE-ANV lists and the previously banned organizations, sent a denounce to the Supreme Court with the aim of preventing a number of EAE-ANV candidatures from being present in the May 2007 local election. In the preamble of the denounce, the public prosecutor states that he is denouncing lists in the following cases:

"To include, from a **quantitative** *point of view, at least three candidates with a relevant and direct relation with the banned party, such as candididates in recent elections or party officials. To reach this figure, in principle, the elections before 1995 are not taken into account, nor the candidates known to belong to EAE-ANV"*

"from a **qualitative** *point of view, that any of the candidates participating in previous elections in direct representation of the banned parties had been elected and have had an institutional role (mayor, councilor, regional MP, provincial MP), which by itself is doubly indicative that occupied a relevant place in the electoral lists, and therefore of its importance in the structure of the banned organization, and that is socially identified as a party official of the banned party, which makes the whole candidature carry a continuity which disappoints the execution of the veredict dictated by the Supreme Court"*

To sum up, the public prosecutor states that he will denounce lists with three or more

[10] In the O-B regressions the number of municipalities is slightly lower since it is not possible to use those municipalities with missing values for any of the pre-ban years, which perhaps contributes to the slight loss of precision

former Batasuna candidates, with one of them having been elected as a councilor or MP for Batasuna in the past. Although the public prosecutor does not strictly follow the rule above and not all lists denounced by the public prosecutor end up being banned, this criteria based on observables and set ex-post (i.e. after the lists were presented) predicts well the subsequent decision taken by the Supreme Court.[11] The definition of links used by the public prosecutor refers to candidates in the 1995 and 1999 local elections, considering just as minor links the candidates present in lists of the banned candidatures in 2003, although the latter had also received media attention before the prosecutors' denounce.[12]

I collect data on the quantity and the quality of the links of each municipal list with previously banned organizations from two sources. The first one is the denounce of the public prosecutor itself, which contains information about the denounced lists.[13] The second one is a media report, which also contains information for the remaining candidatures.[14] I combine data from both sources, taking the largest number of links as the true value for those municipalities where both files contain information. For the municipalities for which there is no information available and where Batasuna was legal in 2007, I assume there were no links at all. For the municipalities for which there is no information available and were not present in 2007, I assume that the number of links is equal to the size of the electoral list.

I use the rule by the public prosecutor to construct an instrument for Batasuna being legal in 2007. To simplify notation, I write the model in first differences, where Δ is the difference between the average post 2007 ban level of support and the average pre 2007 ban level of support. The reduced form reads as follows:

$$\begin{aligned}\Delta Support_m = \gamma I(FormerCouncilors_m \geq 1) + \pi I(FormerCandidates_m \geq 3) \\ + \theta\big(I(FormerCouncilors_m \geq 1) \times I(FormerCandidates_m \geq 3)\big) + \Delta\epsilon_m\end{aligned}$$

[11] In addition to the public prosecutor, the State Lawyer also wrote her own denounce to the Supreme Court, which largely overlaps with the public prosecutor denounce, although without stating any precise criteria

[12] El País, April 2007

[13] Link to the denounce, accessed April 13, 2016

[14] Link to media information, accessed April 13, 2016

Since the public prosecutor states that is denouncing lists with (a) at least one former councilor and (b) three or more former candidates, the excluded instrument is the interaction of (a) and (b). The IV is a triple difference estimator (DiDiD) given by:

$$\hat{\theta}^{DiDiD} = [\overline{\Delta Supp}^{FCo\geq 1}_{FCa\geq 3} - \overline{\Delta Supp}^{FCo\geq 1}_{FCa<3}] - [\overline{\Delta Supp}^{FCo=0}_{FCa\geq 3} - \overline{\Delta Supp}^{FCo=0}_{FCa<3}]$$

Where $Supp$ stands for support for Batasuna, FCo stands for Former Councilors and FCa stands for Former Candidates. Its expectation is given by:

$$E(\theta_m|FCo\geq 1, FCa\geq 3)+[E(\Delta\epsilon_{m_{FCo\geq 3}}-\Delta\epsilon_{m_{FCa<3}}|FCo\geq 1)]-[E(\Delta\epsilon_{m_{FCa\geq 3}}-\Delta\epsilon_{m_{FCa<3}}|FCo=0)]$$

Therefore, the identification assumption is that:

$$\begin{aligned} E[\Delta\epsilon_i|FCa_m\geq 3, FCo_m\geq 1] - E[\Delta\epsilon_i|FCa_m<3, FCo_m\geq 1] \\ = E[\Delta\epsilon_i|FCa_m\geq, FCo_m=0] - E[\Delta\epsilon_i|FCa_m<3, FCo_m=0] \end{aligned}$$

Namely, that the pre-post difference in unobservables between those with more and less than three former candidates is the same for those with a former councilor and those without a former councilor. This is a parallel trend assumption for the change in the outcome (rather than for the outcome), over the number of former candidates (rather than over time), between those with a former councilor and those without. Table 3.13 in the Appendix C shows descriptives for each of these variables, by 2007 legal status.

In practice, instead of first-differences I use fixed effects and interact all the relevant variables with a post-2007 dummy. I also include fixed effects (interacted with a post 2007 dummy) for the number of former candidates, so that the comparison is within municipalities with the same number of former candidates. Table 3.3 reports the first stage, which shows that the interaction between more than three former candidates and a former councilor (i.e., the excluded instrument) predicts well the legal status in 2007. Table 3.4 reports instrumental variables results. Column 1 reports the baseline results, corresponding to the specification in table 3.3. Although the results are imprecise, the point estimate is qualitatively similar to the difference in differences estimate in table 3.1, slightly larger in magnitude (8pp against

Table 3.3: First Stage

	Dependent Variable
	$I(\text{Post 2007}) \times I(\text{Ban in 2007})$
$I(FCo \geq 1) \times I(FCa \geq 3) \times I(\text{Post 2007})$	0.4725***
	(0.134)
Municipality FE	✓
Year FE	✓
Former Candidates FE × I(Post 2007)	✓
$I(FCo \geq 1)$ × I(Post 2007)	✓
Kleibergen-Paap F	12.454
N	1981
Municipalities	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. All regressions control for a cubic in municipality size

Table 3.4: Instrumental Variables

	Dependent Variable			
	Batasuna Support (1)	Batasuna Support (2)	Batasuna Support (3)	Batasuna Support (4)
$I(\text{Post 2007}) \times I(\text{Ban in 2007})$	-0.0883	-0.1062*	-0.107*	-0.1596**
	(0.0909)	(0.0638)	(0.0643)	(.0789)
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Former Candidates FE×I(Post 2007)	✓	✓	✓	✓
$I(FCo \geq 1)$ × I(Post 2007)	✓	✓	✓	✓
$FCa \times I(FCo \geq 1)$ × I(Post 2007)		✓	✓	
Excluding missing links			✓	
Only $FCa = 2$ or $FCa = 3$				✓
Kleibergen-Paap F	12.454	26.069	25.745	19.018
N	1735	1735	1412	450
Municipalities	251	251	203	65

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. All regressions control for a cubic in municipality size

6pp). In column 2, I separately control for the number of former candidates for those with a former candidate and for those without. This is analogous to including group-specific trends in a standard difference in differences regression, and the results, which are similar, suggest that the identification assumption is plausible. In column 3, I exclude from the sample those municipalities for which I had no information on links and were not present in 2007 (and for which I had assumed the number of links was equal to the size of the electoral list in that municipality); in column 4, I consider only municipalities with either 2 or 3 former candidates. Overall, these estimates, although imprecise, suggest that differences in differences estimates are not biased upwards and are capturing a causal effect.

Figure 3.8 in the Appendix C displays the first stage and the reduced form. Circle sizes are proportional to the number of municipalities in each group, which is quite heterogeneous. In the figure, former candidates are normalized (Normalised Former Candidates=–(Former Candidates-2)). Except for one outlier (which is one municipality) at -4 normalised former candidates, which column 4 in table 3.4 shows that is not driving the results, most of the action comes from the difference in legal status for those with 3 or 4 former candidates (-1 or -2 normalised candidates in the graph). It is within this group that the differences in legal status due to having a former councilor arise, and it is within this group (in particular, for those with 3 former candidates, or -1 normalised former candidates in the figure) that we observe the differences in support driving the results.

3.1.2 Regional elections

I also examine whether the ban in local elections translates into less support for Batasuna in regional elections. This is interesting for three reasons. First, to test whether municipalities with different legal status in 2007 were performing differently just two years before, in the 2005 regional election. Second, to test whether a weakened presence at the local level has effects on elections in upper tiers. Last but not least, to test whether the effect of the ban goes beyond the effect on the local infrastructure and support.

I use data on municipal results of regional elections in the Basque Country from 1986 to 2012, which feature a very similar party system to the one in local elections.[15] At the

[15]Regional elections took place in 1986, 1990, 1994, 1998, 2001, 2005, 2009 and 2012

regional level, Batasuna escaped the ban in 2005 and could be present in that regional election, but it could not contest the 2009 one, when it called for a null vote, like it did in local elections under the ban. Hence, I measure support in 2009 with the share of null votes, and to be consistent across years I define support for Batasuna as $\frac{Batasuna\ votes+Nul\ votes}{Total\ votes}$. In every regional election, the legal status of Batasuna was the same across municipalities.

I compare differences in support before (1986-2005) and after the heterogeneous enforcement of the ban in local elections (2009-2012), according to the legal status of Batasuna in 2007. The main summary statistics for either group are displayed in table 3.9 in the Appendix C. Table 3.5 reports difference in differences results. Column 1 in table 3.5 shows that a longer at the local level leads to less support in regional elections. The effect is small compared to local elections which are the elections where Batasuna is usually more successfull, but still meaningful and significant. A longer ban reduced the support for Batasuna by 1.2pp, which is slightly less than 5% of their post-ban average support, and 7% of its standard deviation. This could be due to changes in preferences; to an incumbency externality or *reverse coattail* effect, possibly due to reduced visibility and resources at the local level; or to an aversion to divided goverment (i.e. a preference for having the same party in multiple layers of government). However, recent evidence using close elections suggests that there is no incumbency externality effect to higher governmental tiers in Germany nor in the US (Ade and Freier (2013), Broockman (2009)). Similarly, existing empirical evidence points towards a preference rather than towards an aversion to divided government, especially in the US (Alesina and Rosenthal (1996), Folke and Snyder (2012)). However, Solé-Ollé and Sorribas-Navarro (2008) and Curto-Grau *et al.* (2012) show that partisan alignment across regional and local governments results in increased transfers for municipalities in Spain, which could explain such an aversion.

In column 2, I add a lead of the treatment to see whether in 2005 these municipalities were already differing in support. The point estimate shows that this was not the case, which reinforces the D-i-D identification assumption, as we observe that 2 years before the differential ban, there were no differential trends in support across treated and control municipalities. In columns 3 and 4 I control for the post-ban presence and vote share in local elections. These are outcomes or bad controls, since municipalities where Batasuna was not

legal in 2007 but with the same post-ban presence and post-ban support in votes are likely to have unobservables positively related to support, biasing the coefficient of the ban towards zero or towards being positive. However, the result shows that there is an effect of the length of the ban on support for Batasuna which goes beyond the effects on the local infrastructure of the party and its support, and thus beyond any hypothetical incumbency externality or divided government effect. Hence, these results suggest that a longer ban might have had an effect on voters' preferences, although this would have to be further tested with survey data and is an avenue for future research.

Table 3.5: Effects of the 2007 ban on support in regional elections

	Dependent Variable			
	Batasuna Support (1)	Batasuna Support (2)	Batasuna Support (3)	Batasuna Support (4)
I(Post 2007) × I(Ban in 2007)	-0.0115**	-0.0125***	-0.0124***	-0.0097**
	(0.0058)	(0.0043)	(0.0044)	(0.004)
I(Post 2005) × I(Ban in 2007)		0.0011	-0.0011	0.001
		(0.0045)	(0.0045)	(0.0045)
I(Post 2011) × I(Present in 2011)			0.0007	-0.0672***
			(0.0152)	(0.0169)
I(Post 2011) × I(Vote Share 2011)				0.1874***
				(0.0291)
Municipality FE	✓	✓	✓	✓
Pre-ban presence × election FE	✓	✓	✓	✓
N	1981	1981	1981	1981
Municipalities	251	251	251	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2009, the vote share of Batasuna is computed as the share of null votes. All regressions control for a cubic in municipality size

Results in table 3.12 in the Appendix C show Oaxaca-Blinder estimates matching in pre-ban support in regional elections and pre-ban support and pre-ban presence in municipal elections; in pre-ban support for EA, the post-ban coalition partner; and in post-ban presence, showing very similar point estimates in every case. Table 3.6 shows instrumental variables estimates for support in regional elections. Column 1 reports the baseline results, corresponding to the first stage specification in table 3.3. Although the results are imprecise, the point estimate is qualitatively similar to the difference in differences in table 3.5. In

column 2, I separately control for the number of former candidates for those with a former candidate and for those without, which is analogous to including group-specific trends in a standard difference in differences regression. In column 3, I exclude from the sample those municipalities for which I had no information on links and were not present in 2007 (and for which I had assumed the number of links was equal to the size of the electoral list in that municipality); in column 4, I consider only municipalities with either 2 or 3 former candidates. The results are rather stable across specifications, with point estimates between 1pp and 2pp.

Table 3.6: Instrumental Variables, Support in regional elections

	Dependent Variable			
	Batasuna Support (1)	Batasuna Support (2)	Batasuna Support (3)	Batasuna Support (4)
$I(\text{Post 2007}) \times I(\text{Ban in 2007})$	-0.0216	-0.0182	-0.0114	-0.015
	(0.0307)	(0.0227)	(0.0223)	(0.0281)
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Former Candidates FE × I(Post 2007)	✓	✓	✓	✓
$I(FCo \geq 1)$×I(Post 2007)	✓	✓	✓	✓
$FCa \times I(FCo \geq 1) \times$ I(Post 2007)		✓	✓	
Excluding missing links			✓	
Only $FCa = 2$ or $FCa = 3$				✓
Kleibergen-Paap F	11.425	21.182	20.724	14.416
N	1981	1981	1609	514
Municipalities	251	251	203	65

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. All regressions control for a cubic in municipality size

3.1.3 Street terrorism

On top of ETA, which killed more than 800 people during its existence, ETA's supporters, mostly young people, have carried out thousands of street terrorism attacks in the last decades. These attacks, known as *Kale Borroka* (street fight) consist mostly of burning public buses, private vehicles and trash containers, and attacks to courts, banks, political parties and police's buildings or private property linked to them using cocktails Molotov, and typically arise at night or after demonstrations. de la Calle (2007) studies the determinants of *Kale Borroka* between 1996 and 2000, describing it as a persistent type of non-organizational

violence (i.e. without any permanent organization claiming responsibility for the attacks), which tends to arise in more polarized municipalities. Like ETA violence, all political parties except Batasuna condemned *Kale Borroka*, and being part of these violent squads was typically a first step towards becoming an ETA militant. Between 2006 and 2008, there were more than 350 attacks of this type in the Basque Country, which in earlier periods had been even more frequent. In 2000, Spanish-based insurance companies spent almost 9 million euros due to street terrorism attacks during the three previous years (de la Calle, 2007).

Political bans like the ban of Batasuna often arise as a response to political movements which support or justify violence or terrorism, in which case they can be seen as a counter-terrorist measure. Bueno de Mesquita (2005) argues that counterterrorism decreases the ability of terrorists to carry out attacks, by weakening their infrastructure, but also diminishes their economic opportunities, reducing their opportunity costs, on top of increasing their ideological motivations. Although the ban is a measure targeted to the political wing of ETA, the political and financial links between ETA, street terrorism and Batasuna make Bueno de Mesquita (2005) a suitable framework to think of the possible effects of the ban on street terrorism in the Basque Country. For instance, in 2002, the courts considered Batasuna the civil responsible for a number of street terrorism attacks, arguing that the finances of Batasuna and the *Kale Borroka* were jointly determined.[16]

I estimate the effect of the 2007 on street terrorism using data at the municipality level on street terrorism episodes between April 2006 and June 2008, which is slightly more than one year before and one year after the 2007 ban. Data are obtained from Buesa (2008) and table 3.14 in the Appendix C reports descriptives by treatment group and period.

Figure 3.4 displays the average number of street terrorism epidodes by month and by whether Batasuna escaped the ban in May 2007. Figure 3.5 displays the same data but averaged over quarters rather than months. The figures show that the trends in street terrorism before the ban was extended in May 2007 were similar across either group and that the extension of the ban triggers a peak in street terrorism in the very short run but that after that it leads to a slight decline, the overall effect being ambiguous.[17]

[16] El País, July 2002

[17] The denounces happened the 3rd of May of 2007 and the Supreme Court took its decision the 6th of May. The first episode in May 2007 took place on the 7th of May.

Figure 3.4: Effect of the 2007 ban on Street Terrorism, monthly

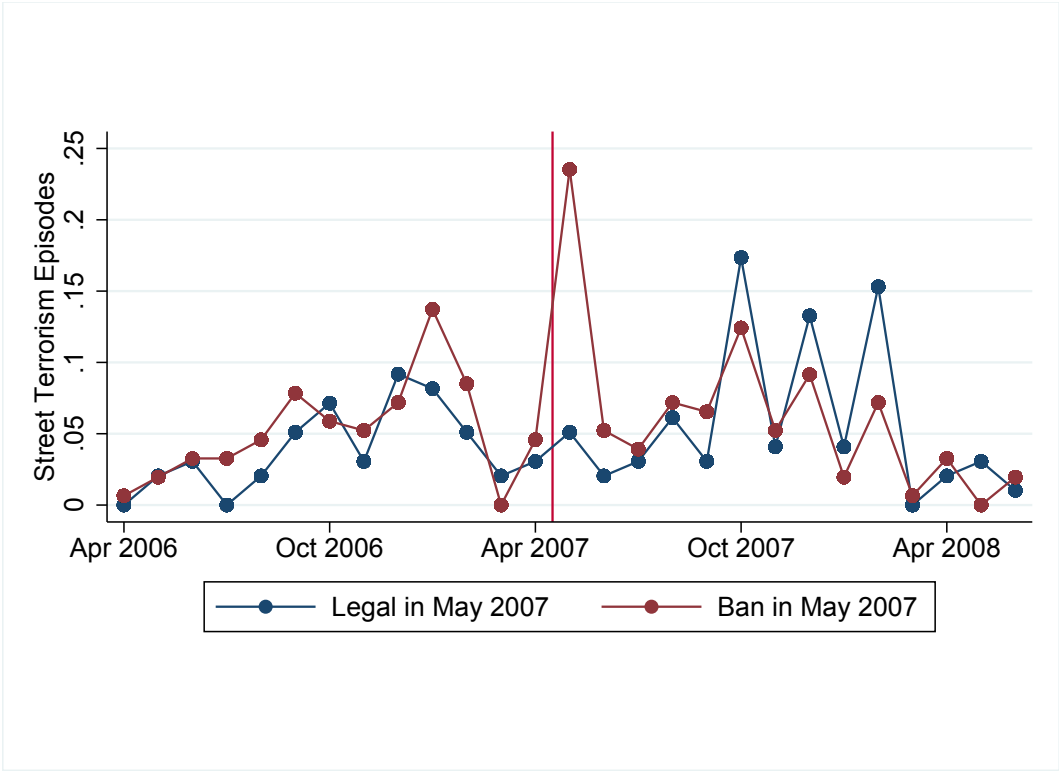


Figure 3.5: Effect of the 2007 ban on Street Terrorism, quarterly

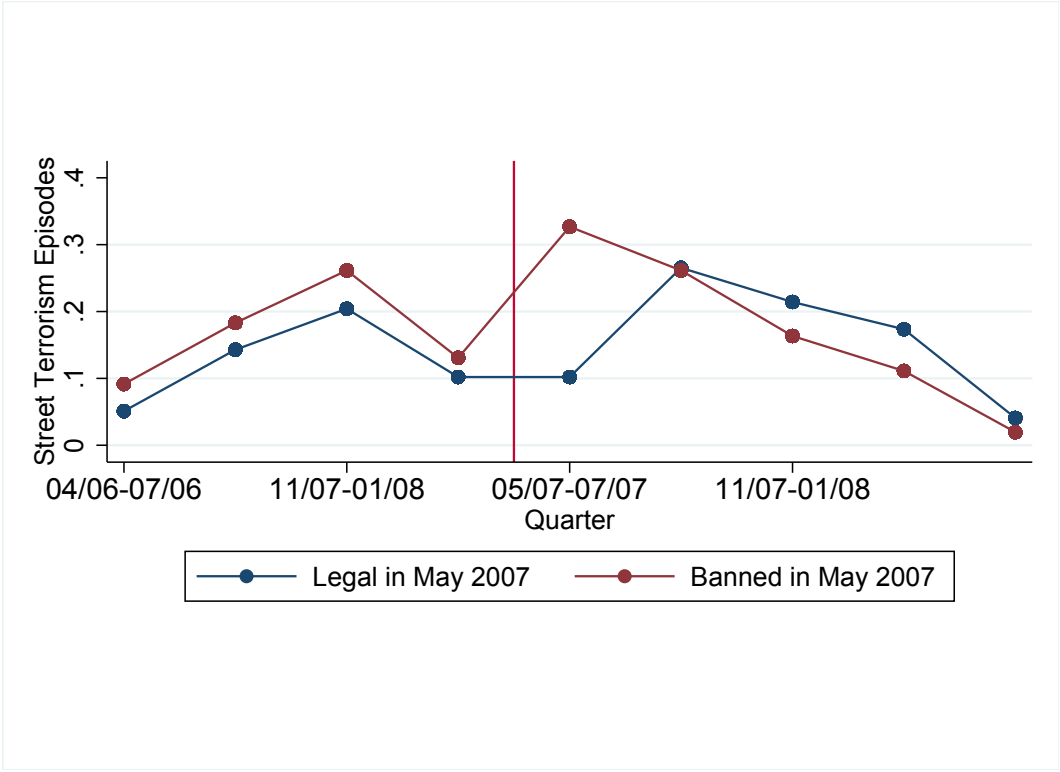


Table 3.7 reports difference in differences estimates. In the top panel, the dependent variable is the log of monthly street terrorism episodes; in the bottom panel a dummy for whether there was a street terrorist episode in a given municipality in a given month. All regressions include municipality FE and period (month× year) fixed effects by pre-ban presence of Batasuna. The regression results in column 1 of either panel show that wherever the ban was extended, the probability of an attack in the following month increased by almost 10pp, with 8% more attacks being carried out in those municipalities. However, if we consider the full post May 2007 period, the overall effect of the extension of the ban is very close to zero. The results in column 3, which exclude May 2007 from the sample, show that this is because after the spike in the very short run, in the remaining months the extension of the ban actually decreased the monthly probability of a street terrorist attack in treated municipalities by 2 percentage points.

One possible interpretation of these results is that the ban increases anger in the short run, increasing street terrorism, but this vanishes quickly and the weakening effect on the local infrastructure of the political movement dominates during the following months. Overall, the average effect over this period is close to zero, but unfortunately the sample covers only the period until June 2008. Hence, an avenue for future research is to extend the series to find out whether the slight decline continues for a longer period of time, which would imply that overall the ban reduced street terrorism, or wheter these differences fade away and the long run effect is indeed close to zero.

These effects are in contrast with some of the existing empirical evidence showing that in developing countries ethnic/political groups are more likely to engage in organized violence when they are excluded from the political system and are unable to pursue their interests or redress their grievances in a peaceful manner (Asal *et al.* (2015), Cederman *et al.* (2011), Bandyopadhyay and Younas (2011)). Abadie (2006) shows that political freedom across countries explains terrorism, but it does so in a non-monotonic way: countries in some intermediate range of political freedom are shown to be more prone to terrorism than countries with high levels of political freedom or countries with highly authoritarian regimes.

Table 3.7: Effects of the 2007 ban on Street Terrorism

	Dependent Variable		
	Log (1+Terrorist Epidodes)		
	(1)	(2)	(3)
I(Post May 2007) × I(Ban in 2007)	0.0779***	-0.0065	-0.013*
	(0.0289)	(0.0073)	(0.0075)
Sample	Until May 2007	Full	Except May 2007
Municipality F.E.	✓	✓	✓
Pre-ban presence × month FE	✓	✓	✓
N	3514	6777	6526
Municipalities	251	251	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample. All regressions control for a cubic in log population.

	Dependent Variable		
	I(Terrorist Episode)		
	(1)	(2)	(3)
I(Post May 2007) × I(Ban in 2007)	0.0942**	-0.013	-0.0212**
	(0.0399)	(0.0103)	(0.0104)
Sample	Until May 2007	Full	Except May 2007
Municipality F.E.	✓	✓	✓
Pre-ban presence × month FE	✓	✓	✓
N	3514	6777	6526
Municipalities	251	251	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample. All regressions control for a cubic in log population.

3.2 Interpretation and limitations

One limitation of this analysis is the possible existence of general equilibrium effects, which would violate the Stable Unit Treatment Value Assumption (SUTVA) which is required for a causal interpretation of the estimates in this chapter (Rubin, 2005). This is because the potential outcome for each municipality might depend on the treatment (ban) assignment of the remaining municipalities. For instance, the loss of visibility and resources which would lead the ban to reduce political support might be attenuated by the fact that Batasuna could be legal in some municipalities in 2007, which would bias the results towards finding no effect of the ban, as the party is still visible in some municipalities and could be redistributing resources across municipalities. On the other hand, municipalities where Batasuna was legal in 2007 could still be benefiting from "backfiring effects" because Batasuna was banned in some other municipalities. For instance, the ban might have increased the expressive value of voting for Batasuna in those municipalities as well, regardless of the particular legal status in their municipality. This would instead bias the results towards finding effects of the ban which are too large.

Still regarding SUTVA violations, there are a number of important developments throughout the period regarding ETA and Batasuna's strategies and positions towards violence that could be driven by the political bans, which being common to all municipalities are absorved by time fixed effects and impossible to relate to any precise variation at the micro level. Although they do not directly relate to street terrorism, they are nevertheless relevant to have a complete picture of the relationship between the ban of the political wing of ETA and terrorism in the Basque Country.

In December 1999, ETA ended a ceasefire which had started in September 1998. After that, ETA was active until March 2006 (although without mortal victims since April 2003), when it announced a permanent ceasefire.[18] During 2005 and 2006, the Spanish Government and ETA held a number of meetings in Geneva and Oslo with the aim of reaching an agreement. One critical question in these meetings was the law of parties and the ban of Batasuna, and a number of observers suggest that this was an important factor which

[18]In June 2005, ETA had declared a ceasefire against elected politicians

led to disagreement and to the end of the ceasefire (Romero Peña (2013), Eguiguren and Aizpeolea (2011)).[19] The ceasefire was *de facto* broken in December 2006, with an attack to Madrid airport, and *de jure* broken in June 2007, one month after the elections where the heterogeneous ban took place. In the communicate announcing the end of the ceasefire, ETA makes reference to the ban:[20]

"...The masks have fallen. Zapatero's mood has become fascism, denying rights to parties and citizens.[21] *(...) Citizens suffer from a lack of democracy. Attacks against the Basque Country, instead of disappearing, are intensifying and aggravating. The Spanish Justice has left thousands of citizens and the leftist independentists, who are the main asset of the peace process, out of these antidemocratic elections. The situation that we live in the Basque Country nowadays is a state of emergency. The recently held elections lack legitimacy. The Spanish government has responed to the permanent ceasefire offered by ETA with arrests, tortures and persecutions of all kinds. The minium democratic conditions needed for a negociation process do not exist (...)"*

Hence, the initial ban might have driven ETA's 2006 ceasefire, possibly aimed at its withdrawal, and the impossibility of an agreement might have in turn contributed to the end of ETA's ceasefire in 2007.[22] At the same time, in January 2011 ETA declared a permanent ceasefire, which was eventually followed by a declaration of definitive end of its armed activity in October 2011. While it is difficult to know to what extent the ban contributed to weaken ETA and contributed to that decision, a number of observers have suggested that ban created a conflict between ETA and its political wing, leading Batasuna to push for its end. For instance, the journalist Florencio Domínguez, one of the leading experts in ETA, states:[23]

"The leftist independentism ("izquierda abertzale") only started to contest ETA when the Strasbourg Court confirmed its ban (in 2009). It then assumed that with ETA being active it would not be possible to get back to the institutions nor do politics. It was, thus, the success

[19] See also El País report

[20] Link to the communicate

[21] Zapatero was the Spanish prime minister at the time

[22] After June 2007, there were 10 victims of ETA. 3 of them in France, 3 in Spain outside the Basque Country and 4 in the Basque Country, 2 of them being in municipalities where Batasuna was legal in 2007 (Mondragón, Azpeitia) 2 of them in municipalities where Batasuna was banned in 2007 (Arrigorriaga, Villarreal de Alava)

[23] Interview with Florencio Domínguez in the Basque Public media

of the ban strategy developed by the State what brought Batasuna to put pressure on ETA for the first time in its history. To develop that task it sought the support of Brian Currin and the mediators that he put in place. Moreover, Batasuna was favored because the police action had left ETA without strong leaders. A Batasuna which saw all doors closed dared to face a weakened ETA which did not have the capacity to rule over its political environment as it used to in the past"

Other authors, such as Whitfield (2014), describe a similar process. Hence, in this case an indiscriminate counter-terrorist policy would have led the moderates not to support violence but rather to push for the end of it, overwhelmed by its costs. In this case, besides the absence of an effect on street terrorism at the local level, the ban would have created dynamics which led to the end of ETA.

The interactions between the ban and the end of ETA are also relevant for the interpretation of the results of the effects of the ban on electoral support. When Batasuna reappeared in 2011, it did it with a major strategic change: its rejection of violence. Shortly after ETA declared a permanent ceasefire in January 2011, Batasuna presented its new electoral platform in February 2011, which for the first time rejected ETA's violence. Although the Courts had reasons to believe that this rejection was a fraud, with the Supreme Court initially banning this new platform before the Constitutional Court revoked this decision by just one vote of difference, this poses a challenge for the interpretation of the results on electoral support. An important question which I cannot answer, hence, is whether we would have observed the same effects on electoral support if Batasuna had not publicly rejected violence in 2011. Nevertheless, the fluctuations in support in the pre-ban period, some of which might relate to the intensity of the conflict and its ceasefires, seemed to be affecting treatment and control municipalities in a similar way. This was the case, for instance, in 1999, when Batasuna obtained its best pre-ban results in local elections after ETA had declared a permanent ceasefire, with media reports suggesting that this was an important factor explaining such results.[24]

[24] ABC, February 2006

Beyond these considerations, overall the results in this chapter are consistent with the qualitative study by Bale (2007), concluding that the pragmatic predictions that political bans routinely occasion –namely *perversity, futility and jeopardy*, as in Hirschman (1991)– are not borne out in reality.

3.3 Conclusions

This chapter exploits the finite length and the heterogeneous enforcement across municipalities of the ban of Batasuna, a political party in the Spanish region of the Basque Country which was banned because of its links to terrorism, to shed light on the consequences of political bans. I estimate the effect of a longer ban (2 electoral terms vs. 1 electoral term) on electoral support by differences-in-differences and I find that a longer ban had a negative effect, driven both by a lower number of candidatures and less votes received. These effects persist for at least two elections after the end of the ban and have spill-overs to support in regional elections. These spillovers persist after conditioning on post-ban presence and support, suggesting that the ban might have had an effect on voters' preferences.

The heterogeneous length of the ban is driven by the ability of the local section of the party being able to find candidates without links to previously banned candidatures, which might be endogenous if this ability is related to time-varying unobservables. To address this concern, I use the rule used by the public prosecutor to denounce local candidatures to construct an instrument which relies on a weaker identification assumption. Instrumental variables estimates, although less precise, are qualitatively similar to differences in differences estimates, suggesting that they capture a causal effect.

While the ban had large effects on electoral support, I find that the extension of the ban had no effects on street terrorism. However, this is the sum of a spike in street terrorism in the very short run (1 month after) and a moderate decline after the extension of the ban for around a year. The results are consistent with an increase in anger by Batasuna supporters in the very short-run, with the negative effect on the local movement infrastructure dominating after the initial anger fades away.

Appendix C

Table 3.8: Descriptive statistics, local elections

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Turnout	0.643	0.601	0.616	0.575	0.632	0.591
	(0.109)	(0.109)	(0.100)	(0.0928)	(0.107)	(0.104)
Batasuna votes	0.148	0.270	0.207	0.397	0.171	0.320
	(0.167)	(0.211)	(0.190)	(0.191)	(0.179)	(0.213)
Null votes	0.0463	0.0214	0.0444	0.0182	0.0455	0.0201
	(0.0898)	(0.0191)	(0.0904)	(0.0150)	(0.0900)	(0.0177)
Batasuna Support	0.194	0.291	0.251	0.416	0.217	0.340
	(0.165)	(0.209)	(0.179)	(0.190)	(0.173)	(0.210)
I(Batasuna list)	0.578	0.757	0.709	0.939	0.630	0.828
	(0.494)	(0.429)	(0.455)	(0.240)	(0.483)	(0.377)
Vote Share EA	0.0864	0.000451	0.109	0.000886	0.0953	0.000621
	(0.121)	(0.00788)	(0.139)	(0.0124)	(0.129)	(0.00989)
Vote Share PNV-EA	0.0399	0	0.0602	0	0.0479	0
(joint lists)	(0.131)	(0)	(0.160)	(0)	(0.143)	(0)
Population	10087.3	10092.2	6338.5	6567.9	8613.9	8713.5
	(38455.7)	(37998.0)	(13143.4)	(12950.6)	(31117.6)	(30760.9)
Municipalities	152	153	98	98	250	251
N	749	305	485	196	1234	501

Treated: municipalities where Batasuna was banned in 2007. In 2003, Batasuna support measured with the share of null votes. 2007 excluded from the sample.

Figure 3.6: Local Elections Turnout, Batasuna+EA Vote Share and Null Vote

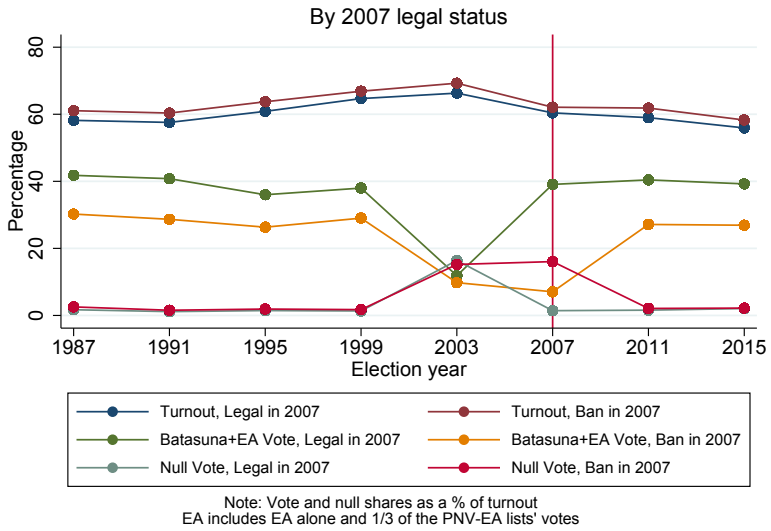


Table 3.9: Descriptive statistics, regional elections

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
Turnout	0.735	0.699	0.736	0.693	0.736	0.697
	(0.0923)	(0.0743)	(0.0894)	(0.0705)	(0.0911)	(0.0728)
Batasuna votes	0.205	0.176	0.248	0.204	0.222	0.187
	(0.134)	(0.210)	(0.130)	(0.228)	(0.134)	(0.217)
Null votes	0.00538	0.0786	0.00507	0.0989	0.00526	0.0865
	(0.00897)	(0.108)	(0.00510)	(0.125)	(0.00768)	(0.116)
Batasuna Support	0.205	0.250	0.248	0.299	0.222	0.269
	(0.134)	(0.174)	(0.130)	(0.171)	(0.134)	(0.174)
Vote Share EA	0.0906	0.0229	0.108	0.0254	0.0975	0.0239
	(0.104)	(0.0333)	(0.116)	(0.0387)	(0.109)	(0.0355)
Vote Share PNV-EA (joint lists)	0.179	0	0.177	0	0.178	0
	(0.263)	(0)	(0.260)	(0)	(0.262)	(0)
Population	8238.0	7965.3	5173.3	5171.8	7034.1	6874.6
	(31561.8)	(29896.8)	(10771.1)	(10351.1)	(25540.8)	(24242.7)
Municipalities	152	153	98	98	250	251
	(0)	(0)	(0)	(0)	(0)	(0)
N	898	306	581	196	1479	502

Table 3.10: Effects of the 2007 ban on support for Batasuna in local elections - with group trends

	Dependent Variable		
	Batasuna Support	I(Batasuna list)	Batasuna Support
	(1)	(2)	(3)
I(Post 2007) $\times$ I(Ban in 2007)	-0.0938***	-0.0609**	-0.0679***
	(0.0218)	(0.0294)	(0.0203)
I(Post 2007) $\times$ I(Batasuna list)			✓
Municipality F.E.	✓	✓	✓
Pre-ban presence FE	✓	✓	✓
Group-specific trends	✓	✓	✓
N	1735	1485	1735
Municipalities	251	251	251

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample. All regressions control for a cubic in log population. All regressions control for a cubic in municipality size

Figure 3.7: Oaxaca-Blinder, matching on pre-ban presence and Batasuna and EA support

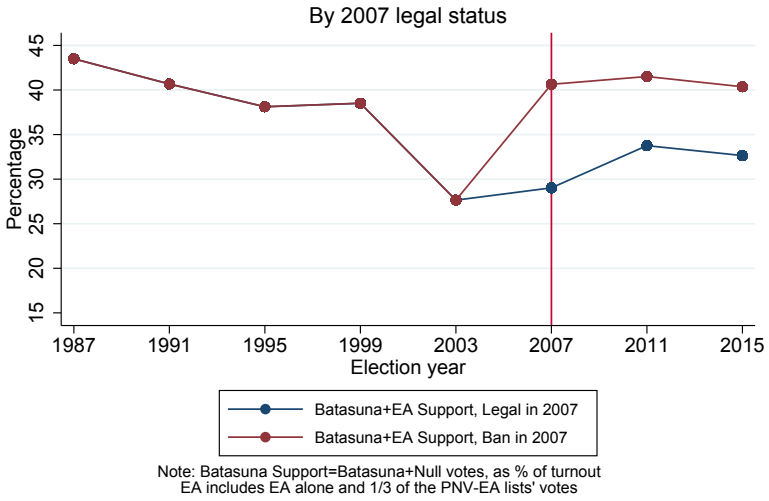


Table 3.11: Effects of the 2007 ban on support for Batasuna + EA, O-B

	Dependent Variable		
	Batasuna + EA Support	I(Batasuna list)	Batasuna +EA Support
	(1)	(2)	(3)
$I(\text{Post 2007}) \times I(\text{Ban in 2007})$	-0.0766***	-0.0608	-0.0542***
	(0.0225)	(0.0379)	(0.0199)
$I(\text{Post 2007}) \times I(\text{Batasuna list})$			✓
Pre-ban presence	✓	✓	✓
Pre-ban support	✓	✓	✓
Pre-ban EA support	✓	✓	✓
Municipalities	237	237	237

Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2003, the vote share of Batasuna is computed as the share of null votes. In all regressions, 2007 is excluded from the sample.

Table 3.12: Effects of the 2007 ban on support for Batasuna in regional elections, O-B

	Dependent Variable			
	Batasuna Support (1)	Batasuna Support (2)	Batasuna Support (3)	Batasuna Support (4)
I(Post 2007) $\times$ I(Ban in 2007)	-0.0132**	-0.0142**	-0.0127**	-0.0097*
	(0.0062)	(0.0058)	(0.0057)	(0.0059)
Pre-ban presence	✓	✓	✓	✓
Pre-ban support	✓	✓	✓	✓
Pre-ban EA support		✓	✓	✓
Post-ban presence (2011)			✓	✓
Post-ban vote share (2011)				✓
Municipalities	232	232	232	232

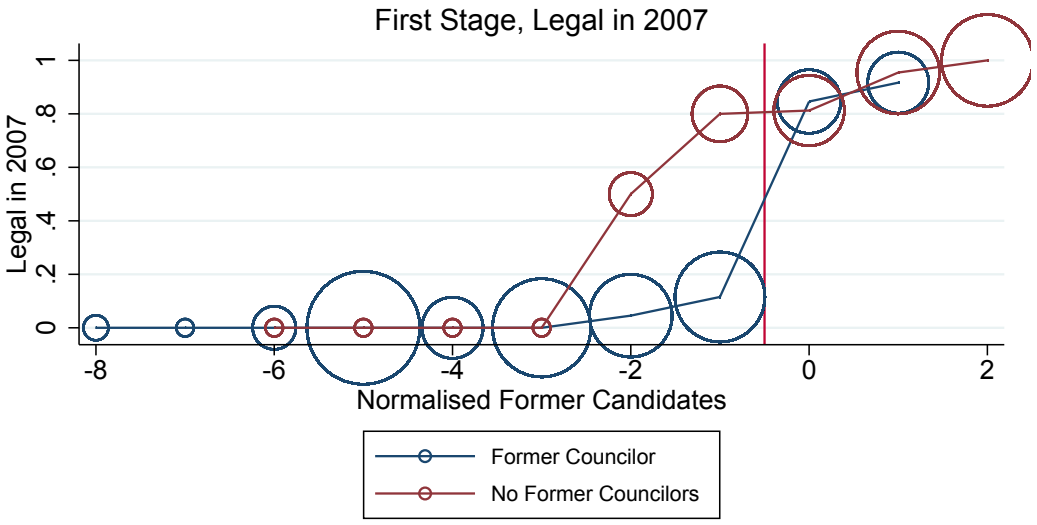
Standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***. In 2009, the vote share of Batasuna is computed as the share of null votes. All regressions include province dummies

Table 3.13: Descriptives, linked candidates

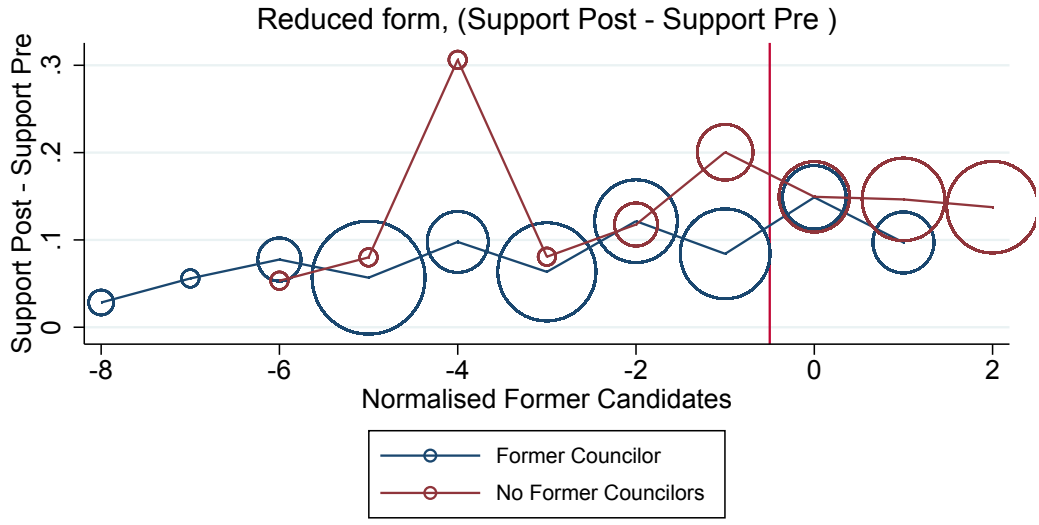
Variable Name	Treated	Control	Total
I(Former Councilor)	0.915	0.265	0.661
	(0.280)	(0.444)	(0.474)
Former Candidates	5.229	1.316	3.701
	(1.819)	(1.118)	(2.481)
I(Former Candidates $\geq$3)	0.954	0.153	0.641
	(0.210)	(0.362)	(0.481)
I(Former Councilor)$\times$(Former Candidates$\geq$3)	0.895	0.0408	0.562
	(0.307)	(0.199)	(0.497)
Municipalities	153	98	251

Treated: municipalities where Batasuna was banned in 2007. For banned municipalities with no information, I(Former Councilor)=1 and Former Candidates=List size

Figure 3.8: Effect of the 2007 ban on Local Support for Batasuna, IV



Note: Normalized Former Candidates= -(Former Candidates-2)
Circle sizes proportional to #municipalities per group



Note: Normalized Former Candidates= -(Former Candidates-2)
Circle sizes proportional to #municipalities per group

Table 3.14: Descriptive statistics, street terrorism, monthly

Variable Name	Treated		Control		Total	
	Pre	Post	Pre	Post	Pre	Post
I(Street Terrorist Episode)	0.0297	0.0369	0.0314	0.0510	0.0303	0.0424
	(0.170)	(0.189)	(0.174)	(0.220)	(0.172)	(0.202)
# Street Terrorist Episodes	0.0513	0.0630	0.0385	0.0569	0.0463	0.0606
	(0.357)	(0.398)	(0.226)	(0.258)	(0.312)	(0.350)
Municipalities	153	153	98	98	251	251
N	1989	2142	1274	1372	3263	3514

Bibliography

Abadie, A. (2006). Poverty, Political Freedom, and the Roots of Terrorism. *American Economic Review*, **96** (2), 50–56.

— and Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review*, **93** (1), 113–132.

Ade, F. (2013). Do constitutions matter? Evidence from a natural experiment at the municipality level. *Public Choice*, **160** (3), 367–389.

— and Freier, R. (2013). Divided government versus incumbency externality effect: Quasi-experimental evidence on multiple voting decisions. *European Economic Review*, **64**, 1 – 20.

Aidt, T. S. and Shvets, J. (2012). Distributive politics and electoral incentives: Evidence from seven us state legislatures. *American Economic Journal: Economic Policy*, **4** (3), 1–29.

Alesina, A. and Fuchs-Schündeln, N. (2007). Goodbye Lenin (or Not?): The Effect of Communism on People. *American Economic Review*, **97** (4), 1507–1528.

— and Giuliano, P. (2009). *Preferences for Redistribution*. NBER Working Paper 14825.

— and Rosenthal, H. (1995). *Partisan Politics, Divided Government, and the Economy*. Political Economy of Institutions and Decisions, Cambridge University Press.

— and — (1996). A theory of divided government. *Econometrica*, **64** (6), 1311–1341.

Angrist, J. D., Imbens, G. W. and Rubin, D. B. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association*, **91** (434), pp. 444–455.

Ansolabehere, S. and Puy, M. S. (2015). Ideology, Nationalism, and Identity in Basque Regional Elections. *Málaga Economic Theory Research Center Working Paper 2015-2*.

Artés, J. and Jurado, I. (2014). *Do majority governments lead to lower fiscal deficits? A regression discontinuity approach*. Mimeo.

Asal, V., Findley, M., Piazza, J. A. and Walsh, J. I. (2015). Political exclusion, oil, and ethnic armed conflict. *Journal of Conflict Resolution*.

Bale, T. (2007). Are bans on political parties bound to turn out badly? A comparative investigation of three "intolerant" democracies: Turkey, Spain, and Belgium. *Comparative European Politics*, **5** (2), 141–157.

Bandyopadhyay, S. and Younas, J. (2011). Poverty, political freedom, and the roots of terrorism in developing countries: An empirical assessment. *Economics Letters*, **112** (2), 171 – 175.

Barros, C. P., Caporale, G. M. and Gil-Alaña, L. A. (2009). Basque terrorism: Police action, political measures and the influence of violence on the stock market in the basque country. *Defence and Peace Economics*, **20** (4), 287–301.

Baskaran, T. (2013). Coalition governments, cabinet size, and the common pool problem: Evidence from the German states. *European Journal of Political Economy*, **32** (C), 356–376.

Bassi, A., Morton, R. B. and Williams, K. C. (2011). The effects of identities, incentives, and information on voting. *The Journal of Politics*, **73** (2), 558–571.

Bawn, K. and Rosenbluth, F. (2006). Short versus long coalitions: Electoral accountability and the size of the public sector. *American Journal of Political Science*, **50** (2), 251–265.

Beath, A., Christia, F., Egorov, G. and Enikolopov, R. (2014). *Electoral Rules and the Quality of Politicians: Theory and Evidence from a Field Experiment in Afghanistan*. NBER WP 20082.

Black, D. A., Berger, M. C. and Scott, F. A. (2000). Bounding parameter estimates with nonclassical measurement error. *Journal of the American Statistical Association*, **95** (451), 739–748.

Blais, A., Hroux-Legault, M., Stephenson, L., Cross, W. and Gidengil, E. (2012). Assessing the psychological and mechanical impact of electoral rules: Aquasi-experiment. *Electoral Studies*, **31** (4), 829 – 837.

—, Kim, J. and Foucault, M. (2010). Public spending, public deficits and government coalitions. *Political Studies*, **58** (5), 829–846.

—, Lachat, R., Hino, A. and Doray-Demers, P. (2011). The Mechanical and Psychological Effects of Electoral Systems: A Quasi-Experimental Study. *Comparative Political Studies*, **44**, 1599–1621.

Bordignon, M., Nannicini, T. and Tabellini, G. (2014). *Moderating Political Extremism: Single Round vs Runoff Elections under Plurality Rule*. CEPR Discussion Papers 10323, C.E.P.R. Discussion Papers.

Bourne, A. (2015). Why ban Batasuna? *Comparative European Politics*, **13** (3), 325–344.

Brennan, G. and Lomasky, L. (1997). *Democracy and Decision: The Pure Theory of Electoral Preference*. Cambridge University Press.

Broockman, D. E. (2009). Do Congressional Candidates Have Reverse Coattails? Evidence from a Regression Discontinuity Design. *Political Analysis*, **17** (4), 418–434.

Buchanan, J. M. (1954). Individual choice in voting and the market. *Journal of Political Economy*, **62** (4), 334–343.

Bueno de Mesquita, E. (2005). The quality of terror. *American Journal of Political Science*, **49** (3), 515–530.

— (2008). The political economy of terrorism: A selective overview of recent work. *The Political Economist*, **10** (1), 1–12.

— and Dickson, E. S. (2007). The propaganda of the deed: Terrorism, counterterrorism, and mobilization. *American Journal of Political Science*, **51** (2), 364–381.

Buesa, M. (2008). *Actividades terroristas de ETA y política antiterrorista: información estadística*. Cátedra de Economía del Terrorismo, Universidad Complutense de Madrid.

Busso, M., Gregory, J. and Kline, P. (2013). Assessing the incidence and efficiency of a prominent place based policy. *American Economic Review*, **103** (2), 897–947.

Cederman, L.-E., Weidman, N. B. and Gleditsch, K. S. (2011). Horizontal inequalities and ethnonationalist civil war: A global comparison. *American Political Science Review*, **105**, 478–495.

Clots-Figueras, I. and Masella, P. (2013). Education, language and identity. *The Economic Journal*, **123** (570), F332–F357.

Coate, S. and Knight, B. (2011). Government Form and Public Spending: Theory and Evidence from US Municipalities. *American Economic Journal: Economic Policy*, **3** (3), 82–112.

Cram, I. (2008). Constitutional responses to extremist political associations - ETA, Batasuna and democratic norms. *Legal Studies*, **28** (1), 68–95.

Curto-Grau, M., Solé-Ollé, A. and Sorribas-Navarro, P. (2012). *Partisan targeting of inter-governmental transfers & state interference in local elections: evidence from Spain*. Working Papers 2012/31, Institut d'Economia de Barcelona (IEB).

de la Calle, L. (2007). Fighting for Local Control: Street Violence in the Basque Country. *International Studies Quarterly*, **51** (2), 431–455.

— and Sánchez-Cuenca, I. (2013). Killing and Voting in the Basque Country: An Exploration of the Electoral Link Between ETA and its Political Branch. *Terrorism and Political Violence*, **25** (1), 94–112.

Dinas, E., Riera, P. and Roussias, N. (2015). Staying in the first league: Parliamentary representation and the electoral success of small parties. *Political Science Research and Methods*, **3**, 187–204.

Downs, A. (1957). An economic theory of political action in a democracy. *Journal of Political Economy*, **65** (2), 135–150.

Downs, W. M. (2012). *Political extremism in democracies: Combating intolerance*. Palgrave Macmillan.

Dragu, T. (2016). The Moral Hazard of Terrorism Prevention. *Mimeo, New York University*.

Duverger, M. (1954). *Political Parties: Their Organisation and Activity in the Modern State*. Methuen; Wiley.

Egger, P. and Koethenbuerger, M. (2010). Government spending and legislative organization: Quasi-experimental evidence from germany. *American Economic Journal: Applied Economics*, **2** (4), 200–212.

Eggers, A. C. (2015). Proportionality and Turnout Evidence From French Municipalities. *Comparative Political Studies*, **48** (2), 135–167.

Eguiguren, J. and Aizpeolea, L. (2011). *ETA. Las claves de la paz: Confesiones del negociador*. AGUILAR DIGITAL.., Penguin Random House Grupo Editorial España.

Feddersen, T., Gailmard, S. and Sandroni, A. (2009). Moral bias in large elections: Theory and experimental evidence. *American Political Science Review*, **103**, 175–192.

Fiorina, M. P. (1976). The voting decision: Instrumental and expressive aspects. *The Journal of Politics*, **38**, 390–413.

Fiva, J. H. and Folke, O. (2014). Mechanical and psychological effects of electoral reform. *British Journal of Political Science*.

Folke, O. (2014). Shades of brown and green: party effects in proportional election systems. *Journal of the European Economic Association*, **12** (5), 1361–1395.

— and Snyder, J. M. (2012). Gubernatorial midterm slumps. *American Journal of Political Science*, **56** (4), 931–948.

Freier, R. and Odendahl, C. (2012). *Do Absolute Majorities Spend Less?: Evidence from Germany*. Discussion Papers of DIW Berlin 1239, DIW Berlin, German Institute for Economic Research.

— and — (2015). Do parties matter? Estimating the effect of political power in multi-party systems. *European Economic Review*, **80** (C), 310–328.

Fuchs-Schündeln, N. and Schündeln, M. (2015). On the endogeneity of political preferences: Evidence from individual experience with democracy. *Science*, **347** (6226), 1145–1148.

Fujiwara, T. (2011). A Regression Discontinuity Test of Strategic Voting and Duverger's Law. *Quarterly Journal of Political Science*, **6** (34), 197–233.

Funk, P. and Gathmann, C. (2013). How Do Electoral Systems Affect Fiscal Policy? Evidence From Cantonal Parliaments, 1890-2000. *Journal of the European Economic Association*, **11** (5), 1178–1203.

Gagliarducci, S. and Nannicini, T. (2013). Do better paid politicians perform better? disentangling incentives from selection. *Journal of the European Economic Association*, **11** (2), 369–398.

—, — and Naticchioni, P. (2011). Electoral rules and politicians' behavior: A micro test. *American Economic Journal: Economic Policy*, **3** (3), 144–74.

Gaines, B. J. (1999). Duverger's law and the meaning of canadian exceptionalism. *Comparative Political Studies*, **32** (7), 835–861.

Gardeazabal, J. (2011). Linguistic polarization and conflict in the Basque Country. *Public Choice*, **149** (3), 405–425.

Garmann, S. (2012). *Do Coalitions Really Cause Larger Government Expenditures? Mixed Evidence from a Regression Discontinuity Design*. Ruhr Economic Papers 0339.

Gerring, J. (2005). Minor parties in plurality electoral systems. *Party Politics*, **11** (1), 79–107.

Gil-Alaña, L. A. and Barros, C. P. (2010). A Note on the Effectiveness of National Anti-Terrorist Policies: Evidence from ETA. *Conflict Management and Peace Science*, **27** (1), 28–46.

Hamlin, A. and Jennings, C. (2011). Expressive political behaviour: Foundations, scope and implications. *British Journal of Political Science*, **41**, 645–670.

Hillman, A. L. (2010). Expressive behavior in economics and politics. *European Journal of Political Economy*, **26** (4), 403 – 418.

—, Metsuyanim, K. and Potrafke, N. (2015). Democracy with group identity. *European Journal of Political Economy*, **40** (PB), 274–287.

Hirschman, A. (1991). *The Rhetoric of Reaction*. Harvard University Press.

Imbens, G. W. and Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, **62** (2), 467–75.

Isen, A. (2014). Do local government fiscal spillovers exist? Evidence from counties, municipalities, and school districts. *Journal of Public Economics*, **110** (C), 57–73.

Johansson-Stenman, O. and Martinsson, P. (2006). Honestly, why are you driving a BMW? *Journal of Economic Behavior and Organization*, **60** (2), 129 – 146.

Kamenica, E. and Egan Brad, L. (2014). Voters, dictators, and peons: expressive voting and pivotality. *Public Choice*, **159** (1-2), 159–176.

Kane, T. J., Rouse, C. E. and Staiger, D. (1999). *Estimating Returns to Schooling When Schooling is Misreported*. Working Paper 7235, National Bureau of Economic Research.

Kawai, K. and Watanabe, Y. (2013). Inferring strategic voting. *American Economic Review*, **103** (2), 624–62.

Kline, P. (2011). Oaxaca-Blinder as a Reweighting Estimator. *American Economic Review*, **101** (3), 532–37.

— and Moretti, E. (2013). Local Economic Development, Agglomeration Economies and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority. *The Quarterly Journal of Economics*.

Kontopoulos, Y. and Perotti, R. (1999). Government Fragmentation and Fiscal Policy Outcomes: Evidence from OECD Countries. In *Fiscal Institutions and Fiscal Performance*, NBER Chapters, National Bureau of Economic Research, Inc, pp. 81–102.

Laakso, M. and Taagepera, R. (1979). "Effective" Number of Parties: A Measure with Application to West Europe. *Comparative Political Studies*, **12**, 3–27.

Liang, C.-Y. (2011). Is there an incumbency advantage or cost of ruling in proportional election systems? *Public Choice*, **154** (3), 259–284.

Lijphart, A. and Aitkin, D. (1994). *Electoral Systems and Party Systems: A Study of Twenty-seven Democracies, 1945-1990*. Comparative European politics, Oxford University Press.

Lizzeri, A. and Persico, N. (2005). A drawback of electoral competition. *Journal of the European Economic Association*, **3** (6), 1318–1348.

Mattozzi, A. and Merlo, A. (2015). Mediocracy. *Journal of Public Economics*, **130**, 32 – 44.

Meredith, M. (2013). Exploiting friends-and-neighbors to estimate coattail effects. *American Political Science Review*, **107**, 742–765.

Merilainen, J. P. (2013). *Do Single-Party and Coalition Governments differn in Economic Outcomes? Evidence from Finnish Municipalities*. Mimeo.

Milesi-Ferretti, G. M., Perotti, R. and Rostagno, M. (2002). Electoral systems and public spending. *The Quarterly Journal of Economics*, **117** (2), 609–657.

Morgan, J. and Várdy, F. (2012). Mixed Motives and the Optimal Size of Voting Bodies. *Journal of Political Economy*, **120** (5), 986 – 1026.

Morton, R. B. and Ou, K. (2015). What motivates bandwagon voting behavior: Altruism or a desire to win? *European Journal of Political Economy*, **40** (PB), 224–241.

Myerson, R. B. (1993). Effectiveness of electoral systems for reducing government corruption: A game-theoretic analysis. *Games and Economic Behavior*, **5** (1), 118 – 132.

Pellicer, M. and Wegner, E. (2014). The mechanical and psychological effects of legal thresholds. *Electoral Studies*, **33** (0), 258 – 266.

Persico, N. and Lizzeri, A. (2001). The Provision of Public Goods under Alternative Electoral Incentives. *American Economic Review*, **91** (1), 225–239.

Persson, T., Roland, G. and Tabellini, G. (2007). Electoral Rules and Government Spending in Parliamentary Democracies. *Quarterly Journal of Political Science*, **2** (2), 155–188.

— and Tabellini, G. (2004). Constitutional rules and fiscal policy outcomes. *The American Economic Review*, **94** (1), pp. 25–45.

—, — and Trebbi, F. (2003). Electoral rules and corruption. *Journal of the European Economic Association*, **1** (4), 958–989.

Pettersson-Lidbom, P. (2008). Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach. *Journal of the European Economic Association*, **6** (5), 1037–1056.

— (2012). Does the size of the legislature affect the size of government? Evidence from two natural experiments. *Journal of Public Economics*, **96** (3), 269–278.

Pradeep Chhibber, K. K. (1998). Party Aggregation and the Number of Parties in India and the United States. *The American Political Science Review*, **92** (2), 329–342.

Primo, D. M. and Snyder, J. M. (2008). Distributive politics and the law of 1/n. *The Journal of Politics*, **70** (02), 477–486.

Rigobon, R. and Stoker, T. M. (2009). Bias from censored regressors. *Journal of Business & Economic Statistics*, **27** (3), 340–353.

Romero Peña, A. (2013). El proceso de negociación entre el Gobierno y ETA durante la etapa de José Luis Rodríguez Zapatero (2004-2011). *Historia Actual Online*, (30), 39–51.

Rubin, D. B. (2005). Causal inference using potential outcomes. *Journal of the American Statistical Association*, **100** (469), 322–331.

Saarimaa, T. and Tukiainen, J. (2015). Common pool problems in voluntary municipal mergers. *European Journal of Political Economy*, **38**, 140 – 152.

Sánchez-Cuenca, I. and de la Calle, L. (2009). Domestic terrorism: The hidden side of political violence. *Annual Review of Political Science*, **12** (1), 31–49.

Sartori, G. (1976). *Parties and Party Systems: A Framework for Analysis*. No. v. 1 in Parties and Party Systems, Cambridge University Press.

Schaltegger, C. A. and Feld, L. P. (2009). Do large cabinets favor large governments? Evidence on the fiscal commons problem for Swiss Cantons. *Journal of Public Economics*, **93** (1-2), 35–47.

Shayo, M. and Harel, A. (2012). Non-consequentialist voting. *Journal of Economic Behavior & Organization*, **81** (1), 299 – 313.

Solé-Ollé, A. (2006a). Expenditure spillovers and fiscal interactions: Empirical evidence from local governments in Spain. *Journal of Urban Economics*, **59** (1), 32–53.

Solé-Ollé, A. (2006b). The effects of party competition on budget outcomes: Empirical evidence from local governments in Spain. *Public Choice*, **126** (1), 145–176.

Solé-Ollé, A. and Sorribas-Navarro, P. (2008). The effects of partisan alignment on the allocation of intergovernmental transfers. Differences-in-differences estimates for Spain. *Journal of Public Economics*, **92** (12), 2302–2319.

Stock, J. H. and Yogo, M. (2002). *Testing for Weak Instruments in Linear IV Regression*. NBER Technical Working Papers 0284, National Bureau of Economic Research, Inc.

Tsebelis, G. (2002). *Veto Players: How Political Institutions Work*. Princeton University Press.

Van der Straeten, K., Sauger, N., Laslier, J.-F. and Blais, A. (2013). Sorting out mechanical and psychological effects in candidate elections: An appraisal with experimental data. *British Journal of Political Science*, **43**, 937–944.

Whitfield, T. (2014). *Endgame for ETA: Elusive Peace in the Basque Country*. Oxford University Press.

Woo, J. (2003). Economic, political, and institutional determinants of public deficits. *Journal of Public Economics*, **87** (3-4), 387–426.