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THREE EMPIRICAL ESSAYS ON POLITICAL ECONOMY

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Introduction

Since the explosion of economists' interest for the economic analysis of questions typically placed until then in the realm of political science, the "political economy" label emerged as a sort of contested term. For some, political economy is narrowly defined as the study of how political constraints affect the economic decision-making process (Drazen, 2000; Persson & Tabellini, 2000). Others, however, advocate for a broader definition claiming that political economy is the application of the economic approach to the analysis of political behavior and institutions (Weingast & Wittman, 2006). In any case, political economy has certainly bridged the agendas of political sciences and economics in the last decades, enabling social scientists to enrich their methodological approach to address a wide range of relevant research questions, as well as improve the understanding about the complex interplay between politics and economics.

This thesis is composed by three essays, each one presenting a contribution to the empirical literature in the field of political economy. The contributions of this thesis are directed to the following specific areas: i) support for democracy and sociotropic evaluation, ii) political economy of media, and iii) economic costs of conflicts. These essays all entailed the construction of new datasets and the use of the most advanced quantitative techniques to address relevant and unresolved questions in political economy.

In the first place, **Chapter 1**, entitled *Support for Democracy and Sociotropic Evaluation in the Shadow of Past Nondemocratic Regimes: The Case of Latin America*, examines from a new perspective the controversial relationship between sociotropic evaluation (i.e. the individuals' evaluation of the country economy) and support for democracy. In line with the David Easton' classic definition (Easton, 1957), many scholars claim that support for democracy is entrenched in profound individuals' values and is not responsive to economic cycles. However, others have found that there could be a relationship between sociotropic evaluation and attitudes towards democracy, depending on the economic and political context. This chapter poses that one of the context in which sociotropic evaluation affects the individuals' support for democracy arises when the evaluation of contemporary democracies deviates from the evaluation of the past non-democratic regimes' performance in emerging democracies. In such a way, past non-democratic regimes serve as a benchmark for the

evaluation of current democratic governments. This chapter provides empirical evidence of this relationship for the case of Latin America, where this phenomenon is widely recognizable. Using a set of multinomial models and an IV-Probit models, we found that the deviation in evaluations impacts support for democracy: as the assessment of the last military government gets better and the present country economy worsens, individuals are less likely to support democracy. This finding reveals some important challenges for democratic consolidation: emerging democracies does not only have to show a good economic performance by themselves, but also they must prove to be better than previous nondemocratic regimes.

In **Chapter 2**, entitled *Television Bias and Electoral Results in Catalonia*, we inquire into the political economy of media for an until then unexplored case. This field of political economy has grown rapidly in the last decades and several excellent surveys have already been written. However, most of the empirical research have thus far focused on highly consolidated democracies (e.g. the USA and Scandinavian countries) and some formerly authoritarian countries (e.g. Russia and East Germany). The Catalonia's political context characterized by a long-standing secessionist conflict with Spain is a newfangled case study to analyze the effect of a sub-national media such as TV3, which has an alleged pro-independence bias, on political outcomes. This paper therefore contributes to the existing literature on the effect of media on politics by identifying to what extent voting results in Catalonia can be explained by exposure of individuals to TV3. By drawing on a natural experiment based on the geographically differentiated expansion of the public channel TV3 in Catalonia in the early eighties and using a Difference-in-Differences Kernel matching method, we found that the introduction of TV3 caused an increase in both the voter turnout and the *Convergència i Unió* vote share in the 1984 Catalan parliamentary elections, political coalition that has mostly managed the channel since its foundation and has been one of the strongest Catalan nationalist forces in Catalonia. Furthermore, we also showed that TV3 could have had a negative effect on the vote share of other political parties competing in the same election. In particular, we found that the exposure to TV3 negatively affected the vote share of *Partit Socialista Unificat de Catalunya* (PSUC), an old left-wing political party that was dissolved in the early 80s after several electoral failures.

In **Chapter 3**, entitled *The Impact on the Stock Market of a Secession Campaign in a EU Member State*, we examine the economic effects of non-violent conflicts taking the Catalan pro-independence movement as a case study. Over the last decades, nationalist discourses seem to have dominated the political landscape in the developed world. Contradicting the idea of a global community, nationalists have fueled conflicts against supra-national structures (e.g. the Brexit, Scotland), as well as have led to the escalation of sub-national strains in regions demanding more sovereignty (e.g. Catalonia). Nevertheless, few studies have thoroughly examined the economic consequences of pro-independence movements in the context of consolidated democracies, which is surprising given the aforementioned success of these nationalist discourses. By using event studies, we analyze how a variety of events related to the Catalan pro-independence movement from 2010 to 2015 affected the stock returns of Catalan firms. We found that the movement, despite its apparent strength and the dramatic claims made by the extremes in the debate, has not had an economically significant impact on the stock returns. Nevertheless, though small, there are some significant effects on both Catalan and non-Catalan firms mostly related to street demonstrations and events against the movement, as well as some differentiated effects related to the firms' economic activity sector and their political position respecting the movement. The lack of large economic impacts and the firms' reaction to some events suggest several possible interpretations about the investors' attitude toward the movement.

Each chapter, of course, raises a lot of relevant questions for future work. With respect to support for democracy, it is important to examine the benchmark across borders. This chapter only focused on a benchmark across time (we compare the economic performance of single countries over time), however individuals plausibly do not solely take the past of their countries as a reference point, but they can also use both the past and present of other geographically or culturally close countries when supporting democracy. For the chapter about political economy of media, it is important to go into a content analysis to study with more detail the mechanism by which TV3 could influence on electoral results. Another topic of great significance for future work in the TV3 case study is the role of competition. The insight that competition amongst media can limit the scope of persuasion has important implications for regulation and public policy, but it has not been explored enough yet. Does TV3 still have an influence on political attitudes in the context of a wider range of media

today (many TV channel, radio stations and internet, social networks, among others)? Providing convincing answers to these questions will help us to better understand the role of media in the Catalan politics today. Finally, the chapter on the economic costs of the Catalan pro-independence movement also raises important questions. For instance, it is crucial to thoroughly inquire into political connections of firms, since there could be differential effects according to the extent of connection with the political parties that are leading the movement. As a final comment, I sincerely hope that the arduous work that I will present in the following pages serves as a contribution to improve our understanding, even modestly, of the functioning of politics and economy.

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Chapter 1

Support for Democracy and Sociotropic Evaluation in the Shadow of Past Non-Democratic Regimes: The Case of Latin America

Abstract

This paper examines to what extent the non-democratic past of countries may affect the citizens' support for contemporary democracies. To do so, we revisit the relationship between attitudes towards democracy and the individuals' evaluation of present country economy (sociotropic evaluation). Specifically, we test whether the "deviation in evaluations", understood as the difference between the individuals' evaluation of the present country economic situation and the individuals' evaluation of the last nondemocratic regime, has some effect on support for democracy in Latin America. Using a set of multinomial models and an IV-Probit model, we found that the deviation in evaluations has a strong effect on support for contemporary democracies: as the assessment of the last military government gets better and the present country economy worsens, individuals are less likely to support democracy. This finding reveals some important challenges for democratic consolidation: emerging democracies does not only have to show a good economic performance by themselves, but also they must prove to be better than previous nondemocratic regimes.

Key Words: Democracy, Economic growth, Instrumental variables

JEL: D72, P52

1. Introduction

The economic success of some past nondemocratic regimes overshadows the performance of contemporary democracies and may therefore hamper democratic consolidation. The performance of nondemocratic regimes becomes then a benchmark for individuals when assessing the performance of emerging democracies, which adversely affect the stability of democratic regimes (Chu et al., 2008). This paper is intended to enhanced our understanding about the effect of past nondemocratic regimes on support for contemporary democracies by providing empirical evidence for the case of Latin America, where this phenomenon is widely recognizable (Remmer, 1978; Biglaiser, 2002; Keech, 2004; Centioni, 2014).

Figures by Latinobarómetro shows that democracies are hardly consolidating in this region. On average between 2000 and 2010 (Table 1), less than half of these contemporary democracies reached the two-thirds level that some scholars suggest as a minimum threshold of mass support for democracy in consolidated regimes (Diamond, 1999, p. 179). Moreover, though support for authoritarianism is still lower than support for democracy, it cannot be regarded as negligible. For instance, preferences for authoritarian regimes reaches 34% in Paraguay, and in other countries such as Brazil, Ecuador, Guatemala, Mexico, Peru and Dominican Republic is above 20%. Furthermore, recent political events suggest that, in fact, past authoritarian governments may still be able to influence current politics. For instance, political parties related to the authoritarian past of some countries, such as the Popular Force Party in Peru (related to *Fujimorism*), the Colorado Party in Paraguay (related to the dictatorship of Alfredo Stroessner), or the National Party in Honduras (related to the dictatorship of Tiburcio Carías Andino) have exerted a great influence on these countries' politics in the last years. Also, figures from Latinobarómetro suggest that many individuals, despite the loss of civil and political liberties and the violation of human rights suffered under some past military governments, have a positive opinion about these regimes. This evidence is registered by the 2010 wave of Latinobarómetro through the following question: *What is your opinion about the last Military Government? Would you say that it was very good, good, about average, bad or very bad?* The results are striking. In some countries, such as Colombia, Guatemala, Honduras, and Paraguay, the share of positive evaluations (both very

good and good) exceeds the share of negative evaluations (both very bad and bad). In others, such as El Salvador, Mexico, and Peru, the negative barely overtakes the positive. And in most cases, the imprecise category of “Average” -that is, neither positive nor negative- reaches the highest proportion. Only some countries such as Argentina, Bolivia, Chile, Panama, Uruguay and Dominican Republic show a clear tendency to badly evaluate the last military governments (Table 2).

This paper is therefore intended to examine to what extent the nondemocratic past may affect the citizens’ support for contemporary democracies. To do so, we revisit the sociotropic voting theory by analyzing the debated effect of sociotropic evaluation on support for democracy. However, instead of analyzing the single effect of sociotropic evaluation, we focus on the impact of the deviation in sociotropic evaluation from the evaluation of the performance of the last nondemocratic regime on support for democracy. Our hypothesis is that sociotropic evaluation can indeed influence support for democracy when it deviates from the evaluation of past nondemocratic regimes. We exploit a dataset from the 2010 wave of the Latinobarómetro survey which covers 17 Latin American countries that experienced authoritarian regimes at some point during the second half of the 20th century, but are considered democracies today. In addition to asking about the evaluation of the present country economy (sociotropic evaluation), as it regularly does, this 2010 wave also asks respondents the opinion about the last military government¹. Thus, using these two evaluations we build a measurement of the deviation in evaluations between the present economic performance² and the performance of the last military government. The potential difficulties when combining these two questions will be discussed in section 3. To inquire into our research question, we employ a set of multinomial models and we also address some potential problems of endogeneity by using instrumental variables for subjective evaluations.

¹ It is necessary to clarify that, for the sake of this research, we take the concepts of “authoritarian government” (used in questions regarding support for democracy) and “military government” as synonyms. After all, most of the dictatorships experienced by Latin American countries during the 20th century was military or civil-military (Loveman, 1999).

² Admittedly, by using the sociotropic question we are restricting the government performance to economic issues. However, for the sake of simplicity as well as the lack of statistical information under authoritarian governments, we would rather focus only on economy. In addition, we believe that economic per capita growth captures one of the most important and visible aspects of the government performance.

This paper is mainly a contribution to the literature about support for democracy. Overall, the effect of sociotropic evaluation (i.e. the evaluation of the present country economy) on support for democracy is contested. In line with the David Easton' classic approach (1957, 1965, 1975), many scholars claim that this *diffuse* support for democracy is entrenched in profound individuals' values and is not responsive to economic cycles (Mattes & Bratton, 2001; Lagos, 2003; Graham & Sukhtankar, 2004; Huang et al. 2008). However, others have found that indeed there is a relationship between sociotropic evaluation and attitudes towards democracy, depending on the economic and political context (Dalton, 1994; Chu, et al., 2008; Magalhaes, 2014; Cordero & Simon, 2016). Our paper therefore differs from that literature in that we incorporate as a key explanatory variable the deviation in the sociotropic evaluation of the current democratic government from the performance of the last military regime. Additionally, this paper is also close to a case-study literature about political learning process such as Dalton (1994) for the case of Germany, Mishler & Rose (2007) for Russia, and Weeks (2002) for Chile, who investigate how the dictatorial past influences the attitudes towards democracy today. However, our paper differs in that we employ a cross-country approach, which enriches the analysis by providing a more generalized evidence.

The main results reveal that the deviation in evaluations has a strong effect on support for democracy. That is, as the evaluation of the present economic situation worsens and the evaluation of the last military government gets better, individuals are less likely to support democracy, and this effect seems to be robust under different specifications. This is a compelling finding because it provides evidence that attitudes to democracy in emerging democracies, besides being affected by the present economy, are also shaped by a comparative measurement of the present economic situation versus the evaluation of the past authoritarian regimes. In terms of policy, this finding implies important challenges for democratic governments. That is, those who advocate democracy as a superior political system must keep in mind that emerging democracies are under the shadow of the past nondemocratic regimes, hence democracies must constantly demonstrate that they are being better than old regimes as to providing welfare to citizens. Furthermore, though economy is important, democrats must also emphasize that democracy, despite its imperfections, can be the least imperfect of all political systems available, and can have invaluable benefits in terms

of civil and political rights. Otherwise, emerging democracies will be continually in danger of breaking down.

The article is structured as follows. The second section presents an analytical framework, some existing empirical contributions and the main hypothesis to be tested. The third section provides a description of the data. The fourth section discusses the empirical strategy and results. Finally, the fifth section draws some conclusions.

2. Analytical Framework

The David Easton's work (1957, 1965, 1975) was the pioneer in distinguishing between the *diffuse* and *specific* support for democracy. The diffuse support refers to a generalized attachment to democracy which changes slowly since its source is largely entrenched in values acquired in early socialization and through social learning (Easton 1957: 395–400; 1965: 125–127; 1975: 445). On the other hand, the specific support is related to the individuals' satisfaction with the effectiveness of governments' performance as to the economic and/or political situation. From this theoretical perspective, some aspects of government performance such as ups-and-downs of the country economic situation (i.e. sociotropic evaluation) may affect the individuals' support for incumbents and political coalitions close to government (specific support), but not undermines the support for democracy as political system (diffuse support), since this is rooted in more durable values and is not responsive to economic cycles (Mattes & Bratton, 2001; Lagos, 2003; Graham & Sukhtankar, 2004; Huang et al. 2008).

In fact, the robust empirical relationship between sociotropic evaluation and specific support for democracy has enabled the development of the renowned Economic Voting Theory: If the economy goes well, voters will electorally support incumbents; but if economy goes bad, voters will punish incumbent by voting for the opposite parties. Nevertheless, some scholars, analyzing different regions in the world, have found that the effect of sociotropic evaluation may go beyond the punishment of incumbents. If the economic performance is bad enough, that may lead to the punishment of the whole democratic system, so threatening the stability of democracy by paving the way to radical alternatives with authoritarian traits (i.e. affecting the diffuse support) (Chu, et al., 2008; Magalhaes, 2014; Cordero & Simon, 2016). In this vein, a growing literature on populism has associated the severity of economic crisis with the

emergence of both right and left-wing populist parties in Europe, opening a fruitful path of research in this field (Kriesi & Pappas, 2015).

The variety of approaches and findings suggests that the effect of sociotropic evaluation on support for democracy indeed exist, but it hinges on the severity and durability of economic crisis as well as some particular contexts. Chu et al. (2008) points out that one of the contexts in which sociotropic evaluation is key to understand diffuse support for democracy is given when countries have recently moved from nondemocratic regimes to democratic ones. In such scenario, it is plausible that individuals' feel disappointed with democracy as a political regime when the evaluation of the country economy under such regime adversely deviates from the assessment of the last nondemocratic government, especially when the performance of the latter is broadly recognized for having brought wellbeing for citizens. Hence, the performance of nondemocratic governments become a sort of benchmark to democratic ones, which ends up affecting support for democracy. In words of Chu et al. (2008, p. 85), "democracies that have inherited records of stunning economic success from nondemocratic predecessors are under pressure to live up to high citizen expectations regarding continued economic excellence".

From a theoretical perspective, this idea largely relies on a notion proposed by Kahneman & Tversky (1979) in their Prospect Theory of decision making process: almost no attribute can be judged in isolation, but can only be judged in relation to something else, thus individuals usually make decisions based on benchmarks. This notion allows us to see that the important thing when explaining support for democracy in countries that have recently experienced nondemocratic regimes is not necessarily the evaluation of the present democratic government *per se*, but rather whether such evaluation outperforms the assessment of the last nondemocratic regime. Conversely, support for democracy will fall as the evaluation of the democratic government underperforms the assessment of the last nondemocratic one.

Some authors have analyzed different problems using the benchmark approach as a central point. For instance, Kayser & Peress (2012), analyzing the relationship of economic performance and electoral accountability, found that when the economy in a single country contracts, voters often punish the government, but when many economies contract, voters turn against their governments much less frequently. They claim that ultimately no economic

figure is innately good or bad, what is considered a good performance in one period or place might be considered bad in another. In a similar vein, Engler (2016), examining the role of corruption as determinant of the emergence of new parties, found that the deviation of the perceived corruption above the traditional corruption level leads to a loss of trust in the political elite and therefore boosts the electoral success of new competitors. Referring to the specific theme of our research, even though there are some compelling case studies that have analyzed the political learning process (Dalton, 1994; Weeks, 2002; Mishler & Rose, 2007), the benchmark approach, to our knowledge, has not been applied from a cross-country perspective.

Restricting our analysis to sociotropic evaluation, which is solely one aspect of governments' performance, we propose to test the following hypothesis:

Hypothesis: In emerging democracies, individuals are more (less) likely to support democracy if their evaluation of the present country economy outperforms (underperforms) past nondemocratic regimes.

3. Data

To analyze empirically the relationship between support for democracy and the deviation in sociotropic evaluation from the evaluation of past nondemocratic government, we employ data of 17 Latin American countries from the 2010 wave of the Latinobarómetro public opinion survey: Argentina, Bolivia, Brazil, Colombia, Chile, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay and Venezuela. All these countries share the characteristic of having experienced authoritarian regimes at some moment of the second half of the 20th century, most of them in the form of military or civil-military dictatorships, and have moved on to democratic governments during the eighties and early nineties as a result of the third wave of democratization. Hence, this group of countries is suitable to test our hypothesis. Accordingly, it is worth noting that we ruled out Costa Rica, which is usually included in the Latinobarómetro survey, because it is the only country in the sample that has not experienced nondemocratic governments. Finally, it is also important to mention that though Latinobarómetro has been conducted on a yearly basis for the last two decades, we restrict

our analysis to the 2010 wave as until now is the only one containing the question on the individuals' opinion about past nondemocratic regimes in Latin America.

3.1. Dependent Variable: Support for Democracy

Latinobarómetro, as the majority of political attitudes surveys, captures the diffuse support for democracy by means of the following three-choices question: “With which of the following statements do you agree most? 1) Democracy is preferable to any other kind of government; 2) Under some circumstances, an authoritarian government can be preferable to a democratic one; and 3) For people like me, it doesn't matter whether we have a democratic or non-democratic regime”. Yet, that is not the only alternative to measure support for democracy and support for authoritarian regimes. Another option derives from the following two-choices question: “Would you support a military government in replace of a democratic one if things get very bad or you would not support under any circumstances a military government?”. Then, we will address the main analysis by using the first survey question as a multinomial dependent variable with three options, and later we will check the robustness of our results using the alternative survey question as a binary dependent variable. Table 3 shows the percentage of support for democracy, support for authoritarian regimes and political indifference in each country in 2010, and Table 4 presents the results of the alternative measurement.

3.2. Main Independent Variable: Deviation in Evaluations

In order to test our hypothesis, the main independent variable has to be able to gauge the deviation between i) the individuals' evaluation of the present country economic situation, and ii) the individuals' evaluation of the last nondemocratic regime. The evaluation of the present country economic situation, or sociotropic evaluation, is captured by the following survey question: In general, how would you describe the country's present economic situation? Would you say that it is...? The respondents then express their evaluation in a scale from 1 (very good) to 5 (very bad).

Table 5 5 presents the percentage of each evaluation category by country.

Regarding the evaluation of the last nondemocratic regime, we do not count on questions equal to that for the sociotropic evaluation. However, as mentioned in the introduction, the 2010 wave of Latinobarómetro asked individuals the following question: What is your opinion about the last Military Government? Would you say that it was...? This question is also scaled in a range from 1 (very good) to 5 (very bad) (See Table 2). Admittedly, at least two concerns may arise from this survey question. Firstly, it is conceptually much broader than the sociotropic one, thus it may also include, among other issues, the respondents' view about the performance of the last military governments in terms of corruption, civil liberties and human rights. Despite the apparent difference between these two survey questions, we consider that it is not an actual limitation, nor does it invalidates the indicator. This is so because it ultimately allows us to test whether the difference in the evaluation of a specific aspect of the performance of contemporary democracies (such as the present economic performance) with respect to a general image about the last military government affects the support of contemporary democracies, which is related to the central hypothesis of our research.

Secondly, individuals have not been exposed in the same way or with the same intensity to the last military governments, which can affect opinion about this type of regimes, and affect, in turn, the attitude towards democracy. Although some Latin American military or authoritarian governments were, to a certain extent, recent (e.g. Chile, Argentina, Uruguay, Peru), they were not lived in person by the youngest. Also, some countries experienced military governments much longer ago (e.g. Colombia, Venezuela), which makes the personal experience of those regimes even more unlikely. Nevertheless, we consider that what is important for the sake of this research is the “image” or idea about the last military government that makes the individuals' evaluation of such regimes possible, no matter if that idea was shaped by going through such experience in the flesh, or transferred by others (socializing with older relatives, friends, or by the learning process through the education system). Moreover, as will be discussed later, we will include a set of control variables in order to capture differences at both individual and country level that can influence the attitude towards democracy. Similarly, we will later propose an instrumental variable approach in order to identify the causal effect of such deviation in evaluations on support for democracy.

Turning to the estimation of our *main independent variable*, this is thus constructed by subtracting the responses of the two survey questions described above:

$$DiE_i = Sociotropic\ Evaluation_i - Last\ Military\ Evaluation_i \quad (1)$$

Where DiE_i stands for the “Deviation in Evaluations” between the evaluation of the present country economic performance (*Sociotropic Evaluation_i*) and the evaluation of the last military government (*Last Military Evaluation_i*) for individual i . Due to that, these questions are ranged between 1 (very good) and 5 (very bad), the variable DiE_i can take values from -4 to 4. It is worth noting that the evaluation of the present economy will be better than the evaluation of the last military government as the indicator takes negative values, i.e. $Sociotropic\ Evaluation_i < Last\ Military\ Evaluation_i$, because *Sociotropic Evaluation_i* will tend to 1 (very good) and *Last Military Evaluation_i* will tend to 5 (very bad), and vice versa. Thus, the most favorable scenario for the present economic situation would be a value of -4 (*Sociotropic Evaluation_i* = 1 (*very good*) and *Last Military Evaluation_i* = 5 (*very bad*)), while the worst would be a value of 4. As Table 6 shows, the percentage of individuals whose evaluation of the last military government is better than the evaluation of the present economy (positive values of DiE_i) is greater in countries such as Colombia, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, Paraguay and Peru.

3.3. Controls

We will also include some control variables at both individual and country level which are relevant for explaining differences in attitudes towards democracy across individuals (See Table 7 for a brief description of all variables included in the analysis). At individual level, we include social demographic features such as sex (male=1, female=0), age (continuous variable), marital status (married=1, otherwise=0), years of education (continuous variable) and religion (catholic=1, otherwise=0). We also include ideology (Dummies variables for the categories “Extreme left”, “Left”, “Right”, “Extreme right” and “No ideology”, the category “Centre” is used as reference), and crime (victim of a crime=1, no victim=0). Additionally, we will control for what people understand as democracy. To do so, we will incorporate, as a control variable, the question: “Some people say that without a National Congress there can be no democracy, while others say that democracy can work without a National

Congress. Which is closer to your view?” It could be said that those who consider a country as democratic only with a National Congress being involved can be considered liberal democrats; by contrast, those who claim that democracy can function without a National Congress are deviating from a liberal concept of democracy.

At country level, we will include five control variables for 2009, that is one-year lagged with respect to the 2010 Latinobarómetro wave. First, an index of democracy built by Polity IV (scale from -10 to 10, where 10 is full democracy and -10 is failed State). The quality of contemporary Latin American democracies varies across countries and, in some cases, has experienced a considerable reduction in the last years (e.g. Venezuela). Undeniably, the current level of democracy is a contextual factor that could explain differences in attitudes towards democracy across countries. Second, we control for the age of democracy, which is the number of years since the last authoritarian regime was overthrown until the year 2010. This control is very important as not all countries became democratic at the same time, hence there are democracies older than others, namely, there are democracies that have had more time to consolidate than others, and that can also explain differences in support for democracy today (See Table 10

for a summary of the authoritarian periods). Third, an index of corruption based on Transparency International (scale from 0 to 10, where 0 is highly corrupt and 10 highly clean). Although the relationship between corruption and democracy is controversial, there are some studies which support the idea of an effect of the former on the latter (Johnston, 1999) (Linde & Erlingsson, 2012). We also include an indicator of inequality for 2009. Inequality is measured by the GINI index using data from SEDLAC. The role of inequality has been emphasized by some authors in order to explain attitudes towards democracy as well as the tension between democracies and dictatorships (Acemoglu & Robinson, 2005; Schäfer, 2013). Finally, we include the real GDP per capita for 2009 at constant 2005 national prices. This variable is important because if economic situation affects support for democracy, GDP per capita should account for differences in levels of support for democracy across countries. Information of GDP per capita is from Penn Tables 8.0.

4. Empirical Analysis and Results

4.1. Multinomial Models

The estimation problem is to figure out whether the deviation in evaluations DiE_i is related to individuals' support for democracy in Latin America, considering that support for democracy is a three-choices categorical variable. Thus, we develop several multinomial logit models to test our hypotheses. A multinomial logit model is a generalization of the logit model for binary choice where the response variable has more than two outcomes ($M > 2$) that are mutually exclusive. As in the binary case, we are interested in how changes in the elements of the regressors affect the response probabilities of decision makers, considering that only $m-1$ probabilities can be freely estimated because probabilities sum to 1³.

The general form for estimating probabilities in a multinomial model, assuming M categories and 0 as the reference category, is as follows:

$$P(\text{Support Democracy}_i = m) = \frac{\exp(\alpha_1 + \gamma_1 DiE_{i1} + \sum_{k=1}^K \beta_{1k} X_{ik})}{1 + \sum_{h=1}^M \exp(\alpha_h + \gamma_h DiE_{ih} + \sum_{k=1}^K \beta_{hk} X_{ik})} \quad (2)$$

For $m = 1, 2, 3 \dots M$.

And for the reference category,

$$P(\text{Support Democracy}_i = 0) = \frac{1}{1 + \sum_{h=1}^M \exp(\alpha_h + \gamma_h DiE_{ih} + \sum_{k=1}^K \beta_{hk} X_{ik})} \quad (3)$$

In our model, $\text{Support Democracy}_i$ specifically has 3 categories: 1 if individuals support democracy, 2 if they prefer authoritarian regimes, and 3 if they considered politically indifferent. γ_m is the effect of the deviation in evaluations DiE_{im} for each alternative m , and β_{mk} is a set of K coefficients for each alternative that corresponds to the effect of a set of K control variables X_{ik} . We are interested in the sign of γ_m for the category ‘‘Support for Democracy’’ when the reference category is ‘‘Support for Authoritarian Regime’’. To confirm our hypothesis, γ_m should be negative, namely an inverse relationship between $\text{Support Democracy}_i$ and DiE_{im} . This is so because, as explained in sub-section 3.2., when the assessment of the last military government is better than the sociotropic evaluation, the

³ Although we also include some country level controls, which it suggests a hierarchical data structure, we discard to perform a multilevel model because the small number of countries make the country-level parameters be estimated much less precisely than would be suggested by OLS estimation of using all individual-level observations (Bryan 2013).

deviation must go to positive values (The largest deviation in favor of the last military government would be when $Sociotropic\ Evaluation_i = 1$ (very good) minus $Last\ Military\ Evaluation_i = 5$ (very bad)). Then, the more positive the deviation, individuals are less likely to support democracy rather than authoritarian regimes, and vice versa. We also include country fixed effects in all multinomial specifications and robust standard errors.

4.1.1. Main Results

Table 8 summarizes the results of the multinomial logit model for the pair of interest: support for democracy when the reference is the preference for authoritarian regimes. Additionally, Table 8 also shows the pairs support for democracy versus political indifference and authoritarian regimes versus political indifference to provide some additional findings. These results are presented in relative risk ratios and the complete models are available at the Annex.

The main results arising from these multinomial models support our research hypothesis. The deviation in evaluations DiE_i exerts a negative and significant effect on the likelihood of supporting democracy rather than authoritarian regimes (columns 5 to 9 at Table 8). A one-unit increase in the deviation (i.e. as the evaluation of the last military government is one-unit better than the evaluation of country economy) implies a reduction of about 27% in the likelihood of supporting democracy rather than authoritarian regimes. These results remain after introducing controls in an escalated manner. Figure 1 shows that the predicted probabilities of supporting democracy goes down in the extent that the evaluation of the last military government is better than the evaluation of country economy (i.e. positive values of the deviation). By contrast, the evaluation of country economy by its own (columns 1 to 4 at Table 8) is no significant after including political controls (column 3) and change the sign when introducing the deviation in evaluations (column 9), which shows that the deviation is more robust in comparison to the single measurement of sociotropic evaluation.

4.1.2. Additional Results

Although is not the core of our research, the effect of the deviation in evaluations on political indifference also deserve some comments. As seen at Table 8, there are two possibilities: when it comes to the pair Support for Democracy versus Political Indifference, increments in the deviation (i.e. the evaluation of the last military government is better than the evaluation of country economy) make individuals be more likely to be indifferent rather than supporting democracy, and when it comes to the pair authoritarian regimes versus political indifferent, individuals are more likely to support former. This ultimately suggest that when evaluation of last military governments is better than the evaluation of country economy, individuals are more likely either to be indifferent or support authoritarian regimes (depending on the pair under analysis), but not support democracies.

Another notable result is that the effect of the deviation in evaluations on support for democracy is conditioned by the age⁴. As Figure 2 suggests, in the extent that the deviation goes toward positive values, namely the evaluation of the last military government becomes better, the log odds of support for democracy rather than authoritarian regimes for both older and younger individuals go down. It is worth noting, however, that the slope of older individuals is steeper, which means that support for democracy decreases faster for this age group. Ultimately, this suggest that oldies, i.e. those who are more likely to have experienced a military dictatorship or at least are more close in time to such regimes, are more inclined to support that kind of regimes than youngers when the sociotropic evaluation gets worse with respect to the evaluation of the last military government. This result provides some insights about how individuals of different ages may attach different weights to political shocks (see Bartels, 2014).

4.1.3. Robustness Check

To check the robustness of our result we use an alternative measure of support for democracy based on the survey question: *“Would you support a military government in replace of a democratic one if things get very bad or you would not support under any circumstances a military government?”*. Then, we coded as 1 whether respondents would not support a

⁴ The approach that we follow for doing the interaction between the deviation in evaluations and age is to compute simple slopes, i.e., the slopes of the dependent variable on the independent variable (deviation) when the conditioning variable (age) is held constant at different combinations of high and low values, say 1 standard deviation above the mean and one standard deviation below the mean (See Figure 2).

military government under any circumstances (support for democracy), and 0, whether they would support a military government as a replacement of a democratic government if things would get very bad (support for authoritarian regimes). As we count on a dependent variable with two possible responses, we run this time a logit model whose results are presented in Table 9.

Columns 1 to 4 at Table 9 shows that, as the case of the multinomial model, there is a significant effect of the deviation in evaluations on support for democracy using the alternative survey question, and this effect remains significant after introducing a set of controls. A one-unit increase in the deviation in evaluations, that is as the evaluation of the last military government is better than the democratic one in one unit, translates into a reduction of about 40% in the likelihood of supporting democracy. By contrast, the effect of the evaluation of country economy by its own is not significant, which provides evidence about the importance of the comparative measurement of evaluation instead of the single sociotropic evaluation when it comes to explaining support for democracy.

4.2. Instrumental Variables: IV-Probit Model

The multinomial logit model developed above can have endogeneity problems, which may lead to draw the wrong causal inference. Endogeneity of DiE_i may arise because of omitted variables, that is unobservable variables that would explain the support for a particular type of political regime regardless of the individuals' assessment of the current economy and the last military dictatorship. Also because of reverse causality, namely $Support\ Democracy_i$ and DiE_i can influence each other. For example, some individuals can be convinced that an authoritarian regime is better than a purely democratic one, thus this idea then can influence the evaluation of the last military government. Ultimately, these endogeneity problems lead to DiE_i being correlated with the error term, i.e. the assumption of conditional independence is not met.

A common technique for addressing endogeneity is through instrumental variables, which have to satisfy two requirements: i) the instrument must be partially correlated with the endogenous variable DiE_i controlling for the other exogenous variables in the model; and ii)

it must be uncorrelated with the errors in the model u_i ⁵. We believe that the deviation of objective economic conditions under democracy with respect to the last nondemocratic regime -specifically the difference between i) the average GDP per capita growth in democracy ($\Delta GDP Democracy_j$) and ii) the average GDP per capita growth in the period of the last nondemocratic government ($\Delta GDP Military_j$) for country j - can meet the requirements for being a good instrument of the deviation in evaluations DiE_i .

$$DiP_j = \Delta GDP Democracy_j - \Delta GDP Military_j \quad (4)$$

Why do we think that these instruments could work well? Firstly, economic conditions under democracy and dictatorship vary considerably across countries offering the potential for explaining variation in individual-level assessments in each country. Secondly, as will be shown in the next section, these objective economic conditions are relevant and predict the subjective evaluation of DiE_i . Furthermore, it is worth mentioning that previous studies in the field of economic voting theory have convincingly employed objective economic conditions as instrumental variables for subjective evaluations of country economy (see Nadeau, Lewis-Beck & Belanger, 2013; Hansford & Gómez, 2015).

Regarding exogeneity, because these variables measure objective conditions, they should be exogenous to individual political preferences. That is, unlike respondents' evaluation of the present economy and the last military government, national economic conditions are not contaminated by individuals' political preferences. Thus, it considerably reduces the problem of reverse causality. Nevertheless, one potential concern with using economic conditions as an instrument for evaluations is that there could be some sort of correlation between the instruments and some country unobservable characteristics of the country that could affect the support for democracy. For this reason, we rely on the inclusion of some country level controls such as the current level of democracy, level of corruption, the age of democracy, and inequality to reduce such source of endogeneity. Finally, with respect to the exclusion restriction, it raises the question of whether the instruments have a direct effect on the dependent variable. According to Hansford & Gómez (2015), the effect of the objective

⁵ This second requirement, also known as exogeneity of the instrument, is equivalent to say that the instrumental variable must neither be correlated with any other unobservable variable that explain *Support Democracy_i* nor must directly affect *Support Democracy_i*, the effect on the dependent variable must be only through the endogenous variable DiE_i (exclusion restriction).

conditions is indirect to preferences for political regimes, since such effect plausibly passes through the evaluation. This reasoning is also consistent with our model.

The rationale of using the instrument is, therefore, to isolate the part of the variation in the subjective evaluation (DiE_i) due to an exogenous source, that is the objective economic conditions, and not to unobservable variables. In other words, there must be a subgroup of individuals -which will be used to obtain the causal effect- whose evaluation of the present economy and the last military government responds to an informed opinion and not because of other unobservable characteristics.

It is worth saying that we are aware that it would be more comprehensive to employ several measures of economic performance as instruments, such as inflation, investment in infrastructure, etc., as well as some measures of social progress such as inequality and social expenditure⁶. As Stiglitz, Sen, & Fitoussi (2009) warn, GDP per capita is not the best measurement of neither economic performance nor social progress. Unfortunately, getting reliable quantitative information from the countries and periods under the rule of military governments in Latin America is not possible⁷. Extending the set of objective conditions to be employed as instruments and check their validity are tasks for future work.

The definition of the authoritarian periods in each country is based on negative values of the “Polity IV score”. This indicator summarizes the level of competitiveness, openness and participation in the electoral processes of each country. For each year and country, a “Polity Score” ranges from -10 to +10, with -10 to -6 corresponding to autocracies, -5 to 0 corresponding to closed anocracies, 0 to 5 corresponding to open anocracies, and 6 to 10 to democracies. Table 10 provides a description of such periods for each country.⁸ To define the democratic periods we estimate the average GDP per capita growth within one decade prior to the year 2010, i.e. since 2000 until 2009. An average indicator built that way will

⁶ Including more instruments, besides doing the analysis more thorough, it would have the statistical advantage of enabling us to test the exogeneity of the instruments by means of overidentification tests (e.g. Sagan or Hansen tests).

⁷ For instance, there is not available information about inequality in the periods of military governments in Ecuador (1972 - 1979), Nicaragua (1934 - 1979), Colombia (1948 - 1957), and Venezuela (1945 - 1959).

⁸ There is one special case in the countries classification. According to Polity IV score, Venezuela is classified since 2009 as a closed anocracy (i.e. negative values in the Polity IV score), which corresponds to the Hugo Chávez’s government. For this research, we take the previous authoritarian regime, the Marco Pérez Jimenez’s government (1945-1959), as the benchmark to compare the present economic performance.

therefore contain recent important economic facts, such as the good performance of the early 2000s boosted by the boom of natural resources and the effect of the 2008 economic crisis. The information about GDP per capita growth is from Penn Tables 8.0.

Thus, following the same rationale of the deviation in (subjective) evaluations DiE , if DiP_j is positive (negative), it means that the economic performance under the more recent democracy has been on average higher (lower) in comparison to the last authoritarian regime. The results of these indicators are shown in Table 11. From these figures, it is evident that the economic performance has not necessarily been better in democracies (at least in the last ten years), but it has been better, in some cases, under authoritarian regimes.

For the sake of ease and to focus only on the pair support for democracy versus support for authoritarian regime, we employ an IV-Probit model. This entails that the dependent variable is no longer a multinomial, but a dummy that takes on 1 whether respondents support democracy, and 0 whether they support authoritarian regimes (we so far discard the choice “politically indifferent”). Nevertheless, we later use other specifications of the dependent variable as well as alternative survey questions to check the robustness of the results. Specifically, we also introduce the dependent variable as one that takes on 1 whether respondents support democracy, and 0 whether they either support authoritarian regimes or are political indifferent. Finally, as an alternative question we employ the same as used in the multinomial model, that is one that takes on 1 whether respondents would not support a military government under any circumstance, and 0 whether they would support a military government in replace of a democratic government whether things get very bad.

We therefore pose a two-stages least squares regression as follows:

$$\widehat{DiE}_i = DiP_j \hat{\delta}_1 + x_i \hat{\beta}_1 + x_j \hat{\beta}_2 + \hat{u}_i \quad (5)$$

$$Support\ Democracy_i = \widehat{DiE}_i \gamma + x_i \beta_1 + x_j \beta_2 + u_i \quad (6)$$

Where the first-stage regression (equation 5) defines \widehat{DiE}_i as the predicted DiE_i regressed on the instrument DiP_j , which takes on 1 if the difference is positive ($\Delta GDP\ Democracy_j > \Delta GDP\ Military_j$) and 0 if negative ($\Delta GDP\ Democracy_j < \Delta GDP\ Military_j$). Furthermore, as detailed in sub-section 3.3., x_i is a set of controls at individual level: male,

age, marital status, years of education, religion, ideology, being victim of a crime, and liberal democrat. x_j is a set of controls at country level: index of democracy, age of democracy, index of corruption, and average of inequality under the democratic period. The second-stage regression (equation 6) will thus be the causal effect of DiE_i -instrumented by objective economic conditions- on $Support\ Democracy_i$. Given that the second-stage equation models a dichotomous dependent variable and the endogenous variable is continuous, the most appropriate model is an IV-probit. Both the first and the second-stage regressions are run using robust standard errors. Finally, as robustness checks we will also run the IV-probit model using robust standard errors clustered by regions as well as employing two alternative dependent variables. Robustness checks will be explained with more detail later on.

4.2.1. Main Results

Instrumenting Subjective Evaluations: After estimating the difference between the average GDP under democracy versus the authoritarian periods, we proceed to test if this indicator is statistically relevant as instrument for the difference between the respondent's evaluation of the present economic performance versus the evaluation of the last military government. In column 2 in Table 12, the estimate for the difference in objective economic performance DiP_j is negative and significant (P-value<0.001), which support our idea that as the difference in objective performance increases (democracy performance is increasingly better than military government performance), the difference in the respondent's evaluation of the present economic situation decreases. More precisely, for those individuals who live in countries where the economic performance of the last ten years is better than the economic performance of the last military government (Dummy=1), the probability that the difference in the evaluation of present economic performance versus the evaluation of the last authoritarian regime is lower (i.e. the latter is better than the former) decrease on average to -0.257.

Although there can be a statistically significant relationship between the instruments and the endogenous variable, some problems can persist. As with any IV model, we need to consider whether these instruments are relevant by relying on two types of tests: identification and weak instruments. The identification test is essentially the test of the rank of the coefficients matrix under the null hypothesis that the equation is under-identified. The Kleibergen-Paap

rk Wald statistic reports a P-value<0.000, which means that the null is rejected and thus the matrix is full column rank, i.e., the model is identified. Furthermore, Stock & Yogo (2005) provide a test for weak instruments. Kleibergen-Paap Wald rk F statistic reports a value of 35.74, which reveals that the bias in our IV estimates is less than 10% of the bias in the OLS estimates⁹. Thus, we can regard our instruments as not weak.

The Effect of Deviation in Evaluations on Support for Democracy: Having defined a relevant instrument for the difference in the respondent's evaluation of the present economic performance versus the last military government, we turn to the main goal of this paper: to estimate the effect of such difference in evaluations on support for democracy in Latin America. Column 1 in Table 12 presents the coefficients of a bivariate probit model without instrumenting, whereas Column 3 shows the estimations of the IV-probit model.

The main result is that the effect of the difference in evaluations is negative and significant in both models (P-value<0.001), which support our hypothesis. In other words, the negative relationship between our measures of DiE_i and support for democracy indicates that as DiE_i increases (i.e. the last military government outperforms the present economic situation), individuals are less likely to support democratic regimes today. The other side of the coin would be that, as DiE_i decreases (i.e. the present economic situation outperforms the last military government), individuals are more likely to support democratic regimes today.

Furthermore, note that the effect in the IV-probit model is almost four times larger than the bivariate probit. While the former indicates that an increase in 1 unit in the DiE_i leads to an average decrease of 0.04 in the probability of support democracy, the latter shows that an increase in 1 unit in the DiE_i leads to an average decrease of 0.15. In other words, it seems that the effect of the difference in evaluations on the support for democracy is underestimated in the bivariate probit model. Thus, it could be said that when isolating the variation in the difference in evaluations only due to the objective economic performance, we are indeed taking into account only the effect of "more rational" individuals, therefore the effect on the support for democracy is larger. Instead, if we do not isolate this variation, the difference in evaluations is influenced by things that we cannot capture, and thus there can be individuals

⁹ Kleibergen-Paap rk Wald statistic is reported instead of Cragg-Donald Wald F statistic when using robust standard errors.

whose preferences for authoritarian regimes are not influenced by the actual economic performance, and although the economy under democracy goes well in comparison to the last military governments, they will prefer authoritarian regimes. Moreover, it is worth noting that the Wald test of exogeneity is rejected ($P\text{-value} < 0.000$), which supports the fact that the difference in evaluations is not exogenous, and thus we must instrument this endogenous variable to make appropriate causal inferences.

4.2.2. Robustness Checks

We check the robustness of our key findings by, firstly, using a different standard error specification, and secondly, employing two alternative dependent variables. Column 1a and 1b in Table 13 shows the predicted probabilities of the IV-probit model with clustered robust standard errors at region level. In this specification, the error terms are assumed to be correlated within clusters, but uncorrelated across clusters. Failure to control for the within-cluster error correlation can lead to misleadingly small standard errors, and consequently to misleadingly large t-statistics and low P-values. Given the sampling design of the Latinobarómetro survey, we cluster at the level of the primary sampling unit, that is, the main regions in which each country's sample is stratified. In total, there are 268 regions. We discarded to cluster by country because the sample is very small (17 countries), which could also lead to misleading standard errors. The result of the first-stage regression shows that the instrument is still negative and significant ($P\text{-value} < 0.1$), which indicates that even after clustering, the instrument can explain the endogenous variable. However, it is important to say that the significance decreases as well as the Kleibergen-Paap rk LM statistic (3.495), which may cast some doubts on the relevance of the instrument. On the other hand, the effect of the differences in evaluation (column 1b) on support for democracy remains negative and highly significant ($P\text{-value} < 0.001$) as in the original IV-probit model.

Secondly, we employ two alternative dependent variables for support for democracy. To remind, the dependent variable used until now is based on the following question: “With which of the following statements do you agree most? 1) Democracy is preferable to any other kind of government; 2) Under some circumstances, an authoritarian government can be preferable to a democratic one; and 3) For people like me, it doesn't matter whether we have

a democratic or non-democratic regime”. Based on this question, we built a dummy variable that takes on 1, whether individuals express their preferences for democracy, and 0, whether, under some circumstances, they prefer authoritarian regimes. That is, we dispensed with the option three. Now, we utilize as the first alternative dependent variable the same survey question but includes the option three as a part of the category 0. Column 2 in Table 13 shows that the effect remains negative and highly significant ($P\text{-value} < 0.001$), but is a bit larger than the original model (-0.187). Additionally, we propose to check the robustness of the results employing another alternative question, the wording is: “Would you support a military government in replace of a democratic one if things get very bad or you would not support under any circumstances a military government?”. Then, we create a dummy variable that takes on 1, whether respondents would not support a military government under any circumstance, and 0 whether they would support a military government in replace of a democratic government if things would get very bad. Column 3 in Table 13 shows that the effect is not significant when considering this alternative measure, nevertheless we obtained the expected sign (negative).

5. Conclusion

This paper contributes to the literature on attitudes toward democracy by providing evidence that when individuals support democracies in countries that have recently experienced authoritarian regimes the subjective evaluation of present economic performance is not the only factor that is at stake, but also the evaluation of the last nondemocratic regime performance plays a role serving as a benchmark for the former.

These findings reveal serious problems for democratic consolidation. First and foremost, democracies seemingly do not just have to economically perform well, but they also need to be better than other forms of political regimes experienced in the past. In particular, democracies must be better than those nondemocratic regimes suffered recently before the third wave of democratization, otherwise citizens would incline to support authoritarian governments because these regimes proved to be more effective when improving citizens’ quality of life. Secondly, these results also imply that democrats have not been able to transmit to citizens the message that democratic values are also related to the preservation of civil and political rights and not only to economic issues. To put it simply, we still do not

understand what Winston Churchill stated in a rough way: “Democracy is the worst form of Government except for all those other forms that have been tried from time to time...” (Churchill, 1947).

Four main paths of research arise from this paper. Firstly, because this paper only examines the subjective evaluations available in the 2010 wave of Latinobarómetro, it is necessary to find pool or panel data to analyze the temporal evolution of such subjective evaluations. Secondly, regarding the IV-probit model, it is necessary to get a wider set of variables on government performance, including political performance under past authoritarian regimes as well as more economic or social indicators related to individuals’ welfare such as education, health, poverty, amongst others, to be proved as instrumental variables. Third, it is also important to strengthen the identification strategy that allows us to do more reliable causal inferences. Thus, future work should inquire into other methodological approaches such as natural experiments. Finally, it is also important to examine the benchmark across borders. This research focused on a benchmark across time (we compare the economic performance of single countries over time), but individuals plausibly do not solely take the past of their countries as a reference point, but they can also use both the past and present of other geographically or culturally close countries when supporting democracy.

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Figures

Figure 1: Marginal Effects of Die Multinomial Model

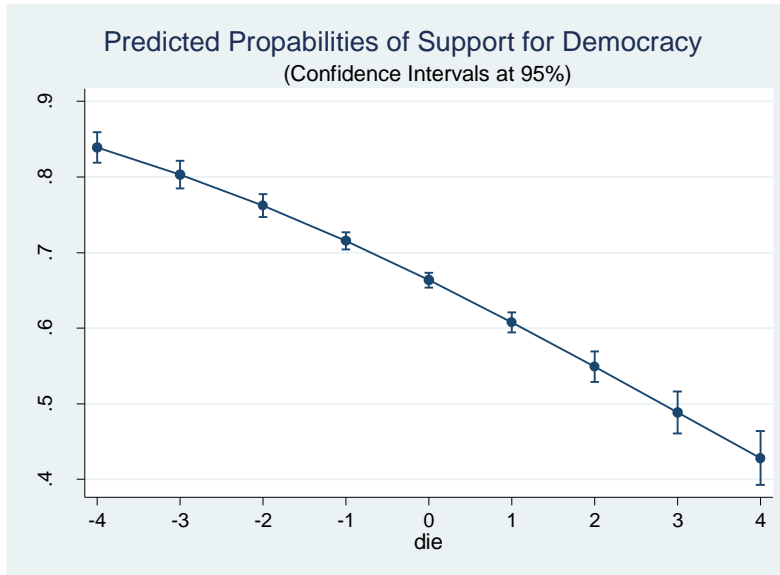
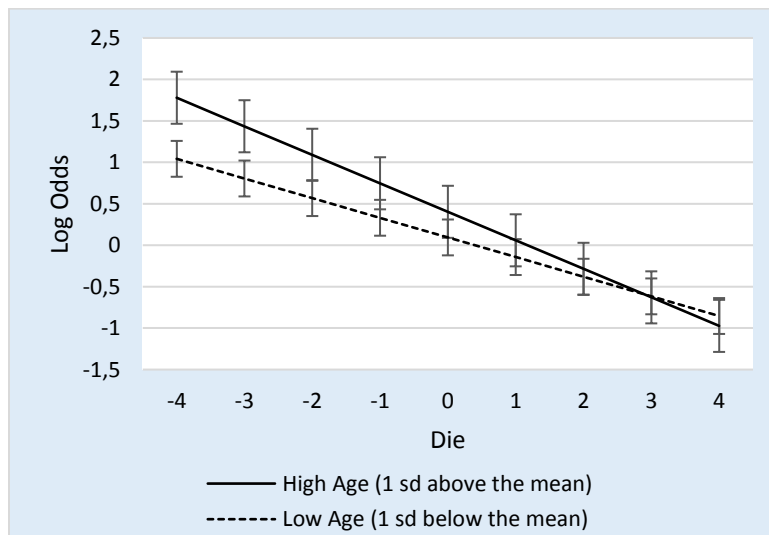


Figure 2: Interactions Deviation in Evaluations (Die) x Age
Support for Democracy/Authoritarian Regime



Tables

Table 1: Average 2000-2010 of Support for Democracy, Support for Authoritarian Regimes and Political Indifference (Figures in percentage %)

Country	Support Democracy	Support Authoritarian	Indifference
Argentina	67.37	19.25	13.38
Bolivia	65.41	17.57	17.02
Brazil	49.78	21.11	29.11
Colombia	56.18	15.42	28.39
Costa Rica	79.52	8.88	11.61
Chile	58.14	15.66	26.20
Ecuador	55.54	22.00	22.45
El Salvador	58.48	16.52	25.00
Guatemala	46.67	25.70	27.63
Honduras	57.95	16.02	26.03
Mexico	53.65	19.83	26.52
Nicaragua	62.20	12.47	25.33
Panama	62.46	17.42	20.13
Paraguay	44.39	36.53	19.08
Peru	58.92	20.06	21.01
Uruguay	81.90	9.65	8.45
Dominican Republic	70.26	19.41	10.33
Venezuela	76.23	13.58	10.20
Total Average	61.39	18.17	20.44

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

Table 2: Opinion about the Last Military Government 2010 (Figures in percentage %)

Country	Very Good	Good	Average	Bad	Very Bad	Total	Total
						Positive	Negative
Argentina	1.75	11.75	16.67	27.08	32.75	13.50	59.83
Bolivia	0.92	6.58	24.17	27.17	21.5	7.50	48.67
Brazil	1.50	16.28	22.18	17.52	13.04	17.78	30.56
Colombia	12.33	33.00	16.92	5.50	2.17	45.33	7.67
Costa Rica*	-	-	-	-	-	0.00	0.00
Chile	2.67	11.00	26.83	26.75	22.75	13.67	49.50
Ecuador	0.75	14.33	31.42	18.50	14.58	15.08	33.08
El Salvador	3.40	16.90	32.30	16.40	6.40	20.30	22.80
Guatemala	7.10	18.90	34.10	15.60	9.00	26.00	24.60
Honduras	11.10	18.70	17.70	10.70	8.60	29.80	19.30
Mexico	4.00	20.25	46.75	16.67	8.83	24.25	25.50
Nicaragua	3.90	12.40	23.70	20.70	12.60	16.30	33.30
Panama	6.90	11.20	26.00	25.00	22.90	18.10	47.90
Paraguay	9.00	27.42	29.92	14.00	8.50	36.42	22.50
Peru	2.75	14.92	32.75	15.67	2.75	17.67	18.42
Uruguay	1.42	11.92	18.00	19.5	32.42	13.34	51.92
Dominican Republic	2.60	8.30	15.60	24.70	34.90	10.90	59.60
Venezuela	2.75	11.67	22.5	18.42	18.5	14.42	36.92
Total Average	4.40	15.62	25.74	18.82	16.01	20.02	34.83

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

* Costa Rica has not experienced military governments.

Table 3: Support for Democracy, Support for Authoritarian Regimes and Political Indifference 2010 (Figures in percentage %)

Country	Support Democracy	Support Authoritarian	Indifference
Argentina	67.29	19.63	13.08
Bolivia	73.59	9.80	16.61
Brazil	60.92	21.66	17.42
Colombia	63.64	10.71	25.65
Costa Rica	74.61	12.33	13.06
Chile	67.99	11.79	20.22
Ecuador	69.08	12.84	18.08
El Salvador	64.33	20.57	15.10
Guatemala	51.77	19.73	28.51
Honduras	60.13	17.28	22.59
Mexico	53.04	11.35	35.60
Nicaragua	67.51	9.91	22.58
Panama	66.05	17.90	16.05
Paraguay	51.10	33.80	15.10
Peru	67.40	16.94	15.65
Uruguay	77.99	15.86	6.15
Dominican Republic	64.22	23.24	12.54
Venezuela	87.59	8.48	3.94
Total Average	66.01	16.32	17.66

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

Table 4: Support for Democracy and Support for Authoritarian Regimes: Alternative Survey Question 2010 (Figures in percentage %)

Country	Support Democracy ¹	Support Authoritarian ²
Argentina	68.83	26.50
Bolivia	74.17	18.75
Brazil	66.03	17.28
Colombia	58.42	34.33
Costa Rica	90.00	8.20
Chile	67.83	21.83
Ecuador	71.42	17.58
El Salvador	57.10	29.00
Guatemala	32.60	58.30
Honduras	47.30	38.20
Mexico	56.25	36.42
Nicaragua	70.10	17.50
Panama	70.40	23.80
Paraguay	40.08	53.00
Peru	48.00	42.42
Uruguay	71.92	22.00
Dominican Republic	70.20	24.60
Venezuela	69.92	22.42
Total Average	62.81	28.45

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

¹ You would not support under any circumstances a military government?

² Would you support a military government in replace of a democratic one if things get very bad?

Table 5: Evaluation of the Present Country Economy (Sociotropic Evaluation) 2010
(Figures in percentage %)

Country	Very Good	Good	Average	Bad	Very Bad	Total Positive	Total Negative
Argentina	1.00	15.58	46.42	28.58	8.33	16.58	36.91
Bolivia	0.42	10.33	56.67	23.67	6.83	10.75	30.50
Brazil	2.16	35.80	45.51	10.13	4.24	37.96	14.37
Colombia	0.67	11.58	58.08	22.50	6.92	12.25	29.42
Costa Rica	4.40	16.60	54.50	16.90	7.10	21.00	24.00
Chile	0.67	25.83	57.75	14.25	1.25	26.50	15.50
Ecuador	1.17	20.00	55.00	19.25	4.33	21.17	23.58
El Salvador	1.00	7.50	39.00	42.50	9.30	8.50	51.80
Guatemala	0.70	4.40	36.70	36.80	19.60	5.10	56.40
Honduras	4.90	8.00	26.40	36.00	23.60	12.90	59.60
Mexico	0.58	7.08	42.75	35.58	13.25	7.66	48.83
Nicaragua	1.20	7.40	42.30	32.90	14.70	8.60	47.60
Panama	10.30	14.90	51.80	16.20	5.90	25.20	22.10
Paraguay	1.33	14.92	44.25	29.33	9.58	16.25	38.91
Peru	0.92	9.17	60.00	24.17	5.17	10.09	29.34
Uruguay	2.92	32.75	49.33	12.33	1.83	35.67	14.16
Dominican Republic	2.20	4.60	27.40	38.20	27.40	6.80	65.60
Venezuela	1.75	19.17	45.17	23.00	10.58	20.92	33.58
Total Average	2.13	14.76	46.61	25.68	10.00	16.88	35.68

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

Table 6: Deviation in the Evaluations of the Present Country Economic from the Evaluation of the Last Military Government by Country 2010 (Figures in percentages%)

Country	Evaluation present economy better than last military government				Equal evaluations	Evaluation present economy worse than last military government				Total Negative Values	Total Positive Values
	-4	-3	-2	-1		0	1	2	3		
Argentina	0.74	7.69	20.11	25.58	24.00	12.97	6.49	2.22	0.19	54.12	21.87
Bolivia	0.11	3.26	18.49	27.52	32.56	13.34	3.99	0.74	0.00	49.38	18.07
Brazil	0.59	8.41	16.23	24.76	31.40	13.98	3.67	0.95	0.00	49.99	18.60
Colombia	0.00	0.36	2.75	6.81	22.22	38.23	24.01	4.90	0.72	9.92	67.86
Costa Rica	-	-	-	-	-	-	-	-	-	-	-
Chile	0.09	4.54	23.26	31.97	24.75	11.49	3.71	0.19	0.00	59.86	15.39
Ecuador	0.31	3.77	14.47	23.69	38.26	14.99	3.46	1.05	0.00	42.24	19.50
El Salvador	0.13	0.80	4.01	13.77	35.56	27.01	15.51	2.81	0.40	18.71	45.73
Guatemala	0.00	0.24	4.92	12.95	28.66	24.82	18.71	7.31	2.40	18.11	53.24
Honduras	0.15	1.06	3.32	13.44	26.28	21.15	21.30	10.42	2.87	17.97	55.74
Mexico	0.09	0.61	4.86	12.85	33.85	27.78	15.54	3.39	1.04	18.41	47.75
Nicaragua	0.28	0.83	8.12	17.88	36.04	20.50	12.93	3.03	0.41	27.11	36.87
Panama	0.77	7.01	18.29	26.07	29.57	11.61	4.27	1.86	0.55	52.14	18.29
Paraguay	0.19	2.26	6.04	13.68	29.81	23.87	17.36	5.57	1.23	22.17	48.03
Peru	0.24	1.18	7.07	16.73	38.63	23.79	9.89	2.24	0.24	25.22	36.16
Uruguay	1.61	17.24	24.60	22.08	20.16	10.69	2.52	0.91	0.20	65.53	14.32
Dominican Republic	0.58	1.51	10.70	22.67	35.23	18.60	7.79	1.74	1.16	35.46	29.29
Venezuela	0.23	2.49	12.78	26.24	35.97	14.71	6.00	1.36	0.23	41.74	22.30
Total Average	0.36	3.72	11.77	19.92	30.76	19.38	10.42	2.98	0.68	35.77	33.47

Source: Latinobarómetro.

Categories “No answer” and “Don’t know” are not reported in this table.

Table 7: Description of Variables

Variable	Description	Source
<i>Dependent variable</i>		
Support for Democracy 1 (Multinomial)	1 (Democracy is preferable to any other kind of government), 2 (Under some circumstances, an authoritarian government can be preferable to a democratic one), 3 (For people like me, it doesn't matter whether we have a democratic or non-democratic).	Latinobarómetro
Support for Democracy 1 (Alternative)	1 (You would not support under any circumstances a military government), 0 (You would support a military government in replace of a democratic one if things get very bad)	Latinobarómetro
<i>Main independent variable</i>		
Evaluation of Country Economy (under current democracy)	Scale from 1 (Very good) to 5 (Very bad)	Latinobarómetro
Evaluation of Last Military Government	Scale from 1 (Very good) to 5 (Very bad)	Latinobarómetro
Deviation in Evaluations	Scale from -4 (Democracy better than Last Military) to 4 (Last Military better than Democracy)	Estimated by authors
<i>Individual controls</i>		
Male	1 (Male), 0 (Female)	Latinobarómetro
Age	Years	Latinobarómetro
Marital status	1 (Married), 0 (Otherwise)	Latinobarómetro
Unemployed	1 (Unemployed), 0 (Otherwise)	Latinobarómetro
Years of education	Respondent's years of education	Latinobarómetro
Religion	1 (Catholic), 0 (Otherwise)	Latinobarómetro
Victim of a crime	1 (Has been victim of a crime), 0 (Otherwise)	Latinobarómetro
Ideology	Scale from 0 (Extreme left) to 10 (Extreme right)	Latinobarómetro
Liberal democrat	1 (Without a National Congress there can be no democracy), 0 (Democracy can work without a National Congress)	Latinobarómetro
<i>Country controls</i>		
Index of democracy	Scale from -10 (Failed State) to 10 (Full democracy)	Polity IV
Age of democracy	Years since 2010 until when democracy was recovered	Polity IV
Index of corruption	Scale from 0 (Highly corrupt) to 10 (Highly clean)	Transparency International
Inequality (Gini)	0 (Perfectly egalitarian), 100 (Perfectly unequal)	SEDLAC
GDP per capita	Real GDP per capita at constant 2005 national prices (in mil. 2005US\$)	Penn Tables 8.0

Table 8: Multinomial Models (Relative Risk Ratios)

Democracy/Authoritarian									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Country Economy Evaluation	0.931*** (0.024)	0.946* (0.032)	0.973 -0.0335	0.943* (0.032)					1.378*** (0.044)
Deviation in Evaluations (DiE_i)					0.728*** (0.018)	0.726*** (0.022)	0.750*** (0.023)	0.749*** (0.022)	0.665*** (0.029)
Democracy/Indifference									
Country Economy Evaluation	0.802*** (0.022)	0.824*** (0.030)	0.851*** (0.032)	0.814*** (0.030)					1.053 (0.043)
Difference in Evaluations (DiE_i)					0.774*** (0.018)	0.782*** (0.022)	0.795*** (0.024)	0.776*** (0.022)	0.781*** (0.029)
Authoritarian/Indifference									
Country Economy Evaluation	0.861*** (0.030)	0.871*** (0.039)	0.874*** (0.041)	0.863*** (0.039)					0.763*** (0.054)
Difference in Evaluations (DiE_i)					1.062*** (0.022)	1.075*** (0.027)	1.059** (0.029)	1.036 (0.027)	1.174*** (0.036)
Controls									
Social-Demographical		X	X	X		X	X	X	X
Political			X	X			X	X	X
Country variables				X				X	
Country fixed effects	X	X	X		X	X	X		X
Constant	X	X	X	X	X	X	X	X	X
N	21,060	11,810	10,918	10,918	14,600	9,354	8,733	8,733	8,733
Pseudo R Squared									

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis.

Table 9: Robustness check: Logit model using an alternative dependent variable¹ (Relative Risk Ratios)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Country									
Economy Evaluation	1.015	1.000	1.004	0.953*					1.986***
	(0.020)	(0.025)	(0.026)	(0.025)					(0.038)
Deviation in Evaluations (DiE_i)									
					0.605***	0.592***	0.596***	0.586***	0.459***
					(0.016)	(0.020)	(0.021)	(0.019)	(0.027)
Controls									
Social-Demographical		X	X	X		X	X	X	X
Political			X	X			X	X	X
Country variables				X				X	
Country fixed effects	X	X	X		X	X	X		X
Constant	2.469***	2.128***	1.839***	2.102	2.206***	1.694***	1.654***	0.111***	0.166***
	(0.092)	(0.161)	(0.175)	(0.480)	(0.068)	(0.154)	(0.170)	(0.604)	(0.216)
N	18,321	11,558	10,660	10,660	14,507	9,273	8,628	8,628	8,628
Pseudo R Squared									

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis.

¹ It takes on 1 whether respondents would not support a military government under any circumstance, and 0 whether they would support a military government in replace of a democratic government whether things get very bad.

Table 10: Description of the More Recent Authoritarian Periods According to Polity IV
Score

Country	Description	Last Dictatorship		Most Recent Democracy	
		Start	End	Start	End
Argentina	Military dictatorship “Proceso de Reorganización Nacional” (Jorge Videla, Emilio Massera and others)	1976	1983	1983	-
Bolivia	Several authoritative figures from the arrival of René Barrientos (A civilian with military support)	1964	1982	1982	-
Brazil	Military dictatorship of Humberto de Alencar Castelo Branco (1964-1967) and several civil and military governments afterwards.	1964	1985	1985	-
Chile	Military dictatorship of Augusto Pinochet	1973	1989	1989	-
Colombia	Military dictatorship of Gustavo Rojas Pinilla	1948	1957	1957	-
Dominican Republic	Joaquín Balaguer (aka “Los Doce Años”)	1966	1978	1978	-
Ecuador	Guillermo Rodríguez Lara (1972-1976) Military triumvirate composed of Alfredo Poveda Burbano, Guillermo Durán Arcentales and Luis Leoro Franco (1976-1979)	1972	1979	1979	-
El Salvador	Era of military authoritarianism (1931-1979). Dictatorship of Martínez (1931-1944) Era of the PRUD (1948-1960) Governments of PCN (1962-1979)	1931	1979	1984	-
Guatemala		1974	1985	1986	-
Honduras	Military government of Tiburcio Carías Andino (1933-1948) and several military Juntas afterwards.	1936	1980	1982	-
Mexico		1917	1988	1994	-
Nicaragua	Anastasio Somoza García and Anastasio Somoza Debayle (aka Somocismo)	1934	1979	1990	-
Panama	Military Junta (1968 a 1969), Omar Torrijos "El Proceso Revolucionario" (1969 a 1981), Rubén Darío Paredes (1981 a 1983), Manuel Antonio Noriega (1983-1989)	1968	1989	1989	-
Paraguay	Alfredo Stroessner (1954-1989)	1954	1989	1989	-
Peru	Alberto Fujimori	1992	2001	2001	-
Uruguay	Military Government	1973	1985	1985	-
Venezuela (1)	Marcos Pérez Jiménez (1945-1959)	1945	1959	1959	2009
Venezuela (2)	Hugo Chávez (2009-2013) Nicolás Maduro (2013 -)	2009			

Source: Polity IV

Table 11: Average of GDP per capita growth in recent democracy (2000-2009) versus last authoritarian regimes (Figures in percentages %)

Country	Δ GDP Democracy	Δ GDP Military	Difference
Argentina	1.55	1.59	-0.04
Bolivia	2.79	1.63	1.16
Brazil	1.03	3.46	-2.43
Colombia	2.35	1.18	1.16
Chile	3.13	0.10	3.03
Dominican Republic	3.33	4.00	-0.68
Ecuador	4.82	6.98	-2.15
El Salvador	1.06	1.81	-0.76
Guatemala	1.15	-0.08	1.23
Honduras	1.55	0.89	0.66
Mexico	1.47	2.11	-0.64
Nicaragua	3.00	4.53	-1.53
Panama	3.36	1.80	1.56
Paraguay	1.21	2.50	-1.29
Peru	5.16	4.08	1.08
Uruguay	0.42	-0.23	0.65
Venezuela	4.77	3.64	1.13
Total Average	2.48	2.35	0.13

Source: Penn Tables 8.0. Estimated by author.

Table 12: IV-Probit Model: The Effect of Deviation in Evaluations on Support for Democracy

	Univariate Probit	IV-Probit ¹	
	(1)	(2)	(3)
	Support for Democracy (dy/dx)	Deviation in Evaluations (dy/dx)	Support for Democracy (dy/dx)
Deviation in Evaluations (DiE_i)	-.045*** (.003)		-.154*** (.021)
Difference in Obj. Perform. (DiP_j)		-.257*** (.042)	
Controls			
Social-Demographical	X	X	X
Political	X	X	X
Country variables	X	X	X
n	7,307	7,307	7,307
Wald test of exogeneity ($\theta = 0$):			P-value=0.0008

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis.

¹ We use maximum likelihood estimator. Other estimators as Newey's two-step does not change the results (not reported).

Table 13: Robustness Checks: Other Specifications and Alternative Dependent Variables

	IV-Probit with Cluster Robust Standard Errors ¹		Alternative dependent variable (1) ²	Alternative dependent variable (2) ³
	(<i>dy/dx</i>)	(<i>dy/dx</i>)	(<i>dy/dx</i>)	(<i>dy/dx</i>)
	(1a)	(1b)	(2)	(3)
Deviation in Evaluations (<i>DiE_i</i>)		-.154*** (.033)	-.187*** (.008)	-.036 (.036)
Difference in Obj. Perform. (<i>DiP_j</i>)	-0.257* (.137)			
Controls				
Social-Demographical	X	X	X	X
Political	X	X	X	X
Country variables	X	X	X	X
n		7,307	8,733	8,836
Kleibergen-Paap rk LM statistic	3.495			
Wald test of exogeneity ($\theta = 0$)		4.68	35.74	0.00
		P-Value = 0.0305	P-Value = 0.0000	P-Value = 0.9537

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by region

¹ Clustered at region level.

² It takes on 1 whether respondents support democracy, and 0 whether they support authoritarian regimes or are political indifferent.

³ It takes on 1 whether respondents would not support a military government under any circumstance, and 0 whether they would support a military government in replace of a democratic government whether things get very bad.

Annex

Table 14: Multinomial Models: Democracy/Dictatorship (Log odds)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Country Economy Evaluation	-0.070***	-0.054*	-0.027	-0.058*	0.115						0.321***
	(0.024)	(0.032)	(0.033)	(0.032)	(0.093)						(0.044)
Country Economy Evaluation x Age					-0.003						
					(0.002)						
Difference in Evaluations (DiE_i)						-0.317***	-0.319***	-0.287***	-0.289***	-0.160**	-0.407***
						(0.018)	(0.022)	(0.023)	(0.022)	(0.062)	(0.029)
Difference in Evaluations (DiE_i) x Age										-0.003**	
										(0.001)	
Social-demographic controls											
Male		0.048	0.063	0.070	0.064		0.058	0.085	0.094	0.087	0.099
		(0.054)	(0.057)	(0.057)	(0.057)		(0.060)	(0.063)	(0.063)	(0.063)	(0.064)
Age		0.004**	0.004**	0.004**	0.017**		0.008***	0.008***	0.008***	0.009***	0.008***
		(0.002)	(0.002)	(0.002)	(0.007)		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Marital status		-0.051	-0.057	-0.039	-0.062		-0.080	-0.074	-0.050	-0.082	-0.078
		(0.055)	(0.058)	(0.057)	(0.058)		(0.061)	(0.064)	(0.063)	(0.064)	(0.064)
Unemployed		-0.117	-0.081	-0.044	-0.084		-0.106	-0.069	-0.059	-0.072	-0.085
		(0.092)	(0.097)	(0.096)	(0.097)		(0.107)	(0.113)	(0.111)	(0.113)	(0.114)
Years of education		0.007	0.010	0.006	0.010		0.008	0.010	0.012	0.010	0.011
		(0.006)	(0.007)	(0.006)	(0.007)		(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
Religion		0.082	0.103*	0.094	0.103*		0.083	0.102	0.103	0.101	0.094
		(0.058)	(0.062)	(0.059)	(0.062)		(0.065)	(0.069)	(0.067)	(0.069)	(0.069)
Victim of a crime		2.29E-04	0.045	0.063	0.044		-0.018	0.041	0.063	0.041	0.019
		(0.057)	(0.060)	(0.058)	(0.060)		(0.063)	(0.066)	(0.064)	(0.066)	(0.066)
Political controls											
Liberal democrat			0.909***	0.875***	0.909***			0.846***	0.843***	0.846***	0.840***
			(0.057)	(0.055)	(0.057)			(0.062)	(0.061)	(0.062)	(0.062)
Extreme Left			-0.008	0.035	-0.008			0.047	0.076	0.047	0.036
			(0.118)	(0.118)	(0.118)			(0.133)	(0.133)	(0.133)	(0.134)
Left			0.078	0.047	0.079			0.091	0.085	0.094	0.080
			(0.090)	(0.088)	(0.090)			(0.097)	(0.096)	(0.097)	(0.098)
Right			-0.014	-0.039	-0.014			-0.001	-0.036	-0.002	0.027
			(0.085)	(0.084)	(0.085)			(0.094)	(0.093)	(0.094)	(0.094)
Extreme Right			0.279***	0.239**	0.279***			0.303***	0.265**	0.310***	0.304***
			(0.107)	(0.104)	(0.107)			(0.117)	(0.114)	(0.117)	(0.118)
No ideology			0.187**	0.212***	0.190**			0.207**	0.202**	0.211**	0.172**
			(0.077)	(0.076)	(0.077)			(0.086)	(0.085)	(0.086)	(0.086)
Country controls											
Index of democracy				-0.071**					-0.116***		
				(0.030)					(0.034)		
Age of democracy				0.010***					0.016***		
				(0.003)					(0.004)		
Index of corruption				0.184***					0.163***		
				(0.032)					(0.033)		
GDP per capita average in democracy				0.170***					0.156***		
				(0.022)					(0.024)		
Inequality I democracy				-0.027***					-0.008		

	(0.010)				(0.012)						
Country											
Fixed effects											
Bolivia	0.772*** (0.127)	0.773*** (0.158)	0.964*** (0.169)	0.965*** (0.169)	0.674*** (0.135)	0.706*** (0.166)	0.842*** (0.176)	0.831*** (0.176)	0.864*** (0.176)		
Brazil	-0.238** (0.108)	-0.114 (0.135)	0.145 (0.140)	0.145 (0.140)	-0.382*** (0.118)	-0.255* (0.146)	-0.0656 (0.15)	-0.0731 (0.151)	0.0998 (0.152)		
Colombia	0.555*** (0.124)	0.567*** (0.158)	0.781*** (0.168)	0.782*** (0.168)	0.835*** (0.142)	0.821*** (0.179)	0.968*** (0.189)	0.949*** (0.189)	1.157*** (0.192)		
Costa Rica	0.547*** (0.124)	0.597*** (0.162)	0.666*** (0.166)	0.670*** (0.166)	0.484*** (0.125)	0.542*** (0.167)	0.553*** (0.175)	0.544*** (0.175)	0.665*** (0.176)		
Chile	0.493*** (0.119)	0.592*** (0.161)	0.613*** (0.167)	0.611*** (0.167)	0.238* (0.125)	0.329** (0.154)	0.616*** (0.161)	0.608*** (0.161)	0.750*** (0.163)		
Ecuador	0.432*** (0.118)	0.457*** (0.145)	0.765*** (0.151)	0.763*** (0.151)	0.00726 (0.125)	0.249 (0.166)	0.338* (0.173)	0.331* (0.173)	0.400** (0.173)		
El Salvador	-0.086 (0.113)	0.141 (0.153)	0.267* (0.159)	0.268* (0.159)	0.032 (0.127)	0.080 (0.159)	0.056 (0.169)	0.049 (0.169)	0.077 (0.170)		
Guatemala	-0.228* (0.117)	-0.246* (0.148)	-0.229 (0.157)	-0.225 (0.157)	0.266* (0.143)	0.416** (0.187)	0.377* (0.194)	0.380* (0.194)	0.439** (0.194)		
Honduras	0.0761 (0.120)	0.169 (0.159)	0.196 (0.167)	0.197 (0.166)	0.573*** (0.130)	0.633*** (0.166)	0.657*** (0.171)	0.651*** (0.171)	0.697*** (0.171)		
Mexico	0.323*** (0.124)	0.332** (0.158)	0.426*** (0.163)	0.431*** (0.163)	0.745*** (0.154)	0.808*** (0.202)	0.832*** (0.213)	0.818*** (0.213)	0.869*** (0.213)		
Nicaragua	0.734*** (0.140)	0.823*** (0.19)	0.889*** (0.200)	0.889*** (0.201)	-0.0239 (0.120)	0.0612 (0.159)	0.222 (0.167)	0.211 (0.167)	0.382** (0.166)		
Panama	0.0496 (0.115)	0.0866 (0.153)	0.281* (0.161)	0.285* (0.162)	-0.581*** (0.106)	-0.518*** (0.132)	-0.381*** (0.140)	-0.386*** (0.140)	-0.272* (0.142)		
Paraguay	-0.808*** (0.099)	-0.793*** (0.125)	-0.611*** (0.133)	-0.612*** (0.133)	0.289** (0.125)	0.322** (0.157)	0.382** (0.165)	0.378** (0.165)	0.491*** (0.166)		
Peru	0.142 (0.111)	0.103 (0.139)	0.203 (0.146)	0.206 (0.146)	0.108 (0.117)	0.0297 (0.147)	0.198 (0.153)	0.188 (0.153)	0.311** (0.154)		
Uruguay	0.329*** (0.111)	0.251* (0.140)	0.383*** (0.145)	0.382*** (0.145)	1.004*** (0.141)	1.107*** (0.188)	1.068*** (0.194)	1.060*** (0.195)	1.134*** (0.194)		
Venezuela	1.099*** (0.129)	1.144*** (0.172)	1.069*** (0.177)	1.067*** (0.177)	-0.217* (0.115)	-0.022 (0.150)	0.059 (0.156)	0.053 (0.156)	-0.069 (0.157)		
Dominican Republic	1.312*** (0.112)	-0.070 (0.140)	0.023 (0.146)	0.018 (0.146)	0.674*** (0.135)	0.706*** (0.166)	0.842*** (0.176)	0.831*** (0.176)	0.864*** (0.176)		
Constant	1.463*** (0.108)	1.105*** (0.199)	0.185 (0.216)	1.222* (0.664)	-0.27 (0.350)	1.137*** (0.0786)	0.644*** (0.184)	-0.115 (0.203)	0.0916 (0.778)	-0.127 (0.203)	-1.211*** (0.255)
n	21,060	11,810	10,918	10,918	10,918	14,600	9,354	8,733	8,733	8,733	

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors

Table 15: Multinomial Models: Democracy/Political Indifference (Log odds)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Country Economy Evaluation	-	-	-	-0.205***	-0.12						0.0515
	(0.220***)	(0.193***)	(0.161***)	(0.030)	(0.087)						(0.043)
Country Economy Evaluation x Age					-0.001						
					(0.002)						
Difference in Evaluations (DiE_i)						0.256***	0.245***	0.229***	-0.253***	-	-
						(0.018)	(0.022)	(0.024)	(0.022)	0.183***	0.247***
										(0.065)	(0.029)
Difference in Evaluations (DiE_i) x Age										-0.001	
										(0.001)	
Social-demographic controls											
Male		0.099*	0.059	0.046	0.059		0.114*	0.074	0.070	0.075	0.077
		(0.054)	(0.058)	(0.057)	(0.058)		(0.061)	(0.065)	(0.064)	(0.065)	(0.065)
Age		0.014***	0.015***	0.015***	0.019**		0.017***	0.018***	0.018***	0.018***	0.018***
		(0.002)	(0.002)	(0.002)	(0.007)		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Marital status		0.041	0.026	0.012	0.025		0.053	0.057	0.044	0.055	0.057
		(0.054)	(0.058)	(0.057)	(0.058)		(0.061)	(0.066)	(0.064)	(0.066)	(0.066)
Unemployed		0.384***	0.352***	-0.359***	0.353***		0.393***	0.331***	-0.354***	0.333***	0.335***
		(0.082)	(0.089)	(0.088)	(0.089)		(0.098)	(0.106)	(0.104)	(0.106)	(0.106)
Years of education		0.047***	0.044***	0.039***	0.044***		0.050***	0.048***	0.0492***	0.048***	0.048***
		(0.006)	(0.006)	(0.006)	(0.006)		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Religion		-0.022	-0.024	-0.094	-0.024		-0.094	-0.093	-0.166**	-0.093	-0.093
		(0.059)	(0.063)	(0.062)	(0.063)		(0.068)	(0.073)	(0.073)	(0.073)	(0.073)
Victim of a crime		-0.092*	-0.072	-0.072	-0.072		-0.034	-0.003	-0.014	-0.003	-0.008
		(0.056)	(0.059)	(0.058)	(0.059)		(0.063)	(0.067)	(0.065)	(0.067)	(0.067)
Political controls											
Liberal democrat			0.802***	0.812***	0.802***			0.813***	0.829***	0.813***	0.811***
			(0.057)	(0.055)	(0.057)			(0.062)	(0.064)	(0.064)	(0.064)
Extreme Left			0.108	0.104	0.108			0.116	0.098	0.115	0.116
			(0.123)	(0.121)	(0.123)			(0.137)	(0.134)	(0.137)	(0.137)
Left			0.146	0.13	0.147			0.199*	0.184*	0.199*	0.197*
			(0.092)	(0.091)	(0.092)			(0.102)	(0.101)	(0.102)	(0.102)
Right			0.220**	0.207**	0.220**			0.278***	0.272***	0.278***	0.282***
			(0.092)	(0.091)	(0.092)			(0.105)	(0.103)	(0.105)	(0.105)
Extreme Right			0.141	0.103	0.141			0.230**	0.198*	0.233**	0.229**
			(0.099)	(0.097)	(0.099)			(0.114)	(0.112)	(0.114)	(0.114)
No ideology			0.120	0.102	0.121			0.161*	0.138	0.162*	0.154*
			(0.075)	(0.073)	(0.075)			(0.085)	(0.084)	(0.085)	(0.085)
Country controls											
Index of democracy				0.052*					0.038		
				(0.028)					(0.031)		
Age of democracy				0.0051*					0.012***		
				(0.003)					(0.004)		
Index of corruption				-0.068**					-0.055*		
				(0.030)					(0.033)		
GDP per capita average in democracy				0.100***					0.078***		
				(0.020)					(0.021)		
Inequality in democracy				-					-0.042***		
				0.0781***					(0.011)		
				(0.009)							
Country Fixed effects											
Bolivia	-0.151	0.0579	0.144	0.144	0.144	-0.141	0.0167	0.110		0.106	0.116
	(0.121)	(0.155)	(0.164)	(0.164)	(0.164)	(0.132)	(0.165)	(0.175)		(0.175)	(0.175)
Brazil	-	-0.185	-0.034	-0.034	-0.034	0.523***	-0.187	-0.041		-0.044	-0.012
	0.498***	(0.156)	(0.166)	(0.166)	(0.166)	(0.134)	(0.170)	(0.180)		(0.181)	(0.183)
Colombia	-	-	-0.391**	-0.391**	-0.391**	0.377***	-0.246	-0.071		-0.077	-0.038
	0.736***	0.614***	(0.158)	(0.157)	(0.157)	(0.130)	(0.168)	(0.183)		(0.183)	(0.186)
Costa Rica	0.0516	0.192	0.222	0.222	0.222	0.403***	0.433***	0.446***		0.448***	-0.429**
	(0.131)	(0.168)	(0.175)	(0.175)	(0.175)	(0.122)	(0.161)	(0.169)		(0.169)	(0.169)
Chile	-	-	-	-	-	-	-0.314**	-0.023		-0.025	0.0001
	0.514***	0.583***	0.558***	0.558***	0.558***	0.492***	(0.157)	(0.164)		(0.164)	(0.166)
	(0.116)	(0.150)	(0.158)	(0.158)	(0.158)	(0.127)					
Ecuador	0.354***	-0.216	0.0468	0.0463	0.0463	-0.0443	0.133	0.312		0.31	0.324
	(0.119)	(0.148)	(0.155)	(0.155)	(0.155)	(0.146)	(0.188)	(0.204)		(0.204)	(0.204)
El Salvador	-0.149	0.113	0.266	0.266	0.266	0.812***	0.560***	0.565***		0.568***	0.559***
	(0.130)	(0.171)	(0.185)	(0.185)	(0.185)	(0.130)	(0.166)	(0.176)		(0.177)	(0.177)
Guatemala	-	-	-	-	-	-0.364**	0.0678	0.0116		0.012	0.0262
	0.945***	0.726***	0.743***	0.741***	0.741***	(0.144)	(0.196)	(0.204)		(0.204)	(0.205)
	(0.119)	(0.153)	(0.162)	(0.162)	(0.162)						
Honduras	-	-0.166	-0.171	-0.171	-0.171	1.047***	0.890***	0.806***		0.808***	0.796***
	0.566***	(0.164)	(0.173)	(0.173)	(0.173)	(0.117)	(0.149)	(0.156)		(0.156)	(0.156)
Mexico	-	-	-	-	-	-	-0.362**	-0.378**		-0.382**	-0.372**
	1.186***	1.081***	0.973***	0.972***	0.972***	0.496***	(0.177)	(0.187)		(0.187)	(0.187)
	(0.110)	(0.140)	(0.148)	(0.148)	(0.148)	(0.135)					

Nicaragua	-	-	-	-	-	-	-	-	-	-	-
	0.490***	-0.371**	-0.377**	-0.378**	-0.266**	-0.17	0.0361		0.0329	0.0607	
	(0.121)	(0.160)	(0.170)	(0.170)	(0.135)	(0.175)	(0.187)		(0.187)	(0.188)	
Panama	-0.306**	-0.241	-0.034	-0.033	-0.107	0.077	0.188		0.186	0.207	
	(0.128)	(0.167)	(0.178)	(0.178)	(0.137)	(0.173)	(0.181)		(0.181)	(0.182)	
Paraguay	-	-0.223	-0.0991	-0.099	-0.005	0.033	0.210		0.210	0.230	
	0.401***	(0.159)	(0.167)	(0.168)	(0.139)	(0.174)	(0.189)		(0.190)	(0.190)	
Peru	-0.19	-0.138	0.00256	0.0033	0.811***	0.819***	0.887***		0.885***	0.904***	
	(0.123)	(0.155)	(0.167)	(0.167)	(0.169)	(0.220)	(0.235)		(0.236)	(0.236)	
Uruguay	0.784***	0.907***	1.076***	1.076***	1.337***	1.438***	1.442***		1.439***	1.451***	
	(0.152)	(0.204)	(0.226)	(0.226)	(0.189)	(0.254)	(0.268)		(0.268)	(0.268)	
Venezuela	1.458***	1.573***	1.506***	1.505***	0.010	0.082	0.149		0.148	0.129	
	(0.175)	(0.237)	(0.248)	(0.248)	(0.143)	(0.173)	(0.178)		(0.178)	(0.178)	
Dominican Republic	0.625***	0.155	0.197	0.195	-0.141	0.016	0.110		0.106	0.116	
	(0.113)	(0.162)	(0.168)	(0.168)	(0.132)	(0.165)	(0.175)		(0.175)	(0.175)	
Constant	2.368***	1.172***	0.450**	3.939***	0.319	1.566***	0.331*	-0.353	1.112	-0.346	-0.533**
	(0.116)	(0.202)	(0.223)	(0.640)	(0.356)	(0.093)	(0.194)	(0.217)	(0.750)	(0.217)	(0.269)
n	21,060	11,810	10,918	10,918	10,918	14,600	9,354	8,733	8,733	8,733	8,733

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors

Table 16: Multinomial Models: Dictatorship/Political Indifference (Log odds)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Country Economy Evaluation	-	-	-	-	-0.235**						-
	0.149***	0.138***	0.134***	0.147***							0.270***
	(0.03)	(0.039)	(0.041)	(0.039)	(0.114)						(0.054)
Country Economy Evaluation x Age					0.002						
					(0.002)						
Difference in Evaluations (DiE_i)						0.061***	0.073***	0.058**	0.036	-0.022	0.160***
						(0.022)	(0.027)	(0.029)	(0.027)	(0.077)	(0.036)
Difference in Evaluations (DiE_i) x Age										0.002	
										(0.001)	
Social-demographic controls											
Male		0.0502	-0.004	-0.023	-0.005		0.055	-0.011	-0.024	-0.012	-0.022
		(0.068)	(0.072)	(0.071)	(0.072)		(0.076)	(0.080)	(0.078)	(0.080)	(0.080)
Age		0.010***	0.010***	0.010***	0.002		0.008***	0.009***	0.009***	0.008***	0.009***
		(0.002)	(0.002)	(0.002)	(0.009)		(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Marital status		0.092	0.083	0.052	0.087		0.134*	0.132	0.095	0.137*	0.135*
		(0.069)	(0.073)	(0.072)	(0.073)		(0.076)	(0.080)	(0.079)	(0.080)	(0.080)
Unemployed		-0.267**	-0.271**	-	-0.269**		-0.287**	-0.261**	-0.295**	-0.260**	-0.250*
		(0.109)	(0.115)	(0.112)	(0.115)		(0.125)	(0.131)	(0.127)	(0.131)	(0.132)
Years of education		0.040***	0.033***	0.033***	0.033***		0.041***	0.037***	0.037***	0.037***	0.037***
		(0.008)	(0.008)	(0.008)	(0.008)		(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
Religion		-0.104	-0.128	-0.189**	-0.128		-0.178**	-0.195**	-	0.269***	-0.194**
		(0.074)	(0.078)	(0.076)	(0.078)		(0.083)	(0.088)	(0.085)	(0.088)	(0.088)
Victim of a crime		-0.092	-0.116	-0.136*	-0.117		-0.016	-0.045	-0.078	-0.045	-0.027
		(0.071)	(0.075)	(0.072)	(0.075)		(0.078)	(0.082)	(0.079)	(0.082)	(0.082)
Political controls											
Liberal democrat			-0.108	-0.064	-0.108			-0.033	-0.013	-0.032	-0.028
			(0.071)	(0.069)	(0.071)			(0.077)	(0.075)	(0.078)	(0.078)
Extreme Left			0.116	0.068	0.117			0.069	0.021	0.068	0.079
			(0.152)	(0.150)	(0.152)			(0.166)	(0.165)	(0.166)	(0.167)
Left			0.068	0.082	0.067			0.107	0.099	0.105	0.117
			(0.115)	(0.113)	(0.115)			(0.124)	(0.122)	(0.124)	(0.125)
Right			0.235**	0.247**	0.235**			0.279**	0.308**	0.281**	0.255**
			(0.113)	(0.110)	(0.113)			(0.124)	(0.121)	(0.124)	(0.124)
Extreme Right			-0.138	-0.136	-0.138			-0.072	-0.067	-0.077	-0.074
			(0.131)	(0.127)	(0.131)			(0.142)	(0.138)	(0.142)	(0.143)
No ideology			-0.067	-0.11	-0.069			-0.046	-0.063	-0.049	-0.018
			(0.096)	(0.093)	(0.096)			(0.107)	(0.105)	(0.107)	(0.107)
Country controls											
Index of democracy				0.124***					0.155***		
				(0.036)					(0.040)		
Age of democracy				-0.005					-0.004		
				(0.003)					(0.005)		
Index of corruption					0.253***					0.218***	
					(0.040)					(0.041)	
GDP per capita average in democracy											0.077***
										(0.028)	
Inequality I democracy					0.051***					-0.034**	
					(0.012)					(0.015)	
Country Fixed effects											
Bolivia											
	0.923***	0.715***	0.820***		0.821***	0.815***	0.689***	0.732***		0.724***	0.748***
	(0.160)	(0.202)	(0.216)		(0.216)	(0.173)	(0.214)	(0.226)		(0.226)	(0.226)
Brazil											
	-0.260*	-0.071	-0.179		-0.179	-0.142	0.068	0.024		0.028	-0.112
	(0.144)	(0.185)	(0.194)		(0.194)	(0.158)	(0.202)	(0.210)		(0.210)	(0.213)
Colombia											
	1.291***	1.181***	1.172***		1.173***	1.212***	1.067***	1.039***		1.027***	1.196***
	(0.151)	(0.194)	(0.209)		(0.209)	(0.173)	(0.222)	(0.236)		(0.236)	(0.240)
Costa Rica											
	0.495***	-0.405*	-0.444**		-0.447**	0.887***	0.974***	0.999***		0.993***	1.093***
	(0.165)	(0.213)	(0.220)		(0.220)	(0.161)	(0.214)	(0.223)		(0.223)	(0.225)
Chile											
	1.006***	1.175***	1.171***		1.169***	0.730***	0.643***	0.640***		0.633***	0.750***
	(0.150)	(0.200)	(0.209)		(0.209)	(0.160)	(0.198)	(0.207)		(0.207)	(0.210)
Ecuador											
	0.786***	0.674***	0.718***		0.717***	-0.0516	-0.116	-0.0264		-0.0214	-0.0764
	(0.152)	(0.188)	(0.197)		(0.197)	(0.170)	(0.224)	(0.236)		(0.236)	(0.237)
El Salvador											
	-0.062	-0.0278	-0.00122		-0.0024	0.844***	0.641***	0.622***		0.617***	0.637***
	(0.154)	(0.207)	(0.220)		(0.220)	(0.158)	(0.201)	(0.210)		(0.210)	(0.211)
Guatemala											
	0.718***	0.480***	0.514***		0.516***	0.630***	-0.348	-0.366		-0.368	-0.413*
	(0.145)	(0.186)	(0.195)		(0.195)	(0.176)	(0.236)	(0.246)		(0.246)	(0.247)
Honduras											
	0.643***	-0.334	-0.367*		-0.369*	1.620***	1.523***	1.462***		1.459***	1.493***
	(0.151)	(0.203)	(0.214)		(0.214)	(0.156)	(0.199)	(0.205)		(0.206)	(0.207)
Mexico											
	1.509***	1.412***	1.399***		1.403***	1.241***	1.170***	1.210***		1.200***	1.241***
	(0.147)	(0.188)	(0.195)		(0.196)	(0.184)	(0.241)	(0.253)		(0.253)	(0.253)

Nicaragua	-	-	-	-	-0.242	-0.231	-0.186		-0.178	-0.321
	1.224***	1.194***	1.267***	1.266***	(0.162)	(0.212)	(0.224)		(0.224)	(0.225)
	(0.168)	(0.226)	(0.238)	(0.238)	0.475***	0.596***	0.569***		0.573***	0.479**
Panama	-0.356**	-0.328	-0.316	-0.319	(0.153)	(0.195)	(0.204)		(0.204)	(0.206)
	(0.153)	(0.203)	(0.215)	(0.215)	-0.294*	-0.289	-0.172		-0.168	-0.26
Paraguay	0.407***	0.569***	0.512***	0.512***	(0.190)	(0.169)	(0.212)		(0.226)	(0.228)
	(0.140)	(0.179)	(0.190)	(0.190)	0.703***	0.789***	0.690***		0.696***	0.593**
Peru	-0.332**	-0.241	-0.201	-0.202	(0.193)	(0.248)	(0.262)		(0.262)	(0.263)
	(0.149)	(0.188)	(0.199)	(0.199)	0.333	0.331	0.374		0.380	0.317
Uruguay	0.454***	0.656***	0.694***	0.694***	(0.224)	(0.301)	(0.315)		(0.315)	(0.315)
	(0.175)	(0.230)	(0.252)	(0.252)	0.438	0.228	0.105		0.089	0.199
Venezuela	0.359*	0.429	0.437	0.438	(0.164)	(0.203)	(0.210)		(0.210)	(0.210)
	(0.207)	(0.280)	(0.291)	(0.291)	-	-	-		-	-
Dominican Republic	0.687***	0.225	0.173	0.176	0.815***	0.689***	0.732***		0.724***	0.748***
	(0.148)	(0.192)	(0.199)	(0.199)	(0.173)	(0.214)	(0.226)		(0.226)	(0.226)
Constant	0.905***	0.067	0.266	2.717***	0.589	0.429***	-0.313	-0.239	1.02	-0.219
	(0.143)	(0.255)	(0.278)	(0.831)	(0.446)	(0.112)	(0.238)	(0.262)	(0.964)	(0.263)
n	21,060	11,810	10,918	10,918	10,918	14,600	9,354	8,733	8,733	8,733

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors

Table 17: The effect of the evaluation of the last military government on support for democracy

	Univariate Probit	IV-Probit ¹	
	(1)	(2)	(3)
	Support for Democracy (dy/dx)	Difference in Evaluations (dy/dx)	Support for Democracy (dy/dx)
Deviation in Evaluations (DiE_i)	-0.045*** (0.003)		-0.154*** (0.021)
Difference in Obj. Perform. (DiP_j)		-0.257*** (0.042)	
<i>Individual level controls</i>			
Male	0.017* (0.009)	-0.093** (0.034)	0.001 (0.009)
Age	0.001*** (0.0003)	0.001 (0.001)	0.001*** (0.0003)
Marital status	-0.009 (0.009)	-0.034 (0.034)	-0.011 (0.008)
Unemployed	-0.012 (0.017)	0.211*** (0.061)	0.015 (0.016)
Years of education	0.002** (0.001)	-0.020*** (0.003)	-0.0008 (0.001)
Religion	0.023** (0.010)	0.033 (0.037)	0.022** (0.009)
Victim of a crime	0.006 (0.009)	0.091** (0.034)	0.018** (0.008)
Liberal democrat	0.125*** (0.009)	-0.268*** (0.034)	0.065** (0.021)
Extreme Left	0.015 (0.019)	-0.182** (0.072)	-0.009 (0.018)
Left	0.012 (0.014)	-0.118** (0.051)	-0.004 (0.013)
Right	-0.007 (0.014)	0.168*** (0.050)	0.015 (0.014)
Extreme Right	0.038** (0.017)	0.301*** (0.062)	0.065*** (0.015)
No ideology	0.035** (0.012)	-0.049 (0.043)	0.022* (0.012)
<i>Country level controls</i>			
Index of democracy	-0.019*** (0.004)	-0.326*** (0.019)	-0.047*** (0.006)
Age of democracy	0.002*** (0.0005)	-0.011*** (0.002)	0.0006 (0.0007)
Index of corruption	0.021*** (0.004)	0.101*** (0.022)	0.019*** (0.004)
Inequality	-0.107 (0.100)	2.340*** (0.331)	0.161 (0.111)
n	7,307	7,307	7,307
Wald test of exogeneity ($\text{athrho} = 0$):			P-value=0.0008

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis.

¹ We use maximum likelihood estimator. Other estimators as Newey's two-step does not change the results (not reported).

Table 18: Robustness Checks: Other Specifications and Alternative Dependent Variables

	IV-Probit with Cluster Robust Standard Errors ¹		Alternative Dependent Variable (1) ²	Alternative Dependent Variable (2) ³
	(<i>dy/dx</i>)	(<i>dy/dx</i>)	(<i>dy/dx</i>)	(<i>dy/dx</i>)
	(1a)	(1b)	(2)	(3)
Difference in Evaluations (<i>DiE_i</i>)		-0.154*** (0.033)	-0.187*** (0.008)	-0.036 (0.036)
Difference in Obj. Perform. (<i>DiP_j</i>)	-0.257* (0.137)			
<i>Individual level</i>				
Male	-0.093** (0.034)	0.001 (0.010)	-0.005 (0.007)	0.002 (0.009)
Age	0.001 (0.001)	0.001*** (0.0003)	0.001*** (0.0003)	0.001** (0.0003)
Marital status	-0.034 (0.034)	-0.011 (0.008)	-0.005 (0.007)	0.015* (0.008)
Unemployed	0.211*** (0.066)	0.015 (0.019)	0.013 (0.014)	0.021 (0.017)
Years of education	-0.020** (0.006)	-0.001 (0.001)	0.001 (0.001)	0.004*** (0.001)
Religion	0.033 (0.048)	0.022** (0.011)	0.009 (0.008)	0.001 (0.009)
Victim of a crime	0.091* (0.051)	0.018* (0.010)	0.017** (0.007)	-0.043*** (0.009)
Liberal democrat	-0.268*** (0.052)	0.065** (0.029)	0.054** (0.017)	0.062*** (0.012)
Extreme Left	-0.182** (0.111)	-0.009 (0.023)	-0.009 (0.015)	-0.010 (0.017)
Left	-0.118** (0.073)	-0.004 (0.015)	-0.004 (0.011)	0.026* (0.013)
Right	0.168** (0.057)	0.015 (0.019)	0.032** (0.011)	0.044** (0.014)
Extreme Right	0.301*** (0.090)	0.065*** (0.020)	0.069*** (0.013)	0.005 (0.017)
No ideology	-0.049 (0.065)	0.022 (0.016)	0.013 (0.010)	0.006 (0.010)
<i>Country level</i>				
Index of democracy	-0.326*** (0.050)	-0.047*** (0.011)	-0.048*** (0.004)	0.019* (0.010)
Age of democracy	-0.011* (0.006)	0.001 (0.001)	0.0001 (0.0005)	0.003*** (0.0006)
Index of corruption	0.101 (0.063)	0.019** (0.007)	0.011** (0.003)	0.003 (0.004)
Inequality	2.340** (1.123)	0.161 (0.188)	0.046 (0.099)	0.072 (0.116)
n		7,307	8,733	8,836
Kleibergen-Paap rk LM statistic	3.495			
Wald test of exogeneity ($\theta = 0$)		4.68 P-Value = 0.0305	35.74 P-Value = 0.0000	0.00 P-Value = 0.9537

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by region

¹ Clustered at region level.

² It takes on 1 whether respondents support democracy, and 0 whether they support authoritarian regimes or are political indifferent.

³ It takes on 1 whether respondents would not support a military government under any circumstance, and 0 whether they would support a military government in replace of a democratic government whether things get very bad.

Chapter 2

Television Bias and Electoral Results in Catalonia

Abstract

To what extent can the electoral results in Catalonia be explained by the exposure of individuals to television? This paper sheds light on this question by drawing on a natural experiment based on the geographically differentiated expansion of the public channel TV3 in Catalonia in the early eighties. Using a Difference-in-Differences Kernel matching method, we found that the introduction of TV3 caused an increase in the voter turnout as well as the *Convergència i Unió* vote share in the 1984 Catalan parliamentary elections, political coalition that has mostly managed the channel since its foundation and has been one of the strongest Catalan nationalist forces in Catalonia.

Key Words: Media, Voting behavior, Catalonia, Elections, Natural experiment

JEL: L82, D72, C99

1. Introduction

The literature on the political economy of media has grown rapidly and several excellent surveys have already been written on this topic (DellaVigna & Gentzkow 2010; Prat & Strömberg 2013; Sobbrío 2014; Strömberg 2015). Nevertheless, most of the empirical research have thus far focused on highly consolidated democracies (e.g. the USA and Scandinavian countries) and some formerly authoritarian countries (e.g. Russia and East Germany). As Sobbrío (2014) claims, additional academic contributions are needed to provide empirical evidence on the effects of media in less explored institutional settings. The Catalonia's political context characterized by a long-standing secessionist conflict with Spain is a newfangled case study to analyze the effect of a sub-national media such as TV3, which has an alleged pro-independence bias, on political outcomes. This paper therefore contributes to the existing literature on the effect of media on politics by identifying to what extent voting results in Catalonia can be explained by exposure of individuals to TV3.

In recent years, TV3 has been the subject of much controversy for its alleged support for the Catalan secessionist movement and strong partisan leaning, which has seemingly favored the political coalition *Convergència i Unió* (CiU). The CiU was a federation of two political parties of Catalan nationalist ideology, created in 1978 and dissolved in 2015, that played a crucial role in the creation of the channel in the early 80s as well as its direction since then¹.

Given this increased political tension in which TV3 seems to play a main role, it raises the question of to what extent voting results in Catalonia can be explained by the exposure of individuals to this channel. Providing empirical support to the effect of the channel over the last decade political tension in Catalonia is still a challenge, due to the fact that the whole region has been covered by TV3 since the mid-80s, thereby impeding to sort out the self-selection problem. However, there is the possibility of exploiting a natural experiment exploiting the geographically differentiated expansion of the channel in the early 80s. Although it is true that the secessionist movement has been more vehement in the last decade, the effect of TV3 on Catalan politics may be traced back to the start of the channel.

¹ The controversy about the TV3 bias has been amply covered by both Spanish and international media. For instance, see the article "Catalan TV Network Reflects Separatist Fervor" published by *The Wall Street Journal* on 8 January 2014.

In fact, it is noteworthy that the emergence of TV3 in 1983 -that is, in the middle of the first two Catalan Parliamentary elections (1980 and 1984) after 40 years of dictatorship- precisely coincided with the consolidation of the CiU as the strongest political force in the region. The number of votes for the CiU rose from 754,448 in 1980 to 1,345,513 in 1984, which represents an outstanding increment in the vote share from 27.83% to 46.80% (see Column 1 in Table 1). Admittedly, the increment in the CiU vote share could have only been a consequence of the consolidation of the Catalan party system, which had been non-existing until then. Nonetheless, it is the CiU the one who shows the greatest vote share increase, which leads to suspect about a possible effect of this channel on the individuals' electoral behavior, and thus raises the need of closer scrutiny.

This paper looks into the effect of TV3 on the electoral results of the 1984 Catalan Parliamentary election, which allows us to exploit a natural experiment based on the geographically differentiated timing in the entry of TV3 during 1983 and 1984. In particular, we analyze the effect of the TV3 availability at municipality level on two political outcomes of interest: voter turnout and the CiU vote share. To do so, we implement a standard two-periods Difference-in-Differences (DD) model with Propensity Score Matching (PSM). Additionally, we examine the impact of the TV3 availability on other political parties competing in the same election, we analyze the duration treatment effect of the channel on voter turnout and the CiU vote share, and we estimate the persuasion rates.

As we will document later, although the Catalan media system intended to reach the whole region in a short timeframe, exogenous technical and logistical constraints presumably caused a delay in the *timing* of the TV3 introduction across municipalities. This implies that TV3 is plausibly exogenous to political attitudes. This delay, however, did not necessarily change the *ordering*; that is, the initial assignation to TV3 is not random, but it responded to an expansion of the channel from the center (Barcelona) to the periphery in accordance to the availability of broadcast centers. Thus, the identifying assumption is that the geographically differentiated timing of the TV3 entry is unrelated to other factors that influence political outcomes once we take into account relevant initial conditions of TV3 placement.

We found that the introduction of TV3 caused an increase both in the voter turnout and the CiU vote share in the 1984 Catalan parliamentary elections. Specifically, we found that municipalities exposed to TV3 present a higher change both in voter turnout and the CiU vote share between 1980 and 1984. For the former the effect is about 5.3 percentage points on average, whereas for the latter is about 9.9 percentage points. These results are robust to several econometric specifications and two distinct placebo tests.

The rest of the paper is organized as follows. Section 2 presents a brief case contextualization. Section 3 reviews some theoretical and empirical literature on media, voting turnout and political persuasion, and we pose the working hypotheses. Section 4 presents the available data. Section 5 explains the empirical strategy. Section 6 shows the main econometric results. Finally, Section 7 presents some final remarks and policy implications.

2. The Timing of the TV3 Entry

Once the Franco dictatorship ended in 1975, Catalan society started a long recovery process of its identity and language. Jordi Pujol, leader of the CiU and president of the Catalan government from 1980 until 2003, promoted two laws so that the Catalan language started to be used in all region within the shortest possible timeframe: the Law on Language Normalization, that regulates the Catalan in teaching², and the Law that created the Catalan Corporation of Radio and Television (Hierro 2012: 90). The idea of implementing a Catalan language normalization policy and the creation of media system did not only come from the CiU. With some nuances, this aim was also shared by other nationalist parties in Catalonia, however the CiU was the one that ultimately led this project and founded the Corporation in 1983. Since then, this political coalition has mainly managed the Catalan media.

Although the CiU wanted to take TV3 to whole Catalonia -and even to other Catalan-speaking regions out of Catalonia- in the shortest time possible, the first broadcasting, which took place on 10 September 1983 through the Tibidabo broadcast center, only reached out Barcelona (the capital city) and some surrounding districts. Subject to the economic and

² See Clots-Figueras & Masella (2013) and Woolard et al. (1990) about the effect of teaching in Catalan on the nationalist sentiment.

technical constraints faced by a project of this magnitude³, the remainder of Catalonia was gradually covered during the next two years by setting new broadcasts and transposer centers.

Drawing on information provided by Montero (1987) and a variety of newspaper articles by *La Vanguardia* from 1983 to 1985 on the TV3 coverage (Table 2), we identify the timing of the TV3 entry in the municipalities of Catalonia. TV3 set three important broadcast centers before April 1984, the Catalan parliamentary election month: Alpocat on 31 December 1983 to cover the districts of Garrigues, Noguera, Segarra, Segrià, and Urgell. Rocaborda on 16 January 1984 to cover Alt Empordà, Baix Empordà, Gironès, and Selva. And La Mussara also on 16 January 1984 to cover Alt Camp, Baix Camp, and Tarragonès. Furthermore, TV3 set some transposers centers in order to cover some shadows in these areas.

By April 1984 Parliamentary elections, some important broadcast centers and transposer centers had not yet been installed: *Montcaro*, which would cover Ribera d'Ebre, Terra Alta, Baix Ebre, and Montsià in the Tarragona province, and the transposers center *Pic del O'rri i Vaqueira*, which would cover the Pirineos area: Val D'Aran, Pallars Sobirà, Pallars Jussà, and Alt Urgell. Moreover, many other transposer centers were set to cover shadows areas.

To sum up, by April 1984 parliamentary elections, 67% (635 out of 941) of municipalities and 92% of population (or 77% excluding Barcelona) was covered by TV3 (Figure 1).

<Figure 1 about here>

3. Theoretical Arguments and Hypotheses

3.1. Media and Voter Turnout

Why could voter turnout be affected by media? The role of media is intrinsically providing information, thus there are at least three distinct theoretical perspectives that could explain this relationship. Firstly, the basic Downs (1957) model states that a voter, in deciding whether or not to vote, calculates the expected utility of either action and votes if benefits exceed costs. Part of these costs are related to obtaining information about the candidates and

³ See Table 2 for pieces of news about the difficulties faced by TV3 to cover the whole territory.

policies (Geys 2006a: 18). Then, it would be expected that the entry of a new media reduces these costs by making voting less costly. Nevertheless, this rational view is not enough to explain voter turnout since the benefits of voting are very close to zero (in part because the probability of affecting the outcome is almost non-existent).

Secondly, a Downs complementary perspective suggests that the decision to vote does not solely lie on a rational choice, but also individuals can see voting as a “civic duty” and thus feel morally obliged to do so (Geys 2006a: 19; Geys 2006b: 648). From this perspective, media, especially when it comes to public media, may enlarge the feeling of civic duty and thus make voting more feasible (Sørensen 2015).

And thirdly, both decision-theoretical (Matusaka 1995) and game-theoretical models (Feddersen & Pesendorfer 1996; Feddersen & Pesendorfer 1997) suggest that better informed individuals are more likely to turnout. Matusaka (1995: 93) argues that “the value of changing the election outcome is higher when the voter is more confident that she is voting for the right candidate”. Feddersen & Pesendorfer (1996; 1997), employing a game-theoretic model, found that uninformed voters have an incentive to abstain and to delegate their vote to those who are better informed. Nevertheless, as Geys (2006a: 25) points out, the central problem of these “information models” is that they cannot explain the mere existence of voter turnout as they assume predisposition to vote in order to achieve positive turnout levels.

Either way, the empirical evidence brings up a different perspective: the effect depends upon the content of the media, which might lead to either a positive or negative effect on voter turnout. On the one hand, some empirical studies find a positive correlation between voters’ information levels and turnout, whether this information is provided by radio (Strömberg 2004), newspapers (Snyder & Strömberg 2010; Gentzkow et al. 2011; Drago et al. 2013; or television (Oberholzer-Gee & Waldfogel 2010; Sørensen 2015). On the other hand, some contributions point out that an increase in the supply of some media may lead to a negative effect on electoral participation due to a *crowding-out* effect on the existing (and more informative) media (Gentzkow 2006; George & Waldfogel 2008).

The ambiguous effect of media on voter turnout leads to ask what was the TV3 effect (if there was) in Catalonia by the 1984 parliamentary election. As explained in the previous

section, given the context in which TV3 emerged, its political implications as well as the good response of people to the channel, we firstly hypothesize that:

H1: The exposure to TV3 led to an increase in voter turnout in the 1983 Catalan parliamentary election.

3.2. Media Bias and Political Persuasion

Theoretically speaking, the existing models of persuasion effects can be divided broadly into two categories (DellaVigna & Kaplan 2008: 81). The first captures rational learning and predicts that exposure to the media may have an impact on beliefs and voting only in the short-run since voters, sooner or later, are able to filter out any bias provided by media. The second channel captures non-rational persuasion and implies that exposure to the media may affect beliefs and voting also in the long-run. Then, understanding the media impact on voting behavior is of interest not only for politics, but also for models of belief updating, and, from a policy perspective, if media bias modifies voting behavior, deregulation of media markets might have a large impact on political outcomes.

The general picture emerging from the empirical literature is that media bias has indeed an effect on political views. Some outstanding cases in the literature are the impact of FOX News in the voting for the Republican party in the US (Albertson & Lawrence 2009; DellaVigna & Kaplan 2007); the impact of public television in Russia (White et al. 2005) and an independent TV channel also in Russia (Enikolopov et al. 2011); the effect of West German television in East Germany attitudes toward authoritarian regimes (Kern & Hainmueller 2009); and the entry of digital TV on the support for Silvio Berlusconi in Italy (Barone et al. 2012). Regarding newspapers, Gerber et al. (2009) also find an effect of *The Washington Post* (a left-leaning newspaper) and the *The Washington Times* (a right-leaning newspaper) on self-reported voting.

With regards to the case of Catalonia, Hierro (2012: 160) draws on a panel data in order to identify whether TV3 induced changes in national identity from 2004 to 2005 examining whether those who self-identified as both Spanish and Catalan in 2004 are more likely to self-identify more in one way or another in 2005 -that is, shaping a clearer national identity- as a result of the exposure to TV3 or TVE. The author does not find any effect of media

exposure on national identity, however this study fails at providing a proper identification strategy.

To sum up, this survey about political persuasion shows that there is still so much to investigate about the influence of TV3 in the Catalan political context. Also, as explained in the case contextualization (Section 2), this case study fits quite well to the idea that the alleged partisan leaning of TV3, as other cases reviewed in this section, could have had some positive effect on the CiU electoral results. Thus, we pose the following second hypothesis:

H2: The exposure to TV3 led to an increase in the CiU vote share in the 1984 Catalan parliamentary election.

4. Data

4.1. Dependent Variables

The main dependent variables are the change in voter turnout and the change in the CiU vote share between the 1980 and 1984 Catalan Parliamentary elections at municipality level. Voter turnout is defined in this study as the share of population that has cast its vote with respect to the total number of people registered to vote, and the CiU vote share is the number of votes for CiU with respect to the total number of votes. Additionally, we consider that is important to analyze the effect of TV3 on the vote share of PSC, PP, and IC, other three political parties in Catalonia that showed notable changes in votes share during this period. By doing so, we expect to bear out if the channel indeed only affected the CiU vote share or, instead, it had an effect on others. The information source is the Statistical Institute of Catalonia (Idescat).

4.2. Treatment Variable

The availability of TV3 in April 1984 is, in the jargon of impact evaluation, the treatment variable of this study. Drawing on the information from Montero (1987) and La Vanguardia newspaper (Table 2) presented in the preceding section (Figure 1), we code as 1 whether the municipality was covered by TV3 before April 1984 (month of the 1984 Catalan parliamentary election), and 0 otherwise. Furthermore, in order to carry out a duration treatment analysis as well as check the validity of the empirical strategy by including placebo treatments, we also code three treatment variables more: i) Duration: using just treated

municipalities, we code as 1 if the municipality i was exposed to TV3 between 4 and 8 months, and 0 if less than 4 months; ii) First placebo: The availability of TV3 in December 1985; iii) Second placebo: A fake treatment randomly assigned on the untreated municipalities.

4.3. Controls

We collect a set of variables from Idescat for both matching initial conditions that determine TV3 placement and control for time-variant variables in the DD model. These are: i) Total population in 1981 and 1984; ii) Share of men with respect to total population in 1981 and 1984; iii) Share of non-native people with respect to total population in 1981 and 1986; iv) Share of people who speak Catalan with respect to total population in 1981 and 1986; v) Share of people that get BUP-COU⁴ with respect to total population in 1981 and 1986; vi) Share of people that get a medium graduate with respect to total population in 1981 and 1986; vii) Share of people that have a professional diploma with respect to total population in 1981 and 1986; ix) the distance from municipality i to Barcelona; and x) Financial institutions in 1981 and 1984 in the municipalities. Note that although the analysis in this study focuses on the elections results from 1980 to 1984, the control variables of total population and share of men are just available for the period 1981-1984, and variables related to education and Catalan speaking knowledge are for the years 1981 and 1986. The reasoning of the inclusion of these co-variables in the econometric analysis is theoretically justified in the next section. Table 3 in the annex presents some summary statistics.

5. Empirical Strategy

We consider the impact of the TV3 entry on two political outcomes related to electoral results of the Catalan parliamentary elections: the change in voter turnout and the change in the CiU vote share between 1980 and 1984 at municipality level. To do so, we resort to a natural experiment based on the geographically differentiated expansion of TV3 in Catalonia. This strategy exploits the timing of the entry described in the Section 2. The key identifying assumption is that the TV3 exposure is plausibly exogenous to political attitudes because the

⁴ *Bachillerato Unificado Polivalente* (BUP) and *Curso de Orientación Universitaria* (COU) correspond to the four-year studies previous to higher education, i.e. the High School in some anglo-saxon countries.

delay in the channel entry was mainly caused by logistical issues. However, we still have to take into account some municipalities initial conditions that plausibly determined the order of assignation to TV3.

In particular, we combine a standard two-periods Difference-in-Differences (DD) methodology with Propensity Score Matching (PSM) in that it compares the change over time (first difference) for the municipalities exposed to TV3 channel versus those “comparable” non-exposed used as a control group (second difference). In particular, the DD strategy can be summarized in the following table.

<Table 4 about here>

Column 1 in Table 4 shows that the difference between y_2^T and y_1^T captures the effect of TV3 on the outcome variable plus any other effect. Also, column 2 displays that the difference between y_2^{NT} and y_1^{NT} captures any other effect than TV3. Thus, the difference of these two differences (Column 3) captures just the effect of TV3 on the outcome variable, that is the causal effect.

To address the concerns about the municipalities initial conditions in the assignation to TV3, we combine the DD estimator with PSM, as proposed by (Heckman, Ichimura, & Todd, 1997). The estimated probability of participation (or propensity score) can be used to match participant and control units in the base (pre-program) year, and the treatment impact is calculated across participant and matched control units within the common support (Khandker et al. 2010: 80). Then, in order to obtain the propensity scores needed to define the common support and match municipalities, we first implement a Logit model of the availability of TV3 as follows:

$$TV3_{i, April 1984} = \beta_0 + \beta_1 X_{i, 1980} + \beta_2 X_{i, 1981} + \varepsilon_i \quad (1)$$

Where the dependent variable $TV3_{i, April 1984}$ is the availability of TV3 in municipality i by April 1983. $X_{i, 1980}$ and $X_{i, 1981}$ are a set of initial conditions (some for 1980 and others for 1981 given the availability of information) that might affect the TV3 placement as well as the subsequent trajectories of municipalities' political outcomes. For $X_{i, 1980}$, these are: i) voter turnout in 1980, ii) CiU vote share in 1980, and for $X_{i, 1981}$, iii) Log of total population in 1981, iv) Share of men with respect to total population in 1981, v) Share of non-native

people with respect to total population in 1981, vi) Share of people who speak Catalan with respect to total population in 1981, vii) Share of people that get BUP-COU with respect to total population in 1981, viii) Share of people that get a mid-graduate with respect to total population in 1981, ix) Share of people that have a professional diploma with respect to total population in 1981, and x) Financial institutions in 1981. The standard errors are clustered at district level⁵.

The inclusion of both the previous voter turnout and the CiU vote share is justified as voting may be habit-forming, namely individuals who voted in the past are more likely to vote again in future elections and for similar political agendas (Geys 2006b: 646). The inclusion of population size is also suggested by the probability of casting the decisive vote in the election (i.e. making or breaking a tie). The share of men is included to control for possible differences in political participation related to sex. The share of non-native people and Catalan speakers can also have an effect on electoral outcomes due to differences on political views related to individuals' origin. Educational variables can also influence the electoral participation in the extent that more sophisticated individuals are more likely to turnout. And finally, the presence of financial institutions (banks or saving banks) in the municipalities is intended to capture two things: First, it is a measure of how modern the municipality economy is, and second, it measures some kind of overlapped interest since *Banca Catalana*, the biggest bank by that time, was owned by Jordi Pujol, the top leader of the CiU (Baiges et al. 1985).

Based on the estimated propensity scores obtained from the previous analysis, with panel data over two time periods $t = \{1,2\}$, the average treatment effect on the treated (ATT) in the common support is given by (Khandker et al. 2010: 61):

$$ATT = \frac{1}{N_T} \left[\sum_{i \in T} (y_{i,2}^T - y_{i,1}^T) - \sum_{j \in NT} W(i,j) (y_{j,2}^{NT} - y_{j,1}^{NT}) \right] \quad (2)$$

Where T and NT denote exposure to TV3 (treatment) and no exposure to TV3 (control) respectively, $y_{i,2}^T - y_{i,1}^T$ is the change in the outcome measure for municipality i exposed to TV3, $y_{j,2}^{NT} - y_{j,1}^{NT}$ is the change in the outcome measure for control municipality j , N_T

⁵ Although the distance from municipality i to Barcelona might affect turnout and political views, we rule it out as it badly affect the balancing test, that is does not perform well to make comparable groups. Furthermore, it is deleted in the DD strategy since is a time-invariant variable.

represents the size of the treatment group, and $W(i, j)$ is the weight used to aggregate outcomes for the matched control municipalities j . The kernel matching is more convenience than other matching methods because it uses a weighted average of all untreated observations in order to construct the counterfactual match for each treated, so that it gives more weight to those control municipalities that are closer matches and less weight to farther observations. Also, Kernel matching reduces the estimation variance insofar as it uses more observations than other matching algorithms.

Specifically, the weights are obtained by the following function (Khandker et al. 2010: 60):

$$W(i, j) = \frac{K \left[\frac{p_j - p_i}{a_n} \right]}{\sum_{k \in NT} K \left[\frac{p_k - p_i}{a_n} \right]} \quad (3)$$

where $K(\cdot)$ is a kernel function, a_n is a bandwidth parameter, and p_i is the estimated propensity score of the treated municipalities. p_j and p_k are the estimated propensity scores of municipalities in the control group.

Although combining DD and PSM has advantages such as reducing the self-selection bias by matching comparable treatment and control areas as well as removing time invariant unobserved characteristics that might affect outcomes, the estimator could still be biased if there are any time variant observed characteristics that affect the outcomes over time. For instance, if social-demographic characteristic like the total population or the average education years change differently for treated and control groups in the period of analysis, this would affect the parallel trend assumption. To reduce the risk of such a bias, we control for the change in the co-variables used to match on initial conditions since they might vary over time and influence the political outcomes.

In terms of a regression framework, we estimate a weighted least squares regression by means of a first-difference equation as follows.

$$\Delta Y_{i,t}(W_i) = \beta_1 TV3_{i, April 1984} + \beta_2 \Delta X_{i,t} + \beta_3 Banks_{i, 1984} + v_j + \eta_p + \varepsilon_{i,t} \quad (4)$$

Where $\Delta Y_{i,t}$ is the change between 1980 and 1984 in the political outcomes of interest, that is the voter turnout and the CiU vote share for municipality i . $TV3_{i, April 1984}$ is the availability of TV3 in the municipality i in April 1984, thus the regression coefficient β_1 measures the

difference-in-difference estimator. $\Delta X_{i,t}$ is the change for time-variant controls. Finally, ν_j are fixed effects at district j , η_p are fixed effects at province p , and $\varepsilon_{i,t}$ is the disturbance term. The standard errors are clustered robust at the district level for all econometric specifications except when including district fixed effects, in this case the standard errors are just robust.

Another source of endogeneity for omitted time-variant variables might come from the effect of other media such as radio or newspapers that came out over the same period. Nevertheless, scarce information does not allow yet to control for the coverage of other type of media at municipality level. Regarding radio, however, the concern can be minimized given the higher audience of TV3 in comparison to Catalunya Ràdio. According to Jones (2007: 521), the audience of Catalunya Ràdio reached out 249,000 individuals in 1986 (4% of the total population of Catalonia), whereas TV3 was about 2,047,000 (34% of the total population), that is almost nine times larger than the radio audience. With regards to newspapers, we still do not count with figures about subscribers at municipality level, then we do not know the geographical differences in the number of readers. These differences could affect the political outcomes so that the results of the effect of TV3 might be biased, future research therefore should be focused on considering the effect of newspapers.

6. Results

6.1. Propensity Score Matching

Firstly, Table 5 displays the results using a Logit model to estimate the propensity scores of areas exposed and not exposed to TV3. In this model we include co-variables as initial conditions that jointly influence the likelihood of treatment and outcomes. It is noteworthy that those municipalities with larger population and presence of banks are more likely to be exposed to TV3. We estimate the scores ensuring they satisfy the balancing property within the region of common support, which is built by dropping treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls. 30 treated municipalities are off the common support, whilst no control municipality is off. Then, we match the treatment and control areas using the default Stata Software specifications, namely Epanechnikov kernel matching with a bandwidth of 0.06.

Table 6 shows the balancing test for co-variates used to estimate propensity scores. It displays the mean differences for the treatment and control groups before and after being matched. As we see, there was a significant difference in six co-variates between those municipalities exposed to TV3 and those not exposed before matching, which affects the comparability of the two groups. These are: the log of total population, share of men, share of non-native from Catalonia, share of Catalan speakers, share of people with a mid-graduate, and share of people with higher education. Once matched, these statistical differences on the average value no longer exist, the treatment and control groups are then more comparable in terms of the co-variates included in the PSM. Then, we move on to apply the DD model with the weights $W(i, j)$ assigned to each control municipality j arising from the kernel matching method.

6.2. Effect of TV3 on Voter Turnout and the CiU Vote Share

With respect to our first hypothesis, we found that municipalities exposed to TV3 present a larger change in voter turnout between 1980 and 1984 (Table 7). All the econometric specifications are statistically significant as to the variable *Availability of TV3 in April 1984*. We first implement a simple difference-in-difference model and estimate the equation 4 without controls, the effect is about 10.5 percentage points (Column 1). In column 2 we add control variables and find that the effect is about 3.8 percentage points, and the R^2 raises from 0.20 to 0.36. When including district fixed effects (Column 3), the effect is about 7.8 percentage points and the R^2 increases until 0.47. Finally, with province fixed effects (Column 4), the effect is about 3.1 percentage points and the R^2 is 0.37. Then, it can be said that the effect of TV3 on voter turnout is positive and statistically significant. On average, including only the estimations with controls and fixed effects, the impact is about 4.9 percentage points.

In addition to favoring the increase in voter turnout, TV3 also seemingly favored the increase in the CiU vote share, which it confirms our second hypothesis (Table 8). All the econometric specifications are statistically significant. With no controls, the effect is about 31 percentage points (Column 1). When including control variables (Column 2), the effect is about 13.6 percentage points, and the R^2 raises from 0.41 to 0.65. With fixed effects at district level

(Column 3) the effect falls until 6.6 percentage points and the R^2 is 0.81. And with province fixed effects (Column 4), the effect is about 9.6 percentage points and the R^2 is 0.73. Then, it can be concluded that the effect of TV3 on the CiU vote share is statistically significant and is on average 9.9 percentage points.

6.3. Effect on Other Political Parties

In addition to increasing voter turnout and the CiU vote share, might it be expected that the exposure to TV3 had had some effect on other political parties competing in the same election? Table 9, 10 and 11 display the estimated coefficients for PSC, PP⁶ and PSUC, respectively. Interestingly, we found that the channel did influence negatively the PSUC vote share. This significant effect is robust when including controls and province fixed effects. On average, the negative impact of TV3 on this political party is about 1.6 percentage points, which is much smaller in comparison to the positive impact on the CiU (9.9 percentage points). The effect on the other two political parties is null.

This finding raises important questions for future work. In particular, why was PSUC negatively affected by the exposure to TV3 while the impact on the CiU was positive? The PSUC was one of the oldest left-wing political parties in Catalonia, created in 1936 shortly after the onset of the Spanish Civil War, and disappeared in 1987 after the dramatic reduction of its vote share in the early 80s⁷. Of course, we cannot claim that the decrease in the PSUC vote share was an effect totally induced by TV3, but this channel seemingly contributed in some extent. Since ultimately the analysis of this paper only considers the correlation between access to the TV channel and vote, and it does not refer to the use that voters did of the TV channel nor the content of the programmes, this finding suggests that is necessary to go into a content analysis of TV3 by that time. By doing so, we may understand in a more detailed fashion the mechanisms by which TV3 could have influenced positively the CiU, while affecting negatively others political parties such as PSUC.

⁶ Idescat labels as “PP” the information about votes for right-wing parties, however it is worth mentioning that this label refers to votes for different right-ideology political parties in Catalonia. For instance, the 1980 figure is for *Solidaritat Catalana*, party that was dissolved after this election.

⁷ In 1987, PSUC was dissolved and formed the coalition *Iniciativa per Catalunya* (IC), which is known today as *Iniciativa per Catalunya Verds* (ICV).

6.4. Duration Treatment Effect

Eight months passed since TV3 began the transmissions until the Catalan parliament elections in April 1984, but not all treated municipalities were assigned to TV3 at the same time. We then split the treatment group in two: those who received TV3 for a period between 4 and 8 months and those who received it for less than 4 months (Figure 2).

<Figure 2 about here>

When reducing the sample to those municipalities exposed to TV3 in April 1984, we have 235 municipalities treated between 4 and 8 months, and 378 municipalities treated for less than 4 months. Similarly, we combine a DD strategy with PSM in order to have more comparable groups. Once obtained the propensity scores, 5 treated municipalities are off the common support, thus we finally count on 230 treated and 378 untreated.

Regarding voter turnout (Table 12), without controlling for any variable (Column 1), the effect is about 16.7 percentage points and the R^2 is 0.23. Including control variables (Column 2), the effect is 4.6 percentage points and the R^2 raises to 0.48. When including district fixed effects (Column 3), the coefficient gives an effect of 34.8 percentage points and the R^2 goes up to 0.52. And with province fixed effects (Column 4), the impact is about 5.1 percentage points and the R^2 is about 0.49.

Also, there is a duration treatment effect on the CiU vote share (Table 13). With no controls (Column 1), the effect is about 30 percentage points and the R^2 is 0.42. Including control variables (Column 2), the effect is 9.2 percentage points and the R^2 raises to 0.73. When including district fixed effects (Column 3), the coefficient is 17.7 percentage points and the R^2 is 0.80. And finally with province fixed effects (Column 4), the impact is about 16.1 percentage points and the R^2 is 0.78.

6.5. Placebo Tests

The first placebo treatment is a “lead” that uses data on TV3 diffusion between April 1984 and December 1985 (i.e. twenty months later than the initial treatment variable). In April 1984, 613 municipalities were treated and 323 untreated, which are the size of our main treatment and control groups. To December 1985, TV3 had reached out 266 municipalities more. Thus, we construct our first placebo variable as a dummy that takes on 1 for

municipalities that were reached by TV3 from April 1984 to December 1985 (i.e. 266 municipalities) and 0 otherwise. If the exposure to TV3 is indeed causing the effects in the outcome, then the “lead” variable should not be significant (otherwise they will capture anticipatory effects or pre-existing trends). Further, we redid the weighting procedure using these new treated municipalities.

Columns 1 and 3 in Table 14 display the results of the first placebo test. We only report the results for regressions that include all controls and district fixed effects. The results show that the lead, Availability of TV3 in December 1985, is not significant neither for the change in the voter turnout nor the CiU vote share once we include controls and the original treatment variable Availability of TV3 in April 1984. These results give support to the idea that there are not pre-existing trends in the DD model, thus the outcome trends are plausibly parallel.

The second placebo randomly assigns the area used as control group to a fake placebo treatment. We produce this random placebo treatment in order to stay with the same ratio of areas in the control and placebo treated as the one found between our treated and control areas, namely 65.64% of treated with respect to the total number of municipalities, that means 211 treated and 110 untreated. And similarly, a DD strategy is combined with PSM in order to have more comparable groups. Once obtained the propensity scores, two treated municipalities are off the common support, then in sum we count on 209 treated and 110 untreated.

Columns 2 and 4 in Table 14 show the results of the fake placebo treatment test. As we can see, the fake treatment has no effect on the political outcomes neither for the change in the voter turnout nor the CiU vote share. In line with the previous placebo test, these results also contribute to support the idea that there are not pre-existing trends in the DD model.

6.6. Persuasion Rates

Persuasion rates are a measure of the percentage of receivers that change the behavior amongst those that receive a message and are not already persuaded (DellaVigna & Gentzkow 2010).

The persuasion rate is defined as follows:

$$f = 100 * \frac{y_T - y_C}{e_T - e_C} \frac{1}{(1 - y_0)} \quad (5)$$

where e_i is the share of group i receiving the message, y_i is the share of group i adopting the behavior of interest, and y_0 is the share that would adopt if there were no message. Due to that y_0 is not observed, we can approximate it by y_C , the turnout in the control group, as long as the exposure to TV3 of the control groups is zero ($e_C = 0$), which is the case in this study⁸The persuasion rate captures the effect of the persuasion treatment on the relevant behavior ($y_T - y_C$), which is the estimated effect of the change in the political outcomes $\hat{\beta}_1$, adjusting for exposure to the message ($e_T - e_C$) and for the size of the population left to be convinced ($1 - y_0$).

For the case of voter turnout, the value of $y_T - y_C$ is on average 0.05 (taking the average of $\hat{\beta}_1$'s estimated with controls and fixed effects). Due to the fact that $y_0 \approx y_C$, then $y_0 = 0.68$, which is the turnout in the control group in the 1984 Catalan Parliamentary election. And the exposure rate can be $(e_T - e_C) = (34\% - 0)$ if we follow the figures about TV3 audience provided by Jones (2007) for 1986 and we assume that this audience is very similar in 1984 and $e_C = 0$. Using the equation 5, the persuasion rate of TV3 on voter turnout in the 1984 Catalan parliamentary election is about 46%. It can be said that about 46% of those who were not already persuaded in the treated population changed their behavior and went out to vote in due to the influence of TV3.

For the case of the CiU vote share, $y_T - y_C = 0.09$ and $y_0 = 0.65$, which is the CiU vote share in the control group in the 1984 Catalan Parliamentary election and we assume the same TV3 audience. Using the equation 6.1, the persuasion rate of TV3 on the CiU vote share in the 1984 Catalan parliamentary election is about 75.6%. It can be said therefore that about 75.6% of those who were not already persuaded in the treated population changed their behavior and decided to go out to vote for CiU due to the exposure to TV3.

⁸ DellaVigna et al. (2010: 5) provides a demonstration of such approximation. Assuming random exposure and a constant persuasion rate f , the share in group i who adopt the behavior is $y_i = y_0 + e_i f (1 - y_0)$. Rearranging this expression gives equation 5. Solving the system for y_0 , it can be obtained $y_0 = y_C - e_C (y_T - y_C) / (e_T - e_C)$. The approximation $y_0 = y_C$ is valid as long as the exposure rate in the control group is small ($e_C \approx 0$) or the effect of the treatment is small ($y_T - y_0 \approx 0$).

Persuasion Rates in the Literature: How large is the persuasion rate? DellaVigna & Kaplan (2007) and DellaVigna & Gentzkow (2010) provide a summary of the persuasion rates obtained by different salient studies. According to DellaVigna & Kaplan (2007), regarding voter turnout, the field experiment carried out by Gerber & Green (2000) (Canvassing, Telephone Calls and Direct Mail) presents the largest persuasion rate: 26.30%, also Green & Gerber (2001) (Phone call or face-to-face contact) with 20.50%. With regards to the effect of biased media on voting behavior, DellaVigna & Kaplan (2007) (FOX News) presents a persuasion rate of 11.60%; Enikolopov et al. (2011) (Non-governmental TV), 10.20%; and Gerber et al. (2009) (Washington Post), 20%. Then, it can be regarded that the persuasion rate of TV3 both on the voter turnout and the CiU vote share of the 1984 Catalan parliamentary election is comparatively larger with respect to other case studies.

7. Final Remarks

This study contributes to the discussion on the media effect on electoral outcomes in a setting unexplored until now: a case study in which a relevant sub-national television channel with a supposedly partisan and pro-independence bias exerts influence on the electoral results. The main conclusion arising from this study is that TV3 indeed caused an increment in the voter turnout as well as the CiU vote share in the 1984 Catalan parliamentary election.

Regarding voter turnout, unlike some studies like Gentzkow's (2006) who finds a negative effect of TV on voter turnout as a result of a crowding-out effect, TV3 seemingly complemented the information provided by traditional media such as TVE, newspapers and radio, and induced individuals to go out to vote in the 1984 Catalan parliamentary election. This finding interesting but not surprising, since even if television as a whole could decrease turnout, a new channel with a great political meaning (such as the case of TV3) that provides lots of information to a particular region would increase turnout. On the other hand, this finding is congruent with other studies focused in other type of media such as radio and newspaper (Strömberg 2004; Oberholzer-Gee & Waldfogel 2010; Snyder & Strömberg 2010; Gentzkow et al. 2011; Drago et al. 2013).

With respect to the CiU vote share, the TV3 effect is in line with most of the related literature on political persuasion (DellaVigna & Kaplan 2007; Gerber et al. 2009; DellaVigna & Gentzkow 2010; Enikolopov et al. 2011; Hierro 2012) We found evidence that the increase

in the CiU vote share -the political party who created and has mostly managed this channel since then- was a phenomenon attributable to TV3.

Furthermore, this paper also showed that TV3 could have had a negative effect on the vote share of other political parties competing in the same election. In particular, we found that the exposure to TV3 negatively affected the vote share of *Partit Socialista Unificat de Catalunya* (PSUC), an old left-wing political party that was dissolved in the early 80s after several electoral failures. We also found that those municipalities exposed longer present a larger effect in both the voter turnout and the CiU vote share in comparison to those less exposed. Finally, the persuasion rate on the voter turnout is about 46%, and the persuasion rate on the CiU vote share is about 75.6%. In comparison to other case studies, these persuasion rates are quite large, which could be explained by the relevance of TV3 in the early 80's Catalan society.

Many questions are still opened for future research. Firstly, it is necessary to find other methodological approaches to examine the causal effect of TV3 on electoral results in more recent years, when the controversy regarding TV3 has been more intense. Secondly, it is also important to go into a content analysis to study with more detail the mechanism by which TV3 could influence on electoral results. Thirdly, another topic of great significance is the role of competition. The insight that competition amongst media can limit the scope of persuasion has important implications for regulation and public policy, but it has not been explored enough yet. Does TV3 still have an influence on political attitudes in the context of a wider range of media today (many TV channel, radio stations and internet, social networks, among others)? Providing convincing answers to these questions will help us to better understand the role of media in the Catalan politics today.

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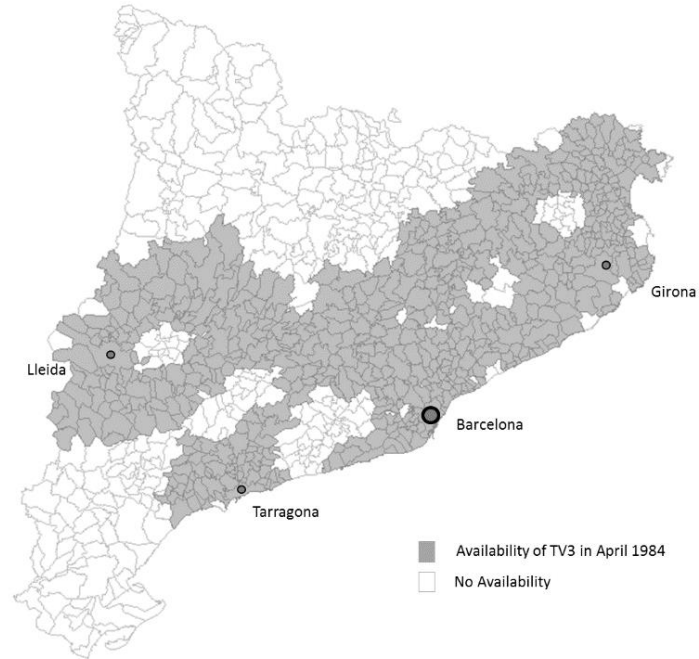
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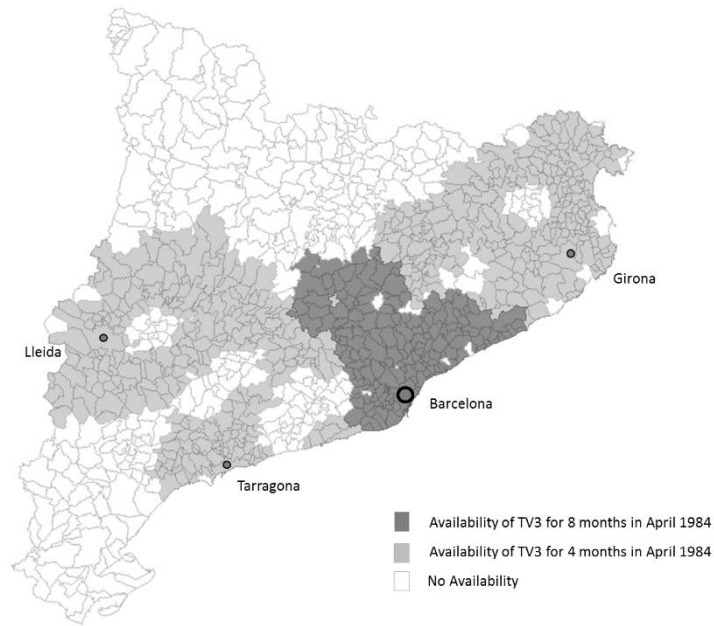
Figures

Figure 1: Availability of TV3 in Catalonia in April 1984



Source: Prepared by the author based on (Montero 1987) and *La Vanguardia* (Table 2).

Figure 2: Availability of TV3 differentiated by duration in Catalonia by April 1984



Source: Prepared by the author based on Montero (1987) and *La Vanguardia* (Table 2).

Tables

Table 1: Results of Regional Elections in Catalonia (Vote share in parenthesis)

Year	CiU	PSC	PP	PSUC/ICV	ERC	C's	CUP	Others	Total	Elected President
1980	754,448 (28.00)	608,689 (22.59)	64,119 (2.38)	509,014 (18.89)	241,711 (8.97)	-	-	516,005 (19.15)	2,693,986	Jordi Pujol (CiU)
1984	1,345,513 (47.01)	865,449 (30.24)	221,697 (7.75)	160,586 (5.61)	126,865 (4.43)	-	-	142,011 (4.96)	2,862,121	Jordi Pujol (CiU)
1988	1,230,356 (46.03)	800,999 (29.96)	143,062 (5.35)	208,689 (7.81)	111,276 (4.16)	-	-	178,741 (6.69)	2,673,123	Jordi Pujol (CiU)
1992	1,218,831 (46.77)	726,099 (27.86)	157,395 (6.04)	171,455 (6.58)	209,881 (8.05)	-	-	122,320 (0.47)	2,605,981	Jordi Pujol (CiU)
1995	1,314,548 (41.36)	797,422 (25.09)	420,341 (13.23)	312,371 (9.83)	304,833 (9.59)	-	-	28,720 (0.09)	3,178,235	Jordi Pujol (CiU)
1999	1,172,721 (38.05)	1,177,777 (38.22)	295,765 (9.60)	78,213 (2.54)	270,176 (8.77)	-	-	87,084 (2.83)	3,081,736	Jordi Pujol (CiU)
2003	1,018,164 (31.22)	1,026,396 (31.47)	390,882 (11.98)	240,235 (7.37)	542,046 (16.62)	-	-	44,018 (1.35)	3,261,741	Pasqual Maragall (PSC)
2006	928,936 (32.19)	789,956 (27.37)	313,368 (10.86)	281,405 (9.75)	414,044 (14.35)	89,544 (3.10)	-	68,640 (0.24)	2,885,893	José Montilla (PSC)
2010	1,198,193 (39.66)	570,405 (18.88)	384,470 (1.27)	229,853 (7.61)	218,152 (7.22)	105,884 (3.50)	-	314,503 (10.41)	3,021,460	Artur Mas (CiU)
2012	1,112,605 (31.14)	523,537 (14.65)	470,759 (13.18)	358,860 (1.00)	496,466 (13.90)	274,652 (7.69)	126,198 (3.53)	209,729 (5.87)	3,572,806	Artur Mas (CiU)

Source: Idescat

Note: CiU: Convergència i Unió; PSC: Partido de los Socialistas de Cataluña; PP: Partido Popular; PSUC: Partit Socialista Unificat de Catalunya; ICV: Iniciativa per Catalunya els Verds; ERC: Esquerra Republicana de Catalunya; C's: Ciudadanos; CUP: Candidatura de Unidad Popular.

Table 2: Information on TV3 coverage from newspaper *La Vanguardia*

Date	Page	Article
Monday, January 02, 1984	42	TV Catalana 1983: un año de grandes esperanzas
Friday, January 06 1984	19	TV3 llegó a las comarcas de Lleida
Friday, January 13 1984	25	El repetidor de TV3 en Rocacorba no cubrirá todas las comarcas de Girona
Thursday, April 05 1984	23	Los hoteleros de la Vall d’Aran se quejan de las anomalías de TV
Saturday, April 07 1984	6	Las islas Baleares quieren “conectar” con TV3
Thursday, June 13 1985	29	TVE y TV3 estudian la cobertura del Pallars Sobira y la Vall d’Aran
Wednesday, June 26 1985	23	A partir de septiembre, TV3 se captará en buenas condiciones en toda la zona del Pirineo
Saturday, July 27 1985	19	La política de reemisores de TV3 perjudica los acuerdos con TVE, según Calviño
Saturday, July 27 1985	40	Calviño inauguró ayer un centro emisor en el Pirineo leridano

Table 3: Summary statistics

Variables	All Municip.	TV3 in April 1984	No TV3 in April 1984	Diff.	Variables	All Municip.	TV3 in April 1984	No TV3 in April 1984	Diff.
CiU Vote Share 1980	0.338 (0.187)	0.341 (0.190)	0.330 (0.181)	0.010	Share Non-Native Catalan 1981	0.152 (0.126)	.169 (.135)	0.118 (0.099)	0.056***
CiU Vote Share 1984	0.648 (0.158)	0.645 (0.151)	0.653 (0.172)	-0.008	Share Non-Native Catalan 1986	0.229 (0.163)	0.253 (0.172)	0.183 (0.130)	0.077***
Voter Turnout 1980	0.582 (0.171)	0.577 (0.185)	0.592 (0.137)	-0.008	Share BUP COU 1981	0.038 (0.024)	0.038 (0.022)	0.037 (0.027)	0.002
Voter Turnout 1984	0.691 (0.088)	0.697 (0.078)	0.681 (0.100)	0.019**	Share BUP COU 1986	0.044 (0.022)	0.045 (0.023)	0.041 (0.022)	0.006**
Total Population 1981	6,370 (59,971)	8,878 (73,929)	1,595 (3,071)	7,145*	Share Mid Graduate 1981	0.016 (0.017)	0.016 (0.018)	0.014 (0.013)	0.002*
Total Population 1984	6,465 (60,376)	9,000 (74,384)	1,621 (3,085)	7,244*	Share Mid Graduate 1986	0.018 (0.013)	0.019 (0.013)	0.016 (0.013)	0.003**
Log Total Population 1981	6.807 (1.560)	6.984 (1.644)	6.468 (1.324)	0.588***	Share Superior 1981	0.014 (0.014)	0.015 (0.012)	0.012 (0.016)	0.002**
Log Total Population 1984	6.828 (1.567)	7.013 (1.649)	6.476 (1.331)	0.610***	Share Superior 1986	0.018 (0.013)	0.018 (0.014)	0.017 (0.013)	0.001*
Share Men 1981	0.505 (0.024)	0.503 (0.020)	0.509 (0.028)	-	Distance to Barcelona (Km)	4,781 (3,112)	4,283 (2,906)	5,736 (3,287)	-1,523***
Share Men 1984	0.505 (0.026)	0.503 (0.023)	0.509 (0.030)	-0.006**	Log Distance to Barcelona	8.105 (1.084)	7.959 (1.139)	8.385 (0.907)	-0.450***
Share Catalan Speakers 1981	0.888 (0.122)	0.883 (0.110)	0.903 (0.137)	-0.022**	Banks 1981	0.465 (0.499)	0.484 (0.500)	0.429 (0.495)	0.054
Share Catalan Speakers 1986	0.954 (0.179)	0.964 (0.138)	0.929 (0.115)	0.037***	Banks 1984	0.496 (0.500)	0.517 (0.500)	0.459 (0.499)	0.057*
Total Observations	941	635	306						

Source: Idescat. Standard errors in parenthesis.

Table 4: Difference-in-Differences

	Municipalities T (TV3)	Municipalities NT (No TV3)	Causal Effect
Period 1 (Before TV3 entry)	y_1^T	y_1^{NT}	
Period 2 (After TV3 entry)	y_2^T	y_2^{NT}	
	$A = y_2^T - y_1^T$	$B = y_2^{NT} - y_1^{NT}$	$A - B$

Table 5: Logit Model of the Availability of TV3 in April 1984

Co-variates	Log Odds
Voter Turnout 1980	0.412 (1.028)
CiU Vote Share 1980	1.879 (1.144)
Log Total Population 1981	0.363** (.178)
Share of Men 1981	-7.895* (4.083)
Share of Non-native 1981	3.533 (2.307)
Share of Catalan Speakers 1981	0.081 (0.878)
Share of BUP COU 1981	-2.246 (4.681)
Share of Mid Graduate 1981	4.485 (5.476)
Share of Professional Diploma 1981	9.167 (7.073)
Banks 1981	-0.943** (0.329)
Constant	1.062 (2.940)
Prob > chi2	0.000
Pseudo R 2	0.065
N	934

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered standard errors at district level in parenthesis.

Table 6: Balancing test for co-variates used to estimate propensity scores

		Kernel Matching					
		Mean		%Bias	%Reduct	p> t	
		Treated	Control		Bias		
Voter Turnout 1980	Unmatched	0.577	0.592	-9.1		0.208	
	Matched	0.595	0.596	-0.7	92.7	0.905	
CiU Vote Share 1980	Unmatched	0.340	0.331	4.8		0.488	
	Matched	0.348	0.354	-3.3	32.5	0.574	
Log Total Population 1981	Unmatched	6.985	6.479	34.0		0.000	***
	Matched	6.821	6.754	4.5	86.7	0.413	
Share of Men 1981	Unmatched	0.503	0.509	-23.2		0.000	***
	Matched	0.503	0.504	-0.8	96.5	0.723	
Share of Non-native 1981	Unmatched	0.169	0.118	42.5		0.000	***
	Matched	0.156	0.153	2.3	94.6	0.695	
Share of Catalan Speakers 1981	Unmatched	0.883	0.903	-16.4		0.015	**
	Matched	0.890	0.900	-5.7	65.1	0.264	
Share of BUP COU 1981	Unmatched	0.038	0.037	6.3		0.395	
	Matched	0.037	0.038	-3.9	37.4	0.472	
Share of Mid Graduate 1981	Unmatched	0.016	0.014	12.3		0.089	*
	Matched	0.015	0.016	-0.6	94.8	0.897	
Share of Professional Diploma 1981	Unmatched	0.015	0.012	15.9		0.016	**
	Matched	0.014	0.014	3.4	78.5	0.543	
Banks 1981	Unmatched	0.484	0.429	11.0		0.112	
	Matched	0.464	0.452	2.5	77.4	0.672	

Note: *** 99% of confidence level, ** 95%, and * 90%.

This “bias” is defined as the difference of the mean values of the treatment group and the (not matched / matched) non treatment group, divided by the square root of the average sample variance in the treatment group and the not matched non treatment group.

Table 7: The Effect of TV3 on Voter Turnout in the 1980-1984 Catalan Parliamentary Elections

	Change in Voter Turnout 1980-1984			
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	0.105*** (0.010)	0.038** (0.010)	0.078*** (0.015)	0.031** (0.013)
<i>Control Variables</i>				
Change in Total Population 1981-1984		-0.179* (0.092)	-0.194** (0.086)	-0.133 (0.094)
Change in Share of Men 1981-1984		0.0815 (0.255)	0.154 (0.245)	0.158 (0.241)
Change in Share of Non-native 1981-1986		0.645*** (0.108)	0.211** (0.103)	0.564*** (0.103)
Change in Share of Catalan Speakers 1981-1986		0.006 (0.041)	0.048 (0.032)	0.018 (0.038)
Change in Share of BUP COU 1981-1986		-0.658** (0.202)	-0.689*** (0.173)	-0.672** (0.191)
Change in Share of Mid Graduate 1981-1986		-0.103 (0.300)	-0.338 (0.311)	-0.246 (0.288)
Change in Share of Professional Diploma 1981-1986		0.432 (0.258)	0.183 (0.327)	0.254 (0.284)
Banks 1984		0.045*** (0.011)	0.026** (0.009)	0.044*** (0.010)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.202	0.361	0.469	0.369
N	904	904	904	904

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 8: The Effect of TV3 on the CiU Vote Share in the 1980-1984 Catalan Parliamentary Elections

Change in the CiU Vote Share 1980-1984				
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	0.311*** (0.006)	0.136*** (0.021)	0.066** (0.022)	0.096*** (0.024)
<i>Control Variables</i>				
Change in Total Population 1981-1984		-0.420** (0.195)	-0.149 (0.124)	-0.116 (0.156)
Change in Share of Men 1981-1984		-0.981 (0.621)	-0.354 (0.443)	-0.381 (0.503)
Change in Share of Non-native 1981-1986		1.932*** (0.253)	0.272* (0.161)	1.197*** (0.289)
Change in Share of Catalan Speakers 1981-1986		-0.237** (0.111)	-0.054 (0.048)	-0.157* (0.076)
Change in Share of BUP COU 1981-1986		0.231 (0.444)	0.141 (0.312)	0.136 (0.312)
Change in Share of Mid Graduate 1981-1986		0.342 (0.682)	-0.474 (0.422)	-0.392 (0.400)
Change in Share of Professional Diploma 1981-1986		1.693** (0.560)	-0.398 (0.447)	0.427 (0.473)
Banks 1984		0.076** (0.023)	0.034** (0.015)	0.065*** (0.019)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.418	0.651	0.816	0.734
N	904	904	904	904

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 9: The Effect of TV3 on the PSC Vote Share in the 1980-1984 Catalan Parliamentary Elections

	Change in the PSC Vote Share 1980-1984			
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	0.017*	-0.008	-0.016	0.004
	(0.009)	(0.008)	(0.014)	(0.008)
<i>Control Variables</i>				
Change in Total Population 1981-1984		-0.067	-0.093	-0.121
		(0.119)	(0.092)	(0.119)
Change in Share of Men 1981-1984		0.145	0.064	0.079
		(0.284)	(0.204)	(0.273)
Change in Share of Non-native 1981-1986		0.231**	0.286***	0.250**
		(0.090)	(0.095)	(0.096)
Change in Share of Catalan Speakers 1981-1986		0.086*	0.052*	0.072
		(0.050)	(0.031)	(0.047)
Change in Share of BUP COU 1981-1986		-0.431**	-0.386**	-0.416**
		(0.193)	(0.160)	(0.182)
Change in Share of Mid Graduate 1981-1986		-0.093	0.104	0.165
		(0.246)	(0.223)	(0.225)
Change in Share of Professional Diploma 1981-1986		-0.641**	-0.519*	-0.520**
		(0.272)	(0.282)	(0.251)
Banks 1984		0.016	0.010	0.0153
		(0.010)	(0.008)	(0.010)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.012	0.087	0.219	0.128
N	904	904	904	904

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 10: The Effect of TV3 on the PP Vote Share in the 1980-1984 Catalan Parliamentary Elections

	Change in the PP Vote Share 1980-1984			
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	0.035*** (0.007)	0.008 (0.009)	-0.003 (0.009)	0.003 (0.005)
<i>Control Variables</i>				
Change in Total Population 1981-1984		-0.073 (0.062)	0.016 (0.043)	0.010 (0.043)
Change in Share of Men 1981-1984		-0.096 (0.126)	0.062 (0.183)	0.070 (0.145)
Change in Share of Non-native 1981-1986		0.222*** (0.062)	0.030 (0.068)	0.035 (0.060)
Change in Share of Catalan Speakers 1981-1986		-0.028 (0.017)	-0.010 (0.025)	-0.005 (0.022)
Change in Share of BUP COU 1981-1986		0.094 (0.150)	0.131 (0.141)	0.063 (0.133)
Change in Share of Mid Graduate 1981-1986		0.113 (0.207)	0.108 (0.248)	0.018 (0.212)
Change in Share of Professional Diploma 1981-1986		1.000*** (0.195)	0.531** (0.250)	0.541*** (0.193)
Banks 1984		0.016** (0.007)	0.005 (0.005)	0.010** (0.005)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.106	0.232	0.489	0.439
N	904	904	904	904

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 11: The Effect of TV3 on the PSUC Vote Share in the 1980-1984 Catalan Parliamentary Elections

	Change in the PSUC Vote Share 1980-1984			
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	-0.060*** (0.005)	-0.014** (0.005)	-0.017 (0.013)	-0.015** (0.005)
<i>Control Variables</i>				
Change in Total Population 1981-1984		0.075 (0.056)	0.067 (0.045)	0.057 (0.048)
Change in Share of Men 1981-1984		0.091 (0.110)	0.007 (0.131)	0.022 (0.097)
Change in Share of Non-native 1981-1986		-0.443*** (0.066)	-0.203*** (0.077)	-0.323*** (0.078)
Change in Share of Catalan Speakers 1981-1986		0.026 (0.023)	-0.011 (0.020)	0.021 (0.022)
Change in Share of BUP COU 1981-1986		0.355*** (0.116)	0.310*** (0.103)	0.361*** (0.107)
Change in Share of Mid Graduate 1981-1986		-0.110 (0.174)	-0.028 (0.185)	-0.142 (0.168)
Change in Share of Professional Diploma 1981-1986		-0.159 (0.196)	0.190 (0.199)	-0.033 (0.219)
Banks 1984		-0.032*** (0.005)	-0.024*** (0.005)	-0.029*** (0.005)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.209	0.436	0.528	0.464
N	904	904	904	904

Note: *** 99% of confidence level, ** 95%, and * 90%.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 12: Duration Treatment on Voter Turnout in the 1980-1984 Catalan Parliamentary Elections

	Change in Voter Turnout 1980-1984			
	(1)	(2)	(3)	(4)
Duration Treatment	0.167*** (0.020)	0.046** (0.014)	0.348*** (0.037)	0.051** (0.017)
<i>Control Variables</i>				
Change in Total Population 1981-1984		-0.569** (0.232)	-0.482** (0.183)	-0.550** (0.233)
Change in Share of Men 1981-1984		-0.796 (0.547)	-0.438 (0.575)	-0.702 (0.536)
Change in Share of Non-native 1981-1986		1.279*** (0.193)	1.090*** (0.268)	1.267*** (0.213)
Change in Share of Catalan Speakers 1981-1986		0.004 (0.093)	-0.033 (0.085)	0.022 (0.092)
Change in Share of BUP COU 1981-1986		-1.695** (0.546)	-1.938*** (0.488)	-1.766** (0.545)
Change in Share of Mid Graduate 1981-1986		-0.442 (0.357)	-0.587* (0.340)	-0.378 (0.352)
Change in Share of Professional Diploma 1981-1986		-0.426 (0.469)	-0.351 (0.498)	-0.718 (0.435)
Banks 1984		0.042** (0.011)	0.013 (0.015)	0.034** (0.012)
District Fixed Effects			X	
Province Fixed Effects				X
R 2	0.231	0.479	0.522	0.485
N	608	608	608	608

Note: *** 99% of confidence level, ** 95%, and * 90%.

Duration Treatment: 1: between 4 and 8 months, and 0: less than 4 months.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 13: Duration Treatment on CiU Vote Share in the 1980-1984 Catalan Parliamentary Elections

Change in CiU Vote Share 1980-1984				
	(1)	(2)	(3)	(4)
Duration Treatment	0.300*** (0.010)	0.092** (0.023)	0.177*** (0.031)	0.161*** (0.030)
<i>Control Variables</i>				
Change in Total Population 1981-1984		0.313 (0.191)	0.113 (0.160)	0.229 (0.173)
Change in Share of Men 1981-1984		-0.377 (0.580)	0.701 (0.489)	0.226 (0.520)
Change in Share of Non-native 1981-1986		1.986*** (0.204)	0.985*** (0.198)	1.362*** (0.285)
Change in Share of Catalan Speakers 1981-1986		-0.570*** (0.107)	-0.330*** (0.081)	-0.403** (0.112)
Change in Share of BUP COU 1981-1986		0.391 (0.576)	-0.762* (0.421)	-0.234 (0.511)
Change in Share of Mid Graduate 1981-1986		0.928** (0.289)	0.595 (0.435)	0.841** (0.318)
Change in Share of Professional Diploma 1981-1986		0.279 (0.695)	-0.391 (0.516)	-0.476 (0.631)
Banks 1984		0.086*** (0.020)	0.015 (0.016)	0.046** (0.017)
District Fixed Effects			X	
Province Fixed Effects				X
R ²	0.421	0.736	0.806	0.780
N	608	608	608	608

Note: *** 99% of confidence level, ** 95%, and * 90%.

Duration Treatment: 1: between 4 and 8 months, and 0: less than 4 months.

Clustered robust standard errors at district level (in parenthesis) except for Column 3 where we instead include district fixed effects and robust standard errors.

Table 14: Placebo Tests on Voter Turnout in the 1980-1984 Catalan Parliamentary Elections

	Change in Voter turnout 1980-1984		Change in CiU Vote Share 1980-1984	
	(1)	(2)	(3)	(4)
Availability of TV3 in April 1984	0.041 (0.034)		0.166 (0.115)	
Placebo 1: Availability of TV3 in December 1985	-0.038 (0.023)		0.113 (0.112)	
Placebo 2: Fake Treatment Group		-0.002 (0.012)		-0.000 (0.019)
<i>Control Variables</i>				
Change in Total Population 1981- 1984	-0.067 (0.086)	-0.030 (0.151)	0.001 (0.129)	-0.167 (0.264)
Change in Share of Men 1981-1984	0.179 (0.272)	0.954 (0.649)	-0.132 (0.528)	-0.364 (1.262)
Change in Share of Non-native 1981- 1986	0.214** (0.106)	0.160 (0.176)	0.190 (0.176)	0.223 (0.262)
Change in Share of Catalan Speakers 1981-1986	0.057 (0.040)	0.075 (0.071)	-0.113* (0.058)	0.040 (0.076)
Change in Share of BUP COU 1981- 1986	-0.600*** (0.198)	-0.497** (0.246)	-0.050 (0.362)	0.768 (0.715)
Change in Share of Mid Graduate 1981-1986	-0.216 (0.247)	-0.674* (0.397)	-0.397 (0.420)	-1.186 (0.883)
Change in Share of Professional Diploma 1981-1986	0.122 (0.252)	-0.192 (0.523)	-0.910** (0.459)	-2.012** (0.998)
Banks 1984	0.022** (0.009)	0.024* (0.013)	0.024* (0.014)	0.057** (0.024)
District Fixed Effects	X	X	X	X
R ²	0.524	0.536	0.819	0.836
N	932	319	932	319

Note: *** 99% of confidence level, ** 95%, and * 90%.

Robust standard errors (in parenthesis).

Chapter 3

The Impact on the Stock Market of a Secession

Campaign in a EU Member State

Abstract

By using event study techniques, this paper examines the economic impact of a variety of news related to the Catalan pro-independence movement between 2010 and 2015 on the stock returns of a Catalan firms' portfolio. We found that the movement, despite its apparent strength and the dramatic claims made by the extremes in the debate, has not had an economically significant impact on the stock returns. Nevertheless, though small, there are some significant effects on both Catalan and non-Catalan firms mostly related to street demonstrations and anti-independence events (i.e. against the movement), as well as some differentiated effects related to the firms' economic activity sector and their political position respecting the movement. The lack of large economic impacts and the firms' reaction to some events suggests several possible interpretations about the investors' attitude toward the movement.

Key Words: Event Studies, Stock Market, Policy Uncertainty, Catalonia

JEL: G14, D81

1. Introduction

Over the last decades, nationalist discourses seem to have dominated the political landscape in the developed world. Contradicting the idea of a global community, nationalists have fueled conflicts against supra-national structures (e.g. the Brexit, Scotland), as well as have led to the escalation of sub-national strains in regions demanding more sovereignty (e.g. Catalonia and Spain). Nevertheless, few studies have thoroughly examined the economic consequences of pro-independence movements in the context of consolidated democracies, which is surprising given the aforementioned success of these nationalist discourses. This paper therefore attempts to shed some light on this issue by taking Catalonia as an emblematic case study.

By using event study techniques, this paper examines the effect of a variety of news related to the Catalan pro-independence movement between 2010 and 2015 on the stock returns of a Catalan firms' portfolio. We expect that if pro-independent events are considered as significantly changing return expectations by investors, stocks of firms highly exposed to the Catalan economy should have shown a larger reaction relative to those less-exposed. Moreover, we explore the impact of some specific type of events such as episodes against the movement (i.e. events that slowdown or block the independence process) and street demonstrations in favor of the movement.

The case of Catalonia contributes to at least two branches of the academic literature. Firstly, some research on the economic cost of conflicts has focused on the effect of *violent* events on stock prices (Abadie & Gardeazabal, 2003; Chen & Siems, 2004; Zussman & Zussman, 2006; Guidolin & Ferrara, 2005; Guidolin & Ferrara, 2007; Castells & Trillas, 2013; Acemoglu et al. 2014), and similarly on the price of other assets (even human assets, in the case of slavery, see Calomiris & Pritchett, 2016). Nevertheless, unlike violent conflicts, the effect of *non-violent* events such as sovereignist strains in modern democracies (Catalonia, Quebec, Scotland, Flanders, Veneto, among others) is less evident and therefore requires closer scrutiny. To our knowledge, there are at least two previous articles that analyze the effects of sovereignist conflicts on stock returns: Beaulieu et al. (2006) examine the case of Quebec and Canada, and He et al. (2015) the case of Taiwan and China. Yet only the former can be considered a highly peaceful conflict; the latter may involve some risk of violence.

Beaulieu et al. (2006) find that the results of the 30 October 1995 referendum about Quebec remaining or not within Canada had a positive impact on stock returns of Quebec firms around 0.2 to 0.6 percentage points; investors seemingly linked the results with a reduction in the economic and political uncertainty. On the other hand, He et al. (2015) find that events harming the relationship between Taiwan and mainland China led to an average daily drop of 2.0 percentage points in Taiwanese stock returns. Our paper also largely differs from Beaulieu et al. (2006) because we do not focus on a single event, but we instead analyze a broader and diverse set of events, which is intended to understand the complex effects of a sustained secessionist movement that cannot be captured in a singular event. This may be useful, because although secessionist referenda in democratic countries are very salient, they are unusual. Also, it differs from He et al. (2015) because, first, a military intervention in the case of Catalonia and Spain is much less probable. Moreover, we do not only study a few number of aggregated events, but rather we classify them and we assess whether they had a differential effect on firms' stock returns. Further, the case study of Catalonia is also quite different from the paper by He et al. (2015) because the Chinese economy is enormous in comparison to Taiwan (Taiwan's GDP is about 0.06% of China's), while Catalonia and the rest of Spain have more balanced economic sizes (Catalonia's GDP is about 19% of Spain's). Secondly, this paper also contributes to the policy debate about the general economic consequences of secessionism in consolidated democracies (Somers & Vaillancourt 2013 for the case of Quebec; McCrone & Linklater 2013 and Bell et al. 2014 for the case of Scotland; and Zipfel & Vetter 2015 for a general view of several European cases). This is a debate of great significance in the current stage of the developed world where a growing nationalist sentiment as well as the negative consequences of the 2008 Great Recession have jointly brought about an increased dissatisfaction with supra-national structures (e.g. the United Kingdom, the European Union), and has facilitated the irruption of movements based on identity and local national sovereignty. Thus, having empirical and reliable findings about the effects of such conflicts provide policy makers and analysts with more elements to understand and, eventually, predict the evolution of public opinion and any real changes that result from it.

The results show that the Catalan pro-independence movement, despite its apparent strength, has not had a sizable effect, neither positive nor negative, on the stock returns of Catalan

companies. There are several possible interpretations of this result. On the one hand, it might be that an eventual separation of Catalonia is not considered credible by investors. Perhaps this lack of credibility of the independence is a result of the strength of the institutional framework under which the conflict has been developed -i.e. a modern democracy that belongs to a strong institutional structure such as the European Union-, which make investors believe that both Spain and Catalonia will ultimately deal with this conflict in a reasonable fashion. It could also be that although investors perceive some negative consequences from the institutional instability created by a potential real secession event, they also perceive that the movement by itself, led by center-right politicians (which historically had defended business interests), was a way to counter-balance and distract the pressure of left-wing populist movements that were also strong in Catalonia and Spain around the times of the global financial crisis that hit strongly Europe's periphery after 2008.

In spite of having not found a sizable effect of the pro-independence movement on stock returns, there are some statistically significant findings that are worth mentioning. Street demonstrations in favor of the independence of Catalonia, which regularly take place yearly on September 11th (the National Day of Catalonia, aka *Diada*), present a negative effect on stock returns of both Catalan and non-Catalan firms. This effect is a drop of about -0.003 percentage points for Catalan firms and -0.002 for non-Catalans. Also, there is a negative effect on stock returns of Catalan companies of about -0.003 percentage points related to the events against the movement (i.e. events that slow down or block the pro-independence process). Additionally, we found that firms that have publicly expressed to be in favor of the movement present higher stock returns related to two types of events: politicians' announcements that show progress of the movement and events against the movement. This suggest that firms benefit from political tension in general, whether the political event means a progress or a slowdown of the movement. Nevertheless, though these impacts are statistically significant, they are economically very small, of about -0.003 and -0.005 percentage points. In sum, the lack of large economic impacts as well as the firms' reaction to some types of events suggest several interpretations about the investors' attitude toward the movement, but the fact remains, and this is our main result, that overall the secession campaign did not have strong effects on investors' expectations, despite some grandiose claims of supporters and detractors of the campaign about its impact on the economy.

Although this is an ongoing conflict, six years are enough to extract some conclusions. Now it seems quite clear that the conflict will not have a clear end, but it will evolve. Even so-called sovereign nation-states that hold referendums about their membership to larger entities (like the UK with the Brexit referendum) see that not even a referendum puts an end to sovereignty conflicts in democratic societies that have close economic and other links with others. If we expect for the conflict to “end” in some meaningful way, we may as well never analyze the consequences of these movements in financial markets.

The paper is structured as follows. The second section provides the historical and political background of the case study. The third section presents the methodological framework, hypotheses and data. The fourth section shows the econometric results. And lastly, the fifth section concludes and discusses possible interpretations of the results.

2. Background

After a centralized military dictatorship that lasted between 1939 and 1975 and that imposed a uniform Castilian-centered Spanish identity, the Spanish government accepted in 1977 the restoration of the historical institution of self-government in Catalonia, the *Generalitat*. The Spanish institutions and the voters in a national (Spanish) referendum approved a democratic Constitution in 1978 that recognized the right of the regions and “nationalities” to elect their own parliaments and governments with responsibilities that included the protection of languages and cultures. In this context, Catalonia elected its own Parliament since 1980 and had its own autonomic government, with important responsibilities in education, health, language protection and police among others. Between 1980 and 2003 the ruling coalition in the Catalan government had been the center-right Convergence and Union (CiU) under the leadership of Jordi Pujol, a charismatic politician. Although in all this period there had been tensions between the central and the Catalan government, the institutional framework was not questioned.

In 2003, CiU lost the regional election for the first time and was replaced by a three-party left wing government led by the former Socialist mayor of Barcelona, Pasqual Maragall, and which included the left-wing pro-independence nationalists of ERC. This government, with the conditioned support of the opposition CiU, promoted the revision of the institutional framework by means of a reform of the Catalan statute of autonomy that resulted from the

1978 Constitution. The reformed statute was approved by the Spanish parliament under a Socialist majority in 2006 with the vote against it of the opposition conservative Popular Party. This party promoted an appeal of the reformed statute in front of the Constitutional Court, and this Court ruled in 2010 that some of the articles of the statute were unconstitutional, after the Catalan voters had approved the reformed statute in a referendum in 2006. Because of the Constitutional Ruling, hundreds of thousands of people took to the streets in massive demonstrations, the nationalists recovered the political initiative, and CiU went back to power in 2010 as the largest minority now under the leadership of Artur Mas. In his first two years in power, Mas followed a pragmatic approach trying to reach agreements with the Catalan branches of the Socialist Party and the Popular Party. But in autumn of 2012, under pressure from citizens' protests as a result of budget cuts and corruption scandals that coincided with the huge impact of the global recession in Spain, he called a snap election under the promise to promote a "national transition" towards an "own state" with the objective of obtaining an overall majority, which he failed to obtain. After the election, he reached an agreement to obtain the external support of ERC, then the second party in the Catalan Parliament.

Since then on, the secessionist movement explicitly received the support of the Catalan government, promoting two important calls for consultation to decide about the independence: the referendum of 9 November 2014, (aka 9N) and the "plebiscitarian elections" (as they were called by Artur Mas) of 27 September 2015 (aka 27S). However, the 9N was declared unconstitutional by the Spanish government nine months before, on February 2015. Despite that, 2.3 millions of voters (37% of the electorate) went to vote, of which 1.86 million (81%) expressed their agreement with the Catalonia independence. Almost one year later, in 27 September 2015, a regional parliamentary election was held in Catalonia, which was taken as a plebiscite for independence by the political coalition *Junts pel Sí*, which was made of CiU and ERC, and subsequently counting on the support of the CUP. Even though they obtained an absolute majority in parliament seats, the pro-secession group did not get it in votes (47,8%: *Junts pel Sí* obtained 39.59% and an anti-EU and anti-capitalist pro-independence group, CUP, obtained 8.21%), which led to conflicting interpretations of the results by both for and against the independence, and brought about an uncertain political climate in the following weeks. However, the interpretation that

apparently ended up prevailing was the failure of the pro-independence movement to reach their own (“plebiscitarian”) objective. But they had enough parliamentary seats to support a new regional government. Accompanying the 9N and 27S processes, street demonstrations have been also held every year on September 11th (The National Day of Catalonia, aka *Diada* in Catalan) as another way of expressing discontent with the current political status of Catalonia in Spain. Unlike prior years, *Diadas* have been increasingly more intense since 2010, and their magnitude has shocked to all analysts in Catalonia and Spain.

Along with the strengthening of the pro-independence movement, leading businessmen have gotten involved into the debate, some of them apparently taken a position in favor of the movement. Three cases have been widely covered by media. Salvador Alemany, the president of *Abertis Infraestructuras* since 2011, company involved in the business of roadways and telecommunications infrastructure, has publicly expressed to agree with the independence of Catalonia. Also, Alemany has been very close to Artur Mas (president of the Catalan government and leader of the pro-independence movement), even participating as an advisor for the Catalan government. *Grifols* is another Catalan company, in the pharmaceutical sector, that through its CEO, Víctor Grífols, has publicly expressed his support to Artur Mas. Similarly, it is also publicly known that some advisors of *Endesa*, company in the electrical and gas sector, are regarded independentist by diverse analysts. However, as the date of the 27S plebiscite in 2015 approached and the movement became radicalized, several big companies such as *Caixa Bank*, *Banco Sabadell*, *Acciona* and *Repsol* expressed publicly to disagree with the secession of Catalonia. Even the aforementioned *Abertis Infraestructuras* and *Endesa*, that had largely considered pro-independentist, joined the concerns of those firms and pointed out the need of moderation of the independence movement.

In early 2016, after the apparent failure of the 27S plebiscite, Artur Mas was unable to maintain the majority in the Catalan Parliament, and then was removed from office. However, independentist political forces are still in power in Catalonia and receive large support by voters, thus tension over sovereignty is expected to continue much longer.

3. Empirical Strategy

3.1. Hypotheses

Any event study is based on the idea that the price of an asset reflects all available information, this is the so-called *efficient market hypothesis*. As Chen & Siems (2004: 349) point out, “prices of individual stocks reflect investors’ hopes and fears about the future, and taken in aggregate, stock price movements can generate a tidal wave of activity”. By analyzing the reaction of firms’ stock returns, the case of Catalonia enables to test some interesting hypotheses about the effect of independence movements on economic activity in highly consolidated democracies.

First and foremost, we test if the regular advances and setbacks of the movement have had some effect on firms’ stock returns. With respect to the advances, an eventual separation of Catalonia may be interpreted by investors as having negative consequences for firms highly exposed to the Catalan economy. This is so as the independence may bring up some costs associated to changes in the rules in terms of fiscal, trade, migration and investment policies, a possible tax increase to finance the transition costs, the status of Catalonia in the European agreements, among others. Thus, we firstly propose the following hypothesis to be tested. If the Catalan pro-independence movement is perceived as credible by investors: *Hypothesis 1: Events that demonstrate progress of the Catalan pro-independence movement have a negative effect on stock returns of firms more exposed to such events.*

By contrast, there are some events that slowdown or block the movement. In this scenario, the permanence of Catalonia within the same institutional framework is strengthened (i.e. status in Spain, the European Union, current trade agreements). As all remains the same, this news is therefore expected not to have any effect on firms’ stock returns. This hypothesis is stated as follows: *Hypothesis 2: Events that slow or block the progress of the Catalan pro-independence movement do not have any effect on stock returns of firms more exposed to the Catalan economy.*

As mentioned in previous section, the pursuit of a legal consultation for reaching the independence of Catalonia has been accompanied by street demonstrations. Although these street demonstrations have not been spontaneous (they are celebrated yearly on 11th September), they have been unpredictable in terms of their magnitude. In addition to meaning an advance of the independence movement, these *de facto* power demonstrations can be considered by investors as a signal of strong citizen discontent with the political situation and

therefore a destabilizing factor of the current order. Thus, in line with Acemoglu et al. (2014), we consider that more intense street protests are associated with lower stock market valuations: *Hypothesis 3: Street demonstrations have a negative effect on stock returns of firms more exposed to the Catalan economy.*

Finally, we can interpret the headway of the independence movement as a political strengthening of the nationalist coalition that has led this process, regardless of whether Catalonia finally reaches the independence or not. Thus, relying on the assumption that firms in favor the movement may have a preferential treatment by government (the Catalan government, in this case), we expect that these companies present positive stock returns when events happen that show advance of the independence movement: *Hypothesis 4: Events that demonstrate progress of the Catalan pro-independence movement have a positive effect on stock returns of firms if they have expressed to be in favor of the movement.*

3.2. Data

3.2.1. Political Events

We counted 31 piece of news related to the Catalan pro-independence movement between 1 January 2010 to 31 December 2015, the period of analysis of this research (see Table 1). As mentioned, these events can be broadly classified in two categories: 1) Pro-independence (19 events), namely events that means an advance of the independence process; and 2) Against independence (10 events), that is events that either slowdown or block the process. Nevertheless, these two categories do not take into account all nuances concerning to a long-standing independence process. For that reason, we go into a more detailed analysis by classifying pro-independence events in two sub-categories: 1a) politicians' announcements (13 events) about the feasibility of carrying out the consultations (the 9N and the 27S) and 1b) street demonstrations (6 events).

In addition, the specific days when the 9N referendum and the 27S plebiscite took place are treated separately as they are regarded neither pro nor against independence. The 9N referendum, as explained in Section 2, was a non-binding consultation as it was declared unconstitutional by the Spanish government, thus its effect on the stock market is expected to be null. Also, the results of the 27S plebiscite were quite controversial, making its expected consequences very difficult to disentangle by investors and analysts.

3.2.2. Economic Data

Using the Datastream database, we construct daily stock returns for the period of analysis (1 January 2010 to 31 December 2015) for firms registered in the Barcelona Stock Market. This is therefore our dependent variable. Additionally, we collect yearly information on assets and leverage (total debt over assets) in order to control for the size of firms, and we also include the Morgan Stanley Capital International (MSCI)-world index to control for the effect of changes or volatility in the stocks of developed markets in the world on the Spanish stock market. Considering the availability of information, the final sample is of 154 firms, which will be classified into two different portfolios according to the degree of exposure to Catalonia.

3.2.3. Portfolio of Catalan Firms

To define a portfolio of firms exposed to the Catalan economy and, therefore, to the risk arising from the Catalan separation movement, one could use firms headquartered in Catalonia as an approximation of such exposure. Nevertheless, as Abadie & Gardeazabal (2003: 122) point out, a classification that relies solely on companies' registered addresses is problematic as registered addresses are sometimes chosen for convenience reasons and do not necessarily imply that the firm has a relevant presence in the region.

Recognizing that registered addresses did not properly capture the economic interest of companies in a specific geographical area, and given that information about economic activities of firms in Catalonia is not available, we resort to an alternative measure based on Google searches. A recent literature has taken advantage of internet search engines such as Google to measure some phenomena. For instance, Baker et al. (2012) and Donadelli (2015) propose google-search-based policy uncertainty indexes for the US, relying on the volume of internet searches based on some key words.

In the same vein, we propose therefore an indicator of the association between firms and Catalonia based on the volume of Google searches. By doing three distinct searches combining the name of the firm and the word "Catalonia" in three different languages (English, Spanish and Catalan), we found an approximation of the extent of association between firms registered in the Barcelona stock market and Catalonia. The results are shown in Table 2 at the annex. Firms are then splitted into two groups according to the average of

the Google search indicator: those above the average are considered more exposed to Catalan economy (84 firms), and those below are less exposed (70 firms). As seen in Table 3, 14 out of the 84 firms headquartered in Catalonia are above the average of the indicator whereas 13 out of the 70 are not.

3.2.4. Firms in Favor of the Pro-independence Movement

We say a firm is in favor of the pro-independence movement if at least one of its top officers has publicly stated in that way. This approach is closed to literature on political connections (see Fisman, 2001; Khwaja and Mian, 2005; Mian, 2005; Voth and Ferguson, 2008; Dube, Kaplan, and Naidu, 2011; Dinç, 2005; Faccio, 2006; Faccio, Masulis, and McConnell, 2006; Leuz and Oberholzer-Gee, 2006; Acemoglu, Hassan, & Tahoun, 2014; and He et al. 2015). Nevertheless, our approach differs in that we do not strictly identify if companies' officers have been involved in politics, but only if they have just taken a position respecting the pro-independence movement.

Based on news from newspapers, we identify three companies as being in favor of the movement (Table 4 at the annex shows the newspapers references of this information). These are: 1) *Abertis Infraestructuras*, 2) *Grifols* and 3) *Endesa*. Additionally, we identify seven firms that express disagreement with the movement, these anti-independence firms are: 1) *Acciona*, 2) *Almirall*, 3) *Banco de Sabadell*, 4) *Banco Popular Español*, 5) *CaixaBank*, 6) *Repsol* and 7) *Telefonica*. It is worth mentioning that these anti-independence firms publicly declared to be against the movement at the end of the period of analysis, that is during 2015, when the movement became radicalized. In fact, as the 27S approached the pro-independence *Abertis Infraestructuras* and *Endesa* joint the declaration of the anti-independence companies about the need of moderation of the movement. Nevertheless, both *Abertis Infraestructuras* and *Endesa*, as well as *Grifols*, have had a long-standing alleged pro-independence biased during the whole analyzed period.

Finally, Table 5 shows the summary statistics (means and standard deviations) of the key variables employed in the econometric analysis.

3.3. Methodology

Event studies are a widely-used methodology in financial economics to measure the economic impact of specific events on the value of firms over a relatively short time (Beaulieu et al. 2006: 8). In this paper, we use a multivariate linear regression framework like that used in He et al. (2015) and Acemoglu et al. (2014). Unlike the earlier event study literature where abnormal returns are constructed relative to a Capital Asset Pricing Model, we incorporate important controls and fixed effects that can strengthen the identification strategy.

The basic regression equation is of the following form:

$$y_{ite} = \beta_0 + \beta_1 Events_{te} + Controls_{ite} + \eta_i + \gamma_e + \varepsilon_{ite} \quad (1)$$

Where y_{ite} is daily percentage return for stock i on day t of event e . As usual in event studies, we first need to define temporal windows for e so that we can estimate the effect of such events on stock returns. A temporal window is made up of two elements: *event window* and *estimation window*. The *event window* is the day or group of days when the political event takes place and stock markets are likely to incorporate changing expectations. Including some days after the event happens is a common practice in event studies since allow us considering potentially lagged reactions and that an event may arrive late in the day when markets are closed. Thus, we check the effect of political events on stock returns across two days: the announcement day and the following trading day, the event windows is hence $[0,1]$. Secondly, the *estimation window* contains the average stock return in pre-event days and serves to identify deviations in this average stock return when comparing with the event window. In this event study the estimation window spans 20 days before the political event, that is the period $[-20, -1]$. The relatively short estimation window is because of the high frequency of salient events in Catalonia around some events under consideration.

The main independent variable, $Events_{te}$, is thus a dummy that takes on 1 in the *event window*, and 0 for the *estimation window*. Both the *event window* and *estimation window* constitutes therefore the *event period* indexed as e (Figure 1 depicts the timeline of an event study). The coefficient β_1 is thus expected to capture the effect of political events on daily

percentage return by estimating the difference in the stock returns of firms between the event and estimation window.

Furthermore, we control for firm fixed effects (η_i) and period fixed effects (γ_e). η_i absorbs the unobserved heterogeneity amongst firms, and γ_e absorbs the unobserved heterogeneity amongst event periods. Thus, the estimated coefficients report the impact of political tension on stock returns within the corresponding event periods. We further include time-variant firm controls such as yearly assets and leverage (total debt over assets) to control for the size of firms, and the MSCI-world index to control for the effect of changes in the stocks of developed markets on the Spanish stock market. In all specifications, we also allow the error term (ε_{ite}) to be correlated across time for any given firm.

Regressions are run separately for both the “Catalan” and “non-Catalan” portfolios and for each type of *aggregated* events specified in section 3.2.1 (total pro-independence, total against independence, total politicians’ announcements and total street demonstrations) so that we can test our hypotheses. Later, we run regressions for each *single* event separately so that we look at within each aggregated category of events with more detail.

Regarding the form of the regression equation, we need to bear in mind that our main independent variable can then sometimes be an aggregated measure of events for which equation 1 properly fits, but other times is a single event. In the latter case, we do not need to introduce period fixed effects γ_e in the regression equation, and given the short time period (because we analyze only one event) nor we need to use firm controls like assets and leverage as they vary yearly (i.e. they are time-invariant in this case), and therefore they are already captured by firm fixed effects η_i .

The identification assumption in this specification lies on the exogeneity of each event, which brings about an unexpected, sudden and one-time increase in political tension. Certainly, when looking at macro level, it is possible that the poor performance of the Spanish and Catalan economies since the 2008 Great Recession had triggered the Catalan pro-independence movement, which leads to an endogeneity problem by reverse causality. As Funke et al. (2015) point out, financial crisis leads to polarization and favor the political rhetoric of right wing parties. However, it is worth noting that in an event study setting, using daily data of stock returns, each event is plausibly exogenous because it is not expected that

sudden changes in the daily stock returns of firms lead to the occurrence of events related to the Catalan pro-independence movement, but it is plausibly the other way around.

Additionally, to test our fourth hypothesis, namely the existence of a differential effect on pro-independence firms, we introduce an interactive term in the equation 1.

$$y_{ite} = \beta_0 + \beta_1 Events_{te} + \beta_2 ProIndep_i + \beta_3 (ProIndep_i \times Events) + Controls_{ite} + \eta_i + \gamma_e + \varepsilon_{ite} \quad (2)$$

Where $ProIndep_i$ takes on 1 if a firm i has expressed to be in favor of the movement. In such a way, the coefficient β_3 should capture the differential effect of the events on stock returns of these firms. In this case, we run regressions only for the group of “Catalan” firms.

Additionally, we examine if there is a differential impact on anti-independence firms. Due to the fact that most of them only stated a political position against the independence during 2015 as the 27S approached and the movement become radicalized, we only analyze a possible differential effect on these firms with respect to three type of events: i) the total events related the 27S, ii) the specific 27S plebiscite, and iii) the declaration on November 9th, 2015, after the controversial results of the 27S plebiscite, by the independentist coalition *Junts pel Sí* about the beginning of the Catalan State as an independent country. Employing the same specification in equation 2, the coefficient β_3 therefore captures the differential effect of the events related to the 27S on stock returns of anti-independence firms.

Finally, we propose other specifications to consider additional results and check the robustness of our findings.

Different-length Event Windows: In event studies is common to include some days before and after the event date in order to capture both anticipated and lagged effects. Thus, for each political event we use several event windows. Specifically, we check the effect of political events on stock returns one, two and three days after the event date, that is event windows [0,1], [0,2] and [0,3]. And for considering possible anticipated effects, we employ an event window from one day before to one day after the event date, that is [-1,1], and we also consider anticipation when events still have not happened by using an event window of two previous days [-2, -1]. The *estimation window* spans 20 days before the political event, that

is the period $[-20, -1]$ for event windows $[0,1]$, $[0,2]$ and $[0,3]$, the period $[-22, -2]$ for the event window $[-1,1]$, and $[-23, -3]$ for the event window $[-2, -1]$.

Volatility: We look at the effect of the events on volatility of stock prices. According to Pástor & Veronesi (2013), there are three aspects of risk associated to political events: price risk, tail risk, and variance risk. In response to a political event considered as bad news, stock prices might drop (price risk), the price drop might be large (tail risk), and return volatility might rise (variance risk). In this case, the dependent variable y_{ite} in the equations (1) is constructed as the standard deviation of the stock returns in the estimation window -namely the days before the political event, compare it to the standard deviation of the whole event period, that is including the days of the political event. We analyze the impact on volatility related to the aggregated measures of political events as well as the single events associated to the 9N referendum and the 27S plebiscite using different-lengths event windows.

Interactions with Sectors: It is expected that political events have a differential effect over firms depending on the sector of the economic activity. In particular, He et al. (2015) point out that manufacturing firms can have a relatively high international exposure through foreign sales. Therefore, for the case of Catalonia, we can also expect that manufacturing firms are especially sensitive to political events. In this case, the regression equation is of the following form:

$$y_{ite} = \beta_0 + \beta_1 Events_{te} + \beta_2 Sector_i + \beta_3 Events_{te} * Sector_i + Controls_{ite} + \eta_i + \gamma_e + \varepsilon_{ite} \quad (3)$$

Where $Sector_i$ is a dummy that takes on 1 if the firm i belongs to the industrial sector, and β_3 captures the interaction between the political events and the industrial sector. In addition to the interaction with the industrial sector, we will also introduce interactions for firms in the financial and service sectors.

The effect of consultations: Until now, we have not analyzed the effect of the two important consultations that have taken place in the last years (the 9N referendum and the 27S plebiscite). This is so because, as explained in previous sections, these events are considered neither pro nor against the movement, thus they are not captured in the aggregated measures of political events. Hence, we separately examine the impact of these consultations looking at their effect on two dependent variables: daily stock returns and volatility, for Catalan and non-Catalan firms and using different-lengths event windows.

Alternative models: Finally, we explore a different econometric specification in order to check whether the basic findings of this research are maintained. Specifically, because we are dealing with high frequency data (daily data), the problems of time dependence in the variance can be serious. An appropriate approach to estimate the volatility of the conditional process of the variance is the GARCH (1,1) model. This model can be written as follows:

$$y_{ite} = \beta_0 + \beta_1 Events_{it} + MSCIworld_{it} + \eta_{ite} \quad (4)$$

$$\eta_{ite} \approx N(0, h_{it})$$

$$h_{it} = a_i + b_i h_{it-1} + c_i \eta_{it-1}^2$$

Where h_{it} is the conditional variance of the time variation and a_i , b_i and c_i are the coefficients of the GARCH (1,1) model. The variance equation describes how the error variance behaves. Notice that h_{it} depends on the error variance and the squared error in the preceding time period. $Events_{it}$, as in the previous OLS models, is a dummy variable equal to 1 for the date of the event and 0 otherwise.

This approach, however, presents some complications for this research. Because of this model is used for time series, this methodology is usually applied on already elaborated financial series (e.g. SP500, Ibex35, etc.) This is not the case for this paper. Thus, we need to aggregate the firm's stock returns of each portfolio. A simple way to do so is by means of an unweighted average. Also, we can weigh firms according to some characteristic such as their market value. We will try both an unweighted and weighted average to construct our Catalan firms "index" and non-Catalan firms "index" and run the GARCH (1,1) model.

4. Results

4.1. Main Results

Table 6 shows the estimated coefficients of the effect of a set of aggregated political events related to the Catalan pro-independence movement on the stock returns of both Catalan and non-Catalan firms.

H1: The effect of pro-independence events: First of all, we did not find any significant effect of the aggregated measure of pro-independence events on the stock returns of Catalan nor non-Catalan firms (Column 1). At first glance, this finding casts doubts over the credibility

of the movement by investors. When disaggregating these pro-independence events in other two categories: total politicians' announcements and total street demonstrations, we found that the former does not have any effect; however, the latter presents a significant and negative impact on both Catalan and non-Catalan firms. The effect of street demonstrations will be discussed later in hypothesis three.

When looking at the impact of each single event (Table 7), we found that though pro-independence events related to politicians' announcements do not have any affect as an aggregated measure, there are some specific effects that brought up some impacts. In the following, we discuss some of these events:

- a) **25 March 2015 – The road to unilateral declaration of independence:** Several political parties such as *Convergència Democràtica de Catalunya*, *Esquerra Republicana de Catalunya*, *Asamblea Nacional Catalana*, *Omnium Cultural*, and *Associació de Municipis per la Independència* stated that the unique road of the Catalan sovereignist process is a unilateral declaration of independence, which will be done in the next 18 months if the sovereignist parties won the elections scheduled for September 27, 2015 (the 27S plebiscite). This announcement produced a slight but statistically significant decreased in the stock returns on Catalan firms (-0.02 percentage points), and in non-Catalan firms (-0.004 percentage points) (Table 7 event number 4).
- b) **27 September 2014 - Official call for 9N referendum:** Artur Mas signs a decree calling to vote for November 9th referendum. The same day, the Spanish Government initiates procedures in order to declare it unconstitutional. Apparently, this event was perceived as positive by the stock market. As shown in Table 7 event number 12, it caused a positive reaction on stock returns of Catalan firms. The impact, however, is quite small, about 0.01 percentage points.
- c) **26 June 2013 - National Pact for the “Right to Decide”:** On this day took place in the Parliament of Catalonia the constituent meeting of the National Pact for the “Right to Decide” in favor of the right to self-determination in Catalonia, formed by several parties, unions and associations. This event was also perceived as having a positive effect on stock return. The impact is about 0.01 percentage points for Catalan firms and 0.007 for non-Catalan.

H2: The effect of anti-independence events: On the other hand, contradicting our hypothesis two, events against the pro-independence movement have a negative effect of -0.003 percentage points on stock returns of Catalan firms (Column 2). We then examine with more detail each event in order to find what is bringing about this negative impact. Some of these events are the following:

- a) **25 February 2015 - Unconstitutionality of the 9N referendum:** The Spanish Constitutional Court unanimously confirmed as unconstitutional the consultation celebrated four months ago, in November 9th, 2014. The so-called 9N referendum was already declared as unconstitutional, therefore the date of the consultation voters already knew that it was a non-binding referendum. The “confirmation” of such unconstitutionality announced on 25 February 2015 seemingly had a negative effect on Catalan firms (-0.0156 percentage points) and non-Catalan (-0.00933 percentage points) (Table 7 event number 5).
- b) **20 September 2012 - Discussion about Catalan tax system:** The Spanish Prime Minister, Mariano Rajoy, refuses the request of the Catalan president Artur Mas to improve the tax system Catalonia. This event produced a slight but significant decreased in the stock returns of both Catalan (-0.00407 percentage points) and non-Catalan firms (-0.00531).
- c) **28 June 2010 – Cutting the autonomy of Catalonia:** A resource of the conservative Popular Party in which the Spanish Constitutional Court partially cuts the autonomous status of 2006 and left without legal effect the chapter that describes Catalonia as a “nation” The stock market reacted to this Spanish government’s declaration, Catalan firms reduced stock returns in -0.0134, and non-Catalan firms in -0.00773.

H3: The effect of street demonstrations: We found a significant and negative effect of street demonstrations on the stock return on Catalan firms, and also on non-Catalan firms (Column 3). This result confirms our hypothesis three and suggests that these manifestations of *de facto* power have impacts even out of Catalonia. Although the aggregated impact of street demonstrations is negative on both portfolios, it is worth mentioning that not all of them were significant nor negative. For instance, at the outset of the movement, the demonstrations

celebrated on July 10th, 2010 and 11th September, 2011 had a positive effect on Catalan firms. However, as time passes the effect of these demonstrations become negative.

H4: The effect of taking a position in favor of the movement: Table 8 shows the estimated coefficients for the interaction between aggregated political events and “pro-independence” firms. It is interesting to note that though the aggregated measure of pro-independence events is insignificant (Table 6 Column 1), there are some significant effects when interacting with pro-independence firms (Table 8 Column 1). Specifically, we found that pro-independence firms present higher stock returns in comparison to the remainder firms. Disaggregating events, we can see that the politicians’ announcements events (Table 8 Column 2) rather than street demonstrations (Table 8 Column 3) are the ones that produces the interaction. In other words, when politicians’ announcements happened, firms in general did not react, but those firms that are apparently politically connected presented an increment in the stock returns 0.003 percentage points higher than the stocks of non-connected firms. Furthermore, politically connected firms seemingly reacted positively to events against the movement, suggesting that this firms benefit from the political tension in general, no matter whether the movement goes forward or backward.

Finally, with respect to anti-independence firms, we examine the interaction of these firms with the events related to the 27S plebiscite. As shown at Table 9, we did not find any significant effect, which suggest that, unlike pro-independence firms, being anti-independence does not entail a differentiated effect.

Magnitudes compared to other studies: The impacts found in this study seem economically small. Both for total street demonstrations and total events against the movement the effect is about -0.003 (Table 6). Similarly, the effects of the single events (Table 7) and all other specifications are not far from these values. When comparing these findings to other studies, we confirm that the effects are in fact larger in other case studies. For instance, Beaulieu et al. (2006) find a positive impact for Quebec firms around 0.2 to 0.6 percentage points. He et al. (2015) report an effect of 2.03 percentage points for Taiwanese firms. Berkman et al. (2011) find a 0.12 percentage points reduction in monthly world stock returns. Fisman et al. (2014) report that the Nikkei 225 Index fell following an adverse shock to Sino-Japanese relations in 2005 by cumulatively 6.1% over the following month. Studies focusing

exclusively on the effects of violent conflicts also document larger responses. Wolfers & Zitzewitz (2009) estimate a 1.50% decline in U.S. stock price in the run-up of the Iraq invasion in 2003, and Zussman & Zussman (2006) relate assassinations of senior political targets to a 0.71 - 1.11% daily drop in the Israeli stock index.

4.2. Additional Results

Different-length windows: Table 9 shows the estimated coefficients of the event studies using different-length event windows. We only found results for the aggregated measures of events against the movement and street demonstrations.

- a) Against independence: The effect spans over all windows around the date of the event. With respect to non-Catalan firms, there seem to be a negative effect only in the event window $[0, 2]$ and in the windows of anticipated effects, $[-1, 1]$ and $[-2, -1]$. The magnitude of this impact is about 0.002 and 0.004 percentage points.
- b) Street demonstrations: The negative effect for both portfolios spans over several days later and it starts one day before the date of the events. This impact is a drop about 0.003 percentage points for Catalan firms and 0.002 for Spanish firms.

Volatility: Table 11 shows the estimated coefficients for the effect of the political events under analysis on volatility of the stock returns of Catalan and non-Catalan firms using different event windows. There is no evidence about increased volatility related to pro-independence events, neither politicians' announcements nor street demonstrations. The lack of an increment in volatility reinforces the idea of the small impact of pro-independence events on stock market. On the other hand, it is worth noting that events against the movement seemingly did produced increased volatility for non-Catalan firms, this volatility spans over some preceding and posterior days with respect to the event day (Column 4).

Interactions with sectors: We only found significant interactions between street demonstrations and sectors. Table 12 shows that the negative effect of total street demonstrations on stock returns of Catalan firms is stronger on industrial firms, having a reduction of 0.004 percentage points more than the rest of the firms (Column 1). On the other hand, the negative impact of street demonstrations seems to be less strong for firms in the financial sector, this is 0.004 percentage points less negative than the rest of the firms

(Column 2). Finally, there is no interaction with firms in the service sector (Column 3). In conclusion, stock returns of industrial firms seem to be more sensitive to street demonstrations than firms in other sectors. However, we must not forget that the magnitude of such effect is economically very small.

The effect of consultations: Table 13 shows the coefficients of the effect of the 9N referendum and the 27S plebiscite on two dependent variables: daily stock returns and volatility. We found that the 9N referendum had a positive impact on Catalan firms (Column 1) and, at the same time, brought about a reduction in volatility in Catalan firms (column 3) in the days around the event. The effect of the 9N for non-Catalans is almost null.

On the other hand, we found that the 27S plebiscite did not have any effect on Catalan firms, but it did have a positive impact on non-Catalans (column 2). This impact spans over all event windows, both before and after the event took place. In addition to increasing the stock returns of non-Catalan firms, the 27S apparently also caused an increment in volatility (column 4). These findings suggest that the 9N, the non-binding consultation, was seen as having positive effect on Catalan firms and reducing uncertainty. By contrast, though the 27S presented a positive impact on non-Catalan firms, the uncertainty seemingly increased, which is consistent with the unclear results arising from this consultation.

Alternative models: As explained at the end of Section 3.3, applying a GARCH model requires to aggregate the portfolio of firms in a single index. To do so, we construct two indexes, one for Catalan and another for non-Catalan firms, by means of an unweighted average of the firms' stock returns. Figure 2 in Annex plots the evolution of the daily stock returns of these two portfolios from 2010 to 2015. As we see, the stock returns of both portfolios seem to be characterized by random, rapid changes and are said to be volatile. The volatility seems to change over time as well. Additionally, because the Lagrange Multiplier (LM) test shows a p-value of 0.0049 in the case of the Catalan portfolio, which is well below 0.05, we reject the null hypothesis of no ARCH (1) effects for this portfolio. By contrast, the LM test for the non-Catalan portfolio shows a p-value of 0.6675, thus we cannot reject the null hypothesis of no ARCH (1) effects in this case.

Table 14 shows the estimated coefficients of the GARCH (1,1) specification. This model did not provide any significant result, which underpin the idea that, in general, the effect of the

Catalan pro-independence movement on the stock market in both Catalonia and Spain has been insignificant. There could have been some effects related to street demonstrations, events against the process, and the 27S plebiscite, as well as some effects in volatility, as shown with the OLS models, but these effects are very small and they are not captured by a GARCH (1,1) specification. We also run the same model using a weighted average of firms according to their market value, but the result does not change significantly (not reported).

5. Final Discussion

In this paper, we provide evidence that the Catalan pro-independence movement, despite being in the top of the political agenda of both Catalonia and Spain in the last years, has not had an economically significant impact on the stock returns. Nevertheless, there are some statistically significant effects on both Catalan and non-Catalan firms that deserve some interpretations.

First and foremost, the lack of large economic effects can be a signal of lacks of credibility among investors about an eventual secession of Catalonia. That is, though the Catalan pro-independence movement has shown to be particularly strong in the last years, investors perhaps think that the sturdy institutional framework in which this conflict has been developed -i.e. a modern democracy that belongs to a stable institutional structure such as the European Union-, will ultimately lead to Catalonia and Spain to find a reasonable solution of this conflict. Additionally, it could also be that though investors perceive some negative consequences from the institutional instability because of a possible secession, they also perceive that the movement by itself, led by center-right politicians which historically had advocated business interests, was only a way to counter-balance the pressure of left-wing populist movements that strongly emerged in Catalonia and Spain around the times of the 2008 global financial crisis. In all of these interpretations, investors might be considering that, sooner or later, the intentions of secessionism will fade.

Despite this economically small impact, firms seem to negatively react to some political events. First, street demonstrations affected negatively the stock returns of both Catalan and non-Catalan firms, and this effect is a bit greater for Catalan industrial companies. This finding suggests that, regardless of the credibility of the movement, firms react to street demonstrations, or the “power of the street” as it is called by Acemoglu, Hassan, & Tahoun

(2014). However, we must bear in mind that the magnitude of this effect is still small. Second, there also seem to be a negative effect related to the events against the movement on stock returns of both Catalan and non-Catalan firms. A possible interpretation is that although these events were supposed to slow or block the process, they were perceived as counterproductive. That is, they could have been perceived as fueling the Catalan pro-independence movement, which would lead to a strong reaction by the impellers of the independence to these perceived grievances coming from the central Spanish government.

Another interesting result has to do with taking a position in favor of the movement. Apparently, those firms that have publicly expressed to be in favor of the independence process present higher stock returns. These effects are related to both politicians' announcements that show advance of the movement and events against the movements. This suggest that politically connected firms benefit from political tension in general, whether the political event means a progress or a slowdown of the movement, perhaps because both type of events are interpreted by investors as a strengthening of the incumbent party and coalitions that are propelling the movement. However, when street demonstrations take place, there is no a differentiated effect between pro and neutral firms, but this type of event affects all companies alike.

We view our results as a first attempt to analyze the complexity of a secessionist movement by taking into consideration a wide and diverse set of political events. Several questions of course remain unresolved. First, despite the supporting evidence we provide, an area for future work is to exploit other empirical designs, such as natural experiments or other sources of potentially exogenous variation, in order to give even more compelling estimates of causal effects. Second, another important area for future work is to thoroughly inquire into political connections of firms, since there could be differential effects according to the extent of connection with the political parties that are leading the movement. And finally, although we make no claim to external validity beyond Catalonia and Spain, we expect that these findings can be useful for feeding an informed debate about the current secessionist movements over the world.

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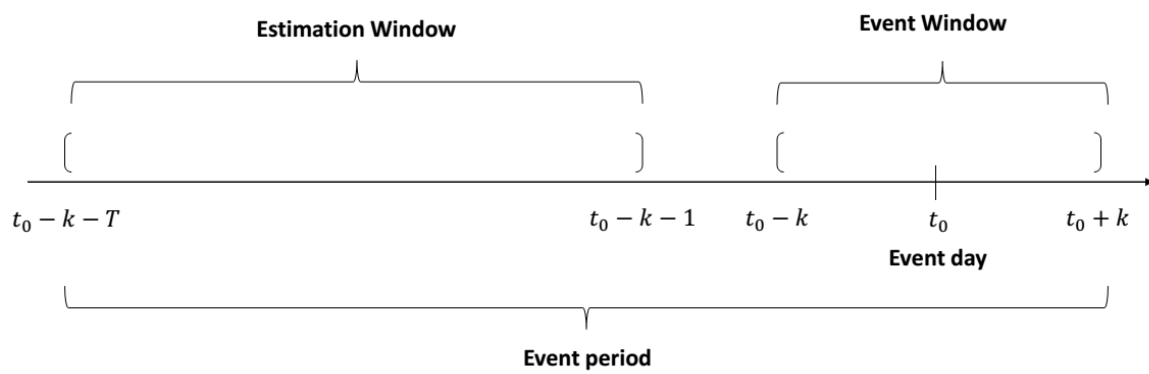
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Annex

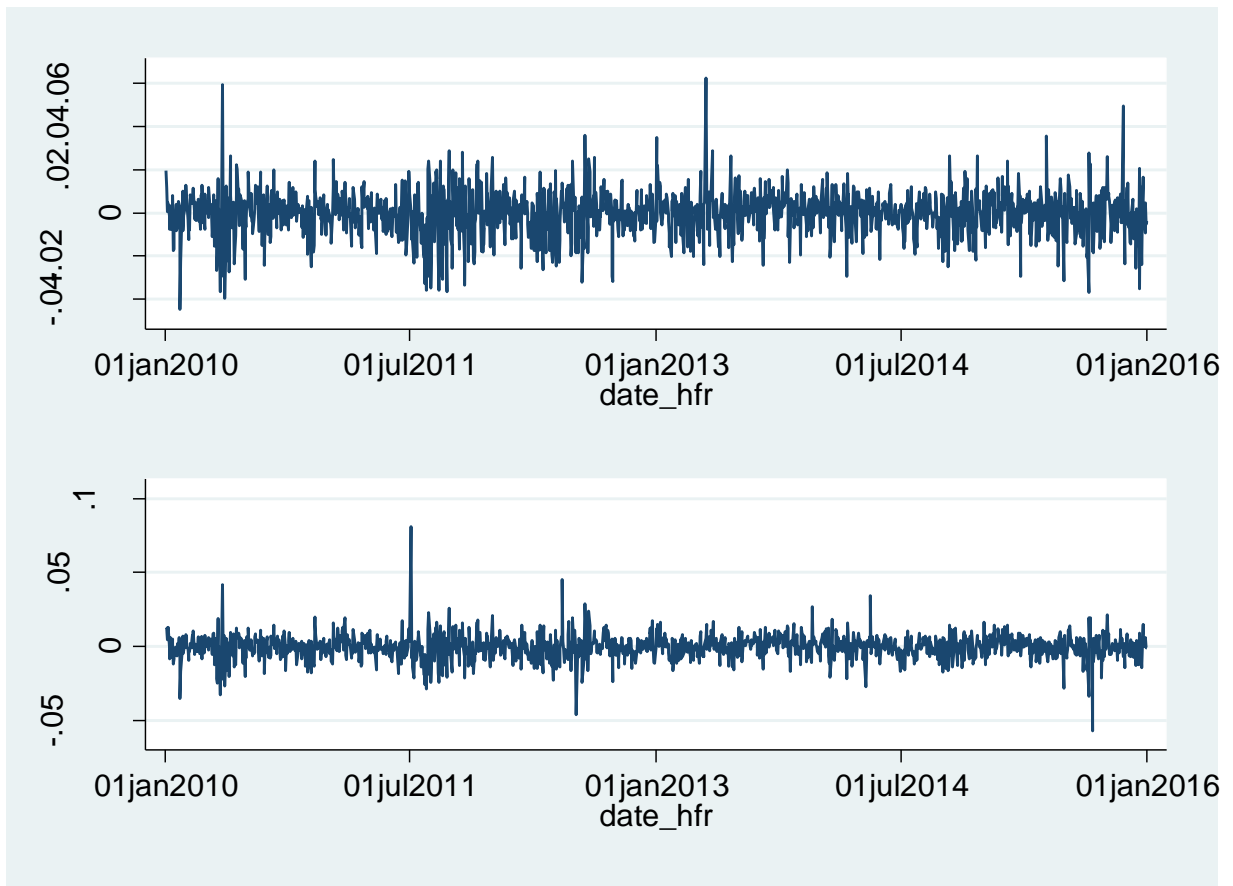
Figures

Figure 3: Timeline for Event Study



Source: Adapted from Guidolin & Ferrara (2005).

Figure 2: Stock Returns of Catalan and Non-Catalan Portfolios



Tables

Table 1: Typology of Events Related to the Catalan Pro-independence Movement from 2010 to 2015

Events	Description	Number
<i>Total events pro</i>		
Total politician's announcements	Announcements about the feasibility of carrying out the consultations (the 9N and the 27S)	13
Total street demonstrations	Celebrated every year in September 11 th the Catalan National Day or <i>Diada</i> .	6
<i>Total events against</i>	Total events concerning the impossibility of carrying out key events related to independence such as 9N, 27S, and also the Jordi Pujol's corruption scandal.	10
<i>Unclassified events</i>		
The 9N referendum	Non-binding consultation declared unconstitutional by the Spanish government	1
The 27S plebiscite	Unclear results	1
Total		31

Table 2: “Catalan” and “non-Catalan” Portfolios based on Google Search-Base Indicator

Firm	Headquarters (Catalonia=1)	Google search total	Ln (Google search total)	Catalan firms (Google search indicator > average)
SERVICE POINT SOLUTIONS SA	1	96500000	18.38505	1
PRIM SA	0	17313000	16.66697	1
ALFA SAB DE CV	0	13890000	16.44668	1
LIBERTAS 7 SA	0	5350000	15.49261	1
INMOBILIARIA DEL SUR SA	0	3319000	15.01517	1
BANCO SANTANDER RIO SA	0	2188000	14.5985	1
AMERICA MOVIL SAB DE CV	0	2077000	14.54644	1
BBVA BANCO FRANCESSESA	0	2054000	14.5353	1
BANCO SANTANDER SA	0	1959000	14.48794	1
BAYER AG	0	1717000	14.35609	1
RONSA SA	0	1448000	14.18569	1
MIQUEL Y COSTAS & MIQUEL SA	1	1437000	14.17807	1
PROMOTORA DE INFORMACIONES SA	0	1289000	14.06938	1
BANCO BRADESCO SA	0	1251000	14.03945	1
CAIXABANK SA	1	1232000	14.02415	1
ABENGOA SA	0	1188300	13.98803	1
ACCIONA SA	0	1181600	13.98238	1
TR HOTEL JARDIN DEL MAR SA	0	1158000	13.9622	1
NH HOTEL GROUP SA	0	1097800	13.90882	1
TELEFONICA SA	0	1095000	13.90626	1
INTER. CONSOLIDATED AIRLINES GROUP SA	0	1081000	13.8934	1
UNION EUROPEA DE INVERSIONES SA	0	1028000	13.84313	1
VOCENTO SA	0	1004000	13.8195	1
BANCO POPULAR ESPAÑOL SA	0	977000	13.79224	1
ENDESA SA	0	973000	13.78814	1
BARON DE LEY SA	0	935000	13.7483	1
REPSOL SA	0	931600	13.74466	1
MAPFRE SA	0	914000	13.72559	1

RED ELECTRICA CORPORACION SA	0	895000	13.70458	1
BANKINTER SA	0	892400	13.70167	1
INDUSTRIA DE DISEÑO TEXTIL SA	0	867000	13.67279	1
APPLUS SERVICES SA	1	861000	13.66585	1
ATRESNEDIA SA	0	861000	13.66585	1
RENTA 4 BANCO SA	0	861000	13.66585	1
AENA DESARROLLO INTERNACIONAL SA	0	857000	13.66119	1
SOTOGRADE SA	0	855000	13.65886	1
CARTERA INDUSTRIAL REA SA	0	848000	13.65064	1
ALMIRALL SA	1	805900	13.59971	1
FERROVIAL SA	0	785800	13.57446	1
UNION CATALANA DE VALORES SA	1	784800	13.57318	1
AMPER SA	0	757000	13.53712	1
BANCO DE SABADELL SA	1	756800	13.53685	1
DEOLEO SA	0	747000	13.52382	1
AMADEUS IT HOLDING SA	0	743000	13.51845	1
BANKIA SA	0	716500	13.48213	1
SACYR SA	0	708300	13.47062	1
ENAGAS SA	0	698900	13.45726	1
CORPORACION GEO SAB DE CV	0	672000	13.41801	1
GENERAL DE ALQUILER DE MAQUINARIA SA	0	644000	13.37545	1
URBAS GRUPO FINANCIERO SA	0	632000	13.35664	1
EBRO FOODS SA	0	608000	13.31793	1
CIE AUTOMOTIVE SA	0	587000	13.28278	1
IBERDROLA SA	0	550000	13.21767	1
NICOLAS CORREA SA	0	548000	13.21403	1
GRIFOLS SA	1	527200	13.17534	1
ARCELORMITTAL ESPAÑA SA	0	521000	13.16351	1
TALGO SA	0	515000	13.15192	1
TUBOS REUNIDOS SA	0	509700	13.14158	1
GRUPO CATALANA OCCIDENTE SA	1	494000	13.11029	1
CAJA DE AHORROS DEL MEDITERRANEO	0	490000	13.10216	1

TECNICAS REUNIDAS SA	0	490000	13.10216	1
LIBERBANK SA	0	488630	13.09936	1
VOLCAN COMPAÑIA MINERA SA	0	482000	13.0857	1
INDRA SISTEMAS SA	0	473000	13.06685	1
ADOLFO DOMINGUEZ SA	0	471800	13.06431	1
CORPORACION FINANCIERA ALBA SA	0	465700	13.0513	1
MEDIASET ESPAÑA COMUNICACION SA	0	441500	12.99793	1
BRADSPARSA	0	434000	12.9808	1
PESCANOVA SA	0	417200	12.94132	1
CEMENTOS MOLINS SA	1	390200	12.87441	1
FLUIDRA SA	1	380600	12.8495	1
NATRA SA	0	367200	12.81366	1
DISTR. INTER. DE ALIMENTACION SA	0	365300	12.80847	1
BRASKEMSA	0	361500	12.79802	1
CODERE SA	0	350900	12.76826	1
LABORATORIO REIG JOFRE SA	1	347300	12.75794	1
TUBACEX SA	0	347000	12.75708	1
CONST. Y AUXILIAR DE FERROCARRILES SA	0	328900	12.70351	1
FOMENTO DE CONST. Y CONTRATAS SA	1	324300	12.68942	1
COM. VINICOLA DEL NORTE DE ESPAÑA SA	0	315400	12.6616	1
ABERTIS INFRAESTRUCTURAS SA	1	303600	12.62347	1
PAPELES Y CARTONES DE EUROPA SA	0	301300	12.61586	1
ACS ACTIVIDADES DE CONST. Y SERV. SA	0	301200	12.61553	1
AZKOYEN SA	0	299600	12.6102	1
VIDRALA SA	0	295000	12.59473	0
MELIA HOTELS INTERNATIONAL SA	0	291000	12.58108	0
ERCROS SA	1	288200	12.57141	0
URBAR INGENIEROS SA	0	284400	12.55814	0
ZARDOYA OTIS SA	0	281800	12.54895	0
EDREAMS ODIGEO	0	280200	12.54326	0
REYAL URBIS SA	0	277100	12.53213	0
PROSEGUR COMPAÑIA DE SEGURIDAD SA	0	274200	12.52161	0

FUNESPAÑA SA	0	270020	12.50625	0
NATURHOUSE HEALTH SA	1	265400	12.48899	0
GRUPO EZENTIS SA	0	261500	12.47419	0
BODEGAS RIOJANAS SA	0	259600	12.4669	0
EUSKALTEL SA	0	243300	12.40205	0
ENCE ENERGIA Y CELULOSA SA	0	241400	12.39421	0
ELECNOR SA	0	235000	12.36734	0
ACERINOX SA	0	231000	12.35017	0
INMOBILIARIA COLONIAL SA	1	226100	12.32873	0
LA SEDA DE BARCELONA SA	1	216600	12.28581	0
AXIARE PATRIMONIO SOCIMI SA	0	211600	12.26245	0
RENTA CORPORACION REAL ESTATE SA	1	207600	12.24337	0
BANCO BILBAO VIZCAYA ARGENTARIA SA	0	206670	12.23888	0
CELLNEX TELECOM SA	1	203600	12.22391	0
ALZA REAL ESTATE SA	0	196100	12.18638	0
MONTEBALITO SA	0	185530	12.13097	0
AIRBUS HELICOPTERS ESPAÑA SA.	0	177500	12.08673	0
CENTRAIS ELE BRASILEIRA SA ELETROBRAS	0	171500	12.05234	0
VISCOFAN SA	0	170500	12.04649	0
BOLSAS Y MERCADOS ESPAÑOLES SA	0	155000	11.95118	0
SNIACE SA	0	153600	11.94211	0
DURO FELGUERA SA	0	145300	11.88656	0
BODEGAS BILBAINAS SA	0	135600	11.81746	0
FAES FARMA SA	0	128500	11.76368	0
LINGOTES ESPECIALES SA	0	126200	11.74562	0
OBRASCON HUARTE LAIN SA	0	121200	11.7052	0
VERTICE TRESCIENTOS SESENTA GRADOS SA	0	120600	11.70023	0
BIOSEARCH SA	0	120100	11.69608	0
GAMESA CORPORACION TECNOLOGICA SA	0	120100	11.69608	0
REALIA BUSINESS SA	0	118000	11.67844	0
HISPANIA ACTIVOS INMOBILIARIOS S A	0	109500	11.60368	0
COMP. PARANAENSE DE ENERGIA-COPEL B	0	105100	11.56267	0

ADVEO GROUP INTERNATIONAL SA	0	104220	11.55426	0
SAETA YIELD SA	0	101600	11.5288	0
APERAM SOCIETE ANONYME	0	93300	11.44358	0
USINAS SIDERURGICAS DE MINAS GERAISSA	0	91000	11.41861	0
SOLARIA ENERGIA Y MEDIO AMBIENTE SA	0	90400	11.412	0
TECNOCOM TELECOM. Y ENERGIA SA	0	88000	11.38509	0
COMPANIA GENERAL DE INVERSIONES SICAV	1	87800	11.38282	0
QUABIT INMOBILIARIA SA	0	78100	11.26575	0
CEMENTOS PORTLAND VALDERRIVAS SA	0	74800	11.22257	0
MERLIN PROPERTIES SOCIMI SA	0	70300	11.16053	0
CLINICA BAVIERA SA	0	62600	11.04452	0
LABORATORIOS FARMACEUTICOS ROVI SA	0	62400	11.04132	0
DOGI INTERNATIONAL FABRICS SA	1	60600	11.01205	0
NYESA VALORES CORPORACION SA	0	60420	11.00908	0
GAS NATURAL SDG SA	1	60000	11.0021	0
NMAS1 DINAMIA SA	0	59470	10.99323	0
LAR ESPAÑA REAL ESTATE SOCIMI SA	0	45200	10.71885	0
HULLERA VASCO LEONESA SA	0	39650	10.58785	0
INYP SA INFORMES Y PROYECTOS SA	0	37210	10.52433	0
IBERPAPPEL GESTION SA	0	36940	10.51705	0
AYCO GRUPO INMOBILIARIO SA	0	28960	10.27367	0
COM. LOGISTICA DE HIDRO. CLH SA	0	28500	10.25766	0
FERSA ENERGIAS RENOVABLES SA	1	19310	9.868378	0
TESTA INMUEBLES EN RENTA SOCIMI SA	0	19310	9.868378	0
LIWE ESPAÑOLA SA	0	9320	9.139918	0
MOBILIARIA MONESA SA	1	9260	9.133459	0
INVERPYME SA SCR	1	6550	8.78722	0
INVERFIATC SA	1	5052	8.527539	0
AGROFRUSE-MEDITERRANEAN AGRI.	0	2544	7.841493	0
EMPRESA NAL. DE ELECTRICIDAD (CHILE)	0	222	5.402677	0

Table 3: Contingency Table of Number of Firms According to Registered Address and Google Search-Base Measures

	Headquartered in Catalonia	No headquartered in Catalonia	Total Google
Google search-base measure <i>above</i> the average	14	70	84
Google search-base measure <i>below</i> the average	13	57	70
Total Headquartered	27	127	154

Table 4: Piece of News about Firm's Position about the Pro-Independence Movement

Firm	News	Date	Source
Pro-independence			
Abertis Infraestructuras	“Salvador Alemany, presidente de Abertis, asesora a Artur Mas”	20 December 2010	El Economista
	“Alemany (Abertis) cree que independencia de Cataluña es viable económicamente”	14 September 2011	La Información
	“Salvador Alemany también cree que la independencia de Catalunya es viable”	14 September 2011	Expansión Catalunya
	“La Generalidad adjudica a Abertis la gestión de las emergencias para los próximos diez años por 132,5 millones”	11 May 2013	La voz de Barcelona
Grifols	“El presidente de Grifols anima a Mas a tener ‘firmeza’ y no arrugarse por las críticas”	03 April 2014	El Confidencial
	“Grifols, un empresario subvencionado que quiere la secesión de Cataluña”	09 April 2014	ABC
	“El 'independentista' Grifols duda entre ubicar en Cataluña su próxima fábrica o llevársela a Estados Unidos”	09 June 2015	Periodista Digital
	“Grifols apoya la independencia de Cataluña... pero se marcha a Irlanda”	23 October 2015	Libre Mercado
	“Grifols pone la bandera catalana pero no la española en su sede de Texas”	24 July 2016	El Nacional
Endesa	“Nueve de los trece asesores de Endesa en Cataluña se declaran independentistas”	27 August 2014	El Confidencial
	“El consejo asesor de Endesa, plagado de independentistas”	12 September 2016	Intereconomía
Anti-independence			
Acciona	“La independencia le costaría a los Entrecanales 300 millones de euros	29 September 2015	Ok Diario
	Los grandes empresarios: ‘Queremos a Cataluña, eviten un voto de ruptura’”	18 September 2015	El Mundo
Almirall	“El presidente de Almirall previene a sus trabajadores de votar a los soberanistas”	18 September 2015	El Mundo
Banco de Sabadell	“La banca se planteará su presencia en Cataluña si hay independencia”	18 September 2015	El País
Banco Popular Español	“La banca se planteará su presencia en Cataluña si hay independencia”	18 September 2015	El País
CaixaBank	“Isidro Fainé (CaixaBank) se ‘moja’ sobre la independencia de Cataluña: ‘Mejor juntos que separados’”	31 January 2014	El Confidencial Digital
	“La banca se planteará su presencia en Cataluña si hay independencia”	18 September 2015	El País
Repsol	“El catalán Antonio Brufau, en contra de la independencia de Cataluña”	03 October 2014	Hispanidad
Telefonica	“Alierta invita a los empresarios catalanes a que ‘hagan números’ de cara al 27-S”	21 September 2015	El Confidencial
	“Alierta: «Los efectos económicos de una supuesta independencia son súper negativos»”	21 September 2015	ABC Economía
	“Pallette suprime el 'consejo político' que Telefónica mantenía en Cataluña”	12 September 2016	El Confidencial

Table 5: Summary Statistics 2010-2015

Variable	Catalan Firms			Non-Catalan Firms
	All Firms	Pro-Independence	Anti-Independence	
Number of firms	84	3	7	70
Daily Stock Return (%)	-0.03 (0.04)	0.01 (0.02)	-0.01 (0.02)	-0.04 (0.03)
Assets (Mill.)	44.7 (160)	26.6 (18.8)	97.4 (93.5)	13.8 (81.1)
Leverage	0.34 (0.24)	0.43 (0.16)	0.35 (0.12)	0.39 (0.31)

Note: The table reports means standard deviations in parenthesis

Table 6: Effect of (Aggregated) Events on Stock Returns

Event Window [0,1] Estimation Window [-20, -1]

	Daily stock returns (%)							
	Total Pro		Total Announcements		Total Street Demonstrations		Total Against	
	(1)		(2)		(3)		(4)	
	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan
Events	-0.001 (0.001)	4.41E-05 (7.26E-04)	-6.92E-04 (0.001)	7.08E-04 (9.4E-04)	-0.003*** (8.37E-04)	-0.002** (0.001)	- 0.003** (0.001)	-0.001 (9.69E-04)
Ln (Assets)	0.002 (0.002)	-0.001 (9.49E-04)	-6.78E-04 (0.001)	1.87E-04 (8.45E-04)	0.002 (0.002)	-0.002 (0.001)	6.97E-04 (0.001)	0.001 (0.001)
Leverage	-0.002 (0.002)	0.001 (0.001)	-0.004 (0.002)	0.002* (0.001)	(0.001)	-0.001 (0.002)	-0.002 (0.002)	3.65E-04 (0.002)
MSCI-world index	1.88E-04*** (2.00E-05)	1.49E-04*** (1.57E-05)	1.11E-04*** (2.69E-05)	9.46E-05*** -1.75E-05	2.77E-04*** (2.15E-05)	2.05E-04*** (2.41E-05)	3.33E-05 (4.06E-05)	9.88E-05*** (1.37E-05)
Firm fixed effects	X	X	X	X	X	X	X	X
Period fixed effects	X	X	X	X	X	X	X	X
Constant	-0.358*** (0.054)	-0.238*** (0.032)	-0.182*** (0.043)	-0.168*** (0.032)	-0.492*** (0.066)	-0.298*** (0.044)	-0.067 (0.057)	-0.182*** (0.033)
Obs.	15,119	12,768	10,479	8,831	5,444	4,631	8,990	7,731
R ²	0.018	0.02	0.012	0.016	0.053	0.033	0.011	0.029

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of Single Events on Stock Returns

Event Window [0,1] Estimation window [-20, -1]

#	Date	Type	Description	Pro-Independence		Against	Unclassified	Coefficients	
				A	D			Catalan	Non-Catalan
1	9 November, 2015	A	Junts pel Sí and the CUP approved a resolution that declares the beginning of the process for creating an independent Catalan state.	X				0.022 (0.028)	-0.004** (0.002)
2	27 September, 2015	C	Catalan Parliament Elections. The coalition Junts pel Sí and Candidatura de Unidad Popular (CUP) raised this election as a plebiscite about the independence of Catalonia.				X	-8.04E-04 (0.012)	0.010*** (0.002)
3	11 September, 2015	D	Diada: The celebration of the "Via Libre to the Catalan Republic" organized by the platform "Ara es l'hora" (Now is the time).		X			-0.009*** (0.001)	-0.001 (0.003)
4	25 March, 2015	A	Convergència Democràtica de Catalunya, Esquerra Republicana de Catalunya, Asamblea Nacional Catalana, Omnium Cultural, and the Associació de Municipis per la Independència, declared that the unique road of the Catalan sovereignist process is a unilateral declaration of independence, which will be done in the next 18 months if the sovereignist parties won the elections [plebiscite] scheduled for September 27, 2015.	X				-0.021* (0.012)	-0.004** (0.002)

5	25	A	The Constitutional Court unanimously confirmed the unconstitutionality of the consultation 9-N.	X	-0.015** (0.006)	-0.009** (0.004)
6	14	A	Artur Mas announced that the regional elections on 27 September 2015 would have a plebiscitary character on the independence of Catalonia	X	2.93E-04 (0.002)	7.6E-04 (0.001)
7	13	A	In response to the Artur Mas' declaration on November 25th, 2014, about the plan of reaching the independence of Catalonia in 18 months, the Popular Party (incumbent party in Spanish government) states that the sovereignty and the Spain Unit are not negociables, therefore Artur Mas cannot change the constitution.	X	0.012*** (0.003)	0.012** (0.005)
8	25	A	Artur Mas presented his plan for Independence of Catalonia in 18 months.	X	-0.004 (0.004)	-0.001 (0.005)
9	21	A	The prosecution presented a complaint against Mas for the celebration of the 9-N.	X	0.011*** (0.002)	0.002 (0.003)
10	9	C	The non-binding 9N referendum on the independence of Catalonia 2014 was held without any incidents.	X	0.005** (0.002)	0.001 (0.001)
11	13	A	Artur Mas recognized that it cannot hold the 9N due to the lack of guarantees.	X	0.018*** (0.002)	0.014*** (0.003)
12	27	A	Artur Mas signed a decree officially calling the vote for 9 November (9N). The Spanish Government initiates	X	0.010*** (0.003)	0.004 (0.002)

			procedures in order to prevent.				
13	19 September, 2014	A	Faced with the denial of the referendum by the Congress of Deputies, Parliament of Catalonia passed its own law consultations by 106 votes in favor and 28 against	X		-0.008 (0.005)	0.002 (0.002)
14	11 September, 2014	D	Diada: With the objective of vindicating the "right of self-determination" a great demonstration called "Via Catalana" or just "V" took place in Barcelona	X		0.003* (0.001)	-3.07E-04 (0.002)
15	25 July, 2014	A	Jordi Pujol, one of the most influential politicians of the Catalan nationalism in the 80s, acknowledged that had hidden abroad a fortune to the Treasury for 34 years.	X		0.001 (0.001)	5.16E-04 (0.002)
16	8 April, 2014	A	The referendum request was rejected by the plenary of Congress of Deputies.	X		-2.68E-04 (0.006)	-0.005 (0.003)
17	14 March, 2014	A	Negative judgment on the "sovereignty" and the "right to decide" in Catalonia by the Constitutional Court.	X		0.011*** (0.003)	7.46E-04 (0.003)
18	16 January, 2014	A	The Catalonia's parliament approved a motion requesting that the Congress of Deputies the transfer of the competency for the referendum.	X		0.005 (0.007)	0.005* (0.003)
19	12 December, 2013	A	A majority of Catalan political parties approved a referendum for independence on November 9, 2014 (9-N).	X		0.002 (0.002)	0.004 (0.004)

20	11 September, 2013	D	Diada: With the aim of vindicating the independence of Catalonia, independentists went to street for carrying out a demonstration called “Via Catalana towards Independencia”, also known as “Via Catalana”	X	-0.011*** (0.002)	-0.002 (0.002)
21	26 June, 2013	A	The National Pact for the Right to Decide in favor of the right to self-determination in Catalonia took place in the Parliament of Catalonia.	X	0.017*** (0.002)	0.007* (0.004)
22	8 May, 2013	A	Precautionary suspension of the Declaration of Sovereignty and the "right to decide" by the Constitutional Court	X	-3.38E-04 (0.009)	0.007 (0.004)
23	13 March, 2013	A	The Catalan Parliament arrogates the powers to dialogue bilaterally with the Spanish Government regarding the "right to decide its sovereignty", expressing in this way its intention to negotiate a referendum on independence regardless of the rest of Autonomies and the rest of the Spanish electoral body.	X	0.003 (0.005)	2.0E-04 (0.002)
24	23 January, 2013	A	The Catalan parliament approved a declaration of sovereignty that allows the region for self-determination.	X	0.004 (0.003)	4.91E-04 (0.003)
25	26 November, 2012	A	Early elections: Artur Mas won the elections but with less support than expected, so it is forced to negotiate with the pro-	X	-0.003* (0.001)	-0.003 (0.002)

			independence left-wing ERC.			
26	27 September, 2012	A	The Catalan Parliament approved on 27 September 2012 a resolution calling for self- determination and the celebration of a referendum in Catalonia.		-3.73E-04 (0.003)	-0.003* (0.002)
27	20 September, 2012	A	The Spanish Prime Minister, Mariano Rajoy, refused the request of the Catalan president Artur Mas to improve the tax system Catalonia.	X	-0.004** (0.001)	-0.005** (0.002)
28	11 September, 2012	D	Diada: Demonstration "Catalunya, not estat d'Europe". Hundreds of thousands of people flooded the streets of Barcelona calling for independence from Catalonia. On the same day, 2012 and 2014 are similar	X	-0.001 (0.003)	9.96E-04 (0.003)
29	11 September, 2011	D	Diada	X	0.014*** (0.004)	0.002 (0.002)
30	10 July, 2010	D	The demonstration "We are a nation, we decide" was a response to an appeal by the conservative Popular Party of June 28, 2010, in which the Constitutional Court partially cuts the autonomous status of 2006 and left without legal effect the chapter that describes Catalonia as a nation.	X	0.003*** (0.001)	0.001 (0.001)
31	28 June, 2010	A	A resource of the conservative Popular Party in which the Spanish Constitutional Court partially cuts the	X	-0.013*** (0.002)	-0.007*** (0.002)

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that describes Catalonia
as a "nation"

Type: A (Announcements), D (Street Demonstrations), C (Consultations).

Source: Chronology elaborated by authors based on news and the article "Independentismo catalán" from Wikipedia.

Table 8: Effect of (Aggregated) Events on Pro-Independence Firms' Stock Returns

Event Window [0,1] Estimation Window [-20, -1]

	Daily stock returns (%)			
	Total Pro (1)	Total Announcements (2)	Total Street Demonstrations (3)	Total Against (4)
Events	-0.001 (0.001)	-9.24E-04 (0.001)	-0.003*** (8.8E-04)	-0.003** (0.001)
ProIndep.	0.002 (0.002)	7.56E-04 (0.002)	-0.003 (0.003)	0.004** (0.001)
Events x ProIndep.	0.003** (0.001)	0.004* (0.002)	6.43E-04 (0.002)	0.005*** (0.001)
Ln (Assets)	0.002 (0.002)	-6.74E-04 (0.001)	0.002 (0.002)	7.0E-04 (0.001)
Leverage	-0.002 (0.002)	-0.004 (0.002)	0.001 (0.001)	-0.002 (0.002)
MSCI-world index	1.88E-04*** (2.00E-05)	1.11E-04*** (2.69E-05)	2.77E-04*** (2.15E-05)	3.32E-05 (4.06E-05)
Firm fixed effects	X	X	X	X
Period fixed effects	X	X	X	X
Constant	-0.358*** -0.054	-0.182*** -0.043	-0.492*** -0.066	-0.067 -0.057
Obs.	15,119	10,479	5,444	8,990
R ²	0.019	0.012	0.053	0.011

Note: Regressions are run only on the "Catalan" portfolio.

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of (Aggregated) Events on Anti-Independence Firms' Stock Returns

Event Window [0,1] Estimation Window [-20, -1]

	Daily stock returns (%)			
	Total 27S (1)	27S (2)	2015 Street Demonstration (3)	Post 27S (4)
Events	-0.008 (0.006)	-0.001 (0.012)	-0.010*** (0.002)	0.026 (0.032)
AntiIndep.	-0.067*** (0.009)	0.007*** (7.35E-04)	-0.025*** (3.58E-04)	-8.02E-04 (0.004)
Events x AntiIndep.	0.008 (0.007)	0.003 (0.005)	0.005 (0.005)	-0.040 (0.032)
Ln (Assets)	0.044*** (0.004)			
Leverage	-0.048 (0.062)			
MSCI-world index	6.45E-05** (2.65E-05)	5.90E-05 (1.42E-04)	2.51E-04*** (3.91E-05)	1.48E-04*** (3.97E-05)
Firm fixed effects	X	X	X	X
Period fixed effects	X			
Constant	-0.845*** (0.131)	-0.109 (0.230)	-0.386*** (0.063)	-0.245*** (0.068)
Obs.	2,800	1,341	1,254	1,344
R ²	0.021	0.062	0.079	0.065

Note: Regressions are run only on the "Catalan" portfolio

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Effect of Events on Stock Returns using Different-Length Windows

Event window	Daily stock returns (%)							
	Total Pro (1)		Total Announcements (2)		Total Street Demonstrations (3)		Total Against (4)	
	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan
<i>Lagged effects</i>								
[0,1]	-0.001 (0.001)	4.41E-05 (7.26E-04)	-6.92E-04 (0.001)	7.08E-04 (9.4E-04)	-0.003*** (8.37E-04)	-0.002** (0.001)	-0.003** (0.001)	-0.001 (9.69E-04)
[0,2]	-0.001 (8.39E-04)	-4.65E-05 (6.25E-04)	-4.58E-04 (0.001)	4.32E-04 (7.77E-04)	-0.003*** (7.04E-04)	-0.002** (9.09E-04)	- (0.001)	-0.002** (9.75E-04)
[0,3]	-9.18E-04 (7.75E-04)	-5.45E-04 (5.52E-04)	-6.44E-04 (0.001)	-2.11E-04 (7.22E-04)	-0.002*** (6.56E-04)	-0.002** (8.43E-04)	-0.002** (0.001)	-0.001 (8.96E-04)
<i>Anticipated effects</i>								
[-1,1]	-3.16E-04 (8.6E-04)	1.54E-04 (6.29E-04)	-3.27E-04 (0.001)	3.47E-04 (8.74E-04)	-0.001* (7.54E-04)	-9.43E-04 (8.89E-04)	-0.002* (0.001)	-0.001* (7.94E-04)
[-2,-1]	0.001** (5.23E-04)	6.70E-05 (5.46E-04)	0.002*** (6.52E-04)	-2.52E-05 (7.73E-04)	-8.41E-04 (8.34E-04)	1.17E-04 (6.69E-04)	-0.002* (0.001)	-0.002** (9.9E-04)
Controls	X	X	X	X	X	X	X	X
Firm fixed effects	X	X	X	X	X	X	X	X
Period fixed effects	X	X	X	X	X	X	X	X

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 11: Effect of Events on Volatility using Different-Length Event Windows
(Estimation Window [-20, -1])

Event window	Volatility							
	Total Pro (1)		Total Announcements (2)		Total Street Demonstrations (3)		Total Against (4)	
	Catalan	Non- Catalan	Catalan	Non- Catalan	Catalan	Non- Catalan	Catalan	Non- Catalan
<i>Lagged effects</i>								
[0,1]	4.36E-04 (5.32E-04)	2.55E-04 (1.71E-04)	7.39E-04 (7.83E-04)	3.39E-04 (2.25E-04)	-1.41E-04 (1.66E-04)	1.03E-04 (2.18E-04)	3.61E-04 (2.45E-04)	7.67E-4*** (2.65E-04)
[0,2]	3.82E-04 (5.75E-04)	2.48E-04 (2.16E-04)	7.0E-04 (8.0E-04)	2.0E-04 (2.0E-04)	-2.58E-04 (2.1E-04)	1.67E-04 (2.35E-04)	4.23E-04 (3.55E-04)	9.19E-4*** (3.42E-04)
[0,3]	4.63E-04 (5.24E-04)	2.42E-04 (2.28E-04)	9.0E-04 (7.0E-04)	2.0E-04 (2.0E-04)	-4.06E-4* (2.42E-04)	2.19E-04 (2.88E-04)	5.92E-04* (3.16E-04)	0.001*** (3.18E-04)
<i>Anticipated effects</i>								
[-1,1]	0.001 (0.002)	1.01E-04 (2.81E-04)	8.0E-04 (7.0E-04)	4.0E-04* (2.0E-04)	-1.41E-04 (1.52E-04)	-1.72E-04 (2.91E-04)	2.93E-05 (5.72E-04)	4.7E-04 (4.44E-04)
[-2,-1]	1.97E-04* (1.01E-04)	3.81E-04*** (1.37E-04)	1.0E-04 (1.0E-04)	3.0E-04 (1.0E-04)	-9.24E-05 (1.4E-04)	-6.48E-05 (2.14E-04)	1.04E-04 (2.26E-04)	1.48E-05 (1.61E-04)
Yearly-variant controls	X	X	X	X	X	X	X	X
MSCI-world index	X	X	X	X	X	X	X	X
Firm fixed effects	X	X	X	X	X	X	X	X
Period fixed effects	X	X	X	X	X	X	X	X

Note: Volatility is measured as the standard deviation of the stock returns in the estimation window, namely the days previous to the political event, and we compare it with the standard deviation of the whole event period, that is including the days of the political event.

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Results for Interactions between Total Street Demonstrations and Sectors

Event Window [0,1] Estimation window [-20, -1]

	Daily stock returns (%)					
	(1)		(2)		(3)	
	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan
Total Street Demonstrations	-8.46E-04 (0.001)	-0.003* (0.001)	-0.004*** (9.32E-04)	-0.002 (0.001)	-0.004*** (8.89E-04)	-0.002** (0.001)
Industry sector	-0.002 (0.008)	0.012* (0.006)				
Total Street Demonstrations X Industry sector	-0.004** (0.001)	0.002 (0.002)				
Financial sector			0.003 (0.010)	0.002*** (6.53E-04)		
Total Street Demonstrations X Financial sector			0.004* (0.002)	-0.003 (0.002)		
Service sector					0.003 (0.008)	-0.0128** (0.006)
Total Street Demonstrations X Service sector					0.002 (0.002)	0.001 (0.003)
Ln Assets	0.003 (0.002)	-0.002 (0.001)	0.003 (0.002)	-0.002 (0.001)	0.002 (0.002)	0.002 (0.001)
Leverage	0.001 (0.001)	-0.001 (0.002)	0.001 (0.002)	-9.84E-04 (0.002)	0.001 (0.002)	-0.001 (0.002)
MSCI-world	2.78E-04*** (2.16E-05)	2.05E-04*** (2.41E-05)	2.78E-04*** (2.16E-05)	2.05E-04*** (2.41E-05)	2.78E-04*** (2.15E-05)	2.05E-04*** (2.41E-05)
Firm fixed effects	X	X	X	X	X	X
Period fixed effects	X	X	X	X	X	X
Constant	-0.489*** (0.058)	-0.311*** (0.041)	-0.492*** (0.066)	-0.298*** (0.044)	-0.492*** (0.066)	-0.298*** (0.044)
n	5,444	4,631	5,444	4,631	5,444	4,631
R2	0.053	0.034	0.053	0.034	0.053	0.033

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 13: Effects of Consultations on Stock Returns and Volatility Different-Length Event Windows
(Estimation window [-20, -1])

Event Window	Daily stock returns (%)				Volatility			
	9N (1)		27S (2)		9N (3)		27S (4)	
	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan
<i>Lagged effects</i>								
[0,1]	0.005** (0.002)	0.002 (0.002)	-8.04E-04 (0.012)	0.010*** (2.56E-03)	-5.70E-4** (2.57E-04)	-7.41E-04 (4.93E-04)	-1.21E-04 (7.97E-04)	5.6E-04 (3.7E-04)
[0,2]	7.31E-04 (0.002)	2.49E-04 (0.001)	0.007 (0.010)	0.014*** (0.003)	-4.73E-04 (3.2E-04)	-8.65E-04 (6.96E-04)	-2.22E-04 (0.001)	9.46E-04** (4.53E-04)
[0,3]	0.003 (0.002)	-9.31E-04 (0.001)	0.008 (0.009)	0.010*** (0.002)	0.002 (0.001)	-0.001 (8.59E-04)	-5.95E-04 (0.001)	8.70E-04* (4.51E-04)
<i>Anticipated effects</i>								
[-1,1]	0.004** (0.002)	7.6E-04 (0.002)	-0.002 (0.010)	0.009*** (0.003)	-5.32E-4** (2.39E-04)	-6.61E-04 (4.52E-04)	-5.30E-05 (7.38E-04)	5.86E-04 (3.58E-04)
[-2,-1]	-8.47E-04 (0.002)	-0.006** (0.002)	0.004 (0.010)	0.011*** (0.002)	-5.0E-4* (2.78E-04)	-5.88E-04 (5.59E-04)	2.73E-05 (8.29E-04)	8.64E-04** (3.69E-04)
MSCI-world index	X	X	X	X	X	X	X	X
Firm fixed effects	X	X	X	X	X	X	X	X

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 14: GARCH (1,1) Model

Event Window	Daily stock returns (%)							
	Total Pro (1)		Total Announcements (2)		Total Street Demonstrations (3)		Total Against (4)	
	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan	Catalan	Non-Catalan
<i>Lagged effects</i>								
[0,1]	0.001 (0.001)	2.95E-04 (0.001)	0.002 (0.001)	-2.0E-05 (0.002)	0.001 (0.004)	0.001 (0.003)	-0.001 (0.002)	-3.48E-04 (0.001)
[0,2]	0.001 (0.001)	3.58E-04 (0.001)	0.001 (0.001)	-4.0E-05 (0.001)	0.001 (0.004)	0.001 (0.003)	-0.003* (0.001)	-9.96E-04 (0.001)
[0,3]	6.84E-04 (0.001)	-5.07E-04 (0.001)	3.0E-04 (0.001)	-0.001 (0.001)	0.001 (0.003)	0.001 (0.002)	-0.002 (0.001)	-7.85E-04 (0.001)
<i>Anticipated effects</i>								
[-1,1]	0.001 (0.001)	6.01E-04 (0.001)	0.001 (0.001)	2.0E-04 (0.001)	0.001 (0.003)	0.001 (0.002)	-0.001 (0.001)	2.47E-04 (0.001)
[-2,-1]	8.42E-04 (0.001)	5.79E-04 (0.001)	2.0E-04 (0.001)	3.0E-04 (0.001)	0.002 (0.003)	0.001 (0.002)	2.64E-04 (0.002)	-6.57E-04 (0.002)

Note: Catalan and non-Catalan indexes are constructed as the unweighted average of the firms' stock returns. weighted average by firms' market value is not reported.

Cluster Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1