

Government Turnover and External Financial Assistance

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Abstract

We study the political incentives shaping governments' decisions to seek assistance from a lender of last resort. We propose that re-elected incumbents are more reluctant than newly elected governments to request assistance, as this action reveals negative information about their past performance. We first provide cross-country descriptive evidence that a change in office is indeed associated with a larger probability of receiving assistance from the IMF. Next, to obtain causal evidence, we analyze the decisions made by 4,000 Spanish municipalities following a credit shock during the Great Recession. Regression-discontinuity estimates show that newly elected local executives were approximately 30 percentage points more likely than re-elected incumbents to publicly agree on a financing program with the national government. Using data from press reports, electoral results and a survey on politicians' views, we show that many re-elected incumbents avoided requesting a public bailout to protect their image, even though this decision was financially suboptimal.

Keywords: Government Turnover, Bailout, Fiscal Consolidation, IMF.

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1. Introduction

Governments that are experiencing financial difficulties may request assistance from international financing institutions (IFIs), both in order to fund spending in the short-run and to meet other financial obligations. This type of lending acts as a substitute for other forms of credit that would typically be supplied by private lenders, but which may be circumstantially unavailable. Between 1992 and 2021, national governments signed over 2,300 different funding plans with the International Monetary Fund (IMF). Similarly, regional governments lacking access to credit, often request national authorities to take on a role analogous to that of IFIs as lenders of last resort.

From the perspective of a government facing financial distress, the decision to seek assistance is influenced by its political incentives and constraints. Politicians must request, bargain over and agree on a specific program with the funding institution. A successful agreement can grant them funds that enhance their fiscal capacity. However, this agreement will also impact voters' perception about the current health of public finances. We hypothesize that, in a context of asymmetric information, this publicity means government officials will have different incentives to request assistance depending on their tenure in office.

We posit that, while continuing governments bear responsibility for their previous fiscal and financial decisions, newly elected governments can more easily attribute the need for assistance to their predecessors. As a result, re-elected incumbents may prefer to endure a tighter borrowing constraint to protect their information rents. In contrast, new governments, whose inherited debt reveals no information regarding their performance, may prefer to request assistance in order to gain fiscal capacity while highlighting the constraints they face. We follow two empirical strategies to explore how these different incentives could impact the probability to reach a bailout agreement depending on government tenure.

First, we use data on all IMF interventions at the international level between 1992 and 2021 to conduct a cross-country descriptive analysis. Our estimates indicate that newcomers are between 3 and 4 percentage points more likely to agree on a funding program with the IMF in a given year. Relative to a base rate of 11%, this result suggests that the impact of government turnover on these decisions is substantial. In any case, this finding is only suggestive, since turnover is likely to be correlated with both demand and supply-side factors associated with IFI funding. Although we implement several strategies to mitigate

endogeneity concerns, the cross-country analysis does not allow us to reach the standards of identification that are customary in contemporary political economics.

Our main analysis overcomes this issue focusing on a setting in which we can apply state-of-the-art methods to identify our causal effect of interest. Namely, we leverage a Spanish national program that paid the arrears of almost 4,000 municipalities. This Supplier Payment Program (SPP, *Plan de Pago a Proveedores*) was introduced in 2012 to deal with unpaid commercial debt (*arrears*) of local governments, and it deployed financial resources of roughly 1% of Spanish GDP. This automatically converted the commercial debt of Spanish municipalities into financial debt with the national government. In practice, the SPP worked as a credit shock for local governments, as it prevented them from using commercial debt to finance their deficit. Crucially, the law establishing the SPP gave local governments two ways to pay back this debt, which implied different financing conditions as well as different publicity levels.

One option was to present a fiscal consolidation program (*adjustment plan*) to the national government. This would grant local governments access to a smoother backloaded adjustment – a two-year grace period, and up to ten years to repay loan and interest – but it would make the adjustment salient. Alternatively, municipalities could choose not to present an adjustment plan, in which case they would repay their debt via retention of central government transfers within five years. This meant that local governments could choose between a smoother repayment scheme, which required a public adjustment program, or a more discreet front-loaded adjustment. In this sense, the dilemma faced by these municipalities was similar to that of financially constrained governments which may request assistance from the IMF.¹

Our empirical strategy is based on a close-election regression-discontinuity design (RDD) that yields exogenous variation in government turnover. This strategy is analogous to that used to study the effect of political turnover on personnel changes and government performance in [Akhtari, Moreira, and Trucco \(2022\)](#) and in [Toral \(2022\)](#). It enables us to avoid municipal-level confounders such as the level of debt, the strength of local economic shocks, and local demographic conditions when identifying the effect of government turnover on the

¹If a country does not have access to alternative sources of funding, a reluctance to request assistance results in a sudden, sharp adjustment, either through deficit reduction, or through an inflationary tax. Conversely, if the government is willing to sign a public agreement with the IMF, it could receive funds which would allow a smoother transition to fiscal stability.

request for assistance. The resulting estimates indicate a large and significant difference between new and ongoing governments: newcomers are roughly 30 percentage points more likely to agree on a public adjustment plan.

Interestingly, the Spanish setting offers four features which make it particularly well suited to study the question at hand. First and foremost, it allows us to perform an RDD with a large sample of municipalities which share the same electoral system and which suffer a simultaneous credit shock. Second, unlike many IMF programs, the Spanish SPP does not involve any conditionality on policies beyond the establishment of a credible path to reduce debt and fiscal deficit. This removes ideological considerations from the decision of adhering to the plan. Third, the fact that the interest rate charged by the government to pay back the debt is subsidized makes the backloaded adjustment a superior alternative for the municipality. In such a setting, any local government which chooses not to present an adjustment plan is making a decision against the interest of their citizens. This allows us to focus on the misalignment of incentives between voters and politicians. Finally, the fact that the timing of the elections and the timing of the program were not under the control of local politicians prevents some potential threats to identification ([Hübscher and Sattler, 2017](#); [Müller, Forthcoming](#)).

The estimates from Spain's SPP indicate that political turnover causes an increased propensity to request assistance. We hypothesize that this occurs because of differences between the incentives of re-elected incumbents and new governments to reveal information on past performance. To provide empirical support for this specific mechanism, we conduct several complementary analyses using data on Spanish municipalities around the SPP period.

We first check whether requesting assistance has any impact on the voters' information set. Using data from Factiva covering the universe of Spanish national, regional and local newspapers, we find that when municipalities agree on an adjustment plan with the national government, this is indeed covered in the press. Consistently, when there is a change in office, the probability that the municipality appears in news related with the SPP jumps. Second, we explore how our estimates of the effect of turnover on the propensity to present a plan vary depending on the amount of arrears. Re-elected incumbents with low levels of accumulated arrears may be less reluctant to present an adjustment program, because the associated signal about their performance is less consequential. In line with this prediction, we find that our main estimated effect is close to zero for municipalities with low levels of

accumulated arrears, while it is positive and significant for the rest. Third, we use data on a survey of Spanish mayors — based on project *Policonstraints* — to understand what these politicians think about correcting their past mistakes. We find that many mayors consider that rectifying policies can be politically costly since it makes errors salient. Also — and maybe related to this — we find that most mayors consider that newcomers are in a better position to face situations of financial distress relative to ongoing incumbents. Finally, we explore whether submitting an adjustment plan affects the probability of re-election. We find a negative and significant association between presenting an adjustment plan and getting re-elected four years after the SPP, but only for ongoing incumbents.

This paper contributes to the literature on the political economics of macroeconomic policy.² Specifically, we show how political constraints affect when and how governments deal with financial difficulties. In this sense, our work relates to studies on the political determinants of stabilization and fiscal reform (for surveys of this vast literature see for example [Alesina 2018](#) or [Mahmalat and Curran 2018](#)). Relative to previous papers, our contributions are twofold. First, we study how political constraints shape the type of adjustment that is carried out, showing that ongoing incumbents are less willing to agree on a fiscal consolidation program even if third party financing gives them fiscal flexibility. Secondly, we deploy a close election regression-discontinuity design that allows us to identify our parameter of interest under relatively mild assumptions, using tools that are standard in the applied micro literature in political economics. In this, we distinguish ourselves from much of the empirical work in macroeconomic stabilization, which has typically relied on cross-country evidence (for recent overviews of this literature, see [Alesina, Favero, and Giavazzi 2019](#); [Kose, Ohnsorge, Reinhart, and Rogoff 2022](#)).

We also contribute to the specific literature that studies the determinants of externally sponsored financial arrangements. Previous work has shown that growth levels ([Knight and Santaella, 1997](#)), political connections with multilateral organizations ([Barro and Lee 2005](#), [Presbitero and Zazzaro 2012](#), [Dreher, Sturm, and Vreeland 2009](#)), and previous interventions of the IMF ([Conway, 2007](#)), can all impact the probability of a future financial arrangement with the IMF. Relative to these papers, our work studies how electoral incentives can also shape these decisions.

²See for example the surveys in [Persson and Tabellini \(1999\)](#); [Alesina and Passalacqua \(2016\)](#) and [Yared \(2019\)](#).

The higher predisposition of newcomers to present an adjustment plan is aligned with the finding that politicians try to shift the blame to other actors in tough times (Bursztyn, Egorov, Haaland, Rao, and Roth, 2022).³ Similarly, the reluctance of ongoing incumbents to present an adjustment plan is consistent with the notion that politicians take decisions to avoid blame and the corresponding loss of voter support (see Weaver 1986 and Hinterleitner 2017). Interestingly, the fact that presenting an adjustment plan is the superior alternative for the municipality relates our work to the classical debate between the Chicago and Virginia schools of political economy (see for example Becker 1976, Becker 1985, Wittman 1989, Crew and Twight 1990, Coate and Morris 1995, Tullock 1989).

In line with the predictions of the Virginia school, we find that politicians are significantly less prone to choose the efficient option – namely, presenting an adjustment plan – when the alternative decision yields private rents. Notably, our mechanism suggest that it is electoral competition that promotes such conduct. This introduces a new perspective on the effect of re-election incentives on politicians’ misbehavior. In the past decade, several empirical studies have shown how re-election incentives can discipline politicians in office, in consonance with the predictions of political agency models (Barro, 1973; Ferejohn, 1986; Banks and Sundaram, 1993). For example, previous work has shown how re-election incentives prevent corruption (Ferraz and Finan, 2011), encourage effort (Fouirnaies and Hall, 2022), and enhance policy implementation (De Janvry, Finan, and Sadoulet, 2012). In this paper, we find that re-election incentives can also induce ongoing incumbents to choose an inferior policy to protect their reputation. This result closely relates to the work exploring the possible negative effects of a politician’s career concerns on voters wellbeing (Canes-Wrone, Herron, and Shotts, 2001; Maskin and Tirole, 2004; Smart and Sturm, 2013), and it adds a new element to the discussion of the consequences of limiting mandates.⁴

Related to this, our findings could also offer a complementary explanation for the positive effect of government turnover on a country’s economic performance reported in Marx, Pons, and Rollet (2022). In this recent study, the authors show that the positive effects of

³A greater predisposition among new elected officials to agree on an adjustment plan, thereby shifting the blame to previous administrations, also links to the literature on Earnings Baths of CEOs during turnovers (see Moore 1973, Strong and Meyer 1987, Elliott and Shaw 1988 or Bornemann, Kick, Pfungsten, and Schertler 2015).

⁴Part of the extensive literature on political budget cycles, starting with (Nordhaus, 1975), also explores a particular aspect of the negative consequences of re-election incentives. Moreover, explicit positive aspects of term limits are highlighted in papers like Coviello and Gagliarducci (2017), which shows that tenure in office worsens procurement outcomes due to collusion between government officials and local bidders, or Bernecker, Boyer, and Gathmann (2021), which shows that re-election concerns might deter policy innovation.

government turnover are not systematically associated with any particular change in policy. On the contrary, they find that when a newcomer is elected, policies do change but in a non-systematic way – i.e., not in a specific direction. The evidence presented in our paper suggests that, while re-elected incumbents may stick to failed policies, fearing the consequences of acknowledging past mistakes, newcomers could be more ready to challenge the status quo.

2. Cross-Country Descriptive Analysis

In this section, we present a cross-country analysis documenting that government turnover is associated with a higher probability of reaching a funding agreement with the IMF. We use a country-level panel covering the 1992-2019 period, which contains information on countries' political institutions, macroeconomic data and indicators identifying new agreements. We focus on democracies – governments elected or appointed as a result of competitive elections.⁵ The resulting panel covers a total of 153 countries. Details on the construction of the dataset and descriptive statistics can be found in [Appendix B](#).

Using this sample, we document three facts. In the first place, government turnover is frequent: over 54% of the country-year pairs in our sample correspond to new governments by parties that took power in the last election (instead of re-elected incumbents). Secondly, IMF agreements are also common, with new financing agreements taking place in 11% of country-year pairs. Finally, new financing agreements are 5.4 percentage points more common in years in which a new government is in power, indicating a positive relationship between government turnover and the request of financial aid.

Naturally, the observed correlation between turnover and IMF funding may be an artifact of country-level characteristics and cyclical fluctuations in economic and electoral performance. For example, we know that during world economic downturns there is a higher frequency of changes in office ([Brender and Drazen 2008](#), [Fair 2009](#), [Nunn, Qian, and Wen 2018](#)) and IMF interventions ([Knight and Santaella, 1997](#)). Likewise, it is possible that countries with more internal ethnic cleavages are at the same time more prone to political instability and have lower economic performance (see e.g., [Alesina and La Ferrara 2005](#), [Gören 2014](#), [Arbatli, Ashraf, Galor, and Klemp 2020](#)). To mitigate these concerns, we use a

⁵We use a definition of competitive elections based on the Index of Electoral Competitiveness from the 2020 Database of Political Institutions. See details in the Appendix.

panel to estimate:

$$Program_{it}^{IMF} = \beta C_{it} + \alpha_i + \delta_t + \gamma_1 \Delta GDP_{it} + \gamma_2 left_{it} + \gamma_3 right_{it} + u_{it} \quad (1)$$

where the dependent variable is a dummy taking value one if country i signs an agreement to receive assistance from the IMF in year t , and value zero otherwise. Country and year fixed effects are represented by α_i and δ_t respectively. Variable C_{it} is a dummy that takes value 1 if the country experienced a change in the party in power in the last election and 0 if it is ruled by the previous incumbent. In some specifications, we also include as controls ΔGDP_{it} – the growth rate of national GDP in U.S. dollars – as well as $left_{it}$ and $right_{it}$, which are dummy variables that take value one if the party in office is left-wing or right-wing respectively.⁶ The coefficient of interest is β , which, under a suitable conditional exogeneity assumption, can be interpreted as measuring the effect of having a new party in power on the probability of making an agreement with the IMF.

In columns 1 through 3 of Table 1, we show estimates of β obtained using variations of the specification in equation 1. In column 1, we present the unconditional linear probability estimate of 5.4%, which corresponds to the difference in IMF funding request rates mentioned above. We include country and time effects in column 2 to account for country-level characteristics and global time trends which can affect both the probability to receive assistance from the IMF and government turnout. We find that the coefficient of interest remains significant, with a size of 3.7% relative to an 11% baseline probability of signing a funding agreement with the IMF. In column 3 we find that, after controlling for country GDP growth and for government’s political orientation, the coefficient of interest remains qualitatively unchanged.⁷

To account for heterogeneity in time-varying confounders across similar groups of countries, in column 4 we follow the approach proposed in [Bonhomme and Manresa \(2015\)](#). These authors propose a data-driven procedure to group countries into economic clusters and estimate a separate set of time effects for each of these clusters. When using this procedure

⁶We obtain these codes from the DPI2020 database. The omitted category corresponds to center parties and parties with no clear ideological alignment.

⁷To mitigate potential concerns of reverse causality, in an additional analysis we also lag one year the *Party change* variable. Results remain unchanged. Likewise, we also add other macroeconomic controls to our main specification: the inflation rate, current account balance, unemployment rate and gross debt. Although we lose almost half of the sample due to missing values, our coefficient of interest remains of similar magnitude and significant at the 5% level.

to group countries into four groups, the coefficient of interest remains significant and similar in magnitude to the one reported in columns 2 and 3. We interpret these estimates as suggestive evidence of a positive causal effect of government turnover on the propensity to request external financial assistance.

TABLE 1
THE IMPACT OF PARTY CHANGES ON IMF PROGRAMS - ALL IMF FUNDING PROGRAMS

	(1)	(2)	(3)	(4)
	IMF Program	IMF Program	IMF Program	IMF Program
Party Change	0.054*** (0.010)	0.037*** (0.010)	0.033*** (0.010)	0.034*** (0.010)
Δ GDP			-0.001* (0.001)	-0.001** (0.001)
Party: Right Orientation			-0.002 (0.022)	-0.001 (0.022)
Party: Left Orientation			0.018 (0.020)	0.015 (0.020)
Observations	3,730	3,730	3,653	3,653
Country & Year FE	NO	YES	YES	YES
GFE*Year	NO	NO	NO	YES

Notes: The Table reports OLS estimates. IMF Program is a dummy variable that takes a value of 1 if the country approves an IMF funding program in that year, and 0 otherwise. Party Change takes a value of 1 if the country is ruled by a new party, and a value of 0 if the country is ruled by the previous incumbent party. Columns (2) and (3) include country and year dummies. Δ GDP is the growth rate of national GDP in U.S. dollars. Political party dummies take a value of one according to the orientation of the chief executive's political party. Column (4) includes group fixed effects (GFE) interacted with year dummies, following [Bonhomme and Manresa \(2015\)](#). The sample used is 1992-2020 and includes all IMF funding programs in the IMF MONA Database. Robust standard errors clustered at the country level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

Although the specifications used in columns 2 to 4 of Table 1 aim to mitigate the omitted variable problem, the assumption needed for exogeneity of the C_{it} variable are still rather strong. To further address this concern, in Section [Appendix B.2](#) of [Appendix B](#) we conduct a complementary analysis restricting our attention to countries undergoing a banking crisis, as recorded in the database in [Laeven and Valencia \(2020\)](#). This seeks to attenuate the concern that unobserved external economic shocks explain both the recent change in office and the decision to request assistance to the IMF. Unsurprisingly, the average probability of an external bailout in this sub-sample is much higher than in the full country panel (48.4%). Remarkably, we find that the probability to sign a funding program with the IMF rises between 25 and 29 percentage points when a newly elected party party is in power during a banking crisis.

Altogether, these results provide evidence on the positive association between a change in office and the decision to ask for external financial assistance. Still, even in the sub-sample of baking crises, claiming exogeneity of our new government indicator remains a

strong assumption. Moreover, in the cross-country sample it is hard to identify whether these estimated effects result from differences in governments' propensity to request assistance or on the Fund's willingness to provide it. In the rest of the paper, we use the case of Spanish Municipalities to leverage close-election regression-discontinuity methods that allow us to make a stronger case for a causal link between tenure in office and the decision to publicly acknowledge the need of external financial support.

3. Spain's Suppliers Payment Program: Data and Institutional Setting

We now focus on achieving clean identification of the effect of interest by exploring an ideal setting, which allows us to complement the previous descriptive analysis with rigorous causal estimates: The Spanish Suppliers Payment Program. This program was introduced during the Great Recession and offered indebted local governments the possibility to publicly agree on an adjustment plan with national authorities in exchange for a smoother re-payment profile of their arrears.

3.1. Institutional Setting

3.1.1. Spanish Municipalities and Mayors

Our units of analysis are Spanish municipalities. In 2011, there were 8,116 municipalities in Spain, each of them ruled by a separate local government. Municipalities are the lowest level of territorial administration in the country. As recognized in the Spanish constitution, municipalities have autonomy in managing their interests. The functions of the municipal government depend on their size, but among others, they include waste disposal, lighting, water and sewage services, land development, and the provision of several local public services.⁸ Regular municipal financing is based on transfers from the national government, which amount to approximately 50% of their income, transfers from regional governments, and local taxes. The most important local tax is a property tax. During the housing boom of 2000-2008, it was also common for municipalities to sell public land to obtain extraordinary revenues.

Municipalities operate as small representative democracies, and are governed by a municipal council and a mayor. The electoral system varies depending on population size. In this paper we focus on municipalities with more than 250 inhabitants, which use a single-

⁸See details in law number 7/1985 (April 2nd 1985). *Ley reguladora de las bases del régimen local*.

district, closed-list, proportional electoral system.⁹ In these municipalities, council seats (from a minimum of 7 to a maximum of 57 in Madrid) are assigned following a D'Hondt rule with a 5% vote share entry threshold. The municipal mayor is appointed by the council under a plurality rule. The most voted party can appoint the mayor directly if it obtains more than 50% of the seats. If no party commands a majority of seats, candidates need to receive the support of the majority of the council to be elected. If none of the candidates obtains this support, the candidate from the most voted party is appointed as mayor. There are no term limits, and local governments cannot call for elections, which occur simultaneously for all Spanish municipalities every four years.

3.1.2. Spain's Supplier Payment Program

The Great Recession caused a sharp deterioration in the health of Spanish public finances. Following an acute reduction in fiscal revenues – due to the ongoing crisis –, as well as the end of the ability to sell public land – due to a nationwide housing bust –, Spanish municipalities started using unpaid commercial debt to finance their deficit. As a result, municipalities' commercial debt increased from 1.6% of GDP in 2007 to 2.6% in 2011, an all-time high in the time series starting in 1995. This increase was mostly explained by a significant build-up of arrears throughout this period (worth 0.9% of Spanish GDP).

In light of the negative impact that mounting arrears had on firms and on macroeconomic dynamics (see [Checherita-Westphal, Klemm, and Viefers 2016](#) and [Delgado-Téllez, Lledo, and Pérez 2017](#)), the Spanish national government decided to adopt various measures in early 2012. These included the so-called Supplier Payment Program or SPP (*Plan de Pago a Proveedores*), aimed at eliminating the stock of arrears accumulated by both regional and local governments, and the Budgetary Stability Law (*Ley de Estabilidad Presupuestaria*) designed to prevent local governments from building up arrears again. Under the SPP, any supplier who had claims against a local government could resort to the state-owned Official Credit Institute (*Instituto de Crédito Oficial*) to get their bills paid in less than two months with no discount. Concomitantly, all overdue commercial debt of municipalities was transformed into financial debt with the national government. This meant that local governments now owed their unpaid bills to an institution which could enforce its payment via retention of funding transfers.

⁹Municipalities with populations under 250 inhabitants have an open list system in which voters may express multiple preferences for different candidates.

The national government offered two different repayment options to municipalities in the program. Municipalities could opt for 10-year debt with a 2-year interest-only grace period. This option required the municipality to submit a fiscal adjustment plan, in an effort to minimize moral hazard in the context of these generous financing conditions.¹⁰ This plan should be discussed and approved by the municipal council (the local equivalent of the parliament). Crucially, this gave the local opposition the chance to make salient the financial situation of the municipality and the spending cuts and tax changes proposed by those local governments in the context of the impending adjustment.

Alternatively, municipalities could opt for a front-loaded adjustment that involved taking on 5-year debt with no grace period, to be paid via retention of fiscal transfers from the national government. Thus, local governments could choose between a smoother repayment scheme which required a public adjustment program, or a more discreet front-loaded adjustment. The map in Figure 1 shows the spatial distribution of municipalities with no arrears, municipalities with arrears that presented an adjustment plan, and municipalities with arrears that did not present an adjustment plan.

Regardless of the option selected, municipal debt with the national government created in the context of the SPP had an interest equal to the Spanish Treasury's funding cost plus a maximum spread of 145 basis points. These were remarkably good funding conditions compared to what regional and local governments could have obtained in capital markets, if anything.

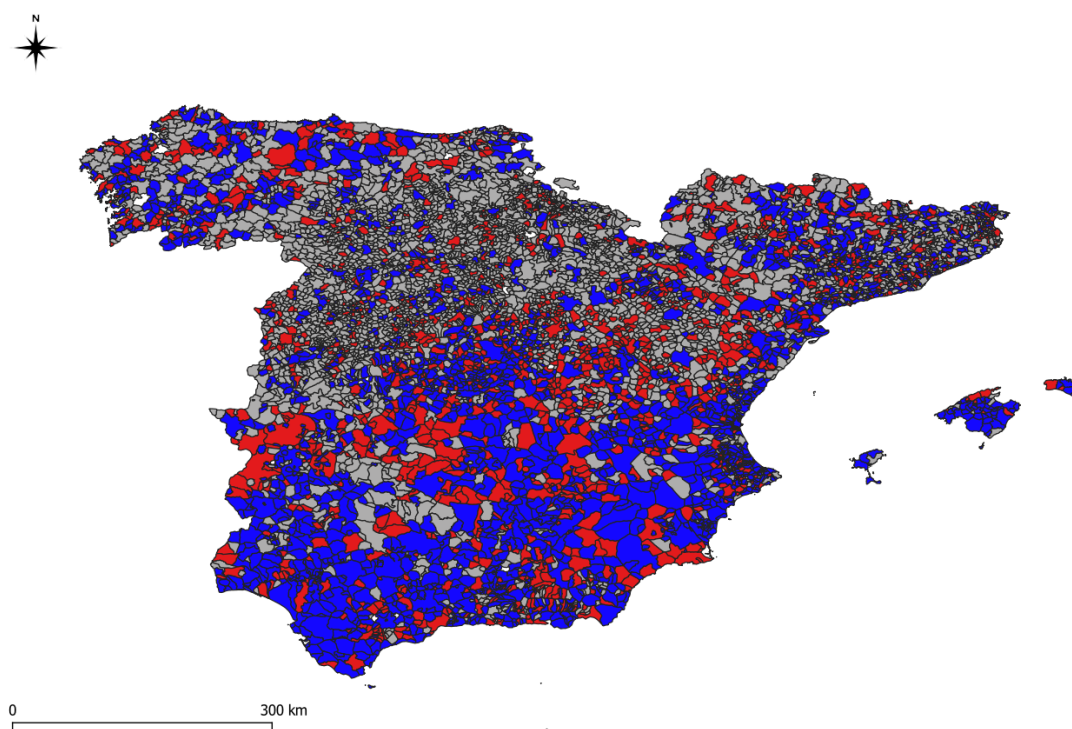
As a result of the fiscal cuts that every municipality with arrears had to make, Spanish local governments brought their aggregate budget balance from a deficit of 0.4% of GDP in 2011 to a surplus of 0.1% in 2012. This was the largest fiscal adjustment ever recorded at the municipal level in one year in the Spanish series. In subsequent years, with a sustained surplus of 0.2% of GDP, Spanish municipalities' further reduced their aggregate financial debt from over 4% of GDP in 2012 to less than 2% in 2022.

3.2. *Data*

We build a municipal panel with yearly information for the period 2008-2015, combining data from several sources. Part of the analysis below will focus on a cross-section from this panel for year 2012.

¹⁰See [Heppke-Falk and Wolff \(2008\)](#) for a discussion on how national government bailouts might induce moral hazard among local debt investors.

FIGURE 1
MAP OF SPANISH MUNICIPALITIES



Notes: In grey, municipalities with no arrears. In blue, municipalities with arrears which presented a plan. In red, municipalities with arrears which did not present a plan. No data is available for Comunidad Foral de Navarra and the Basque Country.

Data on yearly municipal budgets is obtained from the database on local authority budgets, which is made available by the Spanish *Ministerio de Hacienda y Administraciones Públicas* (MINHAP). This database provides information on revenues and spending classified by spending category during the period 2008-2015. This classification includes variables such as government transfers, revenues for different taxes, or total spending. Also from MINHAP, we obtain data on arrears and data on the outstanding debt by municipality, available since 2009.

Electoral results for Spanish municipalities in the 2007, 2011 and 2015 local elections are obtained from the Spanish *Ministerio del Interior*. For every municipality and election year, we have the list of all candidates and the electoral results for all running parties. Data on characteristics and demographics of the candidates are obtained from the MINHAP upon request.

We also use data from *Estadística del Padrón Continuo*, which includes yearly information on population and population by age, and data on employment, obtained from the *Instituto Nacional de Estadística*.

Merging data from these sources, we construct a panel of municipalities which includes the vote shares obtained by all parties, several politicians' characteristics, information on the decision to present an adjustment plan, and other municipal characteristics such as municipal spending, revenues, outstanding debt or arrears. We exclude from the panel municipalities with populations under 250 inhabitants, which have a different electoral system to other municipalities in the country.

Municipal descriptives for our sample are presented in Table 2 and Appendix Table A.1. In Table 2, we present the mean and standard deviation for several variables at the end of 2011, right before the SPP was introduced. We include population, outstanding debt per capita, total spending and revenues per capita, arrears per capita and the fraction of municipalities ruled by the biggest political parties in Spain: the center-left Partido Socialista (PSOE) and the center-right Partido Popular (PP). Panel A shows the information for all municipalities in Spain, panel B includes municipalities that participated in the SPP but did not do an adjustment plan (37% of municipalities with arrears), and panel C includes municipalities that participated in the SPP and carried out an adjustment plan (63% of municipalities with arrears). The average population of all municipalities in our sample is 5.8 thousand inhabitants. Accumulated arrears per capita are on average similar for municipalities that do not do an adjustment plan and for municipalities that do an adjustment plan.

Table A.1 in the Appendix compares averages of several variables across municipalities in which the challenger (column 1) or the incumbent (column 2) won the elections in 2011. That is, the table shows the differences between locations which experienced government turnover in 2011 and locations that did not experience this turnover. We observe that municipalities ruled by a newly elected challenger exhibit a higher probability to do an adjustment plan, have lower spending and revenues per capita, and are more often ruled by PP rather than by PSOE. In the next section, we discuss how we proceed to obtain our effects of interest while avoiding the bias induced by these differences in characteristics.

Finally, in part of our analysis, we use the Factiva database to explore the visibility of the SPP program in the press. This database is a Dow Jones & Company tool which aims to cover the universe of news outlets in Spain, providing access to more than 6 million articles every year in more than 200 Spanish national, regional and local newspapers and magazines.

TABLE 2
SUMMARY STATISTICS

Panel A: All municipalities		
	Mean	Std. dev
Population	5814.50	47427.97
Outstanding Debt pc	251.33	416.43
Total Spending pc	1369.38	1078.49
Total Revenues pc	1374.78	1343.14
Arrears pc	363.10	542.00
Party PP	0.46	0.50
Party PSOE	0.28	0.45
Number Obs	8116	
Panel B: Municipalities No Adj. Plan		
	Mean	Std. dev.
Population	4472.17	24311.26
Outstanding Debt pc	264.59	411.01
Total Spending pc	1448.27	1132.00
Total Revenues pc	1415.76	1134.21
Arrears pc	350.21	710.53
Party PP	0.44	0.50
Party PSOE	0.36	0.48
Number Obs	1337	
Panel C: Municipalities Adjustment Plan		
	Mean	Std. dev
Population	11838.51	78259.99
Outstanding Debt pc	363.07	343.73
Total Spending pc	1143.09	646.78
Total Revenues pc	1120.27	610.58
Arrears pc	371.37	415.15
Party PP	0.46	0.50
Party PSOE	0.32	0.47
Number Obs	2284	

Notes: This table reports means and standard deviations for each variable by municipality in 2011. It also reports total number of observations. Panel A shows summary statistics for all municipalities in Spain, including those without arrears. Panel B shows summary statistics for municipalities with arrears that did not submit an adjustment plan. Panel C shows summary statistics for municipalities with arrears that followed an adjustment plan. We do not provide a separate panel for the municipalities without arrears in 2011 since they are not used in our main analyses.

3.3. Impact of SPP Funding Choice on Municipal Budgets

As discussed above, municipalities with arrears could choose between two options. Either they agreed on an adjustment plan with the national government in exchange for a smoother transition to stabilization, or they chose a more abrupt adjustment via the retention of inter-governmental transfers to pay back their debt.

We next investigate how this choice affected revenues, spending and tax rates set by municipal governments. We exploit municipal budget data to estimate:

$$Y_{jt} = \alpha_j + \delta_t + \sum_{k=2008}^{2015} \omega_k \text{front}_j \times \mathbb{1}\{t = k\} + \sum_{k=2008}^{2015} \gamma_k \log(\text{arrears}_j) \times \mathbb{1}\{t = k\} + \epsilon_{jt} \quad (2)$$

where j indexes municipalities and t indexes years, α_j is a municipality fixed effect, δ_t is a set of time effects, front_j is a dummy taking a value of 1 if municipality j chose the front-loaded option to pay for its SPP obligations, and $\log(\text{arrears}_j)$ is the logarithm of total commercial debt in arrears for municipality j at the end of 2011. We consider three different outcomes Y_{jt} : the natural logarithm of central government transfer revenues per capita, the natural logarithm of total spending per capita, and the urban property tax rate levied by the municipality.

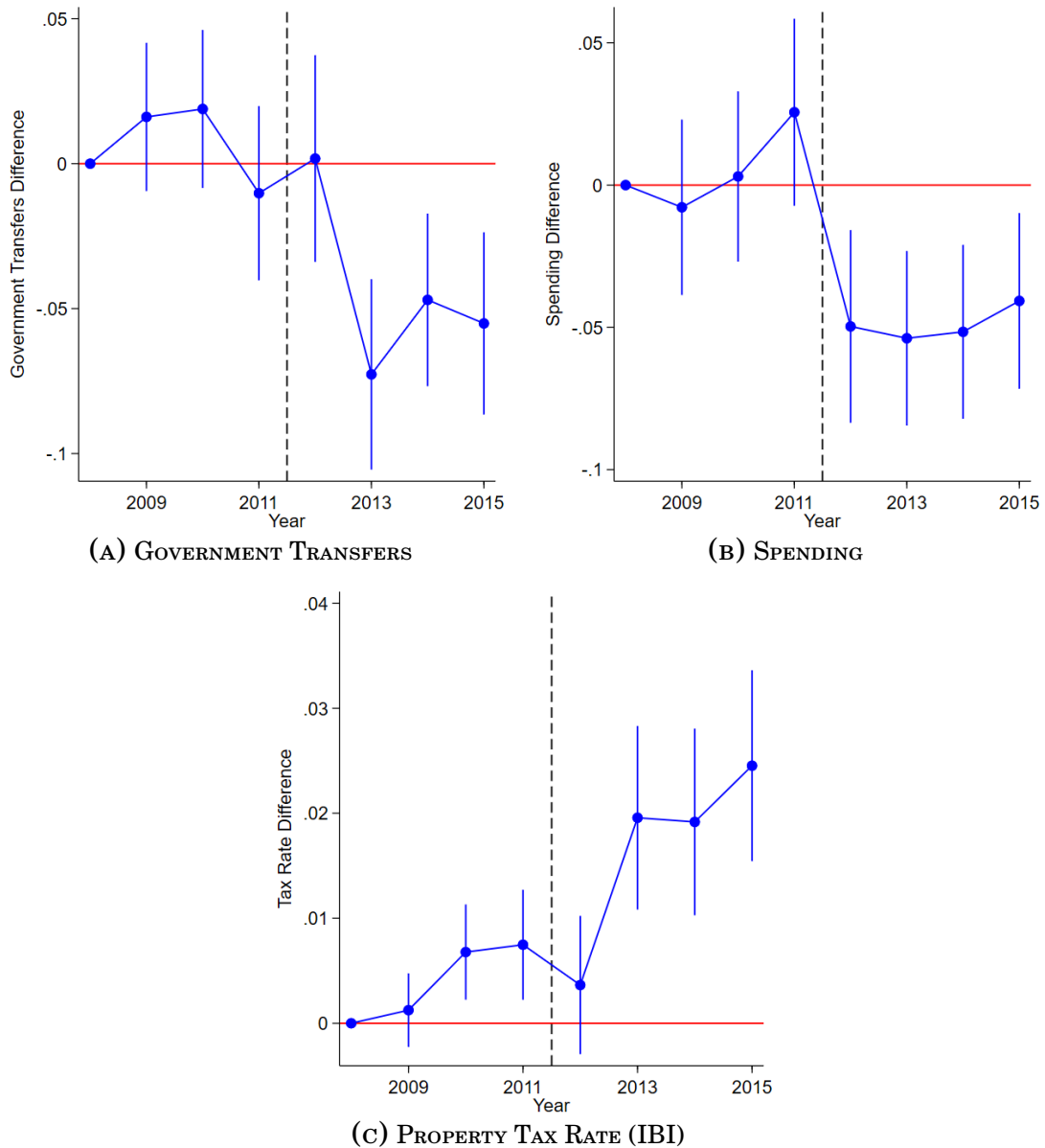
This analysis is restricted to municipalities with unpaid commercial debt, i.e., those that were forced to assume a central government loan by virtue of the SPP. As a result, the coefficients $\{\omega_k\}_{k=2008}^{2015}$ can be interpreted as the differences between municipalities with $R_j = 1$ and those with $R_j = 0$. That is, they indicate the evolution of the difference on transfers, spending and taxes between municipalities choosing the more discreet front-loaded adjustment and municipalities that opted for presenting an adjustment plan. The inclusion of the second sum in equation 2 controls for differences in the levels of arrears per capita across municipalities in both groups.¹¹

Estimates of the sequences of coefficients are reported in Figure 2. In panel A, we display coefficients for the difference in transfers. We observe that the difference in transfers was relatively stable before 2013 but became negative on this year, and stayed negative thereafter. We interpret this as arising from revenue retention by the central government. In the year after the SPP policy was passed, municipalities that opted for the front-loaded adjustment experience an abrupt decrease in the transfers provided by the central government because they began to pay for the debt associated with the SPP.

How did this reduction in transfers affect municipal spending? Panel B shows a relative decline in municipal spending by late 2012, which is consistent with municipalities adjusting their spending levels ahead of the change in transfers. This relative reduction in spending persists to the end of our sample period in 2015. Was all of the front-loaded adjustment

¹¹The longitudinal patterns are quite similar if we exclude the interaction terms in the second sum of equation 2. The only noticeable difference is an earlier convergence of spending levels between both groups of municipalities. Results available upon request.

FIGURE 2
CONSEQUENCES OF GOVERNMENT RETENTION SCHEME



Notes: These figures show point estimates and 95% confidence intervals for the effect of not presenting a plan on: the log of the transfers received from the central government (**panel A**), the log of total municipal spending (**panel B**), and the property tax rate (IBI) (**panel C**), for years 2008-2015. All regressions include municipality fixed effects and year fixed effects. Figures plot the estimated coefficient for the interaction between a year dummy and a dummy that takes value zero if the municipality presents an adjustment plan, and value one otherwise. Standard errors are clustered at the municipality level.

expressed through a reduction in spending? Another contributing factor is indicated in panel C, where we observe a sharp increase in relative property tax rates (IBI) for municipalities that opted for the front-loaded adjustment.¹²

¹²There is a large literature on macroeconomic outcomes of deficit reduction policies. See [Alesina, Favero,](#)

Collectively, the patterns displayed in the three panels in Figure 2 are consistent with the consequences of a front-loaded adjustment translated into both lower spending and higher taxes. It is worth noting that these patterns cannot be given a causal interpretation unless we assume that the fixed effects and interaction terms in equation 2 suffice to deal with potential differences in the trajectories of municipalities making different choices. This is a rather strong assumption in our context. We present these results not to make a strong claim about the consequences of adjustment options for policy at the local level, but rather as suggestive evidence that the expected impacts that the front-loaded adjustment would have on municipal finances are indeed observed in the practice. That is, we see these patterns as mostly descriptive, but nonetheless reassuring.

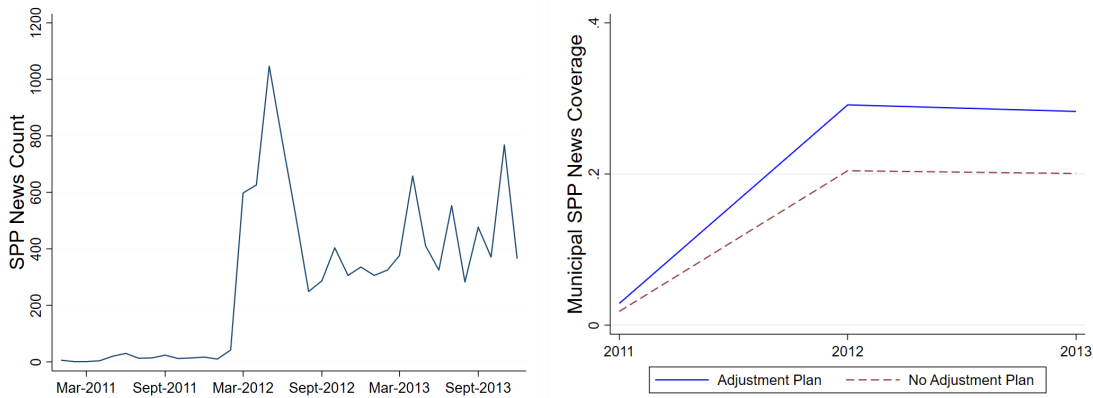
3.4. News Coverage of SPP

There was substantial coverage of the SPP in Spanish national and local news, which evidences the publicity usually associated to these type of arrangements. Panel A of Figure 3, reports the monthly number of news released in all Spanish newspapers that made a mention to the Suppliers Payment Program between 2011 and 2013. As expected, before the program was announced in early 2012, there were virtually no news articles referring to this program. A large spike in coverage took place upon announcement and implementation, followed by a steady coverage of over 400 news articles per month from September 2012.

News coverage of the SPP was higher in municipalities which decided to submit an adjustment plan to the national government in exchange for a smoother repayment path. We document this by using data on the number of news covering the SPP by municipality. Panel B of Figure 3 shows the average by municipality for each year between 2011 and 2013, disaggregated by whether the municipality submitted an adjustment plan to the national government or not. We observe news coverage of the SPP is almost zero in 2011 and increases in both subsequent years. Notably, news coverage is higher in 2012 and 2013 for municipalities submitting an adjustment plan to repay their arrears. We further analyze the link between government choices and news coverage in Section 5.3.

and Giavazzi (2019) for a review or, more specifically, Blanchard and Leigh (2013), which analyzes the relation between planned fiscal consolidation and growth.

FIGURE 3
NEWS COVERAGE OF SPP



(A) AGGREGATE MONTHLY NEWS COVERAGE

(B) RELATIVE COVERAGE BY CHOSEN OPTION

Notes: Panel A represents the total number of times that "Supplier Payment Program" appears in the news every month from January 2011 to December 2013. Panel B represents the average number of news covering the SPP per municipality and year calculated for municipalities choosing whether to submit an adjustment plan to repay the SPP loan. Source: Factiva.

4. Empirical Evidence from SPP

In this section, we use information on Spain's SPP to study whether newly elected municipal governments differ from re-elected governments in the probability to submit a public adjustment plan that allows them to carry out a smoother adjustment to fiscal stability.

As discussed in Section 2, in times of financial distress, both the probability of a change in office and the need of an externally supported fiscal adjustment increase. Hence, identifying the causal impact of tenure in office on the probability to request external support is difficult. The large number of Spanish municipalities, which share a common electoral system, and receive a simultaneous credit shock, allow us to draw quasi-experimental estimates of this causal relation. Moreover, the design of the Spanish SPP, presents a series of features that make it specially well suited to achieve a clean identification of the effect of politicians' private incentives on the decision to request assistance.

All municipalities in our sample had elections just a few months before the Spanish SPP was put in place. [Alesina, Ardagna, and Trebbi \(2006\)](#) and [Alesina, Furceri, Ostry, Papageorgiou, and Quinn \(2020\)](#) explain two potential sources of endogeneity related to the timing of the implementation. First, if governments have discretion on when to call for elections, they will do it before they conduct any fiscal adjustments ([Hübscher and Sattler, 2017](#)). Second, macroprudential regulation is systematically less likely to be implemented before elections ([Müller, Forthcoming](#)). In Spain's SPP, both the timing of elections and the timing of the

program are the same for all municipalities. This means they are completely independent from municipal governments' decisions.

Moreover, unlike many bailout programs sponsored by IFIs, the Spanish SPP has two features that allow us to focus on the possible conflict of interest that incumbents might face. First, the national government imposed no conditionality on policies – municipalities had discretion on how to attain fiscal sustainability –. This leaves aside the ideological discussion that is often present in the decision to adhere to an IMF rescue program, which typically imposes some concrete reforms. Second, the central government has enforcement power to claim back the arrears that it paid in the name of every municipality with overdue commercial debt. This, along with the favorable credit conditions for those who present an adjustment plan, yields a conservative estimate for the difference in net present value (NPV) of presenting a plan of almost 9 euros per capita (see Appendix Table A.2).¹³ The fact that presenting a plan represented a NPV gain of over 80,000 euros for the average municipality, allows us to identify not doing so as a consequence of a principal-agent problem.

4.1. Empirical Strategy

We use a close-election regression discontinuity design (RDD) to induce exogenous variation on whether there was a change in office in 2011. To do so, we create a running variable for municipality j , defined as $\Delta V_j \equiv V_j^C - V_j^I$, where V_j^I is the 2011 vote share of the incumbent party at the end of the 2007 term – just before the 2011 election – and V_j^C is the vote share of the most voted party in the 2011 election excluding the incumbent. From now on, we call these parties the *incumbent* and the *challenger*.¹⁴ Note that ΔV_j will take positive values if the challenger wins the 2011 local election and negative values otherwise.

We use this running variable to estimate the effect of dummy C_j , taking value one if the municipality elects a mayor from a new party, on a dummy outcome P_j which takes a value of one if the municipality submits an adjustment plan to the national government. Spanish

¹³If the discount rate of the local government coincides with the interest rate available to the municipality, the fact that credit is provided at a subsidized rate means that longer credit maturities mean higher NPV. We cannot obtain individual figures for the financing costs of most municipalities in credit markets. Indeed, in spring 2012 most municipalities did not have the possibility to access credit at all. However, we can take the financing cost of Madrid (the capital and biggest city in Spain), which in 2011 had a bond trading in secondary markets, and use it as a lower bound. The average quotation of Madrid's 10 year bond yield in April 2011 was 7.5%, which represented 220 basis points (bps) relative to the 10 year Spanish treasury. Arguably, the financing cost of Madrid must be lower than the cost of other municipalities, and the risk premium was smaller in 2011 than in 2012. Hence, it seems reasonable to use it as a lower bound for municipalities' discount rate to compute the difference between the NPV of presenting and not presenting a plan.

¹⁴It is important to note that the word incumbent here refers to the 2007-2011 incumbent and not the 2011-2015 incumbent.

mayors are not directly elected by voters but appointed by the elected council. Therefore, the probability of having a new mayor does not jump from 0 to 1 when ΔV_j crosses the threshold at zero – our RDD is fuzzy (Imbens and Lemieux, 2008). We estimate our parameter of interest by two-stage least squares (2SLS). The estimating equations are:

$$C_j = \alpha_0 + \tau D_j + \pi_1 \Delta V_j + \pi_2 D_j \Delta V_j + v_j \quad (3)$$

$$P_j = \alpha_1 + \beta C_j + \rho_1 \Delta V_j + \rho_2 D_j \Delta V_j + u_j \quad (4)$$

Our parameter of interest is β , which can be interpreted as the causal effect of having a new party in power on the probability of choosing an adjustment plan.¹⁵ Equations 3 and 4 correspond to our first- and second-stage respectively. Variable D_j is defined as $D_j = \mathbb{1}\{\Delta V_j > 0\}$ and is our instrument for C_j . The third and fourth terms in the right-hand side of both equations correspond to linear terms in the running variable, estimated separately on each side of the threshold.

We estimate the parameter of interest using a local linear regression with a triangular kernel. The state-of-the-art in the estimation of these parameters uses the routine proposed in Calonico, Cattaneo, Farrell, and Titiunik (2017), which incorporates data-driven procedures to select a bandwidth, adjusted standard-errors to account for the bandwidth selector and a bias correction procedure developed by the authors.¹⁶ In Section 4.3 we discuss the robustness to the bandwidth choice, the choice of the kernel, the polynomial length used to adjust for values of the running variable and alternative methods to compute that running variable.

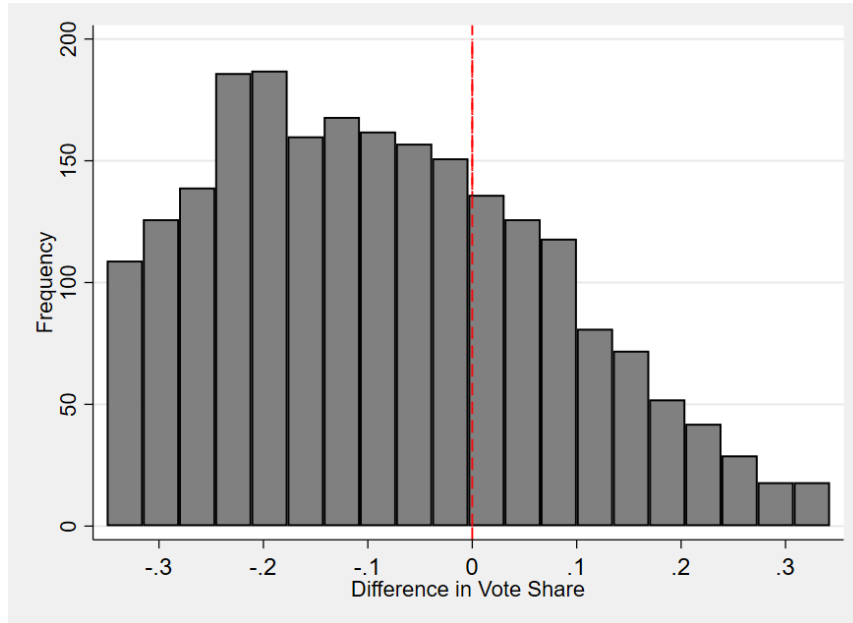
Before we move to report our estimates for β , we discuss the plausibility of some of the assumptions required for the validity of the regression-discontinuity design in our context. In the first place, we discuss the assumption of no manipulation. While parties influence electoral results through their actions, it is unlikely that they can perfectly manipulate electoral outcomes. We provide evidence consistent with this notion by reporting the histogram of the running variable around the threshold in Figure 4. The formal statistical tests described in McCrary (2008) and Cattaneo, Jansson, and Ma (2020) yield large p-values of 65% and 76%,

¹⁵Note that our estimation sample is restricted to municipalities that had accumulated arrears ahead of the introduction of the SPP. Therefore, β measures the propensity to choose the adjustment plan option relative to the front-loaded transfer retention option.

¹⁶Details on these procedures can be found in Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, Farrell, and Titiunik (2017). In our case, implementation is carried out using the most recent version of the Stata `rdrobust` command.

respectively, confirming that perfect manipulation of the running variable is very unlikely in this context.

FIGURE 4
HISTOGRAM OF RUNNING VARIABLE – VOTE MARGIN OF MUNICIPAL CHALLENGER



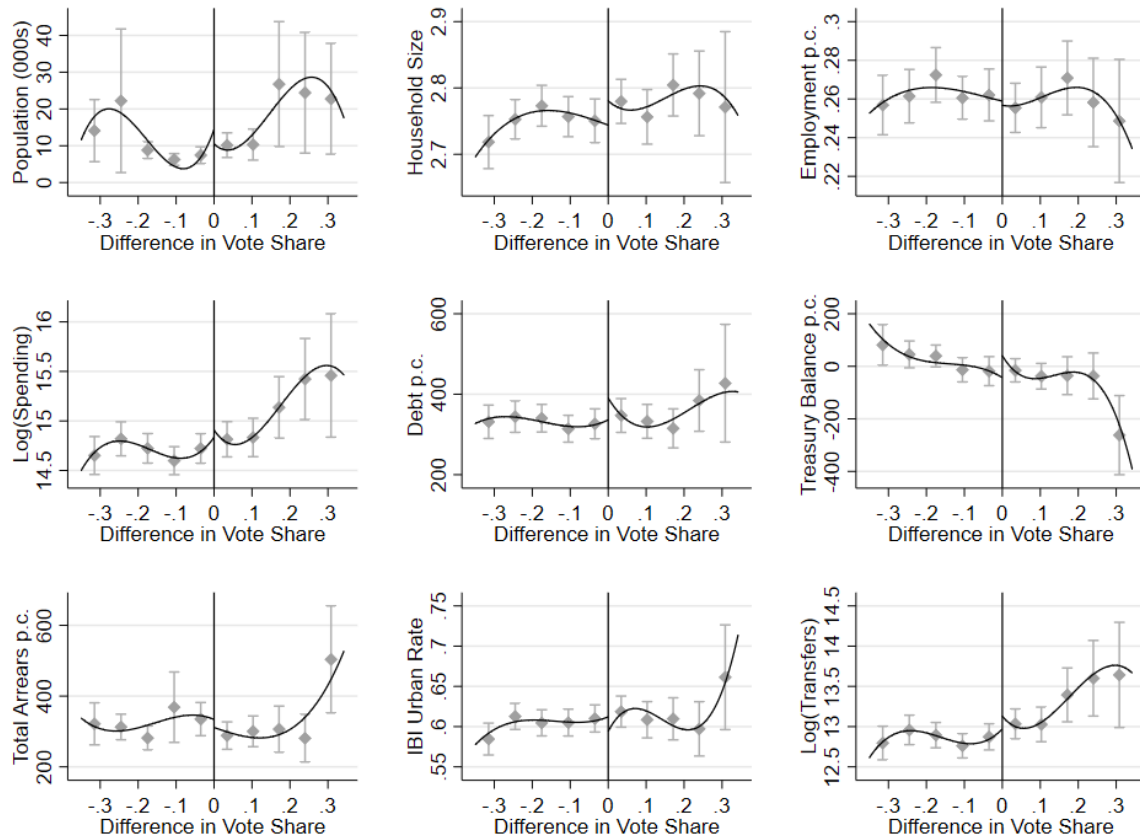
Notes: Histogram of running variable for values between -0.35 and 0.35. The p-value of the Cattaneo, Jansson, and Ma (2020) test of no manipulation is 75%.

To further emphasize the validity of our research design, we also analyze the covariate balancing at the threshold. Our empirical strategy ensures that pre-determined characteristics of the municipalities and the governments in power before 2011 are balanced on both sides of the threshold. Figure 5 and Appendix Figure A.1 illustrate this point. Figure 5 shows that demographic and financial characteristics of municipalities vary smoothly at the threshold. Importantly, this includes variables measuring the level of arrears and debt accumulated by municipalities by 2011. Figure A.1 shows that characteristics of the incumbent government in power before the 2011 election also vary smoothly at the threshold. Appendix Tables A.3 and A.4, display formal tests for these differences at the threshold using 2SLS estimates similar to the ones used for our main outcome of interest. For all outcomes, we observe the effect of interest is statistically insignificant at conventional levels. Thus, we conclude that our RD design successfully deals with predetermined confounders.

4.2. Baseline Results

We illustrate our first-stage in the top panel of Figure 6. The horizontal axis represents our running variable and the vertical axis the probability of having a new party in power at

FIGURE 5
COVARIATE BALANCING – MUNICIPAL CHARACTERISTICS

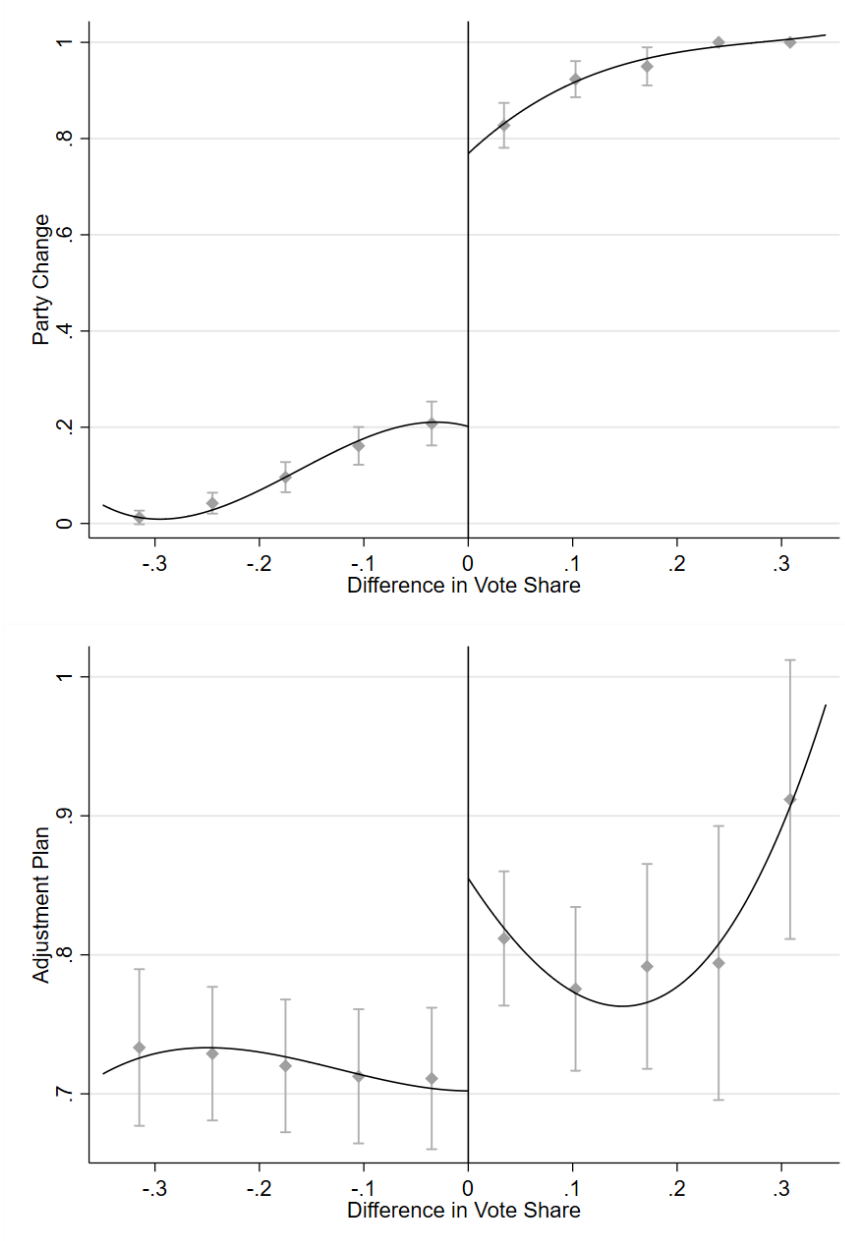


Notes: The horizontal axis represents the vote share difference between the challenger and the incumbent. From left to right and top to bottom the vertical axes represent population (in thousands), household size, fraction of employed population, logarithm of municipal spending per capita, municipal public debt per capita, municipal cash holdings per capita, arrears per capita, municipal housing tax rate and logarithm of central government transfers. Solid lines represent third degree polynomials in the running variable estimated separately for positive and negative polynomials. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

the local level after the 2011 election. Third degree polynomials are estimated separately on both sides of the threshold. Gray dots correspond to averages of the dependent variable for different bins of the variable in the horizontal axis, and vertical lines correspond to 95% confidence intervals. We observe a substantial jump in the probability of having a change in the party in power at the threshold. The gap in probability is roughly 0.5, indicating the design is fuzzy and not sharp.

The bottom panel of Figure 6 illustrates the reduced form effect of crossing the threshold on the probability of having an adjustment plan. Other elements of the graph are analogous to those described in the top panel. The discontinuity at the threshold indicates that when the challenger wins the election we observe an increase in the probability of presenting a

FIGURE 6
PARTY CHANGES AND ADJUSTMENT PLANS: FIRST-STAGE AND REDUCED-FORM



Notes: In both panels, the horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. The top panel illustrates the first stage; hence, the vertical axis measures the probability that the challenger is appointed as mayor. The bottom panel plots the reduced-form relationship between running variable and outcome. Solid lines represent third degree polynomials in the running variable estimated separately for positive and negative values around the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

plan of roughly 0.15.

We now turn to our main empirical results, which are the 2SLS estimates reported in

Table 3.¹⁷ Column 1 reports the effect of a change in the party in power on the probability of presenting a plan. The estimated effect is large and statistically significant, indicating that it is 30 percentage points more likely that newly elected governments submit an adjustment plan than ongoing incumbents. The first-stage F-statistic is 112, well above the conventional threshold for weak instruments. In columns 2 and 3 we add controls. Column 2 includes the controls displayed in Figure 5, which are demographic and financial characteristics of municipalities.¹⁸ Column 3 includes the controls displayed in Figure A.1 of the Appendix, which are characteristics of the incumbent government in power before the 2011 election. The effect remains significant and similar in magnitude.

TABLE 3
CHANGE IN OFFICE & ADJUSTMENT PLANS

	(1)	(2)	(3)
	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.311*** (0.101)	0.293*** (0.102)	0.271** (0.105)
Observations	1097	1037	1081
Bandwidth	.138	.134	.148
First-stage Fstat	112	95	108
Controls	No	Municipality	Prev Govmnt

Notes: The table presents two stage least squares estimates of the effect of a change in municipal government on the probability of presenting an adjustment plan. The first column does not include controls. The second column controls for the municipal characteristics. The third column controls for the previous government characteristics. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

In sum, the results in Table 3 are in line with our hypothesis: newly elected governments are more likely to choose a smoother adjustment than ongoing incumbents. We posit that this reluctance stems from incumbents' hesitation to publicize the poor state of public finances, which they may have contributed to creating. We discuss evidence for this and other mechanisms in section 5.

4.3. Robustness Checks

We now discuss several complementary results to illustrate the robustness of our main findings regarding the SPP policy.

¹⁷We report the associated first-stage coefficients in Appendix Table A.5.

¹⁸We do not include the logarithm of municipal spending per capita because we do not have this information for many municipalities. Still, despite the change in sample size if we include this control in the regression, results remain qualitatively similar.

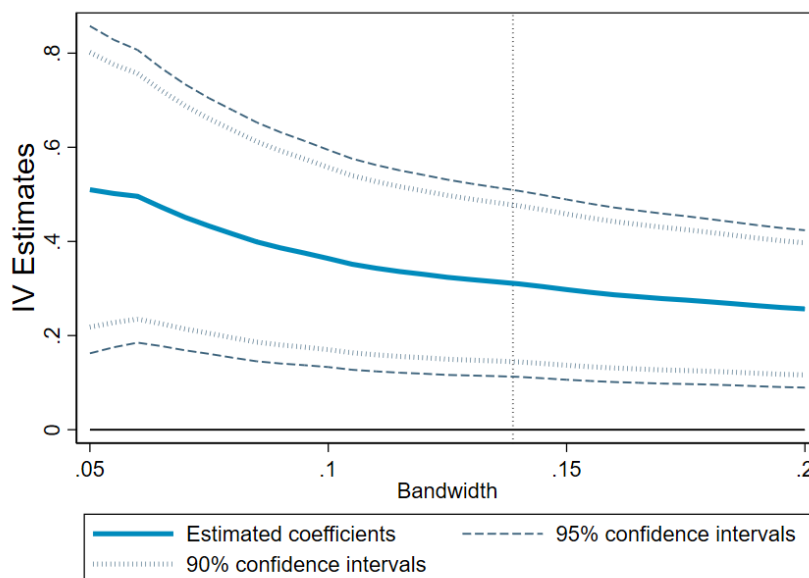
First, we reproduce our RD estimates using an alternative definition of the dependent variable. In our main analysis, we define the dependent variable as a dummy that takes a value of 1 if the municipal government presents a plan, and a value of 0 otherwise. We use this definition because we are trying to understand government decision-making and local governments only decide whether to submit a plan, not whether that plan will ultimately be approved by the national government. That said, we can use an alternative definition which only takes a value of 1 if the municipality presents a plan that is approved. This amounts to classifying those municipalities that present a plan that is not approved together with those that do not present a plan at all. Appendix Table A.6 shows this change in the definition of the dependent variable makes no qualitative difference to our results. This is perhaps not surprising, as less than 7% of all local adjustment plans in the context of the SPP were rejected by the national government.

We also explore the sensitivity of our RD estimates to bandwidth choice. As explained in Section 4.1, the estimation of our parameter of interest uses the routine proposed in Calonico, Cattaneo, Farrell, and Titiunik (2017), which incorporates data-driven procedures to select a bandwidth and adjusts standard-errors to account for the bandwidth selector. In Figure 7, we evaluate the stability of our main estimated effect for different bandwidths around the threshold. We show that, for all bandwidth choices in the the [5%, 20%] interval, our coefficient of interest is statistically significant at 95% confidence intervals and comparable in magnitude to those reported in Table 3.

Our baseline estimates are obtained using a triangular kernel to weight observations around the threshold and a linear polynomial to control for values of the running variable. We can show that these methodological choices are not important in our case. In Appendix Table A.7, we report estimates of the effect of interest when controlling for higher-order polynomials in the running variable and when using a uniform kernel to weight observations. The resulting point estimates fall in the range between 0.2 and 0.4 and are always significant at conventional levels.

Finally, we consider an alternative definition of the running variable. One of the insights present in Folke (2014) and Fiva, Folke, and Sørensen (2018) is that, in multi-party systems, the distance to a change in either the composition of the local council or who wins the election depends on the number of parties running in that election and on the associated distribution of vote shares. In our main analysis, the main running variable is simply defined as the distance between the vote shares of the challenger and the incumbent. Alternatively, we

FIGURE 7
ROBUSTNESS OF RD ESTIMATES TO BANDWIDTH CHOICE



Notes: The horizontal axis represents different bandwidths around the threshold. The vertical axis represents the size of the estimated effect of having a new government on the probability of presenting an adjustment plan. The solid line corresponds to point estimates for different bandwidths. Dotted and dashed lines represent 90% and 95% confidence intervals, respectively.

can determine the running variable by calculating the proportion of votes we would have to re-distribute from the challenger to all other parties running in that local election until that challenger changes from winning to losing the election or vice-versa. This is done by assigning redistributed votes across parties based on their initial vote shares. Estimates of the effect of having a new mayor on the probability of submitting an adjustment plan, obtained when using this alternative running variable, are provided in column 2 of Appendix Table A.8. Reassuringly, the estimate of 0.286 is similar to the one reported in our main analysis.

5. Mechanisms

Our baseline results show that new governments in power have a higher probability of presenting an adjustment plan. We posit that this difference is driven by the fact that presenting an adjustment plan makes the financial problems of the municipality more salient. While a new leadership can blame the previous incumbent for these problems, a continuing incumbent may not be willing to reveal these problems to their electors, even if that implies choosing a sub-optimal policy. In this section, we provide evidence that is consistent with this

mechanism. First, we complement the descriptive results presented in Section 3.4 and use data from Spanish media outlets to explore the impact of presenting an adjustment plan on the voters’ information set. Second, we provide evidence consistent with incumbents avoiding to present an adjustment plan to protect their information rents. Finally, we discuss and discard several plausible alternative mechanisms.

5.1. Impact of the Adjustment Plan on the Voters’ Information Set

We analyze Factiva data to assess the impact of presenting an adjustment plan on voters’ awareness of municipal involvement in the SPP. Using a difference-in-differences approach, we estimate the effect of the local government’s repayment choice on press coverage regarding their participation in the program. Our analysis is based on a panel of municipalities from 2011 to 2013, allowing us to estimate the parameters outlined in equation 5:

$$I(News_{it}) = \alpha + \beta_1 Adj.Plan_i + \beta_2 Post_t + \beta_3 Post_t Adj.Plan_i + \gamma'_1 X_{it} + u_{it} \quad (5)$$

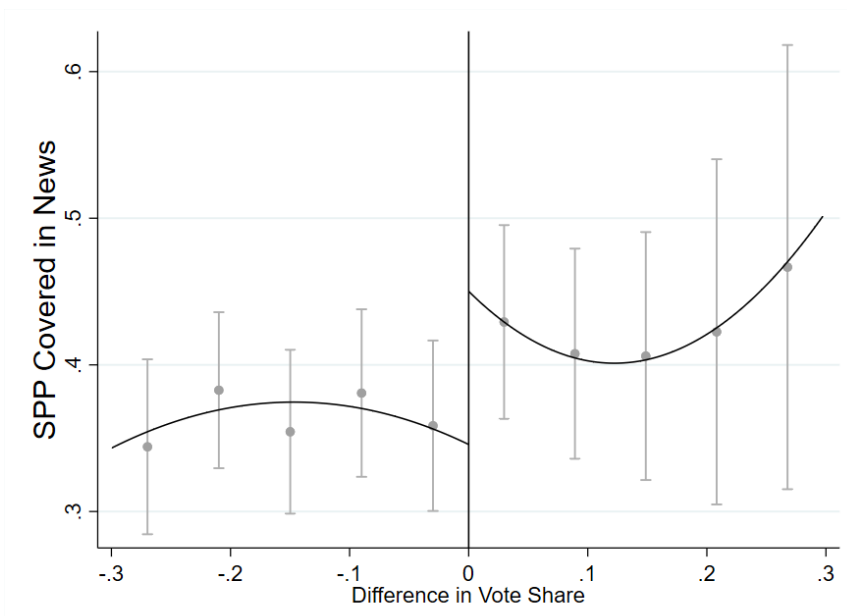
$I(News_{it})$ is a dummy that takes value 1 if *Supplier Payment Program* appears in the news together with the name of the municipality, and zero otherwise. We interpret this variable as a proxy of the visibility of a local government’s involvement with the program.¹⁹ $Adj.Plan_i$ is a dummy that takes value one for municipalities that present an adjustment plan, and zero otherwise. $Post_t$ is a dummy variable that takes value one in 2012 and 2013, and value zero in 2011. Results reported in Appendix Table A.9 indicate that media coverage of SPP is more frequent in municipalities that present an adjustment plan than in those that do not. This finding is robust to controlling for municipality fixed effects and the yearly total number of news per municipality. We interpret it as evidence that presenting a plan impacts voters’ information set.

We also use data on SPP press reports to document how coverage of the policy changes with government turnover. For this purpose, we leverage the RD strategy discussed above. We estimate a modified version of the system in equations 3 and 4, in which the second-stage outcome is replaced by dummy $I(News_j)$, taking value 1 if coverage of SPP associated to municipality j appeared in the press during 2012 and 2013. The associated reduced-form graph is provided in Figure 8. This shows a significant increase in SPP coverage at the

¹⁹We can instead use the log of the number of SPP news as the dependent variable in equation 5. This leads to the same qualitative findings.

threshold. Second-stage estimates, reported in Appendix Table A.10, are in line with this result. A change of the party in power is associated with a 20 percentage point increase in news coverage of the SPP.

FIGURE 8
EFFECT OF CHANGE IN GOVERNMENT ON SPP NEWS COVERAGE



Notes: The figure presents reduced-form relationship between the running variable and the probability that a municipality is featured in news about the SPP in either 2012 or 2013. The horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. Solid lines represent third degree polynomials in the running variable estimated separately for positive and negative values around the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

This result is consistent with two associated facts: i) government turnover increases the probability of submitting an adjustment plan, and ii) submitting a plan increases the press coverage of SPP. Moreover, a second implication of this differential coverage is that it can provide a vehicle through which newly elected governments highlight the limitations of the previous incumbent in power. Appendix C provides two examples of this behavior, one featuring a PP newcomer (center-right), and another a PSOE newcomer (center-left). In both of them, the text evidences how the mayor makes an effort to pass the buck to the previous administration for the volume of unpaid debt to suppliers.

5.2. The Role of Information Rents

Previous papers have shown compelling evidence that electors dislike budget deficits (Brender, 2003; Brender and Drazen, 2008; Drazen and Eslava, 2010). Therefore, it is reasonable to assume that when they find that their government has accumulated arrears, they

TABLE 4
CHANGE IN OFFICE & ADJUSTMENT PLAN - HETEROGENEITY

	(1)	(2)	(3)
	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.040 (0.296)	0.413*** (0.131)	0.407*** (0.151)
Observations	283	422	343
Bandwidth	.11	.164	.124
Plan Proportion	.516	.859	0.828
Value of Arrears	Bottom Tercile	Middle Tercile	Upper Tercile

Notes: The table presents two stage least squares estimates of the effect of a change in municipal government on the probability of presenting an adjustment plan. The first column reports the effect for those municipalities in the bottom tercile of arrears, the second column for municipalities in the middle tercile, and the third column for municipalities in the upper tercile. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

negatively update their beliefs about its quality. In this section, we analyze three pieces of evidence which are consistent with incumbents avoiding to present an adjustment plan to protect their information rents.

First, in Table 4, we run the main specification for three different sub-samples. We divide municipalities according to their pre-existent level of arrears, and explore the effect of a change in office on the probability of presenting a plan. We find no effect for those municipalities in the bottom tercile of the distribution (column 1). The prevalence of presenting a plan for this subsample is 49.4%, and there is no significant difference depending on government's tenure. This changes for the second and third terciles of the arrears distribution (columns 2 and 3). The average probability of presenting a plan rises above 80%, but there are significant differences by tenure. While the majority of newcomers present a plan when the level of arrears is sufficiently high, many re-elected incumbents remain reluctant to do so. This is consistent with the fact that, for challengers, the higher the level of arrears, the larger the gains of presenting an adjustment plan. Conversely, continuing incumbents may fear about the signal that they send to their electorate when arrears are high.

Second, we explore information that Prof. Pedro Rey-Biel and his team gathered in a survey to 126 Spanish Mayors.²⁰ This survey seeks to understand the determinants of evidence-based policy implementation. It includes 31 items including gender, age, level of studies and diverse questions surveying policy evaluation habits, relevance of different economic sectors for the municipality, willingness to get information about policy efficiency, and others. Before

²⁰Project "Policonstraints", Social Research Grant of Fundaci3n La Caixa.

they run the survey, we introduced specific questions about how to carry a fiscal adjustment, policy changes and government turnover.

Table 5 summarizes the answers to the subset of questions that are particularly relevant to our study. In the first question in Table 5, mayors were asked whether they would change a policy if they received evidence that it is not working. An overwhelming majority of mayors (over 90%) declared that they would indeed change it, while less than 3% declared that they would not.²¹ Then, in the follow-up question, they were asked whether they have ever changed a policy that was not working: only 50% declared to have done so.

This finding might simply point out that half of the mayors were never aware of having implemented any flawed policies. Nevertheless, there is another question in the survey which might point otherwise. It asks mayors how much they agree with the following sentence: *"We are human beings and we all make mistakes. Sadly, often we cannot correct past mistakes the way we should, because the opposition would use this to make our errors more salient"*. We were surprised to find that over 20% of surveyed mayors declared to strongly agree with this sentence. We must consider that we are surveying professional politicians. Therefore, we were expecting answers which signal virtue, in line with those in the first question, where the overwhelming majority declared they would correct a policy if they received evidence that it is flawed. By contrast, in this case less than 30% of mayors disagree with the aforementioned statement. This gives us confidence that the mechanism that we are proposing is plausible and in line with the stated views of a subset of local government officials.

Finally, the last question in Table 5 was specifically introduced to learn about our setting. It shows that mayors consider more often that a newly elected government is in a better position to navigate financial problems, compared to an incumbent with responsibility in the previous administration. Taken all together, we consider this evidence consistent with the idea that politicians find it costly to admit previous mistakes and act in consequence.

As a final exercise to emphasize the role of differences in incentives by tenure in office, we use data on the outcome of the municipal elections of 2015 to explore the relation between presenting a public adjustment plan to get better financing conditions and the probability of re-election. Namely, we estimate the following equation:

²¹6% answered *maybe*. In the comment section they qualify their answer raising the following issues: *Does this evidence come from a sample of similar municipalities to my own?, Are the policies in the evaluation really similar to mine?, Is the historical context comparable?*

TABLE 5
SURVEY TO A SAMPLE OF SPANISH MAYORS

	Yes	No	Maybe
1. Would you change a policy if you receive rigorous evidence that it is not working, or that there are better alternatives?	90.7%	2.8%	6.5%
2. Have you ever changed a policy that was not working?	52.3%	46.7%	
	Strongly Agree	Neither Agree nor Disagree	Strongly Disagree
3. How much do you agree with the following statement: 'We are human beings and we all make mistakes. Sadly, often we cannot correct past mistakes the way we should, because the opposition would use this to make our errors more salient'.	20.4%	52.0%	27.5%
	A Newcomer	An Ongoing Incumbent	Does not make any difference
4. Suppose that a newly elected government starts the term with problems in the municipal accounts (for instance: the municipality has trouble to pay its suppliers). This situation would be easier to address for:	36.7%	13.2%	50.0%

Notes: Answers to a subset of selected questions in the context of the "POLICONSTRAINTS" project, Social Research Grant of Fundaci3n La Caixa, directed by Prof. Rey-Biel. 126 mayors from a sample of Spanish municipalities answered this survey.

$$R_j^{2015} = \alpha_0 + \alpha_1 I_j + \alpha_2 Adj.Plan_j + \alpha_3 Adj.Plan_j \times I_j + \gamma X_j + u_j \quad (6)$$

where R_j^{2015} is a dummy taking a value of 1 if the party in power before the 2015 election was re-elected. I_j (Incumbent) takes a value of 1 if the party in power after the election of 2011 was the same as the one in power in 2010 before the election – the one who built up the arrears –. $Adj.Plan_j$ is a dummy taking a value of 1 if the municipality presents an adjustment plan, and X_j is a set of controls including population, debt per capita and outstanding arrears per capita in 2011. The coefficient of interest is α_3 , which indicates the differential re-election probability between incumbents that presented a plan and incumbents that did not present a plan (estimated conditional on presenting a plan). Naturally, the assumptions involved for causal interpretation of α_3 are quite strong in this context, as presenting the plan is an endogenous decision by the government. Thus, we only interpret our findings as suggestive or descriptive in this context.

Estimates for the coefficients in equation 6 are provided in Appendix Table A.11, along with the combined effect of α_2 and α_3 . We observe that governments who agree on an adjustment plan with the national government are significantly less likely to be re-elected than

those which did not present a plan, but only if they were incumbents before 2011. We do not find this is true for newcomers. This is consistent with our proposed mechanism, which suggests that continuing incumbents may bear an electoral cost if they present a plan because it would reveal poor past performance.

5.3. *Alternative Mechanisms: Observed Elected Government Characteristics*

Although all the evidence we have explored so far is consistent with the information hypothesis, there are several alternative mechanisms that are worth exploring. Among them, probably the most natural one is that something else is changing after a change in office, besides tenure.

In Appendix Table [A.12](#) we conduct a 2SLS regression similar to that of our main analyses, using as dependent variables elected government characteristics. We find that the effect of interest is statistically insignificant for all observable characteristics, except for the age of the elected mayor and her partisan affiliation.²²

These findings are predictable. First, mayor's age decreases at the threshold because newcomers are generally younger than incumbents. Reassuringly, we show that baseline results remain the same if we control for mayor's age. Second, PSOE won the majority of the local elections in 2007. Hence, if there is a change in the local government, this will on average be associated with a reduction in the probability of having a PSOE mayor. Likewise, and for the same reason, the probability of having a PP mayor increases at the threshold.

Differences in ideology and/or alignment with the national government could provide alternative mechanisms to explain differences in the propensity to submit an adjustment plan. We follow three different strategies to test whether these are indeed the mechanisms driving our baseline results. First, we control for who is the mayor in 2012 in our main specification. Second, we explore the effect of a change in office segregating the sample according to the party of the incumbent, the party of the challenger, and the party of the mayor in 2012. Finally, we consider an alternative estimation strategy where we estimate the effects of having a mayor from PP or PSOE on the probability of presenting an adjustment plan.

Estimates obtained after trying to account for elected government characteristics are reported in Table [6](#). Column 1 in panel A reproduces our baseline specification for comparison purposes. In column 2, we include two dummy variables that take value one when

²²We show graphically how confounders vary at the threshold in Appendix Figure [A.2](#).

the incumbent is from PP and from PSOE, along with a control for the elected mayor's age. The estimated coefficient of interest continues to be large and statistically significant. In columns 3 and 4 we estimate our main specification after restricting the sample to municipalities where the incumbent was from PSOE and from PP, respectively. We continue to find large and significant effects for both sub-samples, indicating that challengers are more likely to present an adjustment plan, no matter whether incumbents are from PP and PSOE. We do something analogous in columns 1 and 2 of panel B. We report RDD estimates obtained for the sub-samples of municipalities with PSOE and PP challengers, respectively. Again, the effect of a change in mayoral party on the probability of presenting a plan is large and positive. Both challengers from PSOE and from PP are significantly more likely to present an adjustment plan than the incumbents in their respective municipalities. Lastly, in columns 3 and 4 of panel B we provide estimates for different sub-samples based on the party in power *after* the 2011 election. In column 3 we restrict the sample to municipalities ruled by PSOE in 2012. We observe a positive and significant coefficient, of a magnitude comparable to those reported in panel A. This shows that mayors from PSOE who were challengers in the previous term are more likely to present an adjustment plan than mayors from PSOE who were incumbents. In column 4, we replicate this result for PP mayors. Albeit imprecisely estimated due to the reduced sample size, the sign and size of the coefficient suggests that mayors from PP that were challengers in the previous term are more likely to present a plan than mayors from PP who were incumbents.

We can follow an alternative strategy to rule out that political affiliation is driving our main results. We modify our research design to analyze specifically whether either of the main parties is more or less likely to present an adjustment plan to smooth out the payment of arrears. We do so for both PSOE and PP, which controlled most municipalities in Spain since the late 1980s (including the 2010-2015 period). For this purpose, we estimate:

$$M_j^p = \alpha_0 + \tau D_j^p + \eta_1 \Delta V_j^p + \eta_2 D_j \Delta V_j^p + v_j \quad (7)$$

$$P_j = \alpha_1 + \beta M_j^p + \phi_1 \Delta V_j^p + \phi_2 D_j \Delta V_j^p + u_j \quad (8)$$

Where M_j^p is a dummy taking value 1 if municipality j appointed a mayor from party $p = \{\text{PSOE}, \text{PP}\}$ after the 2011 election, ΔV_j^p is the vote margin for party p in municipality

TABLE 6
LEADERSHIP CHANGE & ADJUSTMENT PLANS BY PARTY

	(1)	(2)	(3)	(4)
Panel A	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.311*** (0.101)	0.271*** (0.0837)	0.280*** (0.108)	0.421** (0.196)
Observations	1097	1064	532	275
Bandwidth	.138	.175	.14	.166
Specification	Baseline	Inc. Control	PSOE Inc.	PP Inc.
	(1)	(2)	(3)	(4)
Panel B	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.291*** (0.110)	0.306** (0.124)	0.267* (0.158)	0.346 (0.233)
Observations	565	418	450	229
Bandwidth	.211	.133	.19	.128
Sample 2012	PSOE Challengers	PP Challengers	PSOE Mayors	PP Mayors

Notes: The table presents two stages least squares estimates of the effect of a change in municipal government on the probability of presenting an adjustment plan. In panel A, the first column is the baseline specification, the second column includes mayor’s age, and dummies for PP incumbent and PSOE incumbent as controls, the third column restricts the sample to municipalities with a PSOE incumbent and the fourth column to municipalities with a PP incumbent. In panel B the first column restricts the sample to municipalities with a PSOE challenger, the second column to municipalities with a PP challenger, the third column to municipalities ruled by a PSOE Mayor, and the fourth column to municipalities ruled by a PP Mayor. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold and control for some municipality and government characteristics. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

j in that election and $D_j^p \equiv \mathbb{1}\{\Delta V_j^p > 0\}$.²³ We implement our estimation as we do in our main exercise, using the local linear regression methods in the routine described in [Calonico, Cattaneo, Farrell, and Titiunik \(2017\)](#). The parameter of interest β measures whether party p is more or less likely to opt for an adjustment plan. Results for this exercise are reported in Table 7. Columns 1 and 2 report the effect of having a PP mayor on the probability of presenting an adjustment plan, and columns 3 and 4 report the effect of having a PSOE mayor on the probability of presenting an adjustment plan. Columns 2 and 4 include our usual set of covariates. We find insignificant effects across the board for both parties. The absolute value of the point estimates are at most 1/9 of the effects reported in Table 3, providing conclusive evidence that our main effect of interest is not driven by partisan differences in the propensity to submit an adjustment plan.

Altogether, we conclude that the mayors’ observable characteristics that jump at the

²³ ΔV_j^p , the vote margin for party p , is the difference in vote shares between party p and the most voted party in j after excluding p . We restrict our attention to municipalities in which p is either the mayor, or the most voted opposition party.

TABLE 7
PARTY MAYOR & ADJUSTMENT PLAN

	(1)	(2)	(3)	(4)
	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	-0.00807 (0.0989)	-0.0374 (0.0998)	0.0346 (0.0896)	0.0221 (0.0829)
Observations	1215	966	1575	1294
Controls	No	Yes	No	Yes
Instrumented Var.	PP Mayor	PP Mayor	PSOE Mayor	PSOE Mayor
p-value	0.935	0.708	0.700	0.790
Bandwidth	0.190	0.187	0.227	0.230

Notes: The table presents two stages least squares estimates of the effect of a PP / PSOE mayor on the probability of presenting an adjustment plan. The first two columns instrument for a PP mayor. The first column adds no controls, while the second column controls for arrears per capita and mayor's age. The third and fourth columns instrument for a PSOE mayor. The third column adds no controls, while the fourth column controls for arrears per capita and mayor's age. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

threshold do not explain the results. Likewise, other factors that could be relevant to approve the plan, as the government having the majority, or not having plurality, do not jump at the threshold.

5.4. *Alternative Mechanism: Candidate's Quality*

We have shown that observed mayor's characteristics do not explain why newcomers are more prone to present an adjustment plan than continuing incumbents. Nevertheless, as [Marshall \(2022\)](#) points out, there may be unobservable characteristics of the candidates, which influence electoral performance and may be unbalanced around the threshold. If that is the case, these unobservable characteristics would confound the RD estimates. In what follows, we deal with this concern, paying special attention to one of these possible compensating differentials: candidates' quality.

Papers like [Gelman and King \(1990\)](#) or [Lee \(2008\)](#) have found that incumbents enjoy an electoral advantage. Hence, an average incumbent who faces a close election despite this advantage might be a candidate of relatively poor quality. Moreover, as shown by [Gordon, Huber, and Landa \(2007\)](#) and [Ban, Llaudet, and Snyder Jr \(2016\)](#), the opposition might strategically react to this circumstance. They might nominate their most qualified politicians to compete against poor quality incumbents, expecting to override the incumbency effect. If close elections systematically involve incumbents of relatively low quality matched with challengers of relatively high quality, it is possible that candidate's skills explain the effect of government turnover in the type of adjustment. Newcomers might present an adjustment

plan more often than re-elected incumbents just because they are more competent and it is the superior policy. We explore this hypothesis following three different strategies.

First, we utilize data proxies as indicators of mayor’s quality – such as educational attainment and occupation before taking office – to test whether they are balanced at the threshold. Education and labour market outcomes are often used as proxies of candidate quality in the political science and economics literature (see e.g., [Galasso and Nannicini 2011](#) or [Baltrunaite, Bello, Casarico, and Profeta 2014](#)). Results reported in Appendix Table [A.12](#) indicate that our proxies of quality vary smoothly at the threshold. Naturally, proxies such as indicators for college education or white collar occupation are very imperfect measures of politicians’ competence. Thus, we may still be concerned about the presence of other quality differences that are unobservable to the econometrician.

Our second approach to deal with potential differences in quality relies on the spatial nature of the electoral data in our sample. Following [George \(2019\)](#), we hypothesize that candidate’s votes depend both on their competence/quality and on the popularity of the regional branch of their party platform.²⁴ Thus, a good candidate can obtain a bad result due to a negative shock to her party in her region, and viceversa. We use this feature to create sub-samples of competing candidates with varying differences in quality.

For this purpose, we first construct the leave-one-out average vote swing experienced by candidate n ’s party p in her province s during the election of 2011:

$$Partyswing_{nps} = \sum_{j=1, j \neq n}^N \frac{V_{pjs2011} - V_{pjs2007}}{N - 1}$$

Then, we compute the “no swing” margin of the challenger ($n = c$) over the incumbent ($n = i$) as follows:

$$NoSwingMargin_{cps} = Margin_{cps} - Partyswing_{cps} + Partyswing_{ips} \quad (9)$$

This amounts to estimating the challenger’s margin after detracting regional party shocks. Thus, in municipalities where the challenger has a negative “no swing” margin, the incumbent would have won the election in the absence of regional party shocks. Conversely, in those where the challenger has a positive “no swing” margin, the incumbent would have lost it. Detracting regional party shocks increases the relevance of candidates’ quality on

²⁴Regional variation alone explains a substantial part of electoral performance in this period. In the 2011 elections, between-province variation explained over 50% of the variance in the vote shares of PP, the most voted party in that election.

electoral performance. Therefore, it seems reasonable to claim that those incumbents who, after detracting regional party shocks, would have won the election, are of relatively better quality than those that would have lost it.

Results in column 1 of Table 8 show that our effect of interest in the subsample with a low quality of challengers relative to incumbents (negative “no swing” margin) is roughly 0.3, similar to our baseline RD estimate.²⁵ A very similar estimate is observed in column 2 of Table 8, where we focus on the subsample with a high relative quality of challengers. These findings suggest that differences in quality between both types of candidates at the threshold do not explain our main findings.

TABLE 8
HETEROGENEITY ANALYSIS - CANDIDATES QUALITY

	(1) Adjustment Plan	(2) Adjustment Plan
Party Change	0.302** (0.123)	0.298* (0.163)
Observations	656	438
Bandwidth	.138	.138
First-stage Fstat	56	50
Sample	High Quality Incumbents	Low Quality Incumbents

Notes: The table presents two stages least squares estimates of the effect of a change in office on the probability of presenting an adjustment plan. In the first column, we restrict our attention to municipalities in which incumbents would have won the election in the absence of party shocks. In the second column, we restrict our attention to municipalities in which incumbents would have lost the election in the absence of party shocks. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

Finally, we test whether the main result of the paper is driven by this or any other difference in unobservable characteristics at the threshold, following the approach proposed in Angrist and Rokkanen (2015). Under a conditional independence assumption (CIA), this method allows us to obtain estimates of the RD coefficient corresponding to observations away from the cut-off.

We first present evidence regarding the conditional independence of the running variable. The tests reported in Appendix Figure A.3 show that, conditional on our set of controls, presenting an adjustment plan is unrelated to the running variable. We interpret this as evidence that the CIA required to follow the method in Angrist and Rokkanen (2015) is plausible. In Appendix Table A.13, we report two CIA-based estimators of the RD coefficient

²⁵We perform the analysis setting the bandwidth to be the same as in our main specification. If, instead, we re-calculate the optimal bandwidth we obtain qualitatively similar results.

away of the cut-off. Namely, we use the linear re-weighting method discussed in [Kline \(2011\)](#) and a version of the [Hirano, Imbens, and Ridder \(2003\)](#) propensity score estimator. Reassuringly, both yield positive and significant estimates of the effect of a change in office on the probability of presenting an adjustment plan away from the threshold. This indicates that the effect of interest does not arise due to any compensating differential drivers of electoral performance, including quality, varying at the threshold.

Visual inspection of figures 6 and 7 can be used to confirm this notion. In the reduced-form graph in Figure 6 we can observe that all bin averages to the right of the threshold are systematically above the corresponding averages to the left. Similarly, Figure 7 shows that the main RD estimates are robust to the use of different bandwidths, some including observations quite far from the threshold. This suggests that the relationship between changes of the party in power and the the probability of presenting an adjustment are also present outside the sample of close elections.

6. Conclusions

Since 1992, over 100 different countries have requested and obtained funding assistance from the IMF through 2,300 different funding agreements. Governments' decisions to negotiate and carry out funding agreements with national and international financial institutions are made by politicians constrained by their circumstances and incentives. In this paper, we study how tenure in office and electoral incentives affect these decisions.

Our main argument hinges around two points. First, agreements with institutional lenders are typically public and convey information about past government performance. Second, re-elected incumbents and newly elected governments differ in their incentive to take actions that convey information on past performance to voters. Therefore, we expect these two types of governments to make different choices when facing the decision to request financial assistance. To investigate this, we carry out an empirical analysis yielding both compelling cross-country evidence and causal estimates obtained from data on municipal participation in Spain's SPP.

Utilizing a close election regression discontinuity design, we find that new local governments in Spain are 30 percentage points more likely to present an adjustment plan to the national government. This difference is remarkable, especially considering that opting for such a backloaded adjustment is economically superior in this context. The evidence discussed in section 5 supports the role of incumbents' private information in explaining these

results. Overall, our findings highlight the influence of political expediency on achieving financial stability and inform the design of incentive-compatible bailout programs.

One final point is due regarding the external validity of our findings. As discussed in [Ashworth, Berry, and Bueno de Mesquita \(2021\)](#), it is common for studies in political economics and political science to focus on local governments when studying questions motivated by national politics. The reason is the same that motivates our analysis using SPP information: local government actions are easier to study using the methods of contemporary causal inference. As argued by these authors, the extrapolation between levels is predicated on the notion that both targets are similar. In our case, both local and national governments requesting assistance face a similar trade-off between information disclosure and short-term financial needs. This along with the fact that observed patterns for Spanish municipal governments are similar to those of national governments throughout the last three decades, suggests that external validity is warranted in this context. Moreover, taking on account that the debt issued by sub-national institutions represents over 23% of total public debt in OECD countries, we consider that understanding its dynamics is interesting in its own right.

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Appendix

Appendix A. Appendix Figures & Tables

TABLE A.1
DESCRIPTIVES AND T-TESTS

	Mean differences and T-test		
	Challenger	Incumbent	Difference
Population	12569.563	10974.101	1595.462
Outstanding Debt pc	346.910	336.455	10.455
Total Spending pc	1010.466	1125.667	-115.201***
Total Revenues pc	991.009	1108.272	-117.263***
Arrears pc	340.228	314.250	25.978
Party PP	0.549	0.429	0.121***
Party PSOE	0.184	0.421	-0.237***
Adjustment Plan	0.746	0.668	0.078***

Notes: This table reports means in 2011 for several variables for municipalities in which the challenger won the elections in 2011 (column 1), and for municipalities in which the incumbent won the elections in 2011 (column 2). The last column shows the difference in means for the two groups and its significance. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.2
NPV DIFFERENCE OF PRESENTING VS. NOT PRESENTING AN ADJUSTMENT PLAN

	min	max	mean	sd
All Municipalites	0.00	369.62	8.83	13.21
Mun. with No Adj. Plan	0.00	369.62	8.61	16.88
Mun. with Adj. Plan	0.10	116.36	8.99	9.98

Notes: This table reports the minimum, the maximum, the mean and the standard deviation of the difference in NPV per capita of presenting vs. not presenting an adjustment plan for municipalities with arrears. This estimation uses Madrid's March-April 2011 10Y bond yield as municipalities' discount rate. This is arguably a lower bound for the discount rate of most municipalities in March 2012, which makes our estimation also a lower bound. We report the difference for the full sample of municipalities with arrears, for those that present an adjustment plan to the national government, and for those that do not. The average size of a municipality with arrears is 9,342 inhabitants.

TABLE A.3
BALANCING CHECKS – PREDETERMINED MUNICIPAL CHARACTERISTICS

	(1)	(2)	(3)
	Population (000s)	Household Size	Employment p.c.
Party Change	1.917 (5.475)	0.0516 (0.0634)	-0.00732 (0.0291)
Observations	1225	1216	1084
p-value	0.726	0.416	0.802
Bandwidth	0.157	0.156	0.136
	Log(Spending)	Treasury Balance p.c.	Debt p.c.
Party Change	0.143 (0.337)	45.79 (110.1)	67.03 (79.11)
Observations	1174	1263	1294
p-value	0.672	0.677	0.397
Bandwidth	0.152	0.168	0.170
	Total Arrears p.c.	IBI Urban Rate	Log(Transfers)
Party Change	-20.33 (98.46)	-0.0166 (0.0347)	0.296 (0.356)
Observations	1145	918	1202
p-value	0.836	0.632	0.405
Bandwidth	0.147	0.116	0.156

Notes: Fuzzy-RD estimates, obtained via two-stage least squares, of the effect of the party change dummy on different pre-determined municipal characteristics. Bandwidth selected using the CCT method for each variable and indicated in the foot of each panel. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.4
BALANCING CHECKS – PREVIOUS GOVERNMENT CHARACTERISTICS

	(1)	(2)	(3)
	One-party Majority	Seat Share Mayor	Minority Government
Party Change	-0.0682 (0.111)	-0.0121 (0.0251)	-0.0632 (0.0581)
Observations	1236	1260	1329
p-value	0.540	0.628	0.277
Bandwidth	0.161	0.164	0.175
	Female Mayor	Mayor w/College	White Collar Mayor
Party Change	-0.0799 (0.0955)	-0.0864 (0.140)	-0.0409 (0.139)
Observations	967	733	732
p-value	0.403	0.538	0.768
Bandwidth	0.121	0.122	0.136
	Age of Mayor	PSOE Incumbent	PP Incumbent
Party Change	3.908 (2.777)	-0.0344 (0.107)	-0.0174 (0.0987)
Observations	754	1357	1338
p-value	0.159	0.747	0.860
Bandwidth	0.101	0.178	0.176

Notes: Fuzzy-RD estimates, obtained via two-stage least squares, of the effect of the party change dummy on different pre-determined local government characteristics at the threshold. Characteristics pertain to the party in power or the mayor in office. Bandwidth selected using the CCT method for each variable and indicated in the foot of each panel. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.5
FIRST-STAGES - LEADERSHIP CHANGE

	(1)	(2)	(3)
	Party Change	Party Change	Party Change
Challenger wins Election	0.563*** (0.0530)	0.550*** (0.0561)	0.547*** (0.0526)
Observations	1094	1008	1074
Bandwidth	.138	.131	.148
First-stage Fstat	112	95	108
Controls	No	Municipality	Prev Govmnt

Notes: First-stage estimates for fuzzy regression-discontinuity. Outcome variable in all columns is a dummy taking value 1 if there was a change of the party in power in the 2011 election. In all columns we control for linear terms in the running variable estimates separately at each side of the threshold. First-stage F-statistics included in the table foot. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.6
ROBUSTNESS: ALTERNATIVE DEFINITION OF THE DEPENDENT VARIABLE

	(1)	(2)	(3)
	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.295*** (0.107)	0.234** (0.116)	0.233** (0.110)
Observations	1106	846	1096
Bandwidth	.14	.119	.151
Controls	No	Municipality	Prev Govmnt

Notes: The table presents two stage least squares estimates of the effect of a change in the party in power at the local level on the probability of submitting an adjustment plan that was accepted by the national government. The outcome variable is zero when a SPP eligible municipality presented an adjustment plan that was refused by the national government. The first column does not include controls. The second column controls for the municipal characteristics. The third column controls for the previous government characteristics. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.7
ROBUSTNESS: CHOICE OF POLYNOMIAL LENGTH AND KERNEL

	(1)	(2)	(3)	(4)	(5)
	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.346*** (0.118)	0.403*** (0.140)	0.217*** (0.0808)	0.393*** (0.136)	0.375*** (0.142)
Observations	1648	1982	1396	1192	1730
Bandwidth	.222	.282	.183	.152	.234
Kernel	Triangular	Triangular	Uniform	Uniform	Uniform
Polyn. Deg.	2	3	1	2	3

Notes: The table presents two stage least squares estimates of the effect of a change in the party in power at the local level on the probability of presenting an adjustment plan. The first column does not include controls. The second column controls for the municipal characteristics. The third column controls for the previous government characteristics. We report local linear regressions with triangular kernel and third degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.8
ROBUSTNESS: RUNNING VARIABLE ADAPTED TO PR ELECTORAL SYSTEM

	(1)	(2)
	Adjustment Plan	Adjustment Plan
Party Change	0.186*** (0.0600)	0.286*** (0.0972)
Majority Definition	Votes	Votes
Estimate	Reduced-Form	2SLS
Bandwidth	.069	.082
Observations	948	1100

Notes: The table presents two stage least squares estimates of the effect of a change in municipal government on the probability of presenting an adjustment plan. Running variable adapted to account for multi-party elections, as described in Section 4.3. No additional control included in these specifications. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.9
SPP PRESS COVERAGE & ADJUSTMENT PLANS

	(1)	(2)	(3)
	I(News Sup. Paym. Plan)	I(News Sup. Paym. Plan)	I(News Sup. Paym. Plan)
Adjustment Plan	0.004 (0.003)	0.004 (0.004)	
Post	0.124*** (0.008)	0.130*** (0.008)	0.129*** (0.010)
Adjustment Plan * Post	0.095*** (0.011)	0.093*** (0.011)	0.093*** (0.014)
Total number of news		0.012** (0.006)	-0.006 (0.014)
Observations	10,107	9,919	9,919
Municipality FE	NO	NO	YES
Mean Dep. Var	.133	.133	.133

Notes: The table presents estimates of the probability that written media mention *Supplier Payment Program* together with the name of municipalities that present or do not present an adjustment plan. Observations are at the municipality-year level. The dependent variable is a dummy variable that takes a value of one if *Supplier Payment Program* appears in the news together with the name of the municipality, and zero otherwise. The unconditional mean of the dependent variable is 0.136. *AdjustmentPlan* is a dummy variable that takes a value of one for municipalities that present an adjustment plan, and zero for those that do not. *Post* is a dummy variable that takes a value of one for years 2012 and 2013, and zero for year 2011. The second column controls for the yearly total number of news per municipality (in thousands). The third column includes municipality fixed effects. The sample period is 2011-2013. We report OLS estimates. Robust standard errors are clustered at the municipality level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.10
GOVERNMENT TURNOVER AND PRESS COVERAGE OF SPP

	(1)	(2)	(3)
	SPP News Coverage	SPP News Coverage	SPP News Coverage
Party Change	0.195* (0.109)	0.190* (0.104)	0.230* (0.130)
Observations	1350	1235	1052
Bandwidth	.176	.166	.144
Controls	No	Municipality	Prev Govmnt

Notes: The table presents two stage least squares estimates of the effect of a change in municipal government on the probability that a municipality is featured in news about the SPP as measured using the Factiva sample of news. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.11
MAYOR RE-ELECTED IN 2015

	(1)	(2)
	Re-Elected	Re-Elected
Incumbent 2010	0.149*** (0.0284)	0.152*** (0.0291)
Adjustment Plan	-0.0260 (0.0298)	-0.00635 (0.0304)
Incumbent 2010#Plan	-0.0529 (0.0354)	-0.0675* (0.0358)
Observations	3,621	3,579
Controls	NO	YES
p-value: $\alpha_2 + \alpha_3=0$.00003	.0001

Notes: The table shows OLS estimates with robust standard errors on the probability of re-election in 2015. We exclude from the sample municipalities with population, arrears or financial debt above the 99 percentile. The first coefficient is a dummy that takes value 1 if the mayor in 2011 after the election was the incumbent in 2010 and value zero otherwise. The second coefficient is a dummy that takes value one if the municipal government presents an adjustment plan, and value zero otherwise. The third coefficient is the interaction between the previous two. The first column controls for population, outstanding debt, arrears and a dummy that takes value one if the mayor is from Partido Popular. The second column adds province fixed effects. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.12
BALANCING CHECKS – ELECTED GOVERNMENT CHARACTERISTICS (2011)

	(1)	(2)	(3)	(4)
	One-party Majority	Seat Share Mayor	Minority Government	Female Mayor
Party Change	-0.0384 (0.0606)	-0.0157 (0.0122)	-0.0417 (0.0361)	-0.0548 (0.0693)
Observations	1255	1072	1179	598
Clusters	1260	1072	1179	598
p-value	0.527	0.199	0.248	0.429
Bandwidth	0.164	0.135	0.150	0.074
	Mayor w/College	Hi Sch. Drop-out	White Collar Mayor	Mayor Blue Collar
Party Change	-0.0176 (0.138)	-0.00145 (0.0505)	0.136 (0.124)	-0.0639 (0.105)
Observations	717	1046	853	807
Clusters	979	1405	1277	1219
p-value	0.898	0.977	0.276	0.542
Bandwidth	0.124	0.185	0.167	0.156
	Unemp. Mayor	Age of Elected Mayor	PSOE Mayor (2011)	PP Mayor (2011)
Party Change	-0.0338 (0.0339)	-5.102** (2.321)	-0.404*** (0.112)	0.273** (0.114)
Observations	848	903	1103	1153
Clusters	1271	1132	1103	1153
p-value	0.320	0.028	0.000	0.016
Bandwidth	0.165	0.144	0.139	0.146

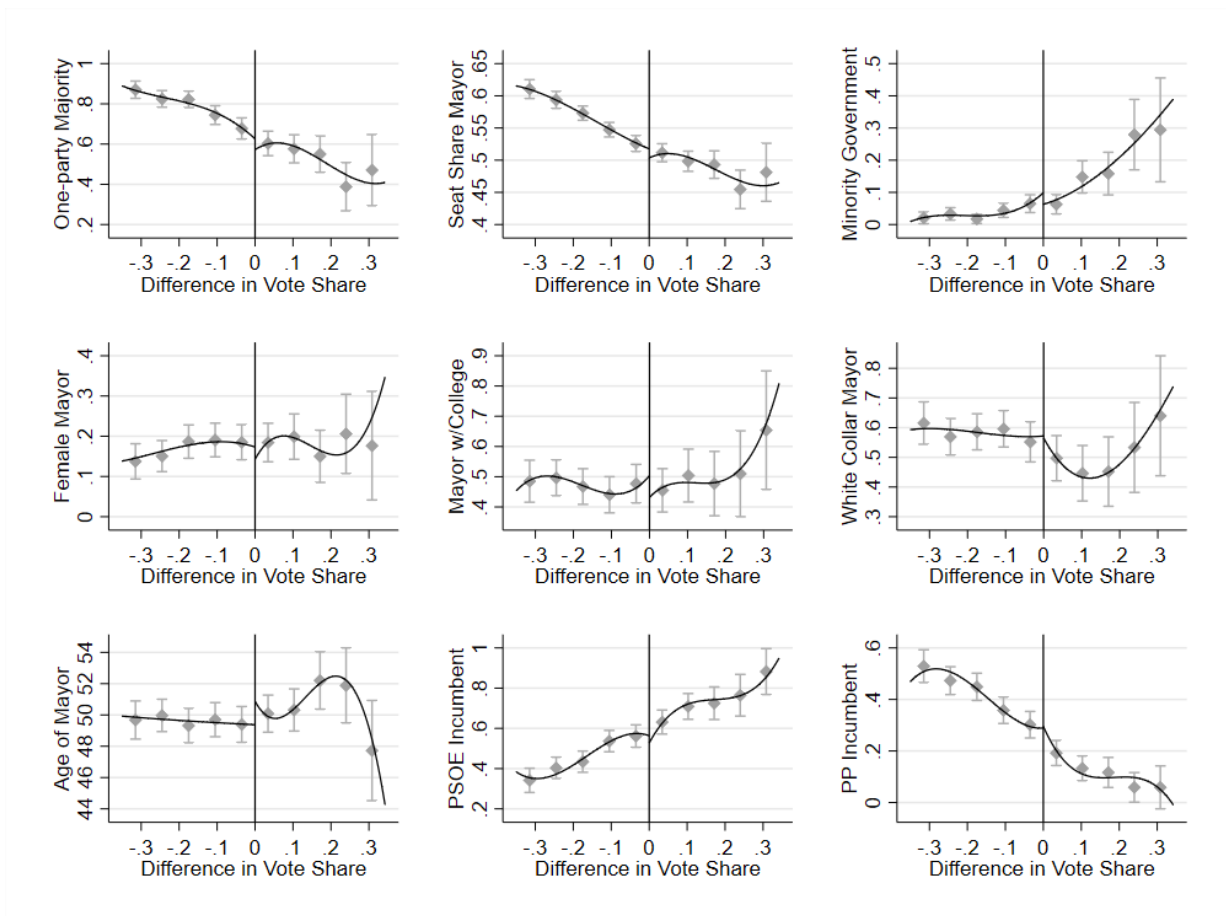
Notes: Fuzzy-RD estimates, obtained via two-stage least squares, of the effect of the party change dummy on different elected government characteristics at the threshold. Characteristics pertain to the party in power or the mayor in office. Bandwidth selected using the CCT method for each variable and indicated in the foot of each panel. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.13
EFFECT AWAY FROM THRESHOLD

	(1)	(2)	(3)
Estimand: Method:	Baseline RD CCT	Angrist & Rokkanen Kline	Angrist & Rokkanen Hirano
Estimate	.31*** (.1) [.11,.51]	.13*** (.04) [.06,.19]	.13*** (.04) [.08,.19]

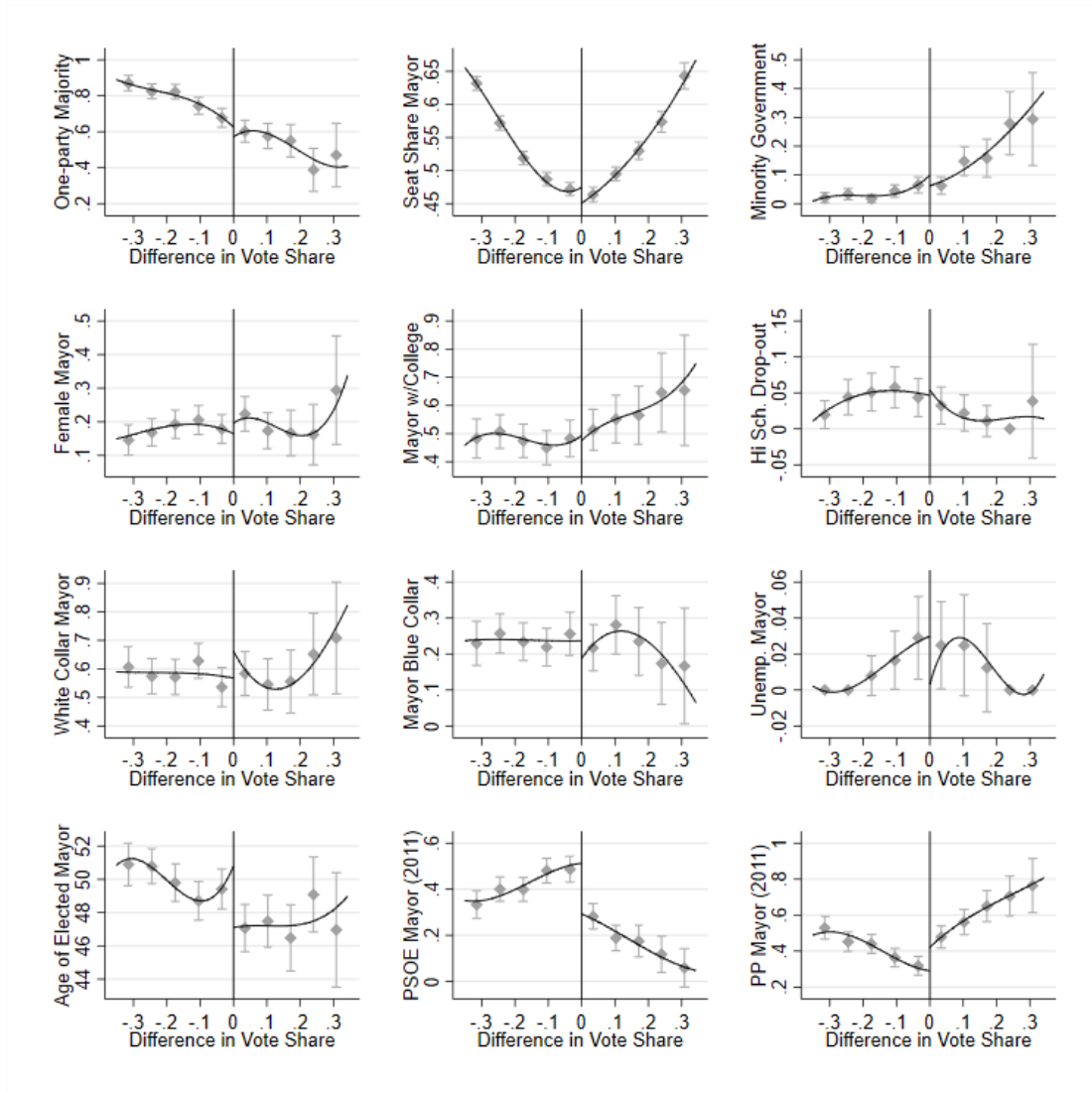
Notes: Fuzzy RD estimates of the effect a change of party on the probability of presenting an adjustment plan. In column 1 we present baseline RDD estimates using the CCT method and the associated bandwidth. In columns 2 and 3, we present estimates of these effects for observations within a 20% bandwidth as described in [Angrist and Rokkanen \(2015\)](#). In column 2, we use the Kline estimator and in column 3 we use a propensity score matching estimator. Standard errors in parentheses and 95% confidence intervals in square brackets. Inference in columns 2 and 3 based on the bootstrap method.

FIGURE A.1
COVARIATE BALANCING – PREVIOUS GOVERNMENT CHARACTERISTICS



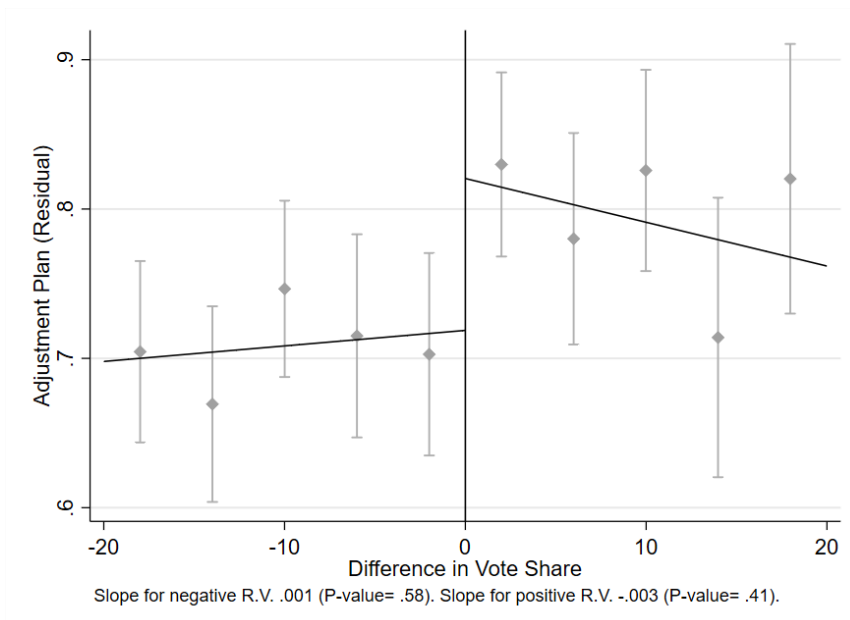
Notes: Balancing tests using the characteristics of the government in power in the period before the 2011 election. The horizontal axis represents the vote share difference between the challenger and the incumbent. From left to right and top to bottom the vertical axes represent fraction of one-party majorities, seat share of the mayor’s party, fraction of minority governments, fraction of female mayors, fraction of mayors with college studies, fraction of white collar mayors, mayors’ age, fraction of municipalities with PSOE major as incumbent, and fraction of municipalities with PP major as incumbent. Solid lines represent third degree polynomials in the running variable estimated separately for positive and negative polynomials. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

FIGURE A.2
COVARIATE BALANCING – ELECTED GOVERNMENT CHARACTERISTICS (2011)



Notes: The horizontal axis represents the vote share difference between the challenger and the incumbent. Solid lines represent third degree polynomials in the running variable estimated separately for positive and negative polynomials. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

FIGURE A.3
CONDITIONAL INDEPENDENCE ASSUMPTION TESTS (ANGRIST AND ROKKANEN 2015)



Notes: Figure shows the conditional relationship between the running variable (difference in vote share) and the Adjustment Plan outcome after controlling for municipal-level and previous government characteristics. Conditioning is carried out by residualizing the outcome. Separate linear terms estimated on both sides of the threshold. The corresponding slope coefficients on each side of the threshold and p-values of a significance test reported below the Figure. In both cases we cannot reject the null hypothesis of coefficients being jointly zero.

Appendix B. Cross-Country Analysis

Appendix B.1. Data Sources and Descriptives

To conduct our cross-country analysis of government turnover and external financial assistance, we build a country-level panel covering the period 1992-2019. This is assembled from different sources that we detail in the following. Features of the political organization of 180 countries covering the period the period 1975-2020 are obtained from the Database of Political Institutions (DPI2020). This also includes information on electoral results for democracies, as well as political orientation of the ruling party. Moreover, it includes a definition of politically competitive elections based on the criteria in [Ferree and Singh \(1999\)](#). We use this index to identify democratic regimes (we keep index values 6 and 7, see [Scartascini, Cruz, and Keefer 2021](#)). Data on IMF funding agreements is obtained from the IMF Monitoring of Fund Arrangements (MONA) Database. This contains the universe of arrangements with the IMF during the period 1992-2021, including all funding arrangements with up to 124 countries during the cited time window. Last, we obtain macroeconomic data from the World Economic Outlook Database, which includes a time series of different indicators for 195 countries for the period 1980-2021.

TABLE B.1
COUNTRY PANEL: DESCRIPTIVE STATISTICS

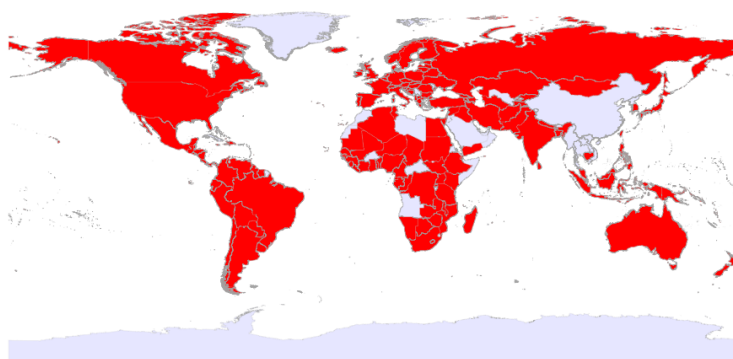
	Full Sample	Had IMF Program	No IMF Program
IMF Agreement	0.11 (0.31)	0.16 (0.36)	0.00 (0.00)
Party Change	0.54 (0.50)	0.57 (0.50)	0.48 (0.50)
GDP Growth Rate	6.89 (12.47)	7.65 (13.16)	5.21 (10.63)
Right-wing Gov	0.28 (0.45)	0.26 (0.44)	0.33 (0.47)
Left-wing Gov	0.26 (0.44)	0.20 (0.40)	0.38 (0.49)
GDP Level (Average)	345.58 (1,342.16)	102.21 (264.13)	891.36 (2,292.84)

Notes: Descriptives of the country panel used in the international analysis in Section 2. Selected panel variables in rows. Cells indicate averages and standard deviations (in parentheses) for each variable. Columns correspond to different sub-samples. The first column corresponds to the Full Sample including all countries with competitive elections. The second corresponds to the sub-sample of countries that had at least one funding agreement with the IMF in the 1992-2019 period. The final column presents descriptives for the sample of countries that had no funding agreements with the IMF in that period.

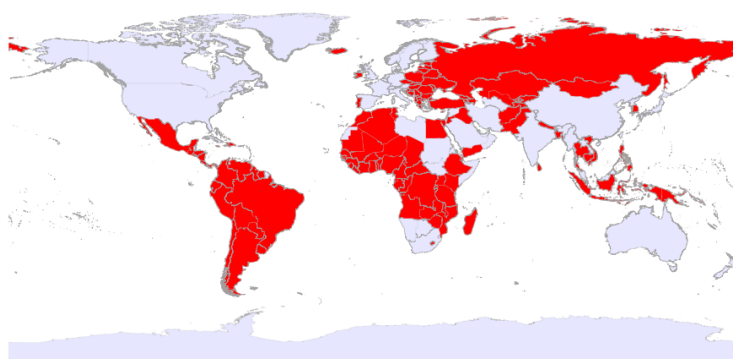
Descriptive statistics for the country-panel are provided in Table B.1. We can observe that IMF programs are relatively frequent, with 11% of country-pairs corresponding with a year

in which a funding agreement was signed. This proportion increases to 16% for countries that signed any agreement (at least 1) in the sample period. Party Change is frequent, with 54% of governments belonging to parties that were not incumbents before the last election. Figure B.1 presents two world maps used to illustrate the sample of countries included in the panel (panel A) and those which made at least one funding agreement with the IMF in the sample period (panel B).

FIGURE B.1
COUNTRY PANEL



(A) COUNTRIES IN SAMPLE



(B) COUNTRIES WITH IMF FUNDING PLANS (1992-2019)

Notes: Panel A represents the sample of countries in our country-panel. This includes countries that were classified as electorally competitive democracies at some point in the period 1992-2019 (see [Scartascini, Cruz, and Keefer 2021](#)). Panel B represents countries that signed a funding agreement with the IMF in that period.

Appendix B.2. Banking Crises

We analyze the relation between government turnover and IMF funding programs in a specific sample. We restrict our attention exclusively to countries that are under financial distress. Using the database in [Laeven and Valencia \(2020\)](#) on banking crises and IMF program interventions, we estimate:

$$Program_{ic}^{IMF} = \beta C_{ic} + \gamma_1 \Delta GDP_{ic} + \gamma_2 left_{ic} + \gamma_3 right_{ic} + u_{ic} \quad (\text{B.1})$$

where $Program_{ic}^{IMF}$ takes value 1 if country i received external financing from the IMF during crisis c , and value zero otherwise (notice that some countries suffer more than one banking crisis over the sample period).²⁶

Estimates of the key parameters of the specification in equation B.1, estimated using our sub-sample of banking crises, are reported in Table B.2. We find that, in this sub-sample, the probability to agree with the IMF after a change in office is roughly 27% higher. Anecdotally, this figure is remarkably close to the main effect we estimate with the Spanish municipal data (see Section 4).

TABLE B.2
THE IMPACT OF PARTY CHANGES ON IMF PROGRAM IMPLEMENTATIONS - BANKING CRISES

	(1)	(2)	(3)
	IMF Program	IMF Program	IMF Program
Party Change	0.285** (0.133)	0.271** (0.133)	0.249* (0.133)
GDP		-0.582 (0.415)	-0.521 (0.417)
Political Party: Right Orientation			-0.075 (0.173)
Political Party: Left Orientation			-0.134 (0.134)
Observations	62	60	60

Notes: This table reports OLS estimates. IMF Program is a dummy variable that takes a value of 1 if the country puts an IMF program in place due to the analyzed crisis, and 0 otherwise. Party Change takes a value of 1 if the country is ruled by a new party, and a value of 0 if the country is ruled by the previous incumbent party. GDP is the growth rate of national GDP in U.S. dollars. Political party dummies take a value of one according to the orientation of the chief executive's political party. The sample used is 1980-2015 and includes all banking crises from [Laeven and Valencia \(2020\)](#). Robust standard errors clustered at the country level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

²⁶Due to the limited size of this sample, these regressions do not include country or year dummies.

Appendix C. Examples of SPP news in Spanish newspapers

FIGURE C.1

NEWSPAPER EXTRACT - PP NEWCOMER BLAMES FORMER PSOE MAYOR - ORIGINAL SPANISH

DOW JONES

Colpisa

SE ECONOMÍA

HD PRESUPUESTOS; Un pueblo de Guadalajara tardará 7.000 años en pagar su deuda

PD 10 May 2012

SN Vocento-Colpisa News Feed

SC VOCCLP

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LP -La alcaldesa, del PP, cifra en 16 millones las facturas pendientes y culpa a su antecesor socialista.

Madrid, 10 may. (COLPISA, Redacción).

TD Siete mil años. Es el tiempo transcurrido hasta hoy desde la aparición del hombre de Cro-Magnon, inventor de la agricultura y la ganadería. Y es también el lapso que necesitará Pioz, un municipio de 3.300 habitantes de Guadalajara, para saldar su actual deuda. La alcaldesa, la 'popular' Amelia Rodríguez Sánchez, cifra en 16 millones de euros los pagos pendientes del consistorio con sus proveedores, que atribuye a la mala gestión del anterior equipo municipal.

El pueblo, situado a 55 kilómetros de Madrid, conocido por su castillo renacentista y sus fiestas de la virgen de la Candelaria, saltó a la fama este jueves después de que el secretario de Estado de Administraciones Públicas, Antonio Beteta, asegurara que un consistorio «pequeño» de Guadalajara necesitará 7.058 años para pagar sus deudas. En Pioz, que según el padrón del INE tenía 3341 habitantes el 1 de enero de 2011, enseguida se dieron por aludidos.

«Por desgracia es nuestro municipio», admitió la alcaldesa. Amelia Rodríguez, aupada a regidora en mayo de 2011, ha presentado al Ministerio de Hacienda facturas que suman 16 millones para acogerse al Plan de Pago a Proveedores. Unas cargas cuyo pago inmediato a escote obligaría a desembolsar 4.789 euros por cabeza a los hombres, mujeres y niños de Pioz. Ni eso ni aplazar la deuda setenta siglos parecen buenas soluciones.

La alcaldesa del pueblo alcarreño culpa del pésimo estado de las cuentas a los socialistas que gobernaron hasta su llegada, y acusa al anterior alcalde, Emilio Rincón, de arruinar el municipio para «toda la vida». El ex regidor admite el descuadre contable, pero rebaja a 8,5 millones la deuda que dejó en 'herencia' tras la última legislatura.

Depuradora y piscina

Entre las facturas enviadas a Hacienda hay una de 2,8 millones de euros por la construcción de una piscina. Y otros 5 millones corresponderían a la construcción de una depuradora de aguas y un punto limpio. Según la alcaldesa, el Ayuntamiento sólo ingresa lo que recauda en concepto de IBI y las aportaciones que recibe del Estado.

DOW JONES

Colpisa**SE** ECONOMY**HD BUDGETS: A town in Guadalajara will take 7,000 years to pay off its debt****PD** 10 May 2012**SN** Vocento-Colpisa News Feed**SC** VOCCLP**CY** © VOCENTO. All rights reserved**LP** The mayor, from the PP, estimates arrears to be 16 million euros and blames her socialist predecessor.

Madrid, 10 may. (COLPISA).

TD Seven thousand years. It is the time elapsed until today since the appearance of the Cro-Magnon man, inventor of agriculture and livestock. And it is also the period that Pioz, a municipality of 3,300 inhabitants in Guadalajara, will need to pay off its current debt. The mayor from PP, Amelia Rodríguez Sánchez, estimates arrears to be 16 million euros, and she attributes them to the mismanagement of the previous municipal team.

The town, located 55 kilometers away from Madrid, known for its Renaissance castle and its festivities of the Virgin of Candelaria, rose to fame this Thursday after the Secretary of State for Public Administrations, Antonio Beteta, ensured that a «small» consistory of Guadalajara would need 7,058 years to pay off its debts. In Pioz, which according to the INE register had 3,341 inhabitants as of January 1, 2011, they immediately felt alluded.

"Unfortunately, it is our municipality," admitted the mayor. Amelia Rodríguez, promoted to councilor in May 2011, has presented unpaid invoices amounting 16 million to the Ministry of Finance, so Pioz can benefit from the Supplier Payment Plan. An amount whose immediate payment would force each men, women, and children in Pioz to disburse 4,789 euros. Neither that nor deferring the debt for seventy centuries look like good solutions.

The mayor blames the Socialists, who had governed until her arrival, for the terrible financial state of the municipal accounts, and accuses the previous mayor, Emilio Rincón, of ruining the municipality forever. The former councilor admits the accounting imbalance, but he lowers the debt he left as an 'inheritance' to 8.5 million.

Treatment plant and pool

Among the invoices sent to the Treasury there is one of 2.8 million euros for the construction of a swimming pool, and another 5 million that would correspond to the construction of a water treatment plant and a clean point. According to the mayor, the City Council's only income comes from what it collects from the property tax and from the National State transfers.

DOW JONES

LAS PROVINCIAS

SE Castellón

HD El Ayuntamiento culpa al PP de la necesidad de un plan de ajuste

WC 408 words

PD 3 April 2012

SN Las Provincias

SC PROVIN

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LP Responsabiliza al anterior equipo de gobierno del PP de las dificultades para pagar a los proveedores

El portavoz del equipo de gobierno de Vila-real, Javier Serralvo, responsabilizó ayer al PP de la localidad, al Consell y a la Diputación Provincial del hecho de que el Ayuntamiento tenga la necesidad de elaborar el plan de ajuste exigido por el Gobierno central para poder pagar la deuda con proveedores acumulada por el anterior equipo de gobierno.

TD Serralvo detalló que los impagos de la Generalitat con el municipio; la decisión de la Diputación de eliminar a la ciudad de los Planes Provinciales de Obras y Servicios (PPOYS), que suponían una inversión anual de cerca de 300.000 euros; y la «herencia» dejada por el PP en el Consistorio han forzado al equipo de gobierno a tomar esta decisión, «que supone una previsión de ingresos de 280.000 euros al año en 10 años para poder hacer frente al crédito para pagar a proveedores».

Para el portavoz, esta cantidad sería mucho mayor «si el PP hubiera seguido en el gobierno municipal», y aludió a la deuda acumulada por ayuntamientos vecinos como Castellón o la Vall d'Uixó, que es «mucho más elevada». «Aquí el PP dejó 6,5 millones de euros por pagar a proveedores, pero el equipo de gobierno actual decidió poner solución a esta situación desde el primer día y llegamos al plan de pagos del Gobierno con 2,2 millones de euros de deuda gracias a la buena gestión realizada hasta ahora», añadió.

Serralvo también se refirió a las últimas declaraciones procedentes del PP local sobre la previsión de remanentes de 2011, respecto a lo que señaló que el PP «es el menos indicado para hablar de buena gestión a Vila-real». «Si existe previsión de remanentes es porque en la segunda mitad de año los vila-realenses contaron con un equipo de gobierno responsable que tuvo que afrontar la situación que nos dejaron en herencia», manifestó.

Para finalizar, el portavoz del equipo de gobierno lamentó también las declaraciones del portavoz de la oposición, Héctor Folgado, en relación a la huelga general. «Han pretendido dar la imagen de que en Vila-real se vivió una huelga salvaje, cuando la realidad es que se vivió un jornada de normalidad, y en los únicos casos en que se produjeron situaciones que condenamos, la policía actuó», afirmó.

FIGURE C.4
NEWSPAPER EXTRACT - PSOE NEWCOMER BLAMES FORMER PP MAYOR - ENGLISH TRANSLATION

DOW JONES

LAS PROVINCIAS

SE Castellón

HD The local government blames PP for the need of an adjustment plan.

WC 408 words

PD 3 April 2012

SN Las Provincias

SC PROVIN

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LP They blame the previous PP government for the difficulties paying suppliers.

The city spokesperson of the Vila-real government, Javier Serralvo, blamed yesterday the local branch of PP in the municipality and the Government of the Province for the need to elaborate the adjustment plan required by the central government to pay the arrears built during the previous administration.

TD Serralvo explained that missing transfers from the Generalitat; the Province Government decision to remove the city from the Provincial Works and Services Plans (PWSP), which involved an annual investment of around 300,000 euros; and the "inheritance" left by PP in the council, have forced the local government to make this decision "which forces the municipality to raise 280,000 euros of additional yearly income in the next 10 years to pay back the credit to pay arrears."

The city spokesperson stated that the debt would be much higher "if PP would have continued in the local government", alluding the debt accumulated by neighboring municipalities such as Castellón or Vall d'Uixó, which is "much higher". "Here, the PP government left arrears amounting 6.5 million euros, but the current local government decided to face this situation from the very first day, and thanks to our good management, we reached the government payment plan with arrears amounting 2.2 million euros", he said.

Serralvo also referred to the latest statement made by the local branch of PP about the treasury balance for 2011. He said that PP "are the least appropriate to speak about good management to Vila-Real residents". "There is a positive treasury balance because, in the second half of the year, the Vila-Real residents had a responsible government, which had to face the situation we inherited from PP", he added.

Finally, the city spokesperson also deprecated the statements made by the opposition speaker, Hector Folgado, about the general strike. "They tried to give the impression that there was a wild strike in Vila-Real. However, the day went by normally, and in the few cases in which there were situations that we disapprove, the police intervened" he said.