

Blaming Your Predecessor: 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 analyze the decisions made by 3,000 Spanish municipalities following a credit shock during the Great Recession. Regression-discontinuity estimates show that newly elected executives are significantly more likely than re-elected incumbents to publicly agree on a financing program with the national government. Analyses of press coverage, news content using ChatGPT and politicians' survey responses indicate that re-elected incumbents avoid requesting a public bailout to protect their image, despite it being financially suboptimal. This shows how electoral incentives can prevent optimal policy adoption. We also provide cross-country descriptive evidence that a change in office is associated with a larger probability of receiving financial assistance from the IMF, which suggests that the effect that we find in Spain can be found in other contexts.

Keywords: Government Turnover, Bailout, Fiscal Consolidation, Electoral Incentives, IMF.
JEL classification: H63, F34, P43

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1. Introduction

In times of financial distress, governments often turn to international financing institutions (IFIs) to address spending needs and fulfil financial obligations. 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. The decision to seek assistance is influenced by the political incentives and constraints faced by the distressed government, which must navigate negotiations and agree on a specific program with the funding institution. Notably, although successful agreements can bolster fiscal capacity, they are usually subject to public scrutiny and can shape public perceptions of the government's financial health.

We hypothesize that, in a context of asymmetric information, this publicity means government officials have different incentives to request assistance depending on their tenure in office. While incumbent governments are accountable for previous fiscal and financial decisions, newly elected administrations can more readily attribute the need for assistance to their predecessors. Consequently, when the true state of the public finances is imperfectly observed by voters, re-elected incumbents may prefer to endure stricter borrowing constraints to safeguard their reputation. 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 employ two empirical strategies to investigate how these different incentives influence the likelihood of reaching a bailout agreement.

Our main analysis leverages on the decisions to request financial assistance in the context of a Spanish program designed to deal with the mounting arrears of over 3,000 municipalities. The Supplier Payment Program (SPP) was introduced in 2012 and deployed financial resources of roughly 3% of Spanish GDP. It automatically converted all municipal commercial debt in arrears into financial debt with the national government. In practice, the program worked as a credit shock for local governments, forcing them to pay back their existing arrears and preventing them from using arrears again to finance their deficit.

Crucially for our purposes, the SPP effectively gave local governments two options for repayment that had different financing conditions as well as different publicity levels. The first option was to present an adjustment plan to the national government. This option was explicitly preferred by the national government and would grant local governments access to a smoother backloaded adjustment—up to ten years to repay the loan and interest. However, it would also make the adjustment salient, since it required submitting a (public) fiscal consolidation program. Alternatively, municipalities could choose not to present a plan and

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. Notably, because the interest rate on the SPP associated debt was heavily subsidized, the backloaded adjustment was the best option for municipalities from a financial perspective.

To explore whether re-elected incumbents are more reluctant to request the public bailout than newly elected officials, we use 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 \(2022\)](#), [Toral \(2023\)](#) and [Marx, Pons and Rollet \(2025\)](#). 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 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.

Notably, 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 suffer a simultaneous credit shock. Second, the SPP setting allows us to observe whether each municipality requests assistance or not—something that is typically not possible with cross-country data. We can therefore avoid confounding the effects of turnover on supply and demand of external funding. Third, the Spanish SPP—unlike e.g., most IMF programs—does not involve any conditionality on policies beyond the establishment of a credible path to reduce debt and fiscal deficit. Local governments can choose to reduce expenditures, increase taxes, or a mix of both, which removes ideological considerations from the decision to adhere to the program. Finally, as the national government had no practical capacity to enforce its preferred option (the adjustment plan), the SPP incentivized local governments to present a plan by offering a subsidized interest rate and a longer repayment period to those who present it. This makes presenting the adjustment program the superior alternative for the municipality: using the full ten years instead of five to pay back SPP debt yielded municipalities a relative financial gain in net present value amounting to 40% of the 2011 municipal deficit. This enables us to identify the problem of misaligned incentives from the choice of not presenting any program.

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.

Using data from Factiva covering the universe of Spanish national, regional and local newspapers, we check whether requesting assistance has any impact on the voters' information set. We show that, when municipalities agree on an adjustment plan with the national government there is a substantial increase in press coverage of the program. We also use ChatGPT to analyze the content of the news covering the SPP and find that new governments blame the previous administration for the financial situation they inherit. Moreover, when a continuing incumbent presents an adjustment plan, this is used instrumentally by the opposition to intensify the criticisms, something that does not happen when a new government presents a plan. This suggests that, while most newcomers present an adjustment plan to benefit from its financial advantages, many re-elected incumbents avoid presenting one to withhold information about their past performance.

To gather further evidence on this mechanism, we perform three additional analyses. First, we use data on a survey of Spanish mayors to understand what these politicians think about correcting their past mistakes. We find many mayors consider that rectifying policies can be politically costly since it makes errors salient. Possibly related to this, we find that most mayors consider that newcomers are in a better position than ongoing incumbents to face situations of financial distress. Second, we explore whether submitting an adjustment plan affects the probability of re-election. We find a negative association between presenting an adjustment plan and getting re-elected four years after the SPP, but only for continuing incumbents. Finally, we conduct an heterogeneity analysis of the effect of a change in office on the probability of presenting a plan. We find this effect is larger in municipalities with more arrears, where the (negative) signal of past performance is stronger. Altogether, these results reinforce the notion that re-elected governments do not present the adjustment plan to protect their information rents, despite this being financially suboptimal.

One limitation of our regression-discontinuity approach is that it does not guarantee balance in elected government characteristics at the threshold. This is an issue with politician characteristic regression discontinuity (PCRD) designs, as described in [Marshall \(2022\)](#). We show that, although two observable characteristics of elected governments are indeed discontinuous at the threshold—political affiliation and age—these characteristics have no direct impact on the decision to request assistance, and keeping them fixed does not affect our main estimates. [Marshall \(2022\)](#) also highlights that, when studying the effect of characteristics which themselves affect electoral outcomes, unobservable attributes will be mechanically

unbalanced at the threshold (i.e., they operate as compensating differentials for a fixed vote share). Applied to our case, this means that, if there is an incumbency advantage, it is likely that the quality of the incumbent is lower than the quality of the challenger in close races. To address this issue, we show that there was no detectable incumbency advantage in Spanish local elections in 2011, mitigating the concern that our research design induces a mechanical imbalance of unobservable characteristics at the threshold.

We close our paper by using data on all IMF interventions between 1992 and 2021 (over 2,300 interventions in 124 nations) to conduct a cross-country descriptive analysis of the relationship between government turnover and external financial assistance at the national level. 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. While the the cross-country analysis does not allow us to reach the standards of identification that are customary in contemporary political economics, it shows that the findings in our main analysis may apply more generally outside the narrow context provided by the Spanish SPP program.

This paper contributes to the literature on the political economics of macroeconomic policy.¹ 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 literature see for example [Alesina 2000](#) or [Mahmalat and Curran 2017](#)). 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 RDD 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 et al. 2022](#)).²

The higher predisposition of newcomers to present an adjustment plan is aligned with the

¹See [Persson and Tabellini \(1999\)](#); [Alesina and Passalacqua \(2016\)](#) and [Yared \(2019\)](#).

²This paper also contributes 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. Relative to these papers, our work studies how electoral incentives can shape these decisions.

finding that politicians try to shift the blame to other actors in tough times (Weaver, 1986; Hinterleitner, 2017; Bursztyn et al., 2022), and it connects the paper with work exploring the empirical foundations of salience theory (Robertson, 1976; Petrocik, 1996; Dolezal et al., 2013). Recent papers have shown that incumbents and challengers choose topics strategically to build a narrative that focus on their strengths and attacks their opponent weaknesses (Green-Pedersen and Mortensen, 2010; Greene, 2018). Here we show that not only do politicians emphasize certain topics to enhance their electoral prospects, they also anticipate the response from their political opponents and favor policies that limit the rhetorical resources of their competitors.

Our findings indicate that politicians are significantly less prone to choose the efficient option—i.e., presenting an adjustment plan—when the alternative decision protects their reputation. This relates our work to the classical debate between the Chicago and Virginia schools of political economy (see Becker 1976, Becker 1985, Wittman 1989, Crew and Twight 1990, Coate and Morris 1995, Tullock 1989). In line with the latter, we find that incumbents discard the superior alternative for the municipality when they consider that doing so yields them private rents. Notably, our mechanism suggests that it is electoral competition that promotes such conduct, introducing a new perspective on the effect of re-election incentives on politicians' misbehavior.

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 (Fourinaies and Hall, 2022), and enhance policy implementation (de Janvry, Finan and Sadoulet, 2012). Interestingly, 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.³ The evidence presented in our paper suggests that newcomers are more eager to challenge the status quo, while re-elected incumbents may stick to failed policies, fearing the consequences of acknowledging past mistakes.

³Part 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.

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

Our main empirical analysis focuses on the unique setting provided by the Spanish Suppliers Payment Program. The SPP 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.

2.1. Institutional Setting

2.1.1. 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.⁴ Regular municipal financing is based on transfers from the national and regional governments, which amount to approximately half of their income, and local taxes. The most important local tax is a property tax. Moreover, 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 most of our analysis we focus on municipalities with more than 250 inhabitants, which use a single-district, closed-list, proportional electoral system.⁵ 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 council chooses the mayor among the list leaders under a majority rule. If no candidate obtains the majority of the council votes, the candidate from the most voted party is appointed as mayor. There are no term limits, and local governments cannot call for early elections, which occur simultaneously for all Spanish municipalities every four years.⁶

⁴For further details, see [Ley \(7/1985\)](#).

⁵Municipalities with populations under 250 inhabitants have an open-list system in which voters may express multiple preferences for different candidates.

⁶For further details, see [Ley Orgánica \(5/1985\)](#).

2.1.2. Spain's Suppliers 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 after a nationwide housing bust—Spanish municipalities began 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 build-up of arrears, and it represents a relevant figure in comparison to Spain's total public debt at the beginning of the period (35% of 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 Duarte Lledo 2017](#)), the Spanish national government decided to adopt various measures in early 2012. These included the Supplier Payment Program (*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 Orgánica, 2/2012](#)) 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 Credito 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.

The law establishing the SPP ([RDL 4/2012](#) of February 24th 2012) offered two different repayment options to indebted municipalities. In an effort to minimize moral hazard, the national government wanted local governments to submit a fiscal adjustment plan. Thus, the SPP gave those municipalities presenting a plan up to 10 years to pay back their debt, with a 2-year interest-only grace period and a subsidized interest rate.⁷ Alternatively, the municipalities that did not present a plan were forced to pay back within five years, via retention of fiscal transfers from the national government. Importantly, the fiscal adjustment plan had to be discussed and approved by the municipal council (the local equivalent of the parliament). This gave the local opposition the chance to make salient the financial situation of the municipality and the associated spending cuts and tax changes proposed by those local governments in the context of the impending adjustment. Thus, local governments should

⁷See [Heppke-Falk and Wolff \(2008\)](#) for a discussion on how national government bailouts might induce moral hazard among local debt investors if they anticipate other bailouts in the future.

choose between a smoother repayment scheme which required a public adjustment program, or a more discreet front-loaded adjustment.

The debt created by the conversion of arrears into debt owed to the national government had an interest rate 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. The elaboration of the adjustment plans was fairly simple, and most of them were prepared within the first two weeks of March 2012. Municipalities could rely on a public servant paid by the national government (the *interventor municipal*) to do the job. For the Ministry to approve the adjustment plan, which granted access to the backloaded adjustment, this plan should ensure that the municipality would have enough revenue to cover its expenses and pay back the amount owed for the arrears within a period of ten years. The SPP imposed no conditionality on policies, and local governments had discretion on how to carry out the fiscal consolidation: They could rise taxes, cut spending, or a combination of both.⁸

It is important to stress that municipalities could not avoid the fiscal consolidation in any case. If they presented a plan, they could choose how quickly they wanted to pay, with a ten-year maximum. If they did not present a plan, they were forced to pay back within five years. Crucially, the fact that the interest rate charged by the national government was substantially subsidized (BBVA Research, 2012), made presenting a plan the financially superior alternative. A very conservative estimate for the financial gain of taking ten years instead of five to pay back roughly amounts to 40% of the average deficit of local governments in 2011 (see Online Appendix B for details). This, along with the fact that the SPP was agnostic about how to carry out the adjustment, made the decision of presenting a plan the objectively optimal decision for the municipality, irrespectively of politicians' ideology or tenure in office.

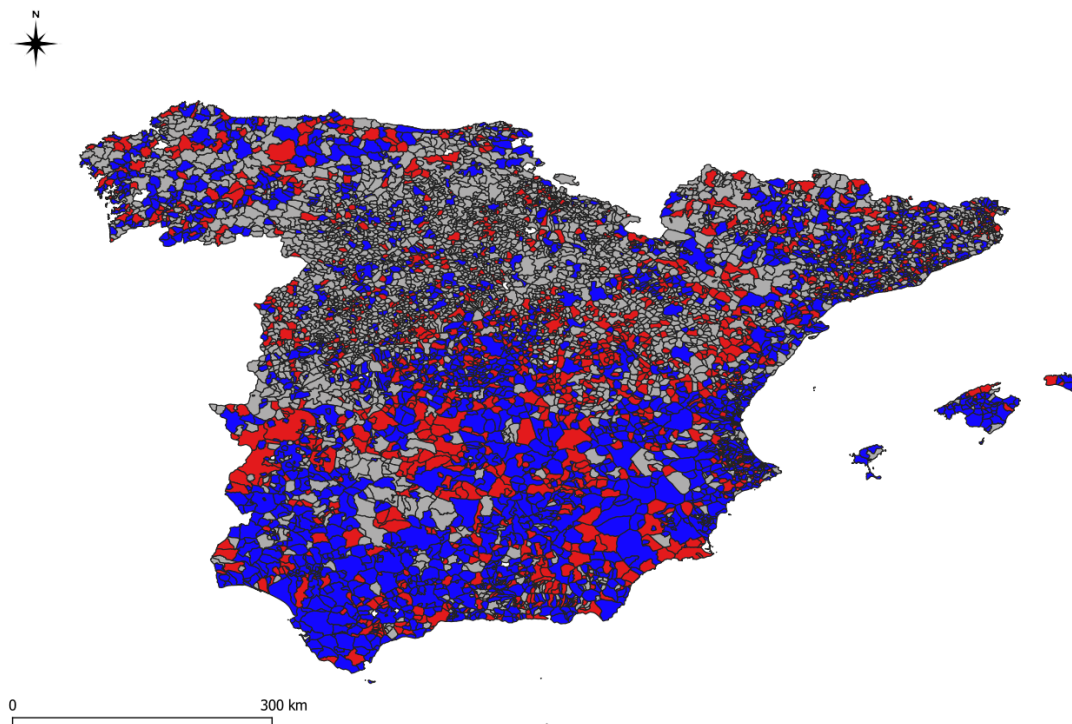
The map in Figure 1 shows the spatial distribution of municipalities with no arrears, municipalities with arrears that presented an adjustment plan, and municipalities with arrears that did not present an adjustment plan.⁹ As a result of the fiscal cuts that every municipality with arrears had to make, 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

⁸Most plans were accepted, only less than 7% of the plans were rejected mainly due to formal defects or lack of financial viability (i.e., municipalities would incur negative net savings or they projected insufficient current revenues to finance both current expenses and the payment of arrears).

⁹Spain is divided in 17 Autonomous Communities, a sub-national level of government. Municipalities belonging to the Basque Country and Comunidad Foral de Navarra are excluded from the SPP, as they are the only two Autonomous Communities that have their own treasury, independent from the Spanish National Treasury.

the largest yearly fiscal adjustment ever recorded at the municipal level 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.

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. Our dataset also includes information on municipalities in the Canary Islands (not shown).

2.2. Analytical Framework

In our empirical analysis, we study the decision faced by governments in arrears about whether or not to present an adjustment plan. To think about this decision theoretically, we build on the argument developed in [Ashworth, Bueno de Mesquita and Friedenber](#) (2018) to illustrate the agency problem between voters and politicians. In their framework, exogenous shocks reveal previously unobservable information about government quality. Their argument goes as follows. Even when a shock itself is outside the incumbent's control, its effects depend on the incumbent's preparedness to deal with it. This preparedness, which is not observable in the absence of the shock, depends on incumbent quality. Hence, external shocks reveal new information to rational voters, who update their beliefs about government quality as a consequence of the shock.

In our particular setting, an external shock—the Global Financial Crisis (GFC) that erupted with the collapse of Lehman Brothers—hit municipalities in the midst of the 2007–2011 electoral term. The severity of its consequences partially hinge on incumbents’ preparedness—such as reliance on extraordinary revenues from land sales to sustain current spending or the lack of countercyclical fiscal policies during the preceding boom—as well as on incumbent’s management of the crisis until the end of the term. At the beginning of the new term in 2012, the incoming government can take an action that makes the adverse consequences of the shock on the municipality more (or less) salient to voters: presenting (or not) an adjustment program to request financial assistance.

The strategic implications of this action hinge on two informational assumptions. First, voters cannot perfectly infer the initial state of local public finances solely from perceived local economic conditions and the municipality’s decision to request assistance or not. This is plausible in our context, as overlapping macroeconomic shocks in Southern Europe in 2012 make it difficult to disentangle local fiscal imbalances from broader economic distress. Second, requesting external assistance is a rare and noticeable event that turns a latent fiscal situation into focal public news coverage, reorganizing political debate around the government’s fiscal situation and its roots. By contrast, choosing not to request assistance typically produces only a gradual and fragmented flow of negative economic news—which blurs the signals and hampers their interpretation. These features generate asymmetric incentives for reelected incumbents, whose poor past performance becomes salient if they formally request financial assistance, and for newly elected governments, who can more easily attribute responsibility to their predecessors.

2.3. Data

We build a municipal panel with yearly information for the period 2008-2016, combining data from several sources. Most of the analysis below will focus on a cross-section from this panel for year 2012.

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. We obtain data on arrears and data on the outstanding debt by municipality since 2009 from the same source.

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*.

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.

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.

Municipal descriptives are presented in Table 1 and Online Appendix Table A.1. In Table 1, 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 slightly lower for municipalities that do not present an adjustment plan than for municipalities that do present a plan.

Table A.1 in the Online 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. 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.

TABLE 1
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: SPP 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: SPP Municipalities Adj. 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: The table reports means and standard deviations for each variable by municipality in 2011. 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. Number of observations in each group indicated at the bottom of the panel. Variables Party PP and Party PSOE are dummies taking value 1 if the municipality is ruled by the corresponding party. All financial variables expressed in euros per capita.

2.4. 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:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k=2008}^{2016} \omega_k front_i \times \mathbb{1}\{t = k\} + \sum_{k=2008}^{2016} \gamma_k \log(arrears_i) \times \mathbb{1}\{t = k\} + \epsilon_{it} \quad (1)$$

where i indexes municipalities and t indexes years, α_i is a municipality fixed effect, δ_t is a set of time effects, $front_i$ is a dummy taking a value of 1 if municipality i chose the front-loaded option to pay for its SPP obligations, and $\log(arrears_i)$ is the logarithm of total commercial debt in arrears for municipality i at the end of 2011. We consider three different outcomes Y_{it} : 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 measured in percentage points of assessed (cadastral) value.

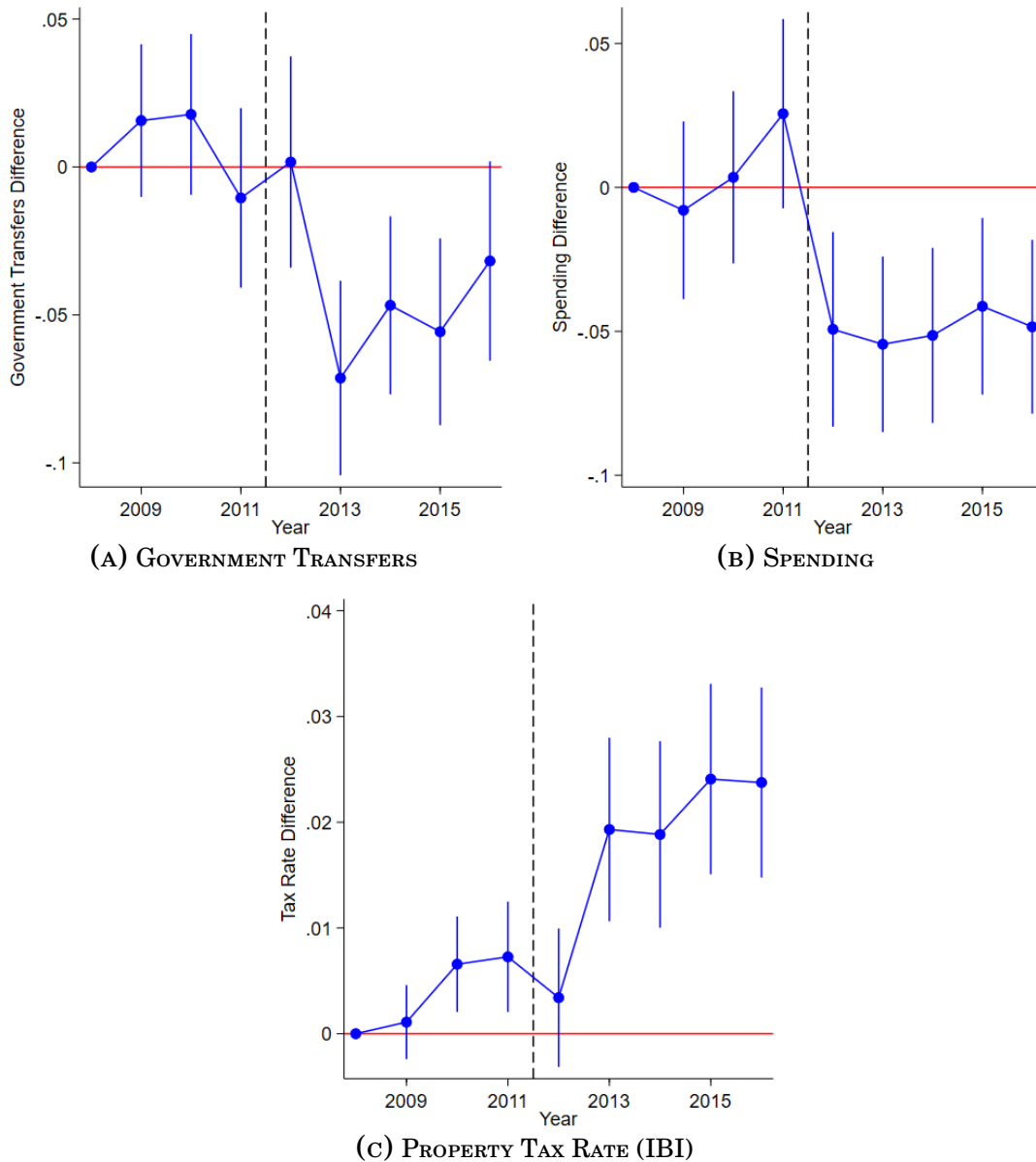
This analysis is restricted to municipalities with unpaid commercial debt, i.e., those that were forced to assume a central government loan by virtue of the SPP. As a result, the coefficients $\{\omega_k\}_{k=2008}^{2016}$ can be interpreted as the differences between municipalities with $front_i = 1$ and those with $front_i = 0$ over time. That is, they indicate the evolution of the difference in transfers, spending and taxes between municipalities choosing the more discreet front-loaded adjustment and municipalities that opted for presenting an adjustment plan. The second sum in equation 1 is included to account for potential temporal differences in shocks across different levels of arrears per capita. These differences can exist if, for example, the local economic shocks during the crisis were different in municipalities with different levels of arrears.¹⁰

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

¹⁰The dynamic patterns are quite similar if we exclude the interaction terms in the second sum of equation 1. The only noticeable difference is an earlier convergence of spending levels between both groups of municipalities. See Appendix Figure A.1.

FIGURE 2
CONSEQUENCES OF GOVERNMENT RETENTION SCHEME



Notes: These figures show point estimates and 95% confidence intervals for the sequence of coefficients ω defined in equation 1. The dependent variable used to estimate these coefficients is the log of the transfers received from the central government in panel A, the log of total municipal spending in panel B, and the property tax rate (IBI) in panel C. Estimation carried out using data for the period 2008-2015. All regressions include municipality fixed effects and year effects. Standard errors are clustered at the municipality level. Dashed line marks the transition between 2011 and 2012.

their spending levels ahead of the change in transfers. This relative reduction in spending persists to the end of our sample period in 2016. Was all of the front-loaded adjustment 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. The increase of roughly 0.02 percentage points

corresponds to 13% of a cross-sectional standard deviation in property tax rates across municipalities in 2012. Following [Blanchard and Leigh \(2013b\)](#), one can argue that, in the context of a recession, the procyclical policies triggered by the front-loaded adjustment would be suboptimal for the municipality. Likewise, the spike in the property tax rate would not conform to arguments in support of smoothing taxation ([Barro, 1979](#); [Bohn, 1990](#)).¹¹

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 1](#) 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 adjustments have on municipal finances are indeed observed in practice. That is, we see these patterns as mostly descriptive but nonetheless reassuring.

2.5. News Coverage of SPP

There was substantial coverage of the SPP in Spanish national and local news, which evidences the publicity usually associated with these type of arrangements. We use the Factiva database to analyze the coverage of the SPP (see dataset description in [Online Appendix C](#)). [Panel A of Figure 3](#), reports the monthly number of news released in all Spanish newspapers that made an explicit mention to the Suppliers Payment Program between 2011 and 2013. As expected, before the SPP 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.

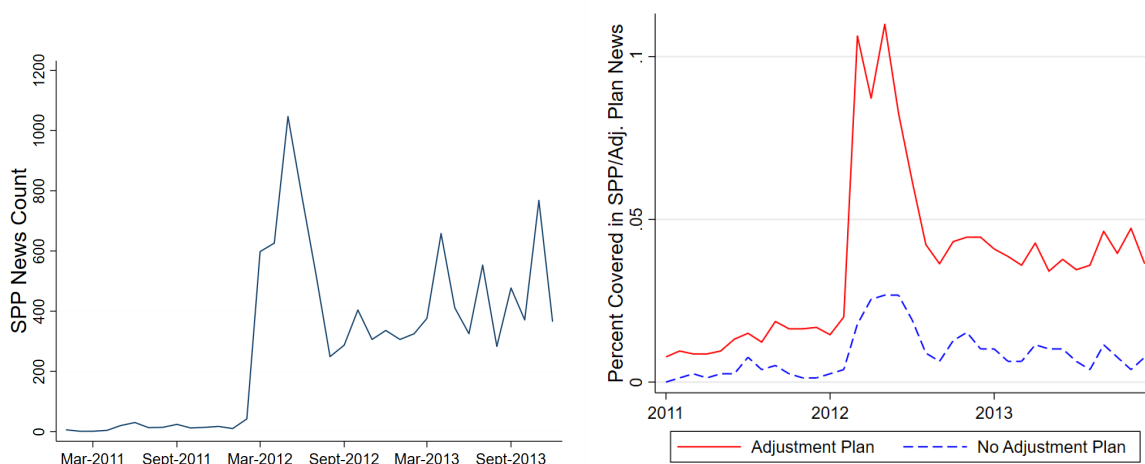
News coverage of the SPP was higher in municipalities which decided to submit an adjustment plan to the national government in exchange for a smoother repayment path. We document this by using data on the number of SPP-related news by municipality in our Factiva sample.¹² [Panel B of Figure 3](#) shows the fraction of municipalities with arrears that were covered in the SPP-related news for each month between 2011 and 2013, disaggregated by whether the municipality submitted an adjustment plan to the national government or not. We observe news coverage is almost zero throughout 2011 and increases after the introduc-

¹¹See [Alesina, Favero and Giavazzi \(2019\)](#) for a review on macroeconomic outcomes of deficit reduction policies.

¹²See [Online Appendix C.2](#) discussing the selection criteria to classify news as SPP-related.

tion of SPP in March 2012. Notably, news coverage is consistently higher in 2012 and 2013 for municipalities submitting an adjustment plan to repay their arrears. This is evidence of the salience of adjustment plans. All municipalities in this sample participated in the SPP, but news coverage was disproportionately higher in those that requested financial assistance to the national government. This is critical to our mechanism as it shows the publicity of adjustment plans in practice. We further analyze the link between government choices and news coverage in Section 4.

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 month calculated for SPP municipalities choosing whether to submit an adjustment plan to repay the SPP loan or not. Source: Factiva.

3. Government Turnover and External Assistance: Evidence from SPP

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

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—but heterogeneous—credit shock, enables us to obtain quasi-experimental estimates of this causal relation. Moreover, the design of the 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.

First, the timing of the elections, just a few months before the SPP was put in place, and the timing when local politicians had to decide whether or not to present an adjustment plan were exogenous to local politicians. This is important, as it prevents the two potential sources of endogeneity highlighted by [Alesina, Ardagna and Trebbi \(2006\)](#) and [Alesina et al. \(2023\)](#): politicians choosing strategically when to call for elections ([Hübscher and Sattler, 2017](#)) and when to conduct the adjustment ([Müller, 2023](#)) to maximize their electoral prospects. Second, we observe the local government’s decision to *present* an adjustment plan. This is an advantage relative to the cross-country analyses relying on the IMF data that will be presented in Section 6, which only includes information on whether the financial assistance is effectively granted. Third, the fiscal adjustment was unavoidable for every municipality with arrears and the SPP imposed no conditionality on policies. This removes ideological considerations from the local government’s decision to present an adjustment plan. Finally, presenting an adjustment plan was financially the superior alternative for the municipality, but its publicity could be against the electoral incentives of continuing incumbents.

Arguably, most of these conditions are not met in other scenarios that involve politicians seeking a financial bailout. This provides a unique setting to analyze the conflict of interest that politicians might face in these contexts.

3.1. Empirical Strategy

We use a close-election regression discontinuity design to induce exogenous variation on whether there was a change in office in 2011. To do so, we create a running variable for municipality i , defined as $\Delta V_i \equiv V_i^C - V_i^I$, where V_i^I is the 2011 vote share of the incumbent party at the end of the 2007 term—just before the 2011 election—and V_i^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*.¹³ Note that ΔV_i will take positive values if the challenger wins the 2011 local election and negative values if it loses.

We use this running variable to estimate the effect of a dummy C_i , taking value one if the municipality elects a mayor from a new party, on a dummy outcome $Ad.Plan_i$ which takes a value of one if the municipality submits an adjustment plan to the national government. Spanish 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_i crosses the threshold at zero—our RDD is fuzzy ([Imbens and Lemieux, 2008](#)). We esti-

¹³Note that the word incumbent here refers to the 2007-2011 incumbent and not the 2011-2015 incumbent.

mate our parameter of interest by two-stage least squares (2SLS). The estimating equations are:

$$C_i = \alpha_0 + \tau D_i + \pi_1 \Delta V_i + \pi_2 D_i \Delta V_i + v_i \quad (2)$$

$$Ad.Plan_i = \alpha_1 + \beta C_i + \rho_1 \Delta V_i + \rho_2 D_i \Delta V_i + u_i \quad (3)$$

Our parameter of interest is β , which can be interpreted as the local effect of having a new party in power on the probability of choosing an adjustment plan.¹⁴ The effect is local in the sense that it is obtained only for municipalities which had close elections in 2011—i.e., municipalities with competitive local elections—and for compliers—i.e., municipalities who appoint a new party when that new mayor wins the election. Equations 2 and 3 correspond to our first and second stages, respectively. Variable D_i is defined as $D_i = \mathbb{1}\{\Delta V_i > 0\}$ and is our instrument for C_i . 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 β using local linear regressions with a triangular kernel. The state-of-the-art in the estimation of these parameters uses the routine proposed in [Calonico et al. \(2017\)](#), which incorporates data-driven procedures to select the bandwidth, adjusted standard-errors to account for the bandwidth selector, and a bias correction procedure developed by the authors.¹⁵ Specifically, bandwidth choice is carried out using the procedure in [Calonico, Cattaneo and Titiunik \(2014\)](#). We will refer to the optimal bandwidth selected by this algorithm as the CCT bandwidth. In Section 3.3, we discuss the robustness of our main results to bandwidth choice, the choice of the kernel, the polynomial length used to fit the running variable and alternative methods to compute the running variable.

Before we move to report our estimates for β , we discuss the plausibility 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 A.2 in the Online Appendix. The formal statistical tests described in [McCrary \(2008\)](#) and [Cattaneo, Jansson and Ma \(2019\)](#) yield

¹⁴Note 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.

¹⁵Further details can be found in [Calonico et al. \(2017\)](#). In our case, implementation is carried out using the most recent version of the Stata `rdrobust` command.

large p-values of 65% and 76%, respectively, confirming that perfect manipulation of the running variable is very unlikely in this context.

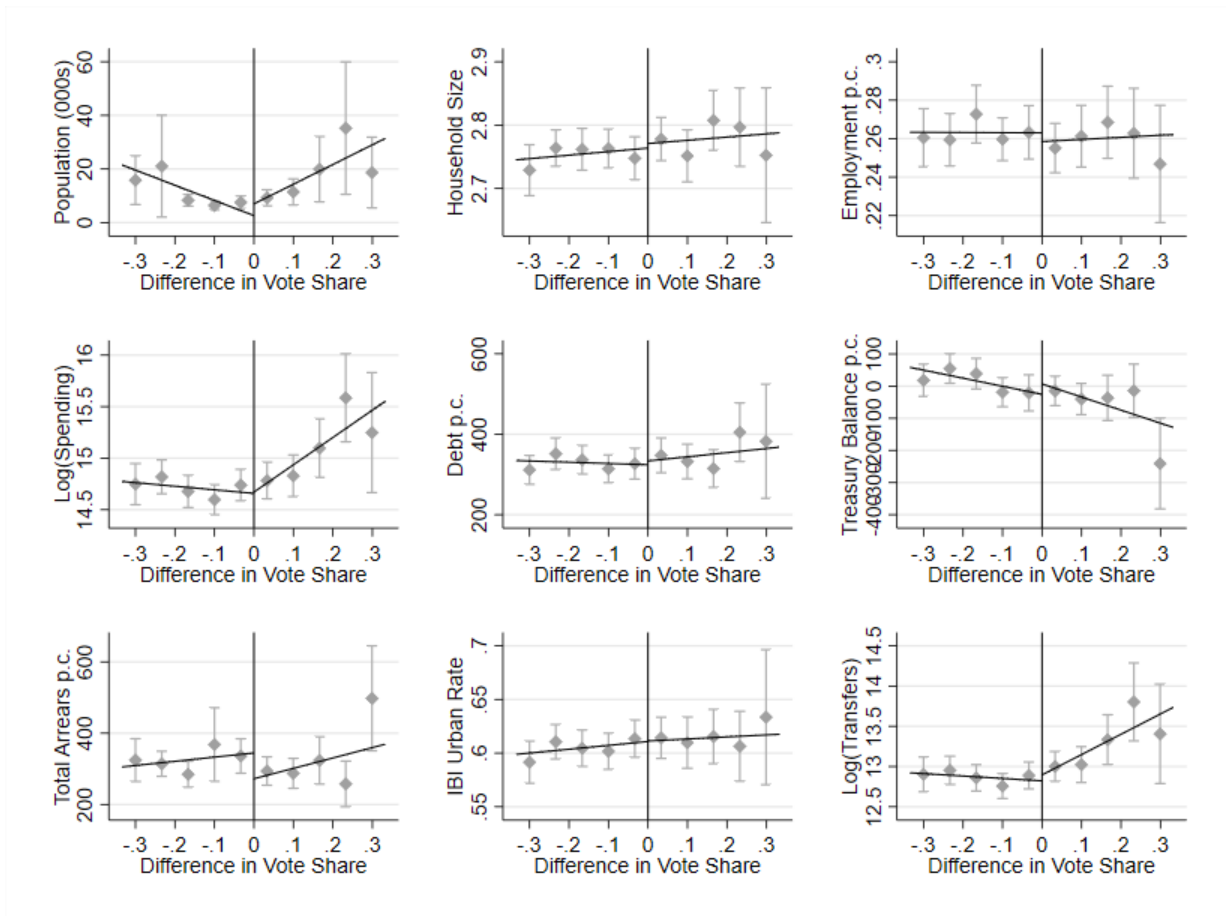
Our empirical strategy ensures that, on average, pre-determined characteristics of the municipalities and local governments appointed in the 2007 election are balanced at the threshold. Figure 4 shows observable demographic and financial characteristics of municipalities vary smoothly at the threshold, including variables that measure the level of arrears and the debt accumulated by municipalities by 2011. Figure A.3 shows that characteristics of the incumbent government in power *before* the 2011 election also vary smoothly at the threshold. Importantly, this includes a dummy which indicates whether the term 2007-2011 was the first one for the incumbent party, or whether they were repeating the mandate. Online Appendix Tables A.2 and A.3, 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.

One limitation of our empirical strategy is that it does not ensure balancing of characteristics of the government appointed in the 2011 election. There are two reasons for this. In the first place, newly elected governments and ongoing incumbents may be different because prior incumbency may correlate with government attributes such as experience, mayoral age, mayoral party, etc. In the second place, as pointed out in Marshall (2022), the focus on close-elections specifically will induce a correlation between prior incumbency status and other determinants of electoral performance if prior incumbency itself affects electoral results. We turn to these issues in Section 5, where, among other tests, we show there is no evidence of incumbency advantage in the 2011 Spanish local elections.

3.2. Baseline Results

We illustrate our first-stage in the left panel of Figure 5. The horizontal axis represents our running variable and the vertical axis the probability of having a new party in power at the local level after the 2011 election. First 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, showing the design is fuzzy and not sharp. Two different features of Spanish local politics determine the presence of a first-stage here. In the first place, as argued in Fujiwara and Sanz (2020), there is evidence of norms leading the most voted party to form a government when the first and

FIGURE 4
COVARIATE BALANCING – MUNICIPAL CHARACTERISTICS



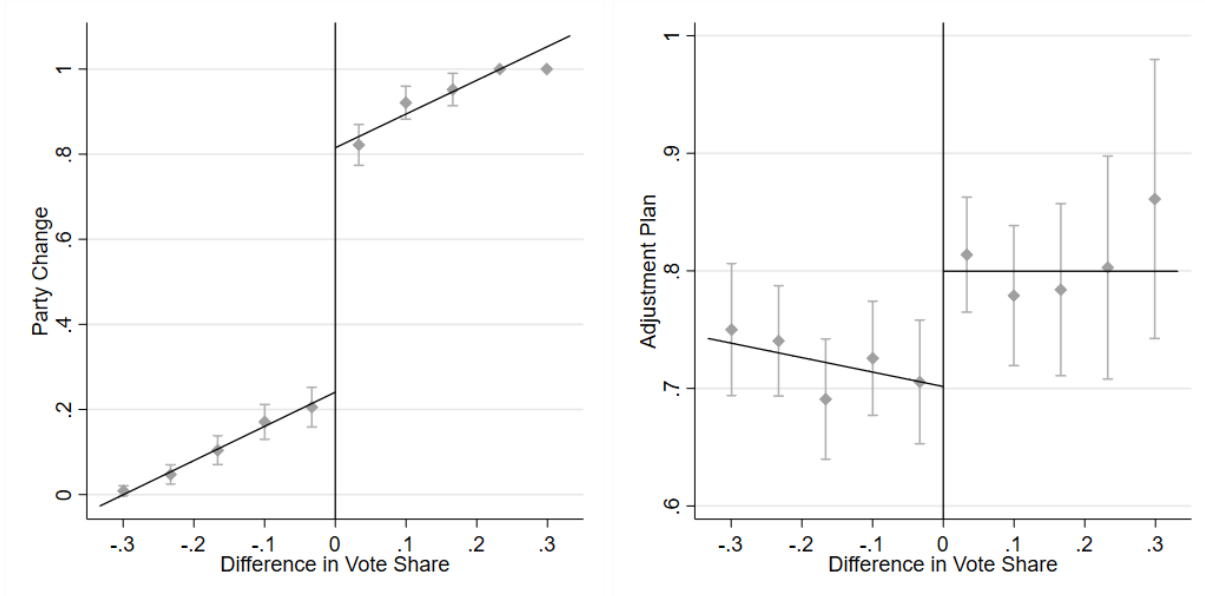
Notes: The horizontal axis represents the vote share difference between the challenger and the incumbent. From left to right and top to bottom the vertical axes represent population (in thousands), average household size, fraction of employed population, logarithm of municipal spending per capita, municipal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

second most voted parties are tied in the number of seats won in the election. Secondly, the Spanish electoral rules mean the most voted party can form a government if other parties in the council are unable to coalesce around an alternative candidate.

The right panel of Figure 5 illustrates the reduced form effect of crossing the threshold on the probability of presenting 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 plan of roughly 0.1.

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

FIGURE 5
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 left panel illustrates the first stage; hence, the vertical axis measures the probability that the challenger is appointed as mayor. The right panel plots the reduced-form relationship between running variable and outcome. Solid lines represent first-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.

Table 2.¹⁶ The estimate in column 1 corresponds to our baseline specification. The 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. In column 2, we report the estimate obtained when adding the pre-determined municipal characteristics featured in Figure 4 to our specification.¹⁷ Column 3 includes the characteristics of the incumbent government in power before the 2011 election, displayed in Figure A.3 of the Online Appendix. The effect remains significant and similar in magnitude across columns.

In sum, the results in Table 2 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 contributed to creating. We discuss evidence for this mechanism in Section 4. It is worth remarking that the RD strategy means our effects are local in the sense that

¹⁶We report the associated first-stage coefficients in Online Appendix Table A.4. First-stage F-statistics are reported in the Table foot and are well above the conventional thresholds used to identify weak instruments.

¹⁷We do not include the logarithm of municipal spending per capita because we do not have this information for many municipalities. Even though including this control in the specification reduces the sample size somewhat, results remain qualitatively similar.

TABLE 2
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
Robust p-value	.003	.006	.014
Mean of dep. var.	.75	.75	.74
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. We report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted separately at each side of the threshold. The first column does not include any additional controls. The second column controls for predetermined municipal characteristics. The third column controls for the previous government characteristics. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

they correspond to municipalities with competitive elections. Arguably, this is precisely the sample that we care about when we think on the effects of reputational risk on policy choices.

3.3. Robustness Checks

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

First, we explore the sensitivity of our RD estimates to bandwidth choice. As explained in Section 3.1, the estimation of our parameter of interest uses the routine proposed in [Calonico et al. \(2017\)](#), which incorporates data-driven procedures to select a bandwidth and adjusts standard-errors to account for the bandwidth selector. In Online Appendix Figure A.4, we show the stability of our main estimated effect for different bandwidths around the threshold. For all bandwidth choices in the the [5%, 20%] interval, the coefficient of interest is statistically significant at 95% level and comparable in magnitude to those reported in Table 2.

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. These methodological choices do not affect our qualitative findings. In Online Appendix Table A.5, 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 weigh observations. The resulting point estimates fall in a range between 0.2 and 0.4 and are 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 \(2017\)](#) 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 as well as the 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 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 in proportion to initial vote shares. Estimates obtained when using this alternative running variable are provided in column 2 of Online Appendix Table [A.6](#). Reassuringly, the estimate of 0.286 is very close to the one reported in our main analysis.

4. Mechanism: Incumbency, Salience and Blame

Our results show that new governments in power have a higher probability of presenting an adjustment plan. Our hypothesis is that this can be explained by the fact that new governments can blame previous incumbents for accumulated arrears. Conversely, a continuing incumbent may not be willing to choose a course of action that reveals the issue to its voters, even if that implies choosing a sub-optimal policy.

In this section, we use a sample of SPP-related news to provide evidence in support of this mechanism. First, we complement the descriptive results presented in [Section 2.5](#) and use information of coverage of SPP in Spanish media outlets to show that presenting an adjustment plan increases the visibility of the SPP to voters—i.e., presenting a plan affects voters' information set.¹⁸ Second, we use ChatGPT to conduct a text analysis of municipal news covering the SPP. We study the origin and destination of criticisms included in these news and show that these are shaped by government turnover and the decision to implement an adjustment plan, in line with our proposed mechanism. Finally, we provide evidence consistent with incumbents avoiding to present an adjustment plan to protect their information rents. Alternative mechanisms are discussed in [Section 5](#).

¹⁸A large empirical literature in economics and political science has shown that voter information often influences voting choices. For a recent example in economics see [Bursztyń et al. \(2023\)](#).

4.1. Impact of the Adjustment Plan on the Voters' Information Set

We use a difference-in-differences approach to estimate the effect of the repayment choices on press coverage regarding inclusion in the SPP. We restrict our attention to municipalities that were included on the program, and measure whether presenting an adjustment plan has any incidence on the probability that this participation is featured in the news. Equation 4 presents our specification.

$$I(News_{it}) = \alpha + \beta_1 Ad.Plan_i + \beta_2 Post_t + \beta_3 Post_t \cdot Ad.Plan_i + \gamma_1' X_{it} + u_{it} \quad (4)$$

Variable $I(News_{it})$ is a dummy that takes value 1 if the *Supplier Payment Program* explicitly mentioned in a news including the name of the municipality and zero otherwise, $Ad.Plan_i$ is a dummy that takes value one for municipalities that present an adjustment plan, and $Post_t$ is a dummy variable that takes value one in 2012 and 2013, and value zero in 2011.¹⁹

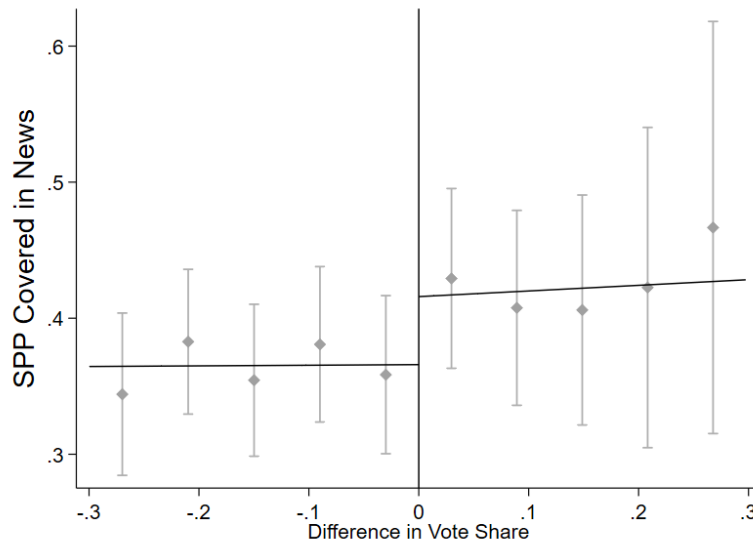
Results are reported in Online Appendix Table A.7 and indicate that presenting an adjustment plan is likely to affect voters' information set, as it increases the likelihood that participation of a municipality on the SPP is covered by the media. This finding is robust to controlling for municipality fixed effects and for the yearly total number of news per municipality.

We can also use our dataset to measure how government turnover impacts SPP coverage. We estimate a modified version of the system of equations 2 and 3 that define our RD, in which the second-stage outcome is replaced by the dummy $I(News_i)$, which takes value 1 if coverage of SPP associated to municipality i appeared in the press during 2012 and 2013. The associated reduced-form graph is provided in Figure 6. It shows a significant increase in SPP coverage at the threshold. Second-stage estimates, reported in Online Appendix Table A.8, indicate that a change of the party in power is associated with a 20 percentage point increase in the likelihood that the news cover the participation of the municipality in the SPP.

This result is consistent with: i) government turnover increasing the probability of submitting an adjustment plan, and ii) submitting a plan increasing the press coverage of the SPP.

¹⁹We can explore the extensive margin using the log of the number of SPP news as the dependent variable in equation 4 instead of the dummy. This leads to the same qualitative findings.

FIGURE 6
EFFECT OF CHANGE IN GOVERNMENT ON SPP NEWS COVERAGE



Notes: The figure plots the 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 first degree polynomials in the running variable estimated separately on each side of the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

4.2. *The Blame Game: Analysis of the SPP News Content*

How is the SPP news *content* shaped by the circumstances of the government in power? To answer this question, we downloaded over 21,800 news articles published in national and local Spanish newspapers and magazines between 2012 and 2013, covering any information related to the SPP or to any adjustment plan associated with a municipality. Using ChatGPT, we identify the municipalities mentioned in the news article and learn whether the news itself contained any criticism directed at the current or previous municipal governments, as well as which is the source of that criticism (i.e., government or opposition). Details of the procedure used to download and process the data can be found in [Online Appendix C](#). The processed sample includes 11,356 articles associated to 805 individual municipalities, with over 60% of them containing some form of criticism directed at the (current or previous) municipal government. An extensive validation of the precision with which ChatGPT3.5 (and other LLMs) identifies the origin and destination of criticism in this sample of news can be found in [Bermejo et al. \(2025\)](#).

We hypothesize that changes in the party in power lead to an increase in criticisms emitted by the new local government in matters relating to the SPP. Naturally, we also expect a concomitant decrease in criticisms aired by the opposition in this regard. The rationale is straightforward: newly appointed governments facing the new regime imposed by the SPP

will showcase to voters that they inherited unpaid commercial debt. Analogously, parties who were previously in power and are now in the opposition, would be less able to criticize the new municipal government over the SPP, as they were in charge of public finances when arrears were built up. We use the text analysis carried out with ChatGPT to investigate whether the press coverage exhibits these patterns.

We begin by carrying out a descriptive analysis of how criticisms vary with recent changes in the party in power. We estimate news-level regressions of different outcomes identifying the origin/destination of criticisms on a dummy C_i which takes value 1 if municipality i experienced a change in the party in power. To mitigate endogeneity concerns, we control for municipal characteristics including population, employment and several fiscal and financial variables.

The results of this exercise are reported in Table 3. We observe that changes in the party in power are associated with an increase in the proportion of criticisms originating in the current government by 10 percentage points, roughly equivalent to the increase in criticisms directed to the previous government in column 3.²⁰ We also observe a decrease in criticisms issued by the current opposition, and criticisms directed to the current government. This is aligned with the hypothesis that the opposition criticizes more fiercely continuing incumbents, while newcomers can blame their predecessors for the need for an adjustment.

To warrant causal interpretation, we complement this analysis with an RD design which induces exogenous variation in C_i . We aggregate our news data at the municipal level and use the same empirical strategy as the one reported in Section 4.1. Reduced-form graphs illustrating the effect of party change on the source of criticisms are reported in Figure 7. Panel A shows that the propensity that an article features criticisms emitted by the local government increases discontinuously at the threshold. Conversely, Panel B shows that the contrary takes place when looking at criticisms emitted by the opposition: a change in office is associated with less criticism coming from the opposition. Columns 1 and 2 of Online Appendix Table A.9, which show the 2SLS estimates, corroborate these findings.²¹ This confirms that, in news covering the SPP, the opposition criticizes more fiercely continuing incumbents, while newcomers try to pass the buck to their predecessors.

Finally, we explore the effect of presenting an adjustment plan on the source and direction

²⁰Criticisms to previous governments are generally lower on average, likely due to the fact that most news focus on current administrations in general.

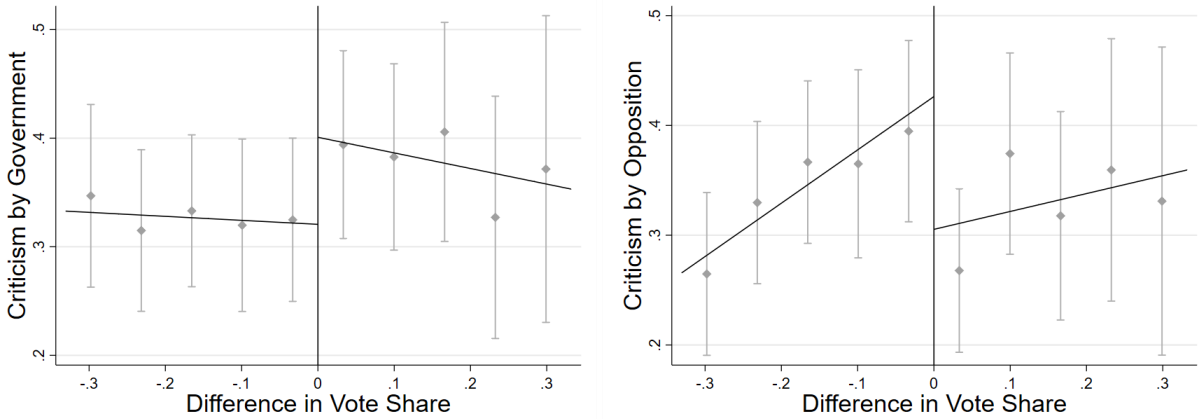
²¹Similar reduced-form and 2SLS estimates obtained using the destination of criticisms as our outcomes of interest are reported in Online Appendix Figure A.5 and Online Appendix Table A.9. While the sign of the reduced-form discontinuities is consistent with our hypothesis, estimates are fairly imprecise and insignificant at conventional levels.

TABLE 3
PARTY CHANGE AND CRITICISM OF LOCAL GOVERNMENT IN SPP NEWS

	Origin/Destination of Criticisms			
	(1) From Govt.	(2) From Opposition	(3) To Prev. Govt.	(4) To Curr. Govt.
Party Change	0.099*** (0.028)	-0.061*** (0.020)	0.091*** (0.012)	-0.047* (0.028)
Mean of dep. var.	0.34	0.34	0.10	0.32
Num. of Municipalities	724	724	724	724
Observations	10837	10837	10837	10837

Notes: Linear probability model estimates obtained using news article-level regressions. In column 1, the dependent variable is a dummy taking value 1 if the article contains criticisms emitted by the government. In column 2, the dependent variable is a dummy taking value 1 if the article contains criticisms emitted by the opposition. In column 3, the dependent variable is a dummy taking value 1 if the article contains criticisms directed at the previous government. In column 4, the dependent variable is a dummy taking value 1 if the article contains criticisms directed at the current government. In all columns, we report estimates of the coefficient of the party change dummy. All specifications control for predetermined municipal characteristics (see Figure 4 for full list). Standard errors clustered at the municipal level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

FIGURE 7
ORIGIN OF CRITICISM IN SPP NEWS: RD REDUCED-FORMS



(A) CRITICISM BY GOVERNMENT

(B) CRITICISM BY OPPOSITION

Notes: Panel A plots the reduced-form relationship between running variable and the fraction of articles that feature criticisms by the local government in our SPP sample of news. Panel B plots the reduced-form relationship between running variable and the fraction of articles that feature criticisms by the local opposition in our SPP sample of news. In both panels, the horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. Solid lines represent first-degree polynomials in the running variable estimated separately on each side of the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

of the criticisms featured in SPP news. For this purpose, we estimate regressions of the form $Y_{ji} = \eta Ad.Plan_i + \gamma' X_i + \varepsilon_{ji}$ where $Ad.Plan_i$ is a dummy taking value 1 if municipality i had an adjustment plan in place in the context of the SPP and X_i is a set of pre-determined

municipal characteristics. We focus on three outcomes of interest Y_{ji} measured at the level of individual articles j : a dummy taking value 1 if the piece of news contains criticisms directed to the current government, a dummy taking value 1 if the criticisms are made by the opposition, and a dummy taking value 1 if the article contains criticism made to the current government by the opposition. We provide separate estimates of η for each outcome and we split the sample depending on whether there was a change in office in the 2011 election.

Results of our analysis of the relationship between adjustment plans and criticisms featured in SPP news are reported in Table 4. Estimates indicate that, when there was no change in office, introducing an adjustment plan is associated with a relatively higher proportion of news criticizing the current government, with more criticism by the opposition and with more criticism to the government by the opposition. Interestingly, none of these patterns are observed if we focus on the sample of municipalities that *did* experience a change in office. We interpret this as suggestive evidence that the opposition will use the publicity associated with presenting a plan to intensify the critics to continuing incumbents for their responsibility in the (poor) state of the public finances. This will not happen when a newcomer with no previous responsibilities adheres to the SPP and presents an adjustment plan.

4.3. Incumbents' Management of Information Rents

We have shown that presenting an adjustment plan makes participation in the SPP more salient to electors. Considering that electors dislike budget deficits (Brender, 2003; Brender and Drazen, 2008; Drazen and Eslava, 2010), and taking on account that the opposition intensifies the criticisms after an adjustment plan, it is reasonable to hypothesize that continuing incumbents internalize this information to adjust their behavior. 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.

We obtain direct evidence of politicians' awareness of the trade-offs involved in implementing local policies from a survey embedded in a larger survey by García-Hombrados et al. (2024), administered to 126 Spanish mayors. 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. We identify four questions in the survey that are particularly relevant to our study as they ask about policy failure and change.

TABLE 4
ADJUSTMENT PLANS AND CRITICISM OF LOCAL GOVERNMENT IN SPP NEWS

	Origin/Destination of Criticisms					
	To Current Gov.		From Opposition		To Gov. from Opposition	
	(1)	(2)	(3)	(4)	(5)	(6)
Adjustment Plan	0.059 (0.067)	0.136** (0.061)	-0.041 (0.081)	0.112** (0.047)	0.024 (0.047)	0.091** (0.043)
Party Change	Yes	No	Yes	No	Yes	No
Mean of dep. var.	0.32	0.33	0.32	0.36	0.17	0.23
Num. of Municipalities	237	335	237	335	237	335
Observations	3857	5750	3857	5750	3857	5750

Notes: Linear probability model estimates obtained using news article-level regressions. For columns 1 and 2, the dependent variable is a dummy taking value 1 if the article contains criticisms directed at the current government. For columns 3 and 4, the dependent variable is a dummy taking value 1 if the article contains criticisms emitted by the opposition. For columns 5 and 6, the dependent variable is a dummy taking value 1 if the article contains criticisms to the current government and criticisms by the opposition. In all columns, we report estimates of the coefficient corresponding to the a dummy taking value 1 if the municipality approved an adjustment plan in the context of the SPP. All specifications control for predetermined municipal characteristics (see Figure 4 for full list). Sample restricted to municipalities participating in SPP. In odd columns, the sample is restricted to municipalities that experienced a change in the party in power in the 2011 election. In the even columns, the sample is restricted to municipalities that did not experience a change in the party in power in the 2011 election. Standard errors clustered at the municipal level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

Table 5 summarizes the answers given by respondents to these questions. Question 1 in the table asks whether mayors would change a policy if they received evidence that it is not working. As one would expect from competent politicians, over 90% of mayors declare that they would indeed change the policy. Interestingly, when they are asked whether they have ever done so (question 2), only 50% answer affirmatively. Of course this might simply point out that half of the mayors were never aware of any flawed policies. But question 3 points otherwise. Inquired about their agreement 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*”, a surprising 20% declared that they strongly agree. This is notable considering that we are surveying professional politicians, who have incentives to signal virtue. Instead, we find that less than 30% of mayors disagree with the aforementioned statement, suggesting that many have just naturalized that this is how politics work. This can also explain why, in question 4, we observe that a significant proportion of mayors consider newcomers are in a better position than incumbents to navigate financial troubles. If politicians consider that the room for manoeuvre is restricted by past decisions, it is reasonable that they think newcomers are in a better shape to take decisive action when dealing with these problems.

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 financial problems (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: Summary of 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. A total of 126 mayors from a sample of Spanish municipalities answered this survey.

To explore whether incumbents' choices actually affect their electoral prospects in this context, we analyze the relationship between the decision to present a public adjustment plan to get better financing conditions, and the probability of re-election in 2015. For this purpose, we estimate the following specification:

$$R_i^{2015} = \alpha_0 + \alpha_1 I_i + \alpha_2 Ad.Plan_i + \alpha_3 Ad.Plan_i \cdot I_i + \gamma X_i + u_i \quad (5)$$

where R_i^{2015} is a dummy taking a value of 1 if the party in power before the 2015 election was re-elected. I_i (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. $Ad.Plan_i$ is a dummy taking a value of 1 if the municipality presents an adjustment plan, and X_i 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. For example, it is possible that the worst incumbents are precisely those that request assistance in the first place.

TABLE 6
MAYOR RE-ELECTED IN 2015

	(1) Re-Elected	(2) Re-Elected
Incumbent 2010	0.149*** (0.0284)	0.152*** (0.0291)
Adjustment Plan	-0.0260 (0.0298)	-0.00635 (0.0304)
Incumbent 2010 \times 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.

Estimates for the coefficients in equation 5 are provided in Table 6, 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 suggests that continuing incumbents may bear an electoral cost of presenting a plan, which is not present for newcomers, rationalizing the behavioral pattern that we observe in our main analysis.

Finally, to provide indirect evidence of our proposed mechanism, in Online Appendix Table A.10 we report estimates from our main RD specification for three different sub-samples. We divide municipalities according to their pre-existent level of arrears, and explore the different effect of a change in office on the probability of presenting a plan in each case. We observe that for those municipalities in the bottom tercile of the distribution (column 1) 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). This pattern is consistent with the fact that the amount of arrears affects the intensity of the (negative) signal on the performance of the previous government. It is only when this signal is large enough that we observe a difference in behavior between newcomers and continuing incumbents.

5. Alternative Explanations

The evidence gathered in Section 4 is consistent with the notion that re-elected incumbents are more reluctant than newly elected governments to request financial assistance, as this action reveals negative information about their past performance. However, there are several alternative mechanisms that are worth exploring. In this section, we analyze these competing explanations.

5.1. Observable Characteristics of the Elected Government

Among all alternative mechanisms, probably the most natural is that something else than the tenure of the elected politician is changing after a change in office. To evaluate whether other politician characteristics change at the threshold, we use an RD specification similar to the one in our main analyses, using as dependent variables attributes of the elected government.

Estimates for different characteristics are reported in Online Appendix Table A.11. 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. 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 (and an increase in the probability of having a PP mayor). These differences in ideology and/or alignment with the national government of elected mayors could provide an alternative explanation to the differences we observe in the propensity to submit an adjustment plan.

We follow three different strategies to test whether changing characteristics at the threshold are indeed the mechanisms driving our baseline results. First, we add controls for characteristics of the 2012 mayor in our main specification. Second, we explore the effect of a change in office segregating the sample according to the party of the incumbent and the party of the challenger participating in the 2011 election. 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 7. Column 1 of the top panel reproduces our baseline specification for

²²Balanced characteristics at the threshold include the seat share of the mayor, the presence of a one-party majority in the council, a dummy taking value 1 for female mayors and dummies corresponding to different education levels and occupations of the elected mayor. We show graphically how these characteristics vary at the threshold in Online Appendix Figure A.6.

comparison purposes. In column 2, we include three covariates in our specifications, dummy variables taking value one when the elected mayor is from PP or from PSOE, and 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 initial incumbents were from PSOE and from PP, respectively. We continue to find large 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. Finally, in columns 5 and 6, we report RD 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 positive and similar in size to the one reported in our baseline specification.

TABLE 7
LEADERSHIP CHANGE & ADJUSTMENT PLANS BY PARTY

	(1)	(2)	(3)
	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.320*** (0.0935)	0.289*** (0.0902)	0.301*** (0.109)
Observations	1150	1076	632
Mean of dep. var.	.74	.74	.75
Bandwidth	.147	.177	.134
Specification	Baseline	With Controls	PSOE Incumbents
	(4)	(5)	(6)
	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party Change	0.502* (0.286)	0.402** (0.182)	0.339*** (0.119)
Observations	174	391	541
Mean of dep. var.	.76	.74	.75
Bandwidth	.087	.178	.14
Specification	PP Incumbents	PSOE Challengers	PP Challengers

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 column 1 we present estimates from our baseline specification. In column 2, we include the mayor's age, and dummies for PP and PSOE mayors as controls. Estimates in columns 3 and 4 are obtained after restricting the sample to municipalities with a PSOE incumbent or a PP incumbent, respectively. Estimates in columns 5 and 6 are obtained after restricting the sample to municipalities with a PSOE challenger or a PP challenger, respectively. In all cases, we report estimates from local linear regressions with a triangular kernel and first degree polynomials fitted separately at the two sides of the threshold. All specifications include a control for the logarithm of municipal arrears per capital. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

The consistent results obtained for different partitions of our sample in Table 7 strongly suggest that partisan affiliation is not driving our main findings. To further stress this point, 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). We restrict our attention to municipalities in which PSOE (PP) is either the mayor, or the most voted opposition party, and use the difference in vote share between PSOE (PP) and the other most voted party as our running variable.

Results for this exercise are reported in Online Appendix Table [A.12](#). 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. 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 [2](#), providing conclusive evidence that our main effect of interest is not driven by partisan differences in the propensity to submit an adjustment plan.

5.2. Incumbency Advantage and Characteristics as Compensating Differentials

Even if observable characteristics can be balanced at the threshold, there are reasons to believe that focusing on close elections in our research design may mechanically induce a correlation between incumbency status and politicians' unobservable quality. The general argument is clearly laid out in [Marshall \(2022\)](#). Consider our case here. Suppose politicians' popularity with voters is driven by two factors: perceived quality or valence, and incumbency, with incumbents being valued more than newcomers. If that were the case, when we compare incumbents and newcomers with similar vote shares—the comparison in our regression-discontinuity design—it must be the case that these two candidates are different in quality. That is, quality has to act as a compensating differential for incumbency to ensure vote shares are equal on average. Given that quality is unobservable, we cannot deploy the type of solutions described in the previous section.

This concern, however, hinges on the assumption that there was some form of incumbency advantage in the 2011 Spanish local elections. While there is a large literature documenting incumbency advantage in competitive elections, recent work has shown that the existence and size of this advantage is context-dependent (see e.g., [Karnik, Lalvani and Phatak 2023](#); [Guriev et al. 2025](#); [Descamps et al. 2025](#)).

We test for the presence of incumbency advantage in our sample using a standard RD framework—a contemporary adaptation of the approach in [Lee \(2008\)](#), incorporating the fact that Spanish voters elect councils and councils appoint mayors. We use PP as the reference party when conducting this analysis, motivated by the fact that this was the party holding the

most governments at the time of the SPP.²³ We rely on estimating the two-step specification:

$$\begin{aligned} M_i^{PP} &= \omega_0 + \nu W_i^{PP} + \omega_1 \Delta V_i^{PP} + \omega_2 \Delta V_i^{PP} W_i^{PP} + \varepsilon_i \\ V_{i,2011}^{PP} &= \gamma_0 + \eta M_i^{PP} + \gamma_1 \Delta V_i^{PP} + \gamma_2 \Delta V_i^{PP} W_i^{PP} + \varepsilon_i \end{aligned}$$

where $V_{i,2011}^{PP}$ is the PP vote share in the 2011 local election, ΔV_i^{PP} is the difference in vote share between PP and the most voted party other than PP in the 2007 elections and will be our running variable. Variable $W_i^{PP} = \mathbb{1}\{\Delta V_i^{PP} > 0\}$, so it is a dummy taking value 1 if PP was the most voted party in the 2007 election, and M_i^{PP} is a dummy taking value 1 if PP appointed a mayor after the 2007 election. Parameter ν is the first stage estimate of interest and η is the incumbency advantage estimate for PP. In addition to estimates for η , we will report reduced-form estimates obtained by replacing outcome $V_{i,2011}^{PP}$ as the outcome in the first-stage equation above.

Reduced-form and second-stage estimates of the incumbency advantage are reported in Panel A and B of Table 8, respectively. Estimates reported in the table show we cannot detect any incumbency advantage using our SPP sample of municipalities. Second-stage estimates are small, at roughly 0.01 depending on the specification.²⁴ The associated reduced-form graph for this exercise is reported in Appendix Figure A.7 also shows no discernible effect. This feature of our context means that one can plausibly assume that the attribute in question does not affect electoral outcomes. This overcomes the close-election problem of PCRD estimates highlighted in Marshall (2022). Note that this does *not* mean that quality will necessarily be balanced at the threshold, just that it will no be mechanically unbalanced.

5.3. Unobservable Characteristics of the Elected Government

In this section, we complement our previous result providing supporting evidence that shows that our main results (see e.g., Table 2) are not driven by an imbalance of unobservable quality at the threshold. First, we use 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 et al. 2014). Results reported in Online Appendix Table A.11 indicate that these quality measures vary smoothly at the threshold.

Moreover, to deal with potential unobservable differences in quality, our second approach

²³Qualitative results are essentially the same when using PSOE as the reference party.

²⁴Traditional incumbency advantage estimates from US House elections (see e.g., Lee 2008 and Erikson, Titunik et al. 2015) is estimated to be in the order of 8%, so much larger than our point estimates.

TABLE 8
NO DETECTABLE INCUMBENCY ADVANTAGE IN 2011 ELECTION

	(1)	(2)
A. Reduced-Form		
	PP Vote Share	PP Vote Share
Party Wins	0.006 (0.014)	0.002 (0.014)
Observations	838	732
Mean of dep. var.	.48	.48
Bandwidth	.148	.132
Controls	No	Municipality
B. 2SLS Estimates		
	PP Vote Share	PP Vote Share
Party Mayor	0.014 (0.025)	0.009 (0.024)
Observations	942	909
Mean of dep. var.	.48	.48
Bandwidth	.173	.171
First-stage Fstat	89	84
Controls	No	Municipality

Notes: Regression-discontinuity estimates of incumbency advantage in the 2011 elections. The outcome variable in both panels is the 2011 PP vote share in local elections. Panel A represents reduced-form estimates, showing how a PP election victory in 2007 affects the party's vote share in 2011. Panel B presents two-stage least squares estimates of the effect of having a PP mayor appointed after the 2007 election on the PP vote share in 2011. First stage F-statistics for each specification included below the estimates. In both panels, we report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted separately on each side of the threshold. The first column does not include any additional controls. The second column controls for predetermined municipal characteristics. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

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.²⁵ 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. The purpose of this exercise is to evaluate whether we obtain similar results when focusing on different portions of the quality distribution (thus narrowing remaining variation in quality).

We construct the leave-one-out average of the vote swing experienced by party p in mu-

²⁵Regional 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.

municipality i belonging to province s during the election of 2011:

$$Partyswing_{ip} = \sum_{\substack{l \in s \\ l \neq i}} \frac{V_{lp2011} - V_{lp2007}}{N_s - 1}$$

where V_{lp2011} is the vote share of party p in municipality l belonging to province s . The number of municipalities in province s is denoted by N_s . We then compute the “no swing” margin of the challenger ($p = G$) over the incumbent ($p = B$) as follows:

$$NoSwingMargin_i = \Delta V_i - Partyswing_{iG} + Partyswing_{iB} \quad (6)$$

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. It seems reasonable to claim that detracting regional party shocks increases the relevance of candidates’ quality on electoral performance. Hence, 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 9 show that our effect of interest in the subsample with challengers whose quality is low relative to the quality of incumbents (negative “no swing” margin) is roughly 0.3, similar to our baseline RD estimate.²⁶ A very similar estimate is observed in column 2 of Table 9, where we focus on the subsample of challengers whose quality is high relative to incumbents. This suggests that differences at the threshold in the quality of candidates do not explain our main findings.

6. Government Turnover and External Assistance: Cross-Country Evidence

In this section, we present a cross-country analysis documenting that government turnover is associated with a higher probability of signing a financing agreement with the IMF. The purpose of the analysis is to provide suggestive evidence that the results reported above using the SPP program may generalize to higher-stakes decisions on macroeconomic policy at the national level.²⁷

²⁶We 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.

²⁷We present three illustrative cases linking incumbency of national governments to the decision to request assistance in [Online Appendix D.5](#).

TABLE 9
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-stage 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 estimates from local linear regressions with a triangular kernel and first degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

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, resulting in a panel which covers a total of 153 countries.²⁸ Using this sample, we first document three descriptive 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). Second, the signing of new IMF financing agreements is also common, occurring in 11 percent of country year pairs across 124 countries. Finally, new IMF financing agreements are 5.4 percentage points more common in years in which a new government is in power, suggesting a positive association between government turnover and the decision to sign an IMF agreement.

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 2008, 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,

²⁸We define democracies as governments elected or appointed as a result of competitive elections. We use a definition of competitive elections based on the Index of Electoral Competitiveness from the 2020 Database of Political Institutions. Details on this definition, as well as the construction of the dataset and descriptive statistics can be found in [Online Appendix D](#)

Gören 2014, Arbatli et al. 2020). To mitigate these concerns, we use a panel to estimate:

$$Program_{ct}^{IMF} = \beta C_{ct} + \alpha_c + \delta_t + \gamma_1 Controls_{ct} + u_{ct} \quad (7)$$

where the dependent variable is a dummy taking value one if country c 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_c and δ_t respectively. Variable C_{ct} is a dummy that takes value 1 if the country experienced a change in the party in charge of the executive in the last election and 0 if it is ruled by the party that had been the incumbent before the last election.²⁹ In some specifications, we also include as controls the three-year moving average of national GDP growth; lagged GDP per capita and lagged total GDP (both measured in U.S. dollars); lagged consumer price inflation; the lagged ratio of international reserves to imports; and two political party dummies equal to one if the chief executive’s political party is right- or left-leaning, respectively.³⁰ 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 signing an agreement with the IMF.

In columns 1 through 3 of Table 10, we show estimates of β obtained using variations of the specification in equation 7. In column 1, we present the unconditional linear probability estimate of 5.4 percent, which corresponds to the difference in the probability of signing an IMF agreement 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 sign an agreement with the IMF and government turnover. We find that the coefficient of interest remains significant, with a size of 3.8% relative to an 11% baseline probability of signing a funding agreement with the IMF. After including several macroeconomic controls and controlling for the government’s political orientation in column 3, the coefficient of interest remains qualitatively unchanged.³¹ Finally, to account for heterogeneity in time-varying confounders across similar groups of countries, in column 4 we follow the approach proposed

²⁹Specifically, the value takes value 1 when a new party comes to power in the first half of the year (before July), or when a new party came to power in the second half of the previous year. The variable continues to be equal to 1 until the next national election. The variable then takes value 0 if the same party stayed in power after the next election. Take for example the tenure of José María Aznar which lasted between May 1996 and April 2004. Variable C_{ct} took value 1 between 1996 and 2000, and value 0 between 2000 and 2003.

³⁰We construct these political party dummies using the DPI2020 database. The omitted category corresponds to centrist parties and parties with no clear ideological alignment.

³¹To mitigate potential concerns of reverse causality, in an separate analysis we also lag one year the C_{ct} variable. Results remain unchanged. We further augment the baseline specification by controlling for the lagged gross debt-to-GDP and lagged current account-to-GDP ratios. Despite losing more than 10% of the sample due to missing observations, the coefficient of interest remains economically similar and statistically significant at the same level.

in [Bonhomme and Manresa \(2015\)](#). Grouping countries into four clusters with different time trends, the coefficient of interest remains significant and similar in magnitude.

TABLE 10
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)	0.038*** (0.010)	0.036*** (0.011)	0.033*** (0.011)
Observations	3,730	3,730	3,045	3,044
Mean of dep. var.	.11	.11	.1	.1
Country & Year FE	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
GFE \times Year	No	No	No	Yes

Notes: The Table reports OLS estimates of parameters in equation 7. The dependent variable in all specifications is a dummy variable that takes a value of 1 if the country signs an IMF funding program in that year, and 0 otherwise. Party Change takes a value of 1 if the country is ruled by a party that came to power in the last national election, and a value of 0 if the country is ruled by the previous incumbent party. Columns (2) and (3) include country and year fixed effects. Column (3) additionally controls for the three-year moving average of national GDP growth; lagged GDP per capita and lagged total GDP (both measured in U.S. dollars); lagged consumer price inflation; the lagged ratio of international reserves to imports; and two political party dummies equal to one if the chief executive's political party is right- or left-leaning. 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 10 aim to mitigate the omitted variable problem, suggesting a causal link between government turnover and receiving financial assistance, the assumptions needed for exogeneity of the C_{ct} variable are still rather strong. In [Online Appendix D](#), we develop additional empirical analyses and provide anecdotal evidence consistent with the proposed mechanism. First, we restrict our attention to countries undergoing a banking crisis ([Laeven and Valencia, 2020](#)). Focusing on this restricted sample we seek 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. Remarkably, we find that in countries undergoing a banking crisis, the probability to sign a funding program with the IMF rises between 22 and 30 percentage points if there is a newcomer in office. Second, we examine the timing of IMF program signings around elections. Consistent with the proposed blame attribution mechanism, the probability of signing an IMF program is significantly higher in the first year after an election in countries with a change in office—but not in others. Finally, we explore heterogeneity across types of IMF programs, distinguishing between development-aid like programs and bailout programs. Consistent with our mechanism, political turnover is associated with IMF participation primar-

ily through bailout programs, which carry greater stigma and are more closely linked to acute macroeconomic distress.

Altogether, these results provide evidence of a positive association between a change in office and the decision to sign for external financial assistance at the country level. Moreover, the documented advantages of back-loaded adjustment programs for economic growth (Blanchard and Leigh, 2013a; Guajardo, Leigh and Pescatori, 2014) suggest that this phenomenon may stem, also in the cross-country framework, from an agency problem between voters and elected officials. That said, the stronger assumptions required to claim exogeneity of the estimates, the impossibility of disentangling the supply and demand of assistance, the political conditionality typically associated with IMF bailouts—which introduces ideological considerations into the decision—and the contested nature of their benefits (Stiglitz, 2002; Balima and Sokolova, 2021), limit the interpretability of the IMF estimates as causal and put the value of the SPP framework into perspective.

7. Conclusions

Since 1992, the IMF has been a key source of funding for over 100 countries, addressing economic challenges on a global scale. Concurrently, sub-national institutions, which hold almost 25% of total public debt in OECD countries, frequently find themselves in need of extraordinary financial support from their national governments. The decision-making process surrounding negotiations with these lenders of last resort is complex and influenced not solely by institutional needs, but by the political constraints facing decision-makers. In this paper, we study how political tenure and electoral incentives impact the decision to request a bailout.

Our central argument revolves around two key points. First, agreements with institutional lenders are typically public and serve as indicators of past government performance. Second, the incentive structures for re-elected incumbents and newly elected governments differ significantly in terms of taking actions that reveal information about past performance to voters. Consequently, we anticipate divergent choices between these two government types when confronted with the decision to seek financial assistance. Our study yields compelling cross-country evidence and causal estimates based on local government participation in the Spanish SPP, which confirm the reluctance of continuing incumbents to publicly request assistance. We further substantiate our findings through an analysis of news content and a survey of Spanish mayors, showing that incumbents choose a suboptimal policy—refraining from presenting a plan—to safeguard their reputation. These results inform the design of

incentive compatible bailouts by highlighting the role that publicity plays in determining the willingness of elected officials to request assistance.

One open question that remains for further research to tackle is whether the mechanism emphasized in this paper operates with policy selection in other areas. In principle, the reluctance of ongoing incumbents to modify dominated policy choices could apply in the case of highly-visible policies which are long-lasting in that their reversal conveys information about the past. Examples of this sort of policies could include large infrastructure projects or significant regulatory reforms associated with the government who championed them. Government turnover may allow to reverse or modify these policies if they are found to be inadequate or mistaken.

More generally, the conclusions of this study also illustrate how past actions and electoral incentives restrain the ability of incumbent parties to choose what is optimal for their citizens. This provides an explanation for the positive effect that political turnover has on government performance (Marx, Pons and Rollet, 2025) and emphasizes the relevance of political alternation in democracies. Quoting the former Vice President of the US, Hurbert H. Humphrey, *To err is human. To blame someone else is politics*. While continuing incumbents fear that the opposition might exploit their attempts to rectify past errors, newcomers are ready to change what is not working and pass the buck to their predecessors.

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Online Appendix

Online Appendix A. Additional Tables & Figures

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
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
Mean of dep. var.	8.9	2.8	.3
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
Mean of dep. var.	14.7	-10.8	327.9
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
Mean of dep. var.	328.5	.61	12.91
p-value	0.836	0.632	0.405
Bandwidth	0.147	0.116	0.156

Notes: Estimates of the effect of the party change dummy on different pre-determined municipal characteristics obtained via two-stage least squares. We report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted separately at each side of the threshold. Bandwidth selected using the CCT method for each variable and indicated in the foot of each case. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.3
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
Mean of dep. var.	.67	.53	.07
p-value	0.540	0.628	0.277
Bandwidth	0.161	0.164	0.175
	Female Mayor	Mayor w/College	Party Change (2007)
Party Change	-0.0799 (0.0955)	-0.0864 (0.140)	-0.0858 (0.113)
Observations	967	733	1216
Mean of dep. var.	.18	.46	.37
p-value	0.403	0.538	0.447
Bandwidth	0.121	0.122	0.156
	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
Mean of dep. var.	50	.59	.27
p-value	0.159	0.747	0.860
Bandwidth	0.101	0.178	0.176

Notes: Estimates of the effect of the party change dummy on different different pre-determined local government characteristics obtained via two-stage least squares. Characteristics pertain to the party in power or the mayor in office before the 2011 election. We report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted separately at each side of the threshold. 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
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
Mean of dep. var.	.47	.47	.46
Bandwidth	.138	.131	.148
First-stage Fstat	112	95	108
Controls	No	Municipality	Prev Govmnt

Notes: First-stage estimates for fuzzy regression-discontinuity estimates (see Table 2 in the text). 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. We report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted 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.5
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
Mean of dep var	.74	.74	.74	.74	.74
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 estimates from local linear regressions with a triangular kernel and first degree polynomials fitted at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.6
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 3.3. No additional control included in these specifications. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.7
SPP PRESS COVERAGE & ADJUSTMENT PLANS

	(1)	(2)	(3)
	I(News SPP)	I(News SPP)	I(News SPP)
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
Mean Dep. Var	.13	.13	.13
Municipality FE	No	No	Yes

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.8
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
Mean Dep. Var	.38	.39	.39
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. We report estimates from local linear regressions with a triangular kernel and first degree polynomials fitted separately at each side of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.9
RD ESTIMATES OF PARTY CHANGE ON ORIGIN OF CRITICISMS BASED ON SPP NEWS

	(1)	(2)	(3)	(4)
	by Government	by Opposition	to Current Gov	to Former Gov
Party Change	0.266* (0.147)	-0.435** (0.192)	-0.227 (0.168)	-0.0333 (0.0939)
Observations	759	759	759	759
Mean of dep. var.	.36	.36	.26	.15
Observations within BW	215	199	247	206
Bandwidth	.092	.084	.104	.087
First-stage Fstat	32	32	33	32

Notes: The table presents two stage least squares estimates of the effect of a change in municipal government on the proportion of news articles in our SPP sample featuring criticisms with specific origins or destinations as indicated in the table head. We report estimates from local linear regressions with a triangular kernel and first degree polynomials fitted at the two sides of the threshold. Bandwidth selected using the CCT criterion. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.10
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)	0.409*** (0.152)
Observations	283	422	338
Bandwidth	.11	.164	.123
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 party change in municipal government on the probability of presenting an adjustment plan. We report estimates from local linear regressions with a triangular kernel and first-degree polynomials fitted at the two sides of the threshold. The first column reports the estimate for municipalities in the bottom tercile of the distribution of arrears per capita. The second column reports the estimate for municipalities in the middle tercile of the distribution of arrears per capita. The third column reports estimates for municipalities in the upper tercile of the distribution of arrears per capita. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

TABLE A.11
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
Mean of dep. var.	.68	.48	.07	.2
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
Mean of dep. var.	.49	.04	.58	.24
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
Mean of dep. var.	.02	48.33	.38	.41
p-value	0.320	0.028	0.000	0.016
Bandwidth	0.165	0.144	0.139	0.146

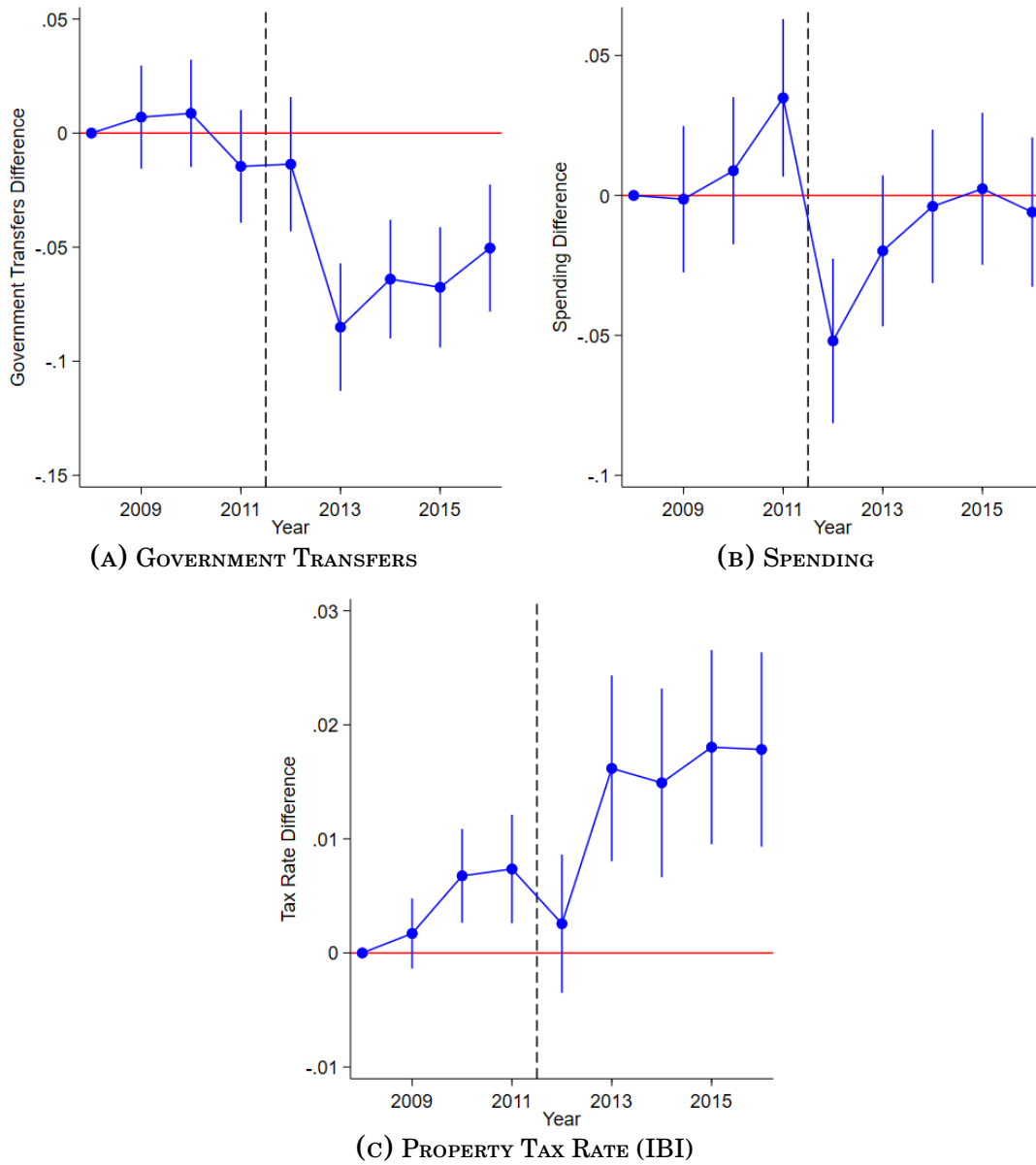
Notes: Estimates of the effect of the party change dummy on different different pre-determined local government characteristics obtained via two-stage least squares. Characteristics pertain to the party in power or the mayor in office after the 2011 election. We report local linear regression estimates obtained using a triangular kernel and first degree polynomials fitted separately at each side of the threshold. 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.12
PARTY MAYOR & ADJUSTMENT PLAN

	(1)	(2)	(3)	(4)
	Adjustment Plan	Adjustment Plan	Adjustment Plan	Adjustment Plan
Party of Mayor	-0.0137 (0.0975)	-0.0375 (0.0997)	0.0340 (0.0904)	0.0200 (0.0845)
Observations	1252	966	1552	1258
Controls	No	Yes	No	Yes
Reference Party	PP Mayor	PP Mayor	PSOE Mayor	PSOE Mayor
p-value	0.888	0.707	0.706	0.813
Bandwidth	0.196	0.187	0.223	0.222

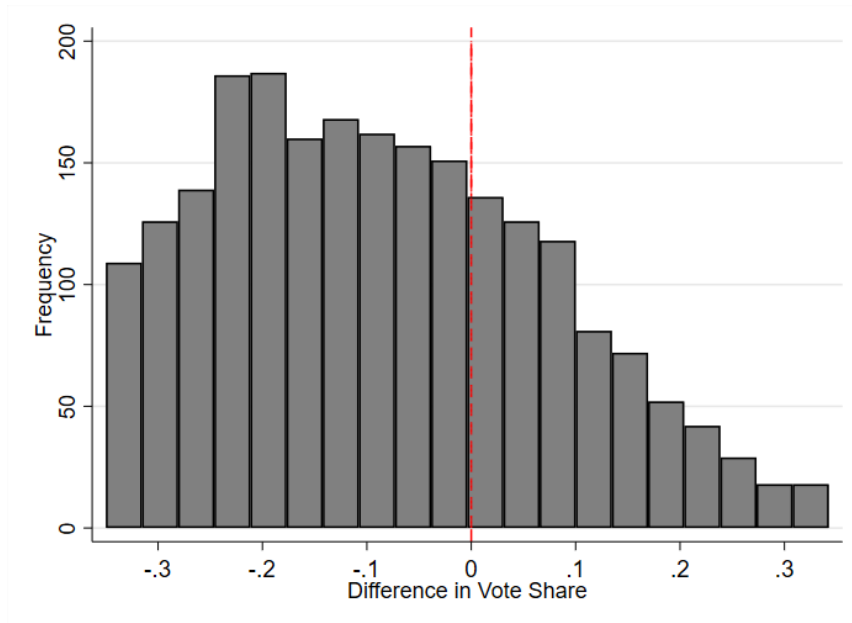
Notes: The table presents two stages least squares estimates of the effect of a PP / PSOE mayor on the probability of presenting an adjustment plan. The first and second columns show the effect of having a PP mayor. The first column adds no controls, while the second column controls for arrears per capita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

FIGURE A.1
CONSEQUENCES OF GOVERNMENT RETENTION SCHEME (NO INTERACTION)



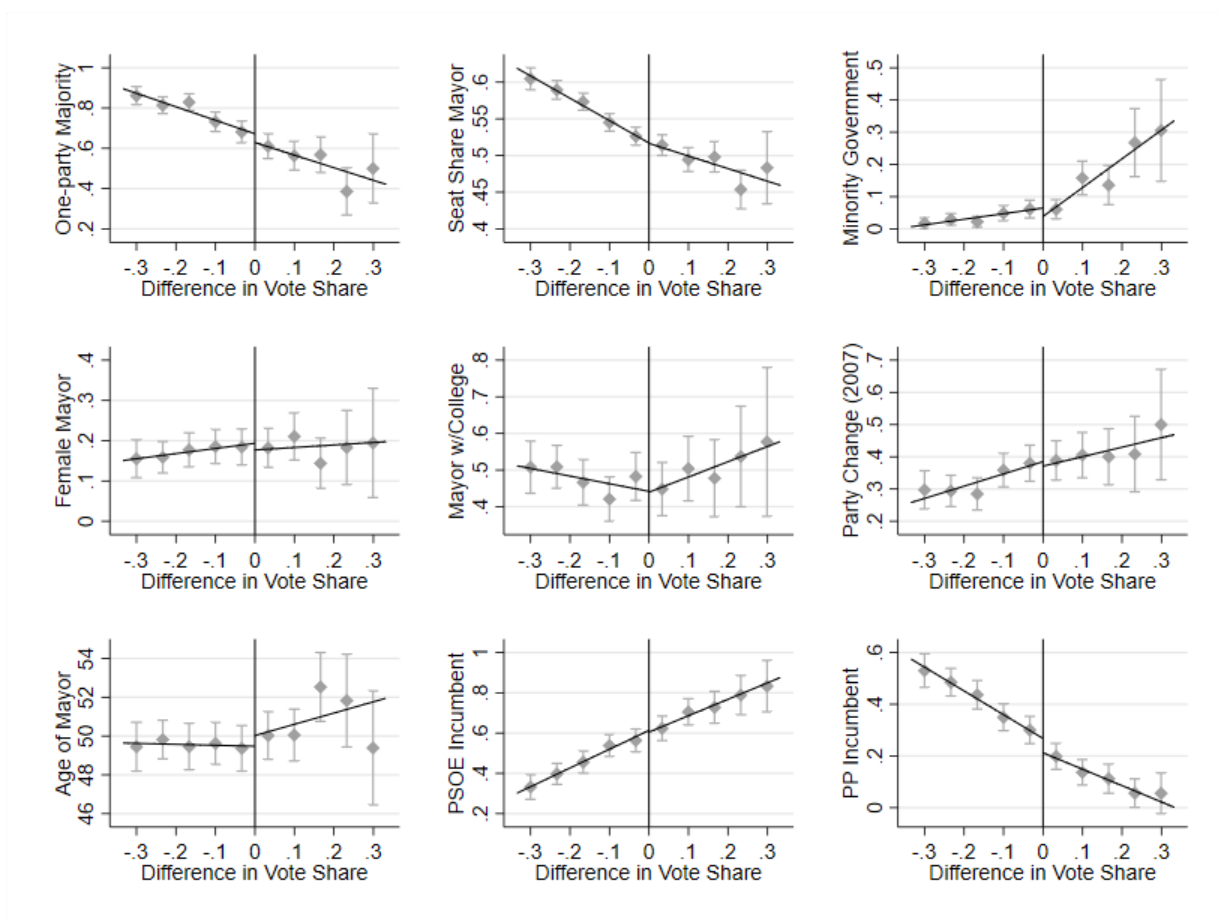
Notes: These figures show point estimates and 95% confidence intervals for the sequence of coefficients ω defined in equation 1. The dependent variable used to estimate these coefficients is the log of the transfers received from the central government in panel A, the log of total municipal spending in panel B, and the property tax rate (IBI) in panel C. Estimation carried out using data for the period 2008-2015. All regressions include municipality fixed effects and year effects. Standard errors are clustered at the municipality level. Dashed line marks the transition between 2011 and 2012.

FIGURE A.2
HISTOGRAM OF RUNNING VARIABLE – VOTE MARGIN OF MUNICIPAL CHALLENGER



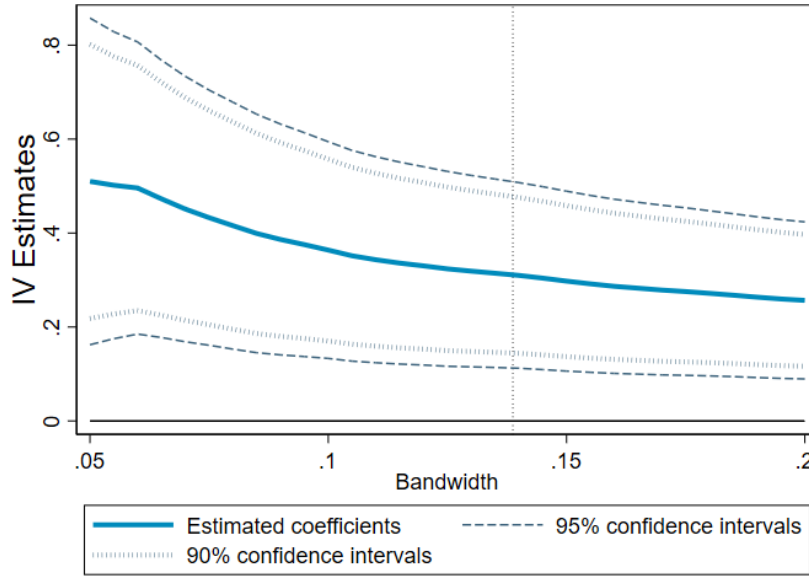
Notes: Histogram of running variable for values between -0.35 and 0.35. The p-value of the [Cattaneo, Jansson and Ma \(2019\)](#) test of no manipulation is 75%.

FIGURE A.3
COVARIATE BALANCING – PREVIOUS GOVERNMENT CHARACTERISTICS



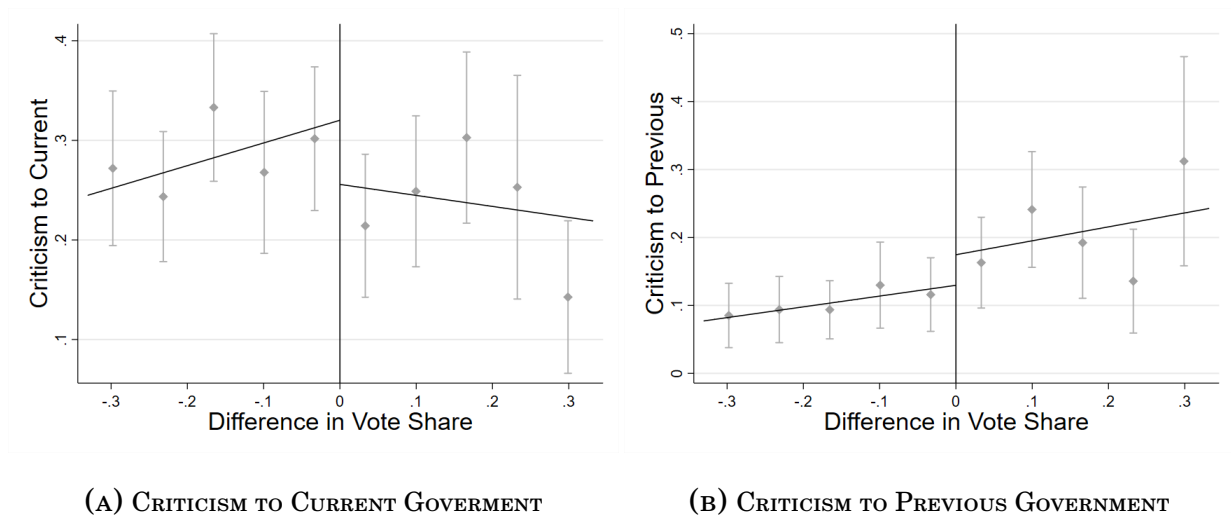
Notes: Balancing tests using the characteristics of the government in power in the period before the 2011 election. The horizontal axis represents the vote share difference between the challenger and the incumbent in that election. From left to right and top to bottom the vertical axes represent fraction 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 first degree polynomials in the running variable estimated separately at each side of the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

FIGURE A.4
ROBUSTNESS OF RD ESTIMATES TO BANDWIDTH CHOICE



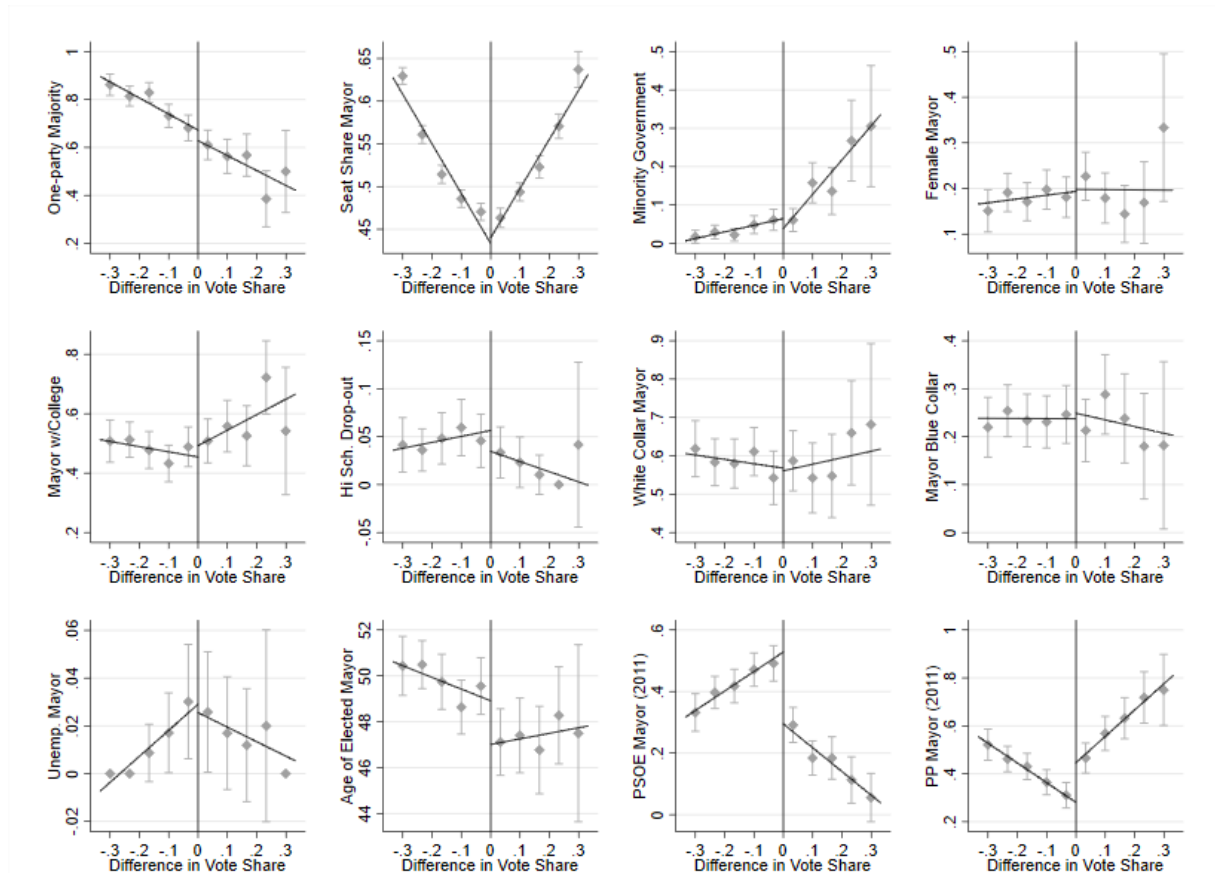
Notes: The horizontal axis represents different bandwidths around the threshold. The vertical axis represents the size of the estimated effect of having a new government on the probability of presenting an adjustment plan. The solid line corresponds to point estimates for different bandwidths. Dotted and dashed lines around the point estimates represent 90% and 95% confidence intervals, respectively. The vertical dotted line indicates the optimal bandwidth used to produce the estimates in column 1 of Table 2.

FIGURE A.5
DESTINATION OF CRITICISM IN SPP NEWS: RD REDUCED-FORMS



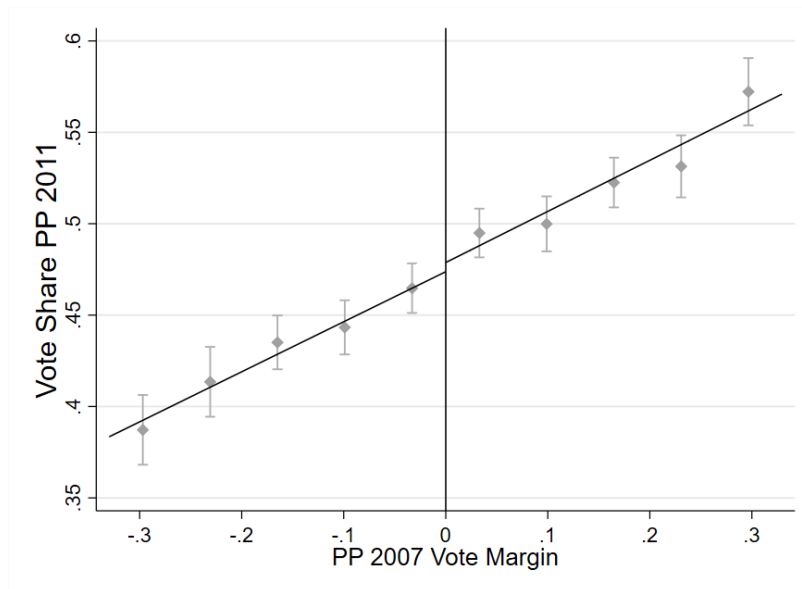
Notes: Panel A illustrates the reduced-form relationship between the running variable and the fraction of articles that feature criticisms to the current government in our SPP sample of news. Panel B illustrates the reduced-form relationship between running variable and the fraction of articles that feature criticisms of the previous government in our SPP sample of news. In both panels, the horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. Solid lines represent first-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.

FIGURE A.6
COVARIATE BALANCING – ELECTED GOVERNMENT CHARACTERISTICS (2011)



Notes: Graphs plot the relationship between the running variable and the characteristics of governments elected after the 2011 election. The horizontal axis represents the vote share difference between the challenger and the incumbent in that election. 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, and fraction of municipalities with a PP major. All variables correspond to governments or mayors appointed after 2011 election. Solid lines represent first degree polynomials in the running variable estimated separately at each side of the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

FIGURE A.7
NO DETECTABLE INCUMBENCY ADVANTAGE IN 2011 ELECTION



Notes: The running variable in the horizontal axis is the PP margin of victory in 2007. In the vertical axis we represent the PP vote share in 2011. Solid black lines represent first degree polynomials in the running variable estimated separately on each side of the threshold. Gray dots correspond to averages for bins of the running variable. Vertical lines correspond to 95% confidence intervals around these averages.

Online Appendix B. Net Present Value of Presenting a Plan

The SPP is a credit shock for municipalities with arrears. They can no longer use commercial debt to finance themselves, and are forced to pay current arrears back to the national government. Those municipalities that present an adjustment plan have a longer time span to pay back. Considering that this debt delivers an interest rate which is subsidized by the central government, it is intuitive that payment schemes that last longer should have higher NPV for municipalities. In this appendix, we compare the NPV of the two possible repayment schemes.

To make this calculation, we assume that municipalities who present an adjustment plan take advantage of the 10 years to pay back, only paying interest in the first two years, an option available to them in the context of the SPP. Municipalities that do not present an adjustment plan are forced to pay all outstanding debt back within five years. These are the resulting NPVs under the two financing schemes:

$$NPV_{NoPlan} = A - \sum_{t=1}^5 \frac{C_{NoPlan}}{(1+r)^t}, \quad \text{where } C_{NoPlan} = \frac{A \cdot i}{1 - (1+i)^{-5}} \quad (\text{B.1})$$

$$NPV_{Plan} = A - \frac{A \cdot i}{1+r} - \frac{A \cdot i}{(1+r)^2} - \sum_{t=3}^{10} \frac{C_{Plan}}{(1+r)^t}, \quad \text{where } C_{Plan} = \frac{A \cdot i}{1 - (1+i)^{-8}} \quad (\text{B.2})$$

The total number of arrears in the municipality is designated by A . The interest rate that the Spanish government charges municipalities is represented by i , which by March 2012 was the interest rate of the Spanish 10 year treasury bond (5.17%) plus 142 basis points. Municipalities follow a French scheme to pay debt back. Moreover, if they present an adjustment plan, they have a two years grace period paying only interests. The discount rate of municipalities is represented by r .

In this context, it is reasonable to set the discount rate equal to the interest rate each municipality pays for debt in the market. In practice this rate is often unavailable as by 2012, before the SPP, most municipalities had no access to credit markets, and those who did have access, had it through the banking system. We decide to use the yield paid by Madrid's long term bonds as a very-conservative discount factor for all municipalities. This yield was 7.5% on April 2011, which represented 220 basis points relative to the 10 year Spanish treasury yield.³² We argue that this can be used to calculate a lower bound for the difference between

³²We take the average yield for April 2011.

the NPV of presenting and not presenting an adjustment plan.

Considering that the interest rate (i) that the central government is charging municipalities is subsidized ($i < r$), the difference between the NPV of presenting a plan minus the NPV of not doing so should be higher the higher is the discount rate (r). Madrid is the capital of Spain, and along with Barcelona, the only municipality that had access to capital markets. There are probably many reasons for this. One is that they are the two most dynamic municipalities in Spain. Another, that when it comes to borrowing, they arguably minimize informational asymmetries for private lenders, as they are under high levels of scrutiny. The interest rate that capital markets charge on debt is typically smaller than the rate charged by the banking system. Moreover, the risk premium in April 2011, the last month with market data for Madrid's debt, was smaller than in March 2012, when the debt crisis in Europe had worsen. Hence, after talking to different experts in the financing industry, we came to the conclusion that it is safe to take this yield as a lower bound for the financing cost of most municipalities before the plan was deployed in April 2012.³³

This leaves us with an interest rate payed to the government of 5.17% plus 142 basis points, and a discount rate of 5.17% plus 220 basis points, which allows us to estimate a lower bound for the net present value of the gains of presenting an adjustment plan.³⁴ To make figures easier to interpret, Table B.1 shows the distribution of these gains for all municipalities, for those who presented a plan, and for those who did not.

The average gain of presenting a plan was roughly 7€ per capita. To put this into context, the average municipal deficit in per capita terms in 2011 was 17.7€ per capita. This means that a very conservative estimate of the NPV gain for presenting an adjustment plan represents 40% of the deficit in 2011—one of the worst years for Spanish public finances in the last three decades. This reinforces the notion that continuing mayors in municipalities with arrears inflicted a sizable cost to their citizens, getting advantage of their private information to pursue spurious interests.

Notably, although taking the advantage of the 10 years to pay back was the best alternative, those municipalities that presented an adjustment plan could also pay their debt back sooner. Hence, the desire to cancel the debt more quickly would not justify not presenting an adjustment plan. Similarly, municipalities did not need to invest substantial resources in the elaboration of the plan. On the contrary, most of them were typically elaborated within two weeks, and municipalities could count on the *interventor*, a municipal officer who's salary

³³We use Madrid instead of Barcelona because its debt has the closest market data to April 2012. It is worth noting that the yield of Madrid and Barcelona bonds are typically close.

³⁴5.17% was the Spanish 10y Treasury yield in April 2012.

is paid by the central government, to get any technical assistance they needed. Therefore, only for municipalities with very low levels of arrears it would not pay off to present a plan. Along these lines, we find that those newcomers that do not present a plan have typically few arrears—on average almost five times less arrears than those newcomers that do present it.

TABLE B.1
NPV DIFFERENCE OF PRESENTING VS. NOT PRESENTING AN ADJUSTMENT PLAN

	min	max	mean	sd
All Municipalities	0.00	294.51	7.04	10.53
Mun. with no Adj. Plan	0.00	294.51	6.86	13.45
Mun. with Adj. Plan	0.08	92.72	7.16	7.95

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.

Online Appendix C. Factiva Database

Factiva is an international news database produced by Dow Jones. It combines over 30,000 sources from over 200 countries and it aims to cover the universe of news outlets worldwide. In Spain, Factiva provides access to more than 6 million articles every year in more than 200 Spanish national, regional and local newspapers and magazines. We exploit two different features of this database.

Online Appendix C.1. The Universe of News in Factiva

We are granted access to an online version of the entire Factiva database containing the universe of news published in Spain through the ElasticSearch Platform. Using this platform, we count the monthly number of news that mention the “Suppliers Payment Program” along with the name of a municipality, as well as the number of times this municipality appears in the news. We do the search both in Catalan and Spanish for the period 2011-2013.³⁵ For both searches, we omit articles tagged in the “Sports” and “Lottery” categories. We use this information to construct Panel A of Figure 3, which depicts the frequency of appearance of the SPP in the news, Table A.7, which analyzes its coverage through regression analysis, and Figure 6 and appendix table A.8, which analyzes the causal relationship between a change in office and the coverage of the SPP. A detailed analysis of these results can be found in Section 4.1.

Online Appendix C.2. ChatGPT Analysis on SPP-related News

We download a subset of Factiva universe of news to analyze its content with ChatGPT. We first access all articles fulfilling the following two criteria:

1. News published in Spain between 2011 and the end of 2013.
2. News containing references to any Spanish local government along with references to the SPP or to an adjustment plan. To that end, we applied the following filters (in Spanish):
 - Terms related to local governments: "munici*" or "ayuntamiento" or “alcalde*”.³⁶
 - Terms related to the SPP or any fiscal consolidation program: "plan de pagos" or "plan de proveedores" or “mecanismo de pago” or “plan de pago a proveedores” or “programa de consolidación fiscal” or “plan de consolidación fiscal” or "plan de ajuste".

³⁵Navarra and the Basque Country, the only two Autonomous Communities where Basque is an official language, do not participate in the SPP (see section 2.1).

³⁶The asterisk (*) is used to keep the root of a word and allows for any ending of the word.

We download the associated news that will be defined as our initial SPP-related news sample. This includes 13,954 articles published in 2012 after the introduction of SPP and 7,673 in 2013, which constitute the subsample of news that we process using ChatGPT3.5. We use this tool to determine whether these articles are talking about any particular municipality, to uncover if there are criticisms to the municipal government, to explore who issues the criticism, and to identify to whom it is addressed. This procedure reduces the sample approximately by half. We use this information in Tables 3 and A.9 and Figures 7 and A.5, which explore the relationship between the content of the news covering the SPP and tenure in office, as well as in Table 4, which investigates how presenting an adjustment plan changes the narrative depending on past government’s responsibilities. A detailed analysis of these results can be found in Section 4.2.

For each news article, we asked GPT the following five questions using the Api of OpenAI:

1. List all the names of municipalities mentioned in this article.
2. How many municipalities are mentioned?
3. Is the municipal management criticized in the article?
4. If there are any criticisms, who issues them?
5. If there are any criticisms, towards whom are they directed?

In practice, we gave GPT a list of possible answers for each question (see details below). Forcing GPT to a limited set of answers instead of using open ended questions simplifies the regression analysis, allowing us to work with categorical variables. We label each category with a number (e.g., *if the answer is yes, respond "1", if the answer is no, respond "2"*) after ensuring that it helps GPT to stick to the answer list.

The code we use to connect to the OpenAI API is written in Python. We loop through the subsample of news. The full text of our query is then tokenized by the model and used to provide a response. As OpenAI has a hard limit on the number of tokens, we skip news that exceed 4000 expected tokens. We use GPT-3.5 as our model of choice and set the temperature or “creativity parameter” of the model to zero.

We rely on a companion methodological work published in Scientific Reports (Bermejo et al., 2025) to validate GPT-3.5 measures. Bermejo et al. (2025) evaluates the same GPT-based coding strategy on the same universe of Spanish-language press articles on the Suppliers Payment Program. It draws a validation sample of approximately 1% of the full corpus used in the current paper and constructs gold-standard labels through a careful author-led process: each article is independently read and annotated by at least two of the authors of the paper, and all disagreements are subsequently discussed and resolved jointly. The per-

formance of GPT-3.5 (together with other large language models) is benchmarked against these gold standards and against outsourced human coders (educated, context-aware university students). Across all tasks, the paper shows that LLMs consistently outperform outsourced coders and align closely with the gold-standard annotations.

The validation further examines several potential threats to measurement validity. First, it shows that while performance deteriorates for longer and more complex articles, LLMs continue to perform substantially better than outsourced human coders in these cases. Second, the results are shown to be robust to alternative prompt formulations. Third, the analysis finds no evidence of political bias in LLM outputs. Finally, the paper documents high consistency across repeated runs of the same model—although comparisons of GPT-3.5 outputs obtained in 2023, 2024, and 2025 reveal modest differences over time, consistent with recent evidence on temporal instability in LLM behavior (Chen, Zaharia and Zou, 2024). Taken together, these validation exercises provide external support for the reliability of the GPT-3.5–based measures used in this paper.

Bellow we provide the entire original text provided to ChatGPT (in Spanish):

Usted es un asistente de investigación.

Su trabajo es leer una noticia y responder preguntas sobre su contenido.

Dé su respuesta en un diccionario de Python estructurado de la siguiente manera: Q1: respuesta a la pregunta 1, Q2: respuesta a la pregunta 2, ...

No agregue ninguna explicación adicional a su respuesta.

No deje ninguna pregunta sin responder.

<News> Here the news text is inserted </News>

- *Q1: Enumere todos los nombres de municipios mencionados en esta noticia separados por una coma. Si en la noticia no se menciona ningún municipio, responda con "0". Si no está seguro, responda con "99".*
- *Q2: ¿Cuántos nombres de municipios están mencionados en la noticia? Si no esta seguro, responda con "99".*
- *Q3: En esta noticia, ¿se critica la gestión municipal? Si la respuesta es afirmativa, responda con "1". Si la respuesta es negativa, responda con "0". Si no esta seguro, responda con "99".*
- *Q4: En esta noticia, ¿quién emite la crítica? Por favor, seleccione una de las siguientes alternativas: Si la critica fue hecha por un concejal de la oposición, responda con "1". Si la crítica fue hecha por un concejal del partido del gobierno, responde con "2". Si la*

crítica fue hecha por el alcalde, responda con "3". Si la crítica fue hecha por el Partido Popular (PP), responda con "4". Si la crítica fue hecha por el Partido Socialista (PSOE), responda con "5". Si en la noticia no hay críticas a la gestión municipal, responda con "0". Si su respuesta no encaja en ninguna de las categorías anteriores, responda "98". Si no está seguro, responda con "99".

- *Q5: En esta noticia, ¿a quién van dirigidas las críticas? Por favor, seleccione una de las siguientes alternativas: Si se critica al gobierno municipal actual, responda con "1". Si se critica al gobierno municipal anterior, responda con "2". Si se critica al gobierno nacional, responda con "3". Si se critica a la oposición municipal, responda con "4". Si se critica al Partido Popular (PP), responda con "5". Si se critica al Partido Socialista (PSOE), responda con "6". Si no hay críticas, responda con "0". Si su respuesta no encaja en ninguna de las categorías anteriores, responda "98". Si no está seguro, responda con "99".*

Online Appendix C.3. Examples of SPP news in Spanish newspapers

We provide two examples of articles of the newcomer blaming the previous mayor for the need to present an adjustment plan. The first article corresponds to a PP newcomer blaming the previous mayor from PSOE, and the second, a PSOE newcomer blaming the previous mayor from PP. We include both the Spanish original article as well as their English translation.

FIGURE C.1
NEWSPAPER EXTRACT - PP NEWCOMER - 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

CY © VOCENTO. Todos los derechos reservados

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 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.

FIGURE C.2
NEWSPAPER EXTRACT - PP NEWCOMER - ENGLISH TRANSLATION

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» consistency 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 man, 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.

FIGURE C.3
NEWSPAPER EXTRACT - PSOE NEWCOMER - ORIGINAL SPANISH

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
CY Copyright 2012 Las Provincias

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 - 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

CY Copyright 2012 Las Provincias

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.

Online Appendix D. Cross-Country Analysis

In this appendix, we describe the data sources used in the cross-country analysis and provide descriptive statistics ([Online Appendix D.1](#)), examine the relationship between government turnover and IMF bailouts through three additional empirical analyses ([Online Appendix D.2](#), [Online Appendix D.3](#), [Online Appendix D.4](#)), and present three illustrative examples consistent with our electoral incentives argument ([Online Appendix D.5](#)).

Online Appendix D.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. IMF programs that do not entail financial assistance, namely Policy Support Instruments and Policy Coordination Instruments, are excluded from the analyses. Last, we obtain macroeconomic data from the World Economic Outlook Database and the World Development Indicators Database, which include a time series of different macroeconomic indicators.

Descriptive statistics for the country-panel are provided in [Table D.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 D.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).

TABLE D.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 per Capita	11.04 (15.83)	5.17 (8.40)	24.24 (20.13)
GDP Level (Average)	282.71 (751.06)	102.21 (264.13)	687.48 (1,198.58)
GDP Growth (3y MA)	7.08 (8.98)	7.94 (9.62)	5.19 (7.02)
Inflation	9.79 (25.89)	11.82 (28.59)	5.30 (17.75)
Reserves to Imports	0.34 (0.26)	0.35 (0.22)	0.33 (0.32)
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)

Notes: Descriptives of the country panel used in the international analysis in Section 6. 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.

Online Appendix D.2. Banking Crises

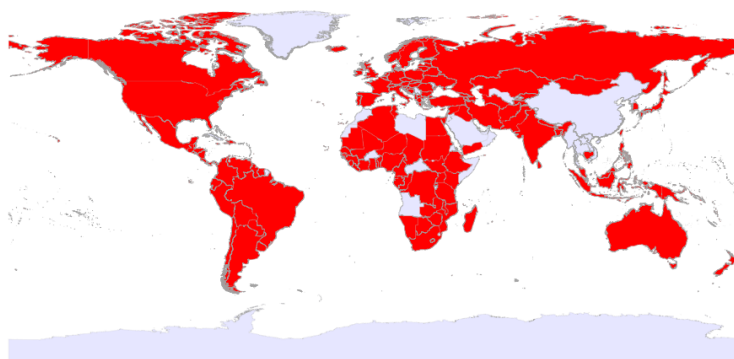
In this subsection of the Appendix, we conduct a complementary analysis restricting our attention to countries undergoing a banking crisis, as recorded in the database in [Laeven and Valencia \(2020\)](#). This seeks to attenuate the concern that unobserved external economic shocks explain both the recent change in office and the decision to request assistance to the IMF. Unsurprisingly, the average probability of an external bailout in this subsample—where all countries are under financial distress—is much higher than in the full sample (48.4%).

Using our restricted sample, we estimate:

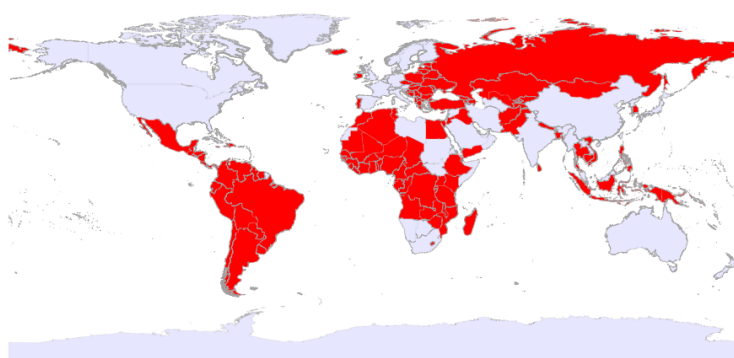
$$Program_{ck}^{IMF} = \beta C_{ck} + \gamma_1 \Delta Controls_{ck} + u_{ck} \quad (D.1)$$

where $Program_{ck}^{IMF}$ takes value 1 if country c received external financing from the IMF during crisis k , and value zero otherwise (notice that some countries suffer more than one

FIGURE D.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.

banking crisis over the sample period).³⁷

Estimates of the key parameters of the specification in equation [D.1](#), estimated using the subsample of banking crises are reported in [Table D.2](#). We find that the probability to agree with the IMF after a change in office is roughly 30% higher. This reinforces the notion of a causal link between government turnover and receiving assistance from the IMF. Anecdotally, the estimated coefficient is remarkably close to the main effect we estimate with the Spanish municipal data (see [Section 3](#)).

Online Appendix D.3. Political Turnover and the Timing of IMF Programs

In this subsection of the Appendix, we examine how the probability of signing a new IMF agreement evolves over the electoral term, distinguishing between newcomers and continuing incumbents. [Figure D.2](#) reports the average probability that a country signs a new IMF

³⁷Due to the limited size of this sample, these regressions do not include country or year dummies.

TABLE D.2
THE IMPACT OF PARTY CHANGES ON IMF PROGRAM IMPLEMENTATIONS DURING BANKING CRISES

VARIABLES	(1) IMF Program	(2) IMF Program	(3) IMF Program
Party Change	0.306** (0.142)	0.289** (0.129)	0.224* (0.132)
GDP Growth (3y MA)		-0.019** (0.008)	-0.018** (0.008)
Observations	53	51	49
Controls	No	No	Yes

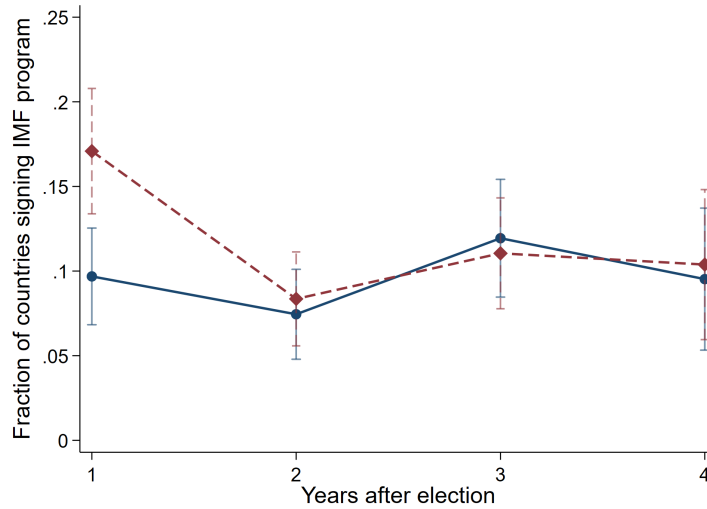
Notes: This table reports OLS estimates. IMF Program is a dummy variable that takes a value of 1 if the country puts an IMF program in place due to the analyzed crisis, and 0 otherwise. Party Change takes a value of 1 if the country is ruled by a new party, and a value of 0 if the country is ruled by the previous incumbent party. In column 2 we control for the three-year moving average of the growth rate of national GDP. Column (3) controls for the three-year moving average of national GDP growth; lagged GDP per capita and lagged total GDP (both measured in U.S. dollars); lagged consumer price inflation; the lagged ratio of international reserves to imports; and two political party dummies equal to one if the chief executive’s political party is right- or left-leaning. 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.

assistance agreement around elections, separating cases of political turnover from those in which the incumbent is re-elected. The probability rises sharply in the first post-election year following turnover, while remaining substantially lower after elections that return the incumbent to office. The gap is largest in year 1 and gradually narrows thereafter. This pattern is consistent with a blame-and-attribution mechanism: newly elected governments inherit prevailing economic conditions without bearing direct responsibility for past policies and therefore face lower reputational costs when requesting IMF assistance early in their tenure. Re-elected incumbents, by contrast, have stronger incentives to avoid such requests, as these may signal past policy failure. As time passes and responsibility becomes less clearly attributable, the difference in probabilities dissipates.

Online Appendix D.4. Political Turnover and IMF Program Types

In this subsection of the Appendix, we explore heterogeneity across types of IMF programs. Following [Barro and Lee \(2005\)](#), we distinguish between non-concessional lending and adjustment programs—namely, Stand-By Arrangements (SBA) and Extended Fund Facility (EFF) programs—and concessional facilities targeted at low-income countries (SAF, ESAF, and PRGF). As emphasized by [Barro and Lee \(2005\)](#), concessional facilities feature highly subsidized interest rates, long maturities, and extended grace periods, and are therefore more appropriately interpreted as a form of foreign aid rather than standard bailout programs. These institutional differences imply substantially different political and informational content. Consistent with our mechanism, Table D.3 shows that political turnover

FIGURE D.2
IMF PROGRAM ENTRY IN YEARS FOLLOWING ELECTIONS



Notes: This table reports average probabilities of signing a new IMF program in year s relative to the election year, separately for elections that result in political turnover (red dashed) and for those that do not (blue solid). IMF Program is a dummy variable that takes a value of one if a country signs a new IMF assistance program in a given year, and zero otherwise. Party Change takes a value of one if the election results in a change in the governing party and zero otherwise. The figure reports 95% confidence intervals.

is associated with IMF participation primarily through non-concessional facilities (SBA and EFF programs), which are closely linked to acute macroeconomic distress and carry greater political stigma. By contrast, we find no statistically significant effects for concessional facilities. Given that these programs are commonly interpreted as development aid—more routine and politically normalized—this pattern aligns with our mechanism.

Online Appendix D.5. Anecdotal Evidence: Three Illustrative Examples

Finally, this subsection of the Appendix presents three illustrative cases that highlight the logic of our argument. When a challenger inherits a severe crisis, requesting external support can be framed as a corrective measure and combined with explicit blame of the previous administration. Carlos Menem was elected president of Argentina in 1989 amid hyperinflation and a collapsed fiscal position. He repeatedly portrayed the economy as ruined by his predecessors—who belonged to the other main Argentine party—and his government quickly negotiated successive Stand-By Arrangements and an Extended Fund Facility with the IMF. In 1995, Menem was re-elected with wide popular support. By contrast, when a re-elected incumbent requests a bailout, the same action can be interpreted as an admission of past policy failure. In Hungary, Ferenc Gyurcsány won re-election in 2006 after a first term marked by large fiscal deficits. Following the 2008 crisis, his cabinet was the first in the EU

TABLE D.3
POLITICAL TURNOVER AND IMF PROGRAMS TYPES

Panel A: Concessional IMF Programs (PRGT)				
	(1)	(2)	(3)	(4)
	IMF Program	IMF Program	IMF Program	IMF Program
Party Change	0.008 (0.007)	0.007 (0.006)	0.004 (0.006)	0.004 (0.006)
Observations	3,730	3,730	3,045	3,044
Mean of dep. var.	.05	.05	.04	.04
Country & Year FE	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
GFE × Year	No	No	No	Yes

Panel B: Non-Concessional IMF Programs (GRA)				
	(1)	(2)	(3)	(4)
	IMF Program	IMF Program	IMF Program	IMF Program
Party Change	0.044*** (0.010)	0.029*** (0.008)	0.030*** (0.010)	0.026** (0.010)
Observations	3,730	3,730	3,045	3,044
Mean of dep. var.	.06	.06	.06	.06
Country & Year FE	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
GFE × Year	No	No	No	Yes

Notes: This table reports OLS estimates of the effect of political turnover on IMF program participation by program type. The dependent variable is a dummy variable that takes a value of 1 if the country signs an IMF funding program in that year, and 0 otherwise. Panel A focuses on concessional IMF facilities under the Poverty Reduction and Growth Trust (PRGT). Panel B focuses on non-concessional IMF programs under the General Resources Account (GRA), including Stand-By Arrangements and Extended Fund Facility programs. Party Change takes a value of 1 if the country is ruled by a party that came to power in the last national election, and a value of 0 if the country is ruled by the previous incumbent party. Columns (2) and (3) include country and year fixed effects. Column (3) additionally controls for the three-year moving average of national GDP growth; lagged GDP per capita and lagged total GDP (both measured in U.S. dollars); lagged consumer price inflation; the lagged ratio of international reserves to imports; and two political party dummies equal to one if the chief executive's political party is right- or left-leaning. 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.

to seek an IMF-EU-World Bank package, a decision the opposition used to emphasize his responsibility for the country's economic difficulties. This episode preceded his resignation in 2009 and the subsequent collapse of his party in the 2010 election. Finally, incumbents facing acute financial distress may sometimes avoid requesting assistance precisely to escape that dynamic. During the Asian financial crisis, Malaysia's prime minister Mahathir Mohamad, in office since 1981 and re-elected in 1995, refused IMF support even as neighboring countries entered Fund programs, instead imposing capital controls and publicly blaming in-

ternational currency speculators for the turmoil. Notably, this strategy appeared electorally advantageous, as he secured re-election in 1999.