Essays on Public Policy Evaluation:

The Case of Spain's Supplier Payment Plan

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I certify that all material in this thesis which is not my own work has been identified and that any material that has previously been submitted and approved for the award of a degree by this or any other University has been acknowledged.

Abstract

The 2008-14 economic crisis significantly worsened Spain's public finances, leading to increased budget deficits and public debt, as well as longer payment periods to vendors. As a result, unpaid invoices by subnational governments peaked in December 2011 at €28.5 billion (2.7% of GDP). In response, the central government launched the Supplier Payment Plan (SPP) in 2012 to address these overdue payments.

This dissertation evaluates the SPP, focusing on the causes of government arrears, the impact of delayed payments on corporations and procurement, and the role of politicians' incentives in bailout agreements.

First, we find that the accumulation of arrears by Spanish municipalities through 2011 (intensive margin) was negatively influenced by construction activity, and positively influenced by current spending and interest payments. Additionally, we also find that construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin. Finally, while our analysis indicates a positive relationship between arrears and the level of interest payments, it also reveals a negative relationship with their deviation from the within-group mean. This suggests municipalities with high interest payments, indicative of credit constraints, likely delayed payments to suppliers as a coping mechanism for limited market or bank credit access.

Second, we examine the impact of government late payments on firms, using the SPP as a natural experiment. We compare firms included in the plan's first phase with those omitted but repaid later, finding that repayment boosts corporate investment, decreases leverage, and enhances cash reserves. Financially constrained firms increase investment and reduce payables,

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benefiting the supply chain, while less constrained firms repay debt. Additionally, we also show how the buildup of arrears impairs procurement relationships, which are only restored upon repayment. Our findings underscore the adverse effects of procurement arrears as well as the efficacy of an unconventional fiscal policy with large real effects like the SPP.

Third, we hypothesize that borrowing from a lender of last resort reveals negative information about a government's past economic performance, making officials with previous government responsibilities more reluctant to request financial assistance. We analyze the decisions made by around 4,000 Spanish municipalities under the SPP, find that newly elected local governments are 30 percentage points more likely than re-elected incumbents to publicly agree on a bailout from the central government. Evidence from news content analyzed using ChatGPT and politician surveys suggest incumbents avoid bailouts to protect their reputations, despite this being suboptimal for citizens.

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Author's declaration

All chapters presented in this thesis were written by José Abad, under the supervision of Vicente Bermejo, Christina Dargenidou, and Pedro-Ángel García-Ares.

The viewpoints articulated in this thesis are exclusively those of the lead author and his co-authors, and should not be imputed to any institutions with which the lead author is or has previously been affiliated with, including the Instituto de Crédito Oficial (ICO), the International Monetary Fund (IMF), or the World Bank Group (WBG).

Chapter 4: Government Arrears and Corporate Decisions: Lessons from a Natural Experiment

José Abad, Vicente Bermejo, Vicente Cuñat, and Rafael Zambrana.

All authors contributed to the analysis design and execution, data interpretation, and manuscript composition. José Abad's additional contributions encompassed idea conceptualization, dataset creation, literature review, institutional setting description, and the design and execution of the financing frictions empirical analysis.

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Chapter 5: Government Turnover and External Financial Assistance

José Abad, Vicente Bermejo, Felipe Carozzi, and Andrés Gago.

All authors contributed to the analysis design and execution, data interpretation, and manuscript composition. José Abad's additional contributions encompassed idea conceptualization, dataset creation, literature review, institutional setting description, and the design and execution of the cross-country empirical analysis.

Chapter 1. Extended Introduction

1.1. Aims, Objectives, and Results

The economic crisis from 2008 to 2014 severely impacted Spain's public finances, leading to a noticeable rise in both the budget deficit and public debt. For a detailed examination of the crisis, see Banco de España (2017). This deterioration in public finances resulted in substantially longer payment periods by Spain's regional and local governments to their vendors. At the municipal level, the aggregate liabilities, including overdue payments (arrears), escalated from 1.5% of GDP in 2005 to 2.6% of GDP in 2011, marking the highest level since 1995. This increase of 1.1 percentage points in overall liabilities during the period predominantly stemmed from a substantial accumulation of overdue payments (arrears). By December 2011, Spanish municipalities had amassed more than 1.8 million unpaid invoices, totaling €9.6 billion (representing 0.9% of GDP) owed to over 115,000 suppliers (Ministerio de Hacienda y Administraciones Públicas, 2013).

In response to the precarious financial state of Spain's regional and local governments and their detrimental effects on businesses and the broader employment situation, the central government established post the November 2011 general elections adopted several urgent fiscal measures in early 2012. Key among these was the Supplier Payment Plan (SPP), designed to settle the backlog of payments owed by these governments up to December 2011. To achieve this, the central government assumed the debts of the subnational entities, clearing their overdue payments through three separate bank transfers in the summer of 2012 (May, July, and September). The SPP was characterized

by two essential aspects significant for our study: Firstly, the program's mandatory inclusion of approximately 7,500 local governments from the fifteen regions under a shared financing system, excluding those from the Basque Country and Navarre. Secondly, a condition for participation was the submission and central government Finance Ministry's approval of a multi-year fiscal consolidation plan.

This dissertation is composed of three empirical studies, each scrutinizing a distinct facet of the SPP. The inaugural essay (Chapter 3) delves into the causation of government expenditure arrears. The subsequent analysis (Chapter 4) seeks to understand the real effects of delayed government payments on corporate decisions as well as on procurement. Finally, the concluding essay (Chapter 5) examines how incentives may impact the probability for a government to reach a bailout agreement with a third-party institution.

1.1.1. Causation of Government Expenditure Arrears

Chapter 3 studies the causation of government expenditure arrears.

The accumulation of government arrears is of significant policy interest due to its major economic consequences. Firstly, it exerts detrimental long-term impacts by diminishing corporate earnings, heightening the risk of corporate failures, and impeding overall economic growth (Checherita-Westphal, Klemm, & Viefers, 2016; Connell, 2014). Additionally, arrears negatively influence bank credit quality, corporate tax compliance, and the efficiency of public services (IMF, 2019a). Secondly, government arrears skew conventional fiscal stance indicators, undermining the government's capacity to deploy fiscal policy as a tool to stimulate economic activity (IMF, 2019a) and causing delays in necessary

fiscal adjustments (Diamond & Schiller, 1991). In this context, Milesi-Ferretti (2004) shows that non-transparent budgeting practices lead to more creative accounting and less fiscal adjustment. These are reasons why managing arrears is a central objective in virtually all IMF-supported programs (Flynn & Pessoa, 2014).

We provide suggestive evidence that the extent and probability of arrears accumulation by Spanish municipalities through 2011 were positively influenced by construction-related unemployment (serving as a gauge for construction activity and an early indicator of the 2008-14 crisis in Spain; see Banco de España, 2017, and IMF, 2012), current spending (in line with available cross-country evidence suggesting that the bulk of government arrears typically centers around obligations to suppliers; see Lacey, Massad, Mastruzzi, & Utz, 2022), and interest payments (a commonly accepted indicator of sub-sovereign creditworthiness; see Liu & Tan, 2009). Additionally, we also find that construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin (how much arrears municipalities accumulate). Finally, while our analysis indicates a positive relationship between arrears and the level of interest payments, it also reveals a negative relationship with their deviation from the within-group mean. This suggests that municipalities with elevated interest payments are more likely to experience a significant reduction in market access or access to bank credit relative to their peers. Interpreting higher interest payments as indicative of credit constraints, this finding implies that credit-constrained municipalities may compensate for reduced market access by delaying payments to their suppliers, thereby accumulating arrears.

1.1.2. Real Effects of Delayed Government Payments on Corporate Decisions and Procurement

Chapter 4 aims to understand the real effects of delayed government payments on corporate decisions as well as on procurement.

Central and subnational governments play a crucial role as key clients for numerous small local enterprises, offering a more consistent demand compared to private sector customers, particularly during economic downturns (Goldman, 2020). However, governments often delay payments, resulting in the accumulation of arrears. Although the economics of procurement have been extensively studied, the financial effects of the relationship between suppliers and government entities, particularly regarding the repercussions of repaying accumulated arrears, remain underexplored. In a frictionless financial market, companies should have the capacity to secure loans by pledging their outstanding receivables from the government (including those in arrears) as collateral, implying that the timing of repayments would not alter corporate actions. Nevertheless, financial frictions might alter corporate plans, making delays in settling government debts costly. Therefore, the degree to which unpaid government debts impose costs on businesses is a relevant empirical question, closely associated with the existence of financial constraints.

Initially, we find that the SPP had disparate impacts on the investment, debt, and liquidity strategies of Phase I and Phase II firms. Specifically, an unexpected liquidity boost amounting to over 10% of a company's assets led to a 14% increase in investment, a 10% decrease in leverage, and a 44% increase in cash reserves. These findings are both statistically and economically significant, corresponding to approximately 30%, 20%, and 40% of the standard deviation in investment, leverage, and liquidity growth among the sampled firms,

respectively. Secondly, we observe a heterogeneous response among firms, indicating that the impact differs based on a firm's borrowing capacity. Firms with financial constraints ramped up their investments and decreased their outstanding debts (payables) to suppliers following the liquidity boost. Thus, substantial governmental arrears can indeed impose costs on financially constrained firms and generate ripple effects throughout the corporate supply chain. Lastly, we also show that firms tend to reduce their business with the public sector when confronted with significant government arrears.

1.1.3. Incentives to Reach a Bailout Agreement with a Third-Party Institution.

*Chapter 5 **explores how incentives may impact the probability for a government to reach a bailout agreement with a third-party institution.***

From the standpoint of a government under financial distress, the choice to seek external help is shaped by its political incentives and constraints. Securing a bailout agreement can provide them with resources to bolster their fiscal capacity. Nevertheless, such agreements also affect how voters perceive the true state of public finances. We hypothesize that, in a context of asymmetric information, government officials will have different incentives to request assistance depending on their tenure in office. Incumbent governments, accountable for their prior economic policies, might opt to (avoid any bailout and) cope with stricter borrowing limits to protect their information rents. Conversely, new administrations, which inherit an adverse fiscal situation without revealing their own fiscal management skills, may be inclined to pursue assistance to expand their budgetary scope while underscoring the fiscal difficulties inherited.

We find that, in the context of the SPP in 2012, newly elected local governments in Spain were 30 percentage points more inclined to submit a fiscal adjustment plan to the central government compared to those that were re-elected. This disparity is particularly striking given that choosing a backloaded adjustment was economically superior under those circumstances. The evidence supports the role of incumbents' private information in explaining these results. Broadly, our results underscore the impact of political considerations on attaining fiscal stability and contribute to the design of incentive-compatible bailout programs.

1.2. Research Methodology

1.2.1. Instrumental Variables (IV) and Two-Step System Generalized Method of Moments (GMM).

[Chapter 3](#) uses both an instrumental-variables (IV) and a two-step generalized method of moments (GMM) approach to study the causation of government arrears.

Several characteristics make the SPP particularly attractive for study. Primarily, it enables the utilization of actual arrears data, presenting a significant advantage over the majority of existing empirical studies that rely on proxies, with the notable exception of IMF (2019a, 2019b), which employs survey data. Additionally, the aggregate amount of arrears as of December 2011 was large enough on an absolute basis as well as relative to history to plausibly infer a detectable effect from some of the key potential determinants suggested in the literature. Concentrating on a single country helps reduce the potential for confounding variables that are unique to specific nations. Moreover, the panel's

large cross-sectional dimension provides the opportunity for greater spatial variation than over time, with variation that is more plausibly exogenous, in line with the literature on subnational fiscal spending multipliers (for a review, see Chodorow-Reich, 2019). Such variation enables the application of a panel econometric strategy.

Initially, we delve into the factors contributing to cross-sectional differences in the level of government arrears. Utilizing a dataset that encompasses over 3,800 municipalities (all of which had outstanding arrears at the end of 2011) spanning seven years (2005-11), we employ a pooled ordinary least squares (POLS) methodology to regress a vector of our variables of interest, lagged by one period to mitigate potential endogeneity issues, on the contemporaneous per capita flow of arrears in each municipality. We add a comprehensive set of controls for municipality-specific economic factors – including labor, fiscal, social, and demographic variables – most of which are also lagged by one period. Additionally, we also add province and time fixed effects to account for, respectively, province-level characteristics and global time trends. Our preliminary findings indicate that while the coefficients for construction-related unemployment are weakly significant, those for current spending and interest payments are highly significant.

Building on our initial examination, we further investigate whether there are variations in the factors influencing the likelihood of arrears accumulation—that is, whether certain municipalities accrue arrears, regardless of their magnitude, while others do not. Utilizing a dataset covering over 7,500 municipalities (excluding those in the Basque Country and Navarre) over a span of seven years (2005-11), we adapt our pooled ordinary least squares (POLS) specification by replacing our dependent variable with a dummy variable that takes a value of one

(zero) if a municipality does (not) incur arrears in any given year. Due to the dichotomous nature of our dependent variable, this modified approach is more accurately referred to as a Linear Probability Model (LPM). The findings reveal that the coefficients for our three primary variables of interest are highly significant.

Despite a combination of lags and controls in our previous two specifications, a potential endogeneity issue persists: Our variables of interest could still be influenced by the arrears themselves. Notably, there is substantial empirical evidence indicating that the accumulation of arrears can exert significant and negative wide-ranging economic consequences, as discussed before. In the presence of simultaneous correlation, the pooled ordinary least squares (POLS) estimator will be both biased and inconsistent.

To address this problem, we follow an instrumental variables (IV) approach, employing a two-stage least squares (2SLS) regression with time (year) and province-level fixed effects. We use the first lags of our variables of interest as instruments, following Anderson & Hsiao (1981) and Todd & Wolpin (2003). This approach aims to mitigate simultaneity concerns by ensuring the regressors are predetermined in an earlier period. Nonetheless, this method introduces a trade-off by potentially reducing the instruments' relevance. Our findings reveal two notable differences compared to the results from our POLS regressions. Firstly, the coefficients estimated via the 2SLS approach are significantly larger, likely due to an omitted variable negatively correlating with our variables of interest or measurement errors (for instance, construction-related unemployment as an imprecise indicator of overall construction activity), causing a downward bias in the POLS estimates. Secondly, while the construction variable loses significance in the continuous-form specification, albeit with a point

estimate of similar magnitude, the binary-form specification maintains the high significance of all three variables of interest. In both cases, the significance and size of the coefficients in the first-stage regression satisfy the instruments' relevance condition.

Collectively, these findings provide suggestive evidence that our variables of interest significantly influence both the level and the likelihood of arrears. To add robustness to our main results, we perform several checks.

Initially, we explore alternative regressors akin to two of our primary variables (employment and financial debt, paralleling construction-related unemployment and interest payments, respectively), albeit with a notable reduction in sample size for the latter. The results remain broadly similar in both cases. Secondly, we augment our control variables at the municipal level to include a subset of political factors, previously excluded to prevent significant sample reduction. This addition comprises dummy variables for the PSOE and PP, the major political parties in Spain at that time, and a variable for the mayor's age. We find no material differences in our results relative to those from our main specification. Thirdly, we evaluate the data for potential non-linear effects by conducting a logistic regression. Results confirm those from our main LPM estimation. Fourthly, we also test whether our results are robust to the use of further (second) lags as instruments in the 2SLS estimations. Results from the reduced-form regression confirm those from our main (continuous- and binary-form) specifications. Results from the first-stage regressions also suggest satisfaction of the instruments' relevant conditions, although the coefficients are lower than those of the first lags, as expected. Fifthly, we also experiment with a within fixed-effects estimator. Introducing municipality-level fixed effects controls for any time-invariant municipality-level characteristics. In turn, this eliminates

potential endogeneity concerns from omitted variables that are time invariant at the municipality level. We implement it by dropping all time-invariant controls as well as the province-specific fixed effects from our main specifications. With a within fixed-effects estimator of a model where the dependent variable is in continuous form, we find the coefficients of the three variables of interest to be all highly significant (just as in the case of our main POLS estimation). However, the coefficient for interest payments, indicative of a municipality's financing costs and borrowing capacity, turns negative (opposite to the positive sign found using our POLS estimator) in the continuous-form specification. In the binary model, all three variables of interest retain high significance and a positive sign, consistent with our main LPM estimation.

Integrating our pooled ordinary least squares (POLS) and within fixed-effects estimates for the continuous-form specification reveals that government arrears are positively influenced by the level of interest payments (as indicated by the POLS estimate) but negatively affected by their deviation from the average within their group (as shown by the within fixed-effects estimate). These findings represent two perspectives of the same phenomenon, suggesting that municipalities under financial constraints are prone to accruing greater arrears compared to those without such constraints. Indeed, municipalities facing financial constraints are likely to incur higher interest payments relative to other municipalities, while simultaneously experiencing diminished access to bank credit and capital markets.

To further address any potential endogeneity concerns, we also experiment with Blundell & Bond's (1998) system Generalized Method of Moments (GMM) estimator for dynamic panel data models, a method well-suited for our dataset with its short temporal span (seven years) but extensive cross-

sectional dimension (over 7,500 municipalities). To adjust for heteroskedasticity and autocorrelation within the error structure, we employ the consistent (two-step) estimator. We follow this approach to estimate the model's continuous-form specification, using collapsed second and subsequent lags as instruments. Lower lags do not resolve autocorrelation issues, as evidenced by the rejection of the AR(2) test's null hypothesis. The point estimates fall within the range delineated by the POLS and within fixed-effects estimations (Bond, 2002). Furthermore, the Hansen test's p-value also falls within the range suggested by Roodman (2009) as indicative of an absence of instrument proliferation (overfitting) concerns. Overall, results remain broadly consistent with those from the within fixed-effect estimator.

1.2.2. Difference-in-Differences (DiD)

Chapter 4 estimates the impact of the accelerated repayment of government arrears on corporate decisions in a difference-in-differences (DiD) setting.

Examining the repayment of arrears empirically faces significant hurdles, notably because the conditions under which arrears are settled are often dictated by the particular context of both the purchasing government entity and the supplying firm, thereby creating a standard endogeneity problem. Ultimately, numerous elements could prompt a municipality to prioritize payments to certain suppliers over others, such as the suppliers' size, the location of their headquarters, their industry, or even personal ties with the mayor or influential local council members. To circumvent this challenge, a viable strategy could

involve implementing a random acceleration in the settlement of government arrears for a subset of firms, offering an unexpected intervention.

Interestingly, the initial rollout of the SPP in 2012 inadvertently excluded a subset of firms, specifically those serving groups of municipalities (*mancomunidades*), from the first phase of repayments. These companies were subsequently incorporated into a revised plan (Phase 2) and compensated a year later, in 2013. The Phase 2 firms constitute an optimal control group due to their resemblance in characteristics and selection criteria to the firms involved in Phase 1. To accurately determine the effect of expedited repayment on government arrears, it is essential to compare a treatment group that undergoes an unforeseen repayment of these arrears with a control group that, despite having comparable outstanding arrears, does not receive repayment concurrently. This setup aims to approximate a perfectly randomized experiment by capitalizing on the seemingly random assignment of the SPP's execution phases.

In this context, the magnitude of our coefficients of interest should be interpreted as a lower bound of the actual impacts, accounting for the possibility that some firms in Phase 2 might have anticipated the 2013 repayment, thereby adjusting their behavior already in 2012.

Initially, we regress a dummy variable that takes a value of one (zero) for firms, unmatched, that participated in Phase 1 (2) multiplied by another dummy variable indexed from 2009 to 2012 that takes a value of zero (one) for each year prior (after) the index on our variables of interest (investment, leverage, and liquidity). We also add a set of fixed effects, including year fixed effects (to control for time-specific shocks and trends that may affect equally all firms within a

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particular year) as well as both industry and region fixed effects (to control for unobserved time-invariant heterogeneity that may systematically affect firms' corporate decisions). Finally, we also cluster standard errors by firm (to control for potential within-firm correlation or heteroscedasticity). Our baseline results show just weakly significant results for investment and liquidity (insignificant for leverage) in 2012.

Subsequently, we run the same regression after implementing an entropy-balancing matching procedure (Hainmueller, 2012). This method involves reweighting the two groups of firms (from Phases 1 and 2) based on the magnitude of the liquidity shock and their total assets in 2011 to enhance their comparability. Results remain quite similar.

By uniformly weighting all firms, regardless of the magnitude of their liquidity injection, the initial two methodologies yield merely an "overall effect" stemming from the repayment shock. Yet, the considerable variation in the shock's magnitude across firms could explain the moderately significant results observed in our primary analysis. Indeed, it is reasonable to anticipate that the timing of repayment, whether in 2012 or 2013, would (not) markedly influence the strategic decisions of firms possessing a substantial (minimal) amount of government arrears.

To address the variability in the liquidity shock's size, we categorize firms based on the amount of arrears repaid. Firms in Phase 1 are divided into four groups according to the liquidity shock relative to their total assets: below 1%, from 1% up to 5%, from 5% up to 10%, and above 10%. For investment, a clear, monotonic relation emerges, indicating that the greater the liquidity shock, the more significant the firm's investment response. This aligns with the hypothesis

that firms delayed investment activities due to late payments and increased them upon receiving the liquidity shock. Regarding leverage, the results suggest that firms used the funds from the unexpected liquidity injection to reduce it, but only when the liquidity shock was largest and government arrears were therefore substantial. Regarding liquidity, a positive, monotonic relationship is observed between the size of the liquidity injection and the increase in cash reserves. One plausible reason for the surge in cash reserves could be that firms experiencing delayed payments opted to maintain higher cash balances as a safeguard against financial distress and to enhance their agility in making future operational and strategic choices.

To add robustness to our main results, we employ a different empirical strategy by assessing the impact of accelerated repayment on corporate decisions using a difference-in-differences (DiD) approach. Instead of comparing firms in Phase 1 against those in Phase 2 across different years, we examine the corporate decisions of both groups before (2009-11) and after (2012) the liquidity shock. By comparing changes over time in the treatment group to changes in the control group, the DiD design helps to isolate the causal effect of the treatment (repayment shock) on the firm's corporate decisions. Furthermore, the DiD approach allows us to mitigate biases in the estimated treatment effect stemming from common firm trends. Consistent with this, we also replace industry with firm-level fixed effects. Results remain quite similar.

Fifthly, we develop a synthetic difference-in-differences (SDiD) approach, following the causal effect estimator for panel data outlined in Arkhangelsky, Athey, Hirshberg, Imbens, & Wager (2021). The SDiD approach constructs a synthetic control group that closely replicates the pre-treatment trend of the treatment group. Each treatment is replicated by re-weighting a sparse

combination of units from the control group, where more importance is given to observations that are closer in time to the treatment point. This technique proves especially advantageous in cases where the treatment effect is heterogeneous or when the assumption of parallel trends may not strictly hold. This is particularly relevant when the number of treated units is small, as with the firms in Phase 2. Results are very similar to those from our main specification. By confirming that the results also hold under the SDiD approach, we can be more confident that our results are not driven by any particular specification of the control group or any potential violation of the parallel trends assumption.

Collectively, these findings offer compelling evidence that larger liquidity shocks result in more pronounced adjustments in corporate strategies. Firms subjected to the most considerable shocks tend to increase their investments, reduce their debts, and build higher cash reserves.

In a frictionless financial market, firms should have the ability to secure loans by offering their overdue government invoices (including those in arrears) as collateral. Under such conditions, we would not expect to see an uptick in corporate investment following a government-provided liquidity boost. However, the observed increase in investments after the repayment shock indicates the presence of financial constraints, pointing to the existence of imperfect financial markets.

Sixth, we examine if the impact of the liquidity shock varies according to the extent of a firm's financial constraints. For this, we use a firm's pre-existing relationship with weak banks as a proxy for financial constraints, identifying weak banks as those with a core equity tier 1 (CET1) ratio below the Spanish banking average in the 2011 European Banking Authority (EBA) Stress Tests. Financially

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unconstrained firms are those associated with at least one strong bank, defined by an above-average stressed CET1 ratio, whereas financially constrained firms are those solely associated with one or more weak banks (and no single strong bank). We find that (i) only financially constrained firms markedly increased their investments following the 2012 liquidity injection; and that (ii) all firms, regardless of whether they were financially constrained or not, significantly decreased liabilities and increased cash reserves. This latter observation might reflect the context of 2012, during which Spain was deep in an economic crisis with limited investment opportunities. Hence, these findings should be considered a lower bound of the effect that a similar program might have on investment under circumstances of broader investment possibilities.

Seventh, the observation that all firms reduced their liabilities following the liquidity shock suggests that they had all accessed some form of temporary financing to bridge the funding shortfall caused by the increase in government arrears. To delve deeper, we re-analyze the data, this time separating firm liabilities into its two principal components: financial debt and accounts payable. Interestingly, we discover that financially constrained firms compensated for their liquidity needs by increasing their accounts payable (that is, by delaying payments to their own suppliers), whereas financially unconstrained firms resorted to increasing their financial debt (through bank loans or bond issuance). This indicates that the repercussions of delayed government payments can ripple through the entire supply chain, affecting not just the direct recipients but also their suppliers and beyond.

Finally, we explore the potential impact of government arrears on public procurement processes. For firms, contracting with the public sector can provide them with a reliable and significant source of income (Goldman, 2020). At the

same time, for the public sector, acquiring goods and services from private entities enables the efficient fulfillment of their public service obligations. In light of this, we investigate whether there is a noticeable difference in public contracting behavior between firms experiencing government arrears and those receiving timely payments. Utilizing the Opentender database, which aggregates data on governmental procurement activities, we match contract-level data (regarding volume and pricing) to a firm-municipality-year level. We then regress a dummy variable that takes a value of one for firm-municipality pairs where the municipality has arrears before 2012 to that firm (zero otherwise) multiplied by a dummy variable indexed from 2009 to 2012 that takes a value zero for each year before (one for each year after) the index, on a dependent variable that denotes whether the firm enters into a new public procurement contract with the municipality. Initially, we employ entropy-matching to match firms with and without arrears within the same geographic area, akin to a Metropolitan Statistical Area (MSA) in the US, to account for local economic factors, adding both year and firm fixed effects. Subsequently, we compare one firm with a municipality that is paying on time against the same firm with a municipality that is not, adding firm-year fixed effects. Furthermore, we also experiment, in both specifications, with the dependent variable in binary form (value one if there is a new contract, zero otherwise) as well as in continuous form (one plus the natural logarithm of the value of all awarded contracts between each firm-municipality pair in a given year). Generally, results show no systematic differences before 2011. However, in 2011, firms were (18.3%) less likely to contract with a municipality they had arrears with than with a municipality they did not have arrears with, but only when we include firm-time fixed effects (no significantly different otherwise). Consistent with this, we find firm-municipality pairs with higher arrears in 2011 also had a

smaller volume of contracts but, again, only when we include firm-time fixed effects (no significantly different otherwise). Across all specifications, differences dissipate following the settlement of arrears in 2012. Collectively, our findings suggest that firms might exhibit reluctance to engage with the public sector following payment delays. Nonetheless, this hesitancy appears to be temporary, as the relationships tend to be reestablished once the public sector's credit standing improves.

1.2.3. Close-Election Regression Discontinuity Design (RDD)

Chapter 5 *uses a close-election regression discontinuity design (RDD) to explore how different incentives, depending on government tenure, can impact the probability to reach a bailout agreement with a third-party institution.*

We start by presenting a cross-country analysis documenting that changes in government leadership are associated with an increased likelihood of securing a financial assistance agreement with the IMF. Leveraging a panel dataset encompassing all IMF interventions in countries with democratically-elected administrations (153) from 1992 to 2021 (spanning 29 years). We regress a dummy variable that takes value one if the country experienced a change in the party in power in the last election (zero if the country continued to be ruled by the previous incumbent) on a dummy variable that takes value one (zero) if the country has (not) entered into an agreement to receive financial support from the IMF. We control for various factors depending on the specification, including GDP growth, the political orientation of the ruling party, country and year fixed effects (to account for country-level characteristics and global time trends potentially

influencing both the likelihood of IMF assistance and government turnover), and group fixed effects interacted with annual indicators (to account for heterogeneity in time-varying confounders across similar groups of countries), following Bonhomme & Manresa (2015). Our coefficient of interest remains significant, with the same sign (positive) and broadly similar in magnitude, across all specifications. These findings provide suggestive evidence of a positive causal relationship between government turnover and the propensity to seek external financial support. Specifically, we ascertain that the likelihood of formalizing a funding agreement with the IMF is 3.4 (all controls) to 5.4 (no controls) percentage points higher when a new party assumes power, compared to a baseline probability of 11% for entering into such an agreement with the IMF.

This observation should be interpreted with caution, however, as the association between government turnover and the likelihood of securing IMF funding may be influenced by both demand and supply-side factors, leading to a classic endogeneity problem. Furthermore, the observed correlation could stem from country-specific attributes or cyclical variations in economic and electoral performance. It is well-documented that periods of economic decline often coincide with an increased rate of governmental changes and a heightened occurrence of IMF interventions, underscoring the complexity of disentangling the causal relationship from these intertwined factors.

To address potential endogeneity concerns, we lag the main regressor (party change) by one year. Results remain unchanged. Additionally, we incorporate further macroeconomic indicators (inflation rate, current account balance, unemployment rate, and gross debt) as controls. Despite losing nearly half of our sample due to missing data, the coefficient of interest retains a similar magnitude and remains statistically significant at the 5% level. Nonetheless, the

assumption needed for exogeneity of the main regressor (party change) remains quite strong. We then perform an additional analysis focusing solely on countries experiencing a banking crisis, according to Laeven & Valencia's (2020) database. This approach aims to reduce the likelihood that unobserved external economic shocks might simultaneously influence both a recent governmental change and the inclination to seek IMF assistance. Not surprisingly, within this subset, the average likelihood of receiving an external bailout is significantly higher than in the complete country panel, at 48%. Remarkably, in the context of a banking crisis, the probability of entering into an IMF funding agreement increases by 25 (all controls) to 29 (no controls) percentage points with a newly elected government. However, even within this more narrowly defined group, maintaining the exogeneity of the government change indicator remains a strong assumption: It is hard to identify whether these estimated effects result from differences in either governments' propensity to request assistance or the IMF's willingness to provide it.

The large number of Spanish municipalities that, sharing a common electoral system, received a simultaneous credit shock under the SPP in 2012, allows us to draw quasi-experimental estimates from this causal relation. The SPP has some features that make it specially well suited to achieve a clean identification of the influence of political leaders' private motivations on their decision to seek a bailout from a third-party institution. Firstly, all municipalities in our study underwent elections in May 2011, shortly before the initiation of the SPP in February 2012. The scheduling of both the elections and the SPP was uniform across municipalities and independent of any municipal government actions. Secondly, the central government did not impose any conditionality, thus municipalities had the autonomy to chart their path to fiscal stability via their

respective fiscal adjustment plans. Thirdly, the central government held the authority to reclaim any overdue payments from any municipality. Fourthly, due to the comparatively more advantageous credit terms for municipalities that submitted an adjustment plan, it is possible to calculate their gain, in NPV terms, compared to the option of forgoing the submission of a fiscal adjustment plan. This calculation facilitates the identification of the latter option as stemming from a principal-agent dilemma.

To be clear, the central government settled all outstanding payments to suppliers on behalf of the municipalities, which were then obligated to repay these sums to the central government. The key distinction lay in the terms of repayment: Municipalities that had a fiscal adjustment plan sanctioned by the central government were afforded funding over a 10-year period, inclusive of a 2-year grace period (effectively 2+8 years). In contrast, municipalities lacking such a plan were allocated funding for a mere 5 years, without the benefit of an interest-only grace period. The financing rates were more favorable than those municipalities could secure at the time (2012) from local banks or the capital markets, with the pricing being identical for both groups. Consequently, given the fiscal adjustment requirement, municipal governments just had two choices: (i) whether to implement the adjustment in a front-loaded or back-loaded manner, and (ii) in the case of opting for a back-loaded adjustment, to ensure the devised plan was viable, irrespective of the adjustment being tax- or expenditure-based.

Against this backdrop, we focus on achieving a clean identification of the effect of interest by exploring an ideal setting (Spain's SPP), which enables us to augment the preceding descriptive evaluation with rigorous causal estimates.

Initially, we delve into the impact of municipalities' chosen repayment strategies on their revenues, expenditures, and tax rates. By analyzing a panel that includes over 3,800 municipalities over an eight-year span (2008-2015), we regress a dummy variable taking a value of one (zero) if the municipality chose the front-loaded (back-loaded) option to pay for its SPP obligations on our variables of interest: per capita central government transfer revenues, total per capita spending, and the urban property tax rate imposed by the municipality. We restrict this analysis to municipalities with arrears as of the end of 2011, and control for differences in the level of arrears per capita across municipalities in both groups. We further add municipality-level and year fixed effects. Collectively, the patterns displayed by the results are consistent with the consequences of a front-loaded adjustment: Similar trends before the SPP (2008-11), but lower transfers, lower spending, and higher taxes after it (2012-15). Nevertheless, these findings should not be interpreted as causal as our specification is unlikely to deal with potential differences in the trajectories of municipalities making different choices. However, these results do offer indicative evidence that the fiscal outcomes expected from front-loaded adjustments manifest in reality.

Secondly, utilizing information from Factiva, which aggregates more than 6 million articles annually from over 200 national, regional, and local newspapers and magazines in Spain, we found negligible media mentions of the SPP prior to its announcement in early 2012. A significant surge in media attention occurred with its announcement in February 2012 and persisted throughout its implementation phase (February – July 2012), transitioning to consistent coverage from September 2012 onwards. Notably, the analysis indicates that press coverage of the SPP was more extensive in municipalities that chose the back-loaded repayment option in return for implementing a fiscal adjustment plan.

Third, to ascertain the impact of a shift in political leadership in 2011, we compare the outcomes in municipalities where the incumbent party narrowly lost (thereby often, but not invariably, leading to a change in political party) against those in municipalities where the incumbent party narrowly won (wherein, typically, there is no change in political leadership). It is crucial to note that in Spain, mayors are not directly elected by voters but are chosen by the local council. Consequently, voters elect council members, who in turn select the mayor. This implies that the likelihood of a new mayor being appointed does not shift abruptly from zero to one as the running variable takes a positive value. Hence, we employ a fuzzy regression discontinuity design (RDD) for close elections.

We construct a running variable that takes positive (negative) values when the challenger (incumbent) secures a victory in the 2011 local elections. Utilizing this running variable, we employ a two-stage least squares (2SLS) approach to assess the impact of a binary variable, which takes a value of one (zero) if the municipality elects a mayor from a new party (re-elects the incumbent), on another binary variable that indicates whether the municipality has (not) submitted a fiscal adjustment plan to the central government. In the first-stage regression, a binary variable that takes a value of one (zero) if the running variable is positive (negative) serves as our instrumental variable for estimating the main regressor in the subsequent reduced-form equation. The parameter of interest is thereby interpreted as the influence of a change in party leadership on the likelihood of submitting an adjustment plan. The demarcation of "close" elections is determined through the optimal bandwidth selection procedure outlined in Calonico, Cattaneo, Farrell, & Titiunik (2017).

Our research design's validity is underscored in two essential respects. Firstly, while political parties may exert some influence on electoral outcomes through their actions, their capacity to precisely control these outcomes is limited. This assertion is supported by the results of formal statistical tests detailed in McCrary (2008) and Cattaneo, Jansson, & Ma (2020). Secondly, we also conduct formal tests to ensure that covariates exhibit smooth variations at the threshold, thereby affirming that our regression discontinuity design (RDD) successfully deals with predetermined confounders.

We find that the impact of a shift in political leadership on the likelihood of submitting a fiscal adjustment plan is both substantial and statistically significant. Specifically, we determine that newly elected administrations are between 27 (all controls) and 30 (no controls) percentage points more inclined to propose a fiscal adjustment plan compared to incumbents. In all specifications, the first-stage F-statistics significantly exceed the conventional threshold for identifying weak instruments, affirming the robustness of our findings.

Fourthly, we undertake several robustness tests for our main findings. Initially, we recalculate our regression discontinuity (RD) estimates by substituting our dependent variable with a binary indicator that is set to one (zero) if a municipality's fiscal adjustment plan is accepted (denied) by the central government. Given that a mere fraction (under 7%) of all SPP-related fiscal adjustment plans were declined, we observe – not surprisingly – no qualitative shift in our results. Secondly, we also assess the stability of our main estimated effect for different bandwidths around the threshold. For all chosen bandwidths within the [5%, 20%] range, our coefficient of interest retains statistical significance at the 95% confidence level and remains similar in size to our main results. Thirdly, upon adjusting for higher-order polynomials and applying a

uniform kernel for weighting observations, we notice a decline in point estimates, which, nonetheless, continue to be significant at conventional levels. Lastly, we recalibrate our RD analysis by redefining our running variable (formerly the vote share difference between the challenger and incumbent) to the proportion of votes that need reallocation from the challenger to other parties for the election outcome to switch from a win to a loss or inversely, following Folke (2014) and Fiva, Folke, & Sørensen (2018). The resulting estimates largely align with those from our main analysis.

Our main findings indicate that newly elected administrations are more likely to submit a fiscal adjustment plan. We hypothesize that this disparity is attributable to the act of presenting a fiscal adjustment plan, which accentuates the municipality's financial issues. New leaders have the opportunity to attribute these problems to their predecessors, whereas incumbent leaders might prefer not to disclose these issues to their constituents, even if it results in the adoption of a less than ideal policy approach.

Firstly, utilizing data from Factiva, we examine how submitting a fiscal adjustment plan influences the information available to voters. By analyzing a dataset covering over 3,800 municipalities from 2011 to 2013 (a three-year period), we employ a difference-in-differences (DiD) methodology to assess the impact of a municipality's repayment decision on its press visibility. Specifically, we regress a dummy that takes value one (zero) for municipalities that (did not) presented a fiscal adjustment plan, a dummy variable that takes value one (zero) in 2012-13 (2011), and the interaction between the two, on a dummy variable that takes value one (zero) if the SPP (does not) appears in the news alongside the name of the municipality. We find that municipalities which submit a fiscal adjustment plan tend to receive more media attention regarding the SPP

compared to those that do not submit a plan. This pattern holds even after accounting for municipality-specific fixed effects and the overall volume of news per municipality each year. We interpret these results as evidence that the act of presenting a fiscal adjustment plan indeed affects the set of information accessible to voters.

Secondly, we refine our regression discontinuity (RD) analysis by replacing the second-stage outcome with a dummy variable that is assigned a value of one if the SPP is mentioned in news articles alongside the municipality's name during 2012-13, and zero otherwise. The second-stage results reveal that a shift in political power is associated with a 20 percentage-point rise in the media coverage of the SPP. This outcome is interpreted as evidence that submitting a fiscal adjustment plan significantly influences the set of information available to voters.

Third, we also hypothesize that shifts in political leadership result in increased (decreased) criticisms from the new government (former government, now opposition) concerning the SPP. The logic is straightforward: New administrations aim to highlight to the electorate the challenging fiscal scenario they have "inherited," underscored by the potentially large arrears, necessitating the formulation and execution of a fiscal adjustment plan. Conversely, parties that have shifted to the opposition are likely less inclined to critique the new government, given their own responsibility for the fiscal conditions and arrears accumulation the current administration is addressing. Utilizing Factiva, we compiled all news articles (exceeding 21,800) from Spanish national and local publications in 2012 and 2013 that pertained to the SPP or associated municipal adjustment plans. With ChatGPT, we identified: (i) all municipalities mentioned, (ii) whether criticisms targeted the current or former government, and (iii) the

criticism's origin, whether from the government or opposition. The refined dataset comprised 11,356 articles linked to 805 municipalities, with over 60% featuring criticism towards either the current or previous administration.

We then followed two complementary approaches. Initially, we ran news-level regressions of different outcomes identifying the origin/destination of criticisms on a dummy variable that takes value one (zero) if a municipality (did not) experienced a change in the party in power. To mitigate endogeneity concerns, we also control for municipal characteristics, including population, employment, and several fiscal and financial variables. Results suggest that changes in the party in power are associated with an increase (decrease) in the proportion of criticisms targeted at the previous government and originated from the current government (opposition). For a causal interpretation, we also implemented a regression discontinuity design (RDD) to introduce exogenous variation, aggregating news data at the municipal level and mimicking the method used to examine the influence of fiscal adjustment plans on public information. The results suggest that (i) the opposition is more critical of ongoing incumbents, and (ii) new administrations tend to shift blame to their predecessors.

Additionally, we also delve into the intrinsic impact of the fiscal adjustment plan, irrespective of any alterations in the party in power. For this, we regress a dummy variable that takes value one (zero) if a municipality had (did not have) an adjustment plan related to the SPP and a vector of pre-determined municipal characteristics, on three potential outcomes of interest: (i) a dummy variable that takes value one (zero) if the piece of news (does not) contains criticisms directed at the current government; (ii) a dummy variable that takes value one (zero) if the criticisms come from the opposition (government); and (iii) a dummy variable that takes value one (zero) if the criticisms come from the opposition (current

government) and are targeted at the current (previous) government. The results indicate that in scenarios where the party in power remains unchanged, the act of presenting a fiscal adjustment plan is associated with (i) a notable increase in news articles critical of the current government, (ii) intensified criticism from the opposition, and (iii) a surge in opposition-led critiques directed at the current administration. Interestingly, these patterns do not manifest following a change in the party in power. This outcome suggests that the continuing opposition tends to capitalize on the exposure afforded by announcing a fiscal adjustment plan to heighten criticism of the continuing incumbent officials for their management of the municipality's public finances. Conversely, this dynamic does not occur when a newly elected mayor undertakes a fiscal adjustment plan in the context of the SPP.

Fourthly, our findings support the notion that incumbents may avoid presenting a fiscal adjustment plan to preserve their informational rents. Initially, we run our main specification on three separate sub-samples based on the pre-existing arrears levels, examining the impact of a change in office on the likelihood of presenting a fiscal adjustment plan. For municipalities in the lowest tercile, the effect is negligible, with a 49% chance of presenting a plan, unaffected by the duration of the government's tenure. Conversely, in the upper two terciles, where the likelihood of presenting a plan exceeds 80%, tenure distinctions become significant: New administrations tend to present plans when faced with substantial arrears, whereas many re-elected incumbents are reluctant to do so. Secondly, insights from a survey of 126 Spanish mayors, conducted as part of the POLICONSTRAINTS project (by Pedro Rey, ESADE), align with the idea that admitting past errors and taking corresponding actions pose a significant challenge for politicians. Lastly, we investigate the 2015 municipal election

outcomes to assess the relationship between the presentation of a fiscal adjustment plan for improved financing terms and re-election probabilities. We regress a dummy variable that takes value one (zero) if the party in power after the 2011 election was the same as (different from) that in power in 2010, a dummy variable that takes value one (zero) if the municipality (does not) presents a fiscal adjustment plan, and the interaction between the two, on a dummy variable that takes value one (zero) if the party in power before the 2015 was (not) re-elected. We add a set of controls, including population, financial debt per capita, and outstanding arrears per capita, all as of the end of 2011. In one specification, we also add province-level fixed effects. Our coefficient of interest is that from the interaction term, which indicates the differential re-election probability between incumbents that presented a plan and incumbents that did not (estimated conditional on presenting a plan). We find that administrations consenting to a fiscal adjustment plan with the central government face a substantial reduction in their likelihood of re-election, compared to those that refrained from submitting such a plan, but only if they were incumbents before 2011. This pattern does not extend to newly elected officials. Such outcomes align with our proposed mechanism, suggesting that ongoing incumbents might incur an electoral disadvantage by presenting a plan, as it could underscore prior fiscal mismanagement. However, given the endogenous nature of the decision to submit a plan, our results should be viewed as just suggestive or descriptive in this context.

Fifth, we explore and ultimately rule out a number of potential alternative mechanisms. Initially, we replicate our fuzzy regression discontinuity (RD) estimates using a two-stage least squares (2SLS) method, with the characteristics of the elected government serving as our dependent variables.

We find that the effect of interest is statistically insignificant for all observable characteristics, except for the age of the elected mayor and for her partisan affiliation. These outcomes were expected, however, as it is common for newcomers to be younger (though, we note, our main findings remain unaltered even if we also control for the mayor's age specifically), and given that the PSOE secured the majority of local elections in 2007, a shift in local governance would typically be associated with a decreased (increased) likelihood of a mayor being from the PSOE (PP). Secondly, we consider the possibility of an ideological alignment between local and central governments influencing the decision to present a fiscal adjustment plan. In this vein, we undertake several analyses: (i) Including a control for the mayor's party in 2012 in our main specification, we observe that our coefficient of interest remains large and statistically significant, suggesting that the effect of a political shift on the likelihood of presenting a fiscal adjustment plan is robust to the inclusion of mayoral party affiliation. (ii) When dividing the sample based on the political party of the incumbent, challenger, and the mayor in 2012, it becomes evident that challengers are consistently more inclined to present a fiscal adjustment. (iii) We also follow an alternative estimation strategy, where we estimate the effects of having a mayor from PP or PSOE on the probability of presenting a fiscal adjustment plan. For this, we reproduce our fuzzy RD estimates by 2SLS-regressing a dummy variable that takes value one (zero) if the municipality appointed a mayor from one party after the 2011 election and the vote margin (i.e., the difference in vote shares between a party and the most voted party, excluding the former) for that party in that election on the same dependent variable as in our main exercise: A dummy variable that takes a value one (zero) if the municipality (does not) presents a fiscal consolidation plan to the central government. The estimation is performed

just as in our main exercise, for both PSOE and PP. We find no significant effects, which decisively suggests that the observed primary effect does not stem from partisan differences in the likelihood of submitting a fiscal adjustment plan to the central government. Third, additional factors that might influence the approval of the plan (such as the government having the majority, or not having plurality) do not exhibit discontinuities at the threshold, indicating that our results are not attributable to any of these variables either.

Finally, there could still be unobservable characteristics affecting electoral outcomes might not be balanced around the threshold (Marshall, 2022), potentially confounding our regression discontinuity (RD) estimates. A key area of concern is, in particular, the quality of the candidates. Initially, we assess whether usual proxies for candidate quality frequently cited in political science and economics research—such as educational attainment and previous occupation—are balanced at the threshold. We find that these indicators are indeed balanced. Second, following George (2019), we posit that a candidate's electoral performance is influenced not only by their personal competence or quality but also by the prevailing sentiment towards their party's regional branch. Consequently, a capable candidate might underperform due to adverse regional reactions to their party, and conversely, a less competent candidate could achieve better results owing to positive regional party dynamics. To account for this, we adjust the challenger's electoral margin by subtracting the impact of regional party shocks. In situations where challengers exhibit a negative (positive) adjusted margin—termed a "no swing" margin—it implies that, absent regional party shocks, the incumbent would have emerged victorious (been defeated). Hence, incumbents who, after accounting for these regional party shocks, would have still won, are deemed to be of comparatively higher quality

than those who would have lost. This indicates that the variation in candidate quality, as adjusted for regional party shocks at the threshold, does not alter our primary findings, suggesting that differences in candidate quality do not drive our observed results. In our final analysis, we also test whether our main results are influenced by differences in unobservable characteristics at the threshold, employing the strategy outlined in Angrist & Rokkanen (2015). Under a conditional independence assumption (CIA), this approach enables the estimation of the regression discontinuity (RD) coefficient for observations distant from the threshold. Initially, we demonstrate that, conditional on our chosen controls, the decision to present an adjustment plan is unrelated to the running variable. This observation supports the viability of the CIA necessary for applying the method proposed by Angrist & Rokkanen (2015). Subsequently, we present estimates of the RD coefficient away from the threshold using two CIA-based estimators: the linear re-weighting method introduced by Kline (2011) and a variant of the propensity score estimator by Hirano, Imbens, & Ridder (2003). Both methods produce positive and statistically significant estimates of the impact of a party change on the likelihood of proposing a fiscal adjustment plan, even when examining data points removed from the threshold. This finding suggests that the identified effect is not attributable to any underlying differences in unobserved characteristics, such as candidate quality, that might vary at the threshold, further reinforcing the robustness of our results.

1.3. Contribution Made

First, we contribute to the literature on government expenditure arrears. This literature primarily delves into the quantification of expenditure arrears

across various national accounting systems in emerging markets (Diamond & Schiller, 1991), their implications at both the microeconomic and macroeconomic levels (Checherita-Westphal, Klemm, & Viefers, 2016; Connell, 2014; IMF, 2019a and 2019b), and strategies for clearing them (Ramos, 1998). While Flynn & Pessoa (2014) offer a narrative account of the reasons behind and consequences of government arrears accumulation, empirical evidence on their causes is still scarce. Lee (2016), utilizing data from 27 European countries spanning 1990-2012, finds government arrears to be positively determined by both government debt and a measure of voice and accountability, but negatively determined by GDP growth, government bond yields, and measures of political stability, non-violence and rule of law. Similarly, using country-level data from sub-Saharan Africa over 2005-2018, IMF (2019b) finds the accumulation of particularly large stocks of arrears are often driven by exogenous factors such as adverse terms-of-trade shocks and political instability. Notably, using data from around 6,700 Italian municipalities for 2003-10, Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) provide empirical evidence linking lower intergovernmental transfers to the increased use of arrears in public investment expenditures by municipalities. In this context, our research highlights the role of financial constraints as a pivotal factor in the accumulation of government expenditure arrears.

One of the principal methodological hurdles in studying the causes or effects of domestic arrears, which explains the scarcity of empirical research on the topic, is the absence of actual arrears data. This issue is exacerbated by the lack of standardized definitions and comprehensive coverage, often stemming from inadequate fiscal accounting practices and infrequent audits (IMF, 2019a). A survey encompassing 121 low- and middle-income nations found that merely

12% consistently publish reliable data on both the volume and aging of arrears (Flynn & Pessoa, 2014). In Europe, the fact that Eurostat's Sector Accounts only include data on "other accounts payable" (ESA-1995 code AF.7) – which aggregates all unpaid bills without distinguishing those in arrears – underscores this challenge. To date, empirical studies have primarily relied on two proxies for estimating expenditure arrears. Checcherita-Westphal, Klemm, & Viefers (2016), followed by Lee (2016), employ a statistical approach combining annual stock data on "other accounts payable" from Eurostat with survey data on average payment duration from Intrum Justitia, a commercial credit management service. Meanwhile, Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) utilize annual stock data from Italian municipalities on "outstanding payments for investment expenditures," which encompasses all unpaid invoices (not just those in arrears) but excludes "current expenditures" data. To the best of our knowledge, our study is the first empirical analysis to utilize real data on government expenditure arrears, with the notable exception of IMF (2019a and 2019b). However, our analysis benefits from a significantly broader cross-section, encompassing over 7,500 entities, compared to the IMF's (2019a and 2019b) panel with a cross-section of just 30. Moreover, while the IMF's dataset may be considered incomplete due to the inconsistent monitoring of arrears across most Sub-Saharan nations and the variability in arrears definitions among countries (IMF, 2019b), our dataset not only offers comprehensive coverage but also ensures uniformity and consistency in the definition of arrears across all municipalities in Spain. This distinction enhances the robustness of our results.

Additionally, our research also contributes to the literature on fiscal federalism (for a review, see Ter-Minassian, 2015), particularly addressing the fiscal risks posed by subnational governments. These entities can generate

considerable fiscal risks for central governments on two main fronts: firstly, through the quality of public service delivery at the subnational level, and secondly, via the potential strain on central government resources stemming from unsustainable subnational fiscal practices (Saxena, 2022). Bova, Ruiz-Arranz, Toscani, & Ture (2016), within their global database covering contingent liability realizations from 1990-2014, identified 13 instances where contingent liabilities from subnational governments materialized, averaging a cost of 3.7% of GDP. This discussion naturally extends to the topic of subnational fiscal rules. De Biase & Dougherty (2022) survey both the academic literature and recent trends within OECD countries, noting that fiscal regulations at the subnational level are typically less stringent than those enforced at the national level. Specifically, in the context of Spain between 2002-15, Lledó, Delgado-Téllez, & Pérez (2017) observed that regions frequently failed to adhere to fiscal deficit targets, a trend that proved to be persistent; moreover, they find no substantial evidence that regional fiscal rules effectively mitigated such non-compliance. In this context, our research highlights the critical need to account for all liabilities, including those beyond financial debt, differentiating payables that are in arrears from those that are not, to preclude subnational governments from bypassing inadequately designed fiscal rules through delayed payments to suppliers and the subsequent accrual of arrears.

Thirdly, our research also contributes to the literature on the financial aspects of procurement. The procurement channel has been shown to be useful in providing firms with a stable income during recessions (Goldman, 2020). Di Giovanni, García-Santana, Jeenas, Moral-Benito, & Pijoan-Mas (2022) build upon this notion, illustrating how firms might leverage procurement relationships as collateral to alleviate financing restrictions. In a similar vein, Gabriel (2022)

shows that Portuguese companies utilize procurement contracts as collateral to augment their borrowing capacity. Bonfim, Zhao, Queiro, & Ferreira (2021) identify a reciprocal effect, noting that an unexpected reduction in government expenditure during a financial crisis makes it more challenging for firms reliant on procurement to secure loans. Barrot & Nanda (2020) delve into the specific impact of formal trade credit terms within procurement contracts, revealing that shorter payment durations can benefit firms, particularly highlighting a positive influence on employment when the US government expedited payments to its contractors, albeit this effect is contingent on the labor market's tightness. Additionally, Checherita-Westphal, Klemm, & Viefers (2016) have indicated that delays in payments by some European governments can impede the liquidity and profitability of the private sector, while Conti, Elia, Ferrara, & Ferraresi (2021) find that stricter regulations to curb late payments can lower firm exit rates. Lee (2021) points out that firms heavily reliant on procurement tend to experience greater growth, a trend more pronounced among financially constrained businesses. Our research uniquely centers on government arrears, distinct from the general terms of formal trade credit in procurement. We examine the implications of a substantial, one-time reduction in arrears, rather than focusing on smaller, ongoing adjustments. Additionally, our analysis sheds light on how the delayed payment of arrears intersects with firms' financing constraints, providing valuable insights into the broader implications of government payment practices on the private sector's financial health and operational stability.

Fourthly, our research also contributes to the literature on the various stimulus policies aimed at directing liquidity towards the corporate sector (Bach, 2014; Banerjee & Duflo, 2014). Assessing the efficacy of targeted policy measures is often challenging, largely due to the potential for selection biases. In

the context of our natural experiment, the government implements an unconventional fiscal strategy, borrowing from financial institutions to expedite the clearance of existing arrears. While the overall liabilities of the government remain unaffected, this maneuver yields tangible outcomes, especially for firms facing financial constraints. This approach underscores the capacity of directed fiscal interventions to impact the economic landscape, highlighting the nuanced role of government actions in alleviating corporates' financial constraints.

Fifthly, our research also contributes to several streams of the trade credit literature by elucidating the potential costs borne by suppliers when extending trade credit to a large purchaser (Murfin & Njoroge, 2015; Klapper, Laeven, & Rajan, 2012). We add to the understanding of the costs of late payment and its interaction with financial constraints. Although the phenomena of late payments has been extensively documented in the literature (Petersen & Rajan, 1997), theoretical discussions have predominantly examined its function as a form of insurance for the buyer (Cuñat, 2007; Wilner, 2001), a concept less applicable to public sector liabilities. Furthermore, the empirical literature on late payment remains very limited. Our work also makes a novel contribution to the limited body of research on trade credit factoring, implicitly demonstrating that firms are unable to discount government arrears, even when the government's credit standing is good.

Sixthly, our research also contributes to the literature on the political economics of macroeconomic policy, particularly regarding how political constraints influence the management of fiscal challenges by governments. This aspect of our work relates to studies on the political determinants of fiscal stabilization and reform efforts (for comprehensive reviews, see Alesina, 2000; or Mahmalat & Curran, 2018). Our contribution is twofold. Firstly, we delve into how

political constraints dictate the nature of fiscal adjustments, revealing that incumbents in continuous office are less inclined to commit to a fiscal consolidation program, even when external financing could offer fiscal leeway. Secondly, by employing a close-election regression-discontinuity design, we are able to pinpoint our parameter of interest with relatively lenient assumptions, utilizing methodologies that are standard in the applied micro-political economics literature. This approach sets our study apart from much of the empirical analysis in macroeconomic stabilization, which often depends on cross-country evidence (for recent reviews of the literature, see Alesina, Favero, & Giavazzi, 2019a; and Kose, Ohnsorge, Reinhart, & Rogoff, 2022).

Seventh, our research also contributes to the literature on the determinants of externally sponsored financial arrangements. Prior studies have demonstrated the influence of various factors such as growth levels (Knight & Santaella, 1997), political ties with multilateral organizations (Barro & Lee, 2005; Presbitero & Zazzaro, 2012; Dreher, Sturm, & Vreeland, 2009), and the history of IMF interventions (Conway, 2007) on the likelihood of securing future financial arrangements with the IMF. Building on these insights, our study introduces electoral motivations as another crucial determinant shaping such financial decisions.

Eighth, the inclination of new administrations to propose an adjustment plan resonates with the notion that politicians often seek to deflect responsibility during challenging times (Weaver, 1986; Hinterleitner, 2017; Bursztyn, Egorov, Haaland, Rao, & Roth, 2022). This aspect of our research aligns with studies delving into the empirical foundations of salience theory (Robertson, 1976; Petrocik, 1996; Dolezal, Ensser-Jedenastik, Müller, & Winkler, 2014), which highlights how incumbents and challengers strategically select topics to construct

narratives that spotlight their strengths and exploit their adversaries' vulnerabilities (Green-Pedersen & Mortensen, 2010; Greene, 2020). Our findings extend this narrative, illustrating that politicians not only highlight certain issues to improve their electoral prospects but also adopt strategies that diminish the discursive ammunition available to their rivals. Specifically, we observe that politicians are significantly less likely to adopt the more efficient option—presenting a fiscal adjustment plan—when opting out could safeguard their public image. This insight situates our study within the broader discourse contrasting the Chicago and Virginia schools of political economy (e.g., Becker, 1976 and 1985; Wittman, 1989; Crew & Twight, 1990; Coate & Morris, 1995; Tullock, 1989). Consistent with the Virginia school's perspective, we find that incumbents forgo the preferable option for their municipality if it offers them personal advantages, suggesting that electoral rivalry fosters such behaviors. This introduces a novel angle on how the pursuit of re-election can influence politicians' willingness to engage in practices detrimental to public welfare.

Finally, over the last decade, empirical research has increasingly validated the notion that the prospect of re-election can serve as a regulatory mechanism for officeholders, aligning with the predictions of political agency models (Barro, 1973; Ferejohn, 1986; Banks & Sundaram, 1993). For instance, studies have demonstrated that re-election incentives can deter corruption (Ferraz & Finan, 2011), motivate greater diligence (Fouirnaies & Hall, 2022), and improve the execution of policies (De Janvry, Finan, & Sadoulet, 2012). Our findings contribute to this body of literature by illustrating that re-election motivations might also lead incumbents to adhere to suboptimal policies as a strategy to safeguard their reputations. This observation ties into the discourse on how a politician's career aspirations can adversely impact voter welfare (Canes-Wrone, Herron, &

Shotts, 2001; Maskin & Tirole, 2004; Smart & Sturm, 2013), introducing a novel aspect to the debate on the implications of restricting term limits. Our research indicates that while re-elected incumbents may cling to unsuccessful policies out of apprehension over admitting previous errors, new entrants are more inclined to disrupt the established order and make choices that are objectively better. This distinction underscores the complex interplay between political incentives, accountability, and policy outcomes, highlighting the nuanced effects of electoral dynamics on governance quality.

Chapter 2. Institutional Setting

2.1. The 2008-14 Crisis

The 2008-14 economic crisis had quite an adverse effect on Spanish general government finances, resulting in a significant increase in both the budget deficit and the public debt level. For a comprehensive account of the crisis, see Banco de España (2017). One result of this deterioration in public finances was a significant increase in the time taken by the general government (i.e., State, regions and municipalities) to pay its suppliers (Figure 1).[1] At the local level, total payables (including arrears) increased from 1.5% of GDP in 2005 to 2.6% of GDP in 2011, an all-time high since 1995 (Figure 2). The net increase in payables (1.1 percentage points) over this period was explained mostly by a significant build-up of arrears. As of December 2011, Spanish municipalities had accumulated over 1.8 million unpaid bills in arrears amounting to €9.6 billion (0.9% of GDP) owed to over 115,000 suppliers (Ministerio de Hacienda y Administraciones Públicas, 2013).[2]

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[1] We use "State" and "central government" interchangeably throughout.
[2] Suppliers include self-employed workers, small- & medium-sized enterprises (SMEs), and large firms.

Figure 1. Spain: Average Payment Delay (Days) per Sector, 2005-11
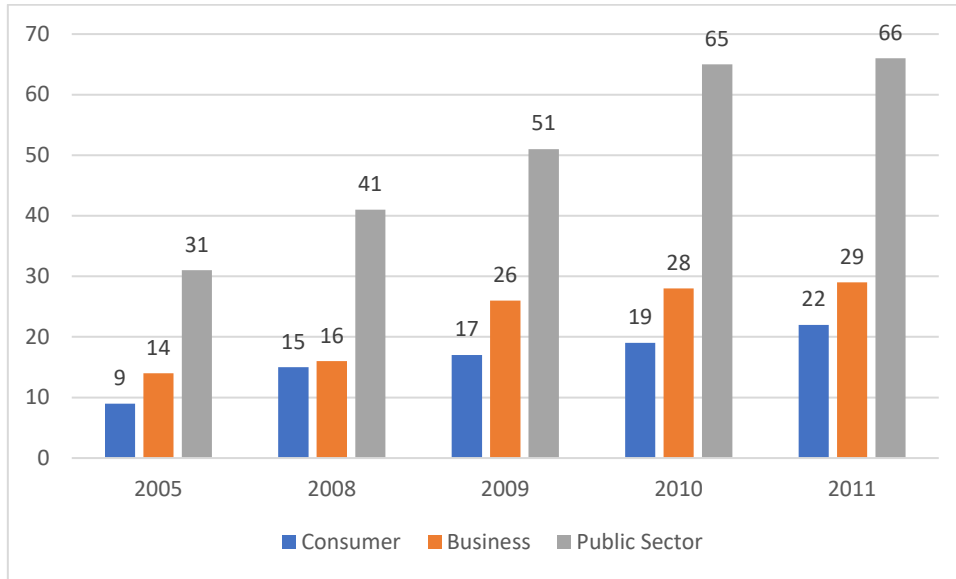
Notes: This Figure shows the average payment delay (in number of days) per sector in Spain. Data has been obtained from the annual European Payment Index (EPI) reports published by *Intrum Justitia* over 2005-11. The relevant data was missing from the 2006-7 reports.

Figure 2. Spanish Municipalities: Unpaid Bills (% of GDP), with & without Arrears; 1Q 1995 – 2Q 2022
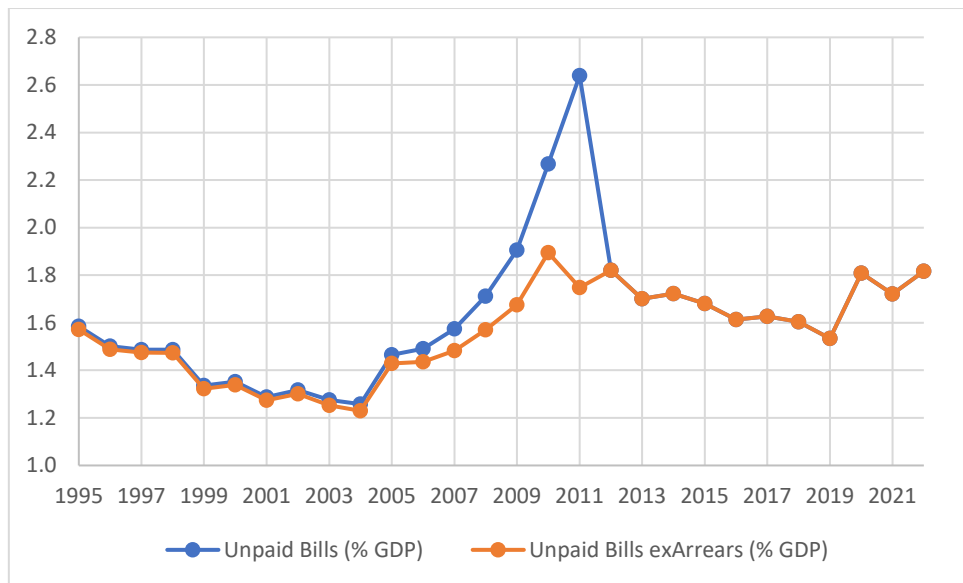
Notes: This Figure shows municipality-level unpaid bills, both including (blue) and excluding (orange) arrears, as a percentage of GDP. The overall level of unpaid bills (1995-2022) has been obtained from the Bank of Spain. The arrears data (1995-2011) has been obtained from the Spanish Finance Ministry.

2.2. Spain's Supplier Payment Plan (2012-13)[3]

To address the weak financial situation of Spanish subnational governments and their negative impact on business and ultimately on the country's broader (un)employment dynamics, the new Spanish government formed after the general elections of November 2011 announced a number of urgent measures early in 2012.[4] Among them was the Supplier Payment Plan or SPP, a policy aimed at clearing the arrears accumulated by regional and local governments over time through December 2011. The Plan was announced on 25th February 2012 (Royal Decree Law or RDL 4/2012).

2.2.1. Special-Purpose Vehicle

The Plan was implemented through the creation of a state-owned special-purpose vehicle (*Fondo para la Financiación del Pago a Proveedore*s – Fund for Financing Payments to Suppliers or FFPS) via a second Royal Decree Law approved on 9th March 2012 (RDL 7/2012).

On the liability side, the FFPS was funded through a €30-35 billion syndicated loan granted by a pool of banks involving the state-owned *Instituto de Crédito Oficial* (ICO) and all private Spanish banks with their contributions matching their market shares in Spain. On the asset side, the amounts transferred to suppliers were net of any outstanding liabilities suppliers might owe in either taxes or Social Security contributions, but gross of any late-payment

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[3] We use "Plan" and "Program" interchangeably throughout.
[4] The problems made headlines in both the domestic and international press. See, e.g., Healthcare in crisis as regions' debt to suppliers reaches record | Spain | EL PAÍS English Edition (elpais.com).

penalties, which the mechanism did not explicitly account for. Importantly, payments by the FFPS were made directly to the suppliers.

As a result, the central government (via the FFPS) subrogated itself as claimant against the subnational governments. For them, a key fiscal implication of the process was that payables (to suppliers) turned automatically into financial debt (to the central government). Figure 3 shows how, in 2012, municipality-level payables fell by about the same amount as financial debt increased, and the overall level of municipality-level liabilities (financial debt plus unpaid bills) was broadly unchanged as a result.

Figure 3. Spanish Municipalities: Total Liabilities (% of GDP) – Financial Debt vs. Unpaid Bills; 1Q 1995 – 2Q 2022
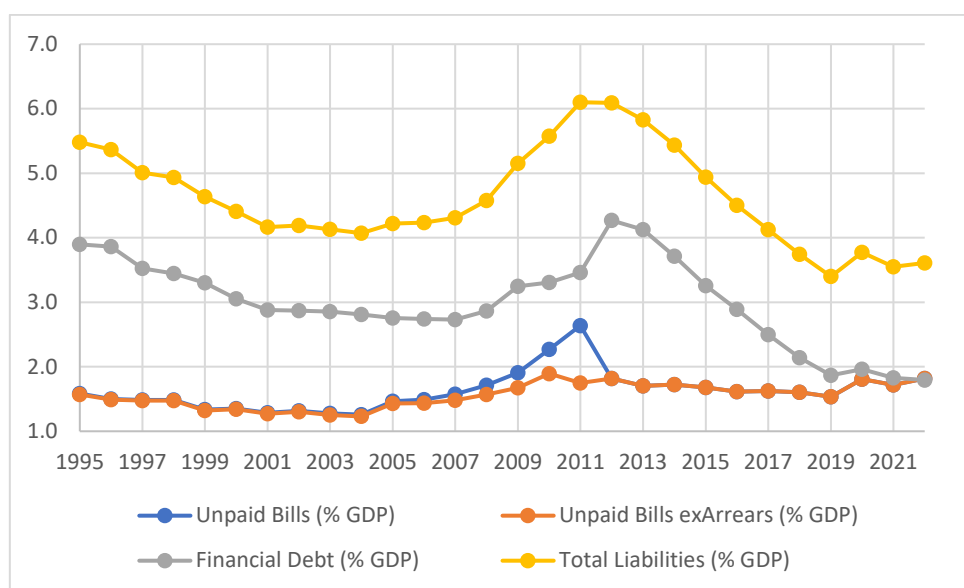
Notes: This Figure shows municipality-level liabilities (yellow) split across unpaid bills (blue) and financial debt (grey), as a percentage of GDP. The overall level of liabilities, unpaid bills, and financial debt (1995-2022) has been obtained from the Bank of Spain. The arrears data (1995-2011) has been obtained from the Spanish Finance Ministry.

2.2.2. Measuring the Stock of Subnational Government Arrears

At the time the Plan was announced in February 2012, the government had only a vague idea of the extent of arrears accumulated by subnational

governments, beyond that it was high.[5] Therefore, the Plan's first step involved a state-run audit that eventually revealed subnational government arrears at €27.3 billion (2.5% of GDP) as of December 2011, of which €9.6 billion (0.9% of GDP) were at the local level, and €17.7 billion (1.6% of GDP) at the regional level.[6] Participation by the 17 Spanish regions was voluntary and ultimately three regions opted out (Basque Country, Galicia and Navarre). Participation was mandatory, however, for the circa 7,500 local governments in any of the fifteen regions that share a common financing system.[7] Over half of these governments (under 4,000) were found to have some outstanding arrears as of the end of 2011.

Once the unpaid bills in arrears had been identified and quantified, the state-owned *Instituto de Crédito Oficial* (ICO), acting as the FFPS's paying agent, settled these bills by transferring their full amounts directly to the suppliers. Payments were made in three different waves (*ciclos*). On the 28th May 2012, €9.3 billion was transferred directly to the suppliers of close to 4,000 local governments. On 25th June 2012, €17.7 billion was transferred directly to the suppliers of the 14 participating regional governments. Finally, on 30th July 2012,

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[5] See Los alcaldes presentan 9.500 millones en facturas impagadas, la mitad de lo previsto | Economía | Cinco Días (elpais.com)

[6] Participating subnational governments were first required to send the full list of unpaid bills in arrears accumulated through December 2011 to the Finance Ministry by March 15th, 2012. Suppliers then had 15 days to check whether the amounts owed to them had been properly reported by the subnational governments. If the suppliers did not dispute information provided by the subnational governments, the information was assumed to be correct. Suppliers had until April 22nd to accept the terms of the proposal and to provide bank and other pertinent details. If any bills were missing from the first batch of information sent to the Finance Ministry, two additional windows (2-10 April, 2-8 May) were opened for subnational governments to amend the information reported. For further details, see Ministerio de Hacienda y Administraciones Públicas (2012).

[7] Under the common financing system (*Régimen Común*), the fifteen participating regions (and the two autonomous cities of Ceuta and Melilla in Northern Africa) receive most of their funding in the form of transfers from the central government. Only two regions (Basque Country and Navarre) operate under an alternative, special financing system (*Régimen Especial*) under which they collect most of their revenues through their own taxes. It is for this reason that the circa 500 municipalities of these two regions were explicitly excluded from the SPP.

€0.3 billion was transferred to the suppliers of local governments that had not been included in the May payment.

After the Plan's first phase was completed, the central government realized the arrears owed by select subnational governments (including the so-called *mancomunidades*) had been mistakenly left out in both the identification and the settlement processes carried out during the first half of 2012.[89] To address this, the government announced in January 2013 a second phase of the Plan to settle the arrears accumulated, also as of December 2011, by these other subnational governments.[10] This phase was formalized by a new decree passed on February 22nd (RDL 4/2013). Finally, in early June 2013, the ICO transferred around €1.1 billion directly to the suppliers of subnational governments (with an 18/82 split between the suppliers of local and regional governments) that had been left out from the 2012 phase mistakenly. The overall amount of arrears settled over the Plan's first two phases totaled €28.5 billion (2.7% of GDP).

2.2.3. Incentive Structure

The fact that no bank in Spain opted out from the funding of the SPP may suggest some arm-twisting by the central government. Still, three features did make participation financially attractive for banks. First, the FFPS's state-owned nature ensured a 0% risk weight (i.e., no capital consumption) for these particular

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[8] See Hacienda estudia cómo ampliar el plan de pago a más proveedores | Economía | Cinco Días (elpais.com)

[9] Spanish municipalities may channel some or all of their purchases through *mancomunidades*. These are legal pools of several municipalities engaged in procurement that seek to achieve some economies of scale and improve their bargaining power. Although from an economic standpoint, municipalities and *mancomunidades* are very similar, they have a different legal status. For further details, see FEMP (2012).

[10] See Montoro anuncia que pronto habrá un nuevo plan de proveedores para ayuntamientos | Partido Popular (pp.es)

exposures. Second was the possibility for a carry trade as banks funded these loans through the European Central Bank's (ECB) second Long-term Refinancing Operation (LTRO) in late February 2012 (Pollack, 19th December 2011). Finally, banks also had the option to convert their loans into bonds in 2013, improving their pool of collateral available for further ECB funding operations while giving them the option to book possible capital gains in case of a sharp compression in Spain's government bond yields, as it eventually happened.

The Plan was also attractive for subnational governments as the funding costs associated with the mechanism (142 basis points above the Spanish Treasury) were lower than the cost of a bilateral bank loan outside the SPP or better than going to the capital markets for larger municipalities with capacity to issue bonds, assuming that was even possible at the time.[11] The cost of municipal bonds outstanding in 2011 suggests this benefit could be at least 50 basis points.[12]

We should also note that moral hazard was a major concern for policymakers at the time (Martínez-Rico, 2021). If subnational governments came to expect a bail-out, what would prevent them from continuing to accumulate arrears in the future, and at an even faster pace than before?[13]

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[11] In early January 2012, the central government bailed the regional government of Valencia out following its inability to repay a €123 million bilateral loan from Deutsche Bank that was already due. See El Gobierno acude al rescate de la Comunidad Valenciana para evitar un impago de 123 millones de euros,Datos macroeconómicos, economía y política - Expansión.com (expansion.com).

[12] At the time of writing, Bloomberg provides data for only one long-term municipal bond outstanding in Spain around the time the SPP was implemented. This is a bond issued by the City of Madrid with a coupon of 4.5% and maturity on 16th May 2036. Also, price data are available for only a few days between late March and early April 2011. Over this short period, the City of Madrid's average risk premium (relative to the Spanish government's longest-duration [15-year] bond available at the time) was around 190 basis points. The implied risk premium (circa 50 basis points) was likely a lower bound for Spanish municipalities, as Madrid is the largest municipality in the country and has the greatest ability to tap the capital markets.

[13] See Heppke-Falk & Wolff (2008) for a discussion on how central government bailouts might induce moral hazard among local debt investors.

Three measures were adopted to minimize such risk. First, the intergovernmental funding provided by the central government (through the FFPS) was guaranteed by each subnational government share of central government tax receipts.[14]

Second, subnational governments were required to submit a fiscal consolidation plan to the central government to be approved by the Finance Ministry. Subnational governments with approved fiscal consolidation plans would receive funding at a rate equal to 10-year Spanish government bond yield plus 142 basis points, for a term of ten years and with a two-year interest-only grace period (i.e., 2+8 years). Governments that either did not present a fiscal consolidation plan or whose plan was not approved were entitled to the same variable rate but with debt service payments deducted directly from their share of central government tax receipts over a five-year period and no interest-only grace period. As a result, fiscal consolidation was expected to be smoother for compliant governments, but more difficult and thus front-loaded for non-compliant ones.[15] To illustrate this point, in Table 57 (Appendix) we show back-loaded adjustments carry a lower cost in net present value (NPV) terms than front-loaded adjustments.[16]

Finally, the government pushed through Parliament a stricter set of fiscal rules as part of a new Budget Stability Law (*Ley Orgánica* 2/2012). Clearly the

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[14] For regional and local governments in any of the 15 regions under a shared financing system (*Régimen Común*), most of the tax revenues are collected by the central government on their behalf. Then the central government transfers part of these revenues back to the subnational governments. Hence, in case of a non-compliant subnational government, the central government had the option to retain any amounts and reduce the transfers accordingly.

[15] Empirical evidence on the impact of fiscal consolidations is mixed. The consensus at the time, however, was that smoother, back-loaded fiscal adjustments, based on credible medium-term plans, were less punitive and therefore preferred (Blanchard & Leigh, 2013a and 2013b).

[16] See Section 7.3.3. (Appendix) for a detailed description of the approach followed to compute the NPV of the two competing choices (presenting vs. not presenting an adjustment plan).

then existing set of rules had not been effective, delivering large fiscal imbalances across all tiers of the Spanish general government over the previous years. On the non-compliance of fiscal rules at the regional and municipal level in Spain, see, respectively, Delgado-Téllez, Duarte, & Pérez (2017) and Bastida, Benito, & Guillamón (2015).

The induced fiscal adjustments of the SPP took Spanish local budget balance from a deficit of 0.4% of GDP in 2011 to a surplus of 0.1% in 2012. The result implied a 0.5 percentage-point improvement in just one year, the greatest fiscal adjustment recorded at the local level going back to 1995 (Figure 4). With an average budget surplus of 0.1% of GDP since 2012, Spanish municipalities' aggregate financial debt has been reduced from over 4% of GDP in 2012 to under 2% in June 2022, implying a net reduction of 2 percentage points over ten years.

Figure 4. Spanish Municipalities: Budget Balance (% of GDP) – Cash vs. Accrued Basis; 1Q 1995 – 2Q 2022
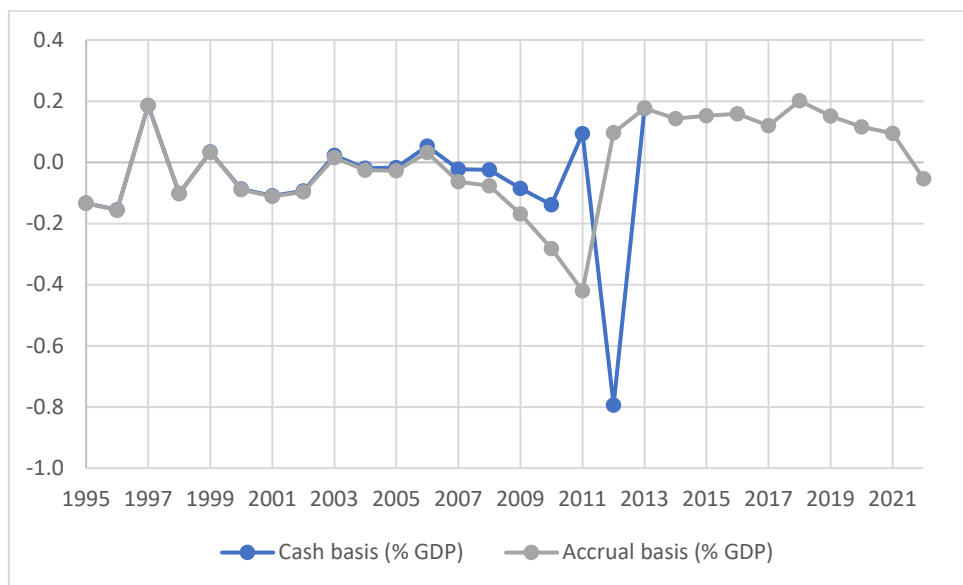
Notes: This Figure shows municipality-level budget balance, both on an accrual basis (grey) and on a cash basis (blue), as a percentage of GDP. Accrual budget balance data (1995-2022) has been obtained from the Bank of Spain. Arrears data (1995-2011) has been obtained from the Spanish Finance Ministry. Cash-basis budget balance has been estimated, for illustrative purposes, by allocating cash outflows to the dates when unpaid bills were effectively paid.

Chapter 3. Determinants of Government Arrears

3.1. Introduction

Government expenditure arrears are financial obligations of any type of government that remain unpaid and past the payment due date (IMF, 2001).[17] Arrears arise when governments are either unable or unwilling to pay their obligations in a timely manner, without proper cause and without subsequent payment of interest (Brondolo, 2009). This can be a temporary phenomenon resulting from liquidity problems during serious economic or financial crises, or a more persistent behavior resulting from weaknesses in a country's public financial management system (Flynn & Pessoa, 2014).

The accumulation of government arrears has two major consequences. First, it has negative long-term effects. It reduces corporate profits, increases the likelihood of corporate bankruptcies, and constrains overall economic growth (Checherita-Westphal, Klemm, & Viefers, 2016; Connell, 2014). Arrears have, moreover, negative second-round effects on bank credit quality, corporate tax compliance, and the quality of public service delivery (IMF, 2019a). Second, government arrears also distort typical measures of fiscal stance. They not only negatively impact a government's ability to use fiscal policy to stimulate economic activity (IMF, 2019a) but also contribute to delays in desired fiscal adjustments (Diamond & Schiller, 1991).[18] These are reasons why control of arrears are a priority in almost all IMF-supported programs (Flynn & Pessoa, 2014).

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[17] We use the terms "government expenditure arrears," "government arrears," "expenditure arrears," "domestic arrears," and "arrears" interchangeably throughout. They all refer to government liabilities (in arrears) to suppliers, unless explicitly stated otherwise.
[18] Milesi-Ferretti (2004) shows that non-transparent budgeting practices lead to more creative accounting and less fiscal adjustment.

We study factors that lead to the accumulation of government arrears over time by exploiting a novel dataset assembled by the Spanish Finance Ministry under the Supplier Payment Plan (SPP), a policy implemented over 2012-13. The plan aimed to clear all arrears accumulated by regional and local governments over time until the end of 2011.[19] For the details of the SPP, see Chapter 2.

We focus on the local government sample because its cross-sectional dimension (circa 7,500) is much larger than that of the regional government sample (14).

A number of features of the SPP make it particularly attractive for study. First and foremost, it allows us to use actual arrears data, which offers a key advantage over most of the existing empirical literature that use estimated arrears, with the notable exception of IMF (2019a and 2019b) that uses survey data. Second, the total amount of arrears as of December 2011 was large enough on an absolute basis as well as relative to history (Figure 2) to plausibly infer a detectable effect from some of the key potential determinants suggested in the literature. Third, focus on one single country should help reduce potential omitted variable issues driven by country specificities. Finally, the panel's large cross-section in the local government sample offers the potential for much greater variation across space than over time and variation more plausibly exogenous, in line with the literature on subnational fiscal spending multipliers (for a review, see

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[19] We use "subnational" and "sub-sovereign" interchangeably throughout. Both terms refer to any governmental entities below the central government level. In the case of Spain, subnational or subsovereign entities could be regions (*Comunidades Autónomas*), provinces (*Diputaciones Provinciales*), municipalities (*Entidades Locales*), or groupings of municipalities (e.g., *Mancomunidades*). For regions, we use "regions" and "regional governments" interchangeably throughout. For municipalities, we use "local governments" and "municipalities" interchangeably throughout. The term "municipality" is broadest as it includes all independent local governments below the province level, regardless of their size. For example, the term "city" is more restrictive as it just refers to municipalities with a population above 10,000 inhabitants (see glosario IGN-AGE - Instituto Geográfico Nacional).

Chodorow-Reich, 2019). This variation makes a panel econometric strategy possible.

In this Chapter, we provide suggestive evidence that the extent and probability of arrears accumulation by Spanish municipalities through 2011 were positively influenced by construction-related unemployment (a gauge for construction activity and an early indicator of the 2008-14 crisis in Spain; see Banco de España, 2017, and IMF, 2012), current spending (in line with available cross-country evidence suggesting that the bulk of government arrears typically centers around obligations to suppliers; see Lacey, Massad, Mastruzzi, & Utz, 2022), and interest payments (a commonly accepted indicator of sub-sovereign creditworthiness; see Liu & Tan, 2009). Additionally, we also find that construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin (how much arrears municipalities accumulate). Finally, while our analysis indicates a positive relationship between arrears and the level of interest payments, it also reveals a negative relationship with their deviation from the within-group mean. This suggests that municipalities with elevated interest payments are more likely to experience a significant reduction in market access or access to bank credit relative to their peers. Interpreting higher interest payments as indicative of credit constraints, this finding implies that credit-constrained municipalities may compensate for reduced market access by delaying payments to their suppliers, thereby accumulating arrears.

We use a standard pooled ordinary least squares (POLS) estimator with both time (year) and province-level fixed effects in our benchmark (continuous-form) specification, and find our three variables of interest to be both highly significant and positively related to changes in the flow of government arrears. To reduce endogeneity concerns – given the influence of the accumulation of arrears

on economic activity documented in the empirical literature – we also use an instrumental variables (IV) approach. In particular, we use a two-step least squares (2SLS) estimator, with the first lags of our variables of interest as the respective instruments. Our results, in terms of both significance and signs, remain broadly unchanged.

Furthermore, our results are also robust to an alternative (binary) specification, to the addition of controls for each mayor's political party affiliation even at the cost of a reduced sample size, and to the use of alternative estimators (including the logistic, within fixed-effects, and two-step system generalized method of moments (GMM) estimators). The 2SLS results are also robust to the use of further lags as alternative instruments.

The rest of the paper is organized as follows. In Section 3.2, we discuss our contributions to the related literature. Section 3.3 provides background information on the institutional setting. Section 3.4 describes the data used for the analysis. The empirical strategy is discussed in Section 3.5 and the main results are shown and discussed in Section 3.6. In Section 3.7 we assess the robustness of our main results, and Section 3.8 concludes.

3.2. Related Literature

The literature on expenditure arrears focuses mostly on the measurement of expenditure arrears under different systems of national accounts in developing economies (Diamond & Schiller, 1991), on their economic impact both at micro and macro levels (Checherita-Westphal, Klemm, & Viefers, 2016; Connell, 2014; IMF, 2019a), and on ways to reduce their levels (Ramos, 1998).

Flynn & Pessoa (2014) provide a narrative account of the causes and consequences of the accumulation of government arrears, but empirical evidence about their determinants is still scarce. Using data for 27 European countries over 1990-2012 compiled by Checcherita-Westphal, Klemm, & Viefers (2016), Lee (2016) finds government arrears to be positively determined by both government debt and a measure of voice and accountability, but negatively determined by GDP growth, government bond yields, and measures of political stability, non-violence and rule of law.[20] In addition, using country-level data from sub-Saharan Africa over 2005-2018, IMF (2019b) finds the accumulation of particularly large stocks of arrears are often driven by exogenous factors such as adverse terms-of-trade shocks and political instability. Finally, using data from 6,700 Italian municipalities for 2003-10, Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) provide empirical evidence linking lower intergovernmental transfers to the increased use of arrears in public investment expenditures by municipalities. In this context, our research contributes to this literature by highlighting the role of financial constraints as a pivotal factor in the accumulation of government expenditure arrears.

A key challenge in assessing either the causes or the consequences of domestic arrears – which helps explain the limited empirical evidence available – is the lack of actual arrears data, compounded by a lack of standardized definitions and coverage, which is often the result of weak fiscal accounting systems and irregular audits (IMF, 2019a). A survey of 121 low- and middle-income countries finds that only 12% of countries regularly publish reliable data

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[20] Lee (2016) pulls "voice and accountability", "political stability and non-violence", and "rule of law" scores from the Word Bank's Worldwide Governance Indicators (WGI). See WGI 2022 Interactive > Home (worldbank.org).

on both the amount and the age of arrears.[21] In the case of Europe, it is illustrative that Eurostat's Sector Accounts data do not report arrears data but just data on "other accounts payable" (ESA-1995 code AF.7), which includes all unpaid bills, whether they are in arrears or not.[22]

So far, the empirical literature has used two different proxies to estimate expenditure arrears. On the one hand, Checherita-Westphal, Klemm, & Viefers (2016) propose a measure, which is also used by Lee (2016), that merges annual, per-country, stock data on "other accounts payable" from Eurostat and survey data on average payment duration from Intrum Justitia, a private credit management company. On the other, Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) exploit annual, stock data from Italian municipalities on "outstanding payments for investment expenditures," which includes all unpaid bills (not just arrears) but also excludes data on "current expenditures". IMF (2019a and 2019b) is to the best of our knowledge, the first empirical work to use actual data on government arrears, following a country-level survey. However, our analysis benefits from a significantly broader cross-section, encompassing over 7,500 entities, compared to the IMF's panel with a cross-section of just 30. Moreover, while the IMF's dataset may be considered incomplete due to the inconsistent monitoring of arrears across most Sub-Saharan nations and the variability in arrears definitions among countries (IMF, 2019b), our dataset not only offers comprehensive coverage but also ensures uniformity and consistency

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[21] According to scoring under Public Expenditure and Financial Accountability (PEFA) assessments for 121 countries, as compiled and reported in Flynn & Pessoa (2014). See Homepage | Public Expenditure and Financial Accountability (PEFA).

[22] See Checherita-Westphal, Klemm, & Viefers (2016) for a discussion of expenditure arrears in the context of ESA-1995 accounting. Data availability is, however, improving. For example, the Bank of Canada and the Bank of England have recently started to include government arrears data compiled by the IMF in their Sovereign Default Database; see Bank of Canada (2020).

in the definition of arrears across all municipalities in Spain. This distinction enhances the robustness of our results.

Finally, our research also contributes to the literature on fiscal federalism (for a review, see Ter-Minassian, 2015), particularly addressing the fiscal risks posed by subnational governments. These entities can generate considerable fiscal risks for central governments on two main fronts: firstly, through the quality of public service delivery at the subnational level, and secondly, via the potential strain on central government resources stemming from unsustainable subnational fiscal practices (Saxena, 2022). Bova, Ruiz-Arranz, Toscani, & Ture (2016), within their global database covering contingent liability realizations from 1990-2014, identified 13 instances where contingent liabilities from subnational governments materialized, averaging a cost of 3.7% of GDP. This discussion naturally extends to the topic of subnational fiscal rules. De Biase & Dougherty (2022) survey both the academic literature and recent trends within OECD countries, noting that fiscal regulations at the subnational level are typically less stringent than those enforced at the national level. Specifically, in the context of Spain between 2002-15, Lledó, Delgado-Téllez, & Pérez (2017) observed that regions frequently failed to adhere to fiscal deficit targets, a trend that proved to be persistent; moreover, they find no substantial evidence that regional fiscal rules effectively mitigated such non-compliance. Bastida, Benito, & Guillamón (2015) find similar evidence for the case of Spanish municipalities. In this context, our research highlights the critical need to account for all liabilities, including those beyond financial debt, differentiating payables that are in arrears from those that are not, to preclude subnational governments from bypassing inadequately designed fiscal rules through delayed payments to suppliers and the subsequent accrual of arrears.

3.3. Institutional Setting

For a comprehensive account of the institutional setting, see Chapter 2.

3.4. Data Description

We discuss first the sample and then the descriptive statistics.

3.4.1. Sample

Our sample consists of a full panel of all the municipalities that participated in the first phase (2012) of the SPP. Our dataset includes data on the stock of arrears per municipality as of December 2011 (from the *Mecanismos de Financiación de las Entidades Locales* database at the Finance Ministry), as well as a breakdown at the unpaid bill level with details on amounts and dates that keeps the identity of the suppliers anonymous (obtained following a request made to the Finance Ministry). This allows us to allocate each unpaid bill in arrears to the year the goods or services were acquired by each municipality. While some bills are dated as far back as the 1970s, most of them are dated since 2005 (Figure 2). The structure of the dataset allows us to use yearly data spanning 2005-2011 (seven years) in our main specification. All yearly data are measured as of the end of December.

The dates are the original bill issue date, not the time when a particular bill is considered to be in arrears. In practice, the time between the two dates may

vary across sectors and countries, depending on customs, traditions and regulations.[23]

We merge the arrears dataset just described with data from three other databases available at the Finance Ministry. The first is the CONPREL (*Consulta de Presupuestos y Liquidaciones de Entidades Locales*) database, which provides maximum granularity on municipality-level revenues and expenditures, comparing annual actual versus budget data, as well as population data. The second database provides municipality-level data on the stock of financial debt (*Deuda Viva de las Entidades Locales*), available only since 2008. The third database (*Consulta de Información Impositiva Municipal*) provides municipality-level data on annual property tax rates (*Impuesto de Bienes Inmuebles* or IBI) for urban properties.

We also draw on some complementary databases, including the SEPE (*Servicio Público de Empleo Estatal*) database, which provides both employment and unemployment data, per sector, at the municipality level. For 2001 Census data on crime and habitability rates, and average household size per municipality, we use the INE (*Instituto Nacional de Estadística*). For municipality-level data on the allocation of funds from the state-run 'Plan E' over 2009-10, we use the Carozzi & Repetto's (2019) dataset.[24] A Spanish Regional Policy Ministry

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[23] The Spanish Law 15/2010 passed on 5th July 2010 sets ceilings for supplier payment periods for both the private and public sectors. While the general limit is 60 days, there are specific and shorter limits for both fresh products and the public sector (30 days). At the time of Spain's SPP, the average payment period was significantly beyond the legally-established limits (see Figure 1; for municipality-specific evidence, see also Delgado-Téllez, Hernández de Cos, Hurtado, & Pérez, 2015). See also Intrum (2011) for average payment period data for both private and public sectors, comparing payment terms offered vs. actual payment times, across European countries in 2011.

[24] The 'Plan E' (*Plan Español para el Estímulo de la Economía y el Empleo*) was a large stimulus plan aimed at boosting economic activity and employment in the middle of the Great Financial Crisis (GFC) that was announced by the Spanish government in November 2008. The plan was carried out in two subsequent steps over a two-year period (2009-10): The State Fund for Local Investments (*Fondo Estatal de Inversión Local* or FEIL) provided local governments with €8 billion for investments in 2009. Then, the State Fund for Local Employment and Sustainability (*Fondo*

database (*Sistema de Información Local*) provided information on the political affiliation of mayors elected in May 2007 for the 2007-11 term. Finally, we also add ages of mayors at the time of their election, following a 'request of public information' made through the Spanish Government's *Portal de la Transparencia*.

3.4.2. Descriptive Statistics

Tables 1 and 2 detail summary statistics for two subsamples of the main sample. We measure arrears in flow terms, defined as changes in the stock, per capita (see Table 27 (Appendix) for variable definitions).

Table 1 details summary statistics for a subsample of municipalities with arrears outstanding as of December 2011. In this subsample, the average municipality has arrears outstanding worth 353 euros per capita (standard deviation of 535); 9,354 inhabitants (standard deviation of 62,541); an employment rate of circa 26% of the population (standard deviation of 14%); a primary budget deficit of 0.38 euros per capita (standard deviation of 284); a stock of financial debt worth 39% of current revenues (standard deviation of 38%); and interest payments of 1.7% of current revenues (standard deviation of 1.8%).

Table 1. Summary Statistics of Local Governments <u>with Arrears</u> Outstanding as of December 2011

	Mean	SD	p25	Median	p75	N
Arrears pc	352.592	534.625	85.819	202.561	429.490	3833
Employment pc	0.259	0.140	0.173	0.232	0.304	3833
Unemployment (Construction) pc	0.016	0.012	0.008	0.014	0.021	3832
Crime Rate (%)	6.517	9.067	0.526	2.883	8.835	3830
Habitability Index (0-100)	59.713	7.406	55.470	59.950	63.980	3830
Population	9353.958	62540.999	387.000	1344.000	5066.000	3833
Household Size	2.683	0.341	2.462	2.699	2.907	3830
Financial Debt pc	326.877	370.464	57.444	232.693	467.564	3833
Financial Debt (% Current Revenues)	38.573	37.934	8.961	30.420	57.210	3546
Interest Payments pc	14.127	16.912	3.838	9.381	18.716	3565
Interest Payments (% Current Revenues)	1.651	1.775	0.506	1.177	2.285	3379
Primary Spending pc	1169.563	787.145	775.335	959.208	1292.313	3565
Current Spending pc	379.570	220.940	242.152	328.971	448.265	3770
Capital Spending pc	384.810	643.963	108.104	210.890	422.198	3766
Personnel Spending pc	362.689	172.094	261.390	331.863	423.475	3770
Transfers (Spending) pc	69.357	144.492	23.163	45.496	79.215	3518
Tax Revenues pc	469.331	288.329	307.862	413.269	563.194	3833
Transfers (Revenues) pc	654.845	706.980	329.955	463.973	717.324	3753
Property Tax (%)	0.600	0.155	0.500	0.600	0.700	3812
Plan E pc 2009-10	5051.797	133587.557	25.778	139.431	828.455	3833
Primary Deficit pc	0.380	284.359	-85.871	-11.214	71.719	3565
Primary Deficit (vs Budget) pc	51.740	310.655	-41.129	29.926	123.658	3218
Treasury Position pc	58.279	545.440	-84.302	23.250	162.804	3769
PP Mayor 2007-11	0.333	0.471	0.000	0.000	1.000	3833
PSOE Mayor 2007-11	0.205	0.229	0.000	0.000	0.462	3833
Age Mayor 2007-11	50.278	9.910	43.000	50.000	57.000	3430

Notes: This Table reports mean, standard deviation, 25th-percentile, median, 75th-percentile, and number of observations for each variable, by municipality. All variables are defined in Table 27 (Appendix).

Table 2 details summary statistics for a subsample of municipalities that did not have arrears outstanding as of December 2011. In this subsample, the average municipality has 2,266 inhabitants (standard deviation of 27,731); an employment rate of 28.4% of the population (standard deviation of 22%); a primary budget surplus of 47 euros per capita (standard deviation of 547); a stock of financial debt worth 15% of current revenues (standard deviation of 30%); and interest payments of 0.7% of current revenues (standard deviation of 1.3%).

Table 2. Summary Statistics of Local Governments <u>without Arrears</u> Outstanding as of December 2011

	Mean	SD	p25	Median	p75	N
Arrears pc	0.000	0.000	0.000	0.000	0.000	3752
Employment pc	0.284	0.220	0.160	0.217	0.306	3752
Unemployment (Construction) pc	0.011	0.012	0.000	0.009	0.016	3752
Crime Rate (%)	3.365	6.372	0.000	0.752	3.922	3748
Habitability Index (0-100)	57.621	8.284	52.400	57.445	62.575	3748
Population	2266.000	27731.028	107.000	259.000	870.000	3752
Household Size	2.546	0.387	2.304	2.547	2.795	3748
Financial Debt pc	141.709	398.096	0.000	0.000	139.674	3752
Financial Debt (% Current Revenues)	14.707	29.531	0.000	0.000	19.773	3059
Interest Payments pc	7.131	18.097	0.586	2.675	8.392	2836
Interest Payments (% Current Revenues)	0.734	1.319	0.074	0.332	0.967	2424
Primary Spending pc	1365.120	1191.598	801.702	1056.562	1515.519	2836
Current Spending pc	469.541	335.167	282.388	383.039	544.443	3696
Capital Spending pc	574.463	970.243	140.259	304.891	654.195	3668
Personnel Spending pc	322.988	207.061	207.560	282.395	378.488	3682
Transfers (Spending) pc	75.228	150.054	24.637	48.273	86.254	2748
Tax Revenues pc	558.076	1130.654	292.311	412.764	603.429	3752
Transfers (Revenues) pc	758.041	910.898	349.491	520.741	848.341	3631
Property Tax Rate (%)	0.556	0.143	0.400	0.550	0.650	3712
Plan E pc 2009-10	9529.082	83487.139	120.447	572.398	2818.148	3752
Primary Deficit pc	-46.859	547.099	-122.191	-33.689	50.208	2836
Primary Deficit (vs Budget) pc	-18.171	563.951	-91.589	-7.194	75.487	2537
Treasury Position pc	725.122	2214.667	104.500	295.393	720.909	3693
PP Mayor 2007-11	0.490	0.500	0.000	0.000	1.000	3752
PSOE Mayor 2007-11	0.142	0.212	0.000	0.000	0.462	3752
Age Mayor 2007-11	53.080	11.175	46.000	52.000	61.000	3243

Notes: This Table reports mean, standard deviation, 25th-percentile, median, 75th-percentile and number of observations for each variable, by municipality. All variables are defined in Table 27 (Appendix).

3.5. Model Specification

We estimate our main model under two alternative specifications: A continuous-form and a binary specification.

3.5.1. Benchmark (Continuous-Form) Specification

We use *i* to index municipalities, *j* to index provinces where municipalities are located, and *t* to index time. A_{ijt} denotes the amount of arrears per capita in municipality *i* of province *j* in year *t*. Letting V_{ijt-1} stand for a vector with our variables of interest in municipality *i* of province *j* in year *t*, we estimate the parameter β_1 in the regression:

$$A_{ijt} = \beta_1 V_{ijt-1} + \beta_2 C_{ijt-1} + \propto_i + \propto_j + \propto_t + \epsilon_{ijt} \qquad (1)$$

Time dummy variables α_t allow for arbitrary common time variation. C_{ijt-1} is a vector of control variables that aim to capture heterogeneity in the evolution of arrears per capita across municipalities. While most controls are time-varying, some are time-invariant. In our main specification, we allow for province-specific fixed effects α_j, and estimate Equation (1) using the standard pooled-OLS estimator, that is, one that imposes $\alpha_i = \alpha$ for all i. Most regressors are lagged by one period in order to address potential endogeneity issues. We use panel-robust standard errors in all estimations.

Our three variables of interest include construction-related unemployment, interest payments, and current spending, all of them at the municipality level.

Construction-related unemployment per capita is a measure of construction-related activity at the municipality level.[25] The burst of a real estate bubble was a prime contributor to the 2008-14 financial crisis in Spain (for details, see Banco de España, 2017); house prices fell by circa 40% over that period. This had two major implications for municipalities in Spain. Meaningful fiscal imbalances developed with the collapse of real estate-related revenues (IMF, 2012).[26] And then followed a banking crisis related to the real estate crisis, leading to tighter domestic credit conditions for both firms and households (see Bentolila, Jansen, & Jiménez, 2018; and Jiménez, Ongena, Peydró, & Saurina, 2014).

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[25] Many geographical cross-sectional studies focus on employment rather than on output variables due to data availability issues at the local level. For a discussion in the literature on subnational fiscal multipliers, see Chodorow-Reich (2019).

[26] Real estate-related revenues (*ingresos asociados a la actividad urbanística*) accounted for 11.5% of local governments' overall revenue base in 2006 – prior to the crisis, ranging from 16.5% at municipalities with over 1 million inhabitants to 8.6% at municipalities with under 5,000 inhabitants (Ministerio de Economía y Hacienda, 2008).

We hypothesize this also fed through to subnational governments' funding conditions, impairing their access to funding. This seems plausible, and could help explain the build-up of arrears as a substitute for lower access to capital and/or bank credit markets. As Caner & Mokhtari (2000) point out in an emerging markets context, arrears provide a cheap funding alternative in the face of liquidity shortages and credit constraints. In order to test for the existence of this channel, and of financial constraints more broadly, we add interest payments over current revenues, a variable that is widely used in subnational credit assessments (see Liu & Tan, 2009), to the vector that comprises our variables of interest.

Finally, current government spending per capita is defined as government spending per capita on goods and services. There is ample theoretical and empirical evidence supporting the notion that politicians cannot resist the temptation to skew public spending towards current expenditure during good times, as this is easier to perceive by voters and the political benefits from such a fiscal policy are therefore more immediate (Rogoff, 1990). In this regard, using a panel of 100 developing countries and 30 developed countries between 1980 and 2014, Ardanaz & Izquierdo (2022) find evidence of a positive relationship between upswings and current primary expenditures, albeit only in developing markets. Furthermore, current spending was indeed the main source of expenditure arrears by local Spanish governments prior to 2012, as reported by the Spanish Court of Auditors (Tribunal de Cuentas, 2013), and there is cross-country evidence showing this also tends to be the case internationally (see Lacey, Massad, Mastruzzi, & Utz, 2022).

Our standard set of municipality-level control variables consists of overall employment data; fiscal variables (changes in the treasury position, tax revenues, transfers received, and deviations in the actual primary balance from the budget);

social variables (crime and habitability rates); demographic variables (population and household size); and others such as 'Plan E' funds received in 2009-10, property tax rates, and a dummy variable that controls for population thresholds (under 5k, 5-20k, 21-50k, 51-75k, and above 75k) associated with changes in the level of central government transfers.[27]

All variables are in continuous form, except for crime, habitability rates, and household size, which are all time-invariant. All macroeconomic and fiscal variables are per capita, except for interest payments, which is normalized by the level of current revenues. All variables are in logs, except for changes in the treasury position, deviations in actual primary balance from the budget, and 'Plan E' transfers, which are all in levels. Both explanatory variables and controls are lagged by one year, except for changes in the treasury position and deviations in actual primary balance from the budget, which are contemporaneous.

All variables are defined in Table 27 (Appendix).

3.5.2. Alternative (Binary) Specification

In addition to the continuous-form dependent variable A_{ijt} (1), we also estimate a specification of the form:

$$ABinary_{ijt} = \beta_1 V_{ijt-1} + \beta_2 C_{ijt-1} + \propto_i + \propto_j + \propto_t + \epsilon_{ijt} \qquad (2)$$

where $ABinary_{ijt}$ is a dummy variable that takes a value of one if arrears per capita for municipality *i* of province *j* at time *t* are positive, and zero otherwise.

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[27] See RDL 2/2004 for population thresholds related to the level of State (i.e., central government) transfers per municipality.

3.6. Empirical Analyses

First, we use the Equation (1) continuous-form specification to explore the factors that drive the *level* of expenditure arrears incurred at the municipality level. Then, we use the Equation (2) binary specification to explore the factors that increase the *likelihood* of arrear generation, also at the municipality level.

3.6.1. Drivers of Level of Arrears

We start by exploring the effects of our variables of interest on the level of government arrears at the municipality level. For this, we estimate Equation (1) with a standard pooled OLS (POLS) regression with both time (year) and province-level fixed effects. Table 3 shows the results.

Table 3. Pooled OLS Regression: Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.08*			.09*
	(.05)			(.05)
Current Spending pc $_{t-1}$		.55***		.60***
		(.08)		(.08)
Interest Payments (% Current Revenues) $_{t-1}$			.14***	.14***
			(.02)	(.02)
Observations	8192	8370	8009	7869
Within R^2	.47	.47	.47	.47
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports POLS estimates of the effect of a vector of covariates on the flow of local government expenditure arrears in a panel spanning over seven years (2005-11) and 3.833 municipalities in Spain. This includes all Spanish municipalities with government expenditure arrears outstanding in any of the years considered. The dependent variable is the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, the overall level of employment, population, household size, the property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are

contemporaneous. All variables are defined in <u>Table 27 (Appendix)</u>. Robust standard errors are shown in parentheses. *** $p<.01$, ** $p<.05$, * $p<.1$

Column (1) shows a positive relation between construction activity (measured as construction-related unemployment per capita) and government arrears. The reason for this is threefold: First, to the extent that construction activity captures broader economic dynamics, weaker activity is normally associated with higher (lower) budget deficits (surpluses), if only due to the role of the so-called automatic stabilizers. Second, weaker construction activity is particularly relevant for local governments in Spain as construction-related revenues account for a large part of their overall revenue base. Third, there is evidence that credit conditions worsened particularly for borrowers that were dependent on banks with the largest exposures to construction. Presumably, such borrowers were concentrated in areas where the weight of construction in overall economic activity was largest. The combination of these three channels may plausibly lead to the accumulation of arrears with suppliers. The coefficient is 0.08, implying that a 1% increase in construction-related unemployment in $t-1$ translates into a 0.08% increase in the flow of arrears in t. The coefficient is both statistically and economically significant.

Using a similar approach for a panel of sub-Saharan countries, IMF (2019b) estimates a coefficient of -0.28 for lagged real GDP growth (highly significant). That is, a 1% drop in real GDP growth in $t-1$ translates into a 0.28% increase in government arrears in t. Importantly, we note that while the authors use the stock of arrears as the dependent variable, we use the flow of arrears (defined as changes in the stock). For a panel of Italian municipalities, Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) estimate a coefficient of -0.09 for *taxable income* (highly significant) when the annual percentage change in the

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stock of arrears is the dependent variable. These results are broadly consistent with our own findings, with their estimated coefficient being remarkably similar to ours.

Column (2) shows a positive relation between current spending for the acquisition of goods and services (per capita) and government arrears. The bulk of unpaid bills with suppliers, regardless of whether they end up being in arrears or not, originated from this item. Therefore, it makes sense that the larger this item, the greater the arrears, all else equal. The coefficient is 0.55, implying that a 1% increase in current spending for the acquisition of goods and services in $t - 1$ translates into a 0.55% increase in the flow of arrears in t. The coefficient is both statistically and economically significant.

Column (3) shows a positive relation between interest payments (as a percentage of current revenues) and government arrears. Interest payments are a critical measure of a borrower's credit profile and capacity for access to credit. Higher (lower) interest payments likely reflect a worse (better) credit profile and therefore the presence of higher (lower) credit constraints. A positive relation means that the higher (lower) the credit constraints, the greater (smaller) the government's incentive to fund its activities through payment delays and arrears with suppliers. The coefficient is 0.14, implying that a 1% increase in interest payments in $t - 1$ translates into a 0.14% increase in the flow of arrears in t. The coefficient is both statistically and economically significant.

Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) focus on arrears from capex-related payments (our focus is on current spending on goods and services)[28] and estimate positive coefficients for both capex (highly significant)

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[28] We find no significant results for capital expenditures. This is likely due to the different setting.

and interest payments (insignificant) when the stock of arrears is the dependent variable. We see these results as broadly consistent with ours.

Finally, column (4) shows that our previous results (point estimates in columns 1 to 3) hold when we look at the combined effect of our three variables of interest. The magnitude of the coefficients remain broadly unchanged. They also remain both statistically and economically significant.

There is, however, a potential endogeneity problem, as these variables may themselves also be influenced by the level of arrears. Not least, there is ample empirical evidence suggesting that the accumulation of arrears may have significant and negative broad-based economic effects, as the accumulation of arrears tends to reduce corporate profits, increase the likelihood of corporate bankruptcies, and constrain overall economic growth (Checherita-Westphal, Klemm, & Viefers, 2016; Connell, 2014), with negative second-round effects on bank credit quality, corporate tax compliance, and the quality of public service delivery (IMF, 2019a).

In the presence of simultaneous correlation, the pooled OLS estimator will be both biased and inconsistent. To overcome this problem, we next follow an instrumental variable approach. For this, we run a two-stage least squares (2SLS) regression with both time (year) and province-level fixed effects, where we instrument each of our variables of interest with their respective first lags, following Anderson & Hsiao (1981) and Todd & Wolpin (2003). By using one lag of the independent variable, we reduce potential simultaneity issues, as the regressors will be pre-determined in an earlier period. By using higher lags, we face a trade-off, however: We reduce potential simultaneity further but the relevance of instruments may be reduced too; we discuss this in our robustness

Others?
There are some
critiques...

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analysis in Section 3.7. The results, which are shown in Table 4, broadly confirm our first findings.

Table 4. 2SLS Regression: Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.14**			.11
	(.07)			(.07)
Current Spending pc $_{t-1}$		.87***		.97***
		(.13)		(.14)
Interest Payments (% Current Revenues) $_{t-1}$			.25***	.29***
			(.03)	(.03)
Observations	7471	7463	7023	6813
R-squared	.33	.34	.35	.35
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports IV estimates of the effect of a vector of covariates on the flow of local government expenditure arrears in a panel spanning over seven years (2005-11) and 3.833 municipalities in Spain. This includes all Spanish municipalities with government expenditure arrears outstanding in any of the years considered. The dependent variable is the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. The explanatory variables are instrumented, with their first lags being used as their own instruments. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability index, the overall level of employment, population, household size, the property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Standard errors clustered at the municipality level are shown in parentheses. *** p<.01, ** p<.05, * p<.1

There are two main differences compared to our POLS regression. First, coefficients estimated with the 2SLS regression are meaningfully larger. This is probably because either an omitted variable is negatively correlated with our variables of interest or there are measurement errors (e.g., construction-related unemployment is arguably a noisy measure of broader construction activity), leading to a downward bias in the POLS estimates of the coefficients.[29] Second,

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[29] For example, in the returns-to-education literature, the usual result is that the 2SLS estimate is larger than the OLS estimate by approximately 25%-50% (see, e.g., Card, 1999 and 2001).

when we look at the combined effect of our three variables of interest (column 4), the coefficient for construction loses significance although the point estimate is of similar magnitude to that in column (1), and slightly larger than those in columns (1) and (4) of Table 3.

Finally, Table 5 shows results of the first-stage regressions. The large and statistically-significant coefficients imply the relevance condition of the 2SLS regression is satisfied.

Table 5. First Stage: Relevance for Instruments in the 2SLS Regressions

	(1) Unemployment (Construction) pc $_{t-2}$	(2) Current Spending pc $_{t-2}$	(3) Interest Payments (% Current Revenues) $_{t-2}$
Unemployment (Construction) pc $_{t-1}$	.68*** (.01)		
Current Spending pc $_{t-1}$		.69*** (.01)	
Interest Payments (% Current Revenues) $_{t-1}$			.67*** (.01)
Observations	16561	18073	16014
Within R^2	.58	.20	.11
Municipality-level Controls	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov

Notes: In each column, we POLS-regress each of our three variables of interest over their respective first lags, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

3.6.2. Drivers of Likelihood of Arrears

In the previous section, we estimated our model under a continuous-form specification [Equation (1)]. This allowed us to estimate, within the subset of municipalities that had generated arrears in any year of our panel, what drivers explain relative cross-sectional differences in the *level* of government arrears. Next, we complement our analysis by also estimating a binary-form specification [Equation (2)] to see whether there are any differences in the drivers that explain the *likelihood* of arrears, that is, whether some municipalities accumulate arrears, regardless of the size of such arrears, while others do not.

For this, we first estimate Equation (2) using a standard POLS regression with both time (year) and province-level fixed effects. Given the binary specification of our dependent variable, this is better known as a Linear Probability Model (LPM).

Table 6 shows the results. The three variables of interest are both highly significant and – as one would expect – positively related to the level of arrears, just as in the benchmark (continuous-form) specification. Columns (1) to (3) show that a 1% increase in any of the three variables of interest (construction-related unemployment, current spending, and interest payments) increases the probability of accumulating arrears by 3, 2 and 4 percentage points, respectively. In terms of the combined effect (column 4), only the coefficient of current spending changes, and rises from 2 to 3 percentage points.

Table 6. LPM Regression: Binary Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.03***			.03***
	(.01)			(.01)
Current Spending pc $_{t-1}$		.02**		.03***
		(.01)		(.01)
Interest Payments (% Current Revenues) $_{t-1}$			.04***	.04***
			(0)	(0)
Observations	20341	22142	20153	18825
Within R^2	.22	.21	.22	.22
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports LPM estimates of the effect of a vector of covariates on the likelihood that local government expenditure arrears are originated, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. The dependent variable is a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Construction-related unemployment, however, is very significant now compared to weakly significant in the benchmark continuous-form specification. This means that weak construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin (how much arrears are accumulated per municipality).

Then, to minimize endogeneity concerns, we also run a two-stage least squares (2SLS) regression with both year (time) and province-level fixed effects,

instrumenting each variable with its first lag.[30] The results, shown in Table 7, confirm our previous findings. The three variables of interest are both highly significant and positively related to the level of arrears. Columns (1) to (3) show that a 1% increase in any of the three variables of interest (unemployment-related construction, current spending, and interest payments) increases the probability of accumulating arrears by 4, 9 and 7 percentage points, respectively. In terms of the combined effect (column (4)), only the coefficient of interest payments changes, and rises from 7 to 8 percentage points. We note the 2SLS estimates are again larger than the POLS estimates (just as with the continuous-form specification, and for the same reasons discussed there), and that the construction coefficient becomes very significant now (insignificant under the continuous-form specification – see column (4) in Table 4) for the same reason discussed above for our POLS estimates under the binary-form specification.

Table 7. 2SLS Regression: Binary Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.04***			.04***
	(.01)			(.01)
Current Spending pc $_{t-1}$		.09***		.09***
		(.02)		(.02)
Interest Payments (% Current Revenues) $_{t-1}$			.07***	.08***
			(0)	(0)
Observations	16561	18073	16014	14564
R-squared	.25	.27	.30	.28
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports IV estimates of the effect of a vector of covariates on the likelihood that local government expenditure arrears are originated, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. The dependent variable is a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. The explanatory variables are instrumented, with their first lags being used as their own instruments. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget,

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[30] Table 5 shows results of the first-stage regressions. The large and statistically-significant coefficients imply the relevance condition of the 2SLS regression is satisfied.

'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Standard errors clustered at the municipality level are shown in parentheses. *** p<.01, ** p<.05, * p<.1

3.7. Validity of the Empirical Strategy and Robustness Checks

We can provide further evidence that our variables of interest drive both the level and the likelihood of arrears, if to different degrees. First, we experiment with two alternative regressors (employment and financial debt) that are similar to two of our variables of interest (construction-related unemployment and interest payments, respectively) and find similar results in both cases. Second, we expand our set of municipality-level controls by also controlling for each mayor's political affiliation, something that we did not do in our preferred specifications in order to avoid excessive sample attrition. We find no material difference in our results. Third, we also experiment with a suite of alternative estimators including a logistic regression for the binary specification; using second lags as instruments in our 2SLS estimations; the within fixed-effects estimator; and the two-step system GMM estimator. Once again, we find no material differences in the results.

3.7.1. Alternative Regressors

We experiment with two alternative regressors in our preferred (benchmark and alternative) specifications: Employment and financial debt.

A. *Employment*

We examine the explanatory power of the overall rate of employment per capita, as a key measure of general economic activity at the municipality level. This would be consistent with the empirical literature on the causation of government arrears, where lagged real GDP growth is widely used as a key explanatory variable of government arrears (see, e.g., IMF, 2019b; and Lee, 2016). Recall that in our preferred specifications, we have already included the overall employment rate per capita among the municipality-level controls.

Results are shown in Table 28 (Appendix). Columns (1) and (2) include the results of the benchmark (continuous-form) specification estimated, respectively, with a standard pooled OLS and a 2SLS (using its first lag as the regressor's instrument). Columns (3) and (4) include the results of the alternative (binary) specification estimated, respectively, with a standard pooled OLS (effectively a LPM) and a 2SLS (using its first lag as the regressor's instrument). All regressions include time (year) and province-level fixed effects, as well as the standard set of municipality-level controls defined earlier. The regressor's coefficient is highly significant and has a negative coefficient across all specifications and estimators. Finally, Column (5) includes the results of the first-stage regression, suggesting that the relevance condition of the instrument used in the 2SLS regressions is satisfied.

B. *Financial Debt*

We also look at the explanatory power of the stock of financial debt (as a proportion of current revenues), as a widely accepted indicator of credit quality and financial health (see Liu & Tan, 2009) that is also used in the literature on the causation of government arrears (see, e.g., Lee, 2016).[31] In our preferred specification, we used interest payments (as a percentage of current revenues) as a proxy for financial debt. The reason for that choice is that while municipality-level financial debt data are available only since 2008 (four years), interest payments data are available since 2005 (seven years).

Sample size is meaningfully reduced when we use financial debt as a variable of interest. The number of observations drops by 34%, from 8,009 in our main specification using interest payments (see column (3) of Table 3) to 5,317 using financial debt (see column (1) in Table 29 (Appendix)).

In Table 29 (Appendix), columns (1) and (2) show the results of the benchmark (continuous-form) specification estimated, respectively, with a standard pooled OLS and a 2SLS (using its first lag as the regressor's instrument). Columns (3) and (4) show the results of the alternative (binary) specification estimated, respectively, with a standard pooled OLS (effectively a LPM) and a 2SLS (using its first lag as the regressor's instrument). All include time (year) and province-level fixed effects, as well as the previous set of municipality-level controls. The regressor's coefficient is highly significant and positive across all specifications and estimators. Finally, column (5) provides the results of the first-stage regression, suggesting we have satisfied the relevance condition of the instrument used in the 2SLS regressions.

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[31] In Spain, the pre-2012 set of municipality-level fiscal rules included borrowing limits and a requirement to implement an adjustment plan for municipalities with a level of *financial debt over current revenues* above a certain threshold, which was generally set at 110%. For a detailed description of the pre-2012 set of municipality-level fiscal rules, see Farfán & Velasco (2009).

3.7.2. Additional Municipality-Level Controls

Our preferred specifications do not include any controls for each mayor's political affiliation among the set of municipality-level controls. Given that our political affiliation data begins with the local elections of May 2007, we did not include any political variables in our preferred specifications in order to avoid excessive sample attrition.

However, political affiliation could potentially be relevant. There is empirical evidence suggesting that left-wing administrations tend to adopt expansionary fiscal policies aimed at boosting aggregate demand to achieve full employment, whereas right-wing governments prefer maintaining smaller, balanced budgets to support a market-driven equilibrium for full employment (Alesina & Rosenthal, 1995; Boix, 1997). For a review of fiscal politics more broadly, see Gaspar, Gupta, & Mulas-Granados (2017).

To see whether political considerations could potentially change either the sign or the significance of any of the three variables of interest, we run our preferred specifications adding three additional controls for a panel covering a shorter time, now spanning the years 2007-11 (five years) only. We include dummy variables for the Conservative Party (*Partido Popular* or PP) and the Socialist Party (*Partido Socialista Obrero Español* or PSOE) – Spain's two leading political parties at the time; as well as a dummy variable controlling for age of the mayor. These dummies are generally time-invariant. For instance, the mayor elected in May 2007 typically stays in office for the entire term, that is, until at least May 2011, but not always. Sometimes the officeholder changes during the term, normally in the context of minority governments.

Sample attrition from adding these municipality-level political controls is large. The number of observations falls by 16%, from 7,869 in our main specification without political controls (see column (4) of Table 3) to 6,773 with political controls (see column (1) in Table 30 (Appendix)).

The results in Table 30 (Appendix) broadly confirm our previous results. In column (1) the results of the pooled OLS estimation are similar to those in our preferred specifications, with the exception of construction-related unemployment, which becomes insignificant. The point estimate is still positive, however, and of a similar magnitude to the POLS estimate of the construction coefficient in our main continuous-form specification (i.e., without political controls) from Table 3. In the column (2) (LPM) results, construction-related unemployment becomes highly significant (it was insignificant in our main estimation).

3.7.3. Alternative Estimators

A. *Logistic Regression*

To test whether the results are robust to the potential presence of non-linearities in the data, we also run a logistic regression. The results shown in Table 31 (Appendix) confirm the results in our main LPM estimation under the binary-form specification from Table 6.

B. *2SLS with 2nd Lags as Instruments*

We also test whether our results are robust to the use of further (second) lags as instruments in the 2SLS estimations. Results from the preferred

continuous and binary forms shown, respectively, in Tables 32 and 33 (Appendix) confirm our previous results from Tables 4 and 7, respectively. Table 34 (Appendix) shows the results from the first-stage regressions, which suggests satisfaction of the instruments' relevant condition, although the coefficients are lower than those of the first lags, as one would expect.

C. Within Fixed-Effects Estimator

We also experiment with a within fixed-effects estimator. Introducing municipality-level fixed effects controls for any time-invariant municipality-level characteristics. In turn, this eliminates potential endogeneity concerns from omitted variables that are time invariant at the municipality level. We implement it by dropping all time-invariant controls as well as the province-specific fixed effects α_j from Equations (1) and (2). Results are shown in Table 35 (Appendix).

Column (1) shows the results of the within fixed-effects estimator of a model where the dependent variable is in continuous form. The coefficients of the three variables of interest are all highly significant (just as in the case of our main pooled OLS estimation – see Table 3). The estimated coefficient for interest payments, a proxy for a municipality's cost of funding and capacity to issue new debt and/or access to bank credit, however, becomes negative (positive in the case of our pooled OLS estimator).

Chiades, Greco, Mengotto, Moretti, & Valbonesi (2019) use a similar approach for a sample of Italian municipalities and estimate a coefficient of -0.08 for interest payments when the annual percentage change in the stock of arrears is the dependent variable (compared to the flow of arrears in our case). This is

consistent with our results. Furthermore, the magnitude of their coefficient is remarkably similar to ours (-0.10).

If we mix our pooled OLS and within fixed-effect estimates for the continuous-form specification, we come up with the following global interpretation: Arrears are positively determined by the *level* of interest payments (pooled OLS estimator) but negatively determined by their *deviation from the within-group mean* (within fixed-effects estimator). What this means is that municipalities with higher interest payments also tend to be associated with a sharper loss of market access and/or access to bank credit relative to peers. Interpreting higher interest payments as indicative of credit constraints, this finding implies that credit-constrained municipalities may compensate for reduced market access by delaying payments to their suppliers, thereby accumulating arrears.

Column (2) shows the results of the within fixed-effects estimator of a model where the dependent variable is in binary form. The coefficients of the three variables of interest are all highly significant with a positive sign, in line with the results obtained with the main LPM estimation (see Table 6).

If we mix our within fixed-effect estimates for the binary- and continuous-form specifications, we come up with the following global interpretation: Interest payments are positively related with the extensive margin (how many municipalities accumulate arrears) but negatively related with the intensive margin (the amount of arrears within the municipalities that do accumulate them).

D. Two-Step System GMM Estimator

Given the potential for endogeneity of government arrears discussed earlier, we also use the system GMM (Blundell & Bond, 1998) estimator for dynamic panel models. This approach has also been followed by Checherita-Westphal, Klemm, & Viefers (2016), and Lee (2016) in the government arrears literature, as well as by Brückner & Tuladhar (2014) in the literature on subnational spending multipliers. It is particularly suitable for our panel dataset, given its rather short time dimension (maximum T = 7, spanning the years 2005-2011) and large cross-section dimension (circa 7,500 municipalities). We also correct for heteroskedasticity and autocorrelation that may be present in the error structure by using the consistent (two-step) estimator.

We follow this approach to estimate the model in our preferred continuous-form specification. Results are shown in Table 36 (Appendix). Construction-related unemployment and current spending are both significant at the 5% level with positive signs, as in the main estimation. Interest payments, with a negative sign, is now insignificant (highly significant in our main estimation). We use collapsed second and subsequent lags as instruments. Lower lags do not resolve autocorrelation issues, as evidenced by the rejection of the AR(2) test's null hypothesis. The point estimates fall within the range delineated by the POLS and within fixed-effects estimations (Bond, 2002). Furthermore, the Hansen test's p-value also falls within the range suggested by Roodman (2009) as indicative of an absence of instrument proliferation (overfitting) concerns. Overall, we interpret these results as broadly consistent with those from the within fixed-effect estimator.

Lee (2016) uses a similar approach for a panel of European countries and estimates coefficients with a positive sign for government debt as a percentage of GDP, and with a negative sign for the natural log of GDP at constant prices

(both highly significant), when arrears as a percentage of total payables is the dependent variable. Taken at face value, these results are broadly similar to ours. Yet, we also note that Lee's (2016) results are likely to suffer from instrument proliferation (i.e., overfitting) issues, as suggested by an "implausibly good" *p*-value of 1.000 in the Hansen test (see Roodman, 2009).

3.8. Summary and Conclusions

We provide suggestive evidence that the extent of government arrears accumulation by Spanish municipalities through 2011 (intensive margin) was negatively influenced by construction activity, and positively influenced by current spending and interest payments. Additionally, we also find that construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin. Finally, while our analysis indicates a positive relationship between arrears and the level of interest payments, it also reveals a negative relationship with their deviation from the within-group mean. This suggests that municipalities with elevated interest payments are more likely to experience a significant reduction in market access or access to bank credit relative to their peers. Interpreting higher interest payments as indicative of credit constraints, this finding implies that credit-constrained municipalities may compensate for reduced market access by delaying payments to their suppliers, thereby accumulating arrears.

We use a standard pooled OLS estimator with both time (year) and province-level fixed effects as our benchmark (continuous-form) specification. To reduce endogeneity concerns, given the potential influence of the accumulation of arrears on economic activity, we also use an instrumental variables (IV)

approach; in particular, we use a two-step least squares (2SLS) estimator, with the first lags of our variables of interest as their respective instruments. The results in both cases are similar. The results are also robust to an alternative (binary) specification, to the addition of political controls even at the cost of significant sample attrition, and to the use of alternative estimators (including within fixed-effects and two-step system GMM estimators). In the alternative (binary) specification, results are also robust to the use of a logistic regression, in order to account for the presence of potential non-linearities in the data. For the 2SLS estimator, results are also robust to the use of the second lag of our variables of interest as their respective instruments.

Overall, our findings underscore the importance of establishing effective subnational fiscal rules to tame the build-up of arrears during severe downturns, as that in Spain at the time of the SPP. Essential to this effort, however, is improving the transparency of subnational fiscal data, as the lack thereof may enable subnational governments to bypass regulations through creative accounting practices. Moreover, our research also calls for the adequate design of backstop mechanisms (such as, e.g., risk-sharing platforms) that can help mitigate credit constraints faced by subnational governments while reducing moral hazard.

Chapter 4. Government Arrears and Corporate Decisions: Lessons from a Natural Experiment

4.1. Introduction

Government procurement, the purchase of goods and services on behalf of a public authority, accounts for a substantial part of the global economy. According to the World Bank, public procurement in 2020 represented between 13% and 20% of world GDP, while global expenditure on procurement was close to 9.5 trillion US dollars.[32] Government contracts have some advantages. Among them, that they are important for many small local businesses (García-Santana & Santamaría, 2023) and provide a more stable demand over non-public customers, especially in recession periods (Goldman, 2020). However, governments can be slow in payment, and arrears often accumulate. This indeed occurred during the European sovereign debt crisis that followed the global financial crisis.

While there is an extensive literature on the economics of procurement, the financial aspects of supplier-government relationships are less explored. In this Chapter, we contribute to filling this gap by exploiting a large-scale financing plan of the Spanish government that repaid accumulated arrears of local governments to their suppliers in two different phases. Repayment significantly affects suppliers' corporate investment, leverage, and liquidity and shows heterogeneous responses according to firm-bank linkages. This heterogeneous reaction of firms to the early repayment of arrears allows us to infer how they dealt with the accumulation of arrears in the first place.

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[32] Global Public Procurement Database: Share, Compare, Improve! (worldbank.org)

The issue of late payments by the public sector is a concern that regulators worldwide share. For instance, the European Commission issued a late-payment directive (LPD) in 2011 to standardize payment terms, impose late payment penalties, and establish uniform regulations.[33] Similarly, in the U.S., States such as Illinois, New York, or Massachusetts enforce interest penalties for late payments on public projects to induce prompt repayment and to ease the financial strain that delayed payments can put on the private sector.[34] Although establishing a late payment interest incentivizes early payment, state comptrollers routinely report the late payment of procurement invoices and compute the interest paid accordingly.

In developed economies, government agencies in distress may delay payments. However, default is extremely rare, and suppliers are ultimately paid. Given this, in a frictionless financial market, firms ought to be able to borrow using their government arrears as collateral, implying that repayment speed would not alter corporate actions. Financial frictions, however, may force firms to change their plans, and the delayed payment of government arrears can thus be costly. Uncertainty about payment times can, therefore, cause firms to postpone investments and avoid future procurement contracts. Given this, the extent to which government arrears are costly to firms is a relevant empirical question

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[33] This directive mandates that payments from government to business that are not completed within 30 days should allow creditors to claim interest and recovery costs. See Directive 2011/7/EU, On Combating Late Payment in Commercial Transactions, 2011 O.J. [L. 48], 2 for more details.

[34] This trend of requiring interest on unpaid bills has expanded to other states. For example, Louisiana Governor John Bel Edwards enacted Act No. 566 on May 30, 2018. This update to the Louisiana Revised Statute Section 38:2191(B), effective August 1, 2018, mandates that public entities pay interest on late payments. According to the amendment, payments are considered late, and interest starts accruing 45 days after the public entity receives a proper request for payment. The interest rate is set at 0.5% daily, capped at 15%.

linked to the presence of financing constraints. We approach this question by analyzing firms' reactions to the accelerated repayment of arrears.

A major challenge in examining this issue empirically is that the payment terms for arrears are frequently influenced by the specific circumstances of both buyer and seller, creating a standard endogeneity problem. To address this problem, one potential approach would involve randomly accelerating the repayment of government arrears for a subset of firms, offering an unexpected intervention. We take advantage of a natural experiment that mimics this ideal field experiment: A program in Spain in 2012 accelerated the repayment of regional government arrears. Some firms received a sizeable unexpected payment and reduced accounts receivable, while other comparable firms did not.

In 2012, the Spanish government paid overdue amounts to over 60,000 firms. In the five years before this, regional governments had accumulated arrears owed to suppliers. The volume of arrears totaled €28.5 billion (equivalent to 2.7% of Spanish GDP). In 2011, this cash injection had been largely unanticipated by firms.[35]

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[35] News coverage on the repayment program appeared for the first time in mid-January 2012. Legislation passed in March, and the payments were made between May and July. See Figure 5 for details on the news coverage.

Figure 5. Appearance of SPP news in Spanish Newspapers

News: Plan de Pago a proveedores

News: Plan de pago a proveedores + Mancomunidad
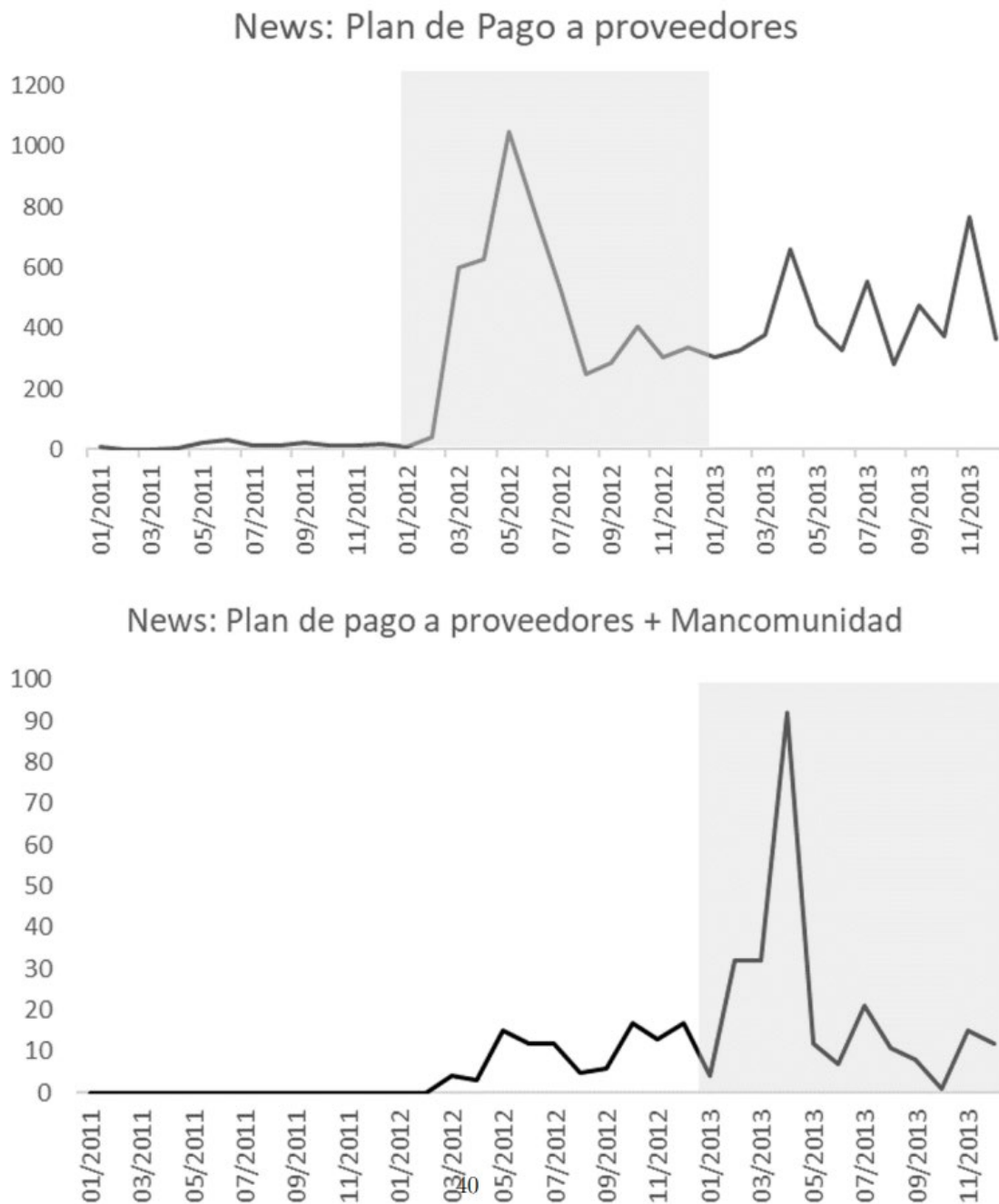
Notes: This Figure represents the total number of times that "Plan de Pago a Proveedores" (top panel) and "Plan de Pago a Proveedores" alongside the word "Mancomunidad" (bottom panel) appeared in the Spanish news every month from January 2011 to December 2013. Source: Factiva.

A key element of this repayment program is that the program accidentally omitted a group of firms from the initial repayment program (Phase I). Suppliers that worked for groups of municipalities (*mancomunidades*) were first overlooked.

These firms were then included in an amended plan (Phase II) and received payment a year later. In total, more than 7,000 firms (with arrears amounting to around €1 billion) were paid in mid-2013 instead of in mid-2012. Importantly, the firms in Phase II received the cash injection a year later for exogenous reasons. We show they constitute a valid control group because they have characteristics and selection margins similar to those of firms in Phase I. The unanticipated nature of the program and the presence of a natural exogenous control group make this liquidity injection an ideal setting to study the effects of the repayment of accumulated arrears.

Our results show that there are real effects associated with the accelerated payment of government arrears. The repayment program affected corporate investment, leverage, and liquidity decisions differently for Phase I firms and Phase II firms. In particular, we find that an unexpected repayment shock equivalent to more than 10% of a firm's assets led firms to increase their investments by 14%, reduce their leverage by 10%, and increase their cash reserves by 44%. These measures are statistically and economically important, representing (respectively) around 30%, 20%, and 40% of the standard deviation of firm investment, leverage, and liquidity growth of the firms in the sample. The consequences of this program help us understand the effects of reducing government arrears and providing liquidity to firms. The effects are also informative about the cost that government arrears entail for firms and how firms have made changes in strategies to minimize these costs, given investment opportunities and financing constraints.

Our findings shed light on the actions that firms took to address the accumulation of arrears and late payments. The results suggest that firms with unpaid customer bills will likely delay investment opportunities and borrow to

continue operations. The added liquidity in these firms after the repayment is consistent with the presence of financial frictions and with a costly uncertainty about future payment delays.

In theory, absent financial constraints, arrears should have no effect on economic activity. However, we do expect a heterogeneous response across firms with different financing constraints. In particular, firms' responses should vary across the ability of firms to borrow during the accumulation of arrears. In principle, firms with government arrears should be able to borrow against their public invoices, using them as collateral in factoring contracts with banks. However, this was a period in which factoring activity was shrinking due to regulatory frictions and banks' liquidity constraints. Moreover, this reduction in factoring activity is heterogeneous across banks. We show that banks that are in a better financial position expand their factoring activity relative to those that are more constrained. Better banks may also be able to extend other forms of borrowing to their customers to help them accommodate the liquidity needs induced by the accumulation of public arrears.

We extend the analysis on the accelerated repayment of arrears by conditioning on the financial constraints of firms. We proxy financial constraints using the firms' pre-determined exposure to specific banks that got more or less affected by the crisis (see Bentolila, Jansen, & Jiménez (2018), Chodorow-Reich (2014), and Jiménez, Ongena, Peydró, & Saurina (2014) for a similar approach). The results show that financially constrained firms increase their investment after the liquidity injection while they reduce a small fraction of their leverage. This suggests that financially constrained firms delayed investment opportunities and indicates that, in contrast with the unconstrained benchmark, large government arrears can indeed be costly to firms that face financing constraints. Conversely,

financially unconstrained firms do not increase their investments after the repayment program and reduce a larger fraction of their leverage instead. This shows that these firms were able to borrow against their collateral or future cash flows and possibly against their accounts receivable with the local governments.

This reduction in leverage is also heterogeneous across financing constraints. Financially unconstrained firms reduce more leverage, and this is mostly financial debt. Conversely, we find that financially constrained firms mostly decrease their accounts payable following the cash injection. This suggests that trade credit might serve as a substitute for bank financing, although it is insufficient to avoid the decrease in investment. Additionally, this finding also sheds light on how the effect of government arrears may be transmitted to the supply chain through trade credit.

We also observe that firms tend to increase their cash reserves, irrespective of their financing constraints. This result is consistent with the fact that firms facing an episode of delayed payments decide to hold onto more cash to help cover future late payments and other short-term costs, even if these firms have the capacity to borrow against these unpaid bills.

Finally, we study how the repayment of accumulated government arrears affects the relationship dynamics between firms and public procurement. Our findings indicate that if public administrations delay their payments, their customers may reduce public procurement contracting with them. This result is significant in terms of the probability and volume of contracting. However, this effect vanishes after the repayment program, which talks about the importance of this intervention to preserve procurement relations between firms and the government.

Our study is linked to several strands of the literature. First, we contribute to the literature on the financial aspects of procurement. The procurement channel is helpful in providing firms with a stable income during recessions (Goldman, 2020). Di Giovanni, García-Santana, Jeenas, Moral-Benito, & Pijoan-Mas (2022) expand on this idea to show the implications of firms using their procurement relationships as a form of collateral that can ease financing constraints. Similarly, Gabriel (2022) shows that Portuguese firms use procurement contracts as collateral to increase their amount of lending. Lee (2021) shows that particularly procurement-dependent firms grow more, which is especially true for financially constrained firms. These papers show different beneficial aspects of procurement for firms. However, Bonfim, Zhao, Queiro, & Ferreira (2021) show that when government spending is unexpectedly cut during a financial crisis, procurement-prone firms find it harder to borrow. In our paper, we also study the negative effects of procurement, but we focus on analyzing delayed payment.

Several other papers have analyzed the negative aspects of the trade credit linked to procurement. Barrot & Nanda (2020) focus directly on formal trade credit terms in procurement contracts and show that shorter formal payment periods can have a positive effect on firms. In particular, they find a positive effect on employment when the US government accelerates payments to business contractors, but only in labor markets that are not too tight. Relatedly, Checherita-Westphal, Klemm, & Viefers (2016) show that increased delays in some European governments' payments can affect the liquidity and profits of the private sector, whereas Conti, Elia, Ferrara, & Ferraresi (2021) show that stricter regulation to minimize late payment reduces firms' exit rate. While these papers are closer to our paper, we focus on a different issue. Specifically, we study

government arrears (i.e., late payment) rather than the formal trade credit terms of procurement (i.e., the contractual maturity). Our natural experiment posits a large one-off reduction of arrears rather than a smaller but more persistent one. We also focus on the interaction of the late payment of arrears with financing constraints.

More broadly, our paper also contributes to the literature on the different stimulus policies to channel liquidity to the corporate sector (Lelarge, Sraer, & Thesmar, 2010; Banerjee & Duflo, 2014; Brown & Earle, 2017). The impact of any directed policy is typically difficult to evaluate, primarily because of potential selection biases. In our natural experiment, the government effectively executes an unorthodox form of fiscal policy. It borrows from banks to accelerate the repayment of accumulated arrears. Even though government liabilities remain unchanged (Figure 3), this policy has real effects, particularly for financially constrained firms.

Finally, our work contributes to several streams of the trade credit literature. We show the potential costs for suppliers when they finance a large buyer via trade credit (Murfin & Njoroge, 2015; Klapper, Laeven, & Rajan, 2012). We add to the understanding of the costs of late payment and its interaction with financing constraints. While late payment has been well documented in the literature (Petersen & Rajan, 1997), the theoretical literature focuses on its role as insurance for the buyer (Cuñat, 2007; Wilner, 2001), which is unlikely relevant in the case of public debt. Moreover, the empirical literature on late payment is very limited so far. We also contribute to the very scarce literature on trade credit

factoring by implicitly showing that firms cannot discount government arrears even when the creditworthiness of the government is good.[36]

The rest of the paper is organized as follows. In Section 4.2, we provide an analytical framework and background information on the institutional setting for the repayment program. Section 4.3 describes the data and the construction of the main variables. The empirical strategy and results are presented in Section 4.4. Section 4.5 discusses how financing frictions may affect the results. Section 4.6 discusses the effects of late payment by public entities on procurement contracts, and Section 4.7 concludes.

4.2. Institutional Background

Our work aims to understand the real effects of delayed payment in procurement by examining how firms respond to accelerated repayment of accumulated arrears. We first describe the procurement process and the institutional setting, and we lay out the natural experiment that we use for identification purposes.

4.2.1. Procurement, Late Payments, and Financing Constraints

From the perspective of a supplier firm, government arrears can be characterized as an illiquid asset with uncertain maturity. In principle, if suppliers are paid with such illiquid assets, it could lead them to liquidity constraints, potentially affecting the firm's ability to invest or continue production.

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[36] See Smith & Schnucker (1994) as one of the few contributions to understanding the factoring contract.

However, late payments in procurement contracts should not significantly impact firms' decision-making, provided that the firms can use arrears as collateral. Arrears can mechanically create liquidity by offering a viable means for obtaining loans. If government arrears are perceived as safe assets, firms may leverage them against future cash flows from their procurement contracts, thus mitigating the effects of delayed payments. In Spain, as in most developed countries, procurement trade credit contracts with municipal and regional authorities are implicitly guaranteed by the central government, suggesting that, barring financing constraints, procurement invoices should effectively generate sufficient collateral to offset any late payments.

The first empirical question of this paper is to evaluate if this holds true. Specifically, we examine whether the anticipated repayment of government arrears produces tangible effects, indicating firms' inability to leverage their government invoices with banks. The unconventional fiscal policy that we use as a natural experiment replaces one illiquid asset (arrears) with a liquid one (cash) in the balance sheet of supplier firms. In principle, if arrears generate their own collateral and liquidity, this fiscal policy should have no effects. However, our results suggest that this is not the case. Despite their "safe" label, arrears are an imperfect form of collateral, and factoring contracts or other types of financing are not adequately protecting firms against illiquidity. In the next section, we provide some arguments explaining why this may be the case.

Figure 6 conceptualizes this setting. The gray arrows show the flow of funds and contracts of the different agents, while the blue and black arrows show the flows of the unconventional fiscal policy of the government. Note that from the joint perspective of the entire government (central, regional, and local), a liability with firms is replaced with a liability with banks. So, this policy does not

entail any additional liabilities for the government as a whole. However, we show empirically that this policy has real effects.

Figure 6. Analytical Framework
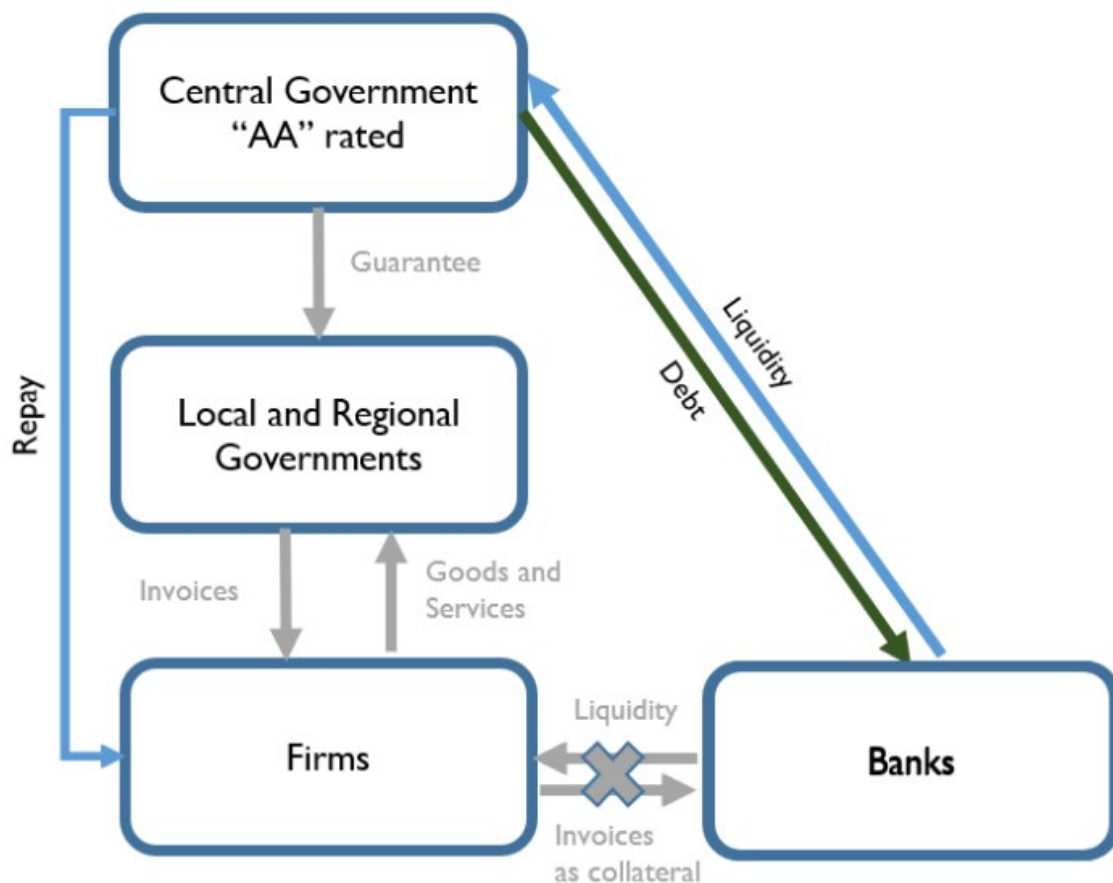
Notes: The Figure represents the financial interrelations between the central government, local and regional governments, firms and banks.

Another empirical question is whether financially constrained and unconstrained firms have a different reaction to government arrears. Even if arrears are an imperfect form of collateral, they should not affect firms that are financially unconstrained in a broader sense. Specifically, firms can offset the liquidity constraints imposed by government arrears if they have access to alternative forms of collateral, sufficient cash flows, or can leverage their commercial relationship with the government. If financially unconstrained firms borrow during the accumulation of arrears to keep investment at its optimal levels,

their early repayment will entail a reduction of leverage and an accumulation of liquid assets.

Conversely, financially constrained firms facing government arrears would reduce investment, use all their available liquidity, and distort their decisions to reduce their needs for liquidity. The firms that had to postpone investment due to illiquidity problems should increase investment after the repayment.[37]

In order to answer these two empirical questions, we take advantage of an empirical setting in which several elements concur: (i) an accumulation of procurement arrears, (ii) a policy that repays these arrears unexpectedly, (iii) groups of comparable treatment and control firms, and (iv) a classification of firms into financially constrained and unconstrained ones. These elements are all present in our empirical design. The next sections describe our empirical setting in more detail.

4.2.2. Public Arrears and the Spanish Setting

The Spanish economy suffered a severe credit crunch that originated from the global financial crisis that developed in 2008 (Banco de España, 2017; Bentolila, Jansen, & Jiménez, 2018; and Jiménez, Ongena, Peydróo, & Saurina, 2014). The financial crisis had a considerable impact on the private sector, leading to higher unemployment and depressed domestic demand (Campos & Reggio, 2015). The public sector was not left unscathed. Spain's public administrations, particularly at the municipal and regional level, experienced

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[37] A similar argument about the differential response of constrained and unconstrained firms in the face of a positive liquidity shock can be found in Banerjee & Duflo (2014).

capital market funding problems, just like local banks, and they delayed payments to suppliers.[38]

Figure 2 shows the evolution of municipal and regional trade credit and the arrears. There is a clear increase in the amount of trade credit used (orange line). More importantly, there is a marked increase in late payments (blue line), that is, trade credit that goes beyond its contractual maturity. The peak of total trade credit use (not yet due and on arrears) happened in 2011, just before the government intervention of 2012.[39] At the same time, municipalities increased total expenditures, exacerbating budget deficits (see Figure 4). The result was that the commercial debt in arrears accumulated by regional and local governments as of December 2011 amounted to €28.5 billion (about 2.7% of GDP).

Simultaneously, the financial crisis created a contraction of the factoring market. The factoring market allows firms to borrow in anticipation of payment of invoices by selling them to banks at some discount. The invoices serve as a form of collateral, and the discount implicitly determines the interest on the loan. Figure 7 compares the evolution of sales (turnover index) with the evolution of factoring loans. While sales declined by 19% between 2007 and 2012, factoring loans as a fraction of GDP fell by 58%. During this period, GDP declined as well, while the prevalence and maturity of trade credit increased, so the fraction of invoices that were being factored shrank even further than 58%. All this evidence, together with a context in which firms had an increased demand for liquidity, suggests that

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[38] Trade credit maturities were generally extended during this period, but the effect was more pronounced in the public sector. Figure 1 compares the evolution of delayed payment days in the private and public sectors.

[39] To get a sense of the severity of late payment by 2011, 35% of total municipal outstanding trade credit was overdue by more than 12 months, 16% by more than 24 months, and 9% by more than 36 months.

the sizable reduction of factoring was considerably driven by supply factors. Some of the reasons that may have induced factoring to shrink are regulatory. Despite the explicit guarantees of the central government, the European banking regulations did not consider factoring for regional government invoices as a safe form of lending. Municipal and local government arrears require more regulatory capital than other forms of loans. Also, banks were not able to re-discount the arrears at the European Central Bank during this period. Facing limited lending capacity, banks shifted their focus to alternative forms of lending, such as sovereign lending (Acharya, Eisert, Eufinger, & Hirsch, 2018; Ongena, Popov, & Van Horen, 2019).

Figure 7. Factoring and Business Turnover Index
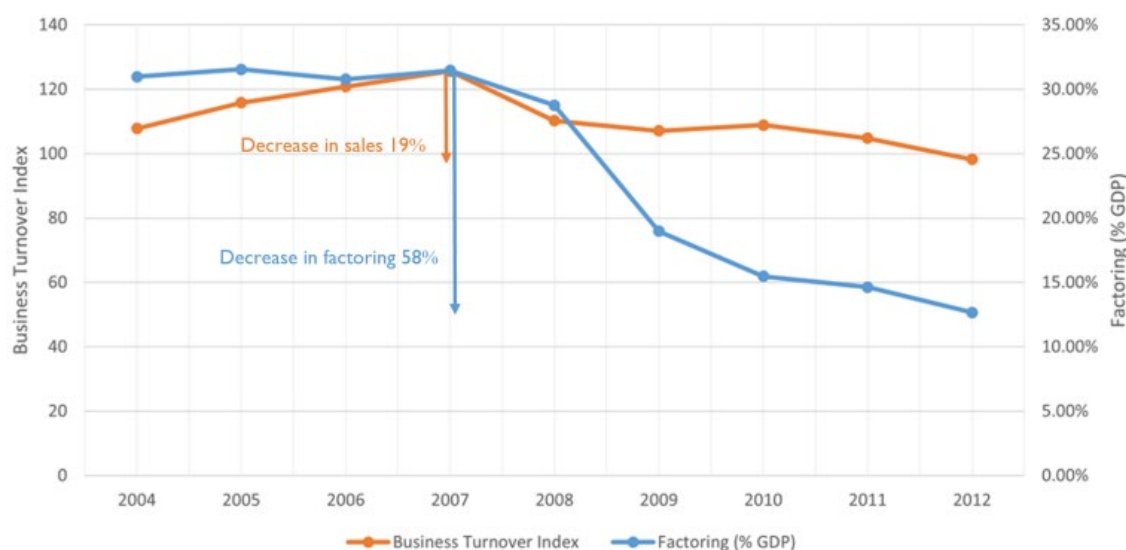
Notes: This Figure shows the evolution of factoring of Spanish firms as a percentage of GDP and the Business Turnover Index for Spanish firms. Data was obtained from the Spanish Statistical Office (INE), refers to the 2004-12 period.

4.2.3. An Unconventional Fiscal Stimulus

The Spanish central government approved the Supplier Payment Plan (SPP) to alleviate the liquidity problems of suppliers to regional and local

governments. For a comprehensive account of the institutional setting, see [Chapter 2](#).

This was an unconventional form of fiscal policy. As [Figure 6](#) shows, all the agents involved substituted an asset for an asset or a liability for a liability. The Fund for Financing the Payments to Suppliers (FFPS) did not incur additional liabilities for the local government. Rather, the central government borrowed directly from banks what was needed to pay local government debts. Local governments were thus released from their debts with suppliers while acquiring debt with the central government. Firms substituted one asset (invoices) with another asset (cash). The plan provided firms with a way to overcome their inability to borrow via factoring. While the implicit guarantee of the central government may not have been enough to allow firms to factor their arrears, the explicit borrowing of the central government provided them with liquidity.

4.2.4. The Natural Experiment

We use the SPP to study the unexpected accelerated repayment of local government arrears. To estimate the causal effect of the policy, we take advantage of an administrative mistake that left some municipalities out of the 2012 phase of the plan (which we label Phase I).

Spanish municipalities may channel some or all of their purchases through *mancomunidades*. These are legal pools of several municipalities engaged in procurement that seek to achieve some economies of scale and improve their bargaining power. Although from an economic standpoint, municipalities and *mancomunidades* are very similar, they have different legal statuses. The first

phase of legislation accidentally did not specify that debts with *mancomunidades* were included in the program, so their debts were not paid in 2012.

In February 2013, another law was passed, resulting in a new round of the SPP, which we label Phase II. With approval to pay the arrears to the suppliers of *mancomunidades*, the ICO transferred in August 2013 over €1 billion to suppliers of regional and local governments.

The important fact for our analysis is that the reason why some firms participated in Phase II was due to an error in the plan's original legislation (it did not include *mancomunidades*), which was unrelated to the characteristics of the suppliers. Firms in Phase I and firms in Phase II have exposure to the public sector, have public arrears, and are very similar in characteristics. This is the basis of our identification strategy. We use the SPP as a random shock that affected treated firms in 2012 (Phase I firms), but that did not affect control firms, a quasi-randomly selected group of firms with similar characteristics (Phase II firms).[40] Last, to explore the role of financial frictions in our setting, we classify firms into financially constrained and unconstrained firms by exploiting their banking relations before the repayment of Phase I.

4.3. Data

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[40] Figure 16 (Appendix) shows an example of water treatment procurement in the region of Andalucia. Some municipalities contract directly for water treatment, while others do so via *mancomunidades*. There are no major selection margins between the two groups. More importantly, the firms that supply *mancomunidades* and municipalities are very similar; indeed, often, firms supply both municipalities and *mancomunidades*. In our main analysis, we use firms that participate only in Phase I as the treated group (e.g., Firm A in the figure) and firms that participate only in Phase II as the control group (e.g., Firm B in the figure). Note that we drop any firms that appeared both in Phase I and Phase II (e.g., Firm C in Figure 16 (Appendix)).

In this section, we describe the data used in this study. First, we elaborate on the data collection process and data sources and then provide summary statistics.

4.3.1. Data Collection

The main data for our analysis are provided by the *Instituto de Crédito Oficial* (ICO). They include anonymous firm information from different phases of the SPP. The data set includes information on each unpaid bill between a firm and each regional and local government, including amounts and payment dates.

The data are matched by the ICO to exhaustive firm-level financial data from the Iberian Balance Sheet Analysis System (SABI).[41]

For Phase I, the firms in the sample (i.e., those that can be matched to SABI) account for 48.2% of all suppliers (64,879 of 134,568) and almost 70% of the funds injected (€19 billion of €27.3 billion). For Phase II, the ICO data set includes 1,848 firms, of which 1,201 are firms that had earlier received funds in Phase I, and 647 are firms that received funds only in Phase II. Once we restrict ourselves to firms with non-missing information for all the relevant variables, our sample has 47,735 firms for Phase I and 526 firms for Phase II.

ICO's data are also matched to Opentender. This database includes public procurement information on contractors, public buyer identifiers, and contract descriptions, including prices and amounts in more than 30 countries.

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[41] SABI data are provided by INFORMA D&B in collaboration with Bureau Van Dijk, which obtains financial information from the Spanish business register. SABI covers the vast majority of companies that are incorporated in Spain but does not cover some very small companies or self-employed individuals.

We obtain data on accounting information on municipalities and regions from a Spanish Finance Ministry database.

Data on the business turnover index and factoring (unpaid bills of exchange) come from the Spanish Statistical Office.

Finally, we measure the media coverage of Phase I and Phase II using Factiva, which aims to cover the universe of news in Spain.[42]

4.3.2. Summary Statistics

Panel A of Table 8 presents summary statistics for firms in Phase I (column 1) and Phase II (column 2) in 2011, just before the repayment. We aggregate all the unpaid invoices with all local and regional governments to calculate the total amount of arrears that each firm has.

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[42] Factiva, provided by Dow Jones, gives access to more than 6 million articles every year in more than 200 Spanish national, regional, and local newspapers and magazines.

Table 8. Summary Statistics: 2011

Panel A: Unmatched

Variable	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Repayment Shock	142.360	102.105	-40.255	(0.460)
Investment	0.009	0.056	0.047	(0.136)
Leverage growth	-0.017	0.003	0.020	(0.408)
Liquidity growth	-0.151	-0.102	0.049	(0.364)
Total assets	5,139.655	6,743.966	1,604.311	(0.161)
Total liabilities	3,244.934	4,549.020	1,304.086	(0.123)
Cash	280.514	269.317	-11.197	(0.806)
Leverage ratio	0.373	0.396	0.022	(0.197)
Total debt	1,554.632	2,323.752	769.120	(0.127)
Long-term debt	814.032	1,168.053	354.021	(0.178)
Short-term debt	578.430	792.305	213.875	(0.195)

Panel B: Matched

Variable	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Repayment Shock	142.360	142.351	-0.009	(1.000)
Investment	0.009	0.048	0.039	(0.230)
Leverage growth	-0.017	-0.001	0.016	(0.720)
Liquidity growth	-0.151	-0.105	0.046	(0.523)
Total assets	5,139.655	5,139.658	0.003	(1.000)
Total liabilities	3,244.934	3,336.247	91.313	(0.886)
Cash	280.514	311.054	30.540	(0.749)
Leverage ratio	0.373	0.381	0.008	(0.718)
Total debt	1,554.632	1,736.338	181.706	(0.656)
Long-term debt	814.032	873.492	59.460	(0.786)
Short-term debt	578.430	629.969	51.539	(0.714)

Notes: This Table reports the mean of firm characteristics for firms in Phase I and Phase II, the differences between the two groups of firms, and the *p*-values associated with those differences. Phase I includes the sample of firms that received the liquidity shock in year 2012, and Phase II includes firms that received the liquidity shock in 2013. **Panel A** compares firms in Phase I and Phase II before matching. In **Panel B**, firms from Phase I and Phase II are matched based on total assets, the liquidity shock, and the region where they are based. Firm characteristics are as of 2011.

We also have information on seized amounts by the central government.[43] For each firm, we measure the repayment shock as the total amount of arrears minus the total amount seized by the government. This results in the effective amount of euros transferred from ICO to the firm. The average repayment shock for firms in Phase I is €142,360, compared to €102,105 for firms in Phase II.

Firms in Phase I had average total assets of over €5 million and average total liabilities of over €3 million. Firms in Phase II had average total assets of more than €6.7 million and average total liabilities of over €4.5 million. As for

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[43] Seized amounts represent debts that firms had with the central government. These seized amounts were deducted from the total amount of arrears paid to the firm.

cash, firms in Phase I and Phase II had about €280,000 and €270,000, respectively.[44]

On average, we do not observe any significant differences in the averages of variables across firms in Phase I and firms in Phase II. This suggests that the two groups of firms are comparable. Nevertheless, in Panel B of Table 8, we match firms in Phase II to firms in Phase I using entropy matching (Hainmueller, 2012) on the first moment of the repayment shock, assets, and region. After matching, we can see that the differences between firm characteristics become even smaller, and there are still no significant differences between the two groups.[45] We use this same matching criterion in parts of the analysis.

4.4. Accelerated Repayments and Corporate Decisions

We are interested in estimating the effect of late payment of arrears of procurement contracts. In particular, we aim to understand whether corporate investments, leverage decisions, and cash hoardings are affected by an accelerated repayment of arrears.

4.4.1. Empirical Strategy

To assess the causal impact of an accelerated repayment of government arrears, we require a treatment group that experiences an unexpected repayment of these arrears and a control group that, despite having a similar amount of unpaid arrears, does not get repaid at the same time. We attempt to mirror the

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[44] In Table 37 (Appendix), we show aggregate descriptive statistics for the entire sample.
[45] We include matched summary statistics for firms in Phase I and Phase II in 2010 (Panel A) and 2009 (Panel B) in Table 38 (Appendix). We do not find any significant differences with respect to the 2011 statistics.

ideal randomized experiment by leveraging the potentially random distribution of the repayment plan's implementation, as we have discussed. It was legislative oversight in 2012 that effectively created two groups of municipalities paid at different times.

Our underlying assumption is that the only difference between firms in Phase I and Phase II is that the former received repayment in mid-2012, while the latter received it a year later, in August 2013. Some of our specifications use entropy matching to improve the balance of both groups of firms.

We use the following specification:

$$y_{jt} = \beta_t Phase1_{\{j \in Ph1\}} \times PostYear_{(t)} + \Lambda + \varepsilon_{jt} \tag{3}$$

where y_{jt} is a set of corporate decisions, including investment, leverage growth, and liquidity growth for firm j, in year t. These three variables are measured as the first difference of the logarithm of fixed assets, total liabilities, and cash, respectively, so the results can be interpreted in terms of net investment, change in total liabilities, and change in cash. $Phase1_{\{j \in Ph1\}}$ is a dummy variable that takes a value of one for firms that participate in Phase I and zero for firms that participate in Phase II. $PostYear_{(t)}$ are dummy variables indexed from 2009 to 2012 that take a value of zero for each year prior to the index and one for each year after the index. We also add a set of fixed effects (Λ), which includes year, industry, and region effects. The coefficient of interest is β_{2012}, which indicates the effect of accelerated repayment on corporate decisions for firms in Phase I versus firms in Phase II. The coefficients β_{2009} to β_{2011} correspond to placebo treatments in which the treated year is the index year, and the control years are the years before. These placebo coefficients measure the differential pre-trends in the corporate investment of the groups. If

the groups are comparable, we should observe insignificant coefficients before 2012. If there is an effect of late payment on any of the variables of interest, we should observe a differential effect of the repayment shock for the $Phase1 \times PostYear_{2012}$ coefficient.[46]

Next, we exploit the heterogeneity in the treatment, that is, in the amount of arrears that are repaid. We sort the firms in Phase I into four different groups according to the amount of repayment over total assets that they receive: below 1%, between 1% up to 5%, between 5% up to 10%, and above 10%. We predict the strongest response from firms that experience the largest repayment shock, that is, those that accumulate more arrears before the repayment program. We also expect that firms that have less accumulated public arrears will have less of a reaction. This "no-effect" result would also serve as a placebo test that confirms that the different reactions from firms in Phase I and Phase II are indeed due to the accelerated repayment.

We match each of the four groups of treated firms in Phase I, with all the firms in Phase II, according to total assets and repayment shock. This matching approach allows a direct comparison of firms that receive a repayment shock in Phase I and firms of a similar size that experience a similar repayment shock a year later in Phase II. As in the previous specification, all the results include year, industry, and region fixed effects, with standard errors clustered at the firm level.

4.4.2. Investment, Leverage, and Liquidity Decisions

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[46] The estimated magnitude should be considered a lower bound if some firms in Phase 2 anticipating repayment in 2013 and changed their corporate decisions already in 2012.

We first analyze the impact of the central government's repayment of arrears on various corporate decisions (investment, leverage growth, and liquidity growth). We exploit the plan's random repayment schedule using the structure outlined in Equation (3).

Table 9 reports the main effects of the repayment shock on investment, leverage growth, and liquidity growth. To control for time-specific shocks, we include year-fixed effects in all columns. Columns 2, 4, and 6 also include industry and region fixed effects to control for unobserved time-invariant heterogeneity across different industries and regions.[47] We cluster standard errors at the firm level to account for potential within-firm correlation or heteroscedasticity.

Table 9. Effects on Corporate Decisions

Panel A: Unmatched Regressions

	Investment		Leverage growth		Liquidity growth	
	(1)	(2)	(3)	(4)	(5)	(6)
Phase I × Post 2009	0.005	0.009	-0.012	-0.006	0.054	0.054
	(0.28)	(0.48)	(-0.45)	(-0.22)	(1.01)	(1.01)
Phase I × Post 2010	-0.006	-0.006	-0.007	-0.007	-0.072	-0.073
	(-0.22)	(-0.23)	(-0.19)	(-0.19)	(-0.87)	(-0.88)
Phase I × Post 2011	-0.046	-0.046	-0.001	-0.001	-0.030	-0.031
	(-1.18)	(-1.18)	(-0.02)	(-0.02)	(-0.37)	(-0.38)
Phase I × Post 2012	0.079**	0.079**	0.002	0.003	0.136*	0.137*
	(2.13)	(2.14)	(0.07)	(0.09)	(1.67)	(1.69)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	155881	155881	157309	157309	142338	142338
Adjusted R^2	0.001	0.003	0.007	0.010	0.005	0.006

Panel B: Matched Regressions

	Investment		Leverage growth		Liquidity growth	
	(1)	(2)	(3)	(4)	(5)	(6)
Phase I × Post 2009	0.006	0.007	-0.006	-0.001	0.042	0.037
	(0.29)	(0.37)	(-0.20)	(-0.03)	(0.75)	(0.67)
Phase I × Post 2010	-0.013	-0.013	-0.026	-0.026	-0.067	-0.070
	(-0.45)	(-0.46)	(-0.65)	(-0.66)	(-0.78)	(-0.81)
Phase I × Post 2011	-0.042	-0.042	0.010	0.010	-0.009	-0.012
	(-1.05)	(-1.05)	(0.25)	(0.27)	(-0.10)	(-0.14)
Phase I × Post 2012	0.080**	0.080**	0.005	0.006	0.126	0.128
	(2.15)	(2.16)	(0.14)	(0.15)	(1.54)	(1.57)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes
Observations	150320	150320	151653	151653	137486	137486
Adjusted R^2	0.003	0.005	0.007	0.013	0.005	0.010

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[47] Results are similar if we include a more saturated model with a triple interaction of year, industry, and region fixed effects. However, to retain enough degrees of freedom, we opt for an intermediate approach where we control for time trends and for time-invariant regional and industry characteristics.

Notes: This Table presents estimates from panel regressions explaining corporate decisions for the period 2009 to 2012. In both panels, the dependent variable in Columns (1) and (2) is the first difference in the logarithm of fixed assets (*Investment*), the first difference in the logarithm of total liabilities in Columns (3) and (4) (*Leverage growth*), and the first difference in the logarithm of cash in Columns (5) and (6) (*Liquidity growth*). *Phase I* is an indicator variable that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2009*, *Post 2010*, *Post 2011*, and *Post 2012* are indicator variables for years 2009-12, 2010-12, 2011-12, and 2012, respectively. Columns (1), (3), and (5) include year fixed effects. Columns (2), (4), and (6) include year, region, and industry fixed effects. In **Panel B**, firms from Phase I and Phase II are matched based on total assets and the size of the liquidity shock. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

The baseline results shown in Panel A suggest that firms in Phase I and Phase II exhibit a similar pattern in terms of investment, leverage, and liquidity decisions for the period of 2009-2011. When the repayment shock hits, we observe significantly higher levels of investment and increases in cash holdings. We do not observe significant changes in leverage growth in 2012. To further reduce any potential differences between the treatment and control groups before 2012, we implement an entropy-balancing matching approach. This method reweighs the two groups according to the size of the liquidity shock and total assets in 2011. Panel B of Table 9 shows that the results remain similar after applying entropy matching, corroborating the findings in Table 8 that both groups are closely comparable and that investment grows after the positive liquidity shock. The point estimates on liquidity growth are similar in both regressions but not statistically significant in matched specifications.

These tests aggregate all firms with arrears and provide an "overall effect" of repayment. This approach, though, gives equal weight to firms that receive minor repayments and those that receive larger ones. The vast diversity in the size of the repayment shock across firms might contribute to the mildly significant outcomes, as the level of arrears varies significantly from company to company.

Thus, we expect that for firms with smaller arrears, the timing of repayment in 2012 or 2013 should not significantly affect their corporate decisions.

To account for the heterogeneity in the size of the repayment shock, we group firms according to the amount of arrears repaid. We sort the firms in Phase I into four different groups according to the amount of cash over the total assets they receive: below 1%, between 1% up to 5%, between 5% up to 10%, and above 10%.

First, we assess firms' responses in terms of investment decisions. The results are presented in Table 10. Each column shows the level of investment of firms in Phase I, stratified by level of arrears, versus a matched sample of firms in Phase II. The results show that firms increase investment monotonically with the level of the repayment shock in 2012. These results are consistent with the hypothesis that firms exposed to late payment might have forgone investment opportunities and reacted by increasing investments upon receiving the repayment shock. These results imply a certain degree of financing constraints of the firms in the sample, and are inconsistent with the view that government arrears can always be used as a reliable source of collateral. Firms experiencing the most substantial repayment shocks (those above 10% of their total assets) show a significant 14% increase in investments compared to their Phase II counterparts. Table 37 (Appendix) indicates this increase in investment is economically important, as it represents about 30% (0.14/0.47 = 0.30) of the standard deviation of the investment growth of the firms in our sample.

Table 10. Effects on Investment Decisions

	Investment			
	< 1%	1% − 5%	5% − 10%	> 10%
Phase I × Post 2009	0.009	0.002	0.006	0.028
	(0.47)	(0.11)	(0.28)	(0.95)
Phase I × Post 2010	-0.007	-0.005	-0.016	-0.082
	(-0.23)	(-0.16)	(-0.45)	(-1.61)
Phase I × Post 2011	-0.046	-0.043	-0.037	-0.022
	(-1.15)	(-1.07)	(-0.89)	(-0.52)
Phase I × Post 2012	0.066*	0.079**	0.094**	0.139***
	(1.78)	(2.09)	(2.40)	(3.55)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	79553	41696	15232	18717
Adjusted R^2	0.006	0.005	0.004	0.005

Notes: This Table presents estimates from panel matching regressions explaining investment decisions for the period 2009 to 2012. Firms from Phase I and Phase II are matched based on total assets and the size of the liquidity shock. The dependent variable is the first difference in the logarithm of fixed assets. *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2009*, *Post 2010*, *Post 2011*, and *Post 2012* are indicator variables for years 2009-12, 2010-12, 2011-12, and 2012, respectively. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. All regressions include year, region, and industry fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

Next, we study the impact of a repayment shock on corporate leverage decisions. Table 11 shows that firms significantly reduce leverage upon receiving a repayment shock above 10% of their total assets. Treated firms reduce their leverage growth by about 10% compared to firms in Phase II. In economic terms, this reduction represents about 23% (0.10/0.44 = 0.23) of the standard deviation of changes in leverage during our sample period. These results suggest that when firms receive an unexpected injection of liquidity, they use these funds to pay off their outstanding liabilities, which would not only reduce their debt burden but also improve their financial health. The repayment is especially pronounced for firms that have substantial arrears, as they receive a greater positive repayment shock, enabling them to reduce their leverage.

Table 11. Effects on Leverage Decisions

	Leverage Growth			
	$< 1\%$	$1\% - 5\%$	$5\% - 10\%$	$> 10\%$
Phase I × Post 2009	-0.020	0.000	0.020	0.080
	(-0.71)	(0.01)	(0.61)	(1.61)
Phase I × Post 2010	-0.002	-0.036	-0.051	-0.128*
	(-0.06)	(-0.89)	(-1.11)	(-1.92)
Phase I × Post 2011	0.014	0.013	0.000	0.023
	(0.37)	(0.33)	(0.01)	(0.54)
Phase I × Post 2012	0.014	0.028	0.014	-0.104**
	(0.39)	(0.75)	(0.35)	(-2.24)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	80015	42097	15398	19099
Adjusted R^2	0.013	0.012	0.012	0.024

Notes: This Table presents estimates from panel matching regressions explaining leverage decisions for the period 2009 to 2012. Firms from Phase I and Phase II are matched based on total assets and the size of the liquidity shock. The dependent variable is the first difference in the logarithm of total liabilities. *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2009*, *Post 2010*, *Post 2011*, and *Post 2012* are indicator variables for years 2009-12, 2010-12, 2011-12, and 2012, respectively. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. All regressions include year, region, and industry fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

Lastly, we study the impact on cash accumulation and present the results in Table 12. We find a positive, monotonic relation between the size of the liquidity injection and the increase in cash holdings after repayment. Firms experiencing the most substantial repayment shocks (again, those above 10% of their total assets) keep about 44.4% more cash than Phase II firms. This increase is economically meaningful. Given the distribution of changes in repayment by firms in our sample, the increase in cash represents about 41% (0.44/1.08 = 0.41) of the standard deviation. As expected, the cash accumulation is particularly evident in firms receiving a greater repayment shock, as they would have more funds to retain.

Table 12. Effects on Liquidity Decisions

| | Liquidity growth | | | |
	< 1%	1% − 5%	5% − 10%	> 10%
Phase I × Post 2009	0.036	0.040	0.035	0.017
	(0.64)	(0.70)	(0.52)	(0.16)
Phase I × Post 2010	-0.063	-0.060	-0.093	-0.083
	(-0.73)	(-0.68)	(-0.93)	(-0.56)
Phase I × Post 2011	-0.006	-0.030	-0.009	0.038
	(-0.07)	(-0.36)	(-0.10)	(0.42)
Phase I × Post 2012	0.049	0.096	0.197**	0.444***
	(0.59)	(1.16)	(2.24)	(4.41)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	72616	38091	13994	17309
Adjusted R^2	0.008	0.010	0.011	0.025

Notes: This Table presents estimates from panel matching regressions explaining liquidity decisions for the period 2009 to 2012. Firms from Phase I and Phase II are matched based on total assets and the size of the liquidity shock. The dependent variable is the first difference in the logarithm of cash. *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2009*, *Post 2010*, *Post 2011*, and *Post 2012* are indicator variables for years 2009-12, 2010-12, 2011-12, and 2012, respectively. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. All regressions include year, region, and industry fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

This result implies that firms use the repayment shock not just for investment and to reduce leverage but also for cash accumulation. A potential explanation for this increase in cash holdings might be that firms that suffer from late payments decide to keep cash as a buffer against financial distress and to gain more flexibility in their future operational and strategic decisions.

Overall, this set of results shows that the intensity of the positive repayment shock affects the firm response, which is monotonically increasing in the level of the shock across all three specifications. In all specifications, we are matching each group of treated firms (Phase I) to a comparable group of non-treated firms (Phase II). In the next set of specifications, we propose alternative specifications to explore this heterogeneous response.

4.4.3. Robustness: DiD and Synthetic DiD

Next, we follow an alternative empirical approach and estimate the impact of accelerated repayment on corporate decisions in a difference-in-differences (DiD) setting. Rather than compare firms in Phase I against firms in Phase II for different years, we compare the corporate decisions of firms in Phase I and Phase II in the period before (2009-2011) and the period after (2012) the repayment shock. By comparing changes over time in the treatment group to changes in the control group, the DiD design helps to isolate the causal effect of the treatment (repayment shock) on the firm's corporate decisions. Furthermore, the DiD approach allows us to mitigate biases in the estimated treatment effect stemming from common firm trends.

In Table 13, we estimate a specification similar to that in Equation (3), but we add a firm fixed effect and collapse all year dummies ($PostYear_{(t)}$) into a unique time indicator variable that takes a value of one in 2012, and a value of zero for the period 2009-11 (*Post 2012*). Effectively, we are comparing the period 2009-11 to 2012. Because we also include year and firm fixed effects, our variable of interest is the interaction term *Phase I x Post 2012*.

Table 13. Effects on Corporate Decisions: DiD

Panel A: Investment

	< 1%	1% − 5%	5% − 10%	> 10%
Phase I × Post 2012	0.027	0.039	0.051*	0.069**
	(1.11)	(1.52)	(1.87)	(2.47)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	78135	40633	14764	17980
Adjusted R^2	0.078	0.072	0.052	0.057

Panel B: Leverage Growth

	< 1%	1% − 5%	5% − 10%	> 10%
Phase I × Post 2012	0.022	0.019	-0.017	-0.159***
	(0.77)	(0.65)	(-0.55)	(-5.11)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	78624	41051	14937	18346
Adjusted R^2	0.042	0.033	0.011	0.024

Panel C: Liquidity Growth

	< 1%	1% − 5%	5% − 10%	> 10%
Phase I × Post 2012	0.042	0.069	0.164***	0.455***
	(0.69)	(1.15)	(2.60)	(7.32)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	70646	36788	13426	16435
Adjusted R^2	-0.157	-0.156	-0.171	-0.165

Notes: This Table presents estimates from a Diff-in-Diff panel matching regressions explaining corporate decisions for the period 2009 to 2012. Firms from Phase I and Phase II are matched based on total assets and liquidity shock. The dependent variables are the first difference in the logarithm of fixed assets (**Panel A: Investment**), liabilities (**Panel B: Leverage Growth**), and cash (**Panel C: Liquidity Growth**). *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2012* is an indicator that equals one for year 2012. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. All regressions include year, region, and industry fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

Panel A details the effect on firm investment decisions. Results in Table 10 are confirmed. In particular, as the extent of the repayment shock increases (from below 1% to above 10% of total assets), we observe a monotonic increase in the effect on investment. The impact is most significant for firms that receive a repayment shock greater than 10% of their total assets, increasing investment by approximately 7% on average. The t-statistic of 2.47 indicates this result is statistically significant at the 5% level.

Panel B evaluates the effect of repayment shocks on leverage decisions. Here, we see that only firms receiving a large repayment shock (greater than 10% of their total assets) show a significant reduction of 16% on average in their leverage. This effect is highly statistically significant, with a t-statistic of -5.11, indicating that the effect is robust at the 1% level. These results are in line with the findings in Table 11.

Panel C investigates the relation between the amount of the repayment shock and liquidity decisions. Here, we see a significant effect for firms receiving repayment shocks amounting to more than 5% of their total assets. Again, the effect is strongest for firms with a repayment shock above 10% of their assets. In economic terms, these firms increase their cash holdings by more than 45%. Results are in line with those described in Table 12.

Taken together, these results provide strong evidence that greater repayment shocks lead to more significant changes in corporate decisions. All the point estimates are monotonically linked to the size of the shock, and the effects are statistically significant for the most affected group of firms. Firms experiencing the most substantial shocks are more likely to increase investments, reduce their leverage, and retain larger cash holdings.

To add robustness to the main results, we also develop a synthetic difference-in-differences (SDiD) approach following the estimator for causal effects with panel data described in Arkhangelsky, Athey, Hirshberg, Imbens, & Wager (2021). The SDiD approach constructs a synthetic control group that best mimics the treatment group's trend in the pre-treatment period. Each treatment firm is replicated by re-weighting a sparse combination of units from the control group. For the re-weighting, more importance is given to those observations

closer in time to the treatment point. This approach can be particularly advantageous when the treatment effect is heterogeneous or when the assumption of parallel trends may not be strictly held. This procedure is often applied when there is a limited number of treated or control units, which is the case for the firms in Phase II.

Thus, in this part of the analysis, we designate the firms in Phase II as the treatment group, and the control group is the firms in Phase I. The weights are chosen to optimally match the pre-adoption outcomes of the firms in Phase II, so they capture any possible trends. The difference between the observed outcomes post-adoption and the predicted outcomes is the estimated treatment effects using the method in Abadie (2021). The results, as shown in Table 14 are similar to those in Table 13. We find that firms in Phase II significantly reduce investment (5.0%), increase leverage (9.4%), and deplete cash (28.4%) compared to firms in Phase I

Table 14. Effects on Corporate Decisions: SDiD

Panel A: Investment

	< 1%	1% − 5%	5% − 10%	> 10%
Phase II × Post 2012	0.001	-0.010	-0.017	-0.050***
	(0.46)	(-0.73)	(-1.60)	(-3.79)

Panel B: Leverage Growth

	< 1%	1% − 5%	5% − 10%	> 10%
Phase II × Post 2012	-0.003	-0.003	0.026	0.094***
	(-0.14)	(-0.10)	(0.82)	(8.09)

Panel C: Liquidity Growth

	< 1%	1% − 5%	5% − 10%	> 10%
Phase II × Post 2012	-0.002	-0.021	-0.103*	-0.284***
	(-0.11)	(-0.84)	(-1.81)	(-5.78)

Notes: This Table presents estimates from SDiD (Arkhangelsky, Athey, Hirshberg, Imbens, & Wager 2021) regressions explaining corporate decisions for the period 2009 to 2012. The dependent variables are the first difference in the logarithm of fixed assets (**Panel A: Investment**), liabilities (**Panel B: Leverage Growth**), and cash (**Panel C: Liquidity Growth**). *Phase II* is a dummy that takes a value of one for firms that received liquidity in Phase II (2013) and zero for firms that received liquidity a year earlier in Phase I. *Post 2012* is an indicator that equals one for year 2012. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. Robust

By confirming that the results hold under the SDiD approach, we show that our results are not driven by any particular specification of the control group or any potential violation of the parallel trends assumption. In essence, this conservative approach provides a more stringent test of the treatment effect and helps to underscore the robustness of our main findings: An accelerated repayment of accumulated public arrears has significant implications for firm investment, leverage, and liquidity decisions.

4.5. The Role of Financing Frictions

In a frictionless financial market, firms should be able to borrow using their government arrears as collateral. If this were the case, we should not observe an increase in investment for financially constrained firms after the government cash injection. As Figure 7 shows, factoring became less of an option during this period due to a severe credit crunch in Spain. In 2007, factoring was above 30% of the Spanish GDP, but after the financial crisis burst, it dropped steadily to almost a third of its previous volume. Such a decline was much more severe than the reduction in economic activity, which can be seen in the Spanish business turnover index.

We analyze whether the effect of the repayment shock on several corporate outcomes depends on how financially constrained firms may be. As a measure of financing constraints, we use the firms' banking relations. We classify

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firms into "Top Banks" if they worked with at least one top bank in 2009.[48] We define top banks as those with a core equity tier 1 (CET1) capital ratio above 7.4% of risk-weighted assets, which was the average CET1 capital ratio for the overall Spanish banking sector in the adverse scenario of the stress tests run by the European Banking Authority (EBA) in 2011.[49]

Bank stress test results are an indicator of a bank's vulnerability and its capacity to lend. Bank-firm relations are known to be quite stable (Petersen & Rajan, 1994; Santos & Winton, 2008; Darmouni, 2020). Jiménez, Ongena, Peydró, & Saurina (2014) and Bentolila, Jansen, & Jiménez (2013) also show that the financial crisis had a heterogeneous impact on Spanish banks, which affected firms through bank-firm linkages. Relatedly, and more closely linked to our paper, factoring transactions shrank sharply during this period (as shown in Figure 7), and they did so differently across banks. Figure 17 (Appendix) shows the average amount of factoring of Spanish SPP arrears by top banks vs. non-top banks. One can see that top banks were able to provide more factoring than non-top banks, particularly after the onset of the financial crisis in 2008.

We use the specification in Equation (3) and split firms into "Top Banks" or "Excluding Top Banks," depending on whether, in 2009, they worked with at least one top bank.[50] We also split firms according to the size of the repayment received. We look particularly at firms that received a repayment shock below 1% of their total assets and firms that received a repayment shock above 10% of their total assets. Firms in the lower repayment group act as an additional control

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[48] If the company does not report bank relations in 2009, we use the last available reported relationship before 2009. If the company does not report any bank information before 2010, we use 2010 information.

[49] See the presentation of the 2011 EBA stress tests results for Spanish banks in: EU - wide stress tests - Other topics of interest - News and events - Banco de España (bde.es)

[50] In Table 39 (Appendix), we show that Phase I and Phase II firms are very similar in 2011, regardless of whether they work with top banks or not.

group for our analyses. Firms in the higher repayment group received a greater shock and are the most "treated" firms, so consistent with our results of Section 4.4.2, they are expected to react more to the shock. The information on these two groups allows us to extract conclusions on whether the reaction of firms to late payment depends on the extent of the accumulated arrears.

Panel A of Table 15 shows that only firms that were not linked to top banks and had accumulated substantial arrears increase their investment significantly after the repayment of 2012. This suggests that firms operating with top banks are not financially constrained and do not curtail investment despite their accumulation of arrears. In particular, firms that did not use a top bank and receive a large repayment shock devote 11% of the cash transfers to increase investment.

Table 15. Effects on Corporate Decisions: Bank Heterogeneity

Panel A: Investment

	Top Banks		Excluding Top Banks	
	< 1%	> 10%	< 1%	> 10%
Phase I × Post 2012	0.023	0.059	0.036	0.112***
	(0.67)	(1.43)	(1.15)	(3.27)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	28725	4353	50828	14364
Adjusted R^2	0.015	0.013	0.007	0.006

Panel B: Leverage Growth

	Top Banks		Excluding Top Banks	
	< 1%	> 10%	< 1%	> 10%
Phase I × Post 2012	0.007	-0.192***	0.023	-0.112***
	(0.20)	(-4.53)	(0.64)	(-3.02)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	28787	4372	51228	14727
Adjusted R^2	0.019	0.037	0.014	0.022

Panel C: Liquidity Growth

	Top Banks		Excluding Top Banks	
	< 1%	> 10%	< 1%	> 10%
Phase I × Post 2012	0.007	0.468***	0.032	0.433***
	(0.06)	(3.55)	(0.49)	(6.61)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	26782	4094	45834	13215
Adjusted R^2	0.013	0.029	0.009	0.025

Notes: This Table presents estimates from panel matching regressions explaining corporate decisions for the period 2009 to 2012. The dependent variables are the first difference in the logarithm of fixed assets (**Panel A: Investment**), liabilities (**Panel B: Leverage Growth**), and cash (**Panel C: Liquidity Growth**). Firms from Phase I and Phase II within the same bank type are matched based on total assets and the liquidity shock. *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2012* is an indicator that equals one for year 2012. We sort our sample into firms that received a liquidity shock below 1% and above 10% of their total assets. The sample "Top Banks" includes all firms that in 2009 worked with at least one bank with a Core Equity Tier 1 (CET1) capital ratio above 7.4% of risk-weighted assets. The sample "Excluding Top Banks" includes all other firms. In Panel A, the dependent variable is the first difference in the logarithm of fixed assets. In Panel B, the dependent variable is the first difference in the logarithm of total liabilities. In Panel C, the dependent variable is the first difference in the logarithm of cash. All regressions include year, region, and industry fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

In Panel B of Table 15, one can see that firms that use at least a top bank significantly reduce liabilities once they receive the repayment shock in 2012. This shows that firms using top banks that receive a large repayment shock repay liabilities to reduce their leverage growth by 19%. Firms that do not use top banks

devote less to reduce leverage (11% reduction). This is an important result, as it highlights that firms that were less financially constrained were able to borrow more and invest more (as shown in Panel A) during the accumulation of arrears. Once the arrears are paid, less constrained firms devote a larger share of their liquidity injection to reduce leverage. This is consistent with having accumulated more liabilities and with having already invested in their most profitable investment opportunities.

Note in Panel C of Table 15 that both groups of firms significantly retain cash after receiving the repayment. Firms with top banks increase cash holdings growth by 47%, and firms without top banks increase cash holdings growth by 43%. This accumulation of cash may be a buffer for future investments or some other form of precautionary saving. Bates, Kahle, & Stulz (2009) find evidence of precautionary motives driving firms to increase their cash ratios in riskier times. This speaks to the interpretation of all our previous results. For example, our results in Panel A of Table 15 may be interpreted as a lower bound of the effect that a similar program could have in the context of expanded investment opportunities. Firms without top banks might be willing to retain cash even if they simultaneously increase investment because greater cash balances make them safer. Harford, Klasa, & Maxwell (2014) show that firms mitigate greater refinancing risk by increasing cash holdings and conserving cash.

In Table 16, we further analyze the impact of late payment on firms' leverage. Panel A shows that firms with top banks that had many arrears significantly reduce financial debt. This confirms the interpretation of Panel B of Table 15 and suggests that less financially constrained firms were able to increase debt levels temporarily to offset the financing needs that originated from the accumulation of arrears. In column 4 of Panel A, however, we show that firms

that did not use top banks do not significantly reduce financial debt after repayment, which suggests they could not increase debt levels when arrears accumulated before 2012. Column 4 of Panel B shows that financially constrained firms significantly decreased their accounts payable after repayment in 2012. These results suggest that financially constrained firms had to recourse to delayed payments to their own suppliers before 2012 because financial debt was unavailable to them. Thus, these results evidence that late payments by local governments may spread through the supply chain, particularly for financially constrained firms.[51] Moreover, these findings speak to the importance of the government policy that we are analyzing, as the repayment program may impact not only firms with arrears, but also connected firms.

Table 16. Decomposing Leverage: Bank Heterogeneity

Panel A: Financial Debt Growth

| | Top Banks | | Excluding Top Banks | |
	< 1%	> 10%	< 1%	> 10%
Phase I × Post 2012	-0.050	-0.328***	0.072	-0.074
	(-0.92)	(-5.20)	(1.34)	(-1.37)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	13047	1467	16838	3318
Adjusted R^2	0.022	0.045	0.026	0.053

Panel B: Accounts Payable Growth

| | Top Banks | | Excluding Top Banks | |
	< 1%	> 10%	< 1%	> 10%
Phase I × Post 2012	0.050	-0.096	-0.005	-0.140***
	(0.98)	(-1.57)	(-0.11)	(-2.89)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	28434	4261	49298	13531
Adjusted R^2	0.018	0.015	0.005	0.010

Notes: This Table presents estimates from panel matching regressions explaining leverage decisions for the period 2009 to 2012. The dependent variables are the first difference in the logarithm of financial debt (**Panel A**), and accounts payable (**Panel B**). *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2012* is an indicator that equals one for year 2012. We sort our sample into firms that received a liquidity shock below 1% of their total assets, and above 10%. The sample "Top Banks" includes all firms that in 2009 worked with at least one bank with a Core Equity Tier 1 (CET1) capital ratio above 7.4% of risk-weighted

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[51] Alfaro, Garcia-Santana, & Moral-Benito (2021) explore the Spanish setting and show that bank credit shocks can propagate downstream in the supply chain and affect suppliers.

Overall, our findings in this section suggest that firms not borrowing from top banks (e.g., firms that are arguably more financially constrained) significantly increase investment and reduce their accounts payable upon the repayment, implying the easing of their financial constraints. Conversely, companies that had the possibility to borrow from top-tier banks, which are less financially constrained, do not increase investment significantly in the period after the repayment. Rather, these less financially constrained firms allocate a greater portion of the repayment to reduce their outstanding leverage and, in particular, financial debt. These results suggest that firms were able to obtain financial debt by borrowing against their accounts receivable with the local governments. Both groups of companies significantly increase their cash holdings, suggesting that, after facing an episode of delayed payments, firms decide to hold more cash to help cover future late payments and other short-term costs, even when they had the capacity to borrow against these unpaid bills.

4.6. Public Procurement Contracts

So far, we have focused on how corporate decisions are affected by the accumulation and early repayment of arrears. Nevertheless, a relevant complementary question is how delayed payments by public administrations influence further procurement contracting. In principle, several effects could be at play. Firms with existing arrears may want to contract with the same public administrations more as a way to enforce the repayment of arrears by keeping

the commercial relationship open. Alternatively, firms may want to cut their commercial relationship with the public administrations if the existing arrears signal further late repayments that they cannot afford. The relevance of each of these forces may also depend on the degree of financing constraints of the firms as well.

In this section, we focus on firms that have contracts with public administrations and study whether there's a distinct behavior in public contracting between firms with unpaid public arrears and firms paid timely. Public procurement contracts are critically important to both firms and public entities. For firms, these contracts can offer a stable and often substantial revenue stream (Goldman, 2020). For public entities, procuring goods and services from private firms allows them to fulfill their public service mandate. Thus, the impact of late payments in this context is particularly relevant.

To examine the impact of delayed payments on public procurement relationships we use a new specification that compares firms with procurement arrears with those with similar procurement contracts that do not have arrears. To do so, we merge the ICO database with the Opentender database. Opentender is a comprehensive European-level public online database that compiles extensive information on government procurement activities. We aggregate the contract-level data in Opentender to a firm-customer-year level, where "customer" refers to a local government of a municipality and "firm" to a potential supplier listed in Opentender. That is, each observation contains data about the volume and pricing of contracts for each local government-firm-year combination. This data allows us to assess the likelihood of firms engaging in new business relationships with public entities, particularly in the context of payment delays. We estimate the following specification:

$$Contract_{jit} = \beta_t Arrears_{j,i} \times PostYear_{(t)} + \lambda_{jt} + \varepsilon_{jt} \qquad (4)$$

where $Contract_{jit}$ denotes whether firm j enters a new public procurement contract with local government i in year t. $Arrears_{j,i}$ is a dummy variable that takes a value of one for firm-local government pairs where the local government has unpaid bills to that firm (before 2012) and zero otherwise. $PostYear_{(t)}$ are dummy variables indexed from 2009 to 2012 that take a value of zero for each year before the index and one for each year after. We also include different sets of fixed effects (λ_{jt}). The coefficients of interest are β_{2011} and β_{2012}, which reflect the effect of late payment and prompt repayment on future business relationships. The coefficients β_{2009} to β_{2010} act as placebo treatments, where the treated year is the index year, and the control years are the preceding ones.

We hypothesize that a firm's level of unpaid bills, or "arrears," significantly affects its decisions regarding contract renewals with public governments. Specifically, we want to check whether firms are more or less inclined to continue contracts with local governments with whom they have outstanding payments still to be collected. To test this, we want to compare firms in the Opentender database with and without arrears. We do this in two different ways. First, firms with arrears are matched to other firms with procurement contracts in 2009 in the same geographic location, similar to a Metropolitan Statistical Area (MSA) in the United States. This matching allows us to consider firms in similar regions and with comparable histories of business interactions with and without arrears. This methodology also allows us to control for local economic conditions and for the propensity to have public contracts. This is crucial since Ferraz, Finan, & Szerman (2015) shows that firms awarded procurement contracts have a higher likelihood of winning more public auctions in the future. We start by including year fixed effects to control for time trends and study the impact of arrears in the cross-

section. In a different specification, we also include firm-by-year fixed effects to implicitly compare the contracting of one firm with a municipality that is not paying on time with the contracting of that same firm with a different municipality that is paying on time.

The results are shown in Table 17. In Columns 1 and 2, the dependent variable is one if there is a new contract between a firm and a local government and zero otherwise. Columns 3 and 4 adopt a more continuous specification where the dependent variable equals one plus the natural logarithm of the value of all awarded contracts between the firm and local government in a given year. Generally, the cross-sectional results presented in Columns 1 and 3 show that public procurement decisions are not affected by public arrears. The muted response by firms experiencing late payments might be explained by several factors. First, it is possible that these firms are exclusively exposed to local governments with whom they have arrears. In this regard, the lack of alternative choices might lead these firms to continue contracting with existing clients. Second, it is also possible that the decision to contract with the public sector depends on other omitted factors that are also correlated with arrears (e.g., firms with more arrears have a higher economic activity and rely more on procurement). To address these two potential concerns, we control for time-varying unobservable characteristics at the firm level in Columns 2 and 4.

Table 17. Effects on Public Procurement

	New Contract		New Contract Price	
	(1)	(2)	(3)	(4)
Arrears × Post 2009	0.053	0.090	0.716	1.253
	(1.08)	(1.63)	(1.06)	(1.59)
Arrears × Post 2010	0.041	0.114	0.602	1.604
	(0.67)	(1.37)	(0.71)	(1.34)
Arrears × Post 2011	-0.030	-0.183**	-0.426	-2.551**
	(-0.56)	(-2.16)	(-0.57)	(-2.16)
Arrears × Post 2012	-0.044	-0.090	-0.518	-0.988
	(-0.86)	(-0.88)	(-0.77)	(-0.71)
Year FE	Yes	Yes	Yes	Yes
Year x Firm FE	No	Yes	No	Yes
Observations	17460	17460	17460	17460
Adjusted R^2	0.011	0.463	0.012	0.449

Notes: This Table presents estimations in which Phase I firms are matched to firms in Phase II. We explore firms' public procurement decisions for the period 2009 to 2012. In this analysis, the unit of observation is at the firm-local government-year level. In columns 1 and 2, the dependent variable is a dummy taking value one if there is a new contract between a firm and a local government and taking value zero otherwise. In columns 3 and 4, the dependent variable is one plus the natural logarithm of the price of all the awarded contracts between the firm and the local government in that year. *Arrears* is a dummy that takes a value of one for firm-local government pairs where the local government has accumulated arrears owed to that firm, and zero otherwise. *Post 2009*, *Post 2010*, *Post 2011*, and *Post 2012* are indicator variables for years 2009-2012, 2010-2012, 2011-2012, and 2012, respectively. We use entropy matching on local government and the existence of a public contract in 2009. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5% or 10% level, respectively.

We find that firms that accumulated arrears with a certain client are less likely to contract with that client in the year before the repayment shock (2011).[52] Firms are about 18.3% less likely to initiate a new contract with local governments with whom they have arrears. We also find that public arrears affect not only the probability of starting a new contract but also the volume of those contracts. It is worth noting that while arrears affect procurement decisions, the impact is only temporary. We find no significant impact of arrears upon the repayment shock in 2012.

These findings shed light on the relationship dynamics between firms and public procurement, particularly regarding the impact of late payments on business ties between customers and suppliers. Our results indicate that if public

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[52] While firms were also exposed to late payments in 2009 and 2010, the amount of accumulated arrears peaked in 2011.

administrations delay their payments, their customers might be hesitant to pursue public procurement contracts with them again in the future. The interruption of public contracting is probably mitigated by the findings in García-Santana & Santamaría (2023), which show there is local concentration in procurement, which is significantly explained by local governments' home bias. However, these effects seem short-lived, and once the creditworthiness of the buyers improves, commercial relationships are restored.

4.7. Conclusion

We study the effect of government arrears on firms' policies. To do so, we exploit as a natural experiment a large accelerated repayment of the government in Spain in 2012. Using a unique data set and a clean causal identification strategy, we find firms' corporate decisions are significantly affected by the unexpected government repayment program.

We show that the impact of this policy is different across firms. Financially unconstrained firms do not increase investment but instead use the liquidity received to repay financial debt and accumulate cash. More financially constrained firms significantly increase investment and repay suppliers after the repayment program, evidencing how this policy had spillover effects in the supply chain through trade credit.

From a policy perspective, our results provide important insight into the effectiveness of an unorthodox fiscal policy that does not change overall public liabilities. Early repayment of arrears affects corporate investment and economic growth, and has heterogeneous effects across firms.

Further, this Chapter sheds light on firms' strategies to counter late payments during economic downturns. While less financially constrained firms can borrow to mitigate the effects of government arrears, financially constrained firms might have to forgo investment opportunities and delay payment to suppliers. Implicitly, our research also sheds light on firms' inability to collateralize public arrears, thereby contributing to the sparse literature on financial factoring.

Our findings also underscore the impact of the late payment of accumulated arrears by public administrations on procurement contracting. We see that firms burdened with substantial arrears tend to shrink from contracting with the public sector. The repayment of the arrears restores the contracting between firms and public administrations. These relationships and their impact on public procurement contracts deserve further attention, given their significant implications for both firms and public entities.

Chapter 5. Government Turnover and External Financial Assistance

5.1. Introduction

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

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

We posit that, while continuing governments bear responsibility for their previous fiscal and financial decisions, newly elected governments can more easily attribute the need for assistance to their predecessors. As a result, re-elected incumbents may prefer to endure tighter borrowing constraint to protect

their information rents. In contrast, new governments, whose inherited debt reveals no information regarding their performance, may prefer to request assistance in order to gain fiscal capacity while highlighting the constraints they face. We follow two empirical strategies to explore how these different incentives impact the probability to reach a bailout agreement depending on government tenure.

We begin by using data on all IMF interventions between 1992 and 2021 to conduct a cross-country descriptive analysis of the relations between government turnover and financial assistance. 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. A limitation of this strategy is that turnover is likely to be correlated with both demand and supply-side factors associated with IFI funding. Although we implement several strategies to mitigate endogeneity concerns, the cross-country analysis does not allow us to reach the standards of identification that are customary in contemporary political economics.

Our main analysis overcomes this issue by focusing on a setting in which we can apply state-of-the-art methods to identify our causal effect of interest. We use information on roughly 4,000 Spanish municipalities during the Great Recession to obtain cross-sectional variation in both government turnover and financial decisions made by local governments after a credit shock. More specifically, we leverage on the decisions to request financial assistance in the context of a national program (the Supplier Payment Program, SPP) designed to deal with the mounting arrears that municipalities had accumulated through 2011. This program was introduced in 2012 and deployed financial resources of roughly

1% of Spanish GDP. It automatically converted the unpaid commercial debt (arrears) of Spanish municipalities into financial debt with the national government. In practice, the program worked as a credit shock for local governments, as it prevented municipalities from using commercial debt to finance their deficit again, and it forced them to pay back their existing arrears.

Crucially for our purposes, the law establishing the SPP gave local governments two options to pay back, which implied different financing conditions as well as different publicity levels. One option was to present a fiscal consolidation program (adjustment plan) to the national government. This would grant local governments access to a smoother backloaded adjustment – up to ten years to repay loan and interest – but it would make the adjustment salient. Alternatively, municipalities could choose not to present an adjustment plan, in which case they would repay their debt via retention of central government transfers within five years. This meant that local governments could choose between a smoother repayment scheme, which required a public adjustment program, or a more discreet front-loaded adjustment. In this sense, the dilemma faced by these municipalities is similar to that of financially constrained governments which may request assistance from the IMF.[53]

Our empirical strategy is based on a close-election regression-discontinuity design (RDD) that yields exogenous variation in government turnover. This strategy is analogous to that used to study the effect of political turnover on personnel changes and government performance in Akhtari, Moreira, & Trucco (2022), Toral (2023) and Marx, Pons, & Rollet (2022). It enables us to

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

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.

The Spanish setting offers five features which make it particularly well suited to study the question at hand. First and foremost, it allows us to perform an RDD with a large sample of municipalities which share the same electoral system and which suffer a simultaneous credit shock. Second, the conditions imposed by the SPP were strictly enforced by the national government. We observe that those municipalities opting for the discreet front-loaded adjustment reduce their expenditures and increase taxes in the short-run to a greater extent than those municipalities that present an adjustment plan. Third, unlike many IMF programs, the Spanish SPP does not involve any conditionality on policies beyond the establishment of a credible path to reduce debt and fiscal deficit. This removes ideological considerations from the decision of adhering to the plan. Fourth, considering that the interest charged by the central government is subsidized, taking more years to pay back the debt yields a higher net present value. This makes the backloaded adjustment the superior alternative for the municipality, which allows us to focus on the misalignment of incentives between voters and politicians. Finally, the fact that the timing of the elections and the timing of the program were not under the control of local politicians prevents some potential threats to identification (Hübscher & Sattler, 2017; Müller, 2023).

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, this results in a substantial increase in press coverage of the program. Analyzing the content of the news covering the adjustment plan using ChatGPT, we 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 by the opposition to intensify the criticisms, which does not happen when it is a new government who presents the plan. This suggests that while most newcomers present the adjustment plan to benefit from its financial advantages, many re-elected incumbents avoid presenting it to hide information about their past performance.

To gather further evidence on this mechanism, we perform three other analyses. First, we explore how our estimates of the effect of turnover on the propensity to present a plan vary depending on the amount of arrears. Re-elected incumbents with low levels of accumulated arrears may be less reluctant to present an adjustment program, because the associated signal about their performance is less consequential. In line with this prediction, we find that our main estimated effect is close to zero for municipalities with low levels of accumulated arrears, while it is positive and significant for the rest. Second, we use data on a survey of Spanish mayors — based on project Policonstraints – to

understand what these politicians think about correcting their past mistakes. We find that many mayors consider that rectifying policies can be politically costly since it makes errors salient. Also – and maybe related to this – we find that most mayors consider that newcomers are in a better position to face situations of financial distress relative to ongoing incumbents. Finally, we explore whether submitting an adjustment plan affects the probability of re-election. We find a negative and significant association between presenting an adjustment plan and getting re-elected four years after the SPP, but only for ongoing incumbents. Altogether, this reinforces the notion that re-elected governments do not present the adjustment plan to protect their information rents – despite this being financially suboptimal.

This paper contributes to the literature on the political economics of macroeconomic policy.[54] Specifically, we show how political constraints affect when and how governments deal with financial difficulties. In this sense, our work relates to studies on the political determinants of stabilization and fiscal reform (for surveys of this vast literature see for example Alesina, 2000; or Mahmalat & 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 regression-discontinuity design that allows us to identify our parameter of interest under relatively mild assumptions, using tools that are standard in the applied micro literature in political economics. In this, we distinguish ourselves from much of the empirical work in macroeconomic stabilization, which has typically relied on cross-country evidence (for recent

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[54] See Persson & Tabellini (1999); Alesina & Passalacqua (2016), and Yared (2019).

overviews of this literature, see Alesina, Favero, & Giavazzi, 2019a; Kose, Ohnsorge, Reinhart, & Rogoff 2022).[55]

The higher predisposition of newcomers to present an adjustment plan is aligned with the finding that politicians try to shift the blame to other actors in tough times (Weaver, 1986; Hinterleitner, 2017; Bursztyn, Egorov, Haaland, Rao, & Roth, 2022), and it connects the paper with the work exploring the empirical foundations of salience theory (Robertson, 1976; Petrocik, 1996; Dolezal, Ennser-Jedenastik, Müller, & Winkler, 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 & Mortensen, 2010; Greene, 2018). Here we show that not only they emphasize certain topics to enhance their electoral prospects, they also internalize this behavior from their political opponents and favor policies that limit the rhetorical resources of their competitors.

Namely, we find that politicians are significantly less prone to choose the efficient option – presenting an adjustment plan – when the alternative decision protects their reputation. This relates our work to the classical debate between the Chicago and the Virginia schools of political economy (see Becker, 1976 and 1985; Wittman, 1989; Crew & Twight, 1990; Coate & 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 suggest that it is electoral competition that promotes

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

such conduct, which introduces a new perspective on the effect of re-election incentives on politicians' misbehavior.

In the past decade, several empirical studies have shown how re-election incentives can discipline politicians in office, in consonance with the predictions of political agency models (Barro, 1973; Ferejohn, 1986; Banks & Sundaram, 1993). For example, previous work has shown how re-election incentives prevent corruption (Ferraz & Finan, 2011), encourage effort (Fouirnaies & Hall, 2022), and enhance policy implementation (De Janvry, Finan, & Sadoulet, 2012). Interestingly, in this paper we find that re-election incentives can also induce ongoing incumbents to choose an inferior policy to protect their reputation. This result closely relates to the work exploring the possible negative effects of a politician's career concerns on voters wellbeing (Canes-Wrone, Herron, & Shotts, 2001; Maskin & Tirole, 2004; Smart & Sturm, 2013), and it adds a new element to the discussion of the consequences of limiting mandates.[56] The evidence presented in our paper suggests that newcomers could be more ready to challenge the status quo and decide optimally, while re-elected incumbents may stick to failed policies, fearing the consequences of acknowledging past mistakes.

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[56] Part 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 & Gagliarducci (2017), which shows that tenure in office worsens procurement outcomes due to collusion between government officials and local bidders, or Bernecker, Boyer, & Gathmann (2021), which shows that re-election concerns might deter policy innovation.

5.2. 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 reaching a funding agreement with the IMF. We use a country-level panel covering the 1992-2019 period, which contains information on countries' political institutions, macroeconomic data and indicators identifying new agreements. We focus on democracies – governments elected or appointed as a result of competitive elections.[57] The resulting panel covers a total of 153 countries. Details on the construction of the dataset and descriptive statistics can be found in Section 7.3.2 (Appendix).

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

Naturally, the observed correlation between turnover and IMF funding may be an artifact of country-level characteristics and cyclical fluctuations in economic and electoral performance. For example, we know that during world economic downturns there is a higher frequency of changes in office (Brender & Drazen,

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[57] We use a definition of competitive elections based on the Index of Electoral Competitiveness from the 2020 Database of Political Institutions. See details in Section 7.3.2 (Appendix).

2008; Fair, 2008; Nunn, Qian, & Wen, 2018) and IMF interventions (Knight & 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 & La Ferrara, 2005; Gören, 2014; Arbatli, Ashraf, Galor, & Klemp, 2020). To mitigate these concerns, we use a panel to estimate:

$$Program_{ct}^{IMF} = \beta C_{ct} + \alpha_c + \delta_t + \gamma_1 \Delta GDP_{ct} + \gamma_2 left_{ct} + \gamma_3 right_{ct} + \mu_{ct} \qquad (5)$$

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 one if the country experienced a change in the party in power in the last election and zero if it is ruled by the previous incumbent. In some specifications, we also include as controls ΔGDP_{ct} – the growth rate of national GDP in U.S. dollars – as well as $left_{ct}$ and $right_{ct}$, which are dummy variables that take value one if the party in office is left-wing or right-wing respectively.[58] The coefficient of interest is β, which, under a suitable conditional exogeneity assumption, can be interpreted as measuring the effect of having a new party in power on the probability of making an agreement with the IMF.

In columns 1 through 3 of Table 18, we show estimates of β obtained using variations of the specification in Equation (5). In column 1, we present the unconditional linear probability estimate of 5.4%, which corresponds to the difference in IMF funding request rates mentioned above. We include country and time effects in column 2 to account for country-level characteristics and global

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

time trends which can affect both the probability to receive assistance from the IMF and government turnout. We find that the coefficient of interest remains significant, with a size of 3.7% relative to an 11% baseline probability of signing a funding agreement with the IMF. In column 3 we find that, after controlling for country GDP growth and for government's political orientation, the coefficient of interest remains qualitatively unchanged.[59]

Table 18. The Impact of Party Changes on IMF Programs - All IMF Funding Programs

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

Notes: The Table reports OLS estimates. IMF Program is a dummy variable that takes a value of one if the country approves an IMF funding program in that year, and zero otherwise. Party Change takes a value of one if the country is ruled by a new party, and a value of zero if the country is ruled by the previous incumbent party. Columns (2) and (3) include country and year dummies. Δ GDP is the growth rate of national GDP in U.S. dollars. Political party dummies take a value of one according to the orientation of the chief executive's political party. Column (4) includes group fixed effects (GFE) interacted with year dummies, following Bonhomme & 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 18 aim to mitigate the omitted variable problem, the assumption needed for exogeneity of the C_{ct} variable are still rather strong. To further address this concern, in Section

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

we conduct a complementary analysis restricting our attention to countries undergoing a banking crisis, as recorded in Laeven & Valencia's (2020) database. This seeks to attenuate the concern that unobserved external economic shocks explain both the recent change in office and the decision to request assistance to the IMF. Unsurprisingly, the average probability of an external bailout in this sub-sample is much higher than in the full sample (48.4%). Remarkably, we find that the probability to sign a funding program with the IMF rises between 25 and 29 percentage points when a newly elected party is in power during a banking crisis.

Altogether, these results provide evidence on the positive association between a change in office and the decision to ask for external financial assistance. Still, even in the subsample of banking crises, claiming exogeneity of our new government indicator remains a strong assumption. Moreover, in the cross-country sample it is hard to identify whether these estimated effects result from differences in governments' propensity to request assistance or on the Fund's willingness to provide it. In the rest of the paper, we use the case of Spanish municipalities to leverage close-election regression-discontinuity methods that allow us to make a stronger case for a causal link between tenure in office and the decision to publicly acknowledge the need of external financial support.

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

We now focus on achieving clean identification of the effect of interest by exploring an ideal setting (Spain's Supplier Payment Program), which allows us

to complement the previous descriptive analysis with rigorous causal estimates. This program was introduced during Europe's sovereign debt crisis and offered indebted local governments the possibility to publicly agree on an adjustment plan with national authorities in exchange for a smoother repayment profile of their arrears.

5.3.1. Institutional Setting

A. 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.[60] Regular municipal financing is based on transfers from the national government, which amount to approximately 50% of their income, transfers from regional governments, and local taxes. The most important local tax is a property tax. During the housing boom of 2000-2008, it was also common for municipalities to sell public land to obtain extraordinary revenues.

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

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

than 250 inhabitants, which use a single-district, closed-list, proportional electoral system.[61] In these municipalities, council seats (from a minimum of 7 to a maximum of 57 in Madrid) are assigned following a D'Hondt rule with a 5% vote share entry threshold. The municipal mayor is appointed by the council under a plurality rule. The most voted party can appoint the mayor directly if it obtains more than 50% of the seats. If no party commands a majority of seats, candidates need to receive the support of the majority of the council to be elected. If none of the candidates obtains this support, the candidate from the most voted party is appointed as mayor. There are no term limits, and local governments cannot call for elections, which occur simultaneously for all Spanish municipalities every four years.

B. Spain's Supplier Payment Program

For a comprehensive account of the institutional setting, see Chapter 2.

Importantly, the central government offered two different repayment options to municipalities in the SPP. Municipalities could opt for 10-year debt with a 2-year interest-only grace period. This option required the municipality to submit a fiscal adjustment plan, in an effort to minimize moral hazard in the context of these generous financing conditions.[62] This plan should be discussed and approved by the municipal council (the local equivalent of the parliament). Crucially, this gave the local opposition the chance to make salient the financial

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

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

situation of the municipality and the spending cuts and tax changes proposed by those local governments in the context of the impending adjustment.

Alternatively, municipalities could opt for a front-loaded adjustment that involved taking on 5-year debt with no grace period, to be paid via retention of fiscal transfers from the national government. Thus, local governments could choose between a smoother repayment scheme which required a public adjustment program, or a more discreet front-loaded adjustment.

The map in Figure 8 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. Spain is divided in 17 Autonomous Communities, a sub-national level of government. Municipalities belonging to the Basque Country and Navarre are excluded from the SPP, as they are the only two Autonomous Communities that have their own treasury, independent from the Spanish National Treasury.

Figure 8. Map of Spanish Municipalities
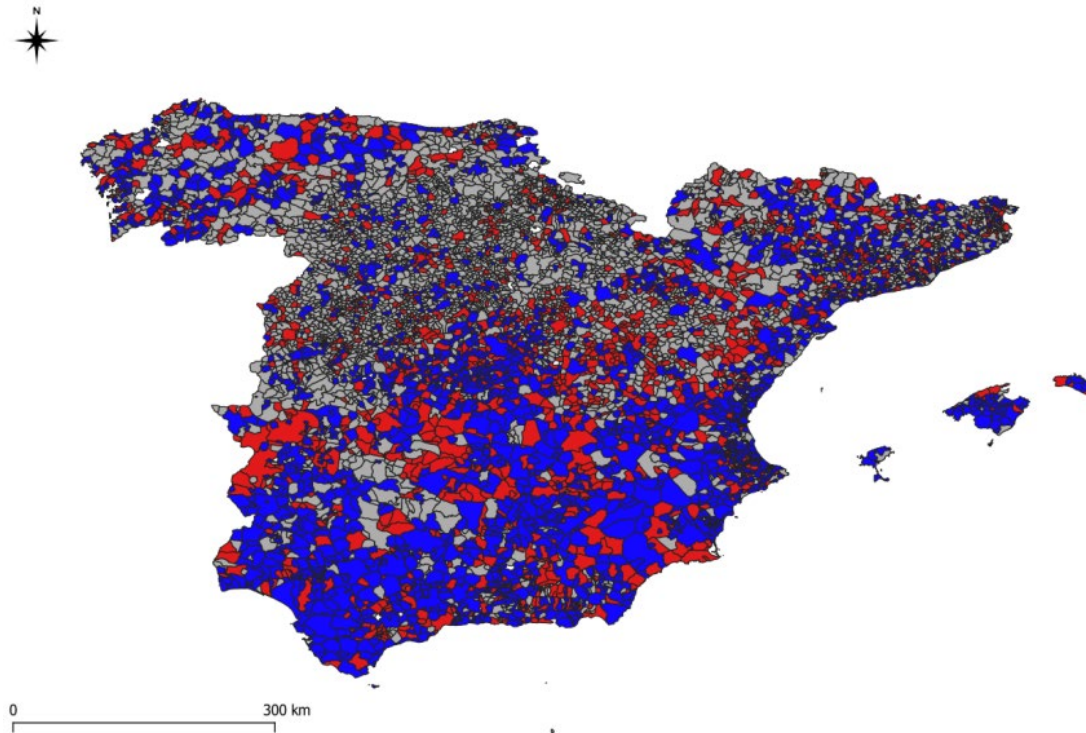
Notes: In grey, municipalities with no arrears. In blue, municipalities with arrears which presented a plan. In red, municipalities with arrears which did not present a plan. No data is available for the Basque Country and Navarre.

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

5.3.2. Data

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

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 from different taxes, or total spending. Also from MINHAP, we obtain data on arrears and data on the outstanding debt by municipality, available since 2009.

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

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

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

Municipal descriptives for our sample are presented in Table 19 and Table 41 (Appendix). In Table 19, we present the mean and standard deviation for several variables at the end of 2011, right before the SPP was introduced. We include population, outstanding debt per capita, total spending and revenues per capita, arrears per capita and the fraction of municipalities ruled by the biggest political parties in Spain: the center-left *Partido Socialista* (PSOE) and the center-right *Partido Popular* (PP). Panel A shows the information for all municipalities in Spain, panel B includes municipalities that participated in the SPP but did not do an adjustment plan (37% of municipalities with arrears), and panel C includes municipalities that participated in the SPP and carried out an adjustment plan (63% of municipalities with arrears). The average population of all municipalities in our sample is 5.8 thousand inhabitants. Accumulated arrears per capita are on average similar for municipalities that do not do an adjustment plan and for municipalities that do an adjustment plan.

Table 19. Summary Statistics

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

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

Table 41 (Appendix) compares averages of several variables across municipalities in which the challenger (column 1) or the incumbent (column 2) won the elections in 2011. That is, the table shows the differences between

locations which experienced government turnover in 2011 and locations that did not experience this turnover. We observe that municipalities ruled by a newly elected challenger exhibit a higher probability to do an adjustment plan, have lower spending and revenues per capita, and are more often ruled by PP rather than by PSOE. In the next section, we discuss how we proceed to obtain our effects of interest while avoiding the bias induced by these differences in characteristics.

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

5.3.3. Impact of SPP Funding Choice on Municipal Budgets

As discussed above, municipalities with arrears could choose between two options. Either they agreed on an adjustment plan with the national government in exchange for a smoother transition to stabilization, or they chose a more abrupt adjustment via the retention of intergovernmental 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:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k=2008}^{2015} \omega_k front_i \times \mathbb{I}\{t = k\}$$

$$+ \sum_{k=2008}^{2015} \gamma_k \log\left(arrears_i\right) \times \mathbb{I}\{t = k\} + \epsilon_{it}$$

(6)

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 one if municipality i chose the front-loaded option to pay for its SPP obligations, and $\log\left(arrears_i\right)$ 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.

This analysis is restricted to municipalities with unpaid commercial debt, i.e., those that were forced to assume a central government loan by virtue of the SPP. As a result, the coefficients $\{\omega_k\}_{k=2008}^{2015}$ can be interpreted as the differences between municipalities with $front_i$ = 1 and those with $front_i$ = 0 over time. That is, they indicate the evolution of the difference on transfers, spending and taxes between municipalities choosing the more discreet front-loaded adjustment and municipalities that opted for presenting an adjustment plan. The inclusion of the second sum in Equation (6) controls for differences in the levels of arrears per capita across municipalities in both groups.[63]

Estimates of the sequences of coefficients are reported in Figure 9. In panel A, we display coefficients for the difference in transfers. We observe that

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[63] The dynamic patterns are quite similar if we exclude the interaction terms in the second sum of Equation (6). The only noticeable difference is an earlier convergence of spending levels between both groups of municipalities. Results available upon request.

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 experienced an abrupt decrease in the transfers provided by the central government because they began to pay for the debt associated with the SPP.

Figure 9. Consequences of Government Retention Scheme

(A) Government Transfers

(B) Spending

(C) Property Tax Rate (IBI)
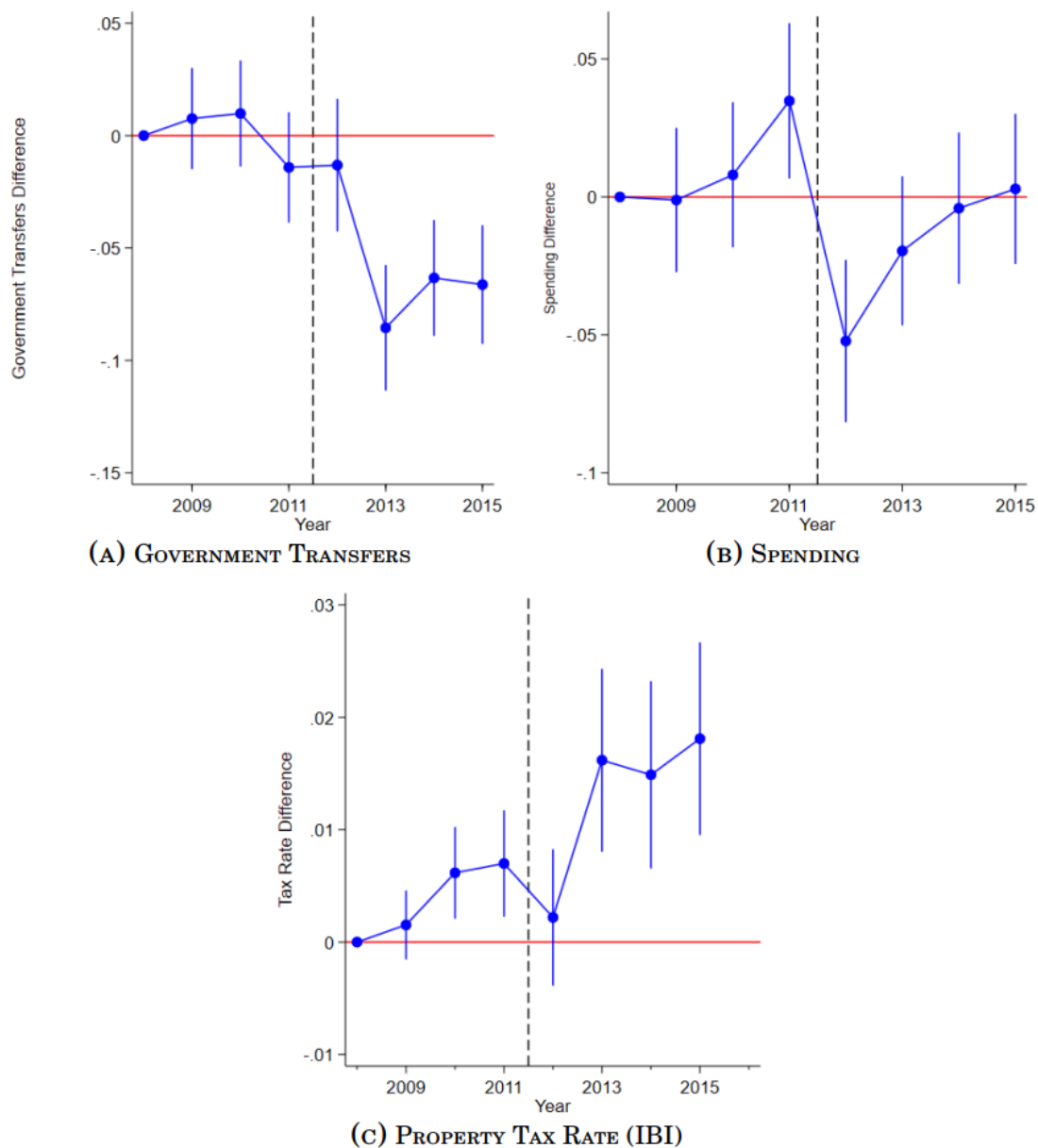
Notes: These Figures show point estimates and 95% confidence intervals for the effect of not presenting a plan on: the log of the transfers received from the central government (**panel A**), the log of total municipal spending (**panel B**), and the property tax rate (IBI) (**panel C**), for years

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2008-2015. All regressions include municipality fixed effects and year fixed effects. Figures plot the estimated coefficient for the interaction between a year dummy and a dummy that takes value zero if the municipality presents an adjustment plan, and value one otherwise. Standard errors are clustered at the municipality level.

How did this reduction in transfers affect municipal spending? Panel B shows a relative decline in municipal spending by late 2012, which is consistent with municipalities adjusting their spending levels ahead of the change in transfers. This relative reduction in spending persists to the end of our sample period in 2015. Was all of the front-loaded adjustment 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. According to Blanchard & Leigh (2013b), these procyclical policies after a recession would be suboptimal for the municipality, and the spike in the property tax rate would contradict the argument made in Barro (1979) for smoothing taxation.[64] Thus, the observed spending and taxing behavior of municipalities with no adjustment plan is aligned with the notion that mayors are making a choice in their own benefit.

Collectively, the patterns displayed in the three panels in Figure 9 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 (6) 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

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[64] See Alesina, Favero, & Giavazzi (2019a) for a review on macroeconomic outcomes of deficit reduction policies.

rather as suggestive evidence that the expected impacts that the front-loaded adjustment would have on municipal finances are indeed observed in practice. That is, we see these patterns as mostly descriptive, but nonetheless reassuring.

5.3.4. News Coverage of SPP

There was substantial coverage of the SPP in Spanish national and local news, which evidences the publicity usually associated to these types of arrangements. We use the Factiva database to analyze the coverage of the SPP (see dataset description in Section 7.3.4. (Appendix)). Panel A of Figure 10, reports the monthly number of news released in all Spanish newspapers that made a mention to the Suppliers Payment Program between 2011 and 2013. As expected, before the 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.

Figure 10. News Coverage of SPP
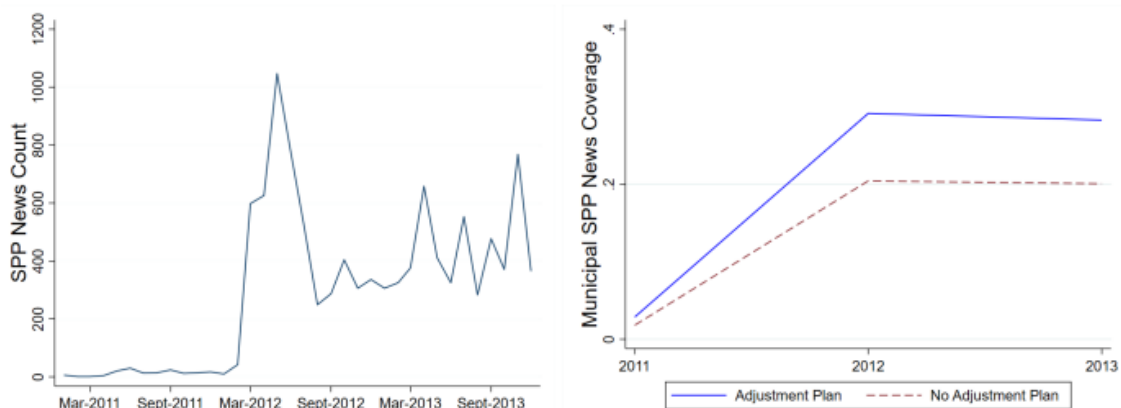
(A) Aggregate Monthly News Coverage   (B) Relative Coverage by Chosen Option

Notes: **Panel A** represents the total number of times that "Supplier Payment Program" appears in the news every month from January 2011 to December 2013. **Panel B** represents the average number of news covering the SPP per municipality and year calculated for

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

News coverage of the SPP was higher in municipalities which decided to submit an adjustment plan to the national government in exchange for a smoother repayment path. We document this by using data on the number of news covering the SPP by municipality. Panel B of Figure 10 shows the average by municipality for each year between 2011 and 2013, disaggregated by whether the municipality submitted an adjustment plan to the national government or not. We observe news coverage of the SPP is almost zero in 2011 and increases in subsequent years. Notably, news coverage is higher in 2012 and 2013 for municipalities submitting an adjustment plan to repay their arrears. We further analyze the link between government choices and news coverage in Section 5.6.

5.4. 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 governments in the probability to submit a public adjustment plan that allows them to carry out a smoother adjustment to fiscal stability.

As discussed in Section 5.2, in times of financial distress, both the probability of a change in office and the need of an externally supported fiscal adjustment increase. Hence, identifying the causal impact of tenure in office on the probability to request external support is difficult. The large number of Spanish municipalities, which share a common electoral system, and receive a simultaneous credit shock, allow us to draw quasi-experimental estimates of this

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causal relation. Moreover, the design of the Spanish SPP, presents a series of features that make it specially well suited to achieve a clean identification of the effect of politicians' private incentives on the decision to request assistance.

All municipalities in our sample had elections just a few months before the Spanish SPP was put in place. Alesina, Ardagna, & Trebbi (2006) and Alesina, Furceri, Ostry, Papageorgiou, & Quinn (2023) explain two potential sources of endogeneity related to the timing of the implementation. First, if governments have discretion on when to call for elections, they will do it before they conduct any fiscal adjustments (Hübscher & Sattler, 2017). Second, macroprudential regulation is systematically less likely to be implemented before elections (Müller, 2023). In Spain's SPP, both the timing of elections and the timing of the program are the same for all municipalities. This means they are completely independent from municipal governments' decisions.

Unlike many bailout programs sponsored by IFIs, the Spanish SPP has two features that allow us to focus on the possible conflict of interest that incumbents might face. First, the national government imposed no conditionality on policies – municipalities had discretion on how to attain fiscal sustainability –. This leaves aside the ideological discussion that is often present in the decision to adhere to an IMF rescue program, which typically imposes some concrete reforms. Second, the central government has enforcement power to claim back the arrears that it paid in the name of every municipality with overdue commercial debt. This, along with the favorable credit conditions for those who present an adjustment plan, yields a conservative estimate for the difference in net present value (NPV) of presenting a plan of over 60,000 euros for the average municipality with arrears (see Section 7.3.3. (Appendix) for details). This number

represents roughly 40% of the average deficit of local governments in 2011.

5.4.1. Empirical Strategy

We use a close-election regression discontinuity design (RDD) to induce exogenous variation on whether there was a change in office in 2011. To do so, we create a running variable for municipality 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*.[65] Note that ΔV_i will take positive values if the challenger wins the 2011 local election and negative values otherwise.

We use this running variable to estimate the effect of a dummy C_i, taking a value of 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 central 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 zero to one when ΔV_i crosses the threshold at zero – our RDD is fuzzy (Imbens & Lemieux, 2008). We estimate our parameter of interest by using two-stage least squares (2SLS). The estimating equations are:

$$C_i = \alpha_0 + \tau D_i + \pi_1 \Delta V_i + \pi_2 D_i \Delta V_i + v_i \tag{7}$$

$$Ad.Plan_i = \alpha_1 + \beta C_i + \rho_1 \Delta V_i + \rho_2 D_i \Delta V_i + v_i \tag{8}$$

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[65] It is important to note that the word incumbent here refers to the 2007-11 incumbent and not to the 2011-15 incumbent.

Our parameter of interest is β, which can be interpreted as the effect of having a new party in power on the probability of choosing an adjustment plan.[66] Equations (7) and (8) correspond to our first- and second-stage, respectively. Variable D_i is defined as $D_i = \mathbb{I}\{\Delta V_i > 0\}$ 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 the parameter of interest using a local linear regression weighted by a triangular kernel. The state-of-the-art in the estimation of these parameters uses the routine proposed in Calonico, Cattaneo, Farrell, & Titiunik (2017), which incorporates data-driven procedures to select a bandwidth, adjusted standard-errors to account for the bandwidth selector and a bias correction procedure developed by the authors.[67] We will refer to the optimal bandwidth selected by this algorithm as the CCT bandwidth. In Section 5.4.3 we discuss the robustness to the bandwidth choice, the choice of the kernel, the polynomial length used to adjust for values of the running variable and alternative methods to compute that running variable.

Before we move to report our estimates for β, we discuss the plausibility of some of the assumptions required for the validity of the regression-discontinuity design in our context. In the first place, we discuss the assumption of no manipulation. While parties influence electoral results through their actions, it is unlikely that they can perfectly manipulate electoral outcomes. We provide evidence consistent with this notion by reporting the histogram of the running

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

variable around the threshold in Figure 18 (Appendix). The formal statistical tests described in McCrary (2008) and Cattaneo, Jansson, & Ma (2020) yield large *p*-values of 65% and 76%, respectively, confirming that perfect manipulation of the running variable is very unlikely in this context.

To further emphasize the validity of our research design, we also analyze the covariate balancing at the threshold. Our empirical strategy ensures that pre-determined characteristics of the municipalities and the governments in power before 2011 are balanced on both sides of the threshold. Figure 11 and Figure 19 (Appendix) illustrate this point. Figure 11 shows that demographic and financial characteristics of municipalities vary smoothly at the threshold. Importantly, this includes variables measuring the level of arrears and debt accumulated by municipalities through 2011. Figure 19 (Appendix) shows that characteristics of the incumbent government in power before the 2011 election also vary smoothly at the threshold. Tables 42 and 43 (Appendix), 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.

Figure 11. Covariate Balancing – Municipal Characteristics
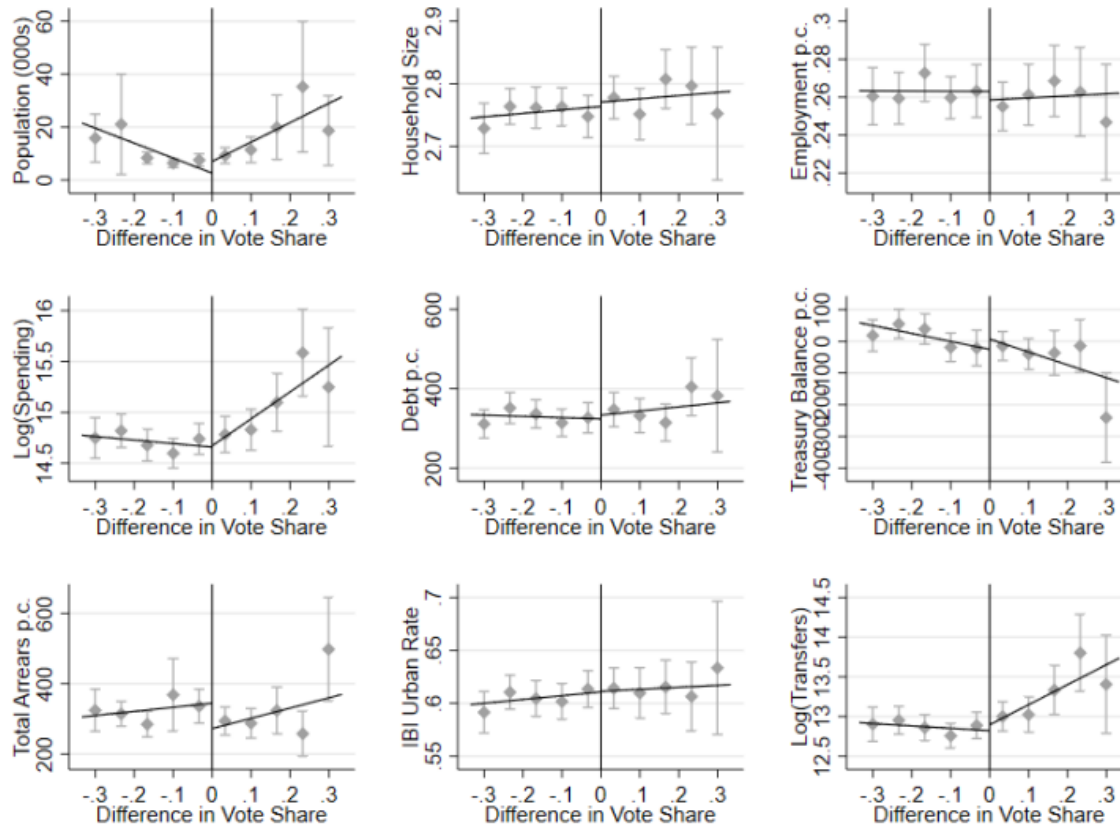
Notes: The horizontal axis represents the vote share difference between the challenger and the incumbent. From left to right and top to bottom the vertical axes represent population (in thousands), average household size, fraction of employed population, logarithm of municipal spending per capita, municipal 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.

5.4.2. Baseline Results

We illustrate our first-stage in the top panel of Figure 12. 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. Third degree polynomials are estimated separately on both sides of the threshold. Gray dots correspond to averages of the dependent variable for different bins of the variable in the horizontal axis, and vertical lines correspond to 95% confidence intervals. We observe a substantial jump in the probability of having a change in the party

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in power at the threshold. The gap in probability is roughly 0.5, indicating the design is fuzzy and not sharp.

Figure 12. Party Changes and Adjustment Plans: First-stage and Reduced-Form
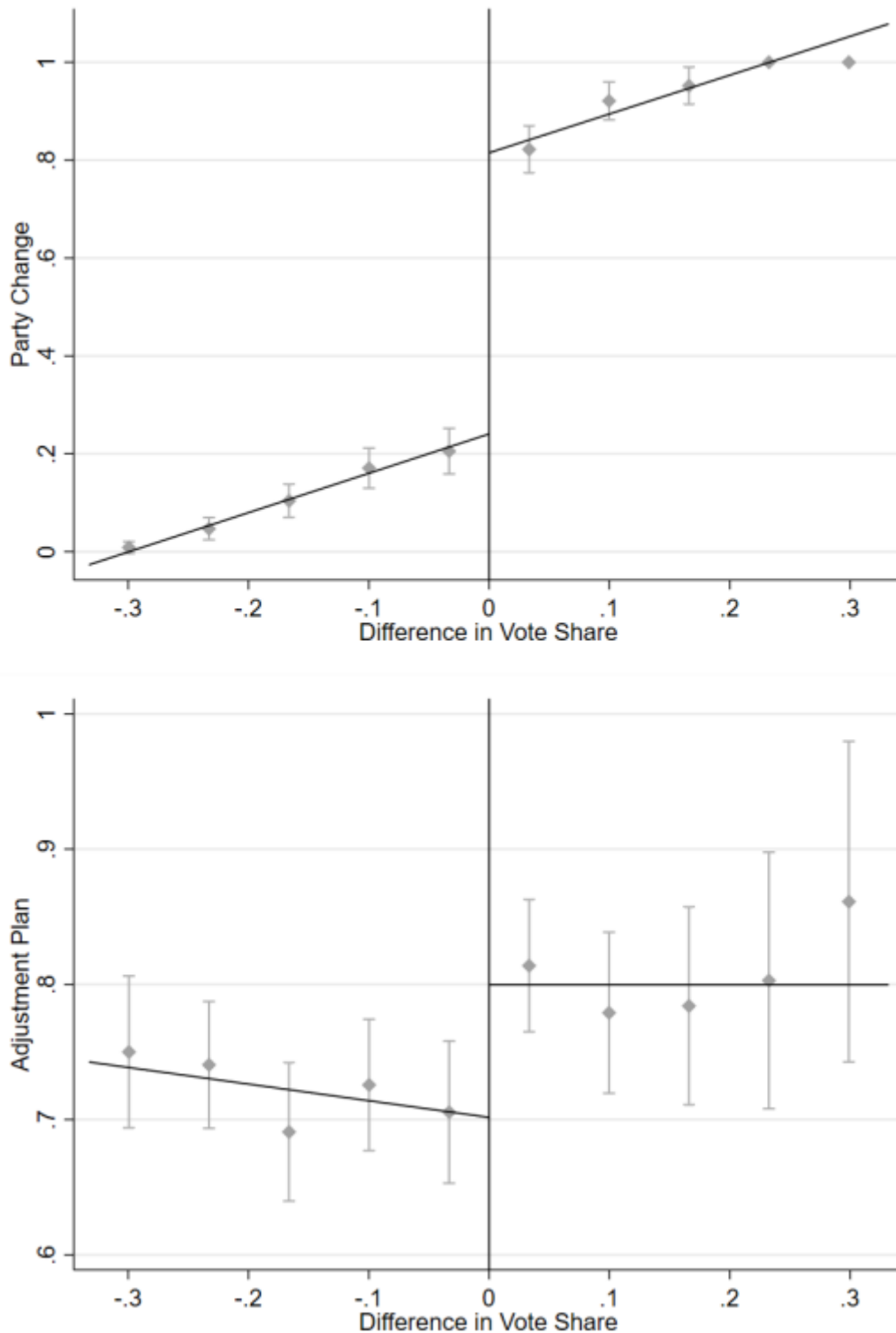
Notes: In both panels, the horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. The top panel illustrates the first stage; hence, the vertical axis measures the probability that the challenger is appointed as

mayor. The bottom panel plots the reduced-form relationship between running variable and outcome. Solid lines represent 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.

The bottom panel of Figure 12 illustrates the reduced form effect of crossing the threshold on the probability of having an adjustment plan. Other elements of the graph are analogous to those described in the top panel. The discontinuity at the threshold indicates that when the challenger wins the election, we observe an increase in the probability of presenting a plan of roughly 0.15.

We now turn to our main empirical results, which are the 2SLS estimates reported in Table 20.[68] Column (1) reports the effect of a change in the party in power on the probability of presenting a plan. The estimated effect is large and statistically significant, indicating that it is 30 percentage points more likely that newly elected governments submit an adjustment plan than ongoing incumbents. The first-stage F-statistic is 112, well above the conventional threshold for weak instruments. In columns (2) and (3) we add controls. Column (2) includes the controls displayed in Figure 11, which are demographic and financial characteristics of municipalities.[69] Column (3) includes the controls displayed in Figure 19 (Appendix), which are characteristics of the incumbent government in power before the 2011 election. The effect remains significant and similar in magnitude.

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[68] We report the associated first-stage coefficients in Table 44 (Appendix).

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

Table 20. Change in Office & Adjustment Plans

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

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

In sum, the results in Table 20 are in line with our hypothesis: newly elected governments are more likely to choose a smoother adjustment than ongoing incumbents. We posit that this reluctance stems from incumbents' hesitation to publicize the poor state of public finances, which they may have contributed to creating. We discuss evidence for this and other mechanisms in Section 5.5.

5.4.3. Robustness Checks

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

First, we reproduce our RD estimates using an alternative definition of the dependent variable. In our main analysis, we define the dependent variable as a dummy that takes a value of one if the municipal government presents a plan, and a value of zero otherwise. We use this definition because we are trying to understand government decision-making and local governments only decide

whether to submit a plan, not whether that plan will ultimately be approved by the central government. That said, we can use an alternative definition which only takes a value of one if the municipality presents a plan that is approved. This amounts to classifying those municipalities that present a plan that is not approved together with those that do not present a plan at all. Table 45 (Appendix) shows this change in the definition of the dependent variable makes no qualitative difference to our results. This is perhaps not surprising, as less than 7% of all local adjustment plans in the context of the SPP were rejected by the central government.

We also explore the sensitivity of our RD estimates to bandwidth choice. As explained in Section 5.4.1, the estimation of our parameter of interest uses the routine proposed in Calonico, Cattaneo, Farrell, & Titiunik (2017), which incorporates data-driven procedures to select a bandwidth and adjusts standard-errors to account for the bandwidth selector. In Figure 13, we evaluate the stability of our main estimated effect for different bandwidths around the threshold. We show that, for all bandwidth choices in the [5%, 20%] interval, our coefficient of interest is statistically significant at 95% confidence intervals and comparable in magnitude to those reported in Table 20.

Figure 13. Robustness of RD Estimates to Bandwidth Choice
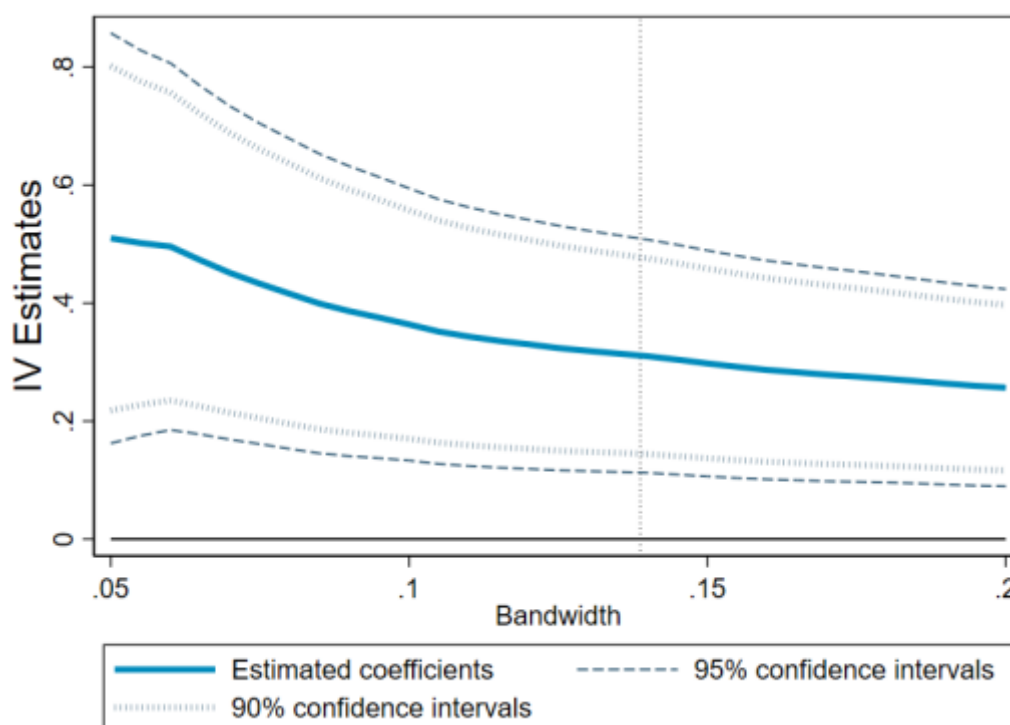
Notes: The horizontal axis represents different bandwidths around the threshold. The vertical axis represents the size of the estimated effect of having a new government on the probability of presenting an adjustment plan. The solid line corresponds to point estimates for different bandwidths. Dotted and dashed lines represent 90% and 95% confidence intervals, respectively.

Our baseline estimates are obtained using a triangular kernel to weight observations around the threshold and a linear polynomial to control for values of the running variable. We can show that these methodological choices are not important in our case. In Table 46 (Appendix), we report estimates of the effect of interest when controlling for higher-order polynomials in the running variable and when using a uniform kernel to weight observations. The resulting point estimates fall in the range between 0.2 and 0.4 and are always significant at conventional levels.

Finally, we consider an alternative definition of the running variable. One of the insights present in Folke (2014) and Fiva, Folke, & Sørensen (2018) is that, in multi-party systems, the distance to a change in either the composition of the

local council or who wins the election depends on the number of parties running in that election and on the associated distribution of vote shares. In our main analysis, the main running variable is simply defined as the distance between the vote shares of the challenger and the incumbent. Alternatively, we can determine the running variable by calculating the proportion of votes we would have to re-distribute from the challenger to all other parties running in that local election until that challenger changes from winning to losing the election or vice-versa. This is done by assigning redistributed votes across parties based on their initial vote shares. Estimates of the effect of having a new mayor on the probability of submitting an adjustment plan, obtained when using this alternative running variable, are provided in column (2) of Table 47 (Appendix). Reassuringly, the estimate of 0.286 is similar to the one reported in our main analysis.

5.5. Mechanisms

Our results show that new governments in power have a higher probability of presenting an adjustment plan. We posit that this difference is driven by the fact that presenting an adjustment plan makes the financial problems of the municipality more salient. As we describe in Section 5.3.4, municipalities start to build their arrears during the 2007-2011 term, after the onset of the global financial crisis in 2008. We hypothesize that, while a new leadership assuming in late 2011 can blame the previous incumbent for the accumulated arrears, a continuing incumbent may not be willing to reveal this issue to their voters, even if that implies choosing a sub-optimal policy. This could explain the differences in behavior documented above.

In this section, we provide evidence in support of that hypothesis. First, we complement the descriptive results presented in Section 5.3.4 and use information of coverage of SPP in Spanish media outlets to explore the impact of presenting an adjustment plan on voters' information set. Second, we use ChatGPT to conduct a text analysis of news covering the SPP. We study the origin and destination of criticisms included in these pieces of news and show that these are shaped by recent changes in government and the decision to implement an adjustment plan. Finally, we provide evidence consistent with incumbents avoiding to present an adjustment plan to protect their information rents. In the next section we discuss and discard several alternative mechanisms.

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

We use a difference-in-differences approach to estimate the effect of the local governments' repayment choices on press coverage regarding their participation on the SPP. We restrict our attention to municipalities that accumulated arrears and are thus participating 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 (9) shows the regression equation.

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

(9)

$I(News_{it})$ is a dummy that takes value one if *Supplier Payment Program* appears in the news together with 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 zero otherwise. $Post_t$ is a dummy variable that takes value one in 2012 and 2013, and value zero in 2011.[70]

Results reported in Table 48 (Appendix) indicate that presenting an adjustment plan impacts a voter's information set, as it increases the likelihood that its participation on the SPP gets media coverage. This finding is robust to controlling for municipality fixed effects and for the yearly total number of news per municipality.

To measure how news coverage changes with government turnover, we use our RD design. We estimate a modified version of the system in Equations (7) and (8), in which the second-stage outcome is replaced by the dummy $I(News_{it})$, taking value one 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 14. It shows a significant increase in SPP coverage at the threshold. Second-stage estimates, reported in Table 49 (Appendix), 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.

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[70] We can explore the extensive margin using the log of the number of SPP news as the dependent variable in Equation (9) instead of the dummy. This leads to the same qualitative findings.

Figure 14. Effect of Change in Government on SPP News Coverage
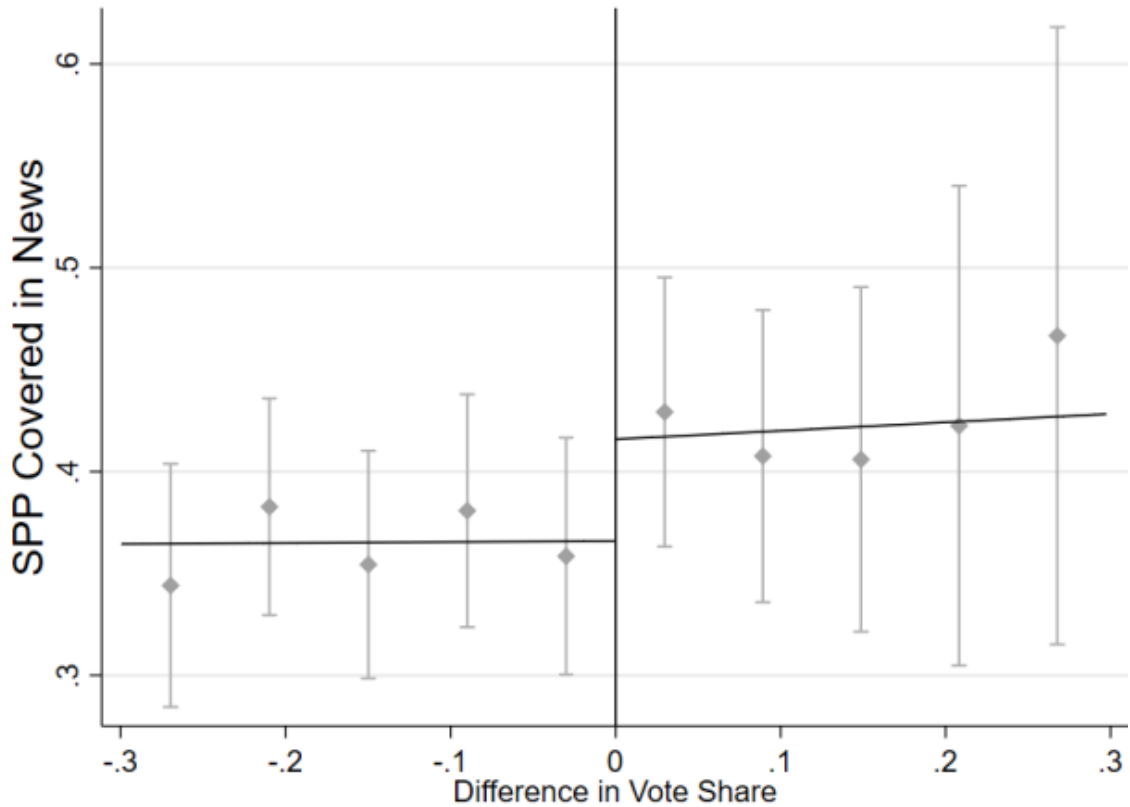
Notes: The figure plots the relationship between the running variable and the probability that a municipality is featured in news about the SPP in either 2012 or 2013. The horizontal axis corresponds to the running variable, defined as the vote-share difference between the challenger and the incumbent. 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.

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.

5.5.2. The Blame Game: Analysis of the SPP News Content

The natural question that emerges after confirming that presenting an adjustment plan impacts voter's information set, is to understand how. For this purpose, 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 to the current or previous municipal governments, as well as which is the source of the criticism (i.e., government or opposition). Details of the procedure used to download and process the data can be found in Section 7.3.4. (Appendix). The processed sample includes 11,356 articles associated to 805 individual municipalities, with over 60% of them containing some form of criticism directed to the (current or previous) municipal government.

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 one 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 21. We observe that changes in the party in power are associated with an increase in the proportion of criticisms originating in the current government, a decrease in criticisms issued by the current opposition, and an increase in criticisms targeted to the previous administration. 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.

Table 21. 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	0.101***	-0.060***	0.092***	-0.047*
	(0.028)	(0.020)	(0.012)	(0.028)
Num. of Municipalities	724	724	724	724
Observations	10934	10934	10934	10934

Notes: Linear probability model estimates obtained using news article-level regressions. In column 1, the dependent variable is a dummy taking value one if the article contains criticisms emitted by the government. In column 2, the dependent variable is a dummy taking value one if the article contains criticisms emitted by the opposition. In column 3, the dependent variable is a dummy taking value one if the article contains criticisms directed at the previous government. In column 4, the dependent variable is a dummy taking value one 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 11 for full list). Standard errors clustered at the municipal level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

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 5.5.1. Reduced-form graphs illustrating the effect of party change on the source of criticisms are reported in Figure 15. 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 Table 50 (Appendix), which show the 2SLS estimates, corroborate these findings.[71]

Figure 15. Origin of Criticism in SPP News: RD Reduced-Forms

(A) CRITICISM BY GOVERNMENT (B) CRITICISM BY OPPOSITION
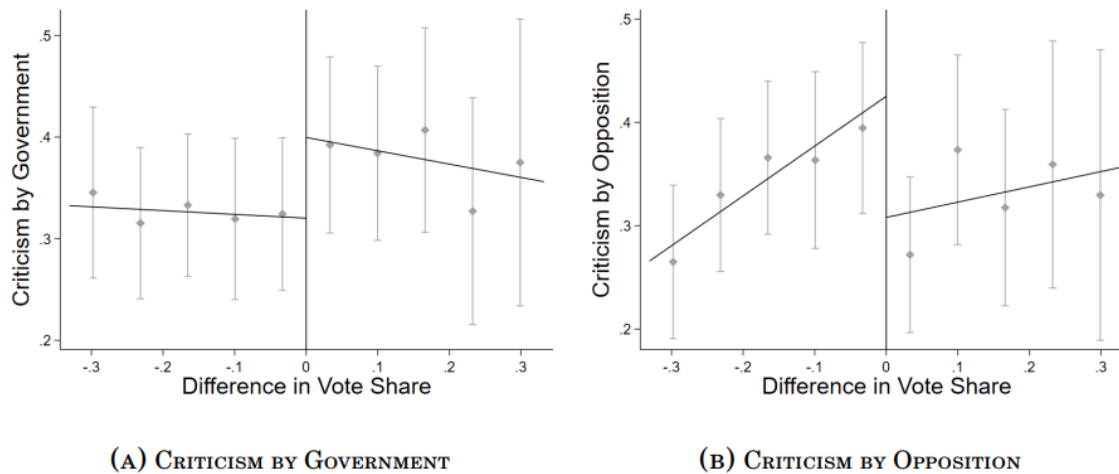
Notes: Panel A plots the reduced-form relationship between running variable and the fraction of articles that feature criticisms by the local government in our SPP sample of news. Panel B plots the reduced-form relationship between running variable and the fraction of articles that feature criticisms 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.

We confirm that, in news covering the SPP, the opposition criticizes more fiercely continuing incumbents, while newcomers try to pass the buck to their predecessors. Next, we explore the effect of presenting an adjustment plan. For this purpose, we estimate regressions of the form:

$$Y_{ji} = \eta Ad.Plan_i + \gamma' X_i + \varepsilon_{ji}$$

(10)

---

[71] Similar reduced-form and 2SLS estimates obtained using the destination of criticisms as our outcomes of interest are reported in Figure 20 (Appendix) and Table 50 (Appendix). While the sign of the reduced form discontinuities is consistent with our hypothesis, estimates are fairly imprecise and insignificant at conventional levels.

where $Ad.Plan_i$ is a dummy taking value one 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 one if the piece of news contains criticisms directed to the current government, a dummy taking value one if the criticisms are made by the opposition, and a dummy taking value one 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 22. 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.

Table 22. 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	0.016	0.123**	-0.026	0.090*	-0.009	0.083**
	(0.034)	(0.050)	(0.047)	(0.048)	(0.029)	(0.038)
Party Change	Yes	No	Yes	No	Yes	No
Num. of Municipalities	237	335	237	335	237	335
Observations	3880	5807	3880	5807	3880	5807

Notes: Linear probability model estimates obtained using news article-level regressions. For columns 1 and 2, the dependent variable is a dummy taking value one 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 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 11 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.

5.5.3. 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 & Drazen, 2008; Drazen & Eslava, 2010), and taking on account that the opposition intensifies the criticisms after an adjustment plan, it is reasonable to think 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.

In Table 23, we run the main specification of Section 4 for three different sub-samples. We divide municipalities according to their pre-existent level of arrears, and explore the effect of a change in office on the probability of

presenting a plan. For those municipalities in the bottom tercile of the distribution (column 1), the prevalence of presenting a plan is 50%, and there is no significant difference depending on government's tenure. This changes for the second and third terciles of the arrears distribution (columns 2 and 3). The average probability of presenting a plan rises above 80%, but there are significant differences by tenure. While the majority of newcomers present a plan when the level of arrears is sufficiently high, many re-elected incumbents remain reluctant to do so. This is consistent with the fact that municipalities with a high level of arrears experience higher gains in terms of NPV when they present a plan. But the signal that the plan sends regarding past government's performance also worsens. This poses a trade-off to continuing incumbents which newcomers do not face.

Table 23. Change in Office & Adjustment Plan - Heterogeneity

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

Notes: The table presents Two-Stage Least Squares (2SLS) estimates of the effect of a 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.

We get direct evidence of politician's awareness of this trade-off using the survey that Prof. Pedro Rey-Biel and his team administered to 126 Spanish

Mayors.[72] This survey seeks to understand the determinants of evidence-based policy implementation. It includes 31 items including gender, age, level of studies and diverse questions surveying policy evaluation habits, relevance of different economic sectors for the municipality, willingness to get information about policy efficiency, and others. Before Prof. Rey-Biel and their team ran the survey, we introduced a couple of specific questions about how to carry a fiscal adjustment and how it is shaped by government turnover.

Table 24 summarizes the answers to the subset of questions that are particularly relevant to our study. 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 a competent politician, an overwhelming majority of mayors (over 90%) declared that they would indeed change it.[73]

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[72] Project "Policonstraints", Social Research Grant of Fundación La Caixa.
[73] 6% answered maybe. In the comment section they qualify their answer raising the following issues: *Does this evidence come from a sample of similar municipalities to my own? Are the policies in the evaluation really similar to mine? Is the historical context comparable?*

Table 24. 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%

	Yes	No	
2. Have you ever changed a policy that was not working?	52.3%	46.7%	

	Strongly Agree	Neither Agree nor Disagree	Strongly Disagree
3. How much do you agree with the following statement: 'We are human beings and we all make mistakes. Sadly, often we cannot correct past mistakes the way we should, because the opposition would use this to make our errors more salient'.	20.4%	52.0%	27.5%

	A Newcomer	An Ongoing Incumbent	Does not make any difference
4. Suppose that a newly elected government starts the term with problems in the municipal accounts (for instance: the municipality has trouble to pay its suppliers). This situation would be easier to address for:	36.7%	13.2%	50.0%

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

Interestingly, when they are asked whether they have ever done so (Question 2), only 50% answers 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 sentence: "*We are human beings and we all make mistakes. Sadly, often we cannot correct past mistakes the way we should, because the opposition would use this to make our errors more salient*", a surprising 20% declared that they strongly agree. This is notable considering that we are surveying professional politicians, who have incentives to signal virtue (as they did in Question 1). We find that less than 30% of mayors disagree with the aforementioned statement, which suggests that many have just naturalized that this is how politics work. This could explain why in question 4, we observe that it is three times more likely that mayors point a

newly elected government as being in a better place to navigate financial troubles (relative to an ongoing incumbent).

Finally, after showing that presenting a plan impacts voters' information set, that politicians are aware, and that they adapt their behavior consequently, we explore how it affects electoral behavior in the next election. Namely, we run the following OLS to explore the relation between presenting a public adjustment plan to get better financing conditions, and the probability of re-election in 2015:

$$R_i^{2015} = \alpha_0 + \alpha_1 I_i + \alpha_2 Ad.Plan_i + \alpha_3 Ad.Plan_i \times I_i + \gamma X_i + \mu_i \qquad (11)$$

where R_i^{2015} is a dummy taking a value of one if the party in power before the 2015 election was re-elected. I_i (Incumbent) takes a value of one 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 one 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. Thus, we only interpret our findings as suggestive or descriptive in this context.

Estimates for the coefficients in Equation (11) are provided in Table 51 (Appendix), along with the combined effect of α_2 and α_3. We observe that governments who agree on an adjustment plan with the central 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 could suggest that continuing incumbents may actually bear an electoral cost if they present a plan, as they fear they would, while newcomers would not be affected.

5.6. Alternative Explanations

The evidence gathered in Section 5.5 is consistent with information rents explaining the reluctance of re-elected incumbents to present an adjustment plan. However, there are several alternative mechanisms that are worth exploring. In this section, we analyze these competing explanations.

5.6.1. 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 Table 52 (Appendix). 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.[74] These findings are predictable. First, mayor's age decreases at the threshold because newcomers are generally younger than incumbents. Second,

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[74] Balanced 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 Figure 21 (Appendix).

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 2013 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, the party of the challenger, and the party of the mayor in 2012. Finally, we consider an alternative estimation strategy where we estimate the effects of having a mayor from PP or PSOE on the probability of presenting an adjustment plan.

Estimates obtained after trying to account for elected government characteristics are reported in Table 25. Column 1 in panel A reproduces our baseline specification for comparison purposes. In column 2, we include two dummy variables that take value one when the incumbent is from PP and from PSOE, along with a control for the elected mayor's age. The estimated coefficient of interest continues to be large and statistically significant. In columns 3 and 4 we estimate our main specification after restricting the sample to municipalities where the initial incumbent was from PSOE and from PP, respectively. We continue to find large and significant effects for both sub-samples, indicating that challengers are more likely to present an adjustment plan, no matter whether incumbents are from PP and PSOE. We do something analogous in columns 1 and 2 of panel B. We report 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 large and positive. Both challengers from PSOE and from PP are significantly more likely to present an adjustment plan than the incumbents in their respective municipalities. Lastly, in columns 3 and 4 of panel B we provide estimates for different sub-samples based on the party in power after the 2011 election. In column 3 we restrict the sample to municipalities ruled by PSOE in 2012. We observe a positive and significant coefficient, of a magnitude comparable to those reported in panel A. This shows that mayors from PSOE who were challengers in the previous term are more likely to present an adjustment plan than mayors from PSOE who were incumbents. In column 4, we replicate this result for PP mayors. Albeit imprecisely estimated due to the reduced sample size, the sign and size of the coefficient suggests that mayors from PP that were challengers in the previous term are also more likely to present a plan than mayors from PP who were incumbents.

Table 25. Leadership Change & Adjustment Plans By Party

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

Notes: The Table presents Two-Stage Least Squares (2SLS) estimates of the effect of a change in municipal government on the probability of presenting an adjustment plan. . In panel A, the first column is the baseline specification, the second column includes mayor's age, and dummies for PP incumbent and PSOE incumbent as controls, the third column restricts the

sample to municipalities with a PSOE incumbent and the fourth column to municipalities with a PP incumbent. In panel B the first column restricts the sample to municipalities with a PSOE challenger, the second column to municipalities with a PP challenger, the third column to municipalities ruled by a PSOE Mayor, and the fourth column to municipalities ruled by a PP Mayor. We report estimates from local linear regressions with a triangular kernel and first-degree polynomials fitted separately at the two sides of the threshold. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

The consistent results obtained for the different partitions of our sample 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 Table 53 (Appendix). Columns 1 and 2 report the effect of having a PP mayor on the probability of presenting an adjustment plan, and columns 3 and 4 report the effect of having a PSOE mayor on the probability of presenting an adjustment plan. Columns 2 and 4 include our usual set of covariates. We find insignificant effects across the board for both parties. The absolute value of the point estimates are at most 1/9 of the effects reported in Table 20, providing conclusive evidence that our main effect of interest is not driven by partisan differences in the propensity to submit an adjustment plan.

5.6.2. Unobservable Characteristics of the Elected Government

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

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

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 & Nannicini, 2011; or Baltrunaite, Bello, Casarico, &

Profeta 2014). Results reported in Table 52 (Appendix) indicate that these quality measures vary smoothly at the threshold.

To deal with potential unobservable differences in quality, our second approach 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.[75] Thus, a good candidate can obtain a bad result due to a negative shock to her party in her region, and vice versa. We use this feature to create sub-samples of competing candidates with varying differences in quality.

We construct the leave-one-out average vote swing experienced by party p in municipality i belonging to province s during the election of 2011:

$$Partyswing_{ip} = \sum_{\substack{l \in s \\ l \neq i}} \frac{V_{lp2011} - V_{lp2007}}{N_s - 1} \tag{12}$$

where V_{lp2011} is the vote share of party p in municipality i 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:

$$NoSwingMargin_i = \Delta V_i - Partyswing_{iG} + Partyswing_{iB} \tag{13}$$

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

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[75] Regional 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.

"no swing" margin, the incumbent would have lost it. Therefore, it seems reasonable to claim that detracting regional party shocks increases the relevance of candidates' quality on electoral performance. 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 26 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.[76] A very similar estimate is observed in column 2 of Table 26, where we focus on the subsample of challengers whose quality is relatively high in relation to incumbents. This suggests that differences at the threshold in the relative quality of candidates do not explain our main findings.

Table 26. Heterogeneity Analysis - Candidate's Quality

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

Notes: The table presents two stages least squares estimates of the effect of a change in office on the probability of presenting an adjustment plan. In the first column, we restrict our attention to municipalities in which incumbents would have won the election in the absence of party shocks. In the second column, we restrict our attention to municipalities in which incumbents would have lost the election in the absence of party shocks. We report 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.

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[76] We 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.

Following the approach proposed in Angrist & Rokkanen (2015), we corroborate that neither quality, nor any other compensating differential, explains our main findings. Under a conditional independence assumption (CIA), the method proposed in Angrist & Rokkanen (2015) allows us to obtain estimates of the RD coefficient corresponding to observations away from the cut-off. The tests reported in Figure 21 (Appendix) show that, conditional on our set of controls, presenting an adjustment plan is indeed unrelated to the running variable. We interpret this as evidence that the CIA required to follow the method in Angrist & Rokkanen (2015) is plausible. Then, in Table 54 (Appendix), we report two CIA-based estimators of the RD coefficient away of the cut-off. We use the linear re-weighting method discussed in Kline (2011) and a version of the Hirano, Imbens, & Ridder (2003) propensity score estimator. Reassuringly, both yield positive and significant estimates of the effect of a change in office on the probability of presenting an adjustment plan away from the threshold. This indicates that the effect of interest does not arise due to any compensating differential varying at the threshold.

5.7. 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 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

Chapter, we delve into an analysis of how political tenure and electoral incentives impact the decision to request a bailout.

Our central argument revolves around two key points. Firstly, agreements with institutional lenders are inherently public and serve as indicators of past government performance. Secondly, 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.

Examining the universe of IMF agreements since 1990, we identify a positive and significant association between reaching an agreement with the IMF and having a government in its first mandate. To obtain state-of-the-art causal estimates of the relationship between turnover and assistance, we leverage on a national program implemented by the Spanish government which operated as a simultaneous credit shock for nearly 4,000 municipalities. We employ a close election regression discontinuity design to show that newly elected local governments in Spain are 30 percentage points more likely to publicly agree to an adjustment program with the national government in exchange for improved financing conditions. We further substantiate our findings through a survey of Spanish mayors and an analysis of news content, concluding that incumbents implement a suboptimal policy — refraining from presenting a plan — to safeguard their reputation. This informs the design of future bailouts in an incentive compatible manner, and more generally, it illustrates how past actions

and electoral incentives restrain the ability of incumbent parties to choose what is optimal for their citizens.

Chapter 6. Conclusion

6.1. Summary

The 2008-14 economic crisis significantly worsened Spain's public finances, leading to increased budget deficits and public debt, as well as longer payment periods to vendors. As a result, unpaid invoices by subnational governments peaked in December 2011 at €28.5 billion (2.7% of GDP). In response, the central government launched the Supplier Payment Plan (SPP) in 2012 to address these overdue payments. In Chapter 2, we provide a comprehensive description of the SPP.

This dissertation evaluates this policy, focusing on the causes of government arrears, the impact of delayed payments on corporations and procurement, and the role of incentives in bailout agreements.

In Chapter 3, we provide suggestive evidence that the extent of government arrears accumulation by Spanish municipalities through 2011 (intensive margin) was negatively influenced by construction activity, and positively influenced by current spending and interest payments. Additionally, we also find that construction activity is better at explaining the extensive (how many municipalities accumulate arrears) than the intensive margin. Finally, while our analysis indicates a positive relationship between arrears and the level of interest payments, it also reveals a negative relationship with their deviation from the within-group mean. This suggests that municipalities with elevated interest payments are more likely to experience a significant reduction in market access or access to bank credit relative to their peers. Interpreting higher interest payments as indicative of credit constraints, this finding implies that credit-

constrained municipalities may compensate for reduced market access by delaying payments to their suppliers, thereby accumulating arrears.

We use a standard pooled OLS estimator with both time (year) and province-level fixed effects as our benchmark (continuous-form) specification. To reduce endogeneity concerns, given the potential influence of the accumulation of arrears on economic activity, we also use an instrumental variables (IV) approach; in particular, we use a two-step least squares (2SLS) estimator, with the first lags of our variables of interest as their respective instruments. The results in both cases are similar. The results are also robust to an alternative (binary) specification, to the addition of political controls even at the cost of significant sample attrition, and to the use of alternative (including within fixed-effects and two-step system GMM) estimators. In the alternative (binary) specification, results are also robust to the use of a logistic regression, in order to account for the presence of potential non-linearities in the data. For the 2SLS estimator, results are also robust to the use of the second lag of our variables of interest as their respective instruments.

Overall, our findings underscore the importance of establishing effective subnational fiscal rules to tame the build-up of arrears during severe downturns, as that in Spain at the time of the SPP. Essential to this effort, however, is improving the transparency of subnational fiscal data, as the lack thereof may enable subnational governments to bypass regulations through creative accounting practices. Moreover, our research also calls for the adequate design of backstop mechanisms (such as, e.g., risk-sharing platforms) that can help mitigate credit constraints faced by subnational governments while reducing moral hazard.

In [Chapter 4](#), we study the impact of the acceleration in the repayment of government arrears (under the Supplier Payment Plan or SPP) in 2012, on firms involved in procurement contracts with local governments. Initiated to support businesses grappling with a severe credit squeeze during a recession, the SPP represents a unique fiscal policy intervention. Leveraging a novel dataset and employing a clean causal identification strategy, we find that corporate investment markedly and positively reacts to this unforeseen liquidity boost from the government. From a policy standpoint, our findings offer valuable insights into the efficacy of unconventional fiscal measures that do not alter the total public liabilities. Specifically, we show the differential effects of early arrears repayment across firms. Financially constrained businesses increased their investments in response, whereas financially unconstrained entities prioritized debt repayment and also bolstered their cash reserves. This distinction illuminates firms' adaptive strategies to late payments in economic downturns, revealing that while less financially constrained firms might leverage borrowing to offset the impacts of government arrears, their financially constrained counterparts could be forced to miss investment opportunities. Implicitly, our research also sheds light on the challenges firms face in using public arrears as collateral, thus making a notable contribution to the limited body of research on financial factoring.

Furthermore, our findings highlight the significant effects of public administrations' delayed payments on procurement contracting practices. We observe that firms burdened with significant arrears often reduce their contracting with the public sector, a trend that is particularly evident among financially constrained firms. Conversely, when arrears are minimal, firms may in fact be more inclined to increase their engagement in contracting with the public sector. This dynamic suggests a nuanced relationship between the financial health of

firms and their willingness to engage in public procurement activities, emphasizing the importance of timely payments by public administrations for maintaining robust participation in public sector contracting.

In Chapter 5, we study how political tenure and electoral motivations influence the decision to seek a bailout. Our analysis hinges on two principal assertions: first, that agreements with institutional lenders are inherently public and reflect on the past performance of governments; second, that the incentive structures for re-elected incumbents versus new governments vary considerably, especially regarding actions that disclose information about previous performance to the electorate. Thus, we predict differing behaviors between these two types of governments in their approach to securing financial aid. Our research presents compelling cross-country evidence, along with causal insights derived from the participation of local governments in Spain's Supplier Payment Plan (SPP), highlighting the hesitance of ongoing incumbents to openly seek assistance. By analyzing IMF agreements since 1990, we uncover a clear and significant link between initiating an agreement with the IMF and being a government in its initial term.

To derive causal insights into how government turnover influences the pursuit of financial assistance, we utilize data from the SPP, an initiative launched by the Spanish government in 2012 that served as a concurrent financial shock across approximately 4,000 municipalities. By employing a close-election regression discontinuity approach, we show that newly elected local governments in Spain are significantly more inclined—by a margin of 30 percentage points—to formally agree on a fiscal adjustment program with the central government, in exchange for better funding terms. This assertion is further corroborated through a survey conducted among Spanish mayors and a comprehensive analysis of

media coverage, which collectively suggest that incumbent governments may opt for a suboptimal policy—specifically, refraining from presenting a plan and therefore accepting worse funding terms as a result—to preserve their public image. These observations not only offer guidance for structuring future financial assistance measures in a way that aligns with the incentives of involved parties but also highlight the broader theme of how historical performance and electoral motivations can limit incumbents' willingness to adopt policies that are in the best interest of their constituents.

6.2. Limitations

It is essential to consider the external validity of our findings in Chapters 3 and 5, that is, the extent to which they can be generalized across different regions and times.

Importantly, subnational data is frequently used in cross-sectional empirical analyses within fiscal policy studies. A pertinent example, somewhat related to our research, is the literature on subnational fiscal multipliers. Chodorow-Reich (2019), in a review of this literature, posits that exploring variations in fiscal policy across different geographic regions, within the same monetary union and during identical calendar periods, offers several advantages. Chief among these is the opportunity for observing a broader range of policy variations spatially rather than temporally, alongside the likelihood of such variations being more plausibly exogenous in relation to the no-intervention paths of outcome variables. This logic is equally applicable to our research.

Moreover, it is a common practice in both political economics and political science to examine local governments to study questions motivated by national

politics. Ashworth, Berry, & Bueno de Mesquita (2021) explain that local government activities lend themselves more readily to analysis via modern causal inference techniques, a rationale that aligns with our decision to exploit SPP data. According to these authors, the logic behind generalizing findings across different levels of government hinges on the similarity of the phenomena under investigation. In our case, both local and central governments facing the decision to seek assistance are navigating a similar dilemma: balancing the need for immediate financial relief against the potential consequences of disclosing information. Given that the behaviors and decision-making processes observed in Spanish municipalities mirror those at the national level over recent decades, we argue that our findings hold external validity, suggesting that the insights gleaned from our analysis of local governments can indeed inform our understanding of broader governmental dynamics.

6.3. Avenues for Future Research

Firstly, regarding the causation of government arrears in the context of Spain's Supplier Payment Plan (SPP) discussed in Chapter 3, a potentially fruitful avenue for research could involve examining the effects stemming from the reduction of the debt limit for municipalities, from 110% to 75% of current revenues. This policy change, announced in May 2010 and implemented from January 1, 2011, had significant financial implications, as municipalities with debt ratios exceeding that limit were banned from using long-term credit to fund capital expenditures. Aimed at curtailing escalating local budget deficits, this policy effectively introduced a credit shock, especially impacting municipalities with debt ratios anticipated to fall within the range implied by the old and the new

thresholds. Such a situation might have incentivized local governments to accrue arrears, potentially providing context to our observation that municipalities accumulating arrears tend to be those facing heightened financial constraints due to reduced market access. Investigating this specific policy shift might necessitate the use of a regression discontinuity design (RDD) to rigorously assess its implications.

Secondly, while Chapter 4 of our study concentrates on the intensive margin (impact per firm) concerning the real effects of delayed payments in procurement, an additional promising avenue of research could involve also examining the extensive margin (number of firms). A potential research question could be whether firms involved in Phase 1 of the SPP exhibit a lower likelihood of exit compared to those in Phase 2. The macroeconomic difficulties Spain faced during this period underscore the significance of the timing of liquidity injections, that is, receiving financial support a year earlier (in 2012 rather than in 2013) could be critical for firms' decisions to remain operational. From a policy perspective, preventing firm closures should be a paramount concern. In this context, recent evidence suggests the implications of financial frictions at the extensive margin might be triple those at the intensive margin (Kochen, 2023), highlighting the importance of assessing the broader impact of such fiscal interventions on the corporate landscape.

Thirdly, exploring the macroeconomic (general equilibrium) effects of the SPP presents another compelling avenue for future research. While Delgado-Tellez, Hernández de Cos, Hurtado, & Pérez (2015) have adopted a top-down (country-level) approach utilizing both a VAR specification and a large-scale macro-econometric model to evaluate the SPP's contributions to real GDP and employment growth cumulatively over 2012-14, adopting a bottom-up (firm-level)

perspective could also offer valuable insights. Specifically, a significant liquidity injection, akin to the SPP, could wield substantial aggregate effects through at least three mechanisms: elevating investment levels, reducing the incidence of firm exits, and influencing the credit network (especially considering the centrality of these firms within the network). To thoroughly investigate these dimensions, the employment of a macroeconomic model may be necessary, bridging the gap between micro-level dynamics and macro-level outcomes.

Fourthly, by analyzing the macroeconomic effects of the differing composition in fiscal adjustment plans (i.e., whether these are tax- or expenditure-based) across the circa 4,000 municipalities that participated in the SPP, a significant contribution could potentially made to that strand of the literature. For a comprehensive review, see Alesina, Favero, & Giavazzi (2019a and 2019b).

Lastly, in Chapter 5, we employed ChatGPT to scrutinize all news articles from 2012 and 2013 found in the Factiva database that related to the SPP or the municipal adjustment plans. Importantly, further empirical investigation into ChatGPT's response accuracy is warranted to assess (i) the correctness of ChatGPT's answers and the conditions influencing this, and (ii) the optimal ways to frame queries to ChatGPT or adjust the empirical approach to address any potential issues. For a recent empirical assessment of ChatGPT's answering capabilities in natural science and engineering, see Balhorn, Weber, Buijsman, Hildebrandt, Ziefle, & Schweidtmann (2024).

Chapter 7. The Appendix

7.1. Appendix for Chapter 3

Table 27. Variable Definitions

Variable	Definition
Arrears pc	Flow of local Government expenditure arrears, measured as changes in the stock, per capita.
Employment pc	Total employment per capita.
Unemployment (Construction) pc	Unemployment in the construction sector per capita.
Crime Rate (%)	Fraction of crime-affected dwellings.
Habitability Index (0-100)	Habitability Index. The index values range from 0 to 100. The higher the value, the more "habitable" the property is.
Population	Population.
Household Size	Average household size.
Financial Debt pc	Financial debt (only available since 2008) per capita.
Financial Debt (% Current Rev.)	Financial debt (since 2008) over current revenues, with current revenues being equal to overall tax revenues (Chapters 1-3 of local Government accounts' income side), current transfers received (Chapter 4) and capital income (Chapter 5).
Interest Payments pc	Interest payments (Chapter 3 of local Government accounts' expenditure side) per capita.
Interest Payments (% Current Rev.)	Interest payments over current revenues (with each component being defined as described above).
Primary Spending pc	Total spending (Chapters 1-7) minus interest payments (Chapter 3), per capita.
Current Spending pc	Spending associated with the purchase of goods & services (Chapter 2) per capita.
Capital Spending pc	Capital spending (Chapter 6) per capita.
Personnel Spending pc	Personnel spending (Chapter 1) per capita.
Transfers (Spending) pc	Overall transfers made (Chapters 4 and 7) per capita.
Tax Revenues pc	Overall tax revenues (defined as above) per capita.
Transfers (Revenues) pc	Overall transfers received (Chapters 4 and 7) per capita.
Property Tax Rate (%)	Property tax (Spain's *Impuesto sobre Bienes Inmuebles* or IBI) rate, shown as a % of the property's tax/cadastral value.
Plan E pc 2009-10	Cumulative 'Plan E' funds received in 2009-10, per capita.
Primary Deficit pc	Primary spending (as defined above) less overall revenues (Chapters 1-7), per capita. Deficit (surplus) is associated with a positive (negative) value.
Primary Deficit (vs Budget) pc	Actual primary deficit less the primary deficit set in the Budget (as defined above in both cases), per capita. A larger (smaller) primary deficit than expected in the Budget is associated with a positive (negative) value.
Treasury Position pc	The so-called *Remanente de Tesoreria*, which is defined as cash (*fondos líquidos*) plus receivables outstanding (*derechos pendientes de cobro*) less outstanding obligations (*obligaciones pendientes de pago*), per capita.
PP Mayor 2007-11	Dummy variable that takes a value of one if the Mayor elected in the May 2007 local elections and in office until the May 2011 elections is from the Conservative Party (*Partido Popular* or PP), zero otherwise. This will generally (but not always) be time-invariant.
PSOE Mayor 2007-11	Dummy variable that takes a value of one if the Mayor elected in the May 2007 local elections and in office until the May 2011 local elections is from the Socialist Party (*Partido Socialista Obrero Español* or PSOE), zero otherwise. This will generally (but not always) be time-invariant.
Age Mayor 2007-11	Age of the Mayor elected in the May 2007 local elections and in office until the May 2011 local elections. This will generally (but not always) be time-invariant.

Table 28. Alternative Regressor - Employment

	(1) POLS	(2) 2SLS (Levels)	(3) LPM	(4) 2SLS (Binary Dependent Variable)	(5) First Stage: Relevance
Employment pc $_{t-1}$	-.21***	-.23***	-.05***	-.04***	.92***
	(.08)	(.08)	(.01)	(.01)	(.01)
Observations	8370	7756	22142	18787	18787
Within R^2	.47	.33	.21	.27	.30
Municipality-level Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: Columns (1) and (2) report, respectively, POLS and IV estimates of the effect of a vector of covariates on the flow of local government expenditure arrears, with the dependent variable being the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. In column (2), the explanatory variable is instrumented, with its first lag

being used as its own instrument. Columns (3) and (4) report, respectively, LPM and IV estimates of the effect of the same vector of covariates on the likelihood that local government expenditure arrears are originated, with the dependent variable being a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. In column (5), we POLS-regress our variables of interest over its first lag. All regressions are run over a shortened panel spanning over four years (2008-11). With regard to the panel's cross-sectional dimension, while regressions in columns (1) and (3) include all Spanish municipalities with government expenditure arrears outstanding in any of the years considered (3.833), regressions in columns (2), (4) and (5) include all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*, including the two *Ciudades Autónomas* of Ceuta and Melilla (7.585). Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors (columns (1), (3) and (5)) and standard errors clustered at the municipality level (columns (2) and (4)) are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 29. Alternative Regressor - Financial Debt

	(1) POLS	(2) 2SLS (Levels)	(3) LPM	(4) 2SLS (Binary Dependent Variable)	(5) First Stage: Relevance
Financial Debt (% Current Revenues) $_{t-1}$	.26***	.31***	.06***	.09***	.73***
	(.03)	(.04)	(.01)	(.01)	(.01)
Observations	5317	3731	9501	6103	6103
Within R^2	.45	.27	.12	.25	.02
Municipality-level Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: Columns (1) and (2) report, respectively, POLS and IV estimates of the effect of a vector of covariates on the flow of local government expenditure arrears, with the dependent variable being the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. In column (2), the explanatory variable is instrumented, with its first lag being used as its own instrument. Columns (3) and (4) report, respectively, LPM and IV estimates of the effect of the same vector of covariates on the likelihood that local government expenditure arrears are originated, with the dependent variable being a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. In column (5), we POLS-regress our variables of interest over its first lag. All regressions are run over a shortened panel spanning over four years (2008-11). With regard to the panel's cross-sectional dimension, while regressions in columns (1) and (3) include all Spanish municipalities with government expenditure arrears outstanding in any of the years considered (3.833), regressions in columns (2), (4) and (5) include all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*, including the two *Ciudades Autónomas* of Ceuta and Melilla (7.585). Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level

of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except financial debt, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors (columns (1), (3) and (5)) and standard errors clustered at the municipality level (columns (2) and (4)) are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 30. Main Regressions, adding Previous Government (2007-11) Controls

	(1) POLS (Arrears pc)	(2) LPM (Binary Arrears pc)
Unemployment (Construction) pc $_{t-1}$	.06	.02***
	(.05)	(.01)
Current Spending pc $_{t-1}$	.64***	.04**
	(.09)	(.01)
Interest Payments (% Current Revenues) $_{t-1}$	.14***	.04***
	(.02)	(0)
Observations	6773	14971
Within R^2	.45	.19
Municipality-level Controls	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov

Notes: Column (1) reports POLS estimates of the effect of a vector of covariates on the flow of local government expenditure arrears, with the dependent variable being the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. Column (2) reports LPM estimates of the effect of the same vector of covariates on the likelihood that local government expenditure arrears are originated, with the dependent variable being a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. Both regressions are run over a shortened panel spanning over just five years (2007-11). With regard to the panel's cross-sectional dimension, while the first regression includes all Spanish municipalities with government expenditure arrears outstanding in any of the years considered (3.833), the second regression includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*, including the two *Ciudades Autónomas* of Ceuta and Melilla (7.585). Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. Both regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of Central government transfers. In addition, we also control for some key features of the local governments in power in between the local elections of May 2007 and May 2011, including dummy variables for the Conservative Party (*Partido Popular* or PP) and the Socialist Party (*Partido Socialista Obrero Español* or PSOE), as well as dummy variables controlling for the major's gender and age. All variables are in continuous form, except the crime and habitability rates, as well as the political dummies described before, which are generally time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from

the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 31. Logistic Regression: Binary Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.29***			.27***
	(.09)			(.09)
Current Spending pc $_{t-1}$		.41**		.47**
		(.18)		(.2)
Interest Payments (% Current Revenues) $_{t-1}$			.62***	.63***
			(.05)	(.06)
Observations	20341	22142	20153	18825
Pseudo R^2	.29	.29	.30	.30
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports Logistic-regression estimates of the effect of a vector of covariates on the likelihood that local government expenditure arrears are originated, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. The dependent variable is a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. All regressions include time [year] and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 32. 2SLS (instrument: 2nd lag) Regression: Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.12			.11
	(.08)			(.09)
Current Spending pc $_{t-1}$		1.16***		1.28***
		(.16)		(.17)
Interest Payments (% Current Revenues) $_{t-1}$			.27***	.35***
			(.04)	(.05)
Observations	6624	6470	6029	5799
R-squared	.31	.32	.33	.33
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports IV estimates of the effect of a vector of covariates on the flow of local government expenditure arrears in a panel spanning over seven years (2005-11) and 3.833 municipalities in Spain. This includes all Spanish municipalities with government expenditure arrears outstanding in any of the years considered. The dependent variable is the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. The explanatory variables are instrumented, with their second lags being used as their own instruments. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Standard errors clustered at the municipality level are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 33. 2SLS (instrument: 2nd lag) Regression: Binary Arrears pc

	(1)	(2)	(3)	(4)
Unemployment (Construction) pc $_{t-1}$	.05***			.05***
	(.02)			(.02)
Current Spending pc $_{t-1}$		.13***		.13***
		(.03)		(.03)
Interest Payments (% Current Revenues) $_{t-1}$			.08***	.09***
			(.01)	(.01)
Observations	13495	14466	12646	11441
R-squared	.24	.27	.29	.28
Municipality-level Controls	Yes	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov	Year & Prov

Notes: This Table reports IV estimates of the effect of a vector of covariates on the likelihood that local government expenditure arrears are originated, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. The dependent variable is a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. The explanatory variables are instrumented, with their second lags being used as their own instruments. All regressions include time [year] and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of Central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Standard errors clustered at the municipality level are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 34. First Stage: Relevance for Instruments (2nd lag) in the 2SLS Regressions

	(1) Unemployment (Construction) pc $_{t-3}$	(2) Current Spending pc $_{t-3}$	(3) Interest Payments (% Current Revenues) $_{t-3}$
Unemployment (Construction) pc $_{t-1}$	.44***		
	(.01)		
Current Spending pc $_{t-1}$		.49***	
		(.01)	
Interest Payments (% Current Revenues) $_{t-1}$			.36***
			(.02)
Observations	13495	14466	12646
Within R^2	.46	.15	.01
Municipality-level Controls	Yes	Yes	Yes
Fixed Effects	Year & Prov	Year & Prov	Year & Prov

Notes: In each column, we POLS-regress each of our three variables of interest over their respective second lags, in a panel spanning over seven years (2005-11) and 7.585 municipalities in Spain. This includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*. Municipalities located in the *Comunidades Autónomas de Régimen Especial* (Basque Country and Navarre) are therefore excluded. All regressions include time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 35. Main Regressions, Adding Municipality-Level Fixed Effects

	(1) POLS (Arrears pc)	(2) LPM (Binary Arrears pc)
Unemployment (Construction) pc $_{t-1}$	.15**	.02***
	(.08)	(.01)
Current Spending pc $_{t-1}$	.66***	.03*
	(.13)	(.02)
Interest Payments (% Current Revenues) $_{t-1}$	-.10***	.01***
	(.03)	(0)
Observations	7869	18825
Within R^2	.48	.23
Municipality-level Controls	Yes	Yes
Fixed Effects	Year & Muni	Year & Muni

Notes: Column (1) reports POLS estimates of the effect of a vector of covariates on the flow of local government expenditure arrears, with the dependent variable being the contemporaneous flow of local government expenditure arrears, measured as yearly changes in the stock. Column (2) reports LPM estimates of the effect of the same vector of covariates on the likelihood that local government expenditure arrears are originated, with the dependent variable

being a dummy variable that takes a value of one if local government expenditure arrears are non-zero, and zero otherwise, in each annual cross-section. Both regressions are run over a panel spanning over seven years (2005-11). With regard to the panel's cross-sectional dimension, while the first regression includes all Spanish municipalities with government expenditure arrears outstanding in any of the years considered (3.833), the second regression includes all Spanish municipalities, with or without government expenditure arrears outstanding, located in the *Comunidades Autónomas de Régimen Común*, including the two *Ciudades Autónomas* of Ceuta and Melilla (7.585). Municipalities located in the *Comunidades Autónomas de Regimen Especial* (Basque Country and Navarre) are therefore excluded. Both regressions include time [year] and municipality-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

Table 36. Main Model, Estimated with Two-step System GMM

	Arrears pc
Arrears pc $_{t-1}$	.60***
	(.05)
Arrears pc $_{t-2}$	.15***
	(.03)
Unemployment (Construction) pc $_{t-1}$	1.00**
	(.46)
Current Spending pc $_{t-1}$	1.42**
	(.67)
Interest Payments (% Current Revenues) $_{t-1}$	-.14
	(.16)
Observations	4068
Municipality-level Controls	Yes
Fixed Effects	Year & Prov
AR(1) p	.00
AR(2) p	.79
Hansen p	.14
Difference-in-Hansen p	.96

Notes: Regressions estimated with Two-Step System GMM, using the second and further lags, collapsed, as instruments. Table reports estimates of the effect of a vector of covariates on the flow of local government expenditure arrears, measured as yearly changes in the stock. Regression run over a panel spanning over seven years (2005-11) and across all Spanish municipalities with government expenditure arrears outstanding in any of the years considered (3.833). It includes time (year) and province-level fixed effects. Municipality-level controls include changes in the treasury position, tax revenues, transfers received, deviations in the

actual primary balance from the budget, 'Plan E' funds received in 2009-10, crime rate, habitability rate, overall level of employment, population, household size, property tax rate and a dummy variable that controls for population thresholds (<5k, 5-20k, 20-50k, 50-75k, >75k) associated with changes in the level of central government transfers. All variables are in continuous form, except the crime and habitability rates, which are time-invariant. All macroeconomic and fiscal variables are per capita, except interest payments, which is normalized by the level of current revenues. All variables are in logs, except changes in the treasury position and deviations in the actual primary balance from the budget, which are in levels. All explanatory variables and controls are lagged by one year, except changes in the treasury position and deviations in the actual primary balance from the budget, which are contemporaneous. All variables are defined in Table 27 (Appendix). Robust standard errors are shown in parentheses. *** p<.01, ** p<.05, * p<.1

7.2. Appendix for Chapter 4

Figure 16. *Mancomunidades* and Municipalities

Notes: This Figure shows the region of Andalusia in Spain, and depicts how Spanish municipalities can interact with suppliers. Source: Feria Toribio (2013).

Figure 17. Factoring of Arrears by Bank Type
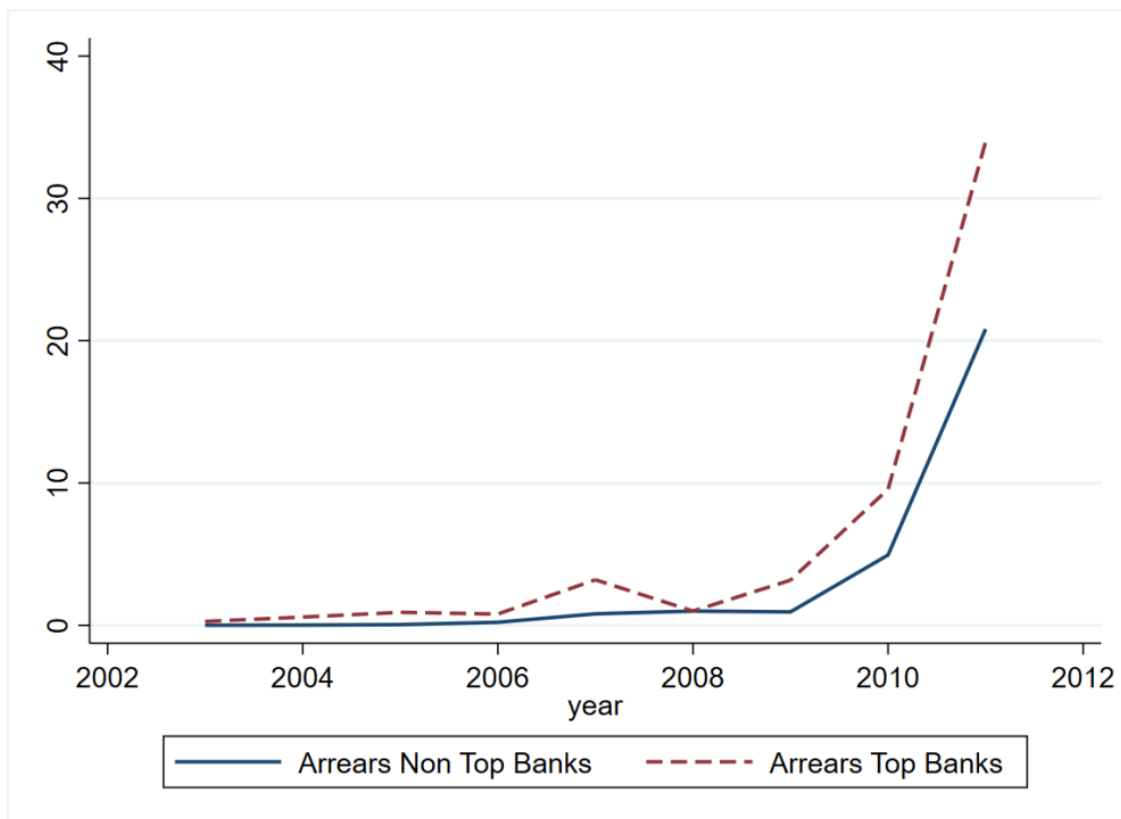
Notes: This Figure shows the evolution of the factoring of arrears from the Spanish SPP by top banks and non-top banks. Top banks include banks with a Core Equity Tier 1 (CET1) capital ratio above 7.4% of risk-weighted assets in the adverse scenario of the EBA stress tests developed in 2011. Non-top banks include banks with a Core Equity Tier 1 (CET1) capital ratio below 7.4%. We sum the amount of arrears that have been factored per year and bank and calculate a weighted average for top banks vs non-top banks. We weigh each bank by their market share in 2011. We normalize the amounts in 2008. The period is 2003-11.

Table 37. Descriptive Statistics: 2009-12

	Mean	Std.Deviation	Perc.25	Median	Perc.75	Observations
Liquidity Shock	125.25	1232.91	1.69	7.30	32.48	203795
Total assets	5146.40	19227.50	383.33	890.00	2448.00	166244
Total liabilities	3255.29	13179.71	220.00	515.00	1392.00	166243
Cash	294.62	1000.47	14.78	53.00	179.00	155219
Employment to assets	0.02	0.03	0.01	0.01	0.02	155888
Leverage ratio	0.37	0.27	0.17	0.33	0.52	107068
Total debt	1528.13	4960.33	127.00	325.00	887.00	107068
Sales to assets	1.32	1.21	0.62	1.03	1.64	158593
Sales growth	-0.02	0.53	-0.22	-0.07	0.05	158097
ROE	10.77	111.45	-0.20	8.64	24.15	165923
Long-term debt	804.44	3082.30	41.00	148.00	438.00	123263
Short-term debt	564.50	2041.63	26.26	93.38	298.00	134762
Investment	0.02	0.47	-0.12	-0.03	0.06	155881
Leverage growth	-0.02	0.44	-0.18	-0.02	0.13	157309
Liquidity growth	-0.05	1.08	-0.60	-0.04	0.47	142338

Notes: This Table reports mean, standard deviation, 25th-percentile, median, 75th-percentile, and number of observations for several variables. The sample covers the period 2009-12.

Table 38. Summary Statistics: 2010 and 2009

Panel A: 2010

	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Liquidity Shock	140.266	140.262	-0.003	(1.000)
Total assets	5,180.161	5,180.271	0.111	(1.000)
Total liabilities	3,313.226	3,439.100	125.874	(0.829)
Cash	299.621	342.933	43.312	(0.710)
Employment to assets	0.017	0.017	-0.000	(0.939)
Leverage ratio	0.366	0.361	-0.005	(0.815)
Total debt	1,551.979	1,730.285	178.306	(0.629)
Sales to assets	1.396	1.463	0.066	(0.392)
Sales growth	0.043	0.101	0.058	(0.346)
ROE	13.335	2.202	-11.133	(0.356)
Long-term debt	813.712	781.347	-32.365	(0.850)
Short-term debt	578.011	701.646	123.635	(0.517)
Investment	0.029	0.041	0.012	(0.587)
Leverage growth	0.023	0.033	0.010	(0.698)
Liquidity growth	-0.057	-0.004	0.053	(0.367)
Observations	42,708	489	43,197	

Panel B: 2009

	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Liquidity Shock	139.862	139.859	-0.003	(1.000)
Total assets	5,096.947	5,097.059	0.112	(1.000)
Total liabilities	3,277.663	3,215.841	-61.822	(0.903)
Cash	309.823	306.548	-3.275	(0.970)
Employment to assets	0.018	0.018	0.000	(0.955)
Leverage ratio	0.364	0.353	-0.010	(0.626)
Total debt	1,506.419	1,578.632	72.213	(0.818)
Sales to assets	1.430	1.522	0.092	(0.248)
Sales growth	-0.048	0.021	0.069	(0.081)*
ROE	16.857	11.428	-5.429	(0.538)
Long-term debt	782.284	786.162	3.879	(0.981)
Short-term debt	574.788	658.844	84.056	(0.581)
Investment	0.032	0.019	-0.014	(0.522)
Leverage growth	-0.004	-0.011	-0.007	(0.805)
Liquidity growth	0.062	-0.030	-0.092	(0.167)
Observations	42,394	468	42,862	

Notes: This Table reports the mean of firm characteristics for firms in Phase I and Phase II, the differences between the two groups of firms, and the *p*-values associated with those differences. Phase I include the sample of firms that received the liquidity shock in year 2012,

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and Phase II includes firms that received the liquidity shock in 2013. Firms from Phase I and Phase II are matched based on total assets, liquidity shock, and region. *Panel A* compares firm characteristics in Phase I and Phase II in 2010. *Panel B* compares firm characteristics in Phase I and Phase II in 2009.

Table 39. Summary Statistics by Bank Heterogeneity: 2011

Panel A: Excluding Top Banks

Variable	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Repayment Shock	81.357	93.988	12.631	(0.869)
Investment	0.014	0.071	0.057	(0.181)
Leverage growth	-0.000	0.037	0.037	(0.500)
Liquidity growth	-0.140	-0.118	0.022	(0.800)
Total assets	2,627.548	2,816.406	188.858	(0.727)
Total liabilities	1,698.016	1,874.819	176.804	(0.714)
Cash	169.688	148.278	-21.410	(0.311)
Employment to assets	0.018	0.020	0.002	(0.630)
Leverage ratio	0.400	0.420	0.020	(0.450)
Total debt	905.104	980.392	75.288	(0.802)
Long-term debt	507.486	569.057	61.571	(0.721)
Short-term debt	314.369	305.689	-8.680	(0.920)

Panel B: Top Banks

Variable	(1) Phase I	(2) Phase II	(3) Difference	(4) P-value
Repayment Shock	285.558	292.644	7.086	(0.979)
Investment	-0.004	-0.017	-0.012	(0.746)
Leverage growth	-0.054	-0.109	-0.055	(0.333)
Liquidity growth	-0.175	-0.069	0.106	(0.412)
Total assets	11,036.557	12,359.460	1,322.904	(0.657)
Total liabilities	6,876.159	7,877.819	1,001.660	(0.626)
Cash	533.931	795.866	261.935	(0.460)
Leverage ratio	0.319	0.283	-0.037	(0.235)
Total debt	2,880.883	3,661.837	780.954	(0.523)
Long-term debt	1,482.203	1,706.474	224.271	(0.738)
Short-term debt	1,157.321	1,560.795	403.474	(0.390)

Notes: This Table reports average firm characteristics for firms in Phase I and Phase II, the differences between the two groups of firms, and the *p*-values associated with those differences. Phase I includes the sample of firms that worked for local government entities that received the repayment shock in year 2012, and Phase II includes firms that received the repayment shock in 2013. Firms from Phase I and Phase II are matched based on total assets, repayment shock, and region. **Panel A** compares firms in Phase I and Phase II for the sample of "Top Banks" (e.g., firms that in 2009 worked with at least one bank with a core equity tier 1 (CET 1) capital ratio above 7.4. **Panel B** compares firms in Phase I and Phase II for the sample of "Excluding Top Banks" (e.g., firms that in 2009 did not work with a top bank). Firm characteristics are all measured in year 2011.

Table 40. Effects on Corporate Decisions with Firm Fixed Effects

	Investment			
	$< 1\%$	$1\% - 5\%$	$5\% - 10\%$	$> 10\%$
Phase I × Post 2010	0.006	0.003	-0.009	-0.078
	(0.20)	(0.12)	(-0.26)	(-1.51)
Phase I × Post 2011	-0.022	-0.024	-0.014	-0.006
	(-0.61)	(-0.65)	(-0.36)	(-0.16)
Phase I × Post 2012	0.040	0.053	0.062*	0.096***
	(1.17)	(1.53)	(1.72)	(2.68)
Year & Firm FE	Yes	Yes	Yes	Yes
Observations	78135	40633	14764	17980
Adjusted R^2	0.078	0.072	0.051	0.058

	Leverage Growth			
	$< 1\%$	$1\% - 5\%$	$5\% - 10\%$	$> 10\%$
Phase I × Post 2010	0.011	-0.026	-0.043	-0.123*
	(0.28)	(-0.67)	(-0.98)	(-1.80)
Phase I × Post 2011	0.024	0.017	0.005	0.017
	(0.64)	(0.45)	(0.12)	(0.40)
Phase I × Post 2012	0.004	0.016	-0.007	-0.133***
	(0.11)	(0.42)	(-0.17)	(-2.74)
Year & Firm FE	Yes	Yes	Yes	Yes
Observations	78624	41051	14937	18346
Adjusted R^2	0.042	0.033	0.011	0.026

	Liquidity Growth			
	$< 1\%$	$1\% - 5\%$	$5\% - 10\%$	$> 10\%$
Phase I × Post 2010	-0.110	-0.107	-0.138	-0.131
	(-1.26)	(-1.18)	(-1.33)	(-0.83)
Phase I × Post 2011	0.017	-0.018	-0.010	0.051
	(0.20)	(-0.21)	(-0.11)	(0.54)
Phase I × Post 2012	0.064	0.111	0.210**	0.462***
	(0.76)	(1.32)	(2.32)	(4.42)
Year & Firm FE	Yes	Yes	Yes	Yes
Observations	70646	36788	13426	16435
Adjusted R^2	-0.156	-0.156	-0.170	-0.164

Notes: This Table presents estimates from panel matching regressions explaining corporate decisions for the period 2009 to 2012. Firms from Phase I and Phase II are matched based on total assets and liquidity shock. The dependent variables are the first difference in the logarithm of fixed assets (*Panel A: Investment*), liabilities (*Panel B: Leverage Growth*), and cash (*Panel C: Liquidity Growth*). *Phase I* is a dummy that takes a value of one for firms that received liquidity in Phase I (2012) and zero for firms that received liquidity a year later in Phase II. *Post 2012* is an indicator that equals one for year 2012. We sort our sample into firms that received a liquidity shock below 1% of their total assets, between 1% and 5%, between 5% and 10%, and above 10%. All regressions include year and firm fixed effects. Robust T-statistics are clustered at the firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5%, or 10% level, respectively.

7.3. Appendix for Chapter 5.

7.3.1. Tables & Figures

Table 41. 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 42. Balancing Checks – Predetermined Municipal Characteristics

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

	(1) One-party Majority	(2) Seat Share Mayor	(3) Minority Government
Party Change	-0.0682	-0.0121	-0.0632
	(0.111)	(0.0251)	(0.0581)
Observations	1236	1260	1329
p-value	0.540	0.628	0.277
Bandwidth	0.161	0.164	0.175
	Female Mayor	Mayor w/College	Party Change (2007)
Party Change	-0.0799	-0.0864	-0.0858
	(0.0955)	(0.140)	(0.113)
Observations	967	733	1216
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	-0.0344	-0.0174
	(2.777)	(0.107)	(0.0987)
Observations	754	1357	1338
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 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 44. First-Stages - Leadership Change

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

Notes: First-stage estimates for fuzzy regression-discontinuity estimates (see Table 20 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 45. Robustness: Alternative Definition of the Dependent Variable

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

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

Table 46. Robustness: Choice of Polynomial Length and Kernel

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

Notes: The Table presents two-stage least squares estimates of the effect of a change in the party in power at the local level on the probability of presenting an adjustment plan. The first column does not include controls. The second column controls for the municipal characteristics. The third column controls for the previous government characteristics. We report 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 47. Robustness: Running Variable Adapted to PR Electoral System

	(1) Adjustment Plan	(2) Adjustment Plan
Party Change	0.186***	0.286***
	(0.0600)	(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 5.4.3. No additional control included in these specifications. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively

Table 48. SPP Press Coverage & Adjustment Plans

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

Notes: The Table presents estimates of the probability that written media mention Supplier Payment Program together with the name of municipalities that present or do not present an adjustment plan. Observations are at the municipality-year level. The dependent variable is a dummy variable that takes a value of one if Supplier Payment Program appears in the news together with the name of the municipality, and zero otherwise. The unconditional mean of the dependent variable is 0.136. *AdjustmentPlan* is a dummy variable that takes a value of one for municipalities that present an adjustment plan, and zero for those that do not. *Post* is a dummy variable that takes a value of one for years 2012 and 2013, and zero for year 2011. The second column controls for the total number of yearly 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 49. Government Turnover and Press Coverage of SPP

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

Notes: The Table presents two-stage least squares estimates of the effect of a change in municipal government on the probability that a municipality is featured in news about the SPP as measured using the Factiva sample of news. 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 50. RD Estimates of Party Change on Origin of Criticisms Based on SPP News

	(1) by Government	(2) by Opposition	(3) to Current Gov	(4) to Former Gov
Party Change	0.324*	-0.433**	-0.225	-0.0335
	(0.172)	(0.191)	(0.168)	(0.0939)
Observations	759	759	759	759
Observations within BW	181	200	244	206
Bandwidth	.077	.085	.103	.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 51. Mayor Re-elected in 2015

	(1) Re-Elected	(2) Re-Elected
Incumbent 2010	0.149***	0.152***
	(0.0284)	(0.0291)
Adjustment Plan	-0.0260	-0.00635
	(0.0298)	(0.0304)
Incumbent 2010#Plan	-0.0529	-0.0675*
	(0.0354)	(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 99th percentile. The first coefficient is a dummy that takes value 1 if the mayor in 2011 after the election was the incumbent in 2010 and value zero otherwise. The second coefficient is a dummy that takes value one if the municipal government presents an adjustment plan, and value zero otherwise. The third coefficient is the interaction between the previous two. The first column controls for population, outstanding debt, arrears and a dummy that takes value one if the mayor is from Partido Popular. The second column adds province fixed effects. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

Table 52. Balancing Checks – Elected Government Characteristics (2011)

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

Notes: Estimates of the effect of the party change dummy on 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 53. Party Mayor & Adjustment Plan

|	(1) Adjustment Plan	(2) Adjustment Plan	(3) Adjustment Plan	(4) Adjustment Plan
Party of Mayor	-0.0137	-0.0375	0.0340	0.0200
	(0.0975)	(0.0997)	(0.0904)	(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 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.

Table 54. Effect Away from Threshold

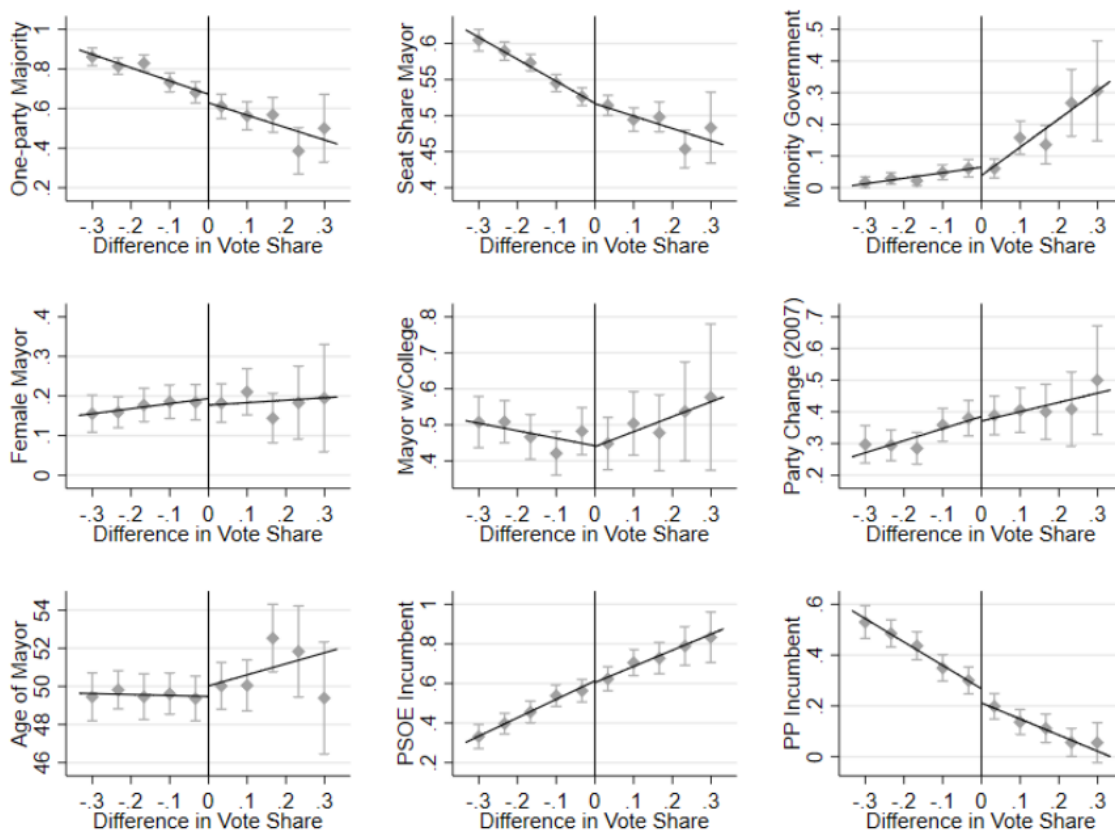| Estimand: Method:	(1) Baseline RD CCT	(2) Angrist & Rokkanen Kline	(3) Angrist & Rokkanen Hirano
Estimate	.31***	.13***	.13***
	(.1)	(.04)	(.04)
	[.11,.51]	[.06,.19]	[.08,.19]

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

Figure 18. Histogram of Running Variable - Vote Margin of Municipal Challenger
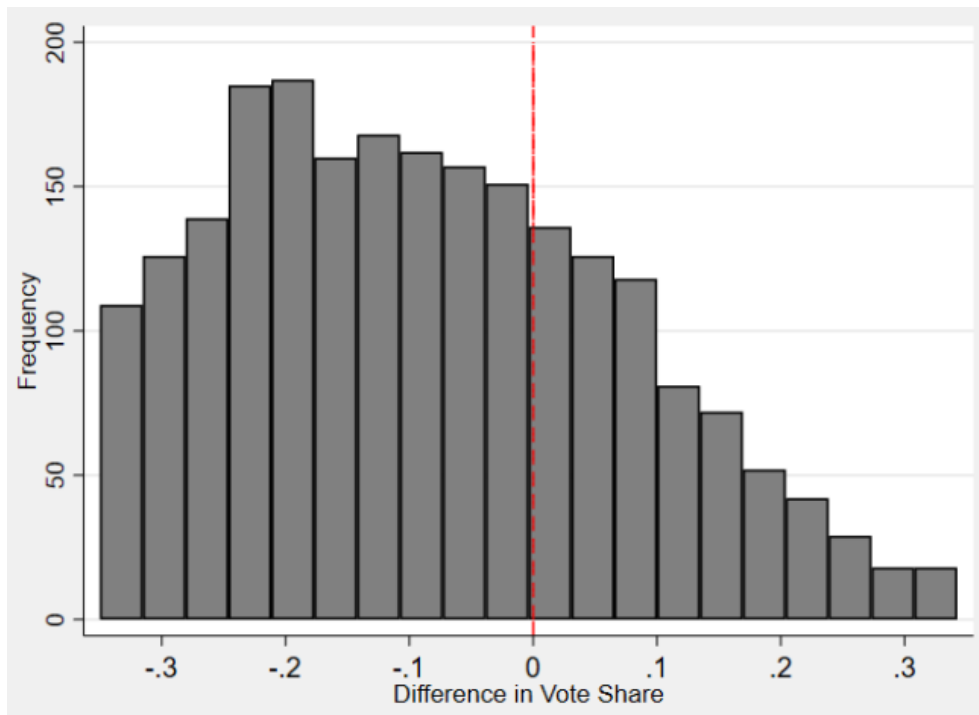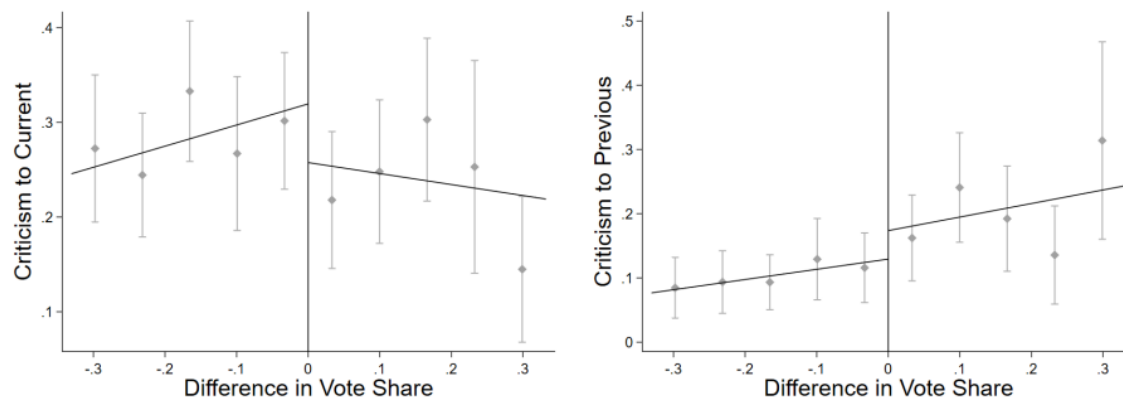

Notes: Histogram of running variable for values between -0.35 and 0.35. The *p*-value of the Cattaneo, Jansson, & Ma (2020) test of no manipulation is 75%.

**Figure 19. Covariate Balancing – Previous Government Characteristics**

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Figure 20. 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 reducedform 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 21. Covariate Balancing – Elected Government Characteristics (2011)
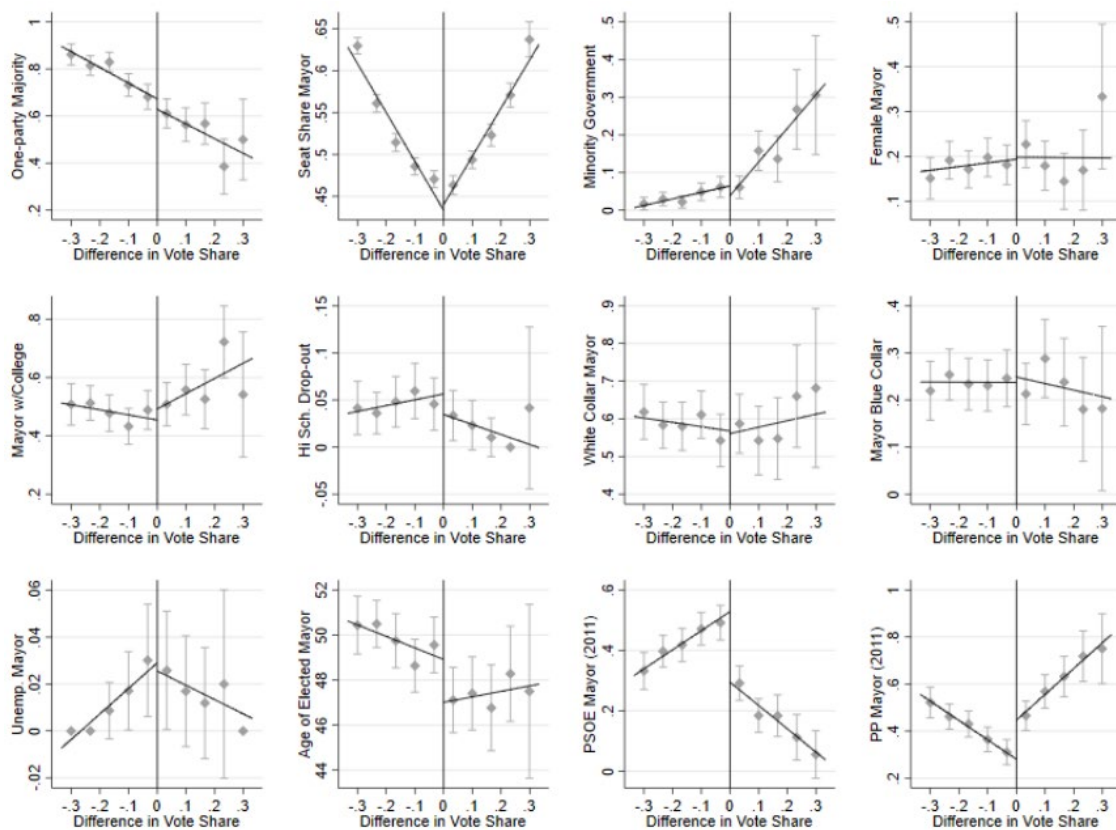
Notes: Graphs plot the relationship between the running variable and the characteristics of governments elected after the 2011 election. The horizontal axis represents the vote share difference between the challenger and the incumbent in that election. From left to right and top to bottom the vertical axes represent fraction of oneparty 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 22. Conditional Independence Assumption Tests (Angrist & Rokkanen, 2015)

Slope for negative R.V. .001 (P-value= .58). Slope for positive R.V. -.003 (P-value= .41).
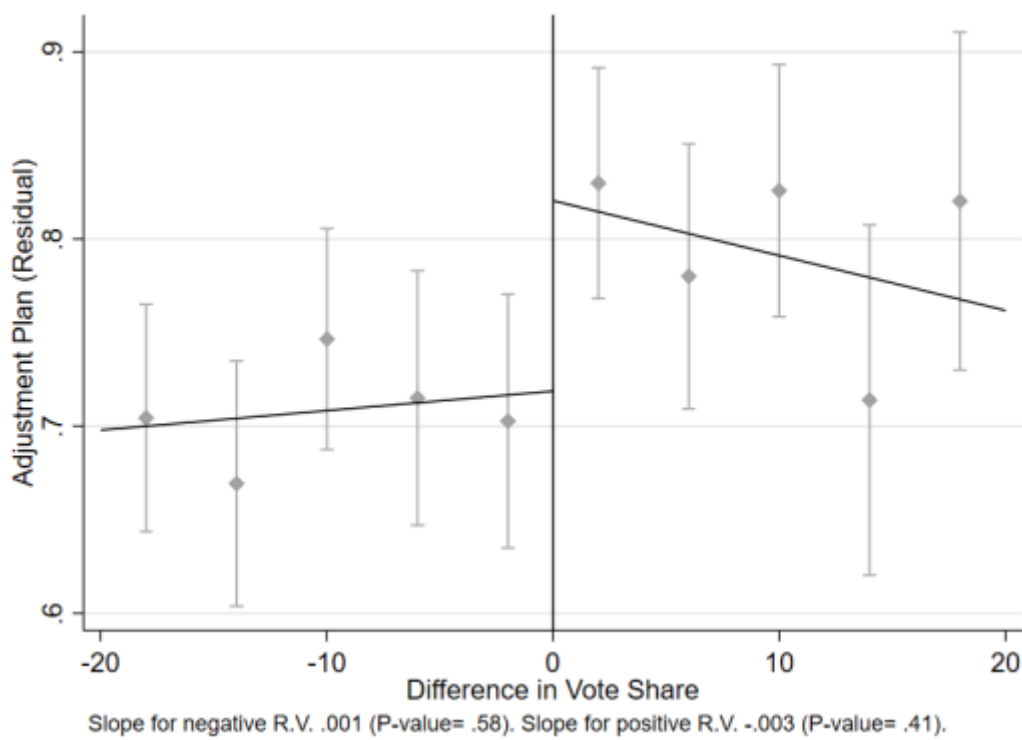
Notes: Figure shows the conditional relationship between the running variable (difference in vote share) and the Adjustment Plan outcome after controlling for municipal-level and previous government characteristics. Conditioning is carried out by residualizing the outcome. Separate linear terms estimated on both sides of the threshold. The corresponding slope coefficients on each side of the threshold and *p*-values of a significance test of these coefficients reported below the Figure. In both cases we cannot reject the null hypothesis of coefficients being jointly zero.

7.3.2. Cross-Country Analysis

A. 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 next. Features of the political organization of 180 countries covering 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 & Singh (1999). We use this index to identify democratic regimes (we keep index values 6 and 7; see Scartascini, Cruz, & Keefer, 2021). Data on IMF funding agreements is obtained from the IMF Monitoring of Fund Arrangements (MONA) Database. This contains the universe of arrangements with the IMF during the period 1992-2021, including all funding arrangements with up to 124 countries during the cited time window. Last, we obtain macroeconomic data from the World Economic Outlook Database, which includes a time series of different indicators for 195 countries for the period 1980-2021.

Table 55. Country Panel: Descriptive Statistics

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

Notes: Descriptives of the country panel used in the international analysis in Section 5.2. Selected panel variables in rows. Cells indicate averages and standard deviations (in parentheses) for each variable. Columns correspond to different sub-samples. The first column corresponds to the Full Sample including all countries with competitive elections. The second corresponds to the sub-sample of countries that had at least one funding agreement with the IMF in the 1992-2019 period. The final column presents descriptives for the sample of countries that had no funding agreements with the IMF in that period.

Descriptive statistics for the country-panel are provided in Table 55 (Appendix). 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 23 (Appendix) presents two world maps used to illustrate the sample of countries included in the panel (panel A) and those which made at least one funding agreement with the IMF in the sample period (panel B).

Figure 23. 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, & Keefer, 2021). Panel B represents countries that signed a funding agreement with the IMF in that period.

B. Banking Crises

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

$$Program_{ck}^{IMF} = \beta C_{ck} + \gamma_1 \Delta GDP_{ck} + \gamma_2 left_{ck} + \gamma_3 right_{ck} + \mu_{ck} \qquad (14)$$

where $Program_{ck}^{IMF}$ takes value one if country *c* received external financing from the IMF during crisis *k*, and value zero otherwise (notice that some countries suffer more than one banking crisis over the sample period).[77] Estimates of the key parameters of the specification in Equation (14) (Appendix), estimated using our sub-sample of banking crises, are reported in Table 56 (Appendix). We find that, in this sub-sample, the probability to agree with the IMF after a change in office is roughly 27% higher. Anecdotally, this figure is remarkably close to the main effect we estimate with the Spanish municipal data (see Section 5.4).

Table 56. The Impact of Party Changes on IMF Program Implementations - Banking Crises

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

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

7.3.3. 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

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

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 carries 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 net present value 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:

$$NPV_{NoPlan} = A - \sum_{t=1}^{5} \frac{C_{NoPlan}}{(1+r)^t}, where \ C_{NoPlan} = \frac{A \cdot i}{1 - (1+i)^{-5}} \tag{15}$$

$$NPV_{Plan} = A - \frac{A \cdot i}{1+r} - \frac{A \cdot i}{(1+r)^2} - \sum_{t=3}^{58} \frac{C_{Plan}}{(1+r)^t}, where \ C_{Plan}$$
$$= \frac{A \cdot i}{1 - (1+i)^{-8}} \tag{16}$$

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 Spanish 10-year bond yield (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 in 2012, as most municipalities had no access to credit markets, and those who did have access, had it through the banking system. We use the yield paid by Madrid's long-term bonds as a 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 bond yield.[78] We argue that this can be used to calculate a lower bound for the difference between the NPVs 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).

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 NPV of the expected savings from presenting an adjustment plan.[79] To make figures easier to interpret, Table 57 (Appendix) shows the distribution of these savings for all municipalities, both for those who presented a plan and for those who did not.

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[78] We take the average yield for April 2011.
[79] 5.17% was the Spanish 10-year Bond yield in April 2012.

Table 57. NPV Difference of Presenting vs. Not Presenting an Adjustment Plan

	min	max	mean	sd
All Municipalites	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.

The average saving from 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 of savings from presenting an adjustment plan represents 40% of the 2011 deficit. Moreover, for a municipality with the average size of Spanish towns, a cost of €7 per capita roughly equals €60,000. 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.

As a clarification, even if using 10 years to pay back was best, municipalities presenting an adjustment plan could also take less time. Thus, if for any reason any municipality would prefer to pay more quickly, that would hardly justify not presenting an adjustment plan which would allow them to decide the exact repayment scheme. 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 local officer whose salary is paid by the central government, to get

any technical assistance they needed.

7.3.4. 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.

A. 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.[80] For both searches, we omit articles tagged in the "Sports" and "Lottery" categories. We use this information to construct Figure 10, which depicts the frequency of appearance of the SPP in the news, Table 48 (Appendix), which analyzes its coverage through regression analysis, and Figure 14 and Table 49 (Appendix), which analyze 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 5.5.1.

B. ChatGPT Analysis

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[80] Navarra and the Basque Country, the only two Autonomous Communities where Basque is an official language, do not participate in the SPP (see Chapter 2).

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

1. News published in Spain during the period 2012-2013.

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**".[81]

    - Terms related to the SPP or any fiscal consolidation program: "*plan de ajuste*" or "*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*".

This yields 14,154 articles in 2012 and 7,673 in 2013. We use ChatGPT to learn 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. We use this information in Tables 21 and 50 (Appendix) and Figures 15 and 20 (Appendix), which explore the relationship between the content of the news covering the SPP and tenure in office, as well as in Table 22, 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 5.5.2.

Preliminary evidence suggests that ChatGPT is successful in analyzing textual data and could outperform other competing language models (Fatouros, Soldatos, Kouroumali, Makridis, & Kyriazis, 2023; Lopez-Lira & Tang, 2023). We

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

made an intensive effort checking internal consistency and manually reviewing ChatGPT answers to reach optimal prompting. For each news article, we asked the following five questions:

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 ChatGPT a list of possible answers for each question (see details below). Forcing ChatGPT 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 ChatGPT 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 ChatGPT3.5 as our model of choice and set the temperature or "creativity parameter" of the model to zero. 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 está 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 está seguro, responda con "99".*

- *Q4: En esta noticia, ¿quién emite la crítica? Por favor, seleccione una de las siguientes alternativas: Si la crítica 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".

C. 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 24. Newspaper Extract - PP newcomer - Original Spanish

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 a escote obligaría a desembolsar 4.789 euros por cabeza a los hombres, mujeres y niños de Pioz. Ni eso ni aplazar la deuda setenta siglos parecen buenas soluciones.

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

Depuradora y piscina

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

Source: Factiva.

Figure 25. Newspaper Extract - PP newcomer - English Translation

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Colpisa

SE ECONOMY

HD BUDGETS: A town in Guadalajara will take 7,000 years to pay off its debt

PD 10 May 2012

SN Vocento-Colpisa News Feed

SC VOCCLP

CY © VOCENTO. All rights reserved

LP The mayor, from the PP, estimates arrears to be 16 million euros and blames her socialist predecessor.

Madrid, 10 may. (COLPISA).

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

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

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

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

Treatment plant and pool

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

Source: Factiva.

Figure 26. Newspaper Extract - PSOE newcomer - Original Spanish

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.

Source: Factiva.

Figure 27. Newspaper Extract - PSOE newcomer - English Translation

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.

Source: Factiva.

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