



# **The Theater of Democracy: An Empirical Test of Public Spending Visibility in Italian Local Politics**

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Dissertation written under the supervision of Professor Joana Silva

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## **Abstract**

*This dissertation explores whether Italian mayors in small municipalities adjust the composition of public spending for electoral purposes. Do incumbents shift resources toward visible categories, such as investment projects, in the years preceding elections? And how did the 2014 Delrio reform, which extended the number of mayoral terms below the 3,000-inhabitant threshold, affect this pattern? To address these questions, I assembled a novel dataset of 150 municipalities for the period 2010–2019, merging budgetary, electoral, and census data. Exploiting the discontinuity created by the Delrio reform, I implement difference-in-differences and event-study designs to isolate the dynamics of visible spending before and after elections. The results indicate no robust evidence of pre-electoral manipulation: investment does not increase systematically, and in some cases it falls after elections. These findings align with rational inattention theory, suggesting that the visibility of spending is not sufficient to trigger systematic political budget cycles when voters face high costs of acquiring information. Also, empirical evidence suggests that other signals, such as tax reductions, may in some cases be better instruments to manipulate an electoral outcome.*

**Keywords:** political budget cycles, rational inattention, visibility, Delrio reform

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## Resumo

*Esta dissertação investiga se os presidentes de câmara em pequenos municípios italianos ajustam a composição da despesa pública para fins eleitorais. Será que os titulares deslocam recursos para categorias mais visíveis, como os investimentos, nos anos que antecedem as eleições? E de que forma a reforma Delrio de 2014, que alargou o número máximo de mandatos apenas para os municípios com menos de 3.000 habitantes, alterou este padrão? Para responder a estas questões, construí um novo conjunto de dados de 150 municípios no período de 2010–2019, combinando informação orçamental, eleitoral e censitária. Explorando a descontinuidade criada pela reforma, aplico modelos de diferenças-em-diferenças e event-study para isolar a dinâmica da despesa visível antes e depois das eleições. Os resultados indicam que não existem evidências robustas de manipulação pré-eleitoral: o investimento não aumenta sistematicamente e, em alguns casos, diminui após as eleições. Estes resultados são consistentes com a teoria da inatensão racional, sugerindo que a visibilidade, por si só, não é suficiente para gerar ciclos políticos orçamentais quando os eleitores enfrentam elevados custos de informação. Evidências adicionais sugerem que outros sinais, como reduções de impostos, podem em certos casos ser instrumentos mais eficazes para influenciar o resultado eleitoral.*

**Palavras-chave:** ciclos políticos orçamentários, inatensão racional, visibilidade, reforma Delrio

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# Introduction

*“In democracies, people listen to plays, but they do not read them. Most people who attend plays go in search of intense emotions of the heart rather than pleasures of the intellect. They expect to find not a work of literature but a show, and provided that the author speaks the language of the country correctly enough to make himself understood and his characters arouse curiosity and awaken sympathy, they are happy. Immediately thereafter, they return to the real world, without asking anything more of the fiction. Hence style is less necessary on the democratic stage, where the breaking of rules is more likely to pass unnoticed.”*

— Alexis de Tocqueville, *Democracy in America*, Vol. II, Part. II, Ch. 19

Alexis de Tocqueville’s observation that democratic societies often prefer appearances to substance remains central to understanding the character of modern politics. When he returned to France in 1832 after nine months in the United States on behalf of the July Monarchy, he was deeply struck by the American form of State, then the most complete democracy in the world. His reflections culminated in his famous work, *Democracy in America* (Tocqueville, 1840/2012), which is still a cornerstone of political theory to this day. Tocqueville’s reflection on the theater of democratic societies captures a fundamental tension: the public behaves as a theater audience, satisfied by what is immediately perceptible rather than by the deeper structure that remains hidden behind the performance. This observation, though formulated in the nineteenth century, remains still incredibly relevant, as it offers a powerful metaphor that can still play a role in helping us navigate current scenarios, where voters rarely scrutinize budget spreadsheets or long-term fiscal balances, but instead respond to what is most tangible and visible: inaugurations of public works, new infrastructure, symbolic investments, or tax cuts.

This theatrical dimension is particularly vivid in Italy, where politics is historically rooted in strong local identities and where municipalities play a central role in providing services and infrastructure. Particularly, ever since the Law 25 March 1993, n. 81, mayors are directly elected by the citizens. Small towns (municipalities with less than 5,000 inhabitants), which account for nearly 70% of all Italian municipalities (70.2% at the time of the reference sample, as Fondazione IFEL (2011) shows, and still 69,9% in 2024 (Tortorella, 2024)) but host only about 17% of the population (Fondazione IFEL, 2011), are especially significant in this respect. They are a part of Italy’s institutional soul and embody both the persistence of civic traditions, as shown by the long-run legacy of self-governing medieval communes (Guiso et al., 2013), and the constraints of modern fiscal discipline imposed by national and European rules. As such, they represent an ideal stage on which to study how the visibility of public spending interacts with electoral incentives.

The aim of this dissertation is to study how changes in re-election incentives shape the

composition of the budgets of these small municipalities. Specifically, it focuses on the 2014 Delrio reform (*Law 56/2014*), which unexpectedly extended the maximum number of consecutive terms from two to three for mayors in municipalities with fewer than 3,000 inhabitants.

The central question is whether this institutional discontinuity, by granting incumbents in treated municipalities the possibility of an additional mandate, led them to strategically adjust the allocation of municipal resources in order to increase their chances of re-election. We aim to investigate whether mayors facing renewed electoral opportunities have stronger incentives to favor visible investments, such as roads, schools, or public buildings, in the period that precedes the election, in order to signal competence to rationally inattentive voters.

This research aims to integrate the literature on Political Budget Cycles (PBC), an idea that originated in the 1970s, with the concept of Political Business Cycles. The pioneers of this strand are McRae (1977) and Nordhaus (1975), who constructed models in which governments manipulate economic, monetary, and fiscal policies to stimulate the economy and maximize consensus immediately before elections. From the earliest applications, however, it became clear that voters' mere "myopia" was insufficient to explain the persistence of PBCs.

A decisive contribution came in the following decade, from Rogoff and Sibert (1988), who introduced an informational dimension. According to the authors, voters only partially observe government competence because they lack a clear understanding of the composition of revenue and spending; politicians can therefore exploit the informational asymmetry to signal their ability by manipulating the most visible budget items or by resorting to deficits. The electorate rationally updates its beliefs in response to these signals. It follows that cycles are not only evident in aggregate variables such as GDP or inflation, but also, and perhaps above all, in the composition of public spending.

A second refinement stresses the role of institutions and heterogeneity across polities. Constitutional arrangements and electoral systems shape the incentives and instruments available to incumbents (Persson & Tabellini, 2002). Cross-country evidence shows that PBCs are systematically stronger in developing countries, where constraints are weaker and information frictions larger, than in advanced democracies (Shi & Svensson, 2002). Consistent with voters disciplining opportunism, electoral returns tend to be lower (or negative) when pre-electoral policy loosening compromises fiscal discipline (Peltzman, 1992), exposing the jurisdiction to deficit. In line with this pattern, PBCs are most evident in new democracies and largely muted in established ones, where fiscal rules are able to tame more effectively PBCs (Brender & Drazen, 2005). This perspective motivates the focus of this dissertation on what governments choose to make visible (the composition of spending) and under which constraints.

A large body of work shows that electoral manipulation often operates not through

the *level* of public outlays but rather through their *composition*. Building on asymmetric-information models where incumbents signal competence to voters (Rogoff & Sibert, 1988), the literature shows that often politicians tend to prioritize “voter-friendly” expenditures without necessarily expanding total outlays, a pattern that is rewarded at the polls so long as it does not result in large fiscal imbalances (Drazen & Eslava, 2010; Drazen & Eslava, 2006).

Comparable patterns appear across a wide set of decentralized contexts. In Portugal, municipal governments cut taxes and increase highly visible capital outlays in pre-electoral periods (Veiga & Veiga, 2007). In Brazil, capital spending rises in the pre-electoral period, whereas current spending tends to spike in the election year itself, with both margins associated with higher reelection probabilities (Sakurai & Menezes-Filho, 2008). French evidence shows that the share of “equipment” expenditure (investment) predicts both vote shares and reelection (Cassette et al., 2009). In Canadian provinces, governments of all stripes tend to stop tax increases during election years and expand program spending in particularly visible areas, such as schools, roads, and recreational facilities (Kneebone & McKenzie, 2001). Across these contexts, a common pattern emerges: a “visibility channel”: when voters are attention-constrained and information is imperfect, politicians manipulate the spending mix toward items that are easiest to observe.

Yet, spending-composition cycles are not uniform across contexts, as they are mediated by institutional quality and the degree of transparency available to voters. In Israel, for instance, as fiscal reporting improved and media scrutiny intensified, voters became more responsive to governments’ overall fiscal performance, and simple pre-electoral spending surges lost effectiveness (Brender, 2003). In Indian states, by contrast, there is little evidence of broad expansions before elections; however, incumbents strategically deploy instruments such as targeted tax reliefs, consistent with the selective rewarding of certain groups of electors (Khemani, 2004). Mexico provides yet another variant: there, the magnitude of pre-electoral increases in infrastructure and transfers actually rises during more democratic periods, suggesting that when electoral competition tightens but transparency does not keep pace, composition cycles may become even more pronounced (González, 2002).

Italy offers especially fertile ground for the study of political budget cycles, both due to the abundance of municipal-level fiscal data and the presence of strict fiscal rules. A consistent picture emerges from the literature: opportunistic dynamics are concentrated in the composition of expenditure, particularly in capital outlays, although their scope is shaped by institutional and informational constraints.

Cioffi et al. (2012) document that Italian municipalities exhibit clear electoral cycles in investment spending, but mainly when mayors are independent from national parties. In these cases, the absence of strong partisan ties amplifies the relevance of local incentives, making visible investments the preferred vehicle to signal competence. Subsequent re-

search emphasizes the importance of transparency and the media environment. Repetto (2016) exploits a reform that mandated the publication of final budgets before elections, showing that pre-electoral investment spending increased by about 28.5 percent in the pre-reform period, often financed through new debt issuance. After the reform, however, the magnitude of these surges declined substantially, and the effect was particularly attenuated in provinces with higher newspaper circulation. The finding that more informed voters discipline opportunistic behavior is perfectly consistent with the informational theories of political cycles.

Other studies highlight alternative channels through which incumbents pursue electoral advantage. Anessi-Pessina and Sicilia (2015) find that local governments strategically bias revenue forecasts, overestimating or underestimating inflows to create fiscal room in election years. Alesina and Paradisi (2017) document that municipalities often resort to the tax lever, studying the case of the then new property tax (IMU) around elections, with the effect concentrated in small municipalities and in areas where fiscal space allows. The impact of fiscal rules is also crucial. Bonfatti and Forni (2019) show that the Internal Stability Pact, which imposed strict budgetary constraints on municipalities, significantly reduced the scope for pre-electoral investment hikes. Likewise, Grembi et al. (2016) analyze a natural experiment in which municipalities below 5,000 inhabitants were exempted from these constraints, finding that treated municipalities responded by increasing deficits and cutting taxes, especially when mayors were eligible for re-election and local councils were fragmented.

The literature also connects fiscal institutions with the quality of politicians. Gagliarducci and Nannicini (2013) exploit the discontinuity in mayoral salaries at the 5,000-inhabitant threshold, showing that higher pay attracted better-educated and more competent politicians, which in turn improved fiscal outcomes. Bellido et al. (2019), in contrast, examine health expenditure across OECD countries and find that electoral cycles are absent in this less visible domain, where political partisanship rather than opportunism shapes budget allocations. Overall, the Italian evidence confirms that political budget cycles hinge on the visibility of fiscal choices, but that their scope is conditioned by fiscal rules, media scrutiny, and institutional design.

Although Italian evidence highlights the role of fiscal rules, party affiliation, and media scrutiny in shaping political budget cycles, these mechanisms are clearly manifestations of a broader informational issue. Voters, even when not entirely inattentive, face limits in their ability to process complex fiscal data and therefore rely on a narrow set of observable cues. This raises a deeper theoretical question: how do citizens decide which dimensions of government action deserve their scarce attention, and how do politicians adapt their strategies accordingly?

This is the angle from which the recent literature on rational inattention is most useful. Building on the pioneering work of Sims (2003), which introduced the notion that agents

face cognitive constraints related to the impossibility of retaining all available information, subsequent models have formalized how these frictions affect choice. In particular, Matějka and McKay (2015) show that when information acquisition is costly, decision rules take the form of discrete-choice logit structures in which noticeable payoffs receive disproportionate weight. Extending this reasoning to the political sphere, Matějka and Tabellini (2021) demonstrate that candidates strategically frame their platforms around issues that inattentive voters are more likely to notice, while broader public-good provision remains underemphasized. As Mackowiak et al. (2023) emphasize in their recent survey, rational inattention thus offers a unifying framework to understand why visibility is not just an epiphenomenon of fiscal politics, but a central determinant of electoral competition.

Despite the richness of the existing literature, we must consider that there are still several gaps that might be filled. To start, the Italian contributions available so far have mainly focused on either aggregate investment cycles or alternative margins, such as taxation; however, we still lack quasi-experimental evidence on how an exogenous reform of reelection incentives reshapes the composition of municipal budgets. In the second instance, and more fundamentally, the existing PBC literature has not yet been systematically connected to the recent theoretical advances on rational inattention, which explicitly model how voters allocate their scarce cognitive resources and why politicians therefore emphasize only the most visible signals of performance.

This dissertation aims to address these gaps and makes three contributions. First, through the construction of a *novel dataset*, it provides new empirical evidence on Italian municipalities, exploiting the 2014 Delrio reform, which unexpectedly extended the maximum number of terms from two to three for mayors in towns below 3,000 inhabitants, creating a sharp discontinuity in reelection incentives. Second, it bridges the empirical tradition on political budget cycles with the theory of rational inattention, offering a unified framework. Third, it contributes to the comparative debate by situating the Italian experience alongside evidence from Latin America, North America, and Europe, thereby highlighting both the commonality of the visibility channel and the ways in which fiscal rules and institutional constraints condition its scope.

Using a panel of 150 Italian municipalities over 2010–2019 and a two-way fixed-effects difference-in-differences design with an event-study specification, I find no systematic evidence of pre-electoral increases in capital expenditure in treated towns (below 3,000 inhabitants) relative to slightly larger controls. If anything, the dynamic coefficients are at most small and centered around zero before elections, while some post-election lags turn negative and occasionally attain marginal statistical significance. Looking at the evidence as a whole, the patterns suggest that the visibility channel through investment is weak in this setting; consistent with recent Italian evidence, the tax lever appears to be a more salient electoral instrument than capital spending under tight fiscal constraints

(Alesina & Paradisi, 2017; Bonfatti & Forni, 2019; Grembi et al., 2016).

As in Italy a “small municipality” is defined as with less than 5,000 inhabitants, the Delrio Law, which unexpectedly extended the maximum number of consecutive terms from two to three for mayors in municipalities below 3,000 inhabitants, generates a treatment and a slightly larger control group. The effect of the reform on these municipalities is estimated using a two-way fixed-effects (municipality and year) framework, with clustering of standard errors at the municipality level. To study dynamics, I estimate an event-study specification with leads and lags of treatment relative to the reform-induced change in reelection incentives.

I interpret these estimates with caution, as recent studies have shown that two-way fixed effects event studies can be biased when effects are heterogeneous (Sun & Abraham, 2021; Callaway & Sant’Anna, 2021; Chaisemartin & D’Haultfœuille, 2020; Chaisemartin & D’Haultfœuille, 2022; Wooldridge, 2021; Jakiela, 2021; Kropko & Kubinec, 2020). For this reason, I do not rely on the baseline specification alone. I first check the pre-trends, which turn out to be small and statistically indistinguishable from zero, and then concentrate on the post-treatment dynamics. In addition, I run a series of robustness checks: trimming extreme outliers, allowing for differential pre-trends, placebo reforms, and falsification tests with non-visible outcomes. I also experiment with population weights, alternative sets of fixed effects, and separate estimations by macro-area. Across all these specifications, the results remain consistent, pointing to the absence of systematic pre-electoral surges in investment spending.

The remainder of the dissertation is organized as follows. Section 2 presents the theoretical framework that connects political budget cycles with rational inattention. Section 3 describes briefly the institutional background of Italian municipalities, with particular attention to the 2014 Delrio reform, and introduces the data sources and the descriptive statistics. Section 4 outlines the empirical methodology, including the TWFE difference-in-differences and event-study approaches. Section 5 discusses the results and robustness checks. Finally, Section 6 concludes by highlighting the absence of systematic pre-electoral surges in investment spending, relating this evidence to the framework of rational inattention, and discussing the broader implications for electoral accountability and the role of fiscal institutions in shaping political budget cycles.

## Theoretical Framework

The theoretical basis for this research is the work of Matějka and Tabellini (2021), which is based on the idea that voters are unable to efficiently allocate their attention to all the factors that could determine their choice between one candidate and another. In

particular, as the granularity of information increases and is presented in greater detail, the cost of information for the individual voter becomes very high, as obtaining information requires more time and attention, so their final choice will be determined by factors that appear more visible to them. In this way, the voter's selective ignorance becomes the most important asset for a mayor intending to implement opportunistic spending behavior. This model is interesting because it shows that rational but uninformed voters can also decide how to allocate their attention, which in this case comes at a cost. The attention they can devote is, in fact, a limited resource and will be allocated to what they deem most important to them. Now, their model is based on US Congressional elections, but its features make it valid and fascinating in this situation as well.

The model considers two opportunistic candidates  $C \in \{A, B\}$  who choose a vector of policies

$$q_C = \{q_{C,i}\}_{i=1}^L. \quad (2.1)$$

Voters are grouped into categories  $J$  with mass  $m_J$ , and their utility depends on the composition of policies through a concave function  $U^J(q_C)$ , to which an idiosyncratic preference shock  $x^v$  is added.

Candidates announce a target  $\bar{q}_{C,i}$  for each instrument  $i$ , but the actual implementation contains a random error:

$$q_{C,i} = \bar{q}_{C,i} + e_{C,i}, \quad e_{C,i} \sim \mathcal{N}(0, \sigma_{C,i}^2). \quad (2.2)$$

Voters do not observe  $q_C$  directly; instead, they receive noisy signals  $s^{v,J}$  on each  $q_{C,i}$ . As a consequence, they choose how much attention  $\xi_{C,i}^J$  to allocate to each dimension, taking into account informational costs. The voting decision is based on the difference between expected utilities conditional on the signals: voters choose  $A$  if

$$\mathbb{E}[U^J(q_A) | s_A^{v,J}] - \mathbb{E}[U^J(q_B) | s_B^{v,J}] \geq x^v. \quad (2.3)$$

In equilibrium, given an attention profile  $\xi$ , each candidate  $C$  will maximize the perceived welfare of his policies induced among voters as the weighted average of their expected utilities from policy  $q_C$ . The first-order conditions for policy choice imply that, for each instrument, the sum of marginal utilities  $u_{JC,i}$  weighted by the masses  $m_J$  and the attention levels  $\xi_{C,i}^J$  is zero:

$$\sum_{J=1}^N m^J \xi_{C,i}^J u_{C,i}^J = 0, \quad \forall i \quad (2.4)$$

On the voters' side, the optimal attention is obtained by maximizing an informational benefit proportional to  $(u_{JC,i})^2 \sigma_{C,i}^2$  minus a logarithmic cost. The solution is

$$\xi_{C,i}^J = \max \left\{ \xi_0, 1 - \frac{\tilde{\lambda}_{C,i}^J}{(u_{C,i}^J)^2 \sigma_{C,i}^2} \right\}, \quad (2.5)$$

where  $\xi_0$  represents a minimum imposed level of attention.

In essence, more *visible* policy instruments (i.e. low information costs  $\tilde{\lambda}$ , high marginal utilities, or large variances  $\sigma^2$ ) receive more attention and, by the effect of condition (2.4), have a greater weight in the candidates' choices. Conversely, less observable components are underweighted.

In the Italian context, capital spending is often the most "visible" instrument that mayors tend to focus on (projects, construction sites, inaugurations), and it is no coincidence that numerous studies document political-budget cycles driven precisely by this component: Cioffi et al. (2012) show that, at the municipal level, electoral expenditure cycles are "primarily driven by capital spending" and that the cycle is more pronounced for mayors who are not affiliated with large political parties. On the other hand, Alesina and Paradisi (2017) highlight how mayors can also tend to manipulate the revenue side: in fact, they showed how, close to elections, municipalities lower the rate of the new property tax (IMU), with significant fiscal effects, especially in small municipalities, a clear example of how tax cuts are also a strong visible policy.

Therefore, the municipality can invest in more "serious" investments, such as underground maintenance or safety measures, which however are difficult to observe or distributed over the long term, and are hence destined to receive less political weight; or focus on highly visible interventions such as the construction of symbolic works (e.g. a large fountain in the main square, improving the public garden). In the case of this thesis, if the Delrio Law has altered the political incentives in the treated municipalities, we expect a post-2014 variation in the composition of expenditure compared to the controls.

Since a part of the capital expenditure is salient (visible works), but a large part is difficult to observe (widespread maintenance and long-term benefits), it is therefore difficult to establish its sign a priori. Our aim is to verify whether the Delrio Law has generated an effect on the share of capital expenditure in the treated municipalities. By introducing an exogenous institutional shock to the mayor's re-election, it has indeed changed the structure of incentives, and therefore we could expect a different effect on the treated group compared to the control group.

# Data and Descriptives

## 3.1 Dataset Construction

To construct the dataset on which this thesis was conducted, I started from an ISTAT document that included the entire list of Italian municipalities with fewer than 5,000 inhabitants, thus definable as "small municipalities", whose populations were updated as of October 9, 2011. This is not a major problem for the analysis, as a census was conducted in Italy every 10 years until 2018, after which it became annual. I then proceeded to extract a random sample of 150 municipalities out of a total of 5,574, ensuring balanced regional representativeness (Molise and the autonomous province of Trento have one fewer municipality, being the least populous regions) and excluding a priori those with fewer than 1,000 inhabitants, as their spending composition cannot realistically be compared to their more populous counterparts. The idea was therefore to reduce extreme heterogeneity bias. Other municipalities excluded from the sample were all those in the Aosta Valley and the autonomous province of Bolzano. Since 1998, the Aosta Valley has independently regulated the term and term limits of mayors, allowing a third consecutive term in municipalities with fewer than 15,000 inhabitants and even a fourth term if one of the three previous terms lasted less than two years, six months, and one day, provided the term was not terminated for reasons other than resignation. Bolzano, however, was excluded because it anticipated the entry into force of the Delrio Law, which introduced the third term, by a few months. Furthermore, the selected sample almost exactly reflects the national distribution (approximately 30%) of small municipalities with fewer than 5,000 but more than 3,000 inhabitants.

The choice of a sample size of 150 municipalities was guided by the need to ensure sufficient statistical power while balancing feasibility within the scope of available resources. This number provides a sufficient basis for the generalizability of the findings and still allows for precise estimates. Moreover, as the sample was stratified to ensure balanced representation across Italian regions, the risk that the results are driven by region-specific characteristics is neutralized, and there is some plausibility that the findings can be generalized to the broader population of small municipalities in Italy.

For each selected municipality, I retrieved CSV files related to the administrative history of each municipality from the Ministry of the Interior's (Department of Internal and Territorial Affairs) website. For each election, each file contains information on the election date, the candidate's last name and first name, office description, list/party affiliation, date of entry into office, date of termination, and status of office (e.g., proclamation, natural expiration of the mandate). Starting from these files, I then proceeded to identify all

the rows related to the mayor’s office. Using the entry and exit dates, I chronologically reconstructed the mandates of each mayor for each municipality.

This work was carried out manually: for each mayor, I verified the correct assignment of the mandate, the number of consecutive mandates, any resignations, and re-election, thus building the mayoral panel. The panel includes variables such as election year, budget year, number of consecutive mandates, re-election indicator, population, and population group (<3,000 inhabitants and 3,000–5,000). I also derived a variable, *pre\_electoral*, which equals 1 in the year of the next election and the previous one, to identify the pre-election period.

The data describing the financial dimension are sourced from SIOPE (Operating System for Public Entities Operations, managed by the Bank of Italy), which provides information on payments made by each entity for each year, starting from January 1, 2008. From the payment data, I extracted current expenditures, capital expenditures, and other expenses; however, it was challenging to identify more specific types of expenditures due to the varying reporting structures of many files. Expenditures were aggregated by municipality and year, normalized for population, and also expressed as a percentage of the total (*perc\_ca*, *perc\_ka*, *perc\_oth*). The proxy for visible expenditure I chose is capital expenditure, since its normal destination, such as public works, infrastructure, schools, and roads, produces tangible benefits that are easily observable by citizens. The choice is linked to Matějka and Tabellini (2021) theory of rational inattention, which posits that voters with limited information capacity tend to focus on visible interventions, amplifying the political impact of these expenditures. The lower visibility of other budget items (current expenditures and transfers) and the findings of Cioffi et al. (2012) research, which shows that capital expenditure is the main driver of PBCs when they are present, further justify this choice.

The expenditure data were then merged with the panel of mayors using the municipality’s MEF (Ministry of Economy and Finance) code and the year. In years in which a mayor changed during the financial year (e.g., municipal elections in June), the database contained two observations for the same municipality in a given year, one for the outgoing mayor and one for the incoming mayor. To maintain consistency in the dataset, I retained only the observation of the outgoing mayor, as the annual budget is largely prepared by the outgoing council, and discarded the observation of the incoming mayor. This reduced the number of rows from 1780 to 1497 unique observations (municipality-year mayor). I created dummy variables for the post-2014 effect (years  $\geq 2014$ ), Treated (municipalities with population <3,000), and pre-election (the year immediately preceding and the year of the next election). I eliminated the cases of the municipalities of Orani (placed under special administration in 2015) and Orciano di Pesaro (abolished in 2016 and incorporated into the new municipality of Terre Roveresche), which would not have allowed the budget to be correctly associated with a mayor; following the merger of Terre Roveresche,

the positions are assigned to deputy mayors and consultants appointed by the municipal council for each former municipality. I also manually corrected the case of the mayor of Ventimiglia di Sicilia: Andrea Pagano, elected in 2008, resigned in March 2010; The council recalled former mayor Andrea Mannina to complete his term; therefore, the 2010 expenses are attributed to the Pagano/Mannina administration.

Finally, I continued by adding four new socioeconomic variables from a document published in the Official Journal of the Italian Republic on September 14, 2021 (document attached to protocol 172637 of 2021). Although their collection occurred after the analysis period (2010–2019), I assume that these characteristics are relatively stable over time for the small municipalities considered, allowing them to be used as structural controls. In other words, since these indicators reflect underlying socioeconomic and territorial conditions, it is plausible that they have not undergone changes that would render their retrospective use invalid. Some municipalities are not included in the 2021 source because they have been suppressed or merged in the meantime; in these cases, it was not possible to associate the corresponding socioeconomic values with them. A variable was also added to indicate the macro-region to which the municipality belonged, based on its region. The added socioeconomic variables are, in fact, four dummies:

- *low\_IRPEF*: equals 1 if the municipality's per capita IRPEF taxable income is below the national average (€ 20.213,73);
- *low\_social*: equals 1 if the municipality's social services spending is below the average (€ 116,00);
- *far\_from\_city*: equals 1 if the municipality is distant from large urban centers;
- *low\_density*: equals 1 if the population density is less than 80 inhabitants per square kilometer.

Indicators such as IRPEF income, social spending, density, and remoteness allow us to control for structural factors that could influence both mayors' spending behavior and voters' propensity to punish or reward them. For example, municipalities with lower incomes or sparse populations may allocate public expenses based on different priorities compared to wealthier or more urban municipalities. Including these indicators in the regressions helps reduce the risk of bias due to unobserved characteristics.

For each municipality, I assigned a macro-area (North, Centre, South and Islands) following the division commonly adopted in official statistics: the North includes Piedmont, Valle d'Aosta, Lombardy, Trentino-Alto Adige, Veneto, Friuli Venezia Giulia, Liguria, and Emilia-Romagna; the Centre includes Tuscany, Umbria, Marche, and Lazio; and the "Mezzogiorno" (which I have referred to here as "South and Islands") includes Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sicily, and Sardinia.

Adding the variable to define macro-areas is helpful to explore how capital spending manipulation may vary between the North, Centre, and Mezzogiorno. The structural differences between Italy’s macro-areas, such as levels of development, infrastructure, political culture, and fiscal transfers, are well known and could generate different electoral responses. Incorporating the macro-area into the model, or estimating separate regressions for each area, allows us to capture these heterogeneities.

In conclusion, the dataset thus constructed includes 150 small municipalities randomly selected from a population of 5,574, observed over the period 2010–2019 and broken down into 1,497 unique observations (municipality–year–mayor). It integrates administrative (mandates, re-election), financial (current expenditures, capital expenditures, other expenditures), socioeconomic (income, social spending, density, distance from urban centers), and geographic (regions and macro-area) information. This structure allows us to systematically analyze the relationship between budget dynamics, institutional constraints, and electoral behavior in small Italian municipalities.

## **3.2 Descriptive Statistics**

To better understand the subsequent empirical section of this thesis, it is helpful to briefly focus on descriptive statistics. This chapter hence presents some descriptive statistics that can enhance our understanding of the territorial composition, economic size, and any heterogeneity over the period under consideration.

### **3.2.1 Sample Composition**

The final dataset comprises 1,497 observations spanning ten years (from 2010 to 2019) and represents 150 small municipalities with fewer than 5,000 inhabitants. Table B.1 provides a summary of the sample: the number of observations, the number of unique years, the time period, and the number of municipalities distinguished by MEF code, ISTAT code, and name are indicated. All of the selected municipalities have a valid ISTAT code.

To verify the sample’s territorial representativeness, observations are counted by region (Table B.2). This way, we can see that almost all regions contribute 80 observations, with the exception of Molise and Trentino-Alto Adige (70 observations), for the reasons mentioned in the previous chapter, and Marche (77 observations), due to the abolition of the municipality of Orciano di Pesaro. Consequently, the three conventional macro-areas (North, Centre, South and Islands) are equally represented.

In Figure 3.1, we can see which years saw the greatest concentration of elections, with the 2009 and 2014 elections standing out as trends, consistent with the five-year electoral cycle. This is interesting because this way, we can always analyze at least two elections per municipality, and in some cases, even three.

The histogram of the average population per municipality (Figure 3.2) shows that the sample covers a fairly uniform range between 1,000 and 5,000 inhabitants, with a particular concentration between 1,500 and 2,500 residents. The graph is consistent with the previous statement that our control group, comprising municipalities with 3,000 and 5,000 inhabitants, represents approximately one-third of the total sample, which aligns with the national distribution.

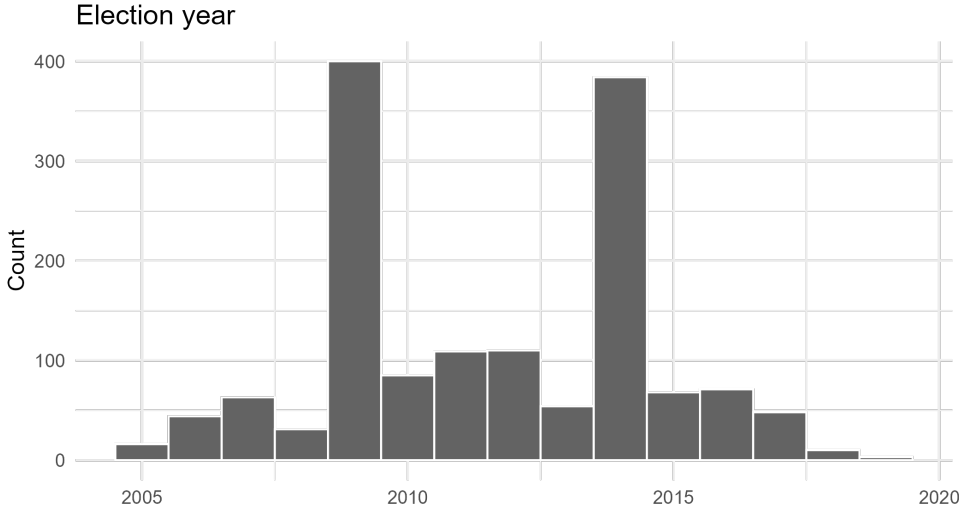


Figure 3.1: Distribution of the municipal election years in the sample. The histogram counts the number of municipality–year observations in each election cycle and highlights peaks around 2009 and 2014.

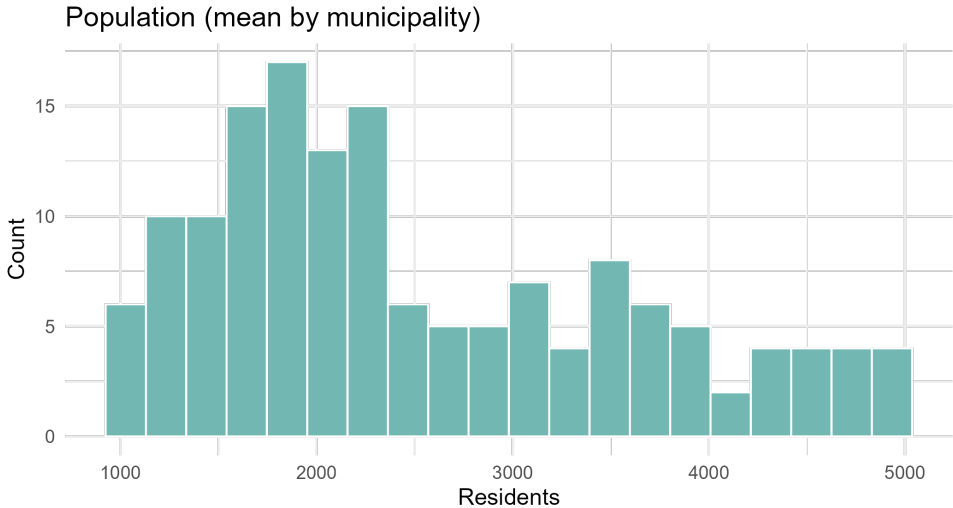


Figure 3.2: Distribution of the average population per municipality (2010–2019). Most municipalities have between 1,500 and 2,500 inhabitants, with the sample covering the entire 1,000–5,000 range.

### 3.2.2 Economic Size of Administrations

It is interesting to briefly examine Tables 3.1 and 3.2 to get a sense of the magnitude of the resources managed by small municipalities. Table 3.1 presents statistics on total and per capita amounts for three spending categories: current expenditure, capital expenditure, and other expenditure, as well as total expenditure. The total annual expenditure of an average municipality is around €3.72 million, with a median of €3.08 million. Of this total expenditure, the largest component is certainly current expenditure (median 1.72 million). Capital expenditure, which will serve as a proxy for "visible" investment below, has a median value of approximately €550,000 and a mean value of €771,000. Per capita expenditure is also highly variable, as can be seen from Figures 3.3 and 3.4; both the distribution of total expenditure and the distribution of capital expenditure are right-skewed. The median for total per capita expenditure is €1,342, while the median for capital expenditure per capita drops to €232. The high interquartile range and long tail demonstrate the presence of municipalities that make investments much higher than the median; these positive outliers likely arise due to socioeconomic disparities between municipalities that depend on their geographical location.

In terms of expenditure shares (Table 3.2), almost 60% of the budget is absorbed by current expenditures (median 60.5%), while capital expenditures represent an average of 20% of total expenditures (median 18.1%). The remaining share is allocated to "other expenditures" (transfers, services, etc.), which on average account for 21%. The distribution of the investment share is also asymmetric: Figure 3.5 shows that most municipalities allocate between 5% and approximately 40% of the budget to investment, with some exceptions exceeding 60% and one outlier that even approaches 80%.

This evidence, therefore, demonstrates not only the strong heterogeneity in the spending capacity of small municipalities but also how the capital expenditure component, although a minority, can assume a significant weight and potentially be more "visible" to voters, making it particularly suitable as a proxy in subsequent analyses.

Table 3.1: Summary stats for total and per-capita amounts by spending category (EUR).

Category	Obs.	Mean	Std. dev.	P10	Med.	P90	Min	Max
Current exp.	1 497	2 053 039.78	1 194 936.46	963 259.58	1 722 707.92	3 638 514.81	386 084.28	10 767 439.97
Capital exp.	1 497	771 034.95	744 271.71	148 899.27	550 744.77	1 634 680.51	365.86	7 755 714.07
Other exp.	1 497	895 884.27	1 164 460.99	223 524.43	467 488.18	2 074 846.47	45 701.05	14 896 585.27
Total exp.	1 497	3 719 958.99	2 270 388.47	1 650 616.62	3 080 099.54	6 597 259.35	488 042.26	26 472 350.76
Pc current	1 496	833.22	366.73	531.31	740.75	1 173.75	79.90	3 164.29
Pc capital	1 496	347.70	383.86	65.31	232.46	738.59	0.08	5 510.34
Pc other	1 496	362.76	402.63	104.41	201.15	939.96	9.46	3 958.70
Pc total	1 496	1 543.68	784.74	817.75	1 341.83	2 571.45	101.00	7 064.57

Table 3.2: Summary statistics for budget shares by spending category.

Share	Obs.	Mean	Std. Dev.	P10	Median	P90	Min	Max
Current expenditure share	1 497	0.5814	0.1500	0.3647	0.6054	0.7639	0.1658	0.9327
Capital expenditure share	1 497	0.2065	0.1341	0.0567	0.1806	0.3983	0.0002	0.7800
Other expenditure share	1 497	0.2121	0.1436	0.0940	0.1559	0.4835	0.0342	0.6996

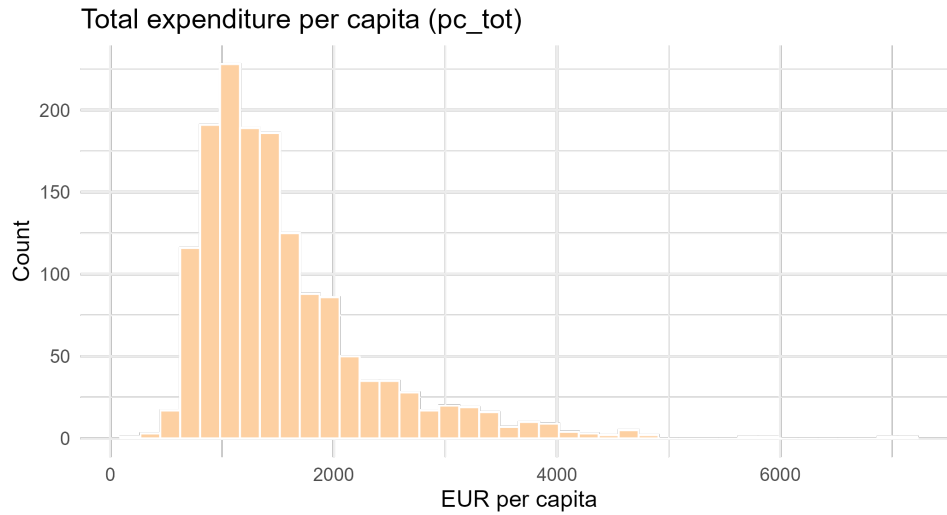


Figure 3.3: Distribution of per-capita total expenditure. The histogram is right-skewed: most municipality–year observations spend between €1,000 and €3,000 per resident, with a few outliers above €5,000.

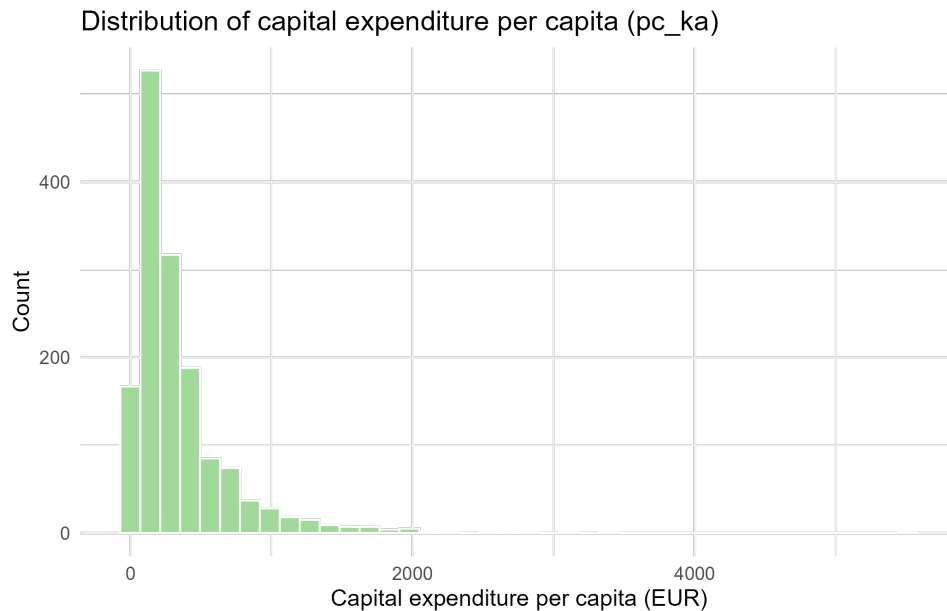


Figure 3.4: Distribution of per-capita capital expenditure. Most observations invest less than €1,000 per resident, with a long right tail reflecting municipalities investing far above the median.

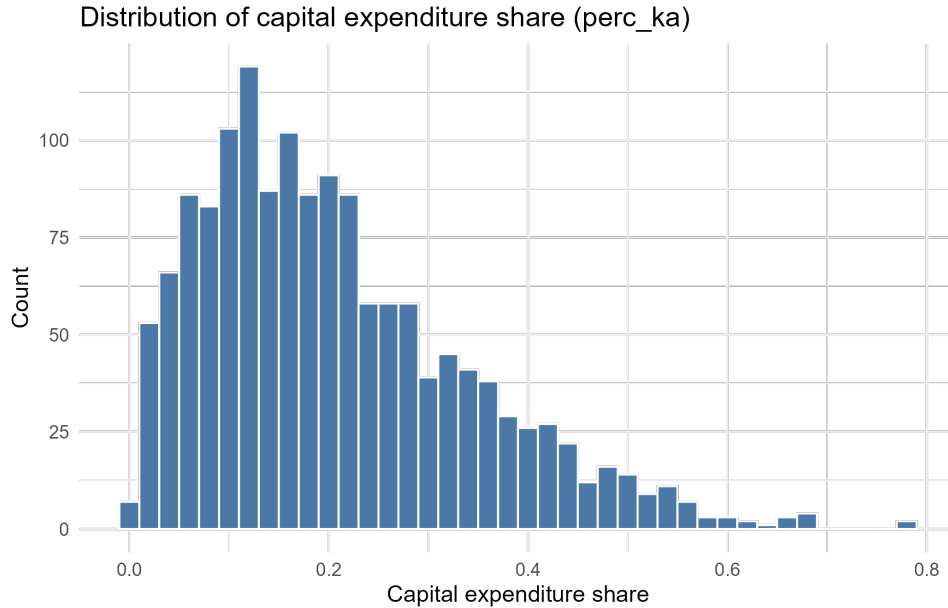


Figure 3.5: Distribution of the investment share (capital expenditure as a proportion of the budget). Most municipalities allocate between 10% and 40% of their budgets to capital expenditure; a few observations exceed 60%.

### 3.2.3 Pre-election Year and Spending Behavior

A crucial test for analyzing the existence of political budget cycles is to understand whether municipalities increase capital spending in the years immediately preceding the elections. For this reason, the `pre_election` variable takes the value 1 in the year of each election and the year preceding it. Table 3.3 reports the mean and median values of the investment share (`perc_ka`) and capital expenditure per capita (`pc_ka`), distinguishing between pre-election years and ordinary years. The differences that emerge between the groups are modest: counterintuitively, the mean investment share drops from 0.209 in non-election years to 0.202 in pre-election years, while per capita expenditure decreases from 354 to 338 euros. The Welch t-test (Table 3.3) shows that these differences are not statistically significant (p-value close to 0.29 for `perc_ka` and 0.41 for `pc_ka`).

Figure 3.6 makes these data even more impactful by visually showing how little change occurs between groups. It is clear that the median differs only slightly and that the variability is large in both groups. The same is true for per capita spending (Figure 3.7), which, however, shows greater concentration. Overall, these descriptive results tell us that systematic political budget cycles (PBCs) do not emerge in small municipalities, as neither the investment share nor per capita capital spending increases significantly in the pre-election years. This aspect is particularly useful for the empirical analysis, as it reduces the risk that mayors' opportunistic behavior could confound the effect of the 2014 reform. In other words, the absence of systematic variations before the elections is a favorable condition for the credibility of the difference-in-differences strategy adopted

below.

Table 3.3: Summary of investment share and per-capita capital expenditure by pre-election status.

Pre-elect. yr	Obs.	Mean inv. share	Med. inv. share	Mean pc capex	Med. pc capex
No	921	0.2094	0.1818	353.95	233.06
Yes	575	0.2019	0.1797	337.69	231.72

Table 3.4: Two-sample Welch t-tests comparing pre-election and non-pre-election years.

Test	t-statistic	p-value
Investment share	1.059	0.290
Per capita capex	0.820	0.413

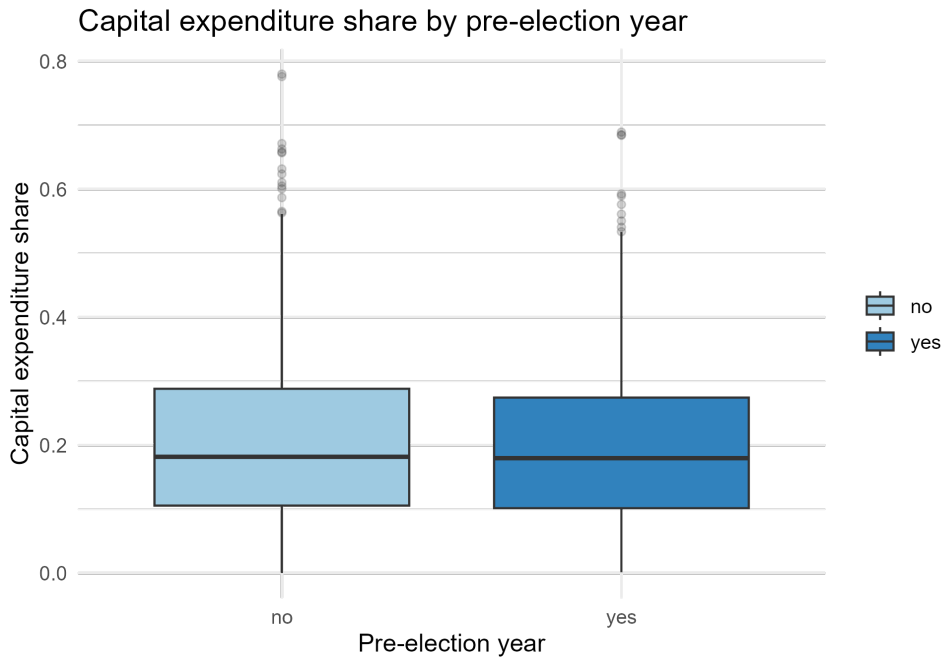


Figure 3.6: Investment share by pre-election status. The boxplots compare the distribution of the share of capital expenditure between non-election years (no) and pre-election years (yes); individual points denote outliers.

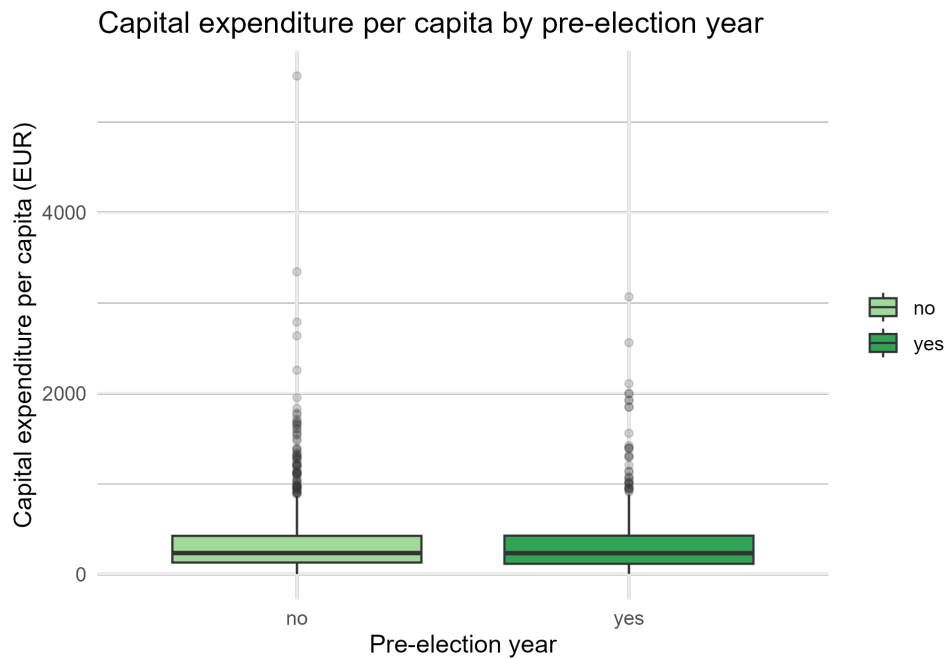


Figure 3.7: Per-capita capital expenditure by pre-election status. The boxplots show the distribution of per-capita capital expenditure in non-election years and pre-election years, with jittered points representing individual observations.

### 3.2.4 Territorial Differences

Another factor of fundamental importance, especially in a context like Italy, is the country’s territorial characteristics. These are often correlated with significant disparities regarding issues such as financial availability and voter preferences. Table 3.5 summarizes the share and per capita spending on investments according to the three macro-areas. Municipalities in the South (South and Islands) have a higher average investment share (21.6%) and per capita spending of €372, while the North stands at intermediate values (average share 20.3%, €316 per capita) and the Centre records the lowest values (average share 19.3%, €354 per capita). Figure 3.8 shows the distribution of perc\_ka by macro-area: in the South, the median is higher and the dispersion is greater, while in the Centre, the values are concentrated at lower levels. However, when comparing pre-election years with other years within each macro-area (Table 3.6 and Figure 3.9), the differences are not statistically significant.

Overall, territorial differences in average spending levels are evident, but it should be noted that they do not translate into systematic pre-electoral behavior: only for municipalities in the North, to be precise, does a hint of statistical significance emerge (p-value close to 0.09), which is still too weak to indicate a true political cycle. In conclusion, despite the presence of structural heterogeneity across macro-areas, my descriptive analysis so far has found no consolidated pre-electoral patterns that could undermine the identification of the reform’s effect in subsequent analysis.

Table 3.5: Investment share and per-capita capital expenditure by macro-area.

Macro area	Obs.	Med. inv. share	Med. pc capex	Mean inv. share	Mean pc capex
North	550	0.1811	195.15	0.2030	316.36
Centre	317	0.1672	228.71	0.1929	354.08
South & Islands	629	0.1984	272.46	0.2165	371.90

Table 3.6: Two-sample t-tests comparing pre-election and non-pre-election years by macro-area.

Macro area	t-statistic	p-value
North	1.6955	0.0906
Centre	0.4373	0.6622
South & Islands	-0.2699	0.7873

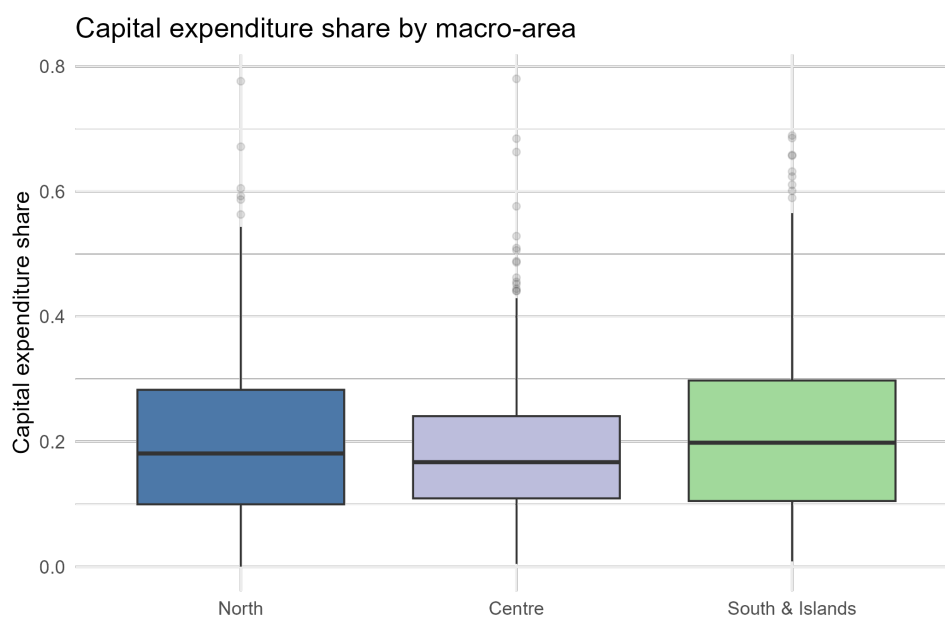


Figure 3.8: Investment share by macro-area. Boxplots compare the share of capital expenditure across the three macro-areas (North, Centre and South & Islands).

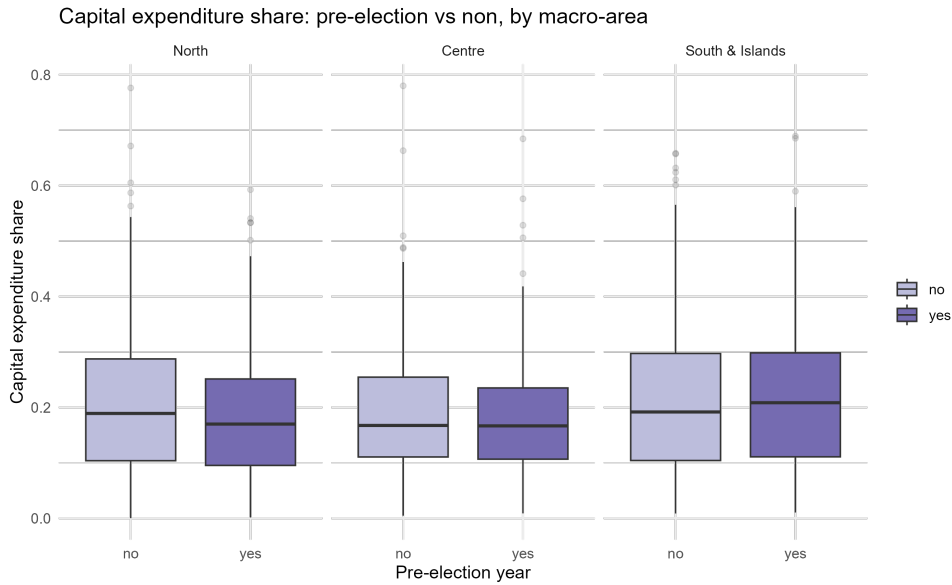


Figure 3.9: Investment share by pre-election status within each macro-area. For each macro-area, boxplots compare the distribution of the investment share in non-election years (no) and pre-election years (yes).

### 3.2.5 Demographic Size and Structural Factors

In order to isolate the effect of the new electoral regime and institutional constraints, it is useful to examine whether demographic size or certain socioeconomic characteristics are correlated with the share of investments. Figure 3.10 discusses demographic characteristics, comparing municipalities with fewer than 3,000 inhabitants (treated) with those between 3,000 and 5,000 inhabitants (controlled). We immediately see that smaller municipalities have a slightly higher median `perc_ka` and greater dispersion. This is likely related to the fact that initiatives such as infrastructure projects, linked to capital expenditure, have a proportionally larger impact on the budgets of very small municipalities. To control for structural factors, four dummies were introduced: taxable income (IRPEF) below the national average, below-average social spending, distance from major urban centers, and population density below 80 inhabitants per square kilometer. A detailed analysis of the differences between the levels of these variables is presented in the Appendix (Figures A.3, A.4, A.5, A.6 and corresponding Tables). In summary, municipalities with per capita incomes below the national average or very low population densities tend to have higher investment shares, while those with low social spending tend to have lower shares. For this reason, these variables will then be used as controls in the regressions.



Figure 3.10: Investment share by treatment group. Municipalities below 3,000 inhabitants (treated) are compared with those between 3,000 and 5,000 inhabitants (control). Treated municipalities show a higher median investment share and greater dispersion.

### 3.2.6 Further Analyses and Robustness Checks (see Appendix)

Further descriptive analyses and checks are presented in the Appendix. Among these:

- Classifications by province and municipality: Tables B.3 and B.4 list the provinces and municipalities in the sample, sorted by median investment share, highlighting how some provinces and small towns (such as Sellano and Denno) stand out for very high `perc_ka` values.
- Reasons for termination of office: Table B.5 shows that in 94% of cases, mayors end their mandates due to natural expiration, while resignations or appointments to a commissionership only rarely occur.
- Number of terms and re-election: Tables B.6 and B.7 show the distribution of consecutive terms and re-election eligibility; Figure A.1 highlights that mayors who are not eligible for re-election tend to allocate slightly higher investment shares than their eligible colleagues.
- Distribution of shares by year, macro-area, and region: Figures A.11, A.12, A.13 allow us to appreciate the annual and regional variations, particularly the heterogeneity within Southern Italy, where some regions (such as Basilicata and Puglia) show high medians, while Sicily has a significantly lower median than any other region.

- Robustness checks: Table B.8 presents a version of per capita capital expenditure with a 1% trimming of the extreme observations; the differences between pre-election and non-election years remain insignificant. Table B.9 reports the same differences after demeaning the shares year by year, showing mean values of almost zero.

# Empirical Model and Methodology

## 4.1 Theoretical Context and Motivation

As Matějka and Tabellini’s theoretical framework demonstrates, voters decide how much information to seek based on their expected utility and the cost of information; therefore, the candidate will strive to maximize the voter’s perceived welfare. When the candidate in question is also the incumbent, it is in their best interest to manipulate capital expenditures. As shown in a study of Colombian voters (Drazen & Eslava, 2006) the incumbent loses a significant number of votes when the budget is in deficit, but capital expenditures are rewarded, unlike current expenditure, which appears to have no effect. Bonfatti and Forni (2019), in their analysis of the Italian case, similarly note that political budget cycles are dominated by capital expenditures. Likewise, this type of visible spending is drastically reduced when more stringent fiscal rules are imposed on municipalities. Still in Italy, Repetto (2016) documents that capital expenditures increase by 28.5% in the year preceding elections and that these increases are financed primarily by new debt. An analysis of all 8,000+ Italian municipalities conducted by Cioffi et al. (2012) also shows that the pre-election cycle is driven almost exclusively by capital expenditures, and that this effect is more pronounced for mayors without a party affiliation. Capex is therefore a plausible proxy for visibility, even in the Italian context, because it focuses on tangible/observable interventions by voters, even in the short term (roads, public buildings, street furniture), while current expenditures tend to be less ”photogenic.”

Over the years, Italy has seen various institutional mechanisms activated to limit prolonged tenure in office and reduce opportunities for manipulation. There has been ongoing discussion, and still is, about the need to prevent local administrators, whether municipal or regional, from exploiting their position to establish a sort of ”fiefdom” for themselves, as the issue remains a topic of heated debate. Law 81/1993, whilst establishing direct elections, limited mayors to two consecutive terms, but the 2014 reform (Delrio Law) extended the limit to three terms for municipalities with a population of fewer than 3,000. This change creates an administrative discontinuity that can be used for quasi-experimental analyses.

### 4.1.1 Unit of Analysis and Variables

The unit of observation is municipality  $i$  in year  $t$ . All municipalities with a population between 1,000 and 5,000 inhabitants are considered, excluding the cases previously mentioned in the Data chapter. Municipalities with a population of fewer than 3,000 residents therefore, constitute the "treated" group, as they are subject to the extension of the term limit, whereas municipalities with a population between 3,000 and 5,000 residents serve as the control group. The main variables are:

- **Outcome:** the share of capital expenditure in total expenditure ( $\text{perc\_ka}_{it}$ ), capital expenditure per capita ( $\text{pc\_ka}_{it}$ ) and the logarithm of capital payments ( $\ln(\text{Capital}_{it} + 1)$ ).
- **Treatment:** an indicator  $\text{Treated}_i$  that is worth 1 if the municipality has fewer than 3,000 inhabitants and 0 otherwise. A temporal indicator  $\text{post}_t$  that is worth 1 for the years from 2014 onwards and 0 before.
- **Pre-election variable:**  $\text{PreElect}_{it}$  is a dummy value that is 1 if year  $t$  immediately precedes the municipal elections and 0 otherwise. The information comes from the Ministry of the Interior.
- **Controls:** average taxable income, per capita social spending, population density, distance from the provincial capital, indicators of socioeconomic deprivation, and dummies by macro-area (North, Centre, South and Islands).

## 4.2 Identification Strategy

The discontinuity created by the Delrio Law allows us to estimate the reform's causal effect on spending composition using a difference-in-differences (DiD) approach. To use this method, we must verify the key hypothesis of parallel pre-trends. This states that, without the reform, investment spending in treated and untreated municipalities would have followed parallel paths. The literature finds that in Italy, pre-election cycles have a particularly significant impact on capital expenditure. After the Domestic Stability Pact was relaxed for small municipalities, these cycles decreased only in municipalities with more than 5,000 inhabitants. Therefore, the 3,000-inhabitant threshold is a plausibly exogenous cutoff with respect to previous trends.

### 4.2.1 Baseline Model (Two-Way DiD)

The baseline specification uses year fixed effects ( $\lambda_t$ ) and municipality fixed effects ( $\gamma_i$ ) to control for time-invariant heterogeneity and shocks common to all municipalities, respec-

tively. The estimated regression is

$$Y_{it} = \beta \left( \text{Treated}_i \times \text{post}_t \right) + \delta \text{PreElect}_{it} + \gamma_i + \lambda_t + \varepsilon_{it}, \quad (4.1)$$

where  $Y_{it}$  is the outcome (in this research, primarily the share of capital expenditure),  $\beta$  measures the average effect of the Delrio reform on the difference between treated and control after 2014, and  $\delta$  captures the effect of the pre-electoral cycle common to all municipalities. Standard errors are clustered at the municipal level to account for unobserved shocks that may be correlated across years within the same municipality, thereby avoiding underestimation of the estimates' variability.

The equation (4.1) corresponds to a two-way model (TWFE) and coincides with the DiD estimator when there are two periods and two groups. However, in the presence of multiple periods and heterogeneous treatments, the literature has highlighted that the TWFE coefficients are weighted combinations of treatment effects and can be biased. To mitigate this problem, in addition to the basic specification, pre-trend tests and sensitivity analyses are performed.

### 4.2.2 Event Study and Testing the Parallel Trends Hypothesis

An excellent way to test the parallel trends hypothesis and study the dynamics of the effects over time is to estimate a model with "leads" and "lags" of the interaction between treatment and relative period. Let  $\text{rel\_year} = t - 2014$  be the variable indicating the number of years before or after the reform. The specification is

$$Y_{it} = \sum_{k \neq -1} \theta_k 1\{\text{rel\_year} = k\} \times \text{Treated}_i + \delta \text{PreElect}_{it} + \delta_i + \lambda_t + \varepsilon_{it}, \quad (4.2)$$

where the year immediately preceding the reform ( $k = -1$ ) is the reference category. The coefficients  $\theta_k$  track the evolution over time of the difference between treated and control municipalities. If the trends are parallel before the reform, ideally, the coefficients for  $k < 0$  should be statistically indistinguishable from zero. However, we must still take into account that, in the presence of heterogeneous effects over time, the interpretation of regressions with *leads* and *lags* requires caution because TWFE estimators can be contaminated by effects from other periods (Sun & Abraham, 2021). Therefore, event analysis is complemented by alternative specifications and placebo tests.

### 4.2.3 Heterogeneity by Geographic Area

As explained in the Data chapter, Italy is a nation with fairly marked structural differences depending on the macro-area it belongs to, so the DiD models are estimated separately for the North, Centre, South and Islands. Indeed, the literature shows that the response

to budget cycles can vary with the level of social capital and the degree of voter control: for example, the study of Italian municipalities conducted by Alesina and Paradisi (2017) finds more pronounced cycles in the South than in the North. Indeed, a substantial literature, among others Putnam et al. (2012) and Guiso et al. (2013) shows how factors such as mutual trust, civic sense, and therefore social participation tend to be lower in the South. In such a context, the opportunistic politician may feel less subject to constraints because he is observed less closely (Nannicini et al., 2013). Therefore, we include an interaction term between the year and macro-area fixed effect and estimate separate regressions for each area.

## **4.3 Further Methodological Considerations**

### **4.3.1 Robustness and Sensitivity**

In addition to the main specifications, the analysis includes several robustness checks: (i) truncated sample estimation, excluding the top and bottom 1% of the expenditure distribution to avoid the influence of outliers; (ii) inclusion of group-specific linear trends ( $\text{Treated}_i \times t$ ) to control for pre-existing differences; (iii) placebo tests that anticipate the reform (e.g., assuming treatment begins in 2012) to verify that the estimated effect is not due to previous shocks; (iv) use of a "falsified" outcome (current expenditure share) to verify that the effect does not extend to theoretically less visible budget components; (v) weighting the regressions by the municipal population; (vi) alternative specifications with fixed effects of geographic area, region, or region-year to account for territorial shocks.

### **4.3.2 Limitations of the TWFE and Recent Alternatives**

Estimation with municipality-year fixed effects requires the assumption of homogeneous treatment effects over time and across units. Recent research (Sun & Abraham, 2021; Callaway & Sant'Anna, 2021) shows that, when the treatment is adopted at different times and the effects vary across groups, TWFE coefficients can be combined weights of heterogeneous effects and may produce biased estimates. The literature proposes alternatives such as group-time estimators or doubly robust differences that aggregate elementary DIDs. In this study, the reform is applied simultaneously to all treated municipalities, reducing heterogeneity in timing; however, this potential problem is acknowledged and sensitivity is controlled with additional analyses.

# Results

In this chapter our aim is to document the patterns followed by capital expenditure before and after the extension of mayoral term limits and to test whether treated municipalities adjust their investment behavior relative to slightly larger control municipalities.

## 5.1 Pre-treatment Balance

Before analyzing the effects of the reform, it is essential to determine whether treated and control municipalities followed similar trajectories in the pre-reform period.

Table 5.1 reports the coefficients from regressions of the outcome variables on the treatment indicator using only data from 2010 to 2013. We can see how treated municipalities begin with a higher share of capital expenditure and a higher per-capita investment level than control municipalities; these differences are statistically significant at conventional levels.

This implies that treated municipalities had higher investment levels even before the reform, reinforcing the need to control for municipal fixed effects in subsequent analyses.

Table 5.1: Pre-treatment balance: estimated differences between treated and control (2010–2013)

	Capital share	Capex per capita
Treated (pre only)	0.045* (0.018)	163.0** (49.2)
Observations	600	600
$R^2$	0.05367	0.03871
Within $R^2$	0.02428	0.03061

**Notes:** Coefficient estimates from regressions of pre-treatment outcomes on the treatment indicator, using only the pre-reform period (2010–2013). The dependent variables are the share of capital expenditure and per-capita capital expenditure. Standard errors clustered at the municipality level are in parentheses. Significance levels: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## 5.2 Main Effects of the Reform

The baseline specification in equation (4.1) estimates the average effect of the Delrio reform on the share of capital expenditure over the total expenses, capital expenditure per capita, and the log of total capital payments. Table 5.2 reports the results. The coefficient on the difference-in-differences interaction ( $\beta$ ) is negative across all outcomes,

but it never reaches a statistically significant level. In other words, once we take into account municipal and year fixed effects, the extension of the term limit from two to three mandates does not reduce the average investment effort of small municipalities relative to the control group. The pre-election dummy ( $\delta$ ) is positive, consistent with the presence of political budget cycles, but it is not statistically significant in the baseline specification.

Table 5.2: Difference-in-differences estimates of the Delrio reform

	Capital share	Capex per capita	Log total capex
Pre-election ( $\delta$ )	0.008 (0.005)	16.5 (13.4)	0.063 (0.041)
DiD ( $\beta$ )	-0.016 (0.015)	-48.7 (34.4)	-0.051 (0.115)
Observations	1,496	1,496	1,496
$R^2$	0.58596	0.58395	0.53538
Within $R^2$	0.00338	0.00282	0.00204

**Notes:** Coefficient estimates from the baseline two-way fixed-effects specification with municipality and year fixed effects.

Standard errors clustered at the municipality level are in parentheses.

Significance levels: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

### 5.3 Dynamic Effects and Event Study

To explore the dynamics of the treatment effect and to test the parallel trends assumption more explicitly, equation (4.2) introduces leads and lags of the treatment. The event study graph (Figure 5.1) plots the estimated coefficients  $\theta_k$  for each relative year  $k$ . The results reveal that treated municipalities already display lower capital expenditure shares two years before the reform is enacted, with the coefficient at  $k = -2$  being significantly negative, at 5% level. We then see the coefficients remaining negative and becoming statistically significant from three years after the reform onwards ( $k = +3$  to  $k = +5$ ), which may imply that the reform did not produce an abrupt change at the time of implementation, but rather a gradual divergence over the longer run. Table 5.3 reports the exact coefficient estimates.

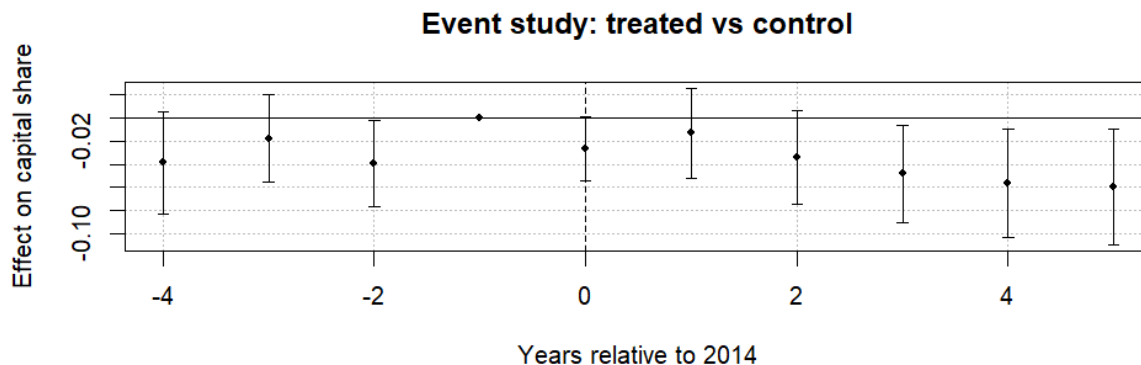


Figure 5.1: Event study: estimated difference between treated and control municipalities in the share of capital expenditure.

Table 5.3: Event-study estimates of the reform impact (reference year 2013)

Relative year $k$	Coefficient $\theta_k$ (s.e.)
-4	-0.039 (0.022)
-3	-0.018 (0.019)
-2	-0.039* (0.019)
0	-0.027 (0.014)
+1	-0.013 (0.019)
+2	-0.034 (0.020)
+3	-0.048* (0.021)
+4	-0.056* (0.024)
+5	-0.059* (0.025)
Observations	1,496
$R^2$	0.58920
Within $R^2$	0.01118

**Notes:** The table reports the coefficients from the event-study specification where the year immediately prior to the reform ( $k = -1$ ) is omitted as the reference category. Standard errors clustered at the municipality level are in parentheses. Significance levels: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## 5.4 Heterogeneity by Geographic Area

Given the marked socioeconomic disparities across Italian macro-areas, the baseline model is re-estimated separately for the North, Centre, and Mezzogiorno, as summarized by Table 5.4. Interestingly enough, the pre-election coefficient is positive and significant only for municipalities in the Centre, possibly indicating more pronounced electoral budget cycles there. On the other hand, the DiD coefficient remains negative in all macro-areas but is not statistically different from zero. This suggests that the lack of an average effect

(at the national level) is not due to the fact that in one macro-area the effect is positive and in another negative, thus offsetting each other. In reality, the effect is zero everywhere, only with slightly different but still insignificant intensity..

Table 5.4: Difference-in-differences estimates by macro-area

	North	Centre	South and Islands
Pre-election ( $\delta$ )	0.004 (0.006)	0.020* (0.010)	0.012 (0.010)
DiD ( $\beta$ )	-0.019 (0.024)	-0.043 (0.032)	-0.005 (0.024)
Observations	550	317	629
$R^2$	0.65882	0.57326	0.55960
Within $R^2$	0.00373	0.02493	0.00399

**Notes:** Separate difference-in-differences estimates for municipalities in the North, Centre and South and Islands.

Standard errors clustered at the municipality level are in parentheses.

Significance levels: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## 5.5 Robustness Checks and Alternative Specifications

The credibility of the baseline results is assessed through a battery of robustness checks. First, the sample is trimmed by excluding the top and bottom 1% of the capex per capita distribution; the DiD coefficient remains negative and insignificant. Second, a group-specific linear trend ( $\text{Treated}_i \times t$ ) is added to allow for differential pre-reform trends; this specification yields a near-zero estimate of the treatment effect. Third, placebo tests shift the treatment to 2012 and 2011; both placebo DiD coefficients are small and statistically insignificant, suggesting that the baseline result is not driven by pre-existing shocks. Fourth, the share of current expenditure is used as a falsification outcome; as expected, no significant treatment effect is found. Fifth, regressions are weighted by municipal population; the estimated coefficients change little. Finally, a specification with year-by-macro-area fixed effects ( $\text{Year} \times \text{Macro area}$ ) yields a similar negative and insignificant DiD estimate. Table 5.5 summarizes the key coefficients from these robustness specifications.

Table 5.5: Robustness checks: alternative specifications and placebo tests

Specification	Pre-election ( $\delta$ )	DiD ( $\beta$ )	Obs.	Outcome
Trim 1%	10.8 (11.7)	-36.6 (30.3)	1,466	Capex pc
Group-spec. trend	0.009 (0.005)	0.007 (0.019)	1,496	Capex share
Placebo 2012	0.008 (0.005)	-0.006 (0.016)	1,496	Capex share
Placebo 2011	0.008 (0.005)	0.006 (0.019)	1,496	Capex share
Falsification outcome	-0.007 (0.006)	0.012 (0.020)	1,496	Curr. acc. share
Pop. weights	0.011* (0.005)	-0.013 (0.015)	1,496	Capex share
Main+ FE (Year $\times$ Macro area)	0.011* (0.006)	-0.019 (0.015)	1,496	Capex share

**Notes:** *Capex pc* = capital expenditure per capita. *Capex share* = capital expenditure over total expenses. *Curr. acc. share* = current expenditure over total expenses. Standard errors clustered at the municipality level are in parentheses.

Significance levels: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

As an additional robustness exercise, I re-estimated the baseline model using different sets of fixed effects. In particular, municipality fixed effects are replaced by broader geographic identifiers: (i) macro-area, (ii) region, and (iii) region-year interactions. This specification absorbs territorial shocks at different levels of aggregation while allowing for cross-sectional variation.

Table 5.6 reports the results. The main findings remain unchanged: the difference-in-differences coefficient is consistently negative but statistically indistinguishable from zero across all specifications. Interestingly, in these models the *Post2014* dummy is large and negative (-5.8 percentage points, significant at the 1% level), while the *Treated* dummy is positive and weakly significant. This pattern indicates that the overall level of capital expenditure declined in all municipalities after 2014, irrespective of treatment status, whereas treated municipalities tended to spend more than controls on average. The pre-election effect remains small and generally insignificant, although in the region-year specification it turns weakly positive.

Table 5.6: Alternative fixed-effects specifications

	(1) Macro-area FE	(2) Region FE	(3) Region×Year FE
<i>Key regressors</i>			
Treated	0.038* (0.019)	0.029 (0.017)	0.035* (0.018)
Post2014	-0.058*** (0.012)	-0.058*** (0.012)	
DiD ( $\beta$ )	-0.012 (0.015)	-0.013 (0.015)	-0.023 (0.015)
Pre-election ( $\delta$ )	-0.0005 (0.006)	0.001 (0.005)	0.016* (0.006)
<i>Controls</i>			
low_IRPEF	0.038 (0.023)	0.037 (0.024)	0.037 (0.025)
low_social	-0.036 (0.021)	-0.026 (0.022)	-0.026 (0.023)
far_from_city	0.014 (0.018)	-0.002 (0.015)	-0.002 (0.016)
low_density	0.031 (0.016)	0.034* (0.015)	0.034* (0.016)
<i>Fixed effects</i>			
macro_area	Yes	No	No
Region	No	Yes	No
Region×year	No	No	Yes
Observations	1,479	1,479	1,479
$R^2$	0.11791	0.29025	0.38118
Within $R^2$	0.11392	0.10902	0.04761

**Notes:** Outcome = share of capital expenditure over total expenditure. Each column estimates the same DiD model with alternative sets of fixed effects: (1) macro-area FE; (2) region FE; (3) region-by-year FE absorbing shocks common to municipalities within the same region and year. The coefficient labeled DiD ( $\beta$ ) is the interaction  $Treated \times Post2014$  and captures the causal effect of the reform. In column (3) the main effect  $Post2014$  is omitted due to collinearity with region-by-year FE. Controls: below-average IRPEF taxable income, low per-capita social spending, remoteness from major cities, and low population density. Standard errors (in parentheses) clustered by municipality.

Significance codes: \*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ ,  $p < 0.1$ .

**Summary of Findings** Across a wide range of specifications, the difference-in-differences analyses do not detect a statistically significant average impact of the Delrio reform on municipal capital expenditure.

While treated municipalities already exhibited higher investment shares before the reform, the main DiD coefficient remains negative and imprecisely estimated.

Event-study estimates show that differences between treated and control municipalities widen gradually in the years following the reform, suggesting a medium-term divergence rather than an immediate effect.

No major heterogeneity is uncovered across macro-areas, and the robustness checks, including placebo dates, trimming, group-specific trends, population weights and alternative fixed-effects structures, reinforce the conclusion that the extension of term limits has not led to a statistically meaningful shift in capital spending patterns at the municipal level.

## Conclusion

The empirical analysis yields three central findings. First, the traditional pre-electoral surge in capital expenditure is largely absent after 2014. The main difference-in-differences estimates are close to zero or negative, with no evidence of systematic investment cycles across the treated municipalities. This result stands in contrast with the classic evidence of strong pre-electoral booms in Italy (Repetto, 2016; Cioffi et al., 2012). Second, the event-study dynamics reinforce this picture. Instead of rising in election years, the relative trajectory of treated municipalities drifts downward in the years following the Delrio reform. From the third year onwards, coefficients turn negative and sometimes statistically significant, indicating a gradual compression of investment. Third, the analysis uncovers no substantial heterogeneity across macro-areas. The Centre shows a modest, short-lived increase, but overall the story is consistent: no robust investment cycles and no offsetting effects hidden in the aggregate. Robustness checks, including placebo reforms, group-specific trends, trimming, falsification outcomes, and alternative fixed effects, support the credibility of these results.

Overall, the evidence points to a structural change: The once dominant political budget cycle in capital spending appears to have faded. The contribution of this thesis is to document this shift and connect it to the framework of rational inattention. Why does this matter? Rational inattention provides a lens. When information is costly and fiscal space is narrow, voters focus on the signals that are easiest to grasp. Incumbents adapt accordingly, emphasizing policies with the highest salience-to-cost ratio.

Under the constraints of the Domestic Stability Pact, capital projects became harder to scale and less electorally rewarding. In this setting, avoiding tax hikes or manipulating

salient rates, as shown by Alesina and Paradisi (2017) on property taxation, may deliver greater political returns than launching new investment projects. Moreover, with the constitutional reform of 2012 (Law No. 243/2012), Italy introduced the principle of a balanced budget, requiring municipalities, regions, and provinces to ensure equilibrium between revenues and expenditures and to contribute to national debt sustainability. This new rule gradually replaced the former Internal Stability Pact and became binding for local governments starting in 2016. Its implementation led to a marked compression of capital expenditure, as administrations redirected resources toward containing current spending in order to comply with mandatory budgetary targets.

The negative coefficients after 2014 are therefore not paradoxical. They reflect a structural compression of capital spending in treated municipalities, consistent with Bonfatti and Forni (2019), who show that fiscal rules dampen both cycles and overall investment. The political “stage” is far from having vanished, but the props have changed.

Several caveats temper these conclusions. The treated and control groups display minor pre-treatment differences, even though robustness checks mitigate concerns. Cash-based accounting may under-record short-term manoeuvres. The TWFE estimator, while standard, blends heterogeneous effects with weights that are not always intuitive. Finally, the focus on small municipalities raises questions of external validity for larger or metropolitan settings.

Future research can move in several directions. One promising avenue is to “unpack” capital expenditure into specific categories, to see whether some items still display cyclical patterns. Another is to extend the analysis to larger municipalities, where both fiscal space and political stakes are higher. Tax policy and user fees also deserve more attention, since manipulation may have shifted to these margins.

Beyond fiscal aggregates, micro-level project data and communication strategies could enable the direct measurement of visibility, although collecting such information would be demanding. Finally, comparative work across countries with different fiscal rules and media environments could clarify how institutional and informational frictions interact in shaping cycles.

In short, the classic investment cycle in small Italian municipalities has largely vanished. Far from contradicting existing theory, this pattern confirms the adaptability of incumbents: when constrained, they do not stop playing the game; they simply change the props.

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# Appendix A: Figures

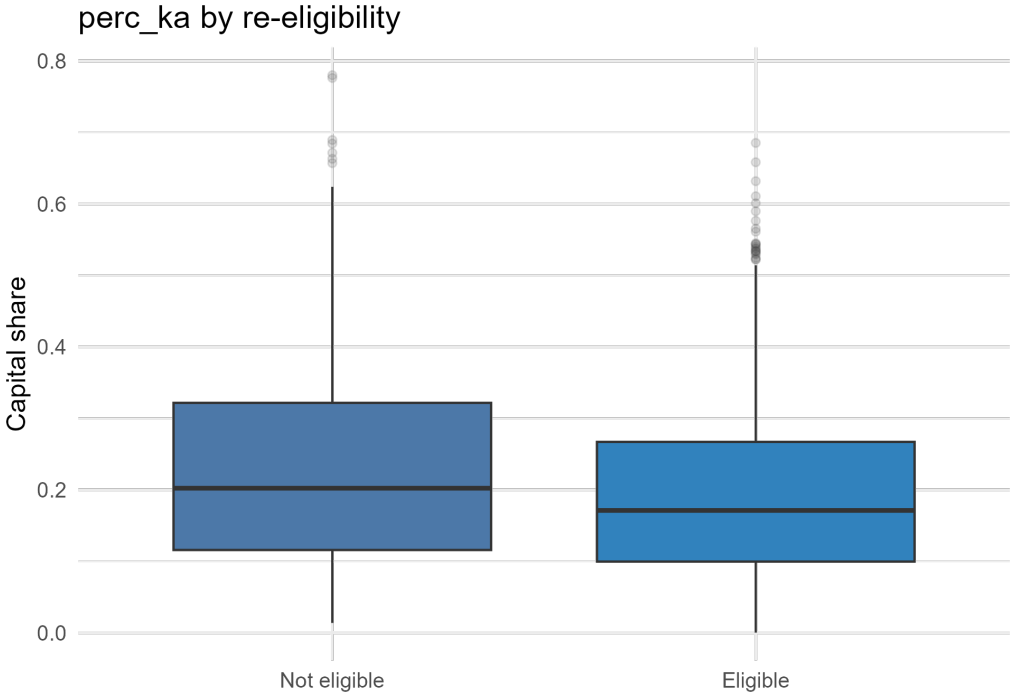


Figure A.1: Distribution of the percentage of capital expenditures by re-election eligibility. Each boxplot shows the share of capital expenditure in total spending for municipalities where the mayor is eligible or not eligible for re-election.

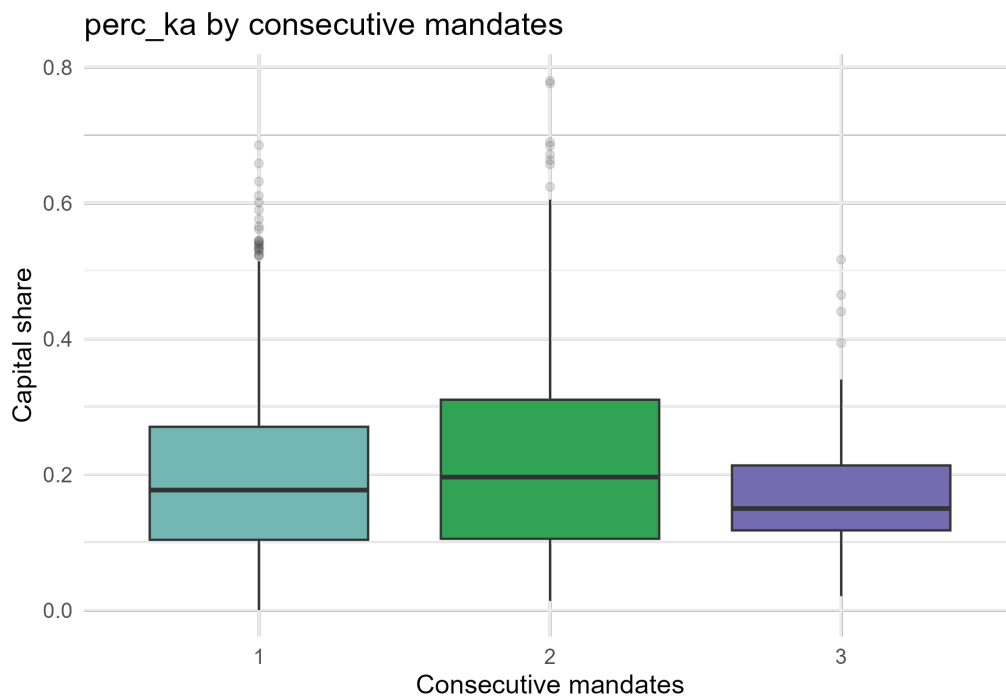


Figure A.2: Distribution of the percentage of capital expenditures by number of consecutive mandates. Categories on the horizontal axis correspond to first, second and third consecutive terms.

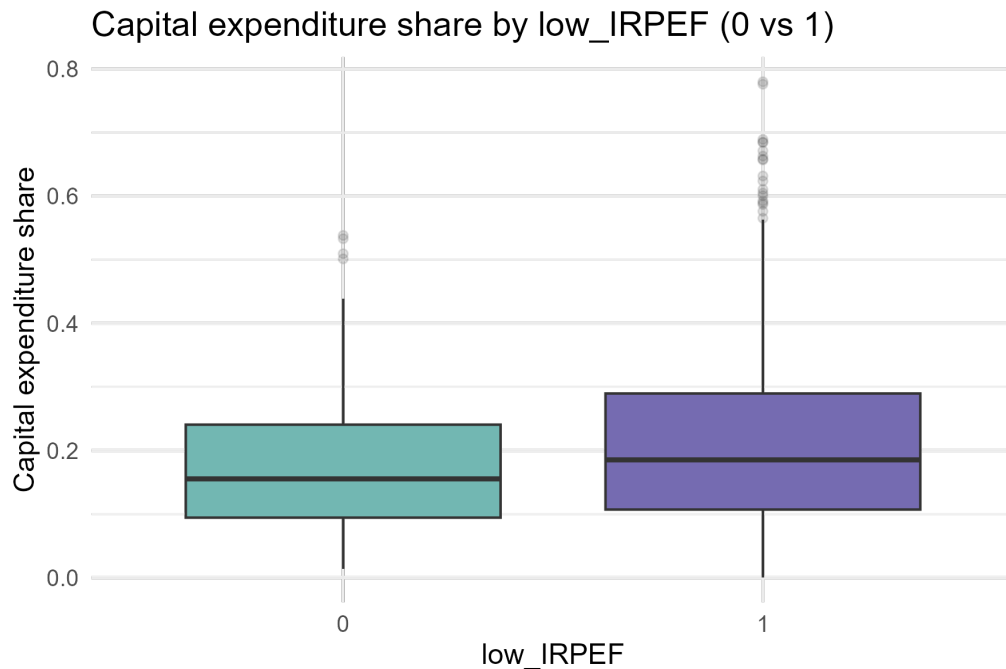


Figure A.3: Distribution of the percentage of capital expenditures by the indicator for low per-capita IRPEF taxable income. The dummy equals one for municipalities in which IRPEF is, on average, lower than the national mean.

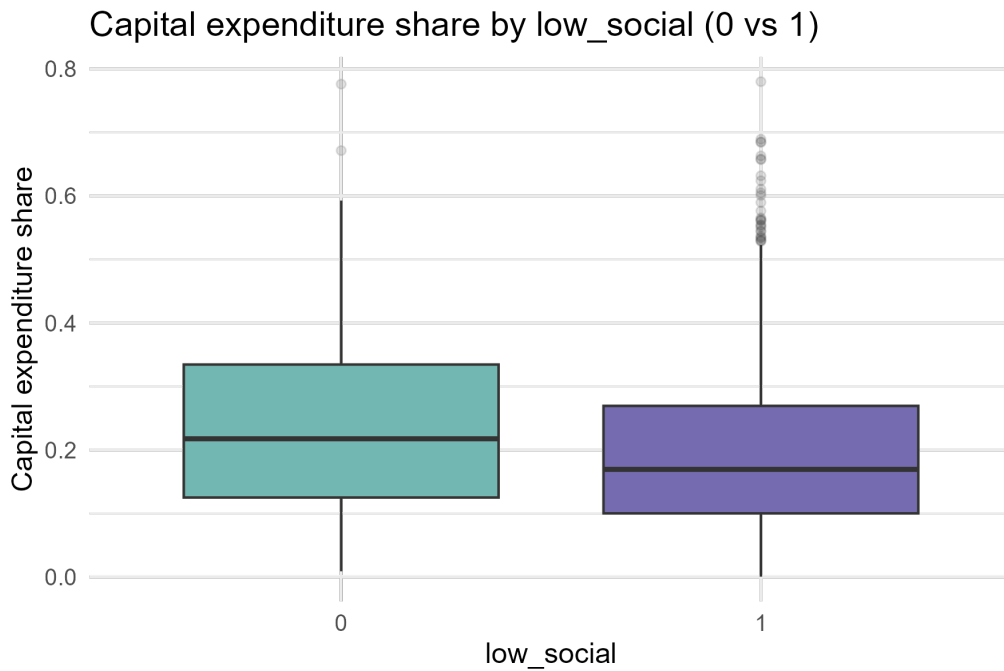


Figure A.4: Distribution of the percentage of capital expenditures by the indicator for low social spending. The dummy equals one for municipalities with relatively low per-capita social expenditure.

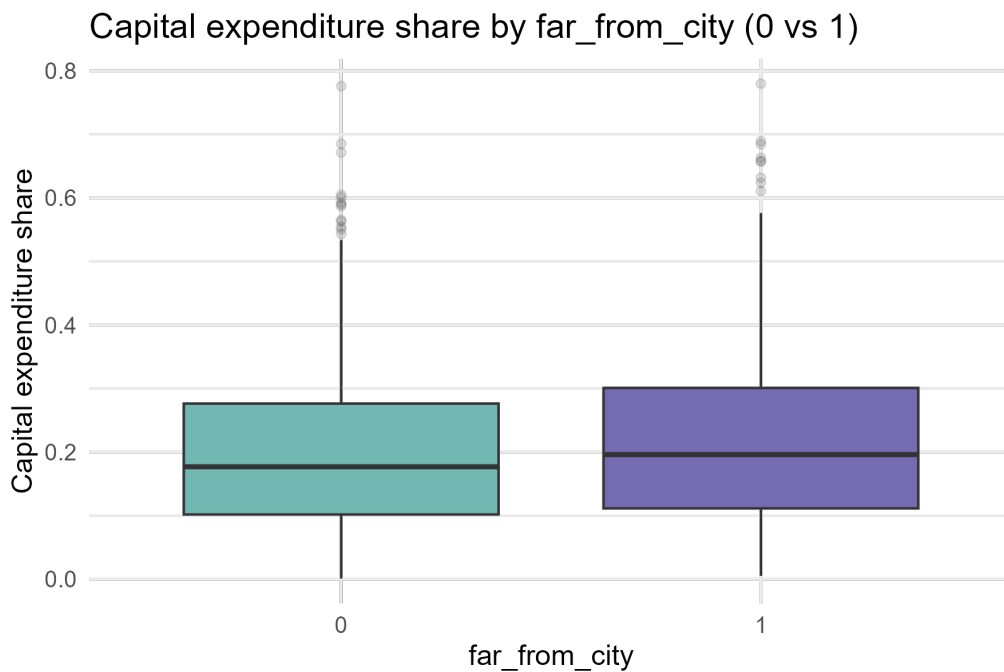


Figure A.5: Distribution of the percentage of capital expenditures by distance from the nearest city. The dummy equals one for municipalities classified as far from a major city.

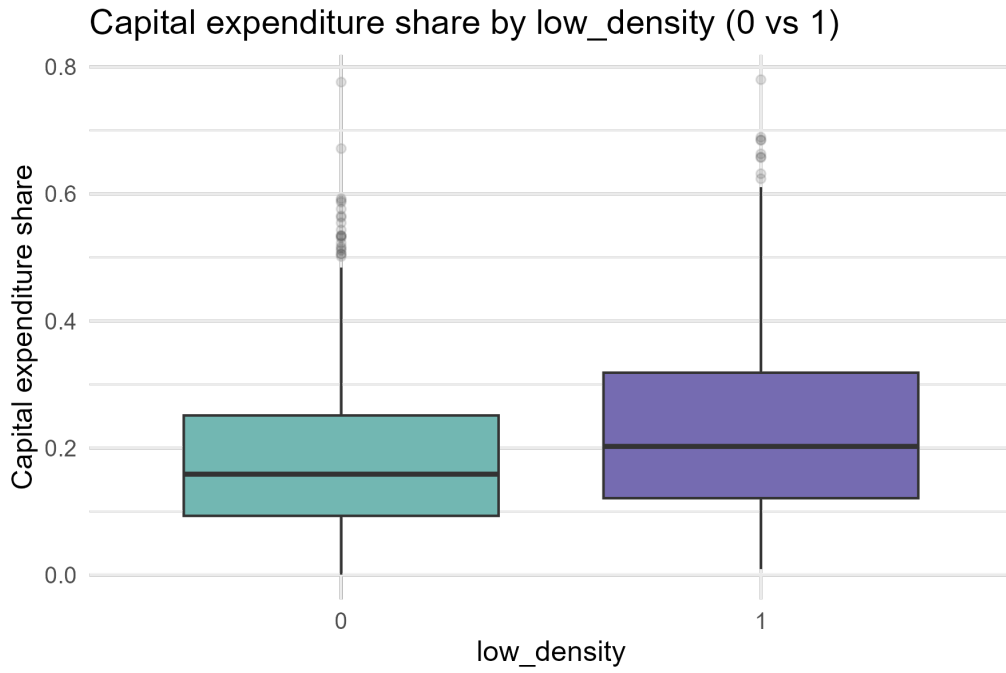


Figure A.6: Distribution of the percentage of capital expenditures by the indicator for low population density. A value of one denotes municipalities with population density below the median.

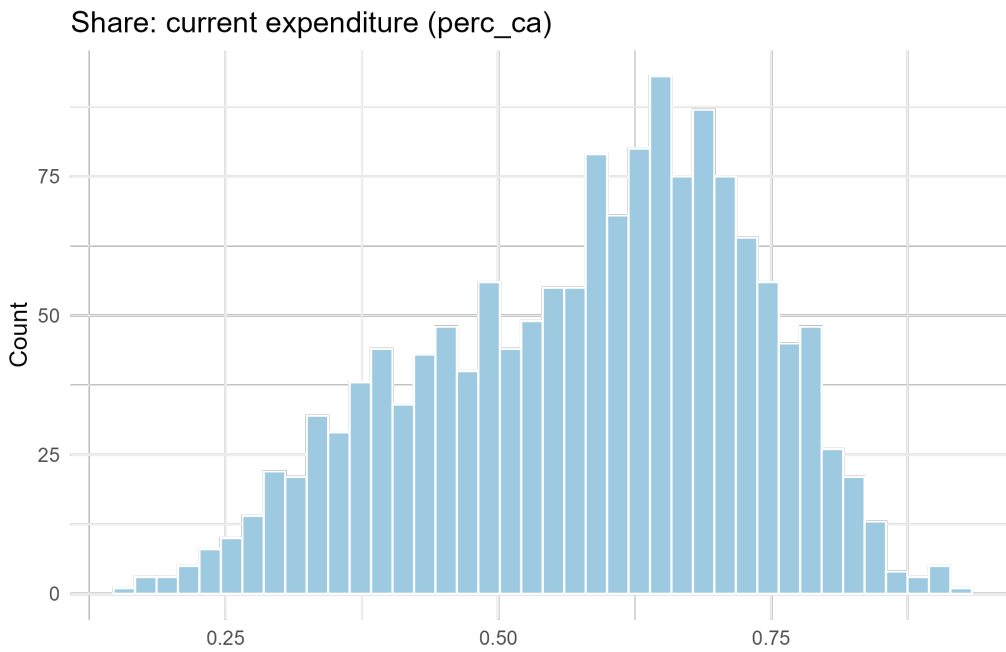


Figure A.7: Histogram of the percentage of current expenditures. The figure shows the distribution of the share of current expenditures across the sample.

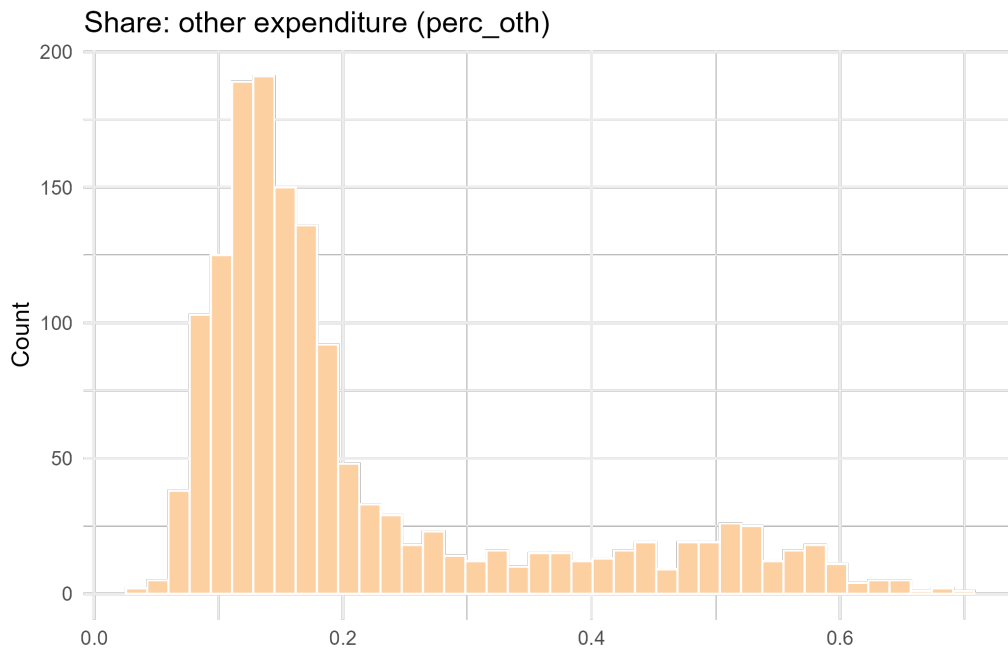


Figure A.8: Histogram of the percentage of other expenditures. The figure shows the distribution of the share of expenditure categories not classified as capital nor current.

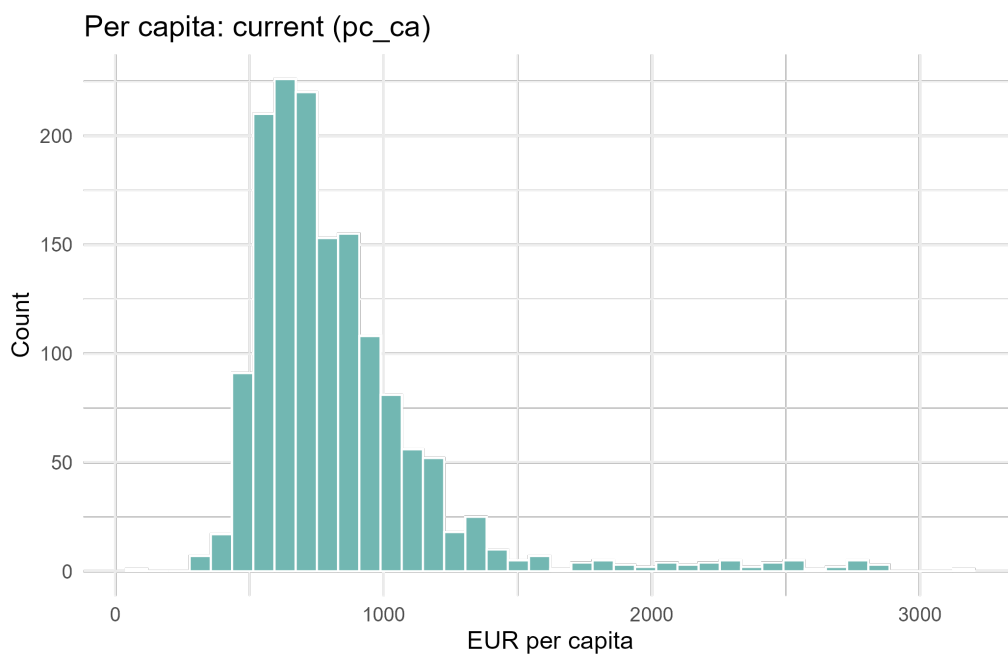


Figure A.9: Histogram of per-capita current expenditures. The figure shows the distribution of current expenditures per capita across the sample.

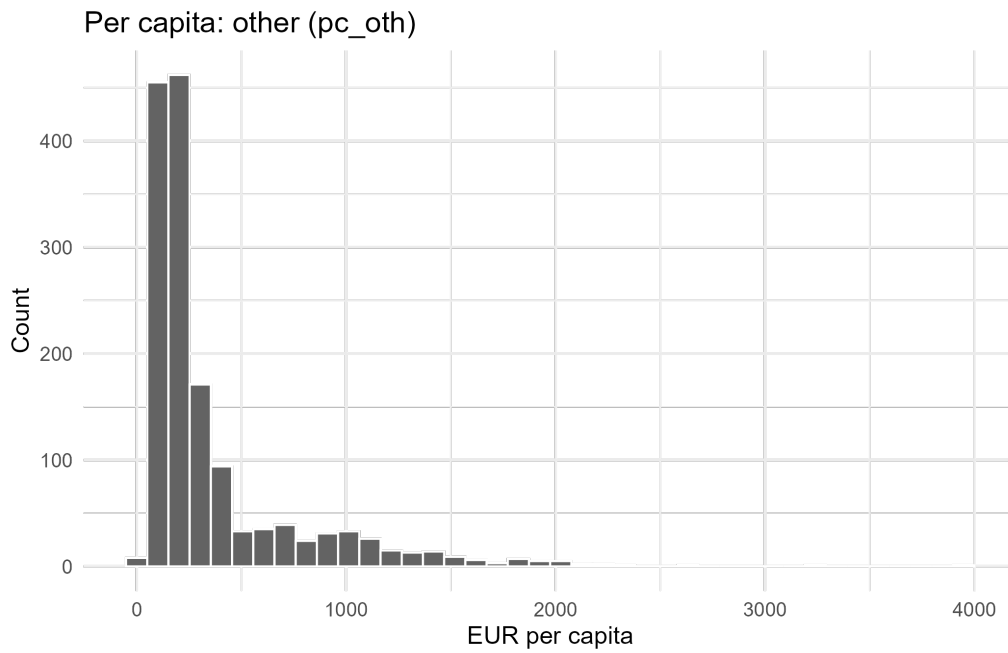


Figure A.10: Histogram of per-capita other expenditures. The figure shows the distribution of other expenditures per capita across the sample.

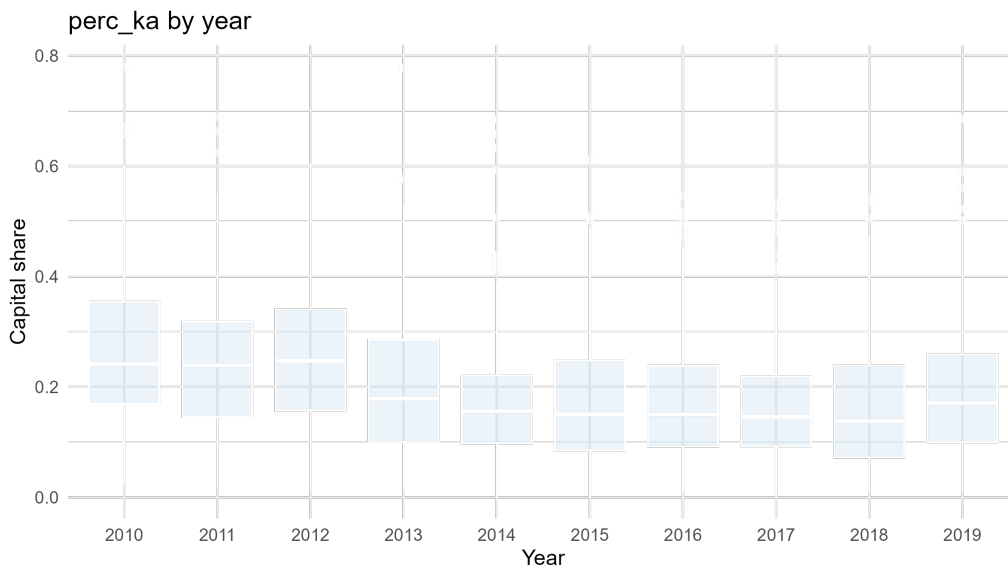


Figure A.11: Yearly distribution of the percentage of capital expenditures, 2010–2019. Boxplots summarize the distribution in each year, highlighting a general decline over time, particularly after 2012 (probably as a consequence of Fiscal Compact)

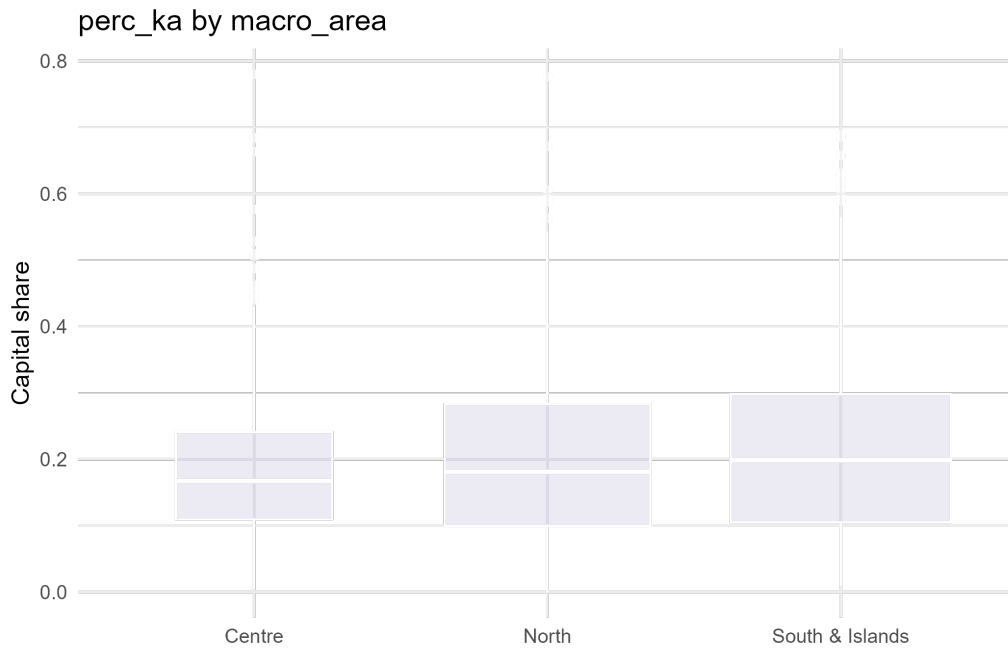


Figure A.12: Distribution of the percentage of capital expenditures by macro-area in increasing order.

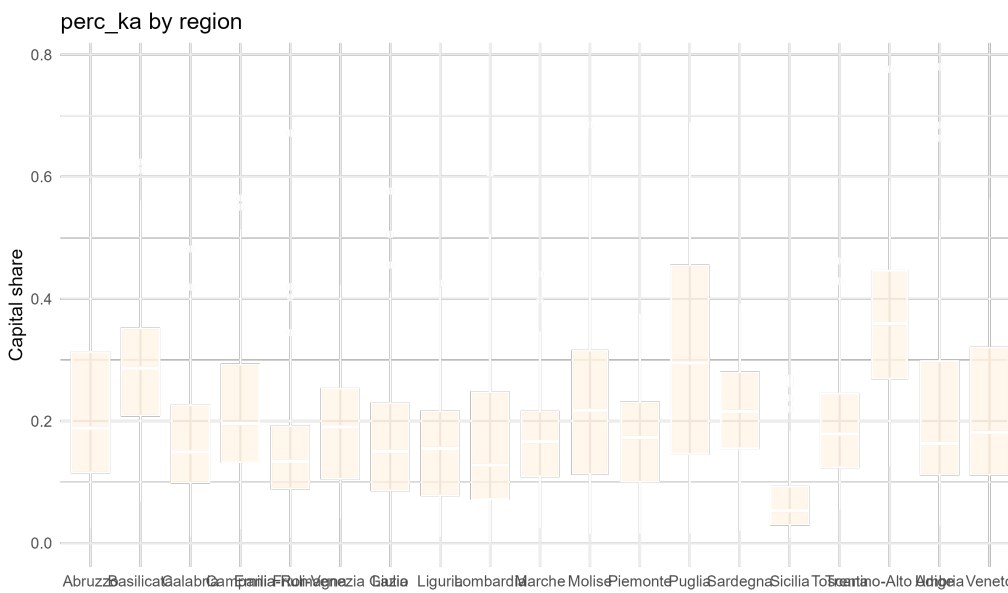


Figure A.13: Distribution of the percentage of capital expenditures by Italian region. Each boxplot summarises the distribution for one region. Region names appear on the horizontal axis.

# Appendix B: Tables

Table B.1: Summary of the sample used in the descriptive analysis.

Metric	Value
Observations	1 497
Years (unique)	10
Years range	2010–2019
Municipalities (MEF)	150
Municipalities (ISTAT)	150
Municipalities (name)	150

Table B.2: Distribution of observations by region.

Region	Observations
Abruzzo	80
Basilicata	80
Calabria	80
Campania	80
Emilia–Romagna	80
Friuli–Venezia Giulia	80
Lazio	80
Liguria	80
Lombardia	80
Marche	77
Molise	70
Piemonte	80
Puglia	80
Sardegna	80
Sicilia	80
Toscana	80
Trentino–Alto Adige	70
Umbria	80
Veneto	80

Table B.3: Median percentage of capital expenditures by province code. The table lists all provinces ranked in descending order of the median share of capital expenditures. “Observations” reports the number of municipality–year observations in each province.

Province code	Median perc_KA	Observations
14	0.418565	10
71	0.404807	40
54	0.373131	20
22	0.359339	70
50	0.333348	10
8	0.323777	10
26	0.322839	20
76	0.286564	80
106	0.259923	10
67	0.256282	30
95	0.255733	10
65	0.249488	40
68	0.248489	20
42	0.246221	10
98	0.244449	10
45	0.242672	10
30	0.231498	20
94	0.225541	30
33	0.220467	20
6	0.217975	20
91	0.217109	10
75	0.215942	40
62	0.214104	10
92	0.211612	20
107	0.209469	10
70	0.208744	40
80	0.206706	10
44	0.199622	20
4	0.195798	10
103	0.192195	10
47	0.191969	10
90	0.184732	10
93	0.183433	30
11	0.177408	10

Province code	Median perc_KA	Observations
56	0.176320	20
29	0.175523	10
52	0.173110	20
43	0.171859	10
24	0.168548	10
64	0.164588	20
53	0.162749	10
31	0.161246	30
60	0.159231	30
104	0.157968	10
55	0.148160	60
41	0.147868	17
13	0.147003	10
79	0.145120	40
25	0.135986	10
78	0.135919	30
57	0.133693	10
38	0.132206	20
9	0.132194	20
58	0.130535	20
28	0.129015	20
109	0.128776	20
10	0.125037	40
49	0.123554	20
69	0.123311	30
99	0.123033	10
61	0.122067	10
40	0.119833	10
15	0.117026	20
96	0.116425	20
37	0.110834	10
19	0.106868	20
23	0.101850	10
35	0.097406	10
87	0.089343	10
1	0.081173	20
83	0.075482	10

Province code	Median perc_KA	Observations
12	0.071627	10
86	0.069980	10
85	0.060615	10
82	0.048907	20
88	0.038435	10
84	0.019707	10

Table B.4: Median percentage of capital expenditures by municipality. The table lists all municipalities ranked in descending order of the median share of capital expenditures. “Observations” reports the number of year observations for each municipality.

Municipality	Median perc_KA	Observations
sellano	0.498986	10
denno	0.494670	10
roseto valfortore	0.493672	10
nemoli	0.477117	10
anzano di puglia	0.448148	10
storo	0.435286	10
castello tesino	0.420488	10
bianzone	0.418565	10
trambileno	0.397368	10
tiggiano	0.384122	10
celenza valfortore	0.376679	10
torre de' passeri	0.374509	10
revine lago	0.357999	10
cermignano	0.353073	10
san severino lucano	0.346247	10
castelnuovo di val di cecina	0.333348	10
castellaro	0.323777	10
san pietro al tanagro	0.317792	10
sant'elia a pianisi	0.316129	10
ruvo del monte	0.304660	10
colliano	0.304447	10
ferentillo	0.301936	10
aldeno	0.294367	10
balvano	0.293752	10
trentinara	0.290097	10

Municipality	Median perc_KA	Observations
rionero sannitico	0.280536	10
caldonazzo	0.279066	10
anzi	0.277399	10
squillace	0.267509	10
canazei	0.263888	10
barumini	0.259923	10
bagnaria arsa	0.255861	10
busachi	0.255733	10
san marcello	0.246221	10
bertonico	0.244449	10
zeri	0.242672	10
latronico	0.241866	10
pesche	0.237228	10
grottolella	0.230692	10
castiglione messer raimondo	0.227492	10
cordovado	0.227084	10
ozzano monferrato	0.223622	10
morfasso	0.220467	10
samatzai	0.219765	10
caorso	0.219317	10
orani	0.217109	10
san nicola manfredi	0.214104	10
vallermosa	0.211612	10
nuxis	0.209469	10
quargnento	0.208800	10
colli del tronto	0.207397	10
san giorgio morgeto	0.206706	10
sagrado	0.204140	10
castelgrande	0.204091	10
caschia	0.202877	10
sesto campano	0.201135	10
vo'	0.199889	10
cannole	0.198072	10
torriglia	0.196198	10
faleria	0.195967	10
meduno	0.195958	10
cervere	0.195798	10

Municipality	Median perc_KA	Observations
arcade	0.194939	10
masera	0.192195	10
marliana	0.191969	10
jelsi	0.190929	10
basciano	0.188313	10
staletti'	0.186261	10
bonorva	0.184732	10
murlo	0.182373	10
castelcivita	0.181405	10
massignano	0.179659	10
giuliano di roma	0.178254	10
calice al cornoviglio	0.177408	10
melara	0.175523	10
piancastagnaio	0.173110	10
fuminata	0.171859	10
atina	0.171697	10
santa maria del cedro	0.171592	10
bargagli	0.169318	10
poiana maggiore	0.168548	10
san giacomo degli schiavoni	0.166198	10
orciano di pesaro	0.164000	7
cinigiano	0.162749	10
pietragalla	0.159218	10
calangianus	0.157968	10
specchia	0.156735	10
stroncone	0.155151	10
san venanzo	0.154005	10
bertiolo	0.153870	10
goro	0.151809	10
robecchetto con induno	0.151061	10
san lorenzo nuovo	0.149995	10
dovera	0.149186	10
bergeggi	0.148690	10
nocciano	0.148667	10
vicovaro	0.147542	10
caslino d'erba	0.147003	10
giove	0.145347	10

Municipality	Median perc_KA	Observations
cerchiara di calabria	0.144739	10
roccamontepiano	0.144650	10
monterubbiano	0.138949	10
cerrione	0.138775	10
secli'	0.137482	10
feroleto antico	0.136297	10
trichiana	0.135986	10
pescorocchiano	0.133693	10
marciana	0.127691	10
mariano del friuli	0.125210	10
sant'ippolito	0.125012	10
mazzano romano	0.124111	10
frisa	0.123311	10
falerone	0.123292	10
sant'agata feltria	0.123033	10
rodi garganico	0.122683	10
caianello	0.122067	10
marcellinara	0.121847	10
verghereto	0.119833	10
falconara albanese	0.115684	10
arrone	0.115626	10
voghiera	0.115590	10
capoliveri	0.112582	10
monghidoro	0.110834	10
san quirino	0.110766	10
boffalora sopra ticino	0.105613	10
sorga'	0.101850	10
urbana	0.101398	10
caprie	0.100335	10
rotondi	0.099810	10
toano	0.097406	10
boissano	0.097064	10
farra d'isonzo	0.095286	10
maniace	0.089343	10
sagliano micca	0.085473	10
palombaro	0.083768	10
busso	0.083547	10

Municipality	Median perc_KA	Observations
guarcino	0.080975	10
porano	0.079519	10
bogliasco	0.076148	10
caronia	0.075482	10
annicco	0.074123	10
pieve ligure	0.072815	10
leggiuno	0.071627	10
nissoria	0.069980	10
marianopoli	0.060615	10
villar focchiardo	0.057258	10
ventimiglia di sicilia	0.053927	10
villafрати	0.044745	10
monterosso almo	0.038435	10
santa elisabetta	0.019707	10

Table B.5: Reasons for the termination of office. The table reports the number of municipalities facing each type of termination of office.

Status of office termination	Observations
Natural expiry of term	1414
Resignation of half plus one councillors	24
Death	8
Failure to adopt budget rebalancing measures	8
Failure to approve budget	8
Resignation	7
Infiltration / influence of organised crime	7
Council reduction due to inability to substitute	6
Total transfer of population	5
Mayor resignation	5
Other causes of loss of elected office or revocation of appointment	4
Unknown	1

Table B.6: Frequency of mayors by number of consecutive mandates. “Observations” reports the number of mayoral terms.

Number of consecutive mandates	Observations
1	939
2	508
3	49
Unknown	1

Table B.7: Re-eligibility status of mayors. The table counts the number of observations where a mayor is eligible or not eligible for another term.

Re-eligible	Observations
0	429
1	1067
Unknown	1

Table B.8: Summary statistics for the per-capita capital expenditures by pre-election year, after trimming the top and bottom one percent of the distribution. Results are shown separately for pre-election years and non-pre-election years.

Pre-election year	Observations	Mean PC_KA	Median PC_KA
No	905	339.66	234.26
Yes	561	311.68	230.46

Table B.9: Two-sample t-test comparing the mean per-capita capital expenditures between pre-election years and non-pre-election years (after trimming the top and bottom one percent of observations).

t-statistic	p-value	Mean (No)	Mean (Yes)
1.788	0.074	339.66	311.68

Table B.10: Summary statistics for the demeaned percentage of capital expenditures by pre-election year. The demeaned variable is calculated by subtracting, for each year, the national mean share of capital expenditures from each municipality's value. Positive numbers indicate above-average capital expenditure in that year, negative numbers below-average. Results show that deviations are close to zero and do not differ systematically between pre-election and non-pre-election years.

Pre-election year	Mean demeaned perc_KA	Median demeaned perc_KA
No	-0.0016	-0.0283
Yes	0.0026	-0.0203