

UNIVERSITÀ DEGLI STUDI DI BERGAMO
E UNIVERSITÀ DEGLI STUDI DI PAVIA
Ph.D. in Applied Economics and Finance Cycle XXXVIII

Essays on Local Government Economics

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Abstract

This dissertation addresses the principal-agent conflict in Italian local government by empirically investigating how mayoral heterogeneity, institutional fiscal constraints, and electoral dynamics shape local public policy outcomes. Chapter 1 examines the influence of the mayor's generational affiliation on the composition of local public expenditure. Results indicate that mayors from older generations allocate larger share of current expenditure to environmental services. This higher allocation is empirically linked to increased costs in environment management, particularly in areas like waste disposal services. Chapter 2 analyzes the impact of central intergovernmental transfers on local government spending behavior, focusing on the Italian context. The chapter provides an extensive review on the literature related to the flypaper effect, and confirms results presented in the literature with an empirical exercise which exploits an instrumental variable derived from a peculiar reform of Italian municipal transfers from the central government. Chapter 3 explores a non-partisan explanation for the incumbency advantage by studying the electoral impact of high-visibility community events. Utilizing the quasi-random timing of the Patron Saint Day celebration relative to municipal elections, the study finds a significant increase in the incumbent mayor's re-election probability. This effect suggests that the successful execution of such events serves as a signal of administrative competence to the electorate.

Introduction

The study of local government economics holds a vital position within the broader fields of public finance and political economy. Over the past several decades, a prominent political and administrative trend has favored decentralization, resulting in the transfer of substantive fiscal and administrative authority from central to sub-national units across many democracies (Hooghe et al., 2010). However, rather than a universal or strictly linear paradigm shift, the extent and durability of this process have varied significantly across countries and time periods. Indeed, the initial waves of decentralization have frequently been met with periods of recentralization or the imposition of strict top-down fiscal constraints, often triggered by macroeconomic crises or the need for national fiscal consolidation (Rodden, 2003; Faguet, 2014). Despite these complex trajectories, this movement—often justified by the principles of fiscal federalism, chiefly the efficiency gains associated with tailoring public goods provision to heterogeneous local preferences (Tiebout, 1956; Oates, 2008)—has firmly established the municipal government as a primary locus for policy implementation and resource management. Consequently, the actions of local political leaders, most notably the municipal mayor, have come under intense scrutiny. The decisions made at this level—spanning budgeting, local taxation, public service provision, and infrastructure development—directly influence citizen welfare, fiscal sustainability, and democratic trust. The fundamental goal of local government economics, therefore, is to move beyond the idealized model of the benevolent local agent and investigate the complex web of political, fiscal, and personal incentives that shape real-world mayoral behavior and policy outcomes.

Despite the theoretical promise of decentralized governance, its practical implementation is consistently challenged by institutional rigidities and, most critically, a pervasive and multifaceted principal-agent problem (Besley, 2006). The mayor,

acting as the agent, is formally mandated to maximize the welfare of their local constituency (the principal). However, the agent's own objectives—including career progression, ideological purity, rent-seeking opportunities, and, crucially, reelection—often create powerful incentives that diverge from the efficiency mandate. While classic fiscal federalism posits that local governments possess an informational advantage over central planners regarding citizen preferences, the principal-agent relationship at the municipal level often suffers from severe informational deficits compared to national politics. These conflicts are particularly pronounced in the local sphere, which frequently operates as a low-information electoral environment characterized by reduced media scrutiny (Snyder Jr and Strömberg, 2010) and a reliance on national heuristics rather than local party accountability (Schleicher, 2007). This specific type of informational asymmetry—where voters lack the detailed information necessary to perfectly monitor local administrative actions—provides mayors with significant insulation against perfect democratic control. Understanding the deviations from the theoretical efficiency benchmark requires isolating the factors that grant local leaders discretionary power over the public purse and public policy. The existing literature suggests that these deviations manifest across a broad spectrum of determinants, encompassing institutional rules, economic constraints, and the personal characteristics of the political elite.

Against this complex backdrop, this dissertation focuses on three critical areas of inquiry that significantly influence the exercise of mayoral discretion and the subsequent policy choices in a decentralized environment: the politician's personal heterogeneity, the impact of institutional fiscal constraints, and the exploitation of electoral opportunities. These three areas are not exhaustive of all forces acting on local governments, but they represent significant channels through which the principal-agent conflict is played out, and they form the empirical core of this thesis.

The first area of investigation revolves around the heterogeneity of political leaders themselves. While economic models often simplify politicians into homo-

geneous maximizers, empirical reality shows that individual traits act as powerful non-pecuniary determinants of policy. A growing body of work finds that personal characteristics—such as gender, professional experience, or education—serve as proxies for unique preferences, political priorities, and ideological leanings that translate into observable differences in public spending (Alesina et al., 2019). This essay contributes by extending this analysis to generational affiliation. Generational effects move beyond simple age effects, acting as a lens that captures distinct long-term value systems, risk tolerance, and political attitudes forged by shared historical and socio-economic experiences (Vlandas et al., 2021). The foundational experiences of a political cohort may dictate intrinsic priorities—from fiscal management styles to environmental sensitivity—leading to persistent and quantifiable differences in budgetary choices. This line of inquiry is particularly salient in policy areas like environmental management, where the benefits often accrue across long time horizons and disproportionately affect future generations, creating a challenge for short-term political incentives. Chapter 1 addresses this by exploring the correlation between a mayor’s generational background and the efficiency of environmental public spending. The chapter finds that older-generation mayors are systematically associated with a higher share of environmental-related expenditure, a pattern that appears to be driven by lower efficiency and higher costs in critical service areas.

The second area concerns the pervasive influence of institutional design and inter-governmental fiscal relations. The structure of fiscal federalism aims to balance local autonomy with national objectives of equity and macro stability, but it inherently introduces complex constraints and dependencies (Breton, 1998). Local governments, rarely fiscally autonomous, rely heavily on central transfers to manage vertical and horizontal fiscal imbalances. While necessary, this dependence can fundamentally alter local incentives and erode the direct link between local taxation and spending decisions. The "flypaper effect," which shows that intergovernmental grants stimulate local public spending more strongly than an equivalent increase in local private

income (Hines Jr and Thaler, 1995; Inman, 2009), remains the central empirical puzzle. Its persistence suggests that transfers weaken local fiscal discipline. However, a complete understanding requires analyzing not just expenditure levels, but also budget composition. Do non-local revenues simply augment existing services, or do they incentivize politicians to divert funds towards politically expedient spending at the expense of necessary, less visible services? Rigorously identifying these non-crowding-out effects and their resulting policy distortions remains methodologically demanding due to the endogeneity of transfers (Bracco et al., 2015). Chapter 2 engages this debate. The chapter provides an extensive review on the literature related to the relationships between central transfer and local governments behavior. Additionally, it provides an empirical exercise, exploiting a major Italian fiscal reform to isolate the effect of central funding. The chapter confirms the empirical relevance of the flypaper effect and provides evidence that the inflow of externally sourced funds shifts municipal priorities toward different policy areas.

The third area of complexity addresses electoral accountability and the strategic demands of the incumbency advantage. Local political markets often exhibit low voter information, which perversely amplifies the electoral advantage enjoyed by incumbents (Trounstein, 2011). This edge is typically attributed to observable factors like name recognition (Kam and Zechmeister, 2013), strategic manipulation of the budget cycle (Alesina and Paradisi, 2017), or signaling administrative competence (valence) (Fowler, 2018). Beyond routine policy and budget manipulation, accountability is also swayed by non-policy events—factors that are external to the mayor’s legislative agenda but profoundly impact the local mood or community sentiment in the run-up to an election. These symbolic or community-building events can be strategically leveraged by incumbents to convert communal social capital and enhanced personal visibility into measurable political consensus. Chapter 3 provides insights into this mechanism. By exploiting the quasi-random timing of fixed-date Patron Saint Day celebrations relative to municipal election dates, the chapter in-

vestigates a specific channel for the incumbency advantage. The empirical evidence suggests that incumbent mayors see a robust increase in their re-election probability when the celebration occurs in close temporal proximity to the vote. This result supports the hypothesis that the successful execution of high-visibility community events serves as a valuable, non-partisan signal of administrative competence, translating short-term positive sentiment into tangible electoral gains.

In conclusion, the three chapters of this dissertation offer an integrated empirical perspective on the behavior of local political agents in Italy. By systematically dissecting how mayors navigate the pressures stemming from their personal history, the institutional rules governing finance, and the strategic demands of the electoral cycle, this work contributes to the existing literature by moving beyond the aggregated analysis of decentralization to explore the micro-foundations of local policy choice and accountability.

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Chapter 1: Politician's generation and public expenditure for the environment. Evidence from local government ^{*}

1 Introduction

The phenomenon of population ageing is widely regarded as one of the main challenges faced by contemporary societies. Extensive literature has analysed its consequences on several economic outcomes. However, a dimension that has received relatively less attention in the economic literature is the extent to which it affects the age composition of political leaders and, in turn, public policy making.

Data on the age of politicians across countries indicates that political arenas are ageing at a faster rate than the general population. Older individuals largely dominate politics worldwide. As of 2024, the median age of heads of government across 187 countries was 62 years old.^[1] The increasing dominance of the elderly

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¹Source: Pew Research Center, <https://www.pewresearch.org/short-reads/2024/05/01/as-biden-and-trump-look-for-reelection-who-are-the-oldest-and-youngest-current-world-leaders/>

²In Italy, at the local level, the average age of a mayor in 1990 was 46, whereas in 2025, it was 53. A similar trend is observed for members of municipal councils and government, whose average age increased from 42 in 1990 to 54 in 2025. Source: own calculation using the Italian registry of local politicians from the Italian Ministry of Interior

in politics has led someone to hypothesise the introduction of an age limit or youth quotas³ with some countries already experimenting with the latter⁴. The concern is that the age composition of the political body is unlikely to be neutral in terms of policy choices. Politicians that are older than the population they govern are less likely to embrace policies matching political preferences of a younger electorate. This is supported by large evidence showing how older individuals hold different perspectives and preferences over policies (Vlandas et al., 2021; Busemeyer and Lober, 2020; Rattsø and Sørensen, 2010; Grasso et al., 2019; Bello and Galasso, 2021). Therefore, older politicians might tend to increase public spending towards the elderly (Curry and Haydon, 2018) and reduce the amount dedicated to long-term policies which predominantly benefit younger generations (McClean, 2021), such as education and environmental protection.

This paper is related to the recent and growing literature focusing on local governments and analysing whether politicians' age matters for public expenditure choices. These studies analyse, among else, alignment of politicians' preferences with their age cohort of belonging (Bailer et al., 2022), differences in engagement with the budget cycle (Alesina et al., 2019), or politicians' age-related preferences with respect to childcare and public education (Baskaran et al., 2024; McClean, 2021). These relationship have been studied measuring nominal age of politicians or employing small age cohorts to account for non-linearity of age. Unlike these studies, we focus on environmental spending and exploit differences in public spending across mayors belonging to different generational cohorts. The generation of belonging is a key factor that affects both political preferences and in-

³See for example Berry (2012)

⁴Such as, among others, Kenya, Sri Lanka, Rwanda or Tunisia

dividual characteristics of politicians (Edmunds and Turner, 2002), consequently shaping their public budget choices.

Existing literature suggests that younger generations have a larger inclination to express heightened concerns for the environment ⁵. Consequently, examining public expenditure on environmental issues becomes a valuable lens through which it is possible to understand whether and how differences in public expenditure choices exist between older and younger politicians both operating in relatively older and younger constituencies.

Our empirical setting is based on Italian municipal governments for which we obtain data on elected and candidate politicians as well as on balance-sheets information including expenditure commitments on different policy sectors (from social services, to local police, local roads and transport, education, and environmental issues, among others). An interesting aspect of Italian municipalities, reinforcing our emphasis on environmental issues, is that the largest per-capita expenditure is allocated to this policy sector (following administration function expenditure) among all sectors.

In order to identify the effect of politicians generation on local public expenditures, our identification strategy relies on exploiting a natural quasi-experimental design provided in the context of democratic elections. When a candidate wins an election with a close margin relative to the second contestant, differences in potential outcomes between the winner and the loser are assumed to be as good as randomly distributed (Lee et al., 2004). By narrowing the sample around closely contested electoral competitions between candidates of different generations, we control for confounding factors affecting budget outcomes, allowing us

⁵See for example Ross and Rouse (2022)

to specifically identify the causal effect of age. We tested our hypothesis employing a model with year and province Fixed-Effects, focusing around three different and increasing margins of victory between the first and second candidates and confirm the robustness of our results in a RDD setting.

We find that municipalities with older mayors allocate a larger share of their budget to environmental expenditures. Further analysis reveals that this effect is primarily driven by the provision of specific services, namely waste disposal and local water management. Specifically, our results show that cities led by older mayors produce more unsorted waste per capita, inherently requiring higher funding for its disposal. Given this evidence, one possible explanation is that older administrators enact less effective policies for controlling local waste production, thereby driving up current environmental spending. Moreover, a heterogeneity analysis reveals that this treatment effect is concentrated in municipalities with a demographic profile younger than the sample median. We interpret this result as evidence of policy responsiveness: younger mayors appear more attuned to the pro-environmental preferences of a younger electorate, successfully implementing waste reduction strategies that keep costs down. This responsiveness creates a stark contrast with the policies of older administrators in similar demographics, widening the expenditure gap. Consistent with this framework, the differential dissipates in towns with an older population structure, where environmental issues may be less politically salient and the electoral pressure for such policies is weaker.

This paper is organised as follows. Section 2 reviews the related literature. Section 3 presents the institutional setting and describes the data. Section 4 discusses our empirical analysis. Section 5 presents the results of the main analysis. Section 6 discusses several robustness checks and the channels behind the results.

Finally, Section 7 concludes.

2 Literature review

Our primary goal is to explore how the generation of politicians influences political preferences and outcomes. The issue of youth under-representation in politics is well-recognised, and the impact of population ageing and generational effects on political preferences has garnered substantial attention.⁶ However, it's crucial to note that politicians differ from the general population, facing unique incentives.⁷ Existing evidence specifically addressing the connection between the age, or generation, of politicians and the economic effects of their policies is limited. This section offers an overview of the current literature on the relationship between age, generations and politicians' preferences.

Contemporary economic theory suggests there might be a significant connection. Leisure time during retirement is universally regarded as a "merit good" across all age cohorts (Profeta, 2002; Galasso and Profeta, 2002). In countries with unfunded social insurance systems, commonly known as pay-as-you-go systems, governments engage in income redistribution from younger and middle-aged workers to pensioners. Consequently, labour income taxes introduce distortions that incentivise older workers to retire as early as feasible to partake in retirement leisure (Mulligan and Sala-i Martin, 2003). At the same time, older individuals are anticipated to manifest a relatively "single-minded" orientation in contrast to other generational cohorts (Mulligan and Sala-i Martin, 2003; Profeta, 2002). The combination of these two characteristics significantly augments the effectiveness

⁶Stockemer and Sundström (2025); Sevi (2021); Bhatti et al. (2012)

⁷Phillips (1998); Browning (1975) Black (1948)

of their political claims. Consequently, as older individuals may primarily seek to support policies that yield personal benefits (Svallfors, 2008) and may elicit reduced effort in politics as they near the conclusion of their careers (Bertrand et al., 2020), the amplified and ever-growing voting power of the elderly can potentially have substantial implications for the long-term financial sustainability of an economy (Galasso and Profeta, 2002).

The existing literature pertaining to the behaviour of both young and senior politicians, closely aligned with our research questions of interest, provides mixed evidence. Dobbs (2022) evaluates the effects of the age of politicians on voter turnout of young constituents as well as on their level of trust towards public institutions by exploiting the introduction of youth quotas in only some African nations. His paper documents the absence of statistically significant disparities between countries that had adopted youth quotas and those that had not. According to his results, the presence of youth quotas did not lead to a heightened interest in addressing the priorities of younger generations. Bailer et al. (2022) investigated the political engagement of German parliamentarians by quantifying the volume of parliamentary questions pertaining to their respective domains. Their study supports the prediction that younger politicians initially exhibit a degree of alignment with the interests of their age cohort at the outset of their careers; however, this alignment gradually diminishes as politicians accumulate more experience. Curry and Haydon (2018) observed that senior members of the United States Congress during the 109th and 110th Congresses exhibited a greater propensity to introduce bills pertaining to "less salient" issues affecting the elderly demographic. Conversely, senior issues that garnered heightened public attention were favoured on the basis of each politician's supporting electorate

preferences rather than individual characteristics. [Alesina et al. \(2019\)](#) conducted a comprehensive study on the causal impact of age on mayors' decision-making processes throughout the various stages of the political budget cycle within Italian municipalities from 1998 to 2014. Their findings indicate that younger mayors exhibit a higher propensity to engage in the budget cycle as upcoming elections draw nearer, and are more likely to secure re-election and ascend the political hierarchy. On the other hand, while younger politicians tend to prioritize long-term strategies that align with the reasonable interests of their age groups, they may also choose to accommodate the preferences of the median voter if there is an incentive for re-election. When the median voter leans towards older age groups, these two effects pull in contrasting directions. [Alesina et al. \(2019\)](#) provide evidence supporting the prevalence of the median-voter effect. [Baskaran et al. \(2024\)](#) examined the relationship between the proportion of younger politicians in local governments across municipalities in Bavaria and tax expenditures allocated to childcare and schools. The results revealed a positive and statistically significant effect, suggesting that younger politicians exhibit a greater propensity to allocate a larger portion of the budget towards public goods that predominantly benefit younger generations. Evidence of a significant influence of the age of politicians on public spending is also provided in [McClean \(2021\)](#), who utilising a regression discontinuity design (RDD) focused on the proximity to the close-election threshold in Japan, examines the causal relationship between the age of winning candidates and public expenditure. The results of the study suggests that younger politicians exhibit a propensity to increase spending in areas such as childcare support and public education. In contrast, older politicians in his sample do not demonstrate a clear preference for expenditures related to the elderly, but

rather display a greater inclination towards short-term spending as opposed to long-term strategies. [Altindag and Mocan \(2019\)](#) by exploiting a quasi-random seat allocation mechanism in India's local elections spanning from 1991 to 2011, analyse differences in the likelihood of party-switching across politicians in different age groups. The study suggests that younger politicians were more prone to switching parties in situations characterized by political uncertainty. [Vlandas \(2018\)](#) applied a fixed-effects model to analyse the political party manifestos of 21 distinct countries spanning a 50-year period. The analysis yielded evidence indicating that political entities tend to exhibit a predilection for "Economic Orthodoxy," characterised by policies aimed at maintaining low levels of inflation and relative price stability, in countries characterised by a higher proportion of older individuals.

In general, the existing literature elucidating the ramifications of politicians' ageing appears to exhibit a limitation in terms of the scope of outcomes considered. For instance, to better understand the causal relationship between age and public economic outcomes, it would be required to ascertain how younger politicians differ in their stances on matters such as environmental policies, social protection, justice, or public debt management. Moreover, politicians at similar stage of their life might behave differently depending on the age composition of their constituency. Furthermore, to the best of our knowledge, no research has been published that investigates the connection between politicians and their respective generational cohorts. Our ultimate objective is to address this lacuna by broadening the spectrum of public expenditures under scrutiny, with a specific focus on environmentally-related expenditures, by analysing the effect of generational cohorts in politics, and by focusing our analysis on understanding

the behavioural differences of young and old politicians in relation with their electorate's age.

3 Institutional framework and data

3.1 Institutional Context

In Italy, municipalities manage about ten percent of total public expenditure and are in charge of several public services. These include local public transport, environmental initiatives, social assistance programs, cultural activities, local police services, sports facilities, tourism promotion, among others. The mayor appoints members to the municipal government and holds responsibility for administering the municipality, managing its budget, and implementing policies. Municipal executive power rests with the *Giunta Comunale* (Municipal Executive), which is headed by the mayor. The primary financial responsibility of a municipality is the formulation of its budget. This process begins with the Giunta drafting the financial plan based on revenue projections and departmental needs. The draft budget is then submitted to the Municipal Council, the legislative and oversight body. The Council reviews the document, often via a dedicated Budget Committee, and is responsible for its final approval through a formal resolution. This approval legally binds the local government to the outlined financial strategy for the fiscal year. Furthermore, the Council exercises essential political control and oversight over the Giunta's financial activities and sets the general policy objectives the budget must adhere to. Crucially, while Italian Municipalities possess financial autonomy, it is not absolute. A significant portion of municipal spending

is mandated by the central State. The municipality is obligated to allocate funds for certain services, such as maintaining the civil registry or managing specific educational infrastructure, thereby acting as a local agent fulfilling national duties. These required expenditures are often supported, at least in part, by transfers from the central government, though the municipality's own revenues from local taxes, charges and fees also play a vital role.

In 1993, a law introduced a direct election of the mayor. In municipalities below 15,000 inhabitants the mayoral candidate who receives the highest number of votes is elected, while for larger municipalities a runoff election takes place in case none of the candidates obtains more than 50% of the votes in the first electoral round. On the ballots, each candidate to the mayor position is endorsed by one or coalition of more political parties or local civic lists (from which council members are elected).

Additionally, in 1999, the term of office for mayors in Italy was extended from four to five years. They are permitted to seek re-election for a second consecutive term but can only do so once, with a mandatory waiting period of at least five years before becoming eligible for re-election again.

3.2 Politicians' generations

In our analysis, a key operational choice consists in the definition of a proxy which would allow us to distinguish between municipal governments according to the generation of the managing politicians. It is worth noting that we rely on a binary measure, which defines municipal governments as old vs young, on the basis of

the mayor’s generation.⁸ The use of mayor’s age and the binary distinction between old vs young is in line with the previous literature.⁹ Our approach actually introduces a novel element as we define the old and young mayors on the basis of their generations instead of a simple age cut-off. The use of age, either as a continuous variable or through specific age-based thresholds, is motivated by varying perspectives on life horizons. Younger politicians, facing a longer life expectancy and often raising young children who will encounter severe environmental challenges, may possess greater incentives to prioritize long-term sustainability and environmental protection policies (McClean, 2021; Prato, 2018; Bisin et al., 2015). However, in addition to life-cycle effects, preferences are also shaped by generational cohorts (Burke et al., 2015; Lyons and Kuron, 2014). Current literature provides evidence that younger generations demonstrate a heightened concern for the environment (Ross and Rouse, 2022). While environmental protection rose to prominence in the late 1970s and 1980s, the issue has garnered significant attention primarily in recent years, following the exacerbation of climate change-related issues. Consequently, it is reasonable to expect a stronger commitment to these issues from younger generations, such as Generation X or Millennials, who have been educated on environmental stewardship since early childhood. The novel contribution of this paper is to focus on the differences in environmental expenditure specifically as they relate to the generational cohorts of politicians.

From the mayors’ age, we identify generational cohorts within our sample

⁸We implement this simplification because of the scarcity of politicians belonging to very old or very young generations (Silent Gen. and Millennials, respectively) in our sample of analysis. We discuss the implication of this approach in Section 6.3, in which we include a robustness check of our main results including all generation separately.

⁹For example, Alesina et al. (2019) defined older and younger mayors by comparing the age of the two main candidates at each elections; McClean (2021) used a 50 years old cut-off; Curry and Haydon (2018) adopted the definition of Seniors of US Medicaid, above 65 years old.

using commonly accepted definitions in the literature (Burke et al., 2015; Lyons and Kuron, 2014). Specifically, we identify four distinct generations within our sample of politicians: the Silent Generation, consisting of individuals born before 1945; the Baby-Boomers generation, with individuals born between 1945 and 1964; the Generation X, with individuals born between 1965 and 1979; and the Millennial generation, with individuals born between 1979 and 1994. Moreover, approximately 50 individuals belonged to the Greatest Generation (1901-1924), which we opted to classify within the Silent Generation cohort. To construct a dichotomous age variable, we define older politicians as those belonging to the Greatest, Silent, and Baby Boomer cohorts, whereas younger politicians comprise Generation X and Millennials.

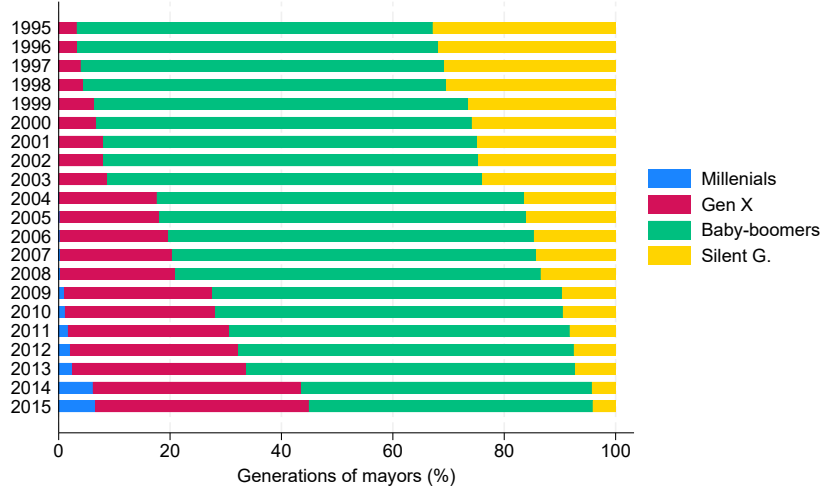
3.3 Data and Descriptive Statistics

3.3.1 Data

Our main dataset integrates municipal budget data with administrative records on local elections and elected officials for the 2004-2015 period. We exclude the 2000-2004 interval from the main analysis due to the scarcity of younger-generation mayors during those years. Given that our identification strategy relies on comparing expenditures across generations, it is necessary to restrict the analysis to a time-frame exhibiting sufficient variation in treatment status. Figure 1 illustrates the temporal evolution of the share of mayors by generation. The data indicate that the presence of younger-generation mayors becomes substantial only starting in 2004, although the older generation remains dominant throughout

the entire period.^[10] Consequently, the limited number of observations for younger mayors prior to 2004 justifies this sample restriction.

Figure 1: Mayors by Generation



Notes. Own calculations on the distributions of mayors by generation.

We also excluded all years after 2015 because a reform active from 2016 onward changes budget reporting standards at municipal levels, making comparisons across these years unfeasible. The financial data comes from AIDA PA,^[11] which contains yearly information on municipal governments' expenditures (and revenues).^[12] Expenditures up to 2015 are classified in current and capital accounts, and information has a detailed breakdown by functions, such as environmental, transport, local police, education, culture or health issues, among others, which roughly correspond to the functions of interventions of the municipal administra-

¹⁰Notably, our dataset does not include politicians from Generation Z or Generation Alpha, as they were too young to held administrative positions.

¹¹A third-party company managed by Moody's. The company consolidates administrative budget data for local entities as provided by the Italian Ministry of Economy and Finance

¹²Monetary values are adjusted at 2015 levels using the consumer price index.

tion.¹³ We are primarily interested in the environmental-related expenditures.

Politicians' data on municipal elections come from the Italian Ministry of the Interior.¹⁴ This dataset comprises information on elected politicians, including their education level, political affiliation, years of experience.

We link these data to socioeconomic and demographic information at the municipal level coming from the Italian National Institute of Statistics.

Additionally, we collect information on local wastes' management: quantities and annual costs per capita related to the disposal of unsorted and sorted wastes. These information refers to the 2010-2015 period and are provided by the *Istituto Superiore per la Protezione e la Ricerca Ambientale* (ISPRA), an Italian public research centre controlled by the Italian Ministry of the Environment. Moreover, we included information on waste management related expenditures derived from *Soluzioni per il Sistema Economico* (SOSE), a public holding created by the Italian Ministry of Economy and Finance for public economic analysis. This company collected (and shared) information on different types of expenditures at municipal level in order to conduct specific functions deliberated by the central government, such as defining indexes of efficiency and computing standardized measurement of local expenditures need. Specifically, we leverage data on waste management expenditures to shed light on the channels through which the generational effect operates, as discussed in Section 6.5.

We exclude Italian regions governed by 'special statutes' from the analysis,

¹³Moreover, Italian municipal budget functions breaks down to two major categories: programs and macro-aggregates. Programs are function-specific and represent different services provided by municipalities. In case of environmental expenditures, for example, we observe expenditures dedicated to waste disposal or water distribution management. Macro-aggregates allow us to identify specific costs, such as those incurred for paying employee wages or for passive interest over debts, for each main function.

¹⁴Available at: <https://dait.interno.gov.it/elezioni/anagrafe-amministratori>

as they are subject to different regulatory frameworks regarding local budget reporting. Furthermore, we exclude municipalities that underwent territorial modifications during the observation period. Changes in administrative boundaries introduce shocks to the share of environmental expenditures, driven by shifts in the demand for services such as waste disposal, water management, and park maintenance. Moreover, we do not consider municipalities where the mayoral mandate was suspended or terminated prematurely due to legal infractions or the death of the mayor. In such cases, the municipality is typically administered by an appointed official rather than an elected mayor, preventing the observation of the leadership’s generation during the transition period.

Finally, we excluded those observations for which it was not possible to match name, date and place of birth of mayors due to misreporting in electoral data. Overall, we exclude 43,199 observations and 1570 municipalities from our sample (around 20%).

3.3.2 Descriptive Statistics

Table [1](#) provides a comprehensive overview of the descriptive statistics for the variables employed in the empirical analysis, spanning the period from 2004 to 2015 and comprising 66,923 observations.^{[15](#)} The primary dependent variable, environmental expenditure as a percentage of the total municipal budget, shows a mean value of 20.058%.^{[16](#)} The distribution of this share exhibits considerable dispersion, as indicated by a standard deviation of 8.681 percentage points. Specifically, while the bottom quartile of municipalities allocates less than 14.867% of

¹⁵In appendix, Table [A.1](#), we also show the averages of the other type of expenditures.

¹⁶Specifically, this expenditure category encompasses costs related to the management of parks, urban environment, waste disposal, water services, public buildings, and civil aid.

their resources to environmental protection, those in the 75th percentile dedicate nearly a quarter of their budget to this sector.

Regarding the key explanatory variables related to the generational profile of the executive, the data shows that 71.5% of the observations involve mayors belonging to the older generation. This suggests a potential generational imbalance in local governance during the period under study. Furthermore, approximately 37.2% of the elections were characterized by mixed-generation races.

The average mayor is approximately 49 years old, with a significant prevalence of male officeholders, who represent nearly 89% of the sample. In terms of human capital and political professionalism, mayors possess, on average, 14.6 years of education and a decade of previous experience in political roles.

The sample also reflects a diverse range of municipal characteristics that could potentially confound the relationship between leadership cohorts and spending patterns. The average municipality in the dataset has a population of approximately 7,362 inhabitants, though the high standard deviation and a median of 2,413 inhabitants indicate a sample composed largely of small to medium-sized towns.

Table 1: Summary Statistics, 2004-2015

	Mean	s.d	p25	p50	p75
Outcome Var.					
Environment Exp (%)	20.058	8.681	14.867	19.686	24.975
Environment Exp (Euro p.c.)	170.318	169.501	106.159	144.767	198.494
Mayor Characteristics					
Female	0.114	0.318	0	0	0
Age at the date of elections	49.083	10.085	42	49	56
Old G. Mayor	0.715	0.451	0	1	1
Mixed Gen. Races	0.372	0.483	0	0	1
Years of education of politician	14.639	3.416	13	13	18
Total years of experience in politic	10.012	6.438	6	10	15
Last Legal Term	0.236	0.424	0	0	0
Municipal Characteristics					
Unemployment rate	9.250	6.919	4.6	6.7	12
Upper Secondary Education (%)	42.463	10.708	34.6	42.1	50
Pop. Size	7,362.082	43,139.488	1,065	2,413	5,892
Older Municipalities	0.525	0.499	0	1	1
Demographic Density	310.540	649.723	52.7	118.5	298.2
Rural Municipalities	0.655	0.475	0	1	1
Median Altitude	403.833	404.147	111	274	567
Unions of Municipalities	0.290	0.454	0	0	1
Tourist accommodation (beds p.c.)	0.119	0.405	0.005	0.020	0.066
Observations	66,923				

Notes. The table reports means, standard deviations, and the 25th, 50th and 75th percentile of share of environmental-related expenditures, old generation mayors and all covariates at the individual and municipal level.

These entities are characterized by an average unemployment rate of 9.25% and a demographic density of 310.5 inhabitants per square kilometer. From a geographical and structural perspective, 65.5% of the municipalities are classified as rural, with a median altitude of 403 meters above sea level. Furthermore, the capacity for policy implementation may be influenced by administrative co-operation, as 29% of the municipalities belong to a Union of Municipalities.¹⁷ Finally, the local economic structure is captured by the level of education-with 42.4% of the population holding an upper secondary degree-and the intensity of

¹⁷The latter are administrative conglomerates of small municipalities which maintain individual independence, but provide some specific services jointly in order to enjoy economies of scale. Information on Union of Municipalities are fundamental in our analysis to control for potential confounding factor related to institutional setting where the mayor have relatively less individual influence on political choices. Further details are provided in Section 6.5

the tourism sector, which averages 0.119 guest beds per capita.

To assess potential threats to our identification strategy, we conduct balance tests comparing covariates between municipalities with older- and younger-generation mayors. Table 2 reports the results for the full sample and for subsamples restricted to municipality-years following close elections, defined by victory margins of 10%, 5%, and 2%. The table reveals significant differences between the two groups across several characteristics, including inhabitants education levels, population size, and the share of the elderly population. Furthermore, younger-generation mayors are more likely to be female, hold higher educational qualifications, and possess fewer years of political experience. The observed correlation between the mayor's age and municipal characteristics suggests that the generation gap may also be associated with missing or unobservable factors, such as local political preferences.

To address these issues and isolate the effect of the mayor's generation on budgetary choices, we include these variables as controls alongside province and year fixed effects. Furthermore, we refine our estimates by restricting the sample to municipalities with narrow victory margins. We further validate the robustness of our results using a Regression Discontinuity Design (RDD), specifically analyzing mixed-generation races where an older-generation candidate defeated a younger-generation opponent by a small margin. We acknowledge that focusing on narrow election margins does not fully eliminate potential confounding biases. Since treatment assignment is defined by the mayor's generation, it is inherently correlated with personal characteristics associated with age (Alesina et al., 2019). However, the combination of province and year fixed effects, individual controls, and the focus on close elections helps mitigate concerns regarding the influence

of the mayor's individual characteristics.

Finally, Table [A.2](#) in the Appendix provides descriptive statistics on the quantities and costs of local waste management. Notably, while the per capita volume of unsorted waste is roughly 20% higher than that of sorted waste, the per capita disposal cost for unsorted waste is approximately 55% greater.

Table 2: T-test, Full Sample

variable	All	All	m.v.=10%	m.v.=10%
	diff	pvalue	diff	pvalue
Female	0.05	0.00	0.07	0.00
Years of education of politician	1.18	0.00	1.08	0.00
Total years of experience in politic	-2.59	0.00	-2.79	0.00
Term limit	-0.05	0.00	-0.05	0.00
Unemployment rate	-0.15	0.01	-0.26	0.02
Upper secondary education	1.37	0.00	1.05	0.00
Pop Size	-1465.47	0.00	-2248.61	0.00
Older median voter	-0.01	0.22	0.00	0.51
Rural municipalities	0.01	0.00	0.01	0.05
Demographic density	-25.07	0.00	-14.74	0.15
Median altitude	0.87	0.80	13.14	0.03
Unions of municipalities	0.01	0.12	0.04	0.00
Tourist accommodation (beds p.c.)	-0.01	0.00	0.00	0.92
variable	m.v.=5%	m.v.=5%	m.v.=2%	m.v.=2%
	diff	pvalue	diff	pvalue
Female	0.06	0.00	0.09	0.00
Years of education of politician	1.18	0.00	1.15	0.00
Total years of experience in politic	-2.78	0.00	-2.62	0.00
Term limit	-0.05	0.00	-0.06	0.00
Unemployment rate	-0.02	0.88	-0.59	0.01
Upper secondary education	1.06	0.00	0.17	0.64
Pop Size	-1097.90	0.02	-1093.08	0.00
Older median voter	-0.01	0.37	0.06	0.00
Rural municipalities	0.00	0.93	0.03	0.11
Demographic density	21.06	0.12	-52.40	0.00
Median altitude	17.14	0.04	26.43	0.04
Unions of municipalities	0.06	0.00	0.09	0.00
Tourist accommodation (beds p.c.)	0.00	0.78	0.01	0.42

Notes. The table presents differences in covariates between municipalities led by mayors from younger generations and those led by mayors from older generations. First column measure the effective differences. Second column reports the p-value of the t-test. The test has been conducted on all observations and on subsample around 10%, 5% and 2% margin of victory between first and second candidate.

4 Identification Strategy

In order to provide an estimate of the effects of the generation of the mayor on environmental expenditure, we proceed as follows. We estimate a model of the

form:

$$Y_{mt} = \beta_0 + \beta_1 OldG.Mayor_{mt} + Z_{mt} + \phi_i + \tau_t + \epsilon_{mt} \quad (1)$$

where Y is the outcome, $OldG.Mayor$ is a dummy which takes value 1 if mayor belongs to the Baby-boomer, Silent, or Greatest generation. Z is a vector of mayor and municipality characteristics of municipality m in the year t . Individual characteristics include sex, years of education, and years of experience.¹⁸ Additionally, we include a dummy for the last year of the term and a dummy which identifies mayors which can run for re-election, in order to avoid potential confounding factors related to different strategic use of the budget cycle (Alesina et al., 2019). Municipalities' variables include share with individuals with upper secondary education, unemployment rate, population size, demographic density, a dummy for identifying older median voters in the municipal distribution, dummies for altitude classes and a dummy for rural municipalities. Moreover, we include a dummy which controls for municipalities belonging to unions of municipalities, which might have less discretion in environment policies, and a proxy of touristic relevance in the form of numbers of available accommodations, as more touristic places faces different incentives and volume of environmental resources usage. The parameters ϕ_i and τ_t represent, respectively, province and year FE.¹⁹

¹⁸We compute years of experience in politics after matching name, birth date and place of birth of each mayor across the entire politician registry. This means that this variable accounts for the experience accumulated not only as a mayor, but also as a counsellor in local political bodies.

¹⁹We use province-fixed effect rather than municipality-fixed effects due to the limited time span of our analysis, which is 11 years (2004-2015). Given that mayoral terms typically last 5 years and the vast majority of the sample belongs to the older generations, we lack sufficient variation within municipalities to include municipal fixed effects. Around 50% of mayors runs two consecutive terms, and even in case of a change of mayor, they still usually belongs to older generations, meaning that the treatment tends to be relatively fixed over time within munic-

Despite the inclusion of an extensive set of controls as well as time and province fixed effects, there may still be concern that variations in the share of expenditures reflect shifts in electorate preferences, which could also influence the age of the mayor. To mitigate this potential threat to our identification strategy, we also present the results when exclusively focusing on municipalities with closely contested elections.²⁰ The margin of victory of the mayor is calculated as the difference in vote share between the winner and the second most voted candidate in the electoral competition. We use a margin of victory of 10, 5, or 2 pp.²¹ This approach enables us to differentiate between variations in the outcome attributable to changes in the generation of the mayor and those resulting from shifts in voter preferences, as in [Alesina et al. \(2019\)](#). Additionally, in a further specification, we exclusively examine close election with mixed-generation races, specifically elections where the two candidates belong to different generations (young vs. old generations). In this case, a positive margin means that the older candidate received more votes, while a negative margin corresponds to a young candidate winning the election. When a candidate wins election by a small margin, municipal and individual characteristics are expected to be balanced with respect to the second candidate, and the small difference in votes received are likely determined by random fluctuations([Lee, 2008](#)). Therefore, this approach establishes a quasi-experimental setting where the winning of the older generation

ipalities. See Appendix D for an extensive discussion on the sample limitation for municipal fixed effects, as well as an alternative identification strategy based on Dynamic Two-Way-Fixed effects with staggered adoption including municipal fixed effects.

²⁰Early applications of this strategy could be found for example in [Lee et al. \(2004\)](#) or [Lee \(2008\)](#).

²¹For municipalities with a runoff election system, which are those with population above 15,000, the margin of victory is calculated based on the results of the second election. For municipalities without a runoff system, where only one election is held, the margin of victory is determined using the results of that single election.

mayor can be considered as-good-as random.

Our second empirical strategy employs a sharp regression discontinuity design in the context of mixed generation races, and uses the margin of victory as a forcing variable. We identify the effect by estimating the following linear regression:

$$Y_{mt} = \beta_0 + \beta_1 OldG.Mayor_{mt} + \beta_2 OldG.Mayor_{it} * MV_{mt} + Z_{mt} + \phi_i + \tau_t + \epsilon_{it} \quad (2)$$

with MV representing the margin of victory. β_2 is our parameter of interest, which identifies the local average treatment effect at the cut-off ($MV = 0$). We report parametric estimates within the optimal bandwidth, chosen according to the one common MSE-optimal bandwidth selector proposed by [Calonico et al. \(2017\)](#).²² The optimal bandwidth selector provides a robust, data-driven methodology to identify a bandwidth that strikes a balance between the increased bias associated with larger bandwidths and the inflated sampling variance characteristic of bandwidths that are too narrow.

5 Main Results

In this section, we present our main result of the effect of mayors of older generation on the share of current environmental-related expenditure. Table [3](#) reports the estimation results of the baseline specification. In the first column we look at the full sample of municipalities, while in columns 2-4 we limit the sample to municipalities-years following close elections, where a close election is decided by

²²In Section [6.4](#) we provide evidences for the validity of standard RDD assumptions, and in Appendix C we discuss potential limitations of applying RDD in our context.

a vote margin of 10, 5, 2%, respectively. Control variables are included in all columns. Estimates show that municipalities with mayors belonging to the older generation (Baby-boomers or older) exhibit a higher share of environment-related current expenditure, with an effect around 1 percentage points higher.

Table 3: Baseline Estimates of older generations effect on share of environmental expenditures

Variables	(1) Env. (%)	(2) Env. (%)	(3) Env.(%)	(4) Env. (%)
Old G. Mayor	0.531*** (0.132)	0.757*** (0.208)	0.853*** (0.281)	0.997** (0.429)
Observations	66,923	21,364	10,747	4,343
Adjusted R-squared	0.382	0.413	0.427	0.452
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the share of environmental related current expenditures. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Considering the average value of the dependent variable, which is 20%, this represents an increase of 2-4% over the mean. The estimated effect increases in the sample of mayoral terms following close elections (columns 2-4).

Table 4 reports the estimates obtained by focusing only on mixed generation races.²³ The estimated coefficient appears similar to the previous one, with the exception of column 4, where is no longer statistically significant.²⁴

²³As explained in Section 4, mixed generation races are defined as those elections where the two most voted candidates in the first round (or in the single round) belongs to an Older Generations (Baby-boomers, Silent) and a Younger Generations (Gen X, Millenials, Gen Z).

²⁴This might be due to the lower number of observations when restricting the analysis to an extremely narrow margin of election.

Table 4: Mixed Generation races subsample estimates of older generations effect on share of environmental expenditures

Variables	(1) Env. (%)	(2) Env. (%)	(3) Env.(%)	(4) Env. (%)
Old G. Mayor	0.384** (0.175)	0.827*** (0.290)	0.920** (0.405)	0.928 (0.601)
Observations	24,897	7,663	3,895	1,619
Adjusted R-squared	0.400	0.441	0.462	0.499
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the share of environmental related current expenditures. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Only for mixed-generation races. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

To better explore the role played by the generation of the opponent candidate, in Table 5, we remove the condition of mixed races and look at how the results changes according to the generation of the second most voted candidate. Specifically, we estimate an Equation akin to Eq.1, which include three dummies: a) a dummy for mayoral terms where the mayor belonged to a older generation, while the opponent belonged to a younger generations (Old Generation Mayors vs Young Generation Opposing Candidate), b) a dummy for mayoral terms where both the mayor and the opponent belonged to a older generation (Old Generation Mayors vs Old Generation Opposing Candidate) c) a dummy for mayoral terms where the mayor belonged to a young generation, while the opponent belonged to a older generation (Young Generation Mayors vs Old Generation Opposing Candidate). Therefore, the omitted category consists of mayoral terms in which both the most two voted candidates belongs to young generations.

Table 5: Older generations effect on environmental expenditures with respect to opposing candidate generation

Variables	(1) Env. (%)	(2) Env. (%)	(3) Env. (%)	(4) Env. (%)
Old G. Mayor vs Young G. Opposing Candidate	0.411* (0.210)	0.811** (0.340)	0.959** (0.462)	0.734 (0.706)
Old G. Mayor vs Old G. Opposing Candidate	0.517*** (0.196)	0.704** (0.311)	0.849** (0.419)	0.726 (0.638)
Young G. Mayor vs Old G. Opposing Candidate	-0.0517 (0.193)	-0.0121 (0.318)	0.0296 (0.433)	-0.377 (0.674)
Observations	66,860	21,348	10,738	4,340
Adjusted R-squared	0.382	0.412	0.427	0.451
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from a model akin to Eq. 1 per the share of environmental related expenditures, distinguishing old and young winners when running against old and young opposing candidates. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

According to the results, mayors of older generation are associated with higher environmental expenditures compared to younger mayors, regardless the generation of the other candidate. These results suggests that the generational difference in environmental expenditures depends only on the age of the winner, increasing our confidence when comparing results between baseline OLS and RDD models, where we are forced to analyse mixed-generation races only because of the monotonicity assumption.

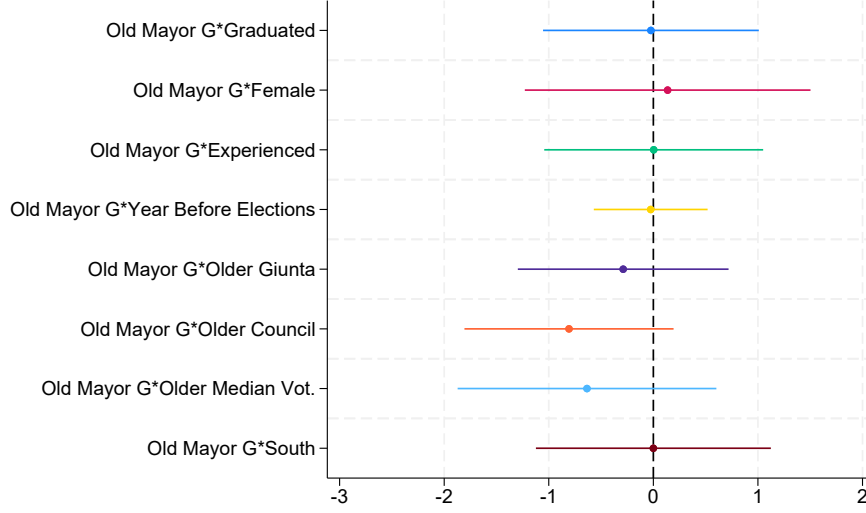
6 Further Analyses and Robustness checks

6.1 Heterogeneity analysis

To explore potential heterogeneous effects, we augment our baseline model by estimating a series of separate specifications. In each of these models, we interact our primary variable of interest, *Old G. Mayor*, with a single individual or

municipal characteristic.

Figure 2: Heterogeneity Analysis



Notes. The figure reports the estimated coefficients on the interaction term between the Old Mayor Gen. dummy variable and each variable of interest (dots), along with their 95% confidence intervals (bars). The estimates are obtained using an empirical specification similar to Equation (1), restricting the sample to a 5% margin of victory. Graduated is a dummy variable equal to one if the mayor holds a bachelor's degree or higher, and zero otherwise. Female is a dummy variable equal to one if the mayor is female, and zero otherwise. Experienced is a dummy variable equal to one if the mayor's years of experience are above the median of the national distribution, and zero otherwise. Year-Before-Elections is a dummy variable equal to one if the mayor is eligible for a second term and the current year immediately precedes the next election, and zero otherwise. Older-Giunta and Older-Council are dummy variables equal to one if the average age of the politicians in the respective political body is above the median of the national distribution, and zero otherwise. Older-Median-Voter is a dummy variable equal to one if the mean age of the constituency is above the median of the national distribution, and zero otherwise. South is a dummy variable equal to one if the municipality is located in the Southern Italy macro-area, and zero otherwise.

Specifically, we sequentially test interactions with the mayor's gender, years of education, and prior political experience, as well as with municipal-level factors such as the average age of the local council, the presence of an older median voter, and a Southern region indicator. For all these estimations, we restrict our analysis to a sample of close elections, focusing on a victory margin of 5 p.p. Figure 2 plots the estimated coefficient of the interaction term for each of these variables.²⁵

²⁵Individual estimated coefficient for the main generational variable are always consistent with the baseline. Individual estimated coefficients of each variable considered in the heterogeneity

Estimates shows there are no clear heterogeneities in generational effect based on individual and municipal characteristics. The effect does not appear to be heterogeneous with respect to mayor’s education, gender or political experience, nor with respect to the age of other local political bodies. No clear heterogeneity is found with respect to the last year before election, which indicates that there is no evidence of strategic use of budget cycle affecting environmental expenditures.²⁶

6.2 Other functions of expenditures

We extend the analysis to examine how the mayor’s generation affects the broader composition of public spending across other current expenditure functions, specifically investigating justice, police, education, culture, sport, tourism, transport, social services, and economic development. Figure 6 plots the coefficients for the older-generation mayor indicator using the share of current expenditure for each function as the dependent variable. The sample is restricted to mayoral terms following close elections, defined by a victory margin narrower than 5%.²⁷

Estimates indicate that older-generation mayors allocate a significantly lower share of the budget to social and cultural expenditures. Specifically, we observe a reduction of 0.54 percentage points in social spending (relative to a baseline average of 10%) and 0.17 percentage points in cultural spending (relative to a 2% average). The Social function broadly encompasses welfare services and residen-

analysis are never significant on their own. Estimates on full sample or alternative margins of election are consistent with the 5 p.p. margin of election.

²⁶Despite no clear evidence of heterogeneity with respect to the age composition of the electoral basis is found in this analysis employing interaction terms in the main model, the estimated coefficient is almost significant at 10% and appears to reduce the impact of the generational effect on the share of environmental expenditures. In Section 6.4 we explore this type of heterogeneous effect in an RDD context.

²⁷Estimates obtained using the full sample or alternative victory margins are consistent with these findings.

tial care for the elderly, while also notably including early childhood education services, which were classified under this category rather than the Education function prior to the 2015 reform. Similarly, the Cultural function aggregates expenses for the management of historical heritage, libraries, museums, and the promotion of cultural events. Conversely, older mayors allocate a higher share of current expenditures to transportation-comprising road maintenance, public lighting, and transit operations-with an increase of 0.31 percentage points relative to a 10% baseline.

Table 6: Other functions of current expenditures

	(1)	(2)	(3)	(4)	(5)
Variables	Management (%)	Police (%)	Social (%)	Development (%)	Transportations (%)
Old G. Mayor	-0.464	-0.182	-0.537*	0.027	0.313*
	(0.392)	(0.124)	(0.324)	(0.038)	(0.180)
Observations	10,747	10,747	10,747	10,747	10,747
Adjusted R-squared	0.378	0.221	0.350	0.126	0.277
	(6)	(7)	(8)	(9)	(10)
VARIABLES	Justice (%)	Education (%)	Culture (%)	Sport (%)	Tourism (%)
Old G. Mayor	-0.011	-0.131	-0.175**	0.045	0.015
	(0.012)	(0.179)	(0.076)	(0.058)	(0.051)
Observations	10,747	10,747	10,747	10,747	10,747
Adjusted R-squared	0.192	0.386	0.276	0.268	0.272
Margin of victory (p.p.)	5	5	5	5	5
Province FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the the share of other functions of current expenditures. Only for 5% margin of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Collectively, this evidence suggests that the larger budget share allocated by older-generation mayors to environmental-and, to a lesser extent, transportation-expenditures likely crowds out resources that younger mayors would otherwise direct toward social and cultural services.

6.3 Robustness checks

We conduct a series of robustness checks to validate our main findings. First, we test the stability of our results by aggregating the data at the mayoral term level rather than relying on annual observations. This approach allows us to estimate the average effect over the entire mandate of each mayor. The results, reported in Table [A.3](#), are consistent with the baseline estimates.

Second, we replicate the analysis by clustering standard errors at the province, province-by-year, region, and region-by-year levels, as alternatives to clustering at the municipality level. We perform this sensitivity analysis on both the full sample and the restricted sample of mixed-generation races with a 5% victory margin (Table [A.4](#)). In all cases, the statistical significance of the estimates remain robust.

Third, we assess the sensitivity of our results to the choice of the observation period by extending the time window to 2000–2015 and by restricting it to the more recent 2010–2015 period (Table [A.5](#)).²⁸ The estimated coefficients across these alternative time-frames are consistent with those obtained in the main analysis.

Fourth, we replicate the analysis by disaggregating the generational variable

²⁸The latter restriction facilitates comparison with the estimated effects on waste quantities presented in Section [6.5](#) for which data are available only for the 2010–2015 period.

into specific indicators for each cohort (Generation X, Millennials, Baby Boomers, and the Silent Generation), rather than relying on a single binary dummy for older generations (Table [A.6](#)). The results indicate that the effect is primarily driven by the difference between Baby Boomers (the omitted reference category) and Generation X, with the latter associated with lower environmental expenditures. The estimated coefficients for the Millennial generation resemble those of Generation X but lack statistical significance. This is likely attributable to their substantial under-representation in local politics during the observation period, as documented in previous sections.

Fifth, while our primary analysis focuses on expenditure shares to proxy for the political preferences and allocation decisions of different generations, we validate our results using per capita current environmental expenditures (in Euro) as the dependent variable (Table [A.7](#)). The estimates obtained are consistent in statistical significance with those derived using expenditure shares.

Finally, in Appendix [7](#), we address the well-known empirical challenge of disentangling life-cycle ageing from generational cohort effects. To this end, we first provide additional estimations replacing our baseline generation dummy with both a continuous age variable and age-bin indicators. Furthermore, we estimate a demanding specification that simultaneously includes our generation-based treatment variable alongside continuous age as a control. Although this simultaneous inclusion inherently introduces collinearity, the continuous age coefficient drops to near zero while the generational estimates remain stable in magnitude, suggesting that our findings are driven by cohort differences rather than a simple ageing process.

6.4 Regression Discontinuity Design

To corroborate our panel data findings and further address potential endogeneity concerns, we implement a Regression Discontinuity Design (RDD) as an additional robustness check. Before presenting the results of the RD estimates, we test the validity of the RDD design in our context.

Firstly, we verify that municipal characteristics are continuous around the threshold. The results are reported in Table C.1. There are no significant discontinuities for most municipal covariates, with the exception of the share of the population with upper secondary education, which presents a discontinuity at the 10 percent level. However, estimates on mayors' individual characteristics shows that younger mayors display higher level of education and lower years of experience in politics compared to older generations. We include both these variables as control in the baseline OLS and RDD regressions to mitigate the impact of potential biases caused by these characteristics.²⁹

Secondly, we assess the validity of the identification strategy by checking that the running variable exhibits continuity at the cut-off point.³⁰ This verification is essential to ensure that mayors belonging to either older or younger generations are not systematically more likely to win elections by a close margin, which would indicate a manipulation of the running variable. To formally test this condition, we rely on the density test proposed by McCrary (2008), which estimates the density of the running variable on both sides of the threshold to detect any

²⁹See Appendix C for a discussion on the limitation of the RDD in our context.

³⁰As explained in Section 4, the running variable is defined as the difference in percentage votes received between an older generation winner and a younger generation second candidate for the positive part, and the difference between a younger generation winner and an older generation second candidate for the negative part.

statistically significant discontinuity. Figure 3 illustrates the results of this test, plotting the density of the margin of victory along the horizontal axis. In this framework, the zero value on the x-axis represents the cut-off point for election victory. Observations located to the right of the zero threshold correspond to instances where mayors from the older generations won the election, while the area to the left of the zero threshold comprises the winning mayors from the younger generations.

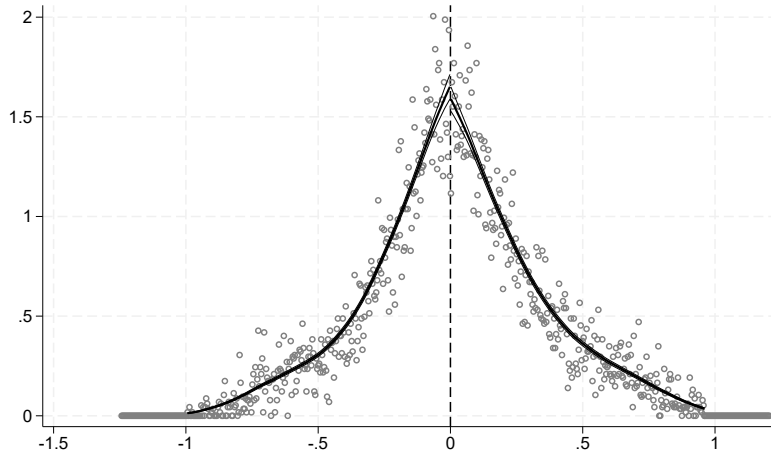


Figure 3: Manipulation Test, McCrary Test

Notes. The figure reports the density plot of the margin of victory together with a local second-order polynomial control function and 95 percent confidence intervals according to McCrary (2008) tests.

The visual and statistical evidence provided by the test confirms the absence of a discontinuity in the density of the margin of victory around the cut-off, thereby supporting the assumption of local randomization inherent to the regression discontinuity design.

Finally, the monotonicity assumption holds by design, as our RDD analysis specifically focuses on elections where the winner is older than the second candidate, with younger winners serving as the control group.

Table 7: RDD estimates

	(1)	(2)	(3)
Variables	Env. (%)	Env. (%)	Env. (%)
	Mixed G. Races	Mixed G. Races	Mixed G. Races
	All Mun.	Young Municipalities	Old Municipalities
Old G. Mayor	0.887** (0.450)	1.423** (0.655)	0.156 (0.605)
Observations	25,110	12,097	13,013
Optimal Band.	32.8	28	31.6
Controls	YES	YES	YES
Year FE	YES	YES	YES
Province FE	YES	YES	YES

Notes. The table reports sharp RD estimates from Eq. (2) for share of environmental-related expenditures in mixed-generation elections. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7 reports the results of a parametric estimate within the optimal bandwidth, chosen according to the one common MSE-optimal bandwidth selector proposed by Calonico et al. (2017). Estimates include control variables. The results confirm previous findings. Mayors from the older generation are associated with higher environmental-related expenditures. In columns 2 and 3, we focus only on young and old municipalities, respectively.³¹ This result suggests that the difference in expenditures related to the environment is primarily driven by municipalities with a higher proportion of younger individuals. Drawing on this evidence, we posit that mayors from various generations adjust their present environmental spending in accordance with the age composition of the munici-

³¹As discussed in the previous section, old and young municipalities are defined with respect to the median population age.

pality. In instances where there is a greater proportion of younger individuals, younger mayors tend to better allocate resources, reducing environmental-related expenditures.

6.5 Channels: Other outcomes related to environmental issues

The evidence provided in previous sections shows that mayors from older generations appear to engage in higher expenditure for the management of environment issues compared to their younger counterparts. This disparity is more pronounced when the population of the municipality is younger. Our main hypothesis on the mechanism explaining such difference lies in what the Italian environmental budget function represents in terms of current expenditures. Current environmental expenditures are composed by all the expanses needed to manage local waste disposal, water sources and distribution, urban environment and municipal parks. Table A.8 presents the share of each specific program over total current expenditures in environment. Around 66% of expenditures are dedicated to waste disposal management and almost 16% are dedicated to water services. All other sub-programs are much less relevant in the composition of current environmental expenditures.³² Consequently, given that the current literature indicates that younger generations pay more attention and behave more carefully in terms of environmental issues (Vlandas et al., 2021; Ross and Rouse, 2022), we expect older mayors to enact policies less effective in managing environmental resources, causing an increase in current expenditures.

³²Subprograms includes expenditures for public parks, urban environment, public buildings and civil aid.

However, disaggregating the analysis to focus on specific environmental sub-functions, such as waste disposal and water management, introduces measurement challenges due to heterogeneity in service provision models. Municipalities typically manage these environmental services through one of three arrangements: (i) direct autonomous management, (ii) delegation to a “Union of Municipalities,” or (iii) participation in a specific consortium. Unions are institutional associations formed by municipalities to jointly manage services, thereby exploiting economies of scale and enhancing efficiency.³³

Crucially, when environmental services—most notably waste collection or water treatment—are delegated to a Union or consortium, the associated current expenditures are not recorded under the corresponding environmental budget function. Instead, these costs are aggregated within the general expenditure item for current transfers to other government institutions.³⁴ Consequently, this accounting practice obscures the specific allocation for environmental services, resulting in a systematic prevalence of zero values in the waste and water sub-functions for municipalities adopting these delegated management models. A similar problem arises when municipalities form a consortium. The latter differs from unions in the fact that it exists only for the purpose of managing one specific sub-program collectively. Even in this case municipalities could report their shares of costs sustained by the consortium either under the related budget function, or under transfers to other entities. On top of that, all three configurations have the possibility to completely externalize the service to a private contractor. What

³³The institutional framework for Unions of Municipalities and Consortia is defined by the Italian Consolidated Law on Local Government (Legislative Decree No. 267/2000, Articles 31 and 32).

³⁴Specifically, Presidential Decree No. 194/1996, which regulated local government accounting prior to the harmonization reform in 2015.

that means is that they fully delegate decisions on the specificity of the service provided, sustained costs and, sometimes, even the collection of local tariffs and taxes related to the service to a private firm. In this case, they are only obliged to reports in the municipal budget differences between costs and profits sustained by the private contractor, often with a certain time delay with respect to the year in which the service have been provided. Again, these transactions may fall under the specific budget function or aggregated with other transfers to private entities.³⁵ Ideally, to accurately isolate a mayor's influence on environmental expenditures, we should exclude all municipalities that delegate or externalize these services, as their mayors lack direct control over the associated costs. However, we face significant data limitations. First, while we can consistently observe whether a municipality belongs to a Union of Municipalities during our sample period, we cannot observe which specific services it delegates. Second, data on consortia are only available from 2011 onwards, and systematic records on private externalization contracts are entirely absent.

To circumvent these constraints and properly restrict our sample, we rely on two conservative assumptions. First, using data from the AIDA PA database, we assume that any municipality belonging to a Union delegates its waste and water management services, and we consequently drop them from our analysis. Although this approach is strict, it guarantees that the municipalities retained in our sample are definitively not delegating these services to a Union. This procedure excludes 1,868 out of 6,671 municipalities (approximately one-third of our main sample). As shown in Table A.9, our baseline results regarding the environmental budget share remain robust to this exclusion.

³⁵Presidential Decree No. 194/1996

Second, to address consortia and private externalization, we assume that current expenditures for waste disposal and water management cannot reasonably be zero or near-zero in any given year, as these are essential services for any city. If such costs are not reported under the corresponding municipal budget items, they must have been accounted for elsewhere, strongly indicating that the municipality either belongs to a consortium or has fully externalized the process to private contractors. Consequently, to filter out these unobserved delegated models, we assess the robustness of our results by subsequently trimming the bottom 1%, 5%, and 10% of the waste disposal and water service expenditure distributions. In Table [A.10](#) we excluded that older generations disproportionately belongs to this part of the distribution. All these percentile exclusions seems to be perfectly reasonable when comparing the distribution of total per-capita costs measured from ISPRA data on waste management in the period 2011 to 2015, which are not derived from municipal budget, and to data derived from SOSE. ISPRA data, SOSE data and budget data on wastes disposal costs display similar means (135.34 for ISPRA, 143.67 for SOSE and 128.33 for budget data) but differs in bottom part of the distribution.^{[36](#)}

Table [8](#) presents estimates for our main model using the Euro per-capita budget expenditures on waste disposal as the outcome variable. Table [9](#) shows the same estimates for water management services. In both cases, mayors of older generations display higher expenditure for the provision of such services.

³⁶The first percentile in the ISPRA dataset correspond to 54.2 Euro per-capita. The same percentile in SOSE data equals 49.36 Euro per-capita. These values are similar to the tenth percentile in budget data, 60.39 Euro per-capita. The fifth percentile in budget data for wastes expenditures correspond to around 2 Euro per-capita, and the first percentile is equal to zero. These values are unreasonably low considering the distribution of wastes costs accounted by ISPRA and SOSE, and may indicate those municipalities simply reporting the much smaller costs sustained to compensate private contractors of losses experienced for the service provided.

Baby-boomers and Silent-gen mayors spend on average between 4 and 8 Euro per-capita more in waste disposal, compared to an average cost of 108 Euro per-capita, and between 3 and 6 Euro per-capita in management of water services, compared to an average cost of around 33 Euro per-capita.

Table 8: Older generations effect on waste budget expenditures

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Wastes (Euro p.c.)					
Old G. Mayor	4.350** (1.866)	8.652** (4.274)	4.059** (1.885)	8.622** (4.339)	4.364** (1.888)	8.572** (4.361)
Observations	45,187	7,642	43,561	7,337	41,399	7,008
Adjusted R-squared	0.240	0.325	0.236	0.310	0.255	0.331
Margin of victory (p.p.)	NO	5	NO	5	NO	5
Province FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Bottom pctlile exluded	1	1	5	5	10	10

Notes. The table reports estimates from Eq. 1 for the Euro per-capita current expenditures in waste sub-program of environment. Column (1) and (2) excluded all zero values in the outcome variable. Column (3) and (4) exclude values below the fifth percentile. Column (5) and (6) exclude values below the tenth percentile. Even columns reports estimates in sample restricted around 5% margin of victory. Monetary values are adjusted at 2015 level. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Older generations effect on water services budget expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Water services (Euro p.c.)					
Old G. Mayor	3.174*** (0.958)	5.682*** (2.129)	3.263*** (0.972)	5.661*** (2.165)	3.552*** (1.036)	6.117*** (2.338)
Observations	44,245	7,546	43,651	7,434	40,822	6,958
Adjusted R-squared	0.352	0.366	0.350	0.363	0.341	0.352
Margin of victory (p.p.)	NO	5	NO	5	NO	5
Province FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Bottom perc. excluded	1	1	5	5	10	10

Notes. The table reports estimates from Eq. 1 per the Euro per-capita current expenditures in water services sub-program of environment. Column (1) and (2) excluded all zero values in the outcome variable. Column (3) and (4) exclude values below the fifth percentile. Column (5) and (6) exclude values below the tenth percentile. Even columns reports estimates in sample restricted around 5% margin of victory. Monetary values are adjusted at 2015 level. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

We further validate our findings by re-estimating the OLS specification using data on local waste generation and waste disposal costs from ISPRA (discussed in Section 3.3) for the 2010-2015 and 2011-2015 periods, respectively. We restrict the sample to mayoral terms following close elections, defined by a victory margin of less than 5%.

Tables 10 and 11 report the results. The estimates indicate that municipalities governed by older-generation mayors exhibit higher per capita levels of unsorted waste (Table 10). Similarly, the costs associated with unsorted waste disposal are higher compared to municipalities led by younger mayors (Table 11). Remarkably, we estimate a differential of approximately 4 Euro per capita, a magnitude consistent with the gap identified using municipal budget data.

Taken together, this evidence reinforces the hypothesis that older mayors may

assign a lower priority to environmental issues compared to their younger counterparts. This lower concern translates into a reduced effectiveness in curbing the generation of unsorted waste, thereby driving up disposal costs and ultimately explaining the higher share of the budget allocated to environmental services observed in our main analysis.

Table 10: Sorted and Unsorted Waste, Quantity (2010-15)

	(1)	(2)
Variables	Unsorted waste (Kg pc)	Sorted waste (Kg pc)
Old G. Mayor	11.600** (5.301)	1.765 (3.741)
Observations	5,239	5,210
Adjusted R-squared	0.462	0.630
Margin of victory (p.p.)	5	5
Province FE	YES	YES
Year FE	YES	YES

Notes. The table reports estimates from Eq. 1 for waste-related quantity variables in the period 2010-2015. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Sorted and Unsorted Waste, Costs (2011-15)

	(1)	(2)
Variables	Unsorted waste costs (Euro pc)	Sorted waste costs (Euro pc)
Old G. Mayor	4.179** (1.875)	-0.625 (1.398)
Observations	2,349	2,373
Adjusted R-squared	0.379	0.227
Margin of victory (p.p.)	5	5
Province FE	YES	YES
Year FE	YES	YES

Notes. The table reports estimates from Eq. 1 for waste-related costs variables in the period 2011-2015. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7 Conclusions

This paper examines the impact of a mayor's age on local public finance, with a specific focus on environmental expenditures. Our findings indicate that municipalities led by older mayors tend to allocate a larger share of their budget to these functional areas. Further analysis reveals that these municipalities are characterized by a higher per capita production of unsorted waste, which inherently translates into higher disposal costs, alongside greater expenditures for local water management. Taken together, this evidence suggests that older mayors may be less proactive in implementing preventive environmental policies, potentially reflecting a lower political prioritization of these issues. Younger mayors which, according to current literature, shows greater political preferences in terms of facing long-term consequences of human behaviour on the environment, could

potentially be more efficient in delivering environmentally-related services, freeing an higher percentage of the budget for other municipal policies. Investigating heterogeneous effects across various individual and municipal characteristics, our results reveal that the demographic profile of the local electorate significantly shapes the policy divergence between older and younger mayors. Specifically, the difference in the share of environmental expenditure between younger and older mayors becomes more pronounced in younger municipalities. In such cases, there is likely a greater concern for environmental issues, and younger mayors, moved by electoral motives, might tend to put more effort into waste management.

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Appendix A: Additional tables and graphs

Table A.1: Summary Statistics: Share of expenses by category, 2004-2015

	Obs.	mean	sd	min	max
Outcome Var.					
Management (%)	66,923	39.895	10.741	0	100
Justice (%)	66,923	0.087	0.429	0	17
Police (%)	66,923	4.371	3.132	0	47
Education (%)	66,923	9.862	4.907	0	41
Culture (%)	66,923	1.878	2.106	0	69
Sport (%)	66,923	1.500	1.542	0	42
Tourism (%)	66,923	0.699	1.650	0	44
Transportations (%)	66,923	10.128	4.781	0	61
Social (%)	66,923	10.059	7.900	0	72
Development (%)	66,923	0.489	1.136	0	50

Notes. The table reports means and standard deviations of share of other types of expenses.

Table A.2: Summary on wastes, 2010-2015

	(1)				
	Obs.	mean	sd	min	max
Paper (Kg per capita)	30,239	43.508	23.140	3	124
Glass (Kg per capita)	30,119	35.245	16.920	2	95
Plastic (Kg per capita)	29,641	17.307	10.879	1	55
Unsorted wastes (Kg per capita)	31,367	247.580	133.884	56	697
Sorted wastes (Kg per capita)	31,228	202.926	111.251	6	517
Total wastes (Kg per capita)	31,367	462.543	147.517	203	1012
Unsorted wastes costs (Euro per capita)	14,227	59.594	34.059	13	191
Sorted wastes costs (Euro per capita)	14,227	37.866	22.693	2	113
Total wastes costs (Euro per capita)	14,224	98.666	36.461	42	246

Notes. The table reports means, standard deviations, minimum and maximum of waste-related quantities and costs at municipal level. Costs are only observed for the time-period 2011-2015 and for a smaller subsample of municipalities that do not belong to consortiums.

Table A.3: Older generations effect on environment-related expenditure over mandate

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env.(%)	Env. (%)
Panel 1: All elections				
Old G. mayor	0.702*** (0.162)	1.042*** (0.267)	0.932** (0.371)	1.079* (0.588)
Observations	9,529	3,035	1,522	618
Adjusted R-squared	0.424	0.444	0.450	0.464
Panel 2: Mixed Generation Races				
Old G. mayor	0.481** (0.224)	1.008*** (0.386)	0.837 (0.608)	0.705 (1.052)
Observations	3,439	1,046	524	227
Adjusted R-squared	0.443	0.482	0.475	0.458
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the share of environmental related current expenditures over mandate. Panel 1 reports estimates for the full sample. Panel 2 reports estimates only for mixed-generation races. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Older generations effect on environment-related expenditure with alternative S.E. clusters

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env.(%)	Env. (%)
Panel 1: All elections				
Old G. mayor	0.853*** (0.249)	0.853*** (0.149)	0.853** (0.340)	0.853*** (0.157)
Observations	10,747	10,747	10,747	10,747
Adjusted R-squared	0.427	0.427	0.427	0.427
Panel 2: Mixed Generation Races				
Old G. mayor	0.920** (0.409)	0.920*** (0.232)	0.920** (0.335)	0.920*** (0.220)
Observations	3,895	3,895	3,895	3,895
Adjusted R-squared	0.462	0.462	0.462	0.462
Margin of victory (p.p.)	5	5	5	5
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster SE	Province	Province*Year	Region	Region*Year

Notes. The table reports estimates from Eq. 1 for the share of environmental-related expenditures with alternative clusters. Panel 1 reports estimates for the full sample. Panel 2 reports estimates only for mixed-generation races. Only for 5% margin of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Older generations effect on environment-related expenditure for alternative time periods

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env.(%)	Env. (%)
Panel 1: From 2000 to 2015				
Old G. Mayor	0.558*** (0.129)	0.730*** (0.204)	0.798*** (0.273)	0.957** (0.415)
Observations	84,991	26,500	13,424	5,388
Adjusted R-squared	0.337	0.367	0.378	0.410
Panel 2: From 2010 to 2015				
Old G. Mayor	0.356** (0.143)	0.320 (0.239)	0.599* (0.325)	1.098** (0.498)
Observations	35,503	11,285	5,758	2,331
Adjusted R-squared	0.428	0.453	0.476	0.509
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the share of environmental-related expenditures with alternative time-periods. Panel 1 reports estimates for the period from 2000-2015. Panel 2 reports estimates for the period from 2010-2015. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Older generations effect on environment related expenditure including each generation separately

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env.(%)	Env. (%)
Silent and Greatest Gen.	0.099 (0.197)	0.428 (0.299)	0.321 (0.414)	0.388 (0.620)
Generation X	-0.542*** (0.136)	-0.703*** (0.217)	-0.813*** (0.292)	-0.950** (0.448)
Millenials	0.005 (0.335)	-0.566 (0.525)	-0.835 (0.670)	-0.800 (0.984)
Observations	66,923	21,364	10,747	4,343
Adjusted R-squared	0.381	0.412	0.426	0.452
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from a model akin to Eq. 1 for the share of environmental related current expenditures including each generation separately. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Older generations effect on Euro per-capita environment-related expenditure

	(1)	(2)	(3)	(4)
Variables	Env. (Euro p.c.)	Env. (Euro p.c.)	Env. (Euro p.c.)	Env. (Euro p.c.)
Old G. Mayor	6.306** (2.880)	9.360*** (3.330)	14.340*** (4.905)	23.410*** (6.095)
Observations	65,658	20,922	10,540	4,289
Adjusted R-squared	0.168	0.202	0.307	0.411
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for environmental related current expenditures in Euro per-capita. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Monetary values are adjusted at 2015 level. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Summary Statistics: Share of expenses by programs of environment expenditures, 2004-2015

Variables	Obs.	Mean	Std. Dev.
Parks (%)	46,046	8.705	13.608
Urban Env. (%)	65,785	5.904	11.310
Wastes (%)	65,785	66.662	25.871
Water services (%)	65,785	15.912	17.858
Public buildings (%)	65,785	1.142	4.258
Civil Aid (%)	46,046	1.904	7.011

Notes. The table reports observations, means and standard deviations for the share of sub-programs of the environment budget function.

Table A.9: Baseline model excluding unions of municipalities

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env. (%)	Env. (%)
Old G. Mayor	0.732*** (0.163)	0.934*** (0.256)	1.110*** (0.353)	0.808 (0.525)
Observations	46,341	15,554	7,855	3,172
Adjusted R-squared	0.372	0.405	0.424	0.461
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the share of environmental related current expenditures excluding unions of municipalities. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Probability of belonging to the bottom part of waste expenditure distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Bottom 1st pctl	Bottom 5th pctl	Bottom 10th pctl	Bottom 1st pctl	Bottom 5th pctl	Bottom 10th pctl
Old G. Mayor	0.000 (0.003)	0.007 (0.008)	-0.002 (0.005)	0.000 (0.009)	-0.002 (0.006)	-0.001 (0.012)
Observations	46,339	7,855	46,339	7,855	46,339	7,855
Adjusted R-squared	0.135	0.172	0.285	0.378	0.328	0.379
Unit	Year	Year	Year	Year	Year	Year
Reference Period	2004-15	2004-15	2004-15	2004-15	2004-15	2004-15
Margin of victory (p.p.)	NO	5	NO	5	NO	5
Province FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes. The table reports estimates from Eq. 1 for the probability of belonging to the bottom 1st, 5th and 10th percentile of waste expenditures distribution. Even columns reports estimates in sample restricted around 5% margin of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B: Other approaches to age effect

When analyzing the effect of a mayor’s generation on policy outcomes, it is important to acknowledge that these effects can be driven by factors related to both the aging process and the year of birth. In the literature, this is widely recognized as the classic identification challenge of separating the effects of age (years since birth), generational cohort (year of birth), and observation periods (Nemcok and Wass, 2021; Neundorff and Niemi, 2014; Yang and Land, 2013). Generational cohorts are broadly defined as groups of individuals who share analogous cultural and social experiences during their formative years and possess a measure of “generational consciousness” (Edmunds and Turner, 2002). Conversely, age is inherently linked to the human life cycle, while period effects capture the impact of exogenous shocks that uniformly affect all age groups (Jennings and Niemi, 2014).

Disentangling these dimensions has proven to be a significant challenge in regression analysis. Age and birth cohort variables are highly collinear, inherently leading to inflated standard errors and unreliable p-values. Despite this limitation, this section provides three robustness checks to yield additional evidence on how life-cycle and generational cohort dynamics affect current environmental expenditures.

In Table B.1, we present estimates of the simple linear effect of age, measured in years, on the share of environmental expenditures. Similar to our baseline estimates, this effect is positive and statistically significant. In Table B.2, we conduct a related analysis, replacing the older generation dummy with a set of indicator variables that divide mayors into roughly equally spaced age bins based

on the sample distribution. The excluded reference category comprises all mayors aged 18 to 39.

Table B.1: Estimates of age effect on share of environmental expenditures

Variables	(1) Env. (%)	(2) Env. (%)	(3) Env. (%)	(4) Env. (%)
Mayor's Age	0.021*** (0.006)	0.029*** (0.009)	0.033*** (0.013)	0.027 (0.019)
Observations	66,923	21,364	10,747	4,343
Adjusted R-squared	0.382	0.413	0.427	0.451
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from a model akin to Eq. 1 with continuous age variable as treatment for the share of environmental related current expenditures. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The estimated coefficients are consistent with our findings using the generation dummy. The effect appears to increase with age, peaking at 1.2 percentage points for individuals over 60 years old. This aligns with our generational perspective, as the majority of mayors from younger cohorts (Millennials and Generation X) are concentrated in the two youngest age bins (18 to 44 years). Finally, Table [B.3](#) presents estimates of the baseline model from Equation 1, augmented with the mayor's continuous age as an additional control variable. Interestingly, while the statistical significance of the generation indicator dropsâbecoming significant only at the 10% level for the 2% and 10% victory margins, and statistically insignificant for the 5% marginâthe magnitude of the point estimates remains remarkably similar to the baseline. Conversely, the coefficient for linear age becomes statistically insignificant across all specifications and drops to near zero. Although the reduced statistical significance of the generation variable is an expected conse-

quence of the inherent collinearity between age and cohort, these results suggest that the observed divergence in environmental expenditures is driven primarily by generational cohorts rather than life-cycle ageing.

Table B.2: Estimates of age bins effect on share of environmental expenditures

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env. (%)	Env. (%)
Age between 40 and 44	0.381** (0.168)	0.690** (0.295)	0.478 (0.413)	-0.186 (0.639)
Age between 45 and 49	0.390** (0.177)	0.810*** (0.301)	0.811* (0.418)	0.402 (0.634)
Age between 50 and 54	0.606*** (0.175)	0.876*** (0.293)	0.712* (0.400)	0.884 (0.645)
Age between 55 and 59	0.640*** (0.182)	0.686** (0.303)	0.742* (0.412)	0.518 (0.619)
Age above 60	0.744*** (0.190)	1.114*** (0.308)	1.187*** (0.442)	0.710 (0.676)
Observations	66,923	21,364	10,747	4,343
Adjusted R-squared	0.382	0.413	0.427	0.451
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from a model akin to Eq. 1 with five dummies representing equally spaced age bins as treatment on the share of environmental related current expenditures. The excluded category contains all mayors with age between 18 and 39 years. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Estimates of older generation effect on share of environmental expenditures with linear age as control

	(1)	(2)	(3)	(4)
Variables	Env. (%)	Env. (%)	Env. (%)	Env. (%)
Old G. Mayor	0.417** (0.204)	0.614* (0.325)	0.646 (0.438)	1.169* (0.685)
Mayors' Age	0.006 (0.009)	0.008 (0.014)	0.012 (0.019)	-0.010 (0.029)
Observations	66,923	21,364	10,747	4,343
Adjusted R-squared	0.381	0.412	0.426	0.452
Margin of victory (p.p.)	NO	10	5	2
Province FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes. The table reports estimates from a model akin to Eq. 1, with continuous age variable included as control, on the share of environmental related current expenditures. First column presents results for the full sample. Subsequent columns presents estimates for smaller margins of victory. Individual and municipal time-varying control variables are included in all regressions. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C: Limitations of RDD for close-election analysis

In principle, a Regression Discontinuity (RD) design allows for the identification of causal treatment effects by accounting for unobserved time-varying characteristics of both municipalities and individual mayors. However, as noted by [Alesina et al. \(2019\)](#) and [Brollo and Troiano \(2016\)](#), the core assumption of continuous potential outcomes may be partially violated in this specific context. This violation arises because treatment assignment is not solely determined by the margin of victory, but is fundamentally conditional on the candidate's age. Consequently,

the discontinuous jump at the threshold captures the simultaneous effect of age alongside other individual traits inherently correlated with it—a methodological challenge defined in the literature as a “compound treatment” (Keele and Titiunik, 2015; Hernán and VanderWeele, 2011; VanderWeele, 2009).

Conversely, this identification challenge is not expected to invalidate the continuity of potential outcomes regarding municipal-level characteristics. This expectation is supported by the results in Table C.1, which reports RDD estimates used to test for discontinuities in baseline covariates at the cutoff. Indeed, municipal characteristics appear continuous at the threshold, whereas individual-level variables exhibit significant discontinuities. Specifically, older mayors in the sample tend to be less educated, are less likely to be female, and possess more extensive political experience. Given these findings, we adopt a multi-step empirical strategy: we begin with standard Ordinary Least Squares (OLS) estimations, subsequently restrict the sample to a narrow margin of victory defined independently of candidate age, and finally focus on mixed-generation races. In the latter, where the margin of victory depends on the age differential between the top two candidates, we employ both OLS and RDD estimators to ensure robustness.

Table C.1: RDD Balance Test for Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Municipal Characteristics</i>						
Variables	Pop Size	Unemp. Rate	Upper Ed.	Old Median	Pop. Density	Tourist Acc.
RDD Estimate	-621.800 (750.500)	-0.320 (0.533)	1.325** (0.649)	-0.066 (0.044)	4.840 (49.920)	-0.033 (0.033)
Observations	25,776	25,252	25,252	25,786	25,252	25,591
<i>Panel B: Mayor Characteristics</i>						
Variables	Female	Ed. Years	Exp. Years			
RDD Estimate	-0.049* (0.027)	-1.364*** (0.263)	3.970*** (0.430)			
Observations	25,786	25,742	25,786			

Notes. The table reports sharp RD estimates of the discontinuity in covariates at the cut-off point in mixed-generation elections. The estimates are obtained from local linear regressions within the optimal bandwidth. Robust standard errors clustered at the municipal level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix D: Municipal FE and dynamic of the effect

A potential concern is that province and year fixed effects, alongside individual and municipal controls, may not be sufficient to capture all unobservable time-invariant heterogeneity across municipalities. However, sample characteristics constrain the feasibility of employing municipal fixed effects, particularly given the focus on the generational impact on environmental expenditures. Specifically, within the subsample of mixed-generation races, 75% of the observations exhibit no variation in treatment status, while 24% change status only once. To quantify

the proportion of treatment variance absorbed by municipal α_m and time τ_t fixed effects, we regress the old-generation mayor indicator solely on time and municipal fixed effects, using the following specification:

$$OldG.Mayor_{mt} = \alpha_m + \tau_t + u_{mt} \quad (3)$$

From this regression, we obtain an adjusted R-squared of 75%, indicating that time and municipal fixed effects absorb most of the variation in treatment status. This finding is corroborated by a decomposition of the variance of the old-generation indicator. The within-municipality standard deviation is 0.23, whereas the between-municipality standard deviation is 0.46. Consequently, variation within the same municipality over time accounts for 20% of the total variance. Restricting the analysis to subsamples with narrow election margins exacerbates this issue, further reducing the identifying variation available for a fixed effects estimator. Collectively, these evidence argues against the use of municipal fixed effects. Such a specification would likely yield imprecise estimates with large standard errors, as the treatment variable is highly collinear with both local-specific trends and stochastic shocks.

In this section of the appendix, we seek to address this concern while simultaneously broadening the analysis to capture the dynamic trajectory of environmental expenditures during the mayoral term. To this end, we estimate a Dynamic Two-Way Fixed-Effects (TWFE) model with staggered adoption on the 2000–2015 sample, defined as follows:

$$Y_{mt} = \alpha_m + \tau_t + \sum_{k=\underline{K}, k \neq -1}^{\bar{K}} \beta_k \times \mathbf{1}(t - E_m = k) + \mathbf{Z}'_{mt}\gamma + \varepsilon_{mt} \quad (4)$$

In this specification, we invert the treatment definition to focus on municipalities experiencing a transition from a mayor of an older generation to one of a younger generation. This strategy allows us to leverage a larger control group, comprising municipalities with older-generation mayors that never switch to a younger generation, as well as those that have not yet experienced such a generational turnover. Consequently, E_m denotes the year of the first election marking a shift from an older to a younger generation, while k measures the time relative to this event. Finally, $\mathbf{Z}'_{mt}$ represents the vector of time-varying controls consistent with the main model, while α_m and τ_t denote municipality and year fixed effects, respectively.

In this case, our identification strategy relies on the parallel trends assumption. This requires that, prior to the transition from an older to a younger generation mayor, trends in the share of current environmental expenditure evolve similarly between municipalities experiencing the switch and those that do not. If pre-treatment trends are parallel, it is plausible to assume they would have followed the same trajectory in the absence of the generational change. Consequently, any subsequent deviation from this path can be attributed to the mayor's generation. However, ensuring the validity of the Dynamic TWFE specification requires addressing specific constraints imposed by the sample structure.

First, we restrict the analysis to municipalities with complete electoral histories throughout the observation period. Gaps in electoral data could result in the

misclassification of treated units as untreated if a younger-generation mayor was elected during unobserved intervals, thereby introducing bias into both pre-trend and post-treatment estimates. Second, we exclude municipalities governed by a younger-generation mayor at the beginning of the panel, effectively removing “always-treated” units from the analysis (Goodman-Bacon, 2021). Third, for municipalities where a younger-generation mayor is subsequently succeeded by an older-generation one, we do not discard the unit entirely. Instead, we right-censor the observations, excluding only the years following this second switch. This ensures that the treatment remains an absorbing state within the observed window (Callaway and Sant’Anna, 2021). Finally, we exclude municipalities for which we cannot observe the full duration of a specific mayoral term. This restriction eliminates partial mandates-typically occurring at the beginning or end of the sample period-where the lack of data on the complete political cycle prevents an accurate assessment of expenditure trends. Following these restrictions, the sample is reduced from 6,667 municipalities (approximately 89,000 observations) to 3,964 municipalities (approximately 36,000 observations). Within this final sample, 48% of municipalities experience a transition to a younger-generation mayor.

Collectively, these restrictions ensure that the comparison is limited to municipalities observed for a sufficient duration both before and after the generational transition, while also eliminating the risk of misclassifying treatment status-specifically, the inclusion of untreated units in the treatment group or treated units in the control group. However, we acknowledge that these exclusions may constrain the causal interpretation of the results, as we cannot definitively assess whether the sample selection is driven by a random process or by specific

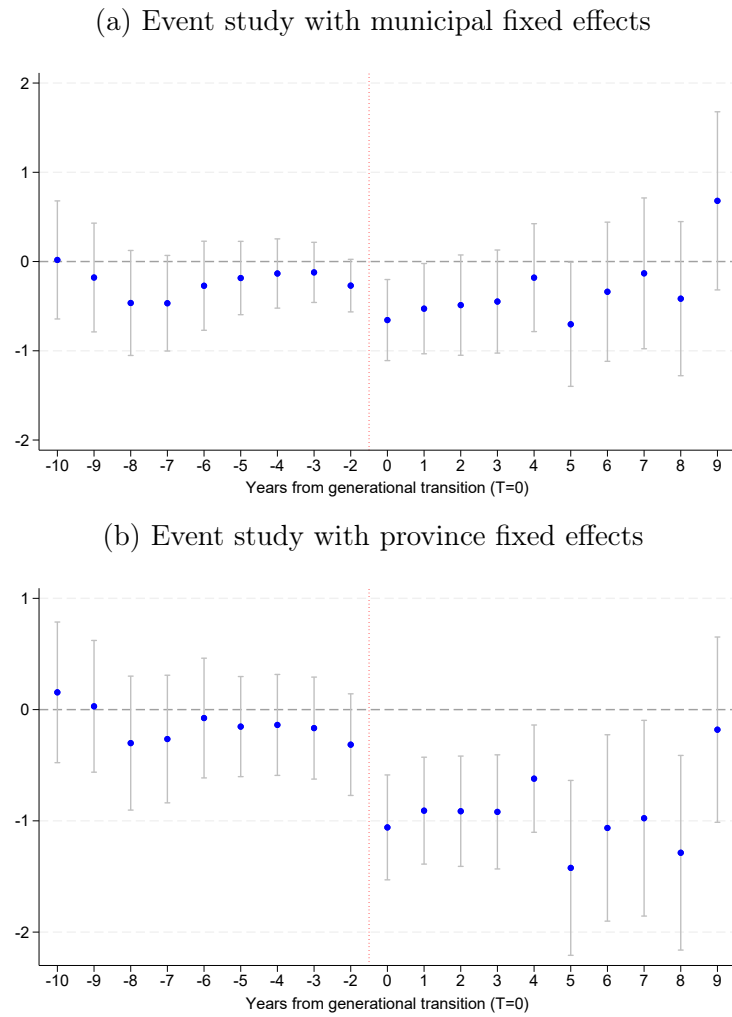
unobserved characteristics. Nevertheless, when viewed in conjunction with the evidence presented in the main analysis, these estimates offer valuable insights into the dynamics of generational effects on environmental expenditures. Furthermore, this specification allows for a formal test of the parallel trends assumption and ensures robustness to time-invariant municipal characteristics.

Figure [D.1](#) displays the estimated dynamic effects of a transition from an older- to a younger-generation mayor for each year relative to the generational switch. As previously noted, given that municipal fixed effects absorb a substantial portion of the variation in both the outcome variable and the treatment, and to facilitate comparison with the main analysis, we also report estimates using province fixed effects. First, we observe no significant pre-trend differences in either specification, lending support to the validity of the parallel trends assumption.

Consistent with the main analysis, we find that younger-generation mayors allocate a lower share of the budget to current environmental expenditures. While the magnitude of the effect is smaller in the municipal fixed effects specification compared to the model with province fixed effects, the confidence intervals largely overlap. Focusing on Figure [D.1a](#), the differential in the environmental share of current expenditures is statistically significant during the first two years of the first term. However, it loses significance in the subsequent three years, with the coefficient for the final year approaching zero. The effect becomes significant again in the first year of the second term before turning insignificant for the remainder of the observed period. These results indicate a diminishing expenditure differential within the electoral term, a pattern that runs counter to the standard political budget cycle. This trajectory is corroborated by the

province fixed effects specification, with the notable difference that the estimated coefficients remain statistically significant throughout, except for the final year of the second term. We propose two potential mechanisms driving this pattern. On the one hand, younger mayors may increase environmental expenditures as elections approach, effectively narrowing the gap with their older counterparts. This interpretation aligns with the hypothesis that younger politicians are more prone to strategically engaging in political budget cycles, as suggested by [Alesina et al. \(2019\)](#). On the other hand, older-generation mayors might divert resources away from environmental expenditure toward other expenditure categories perceived to yield higher political returns. However, without observing the specific preferences of voters within each municipality, it is currently not possible to determine a priori which type of spending generates a greater electoral payoff. Consequently, we cannot definitively disentangle which of these two mechanisms dominates in our setting. Nevertheless, the re-emergence of the differential in the second term—characterized by even larger negative coefficients in the province fixed effects specification—provides further evidence that generational differences significantly influence the allocation of resources to environmental current expenditures.

Figure D.1: Event study: Younger generations effect on environment-related expenditure



Notes. The figure reports relative-year estimates with 95% confidence intervals of the difference in share of current environmental expenditures between municipalities with younger-generation mayors with respect to older-generation mayors, before and after the generational transition. The period analysed include two mandates before and two mandate after treatment. Standard errors are clustered at municipal level.

Chapter 2: Fiscal policy, local autonomy and the flypaper effect: How central transfer affects local government spending behavior

1 Introduction

In recent years, various countries have begun shifting from a centralized system of governance, where public goods are provided through local authorities functioning as mere extensions of central power, to a decentralized model that divides responsibilities between local and central authorities on different public issues (Martinez-Vazquez et al., 2017). This decentralization delegates part of the responsibility for both revenues and expenditures to local decision-makers. Such a transition requires well-defined institutional structures to strike a precise balance between the capacity of sub-national governments to generate resources for their own expenditures and the need to ensure the equitable provision of public goods and services across the national territory. National authorities can redistribute resources across sub-national entities by collecting them at the central level and allocating top-down grants, the so-called vertical grants. They can also equalize resources by requiring sub-national governments to share among themselves based

on local needs, in the form of horizontal grants. Ultimately, the goal of equalization can be achieved through a combination of these two approaches. In either case, intergovernmental grants, whether vertical or horizontal, represent one of the primary instruments for redistributing national resources among sub-national governments (Moisio and Bover, 2023).

While one might generally assume that the source of income should not influence the behavior of local policymakers, both economic theory and empirical evidence challenge this conclusion. Positive shocks in central government grants reduce the efficacy of local institutions by lowering the quality of candidates in local elections (Brollo et al., 2013). Besley (2006) argues for decentralization based on a trade-off between the coordination benefits of centralized government and the accountability of local governments. Similarly, Oates (2008) review of second-generation fiscal theories highlights the same issue, emphasizing that reliance on local resources enhances the accountability of incumbent candidates. More fiscally autonomous local governments provide a similar level of services with lower expenditures, offering evidence that local governors tend to behave more responsibly when they are less dependent on central grants (Boetti et al., 2012).

Moreover, the literature on the so-called flypaper effect (see Inman (2009), for a review) provide evidences that funds received from external grants lead to greater public expenditures compared to those collected through local taxation. For instance, Bracco et al. (2015) identified a flypaper effect in Italian municipalities of approximately 0.4, meaning that each additional euro in grants results in 0.4 euro more in local expenditures compared to an additional euro raised through local personal income taxes. Similarly, Dahlberg et al. (2008) found that

additional federal grants in Sweden increases local spending, but leaves local tax rates unchanged.

The evidences collected thus far indicates that local policymakers are not neutral with respect to revenue sources. However, understanding how intergovernmental grants influence the fiscal policies of sub-national governments has proven to be challenging, as such transfers may depend on both the individual characteristics of politicians and local factors within the sub-national unit.

For example, there are strong evidences that politicians aligned with the central government receive more grants than those belonging to opposition parties, particularly in the lead-up to local elections. This pattern has been observed in various countries, including Italy, India, Norway, Spain, and Sweden (Bracco et al. (2015); Curto-Grau et al. (2018); Arulampalam et al. (2009); Sørensen (2003); Johansson (2003)). Moreover, in some cases, such as for Italy, intergovernmental transfers are tied to previous local expenditures (Shah, 2006), which in turns are affected by those same grants, introducing concerns for potential reverse causality biases in the analysis.

The primary goal of this paper is to deepen our understanding of how municipal governments adjust their budget decisions in response to variations in intergovernmental grants. Specifically, it explores whether an influx of external resources translates into a greater change in public expenditures compared to an equivalent change in internal, own-source revenues.

To contextualize and clarify these dynamics, the first part of this paper reviews the extensive literature on fiscal federalism and the effects of central grants. Moving from foundational normative modelsâwhich assume revenue neutrality and benevolent policymakingâto second-generation political economy theories,

the literature reveals a complex web of behavioral distortions and institutional frictions. A central empirical regularity in this field is the “flypaper effect,” where external funds stick to the public sector, systematically expanding local budgets rather than being returned to citizens through tax cuts. The literature attributes this phenomenon to fiscal illusion and principal-agent dynamics, illustrating how self-interested local politicians often exploit information asymmetries to maximize highly visible, short-term public expenditures. Furthermore, heavy reliance on external transfers is shown to weaken local accountability, incentivize political opportunism, and structurally alter the composition of locally provided services.

Building directly on these theoretical premises, I conduct an empirical exercise to test these behavioral responses in the context of Italian municipalities in the period 2016 to 2019. By analyzing municipal budgets, this study investigates how variations in central grants relate to changes in local revenues, expenditures, and the specific allocation of resources across locally provided services. To mitigate the confounding biases widely recognized in the literature—such as the endogeneity of grant allocation and political favoritism—I exploit an instrumental variable derived from the 2015 Italian municipal finance reform. This reform redistributed equalization transfers based on standardized measurements of local fiscal capacity and expenditure needs, which were exogenously computed by a technical committee. I construct an instrument based on these standardized criteria and employ it within a baseline Two-Stage Least Squares (2SLS) framework, incorporating municipal and year fixed effects, alongside the full set of covariates employed in the calculation of the standardized expenditure needs and mayor-level controls.

Through this main empirical framework, I test whether variations in inter-governmental grants: (*i*) relate to changes in overall current and capital expendi-

tures; (ii) induce crowding-out effects on own-source revenues; and (iii) shift the composition of the budget across locally provided public services (e.g., education, environment, administration).

To ensure the validity of the baseline estimates, the analysis includes an additional robustness check using an overidentified model. This specification incorporates historical municipal expenditures from 1979 as a second excluded instrument. Drawing on prior evidence from Italian sub-national grants (Chiades et al., 2019), this historical baseline proves highly relevant in explaining current intergovernmental transfers. Because part of the effective grant calculation remains path-dependent even after 2015, this supplementary instrument increases the precision of the estimates and helps mitigate potential concerns regarding unobservable, time-varying local characteristics.

The empirical results align closely with the theoretical predictions of political opportunism in grant-dependent municipalities (Boetti et al., 2012; Besley, 2006). Specifically, increased transfers are associated with a significant rise in short-term current expenditures, while capital investments remain largely unaffected. Simultaneously, the estimated coefficients on own-source revenues show no evidence of downward adjustments in local tax rates in the short run. Furthermore, a heterogeneity analysis reveals that this expansionary spending response is substantially stronger among mayors who are less politically experienced, possess a larger electoral mandate, or are highly educated. This suggests that the magnitude of the flypaper effect is significantly mediated by the individual political incentives, electoral constraints, and administrative capacity of local policymakers.

Regarding budget composition, the results indicate that increased transfers correspond to a higher share of spending on culture, civil aid, and social policies,

alongside a relative decrease in the shares dedicated to public order, education, and transportation.

Overall, these findings lend strong empirical support to the flypaper effect literature, confirming that external revenues stimulate local spending more than tax-based revenues and actively shape the internal composition of municipal budgets. Ultimately, this analysis underscores a critical trade-off for policymakers designing fiscal decentralization: while central transfers are necessary to ensure the equitable provision of services, they inevitably introduce behavioral distortions that must be carefully balanced against the need to preserve local accountability.

The rest of the paper is organized as follows: Section 2 presents an extensive review of the literature, highlighting the core foundational theory and outlining the main subject of interest within fiscal decentralization researches, as well as providing additional evidences supporting the empirical results found in this paper; Section 4 explains the identification strategy of the empirical exercise; Section 3 provides descriptive statistics of the available dataset; in Section 5 I present the main empirical results of the analysis; Section 6 offers some concluding remarks.

2 Literature review

The architecture of fiscal federalism, which governs the division of fiscal responsibilities among different levels of government, is a central topic in public economics. Intergovernmental grants are a primary instrument within this architecture, used by central governments to influence sub-central fiscal policy, address vertical fiscal imbalances, and pursue equity objectives. A large body of the political economics literature has sought to understand the theoretical underpinnings and empirical

consequences of these transfers. This section aims at organizing key contributions to this field and providing evidence of the distortion introduced by intergovernmental grants.

2.1 Theoretical Foundations of Fiscal Federalism

A foundational element within this theoretical landscape is the work of [Bradford and Oates \(1971\)](#). They develop a formal model to analyze the effects of unconditional grants on collective fiscal decisions. Their central contribution is the theoretical proposition that, under a frictionless political process, a lump-sum grant to a local government is equivalent in its allocative and distributive effects to a set of lump-sum grants made directly to its individual citizens. This equivalence theorem implies that grants should be treated like any other form of community income, leading to a mix of increased public spending and tax relief dictated by residents' preferences.

[Oates \(1999\)](#) offers a comprehensive review of these "first-generation" theories. The author outlines the core principles of fiscal federalism, most notably the Decentralization Theorem. This theorem posits that, in the absence of externalities, public goods are most efficiently provided by the most local level of government, as it can better tailor services to the specific needs of its citizens. Furthermore, the study highlights the economic rationale behind intergovernmental grants. These include addressing situations where local policies affect neighboring regions (inter-jurisdictional spillovers), ensuring a fair distribution of resources across territories (fiscal equalization), and leveraging the central government's greater efficiency in tax collection.

The theoretical framework has since evolved to account for more complex institutional realities. Oates (2005) and Oates (2008) describe this shift toward “second-generation” theories of fiscal federalism. While earlier models assumed that policymakers act purely as “benevolent” public servants seeking to maximize overall social welfare, these newer theories adopt a more realistic political economy perspective. They recognize that public officials also respond to personal and political incentives, such as securing re-election or expanding their budgets.

Consequently, the focus shifts to how institutional design and political processes shape economic results. A key concept explored in this literature is the “soft budget constraint.” This phenomenon occurs when local governments overspend or accumulate debt with the expectation that the central government will eventually bail them out. This behavior is often the result of “imperfect inter-governmental commitment mechanisms”—meaning the central authority cannot credibly commit to withholding a financial rescue. By accounting for these dynamics, the second-generation framework provides a more nuanced theoretical lens for understanding the inefficiencies and empirical regularities actually observed in decentralized systems.

2.2 The flypaper effect

Despite the theoretical prediction derived from the Bradford and Oates model—which posits that unconditional grants and increases in private income should have identical effects on local spending—empirical research has consistently uncovered a significant deviation. Hines Jr and Thaler (1995) provide a seminal review of this phenomenon, famously termed the “flypaper effect” by Arthur Okun be-

cause “money sticks where it hits” (Inman, 2009). Specifically, this anomaly highlights that intergovernmental grants stimulate public spending far more than an equivalent increase in local citizens’ private income.

In their survey of numerous studies, Hines Jr and Thaler (1995) note that the estimated increase in public spending from an additional dollar of grant revenue typically ranges from 0.25 to over 1.00. This is remarkably higher than the theoretically expected 5 to 10 cents. As the authors explain, this 5-10 cent benchmark is rooted in simple economic theory: a lump-sum grant to a local jurisdiction is theoretically equivalent to a community-wide increase in resident income. Therefore, residents should allocate this additional wealth just as they would any other income increase. The share devoted to public goods and services should merely reflect the local marginal propensity to spend out of income, which typically hovers around 5 to 10 cents on the dollar. The remainder should theoretically be absorbed by private consumption through equivalent tax cuts.

The authors argue that this stark discrepancy cannot be entirely dismissed as a mere artifact of econometric misspecification. Early critiques suggested the flypaper effect might just be a statistical illusion driven by endogeneity—for instance, if central governments systematically direct grants to municipalities that already have unobservably high spending needs—or by omitted variable bias regarding local political preferences. However, studies correcting for these methodological issues still find a robust flypaper effect.

Instead, the literature heavily points toward behavioral explanations. One primary mechanism is “fiscal illusion”: citizens may not be fully aware of the influx of external grant money, causing them to underestimate the true marginal cost of public services and fail to demand the tax cuts that rational models predict.

Another key factor is a lack of fungibility, often linked to “mental accounting.” Rather than pooling all revenues together, policymakers and bureaucrats tend to psychologically compartmentalize grant money, treating it as a separate budget implicitly earmarked for public projects rather than as a resource that can substitute for local taxation.

Inman (2009) offers a comprehensive review of the flypaper effect, contributing a detailed taxonomy of its potential causes. The author categorizes behavioral explanations into two broad groups: those consistent with voter preferences (such as models where grants effectively lower the perceived tax price of public services) and those driven by the interests of government officials. The latter group relies on principal-agent models, wherein self-interested bureaucrats or politicians exploit voters’ ignorance about external grant revenues to illegitimately expand local budgets.

To disentangle the relative contribution of these two competing explanations, Inman argues that empirical research must examine the role of political institutions and voter information. If the flypaper effect were purely a reflection of genuine voter demand, its magnitude should remain relatively stable regardless of the local political framework. However, if it stems from the opportunistic behavior of public officials, the effect should weaken when institutions promote greater transparency, allow for direct democracy, or enable stronger voter monitoring.

Reviewing the empirical evidence, the study highlights that institutional variation is indeed the key to disentangling these forces. The flypaper effect tends to shrink significantly in municipalities with open town meetings, strict financial disclosure rules, or highly competitive elections. Therefore, while econometric issues like the endogeneity of grants are important to address, the remaining ro-

bust flypaper effect is most compellingly explained by principal-agent dynamics and fiscal illusion, where politicians actively leverage asymmetric information to maximize public spending.

Recent causal evidence continues to confirm the existence of the flypaper effect. [Dahlberg et al. \(2008\)](#) use a regression discontinuity design to study a grant program in Sweden where municipalities with net out-migration above a 2% threshold receive additional funds. The grant effect leads to a significant increase in local spending ("crowding-in") but has no statistically significant effect on local tax rates. Similarly, [Köthenbürger and Loumeau \(2023\)](#) employ a regression kink design in the Swiss canton of Vaud, also finding that unconditional grants lead to a nearly one-for-one increase in public expenditures with no corresponding reduction in local income taxes. The effect is not limited to general-purpose grants. [Leduc and Wilson \(2017\)](#) found no evidence of crowding-out responses in terms of own revenues when U.S. states experienced an increase in federal transfers for highway-related spending. On the contrary, they observed a crowding-in effect over a three-year period, where a one-dollar increase in transfers led to more than a two-dollar increase in highway-related expenditures. Conversely, [Knight \(2002\)](#) contested empirical results on the flypaper effect anomaly by modeling federal grants as the outcome of a legislative bargaining process. His empirical strategy uses the political power of state congressional delegations as an instrument for federal highway grants to U.S. states. He argues that, after accounting for endogeneity, federal highway grants actually "crowd-out" state-funded highway spending, a reversal of the typical flypaper finding, suggesting the flypaper effect could be a statistical artifact of omitted variables.

A significant branch of the literature explores how political factors shape both

the allocation and the effect of grants. [Johansson \(2003\)](#) investigates whether political variables influence the distribution of intergovernmental grants in Sweden. He uses a panel dataset of Swedish municipalities to show that grants are not allocated solely based on formula-based criteria. Political representation in the national parliament and the political color of the local government significantly affect the amount of grants received, indicating that grant allocation is a political process. [Sørensen \(2003\)](#) reaches a similar conclusion in the Norwegian context. Analyzing panel data for municipalities, he tests various political economy models of grant allocation, concluding that both the ruling party at the national level and the representation of local interests in parliament play a significant role in grant distribution. This partisan alignment can also be influenced by electoral incentives. [Curto-Grau et al. \(2018\)](#) study whether electoral competition curbs party favoritism in the allocation of grants from regional governments in Spain using a regression discontinuity design based on close elections. They found that while partisan favoritism in grant allocation is significant, this bias is substantially reduced in municipalities where the incumbent faces stronger electoral competition, concluding that electoral accountability can mitigate the politicization of grant distribution. In a study of Italian municipalities, [Bracco et al. \(2015\)](#) develop a political agency model where intergovernmental grants act as a tool for the central government to signal the competence of politically aligned local incumbents. They test this theory using a regression discontinuity design that exploits the outcomes of very close mayoral elections. The empirical results support their predictions, showing that aligned municipalities receive significantly more grants—on average, 40% more than unaligned municipalities—and that this effect is stronger in the run-up to elections. This, in turn, increases

the re-election probability of an aligned incumbent mayor by approximately 30%. They also use political alignment as a novel instrument to estimate the flypaper effect, finding that each euro from a grant increases local public spending by about 0.4 euros. The political environment not only affects grant allocation but also mediates its impact, offering a potential explanation for the flypaper effect. [Boetti et al. \(2012\)](#) analyze the flypaper effect in Italian municipalities during a period of fiscal decentralization. Using panel data, they find a significant flypaper effect and show that its magnitude is influenced by the degree of fiscal autonomy and the political structure of the municipality. In summary, the literature establishes the flypaper effect as a robust empirical phenomenon that consistently defies classical economic predictions. The accumulated evidence points to three main conclusions regarding the nature, mechanics, and allocation of intergovernmental grants. First, the persistent tendency of grant money to “stick where it hits” is rarely a mere econometric artifact; rather, it is fundamentally driven by fiscal illusion and principal-agent dynamics. Self-interested policymakers leverage asymmetric information and mental accounting to expand local public budgets instead of passing the financial windfall back to citizens through tax cuts.

Second, the magnitude of this effect is highly sensitive to the local institutional context. As the literature demonstrates, mechanisms that enhance transparency, political competition, and voter monitoring can significantly constrain the opportunistic expansion of public spending by local officials. Finally, the distribution of central transfers itself is rarely a purely formulaic exercise based on objective local needs. Instead, it is deeply embedded in the political process and heavily distorted by political economy factors, such as partisan alignment between tiers of government and strategic electoral incentives.

Ultimately, this body of literature highlights that evaluating the true impact of intergovernmental transfers requires moving beyond standard economic models to fully account for the complex behavioral, political, and institutional environments in which local policymakers operate.

2.3 Asymmetries and Frictions in Local Fiscal Adjustment

The Oates' model predicts that shock in revenues from external sources translates into equivalent crowding-out adjustment in local taxation. However, fiscal adjustments to shocks are often neither immediate nor symmetrical. For instance, local governments may react differently to positive and negative fiscal shocks. [Deller and Maher \(2006\)](#) analyze Wisconsin's unconditional shared revenues program and find evidence of "fiscal replacement." Using a model of structural change, they conclude that when aid is cut, local governments tend to replace the lost funds with own-source revenues rather than cutting spending proportionately. [Rattsø and Tovmo \(2002\)](#) investigate the fiscal behavior of Danish local governments using a dynamic panel data model. They also find evidence of asymmetric adjustment to shocks in grant allocations, concluding that local governments exhibit fiscal discipline in response to negative grant shocks by cutting expenditures, but utilize positive shocks both to increase spending and to accumulate financial savings. However, the evidence on asymmetry is not unanimous. [Gamkhar and Oates \(1996\)](#) conduct a time-series analysis of U.S. state and local government expenditures from 1953 to 1991. Their contribution is a direct test for asymmetries in the response to changes in federal grants. In contrast to other studies, they find no significant evidence of an asymmetric response; the stimulative ef-

fect of a one-dollar increase in grants is statistically indistinguishable from the contractionary effect of a one-dollar decrease, with the flypaper effect operating on both directions.

Recent work suggests that these conflicting findings may be reconciled by taking a longer-term view. [Helm and Stuhler \(2024\)](#) provide causal evidence on these dynamics using a "Census Shock" in Germany for identification. Tracking the fiscal responses over an extended time horizon, the authors show that the flypaper effect is primarily a transitional, short-run phenomenon rather than a permanent behavioral anomaly.

To explain this, the study breaks down the timing of fiscal adjustments. They find that local governments adjust their public spending relatively quickly, typically within five years of experiencing a grant shock. In contrast, adjustments to local tax rates are highly protracted and "sticky," taking over a decade to fully materialize. Consequently, in the short run, positive grant shocks translate almost entirely into higher public spending because local policymakers delay cutting taxes, thereby generating the classic flypaper effect. In the long run, however, local governments eventually adjust their tax rates downward to substitute for the external grant revenue. Once this delayed tax adjustment occurs, the flypaper effect dissipates, and the fiscal response aligns with standard economic theories of crowding-out.

Furthermore, the study reveals that the fiscal response is asymmetric in the short run—with municipalities reacting more swiftly to revenue losses than to revenue gains—but becomes symmetric over the long run. This dynamic suggests that such asymmetries reflect temporary institutional or political frictions rather than ingrained behavioral biases.

The interpretation of local fiscal behavior inherently depends on the degree of effective autonomy held by municipalities. Specifically, fiscally constrained local governments are significantly less capable of responding to shocks in grant allocations because their limited fiscal autonomy restricts their policy options. Lacking the flexibility to adjust their own revenues downward, these municipalities are unable to enact a proper crowding-out response, which inevitably amplifies the flypaper effect.

Building on this distinction, [Mauri \(2024\)](#) proposes a novel empirical test to differentiate between genuinely “fiscally constrained” and “fiscally autonomous” behavior. The study develops a tax competition model predicting that a constrained government, when experiencing a revenue windfall, will respond with larger tax cuts if the perceived mobility of its tax base is high. Applying this framework to a transfer reform in a Swiss canton, the author uses land-use reserves as a proxy for taxpayer mobility. The empirical results confirm the model’s predictions: municipalities with more available residential land—and thus a greater capacity to attract new residents—enact significantly larger tax cuts. This demonstrates that local responses to central grants are shaped not only by formal fiscal autonomy but also by the competitive geographic environment in which municipalities operate.

Taken together, this strand of literature highlights that municipal responses to intergovernmental grant shocks are neither instantaneous nor uniform. Rather, they are fundamentally shaped by temporal frictions and structural constraints. While short-run adjustments frequently exhibit asymmetries and produce classic flypaper effects—largely due to the political “stickiness” of tax policies—these behavioral anomalies tend to dissipate over longer horizons as local governments

eventually adjust their revenue structures. Crucially, the capacity to enact these fiscal adjustments is inherently bound by a municipality’s effective fiscal autonomy and its competitive geographic positioning. Therefore, accurately evaluating the impact of central transfers requires a nuanced approach that explicitly accounts for both the temporal dynamics of the revenue shock and the local constraints that dictate policymakers’ available policy options.

2.4 Consequences of Decentralization and Grant Allocation

A crucial question is whether intergovernmental transfers achieve their intended goals. [Litschig and Morrison \(2013\)](#) evaluate a large, formula-based federal transfer program in Brazilian municipalities. They find that the grants lead to significant improvements in education outcomes, including higher school attendance and literacy, and contribute to poverty reduction. [Gordon \(2004\)](#) provides a causal analysis of the U.S. federal Title I program for low-income students. Using discontinuities in grant allocation arising from Census updates for identification, the study concludes that while grants initially boosted school spending, there was substantial crowd-out within three years, raising questions about the program’s long-term effectiveness. [Bianchi et al. \(2023\)](#) study a 1993 Italian reform that increased the fiscal autonomy of municipalities by replacing central government transfers with revenues from a new local property tax. Leveraging plausibly exogenous variation in the tax base created by WWII bombing, they find that this fiscal decentralization induced local politicians to cut wasteful spending while expanding key public services, such as nursery schools. This expansion of childcare,

in turn, led to a significant increase in female labor supply—by up to 20 percent in the most affected municipalities—thereby reducing the gender gap in employment. Examining a later period of fiscal consolidation in Italy, [Chiades et al. \(2019\)](#) uncover a perverse effect of centrally imposed austerity measures. They argue that when faced with cuts to intergovernmental transfers, municipalities react by increasing their expenditure arrears, effectively using non-payment to suppliers as a form of non-conventional, short-term debt to ease financial constraints. To address endogeneity, the authors employ a novel instrumental variable strategy, using the level of transfers allocated to municipalities after a major, exogenous public finance reform in 1978 as an instrument for current transfers. Their findings show that a 10% reduction in intergovernmental transfers leads to a statistically significant increase of at least 1.7% in arrears on investment expenditure, highlighting how centrally mandated fiscal consolidation can lead to a “camouflaging” of local government debt.

The source of public funds can have profound effects on governance. [Gadenne \(2017\)](#) examines how government accountability in Brazilian municipalities is affected by revenue source. Using an RDD based on grant eligibility rules, she shows that when local governments receive more grant revenue, they reduce their local tax effort and provide fewer public goods. The conclusion is that reliance on external transfers can weaken the link between citizens and their government, reducing accountability. This can have severe consequences, including corruption. [Brollo et al. \(2013\)](#) investigate the link between federal transfers and political corruption in Brazil, terming their finding the “political resource curse.” Their RDD-based empirical strategy shows that larger transfers lead to a significant increase in political corruption, especially for mayors not constrained by re-election

incentives. The overarching effects of decentralization are summarized by [Faguet \(2014\)](#) in a comprehensive review, highlighting that the outcomes of decentralization are highly contingent on context. Successful decentralization requires not just the devolution of authority but also strong mechanisms for local accountability to make it effective.

The relationship between decentralization and economic growth remains ambiguous. [Baskaran and Feld \(2013\)](#) find no robust positive relationship between fiscal decentralization and economic growth in OECD countries when using refined measures of autonomy. [Bodman \(2011\)](#) reinforces this finding, suggesting the relationship may be non-linear or even negative when using more sophisticated indicators.

In synthesis, the empirical evidence underscores that the developmental and governance outcomes of intergovernmental transfers are highly complex and context-dependent. While targeted grants can successfully achieve specific social objectives—such as improving educational outcomes and reducing poverty—their long-term efficacy is frequently compromised by crowding-out effects. Crucially, the literature highlights a fundamental trade-off regarding the source of municipal financing: heavy reliance on external transfers tends to sever the accountability link between citizens and policymakers. This dependency can foster political corruption and incentivize opaque financial practices, such as the strategic accumulation of expenditure arrears during periods of centrally mandated austerity. Conversely, shifting toward genuine fiscal autonomy often encourages more efficient public spending and the expansion of key services. Ultimately, because the broader macroeconomic benefits of decentralization remain ambiguous, the academic consensus suggests that merely devolving financial resources is insufficient;

successful decentralization must be strictly coupled with robust mechanisms for local accountability.

2.5 Summary and Implications for the Empirical Analysis

The overarching narrative emerging from this body of literature fundamentally challenges the traditional assumption of revenue neutrality in local public finance. Moving from the first-generation normative models to the second-generation theories of fiscal federalism, it becomes clear that the source of municipal income inextricably dictates the behavior of local policymakers. Intergovernmental grants do not merely act as an exogenous influx of community wealth; rather, they introduce a complex web of behavioral distortions, temporal frictions, and political opportunism.

By connecting the robust evidence of the flypaper effect with the dynamics of local fiscal adjustment, a clear behavioral pattern emerges. Because local tax rates are politically costly and administratively “sticky”, and because fiscal illusion shields politicians from immediate voter backlash, local administrators facing a positive grant shock are strongly incentivized to expand budgets rather than enact tax cuts. Furthermore, as the literature on accountability and the “political resource curse” demonstrates, managing external funds rather than own-source revenues severs the link of accountability between citizens and their local government. Consequently, self-interested policymakers are more likely to channel these grant revenues toward highly visible, short-term current expenditures to maximize immediate political consensus, while leaving long-term capital investments largely unaffected.

Crucially, this theoretical framework suggests that the influx of central transfers does not simply inflate the size of local budgets symmetrically, but actively distorts their internal composition. Since fiscally constrained municipalities lack the autonomy to optimize their revenue structures, their reliance on external grants structurally reshapes their spending priorities across different public services.

Therefore, the empirical exercise conducted in this paper is deeply anchored in, and supported by, these theoretical premises. By investigating how variations in intergovernmental grants affect current expenditures, capital investments, and the specific allocation of resources across locally provided services (such as culture, social policies, and education), this analysis provides a direct empirical application of second-generation fiscal federalism in the Italian context. Furthermore, recognizing the literature's broad consensus on the endogeneity and political manipulation of grant allocations, the adoption of an instrumental variable approach—exploiting the 2015 institutional reform—represents a concerted effort to mitigate these longstanding confounding factors. While this empirical strategy is not without its limitations, it provides a valuable framework to explore the behavioral responses of local policymakers to external revenue shocks, thereby complementing and expanding upon the extensive evidence already established in the field.

3 Data and descriptive evidence

Firstly, to conduct my analysis, I focused on the period 2016–2019. There are two main reasons for this choice. On one hand, reliably comparing budget com-

position and transfer data before and after 2015 is empirically unfeasible due to a comprehensive accounting reform implemented by the Italian government. This reform fundamentally overhauled the reporting structure of sub-national budgets by replacing the traditional expenditure classification—historically organized by “functions”—with a new, broader framework structured around “missions”. Because these newly defined missions frequently aggregate, split, and reclassify expenditures under entirely different accounting headings compared to the previous system, there is no direct one-to-one correspondence between the old and new regimes. Consequently, any attempt to harmonize these variables across the reform threshold would necessitate highly arbitrary assumptions regarding the mapping of specific budget items, thereby compromising the reliability of the empirical analysis. Additionally, any effect captured by my model might be biased by the mechanical impact of this reporting reform.¹ At the other end of the temporal sample, data from 2020 onward were deliberately excluded to avoid confounding the analysis with the severe fiscal anomalies induced by the COVID-19 pandemic. The primary rationale for this restriction is the fact that the pandemic generated massive, non-random exogenous shocks to local public finances. During this emergency period, the central government deployed an unprecedented volume of extraordinary intergovernmental transfers that were entirely decoupled from the standard allocation criteria. Consequently, retaining these years would severely distort the empirical estimates, as the observed variations in municipal budgets would reflect ad-hoc crisis management and emergency relief rather than

¹This budget reform also precludes the use of identification strategies relying on parallel trends assumptions, such as Difference-in-Differences (Diff-in-Diff) or Difference-in-Discontinuity Design (Diff-in-Disc).

the structural, behavioral responses to standard equalization grants.²

The sample was further restricted to municipalities in regions with Ordinary Statutes.³ Additionally, data regarding the structural determinants of local expenditure (the so-called standardized spending needs, which will be extensively discussed in Section 4) was unavailable for approximately 200 municipalities in 2015. Thus, the main sample comprises around 6,460 municipalities out of 6,784 in Ordinary Statute regions, yielding approximately 25,700 observations.

The two primary data sources for the analysis include detailed municipal budgetary information on revenues and expenditures for the period of interest,⁴ as well as two additional datasets related to the 2015 institutional reform: one detailing the components of the municipal solidarity fund (Fondo di Solidarietà Comunale, hereafter FSC) and another containing the local determinants of the aforementioned standardized spending needs.⁵ To increase the precision of the estimates and expand on potential heterogeneity in the effect of transfers, I retrieved some individual characteristics of mayors from the Italian Ministry of the Interior.⁶ Specifically, I included control variables for gender, age at the date

²To expand the analysis to the post-2020 period, additional controls would be required, accounting for the municipal effects of COVID-19 and the pandemic-related transfers received from the government. This remains an area for future research.

³These regions have special status, and their unique regional covariates changing over time could introduce biases not captured by a Fixed-Effects model.

⁴Municipal budget data was sourced from AIDA PA, a third-party company managed by Moody's. The company consolidates administrative budget data for local entities as provided by the Italian Ministry of Economy and Finance.

⁵Both datasets are publicly available on the Opencivitas website: <https://www.opencivitas.it/it/open-data>. Opencivitas is managed by the technical committee responsible for defining the structural municipal needs. The platform was established as part of the 2015 local finance reform (detailed in Section 4) to enhance transparency. Determinants of local standardized demand are available for the entire period, but data on FSC components is missing for 2015 and 2016. For these missing years, the data were kindly provided by Francesco Porcelli, a member of the Technical Committee. The validity of this source was confirmed by comparing overlapping years, yielding perfect matches.

⁶Available at: <https://dait.interno.gov.it/elezioni/anagrafe-amministratori>.

of election, education measured as a dummy accounting for graduated mayors, years of experience in politics, votes received at previous elections, and a dummy accounting for mayors born in the city where they are elected. In the final dataset I also included information on per capita intergovernmental grants from 1979.⁷

Table [1](#) presents summary statistics for the main sample in the period 2016â2019.⁸ for the entire sample and by standard Italian's macro-area. Municipalities received in form of grants, on average, around 200 Euro per capita, accounting for 20% of their current revenues. Despite half of current revenues being sourced internally, grants constitute a significant component of municipal budgets and vary considerably across cities, underscoring their importance in explaining differences in spending behavior. Souther regions received on average more grants per capita compared to norther and central regions and collected less revenues from taxes, resulting in an higher share of revenues derived from central sources. Conversely, regions in the South displays an above average current and capital per capita expenditures, with the latter being more than twice of the other two groups.

⁷These data were kindly provided by Luigi Moretti

⁸Additionally, I included summary statistics for share of current and capital expenditures, as well as for local determinants of standardized spending needs in Appendix [E](#)

Table 1: Summary statistics

	South	Centre	North	Total
Current intergovernmental transfers	283.4 (345.2)	233.0 (544.0)	155.7 (158.8)	201.8 (306.0)
Current intergovernmental transfers, share	26.87 (15.19)	18.23 (11.68)	17.87 (10.67)	20.37 (12.83)
Current tax revenues	435.0 (232.3)	547.4 (272.0)	537.8 (420.3)	511.2 (361.1)
Current tax revenues, share	46.58 (17.82)	52.42 (15.26)	57.13 (12.72)	53.57 (15.34)
Current no-tax revenues	222.8 (350.8)	272.4 (402.4)	234.9 (370.0)	237.1 (370.2)
Current no-tax revenues, share	18.45 (12.10)	21.25 (11.87)	21.36 (10.56)	20.55 (11.27)
Current Expenditures	910.6 (618.1)	1024.8 (821.5)	835.1 (592.7)	883.4 (641.5)
Capital Expenditures	708.8 (2657.7)	293.2 (659.0)	298.9 (700.4)	409.7 (1518.2)
Observations	7,088	3,804	15,080	25,972

Notes. The table reports means, and standard deviations in parentheses, of Euro per capita and share of municipal current and capital revenues and expenditures. Means are calculated for the main sample of analysis and the three sub-samples of the main Italian macroareas.

Table 2 reports similar summary statistics, but separated in five different population classes. Central government grants are much more relevant in small municipalities with population below 5,000 inhabitants, both in per capita terms and as share of total current revenues. Same population groups also show higher level of current and capital expenditures.

Table 2: Summary statistics by population class

	Below 1000	1000-5000	5000-15000	15000-100000	Above 100000	Total
Current intergovernmental transfers	333.3 (484.1)	189.5 (245.8)	111.7 (104.3)	121.3 (65.25)	194.1 (89.50)	201.8 (306.0)
Current intergovernmental transfers, share	24.50 (15.65)	21.76 (12.55)	15.25 (8.570)	15.02 (7.724)	15.77 (7.160)	20.37 (12.83)
Current tax revenues	630.3 (475.8)	463.9 (352.2)	468.1 (239.8)	510.3 (166.5)	685.1 (169.8)	511.2 (361.1)
Current tax revenues, share	47.33 (17.55)	52.34 (15.05)	60.34 (11.16)	60.11 (9.683)	55.56 (8.031)	53.57 (15.34)
Current no-tax revenues	390.9 (628.3)	202.5 (247.5)	160.1 (133.9)	169.7 (105.1)	286.3 (181.7)	237.1 (370.2)
Current no-tax revenues, share	22.44 (13.91)	20.22 (11.06)	19.63 (8.991)	19.19 (8.443)	21.34 (7.346)	20.55 (11.27)
Current Expenditures	1262.1 (972.1)	799.2 (492.2)	682.5 (297.2)	735.1 (199.4)	1090.6 (319.0)	883.4 (641.5)
Capital Expenditures	944.2 (2909.3)	299.9 (520.1)	152.3 (150.4)	128.8 (226.6)	151.7 (86.30)	409.7 (1518.2)
Observations	6,386	11,441	5,597	2,403	145	25,972

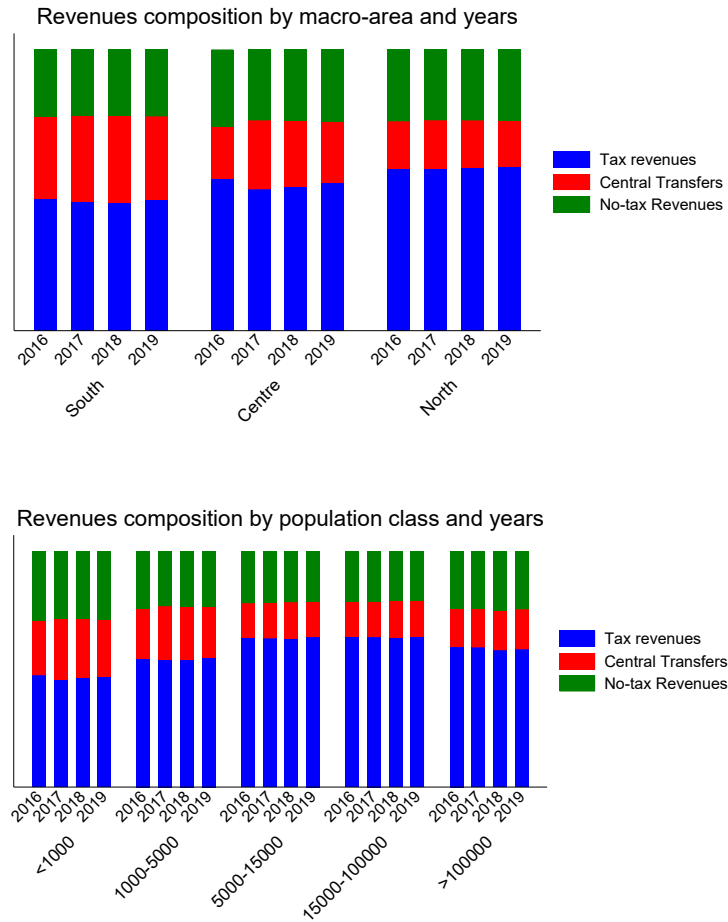
Notes. The table reports means, and standard deviations in parentheses, of Euro per capita and share of municipal current and capital revenues and expenditures. Means are calculated for different population classes: below 1,000 inhabitants; between 1,000 and 5,000 inhabitants; between 5,000 and 15,000 inhabitants; between 15,000 and 100,000 inhabitants; above 100,000 inhabitants.

All pattern identified in current revenues composition, both in terms of macro-areas and population classes, appears to be stable over time in the period of interest, as show in Figure [1](#).

Figure [2](#) shows linear trends for current revenues, current and capital expenditures in Euro per-capita. Both current revenues from taxes and current expenditures experienced a small but steady increase in the period considered. On the other hand, current revenues from other internal sources unrelated to taxation remain stable for the four years considered. Central transfers rises between 2016 and 2017, but stabilized around the average of 200 euros per capita in subsequent years. Only capital expenditures displays a reduction between 2016 and 2018, but rises significantly in 2019 compared to the starting year of the analysis.

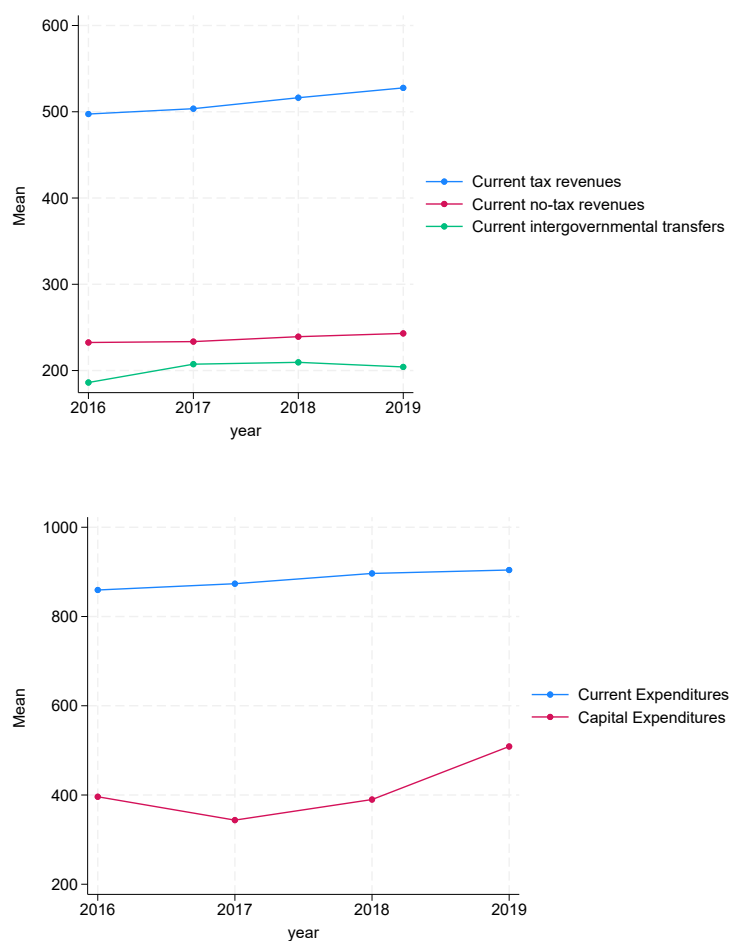
Before delving into the formal empirical model, examining the raw correla-

Figure 1: Current revenues composition by year



Notes. The graph reports the percentage share of tax revenues, central transfers and no-tax revenues on the total revenues for each year of the period of analysis 2016-2019. First figure reports the share for the three macro-areas. Second figure reports the share for each population class.

Figure 2: Year Trends



Notes. The graph reports the trend in Euro per-capita revenues and expenditures in the period of analysis.

tions between central transfers and the main municipal fiscal outcomes provides a valuable baseline. This preliminary graphical inspection not only illustrates the apparent statistical relationships in the data but also visually highlights the very endogeneity and simultaneity puzzles that the subsequent empirical strategy must address.

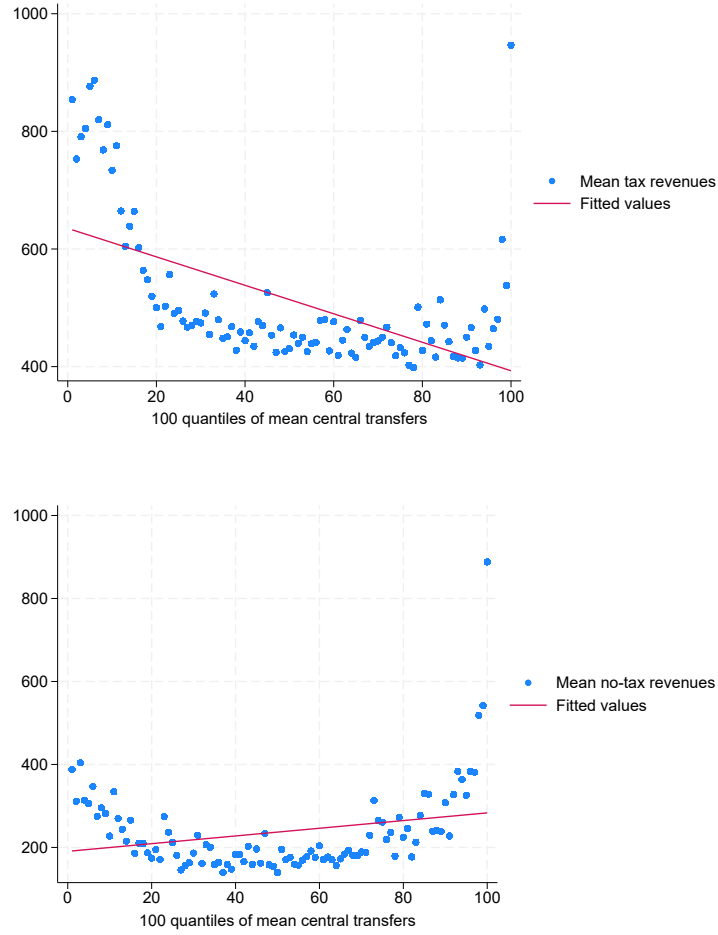
Figures 3 and 4 display the linear correlations between current central transfers and key municipal outcomes, namely current tax and non-tax revenues, as well as current and capital expenditures. The data suggest a positive relationship between central transfers and both types of expenditures, although the fitted regression line for current expenditures exhibits a relatively modest slope. A similar pattern emerges when examining the correlation with non-tax current revenues.

Conversely, there is a distinct negative correlation between local tax revenues and intergovernmental transfers. While this might initially be interpreted as naive evidence of a crowding-out effect—suggesting that external transfers reduce local tax effort—it strongly underscores the hypothesis of reverse causality discussed in the previous section. Specifically, municipalities endowed with larger tax bases naturally collect more tax revenues while simultaneously receiving fewer equalization grants, since the national equalization framework inherently penalizes wealthier municipalities.⁹

Turning to budget composition, Figure 5 presents the shares of current and capital expenditures dedicated to the most relevant municipal missions. For this visualization, the sample is partitioned into five subgroups based on the quintiles of central current transfers. The graphical evidence suggests that higher levels of

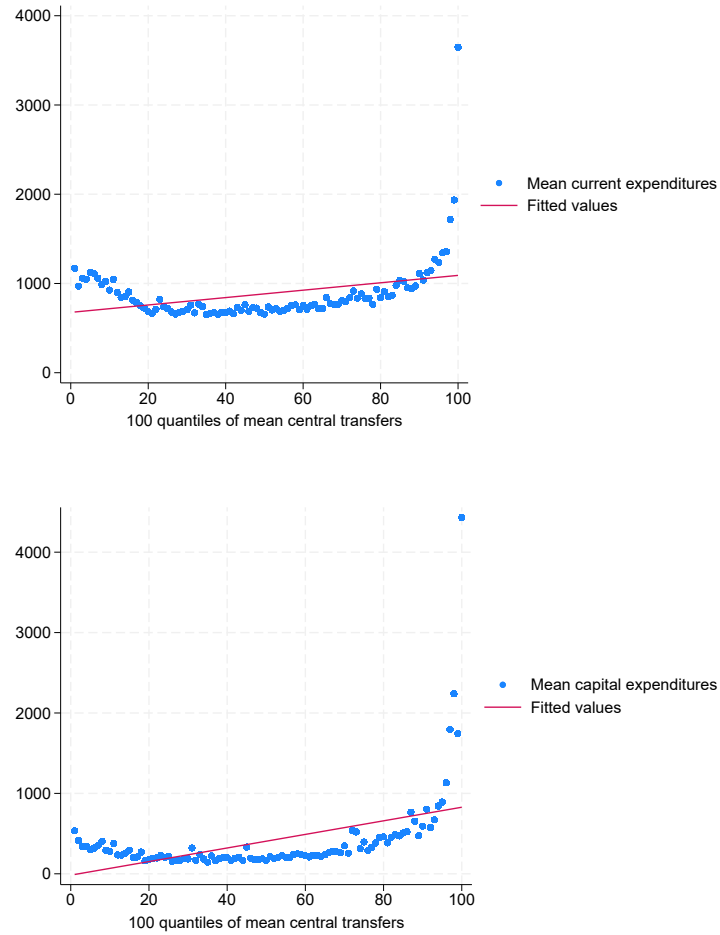
⁹The specific statutory mechanism driving this dynamic, and its role as a confounding factor, will be formally addressed in Section 4.

Figure 3: Central current transfers correlation with current revenues



Notes. The graph reports the average tax and no-tax revenues in Euro per-capita for 100 equally-spaced groups (quantiles). To obtain each quantiles, I computed the mean value of the current central transfers for each municipality over the entire period. Next, I sorted the municipalities in ascending order based on this mean value and partitioned the sample into 100 equal-sized groups, effectively representing the percentiles of the current central transfers' distribution.

Figure 4: Central current transfers correlation with expenditures



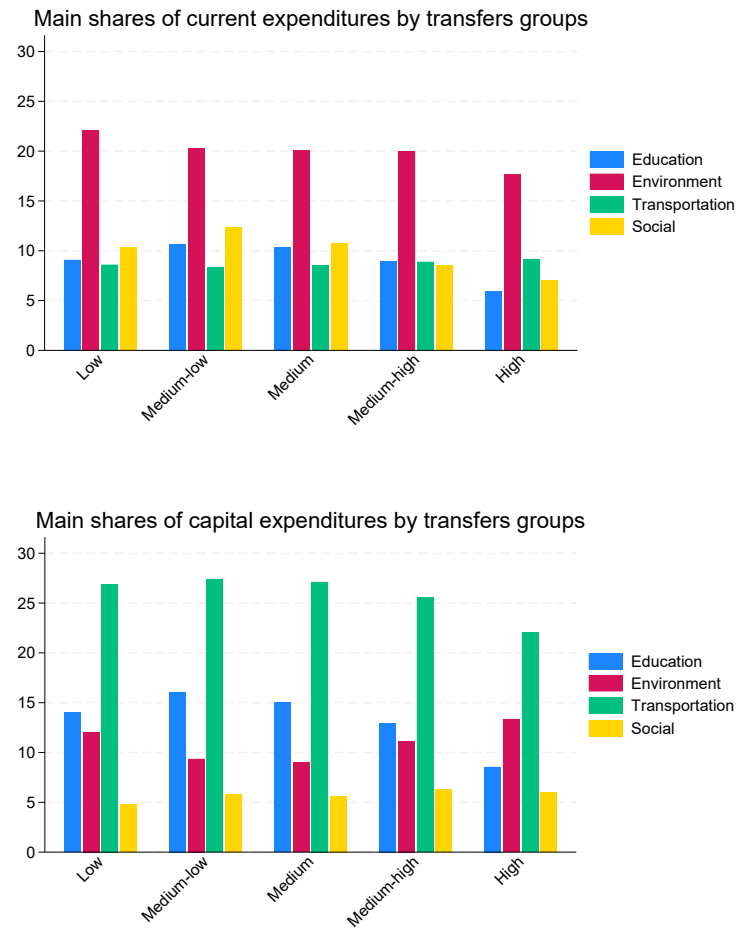
Notes. The graph reports the average current and capital expenditures in Euro per-capita for 100 equally-spaced groups (quantiles). To obtain each quantiles, I computed the mean value of the current central transfers for each municipality over the entire period. Next, I sorted the municipalities in ascending order based on this mean value and partitioned the sample into 100 equal-sized groups, effectively representing the percentiles of the current central transfers' distribution.

grant receipts are associated with lower shares of current expenditures allocated to environmental, educational, and social policies, whereas the portion of the current budget dedicated to local transportation appears largely unaffected. Conversely, when examining the composition of capital expenditures, municipalities receiving larger grants from the central government tend to invest a greater share of their capital budget in environmental policies, while allocating proportionally less to education and transportation.

Overall, the descriptive evidence presented thus far indicates that central grants constitute a crucial component of current revenues for all municipalities throughout the period of interest. Furthermore, these transfers appear to be systematically correlated with significant variations in local budgets, affecting total revenues, aggregate expenditures, and internal expenditure composition. Notably, municipalities in Southern Italy, as well as those with smaller populations, receive higher per capita grants and exhibit a greater dependency on central government support, while simultaneously recording higher levels of both current and capital expenditures.

As the descriptive evidence and the preliminary graphical inspection highlight, evaluating the true impact of transfers is heavily plagued by simultaneity puzzles. The apparent negative correlation between local tax revenues and central grants, for instance, does not necessarily imply a behavioral crowding-out effect, but rather mechanically reflects the underlying institutional design. To rigorously disentangle these dynamics, a formal empirical strategy is required.

Figure 5: Main shares of expenditures by central transfers quintiles



Notes. The graph reports the average share of current and capital expenditures dedicated to education, environment, transportation, and social expenditures. The average is measured for five equally-spaced groups divided according to their mean level of total grants received over the entire period.

4 Identification Strategy

4.1 Institutional context

Since the early 1990s, Italy has gradually delegated more responsibilities related to the provision of public services onto local governments of each municipality, to the point where almost 10% of Italian public current expenditures are sustained by the latter in 2024.^[10] However, even if many public policies are ultimately decided at local level, availability of local resources to support such policies greatly differs throughout the country, especially considering the large economic differences between northern and southern cities, as well as between small and large cities. Consequently, intergovernmental transfers are a fundamental instrument to achieve equalization in the provision of public goods. Current untied transfers account on average for 20% of current revenues in the municipal budget in the period analyzed, from 2016 to 2019. The average share of transfers over total current revenues reaches 25% when focusing on southern cities, or on cities with less than 5000 inhabitants, which are almost 70% of all Italian municipalities. Understanding how transfers are assigned to municipal government is key to comprehend why they represented a challenge in previous empirical analysis, and how the 2015 reform could be exploited to identify their impact on local politician behaviors.

Since 1979 up to 2015, central grants were redistributed between municipalities following what is currently known as the “historical criterion”.^[11] Although the precise allocation methodology employed over the years varied slightly, the

¹⁰Source: Italian Ministry of Economy and Finance open data. Available at <https://openbdap.rgs.mef.gov.it/>

¹¹Introduced by the so-called Stammati’s law, law-decree n. 946 of 29 December 1977

substance of the historical principle remained unchanged up to 2015. Under this system, current allocations were fundamentally anchored to a municipality's expenditure levels and grant receipts from the immediately preceding years, rather than being calculated on objective measures of local needs or actual tax capacity. Each subsequent year's transfer was largely determined by the previous year's allocation, effectively creating a path-dependent funding structure. As a result, municipalities that historically managed to secure higher funding levels maintained their advantage over time. While this principle was simple to implement and common in decentralization policies, it introduced incentives for local politicians to spend more in order to receive more grants in subsequent periods. Moreover, receiving more transfers allowed for higher level of expenditures, exacerbating the problem and introducing a difference in grants received persistent over time. Chiades et al. (2019) shows that 1979 transfers were relevant in explaining the pattern of central government transfers to Italian municipalities for the entire period 2003-2014.¹² This dynamic poses a significant challenge from an empirical perspective, as it raises concerns regarding potential reverse causality in studies analyzing the impact of central transfers on local spending policies. The direction of this bias is ambiguous a priori due to two confounding mechanisms. On one hand, under a historical allocation criterion, elevated local spending mechanically translates into higher grant allocations in subsequent years, generating an upward bias. On the other hand, high local expenditure might simply reflect a municipality's strong tax capacity; since equalization frameworks typically allocate fewer grants to wealthier municipalities, this mechanism would simultaneously generate

¹²In Section 5.6 I provide evidence that are still relevant in explaining a part of transfers received in the period 2016-2019.

a downward bias. Consequently, the net effect of these opposing forces on the estimated coefficients cannot be theoretically predetermined. Moreover, municipal grants from the central government are often correlated with the characteristics of local politicians. In particular, if local parties are politically aligned with the central government, they typically receive a higher volume of transfers (Bracco et al., 2015). This phenomenon introduces a severe confounding factor in standard OLS estimations. Specifically, unobserved characteristics of local politicians—such as their bargaining power, political acumen, or strategic alignment with the central ruling coalition—simultaneously influence both the allocation of grants and the municipality’s fiscal decisions. For instance, a politically aligned mayor might not only secure larger transfers but also pursue expansionary spending policies to maximize electoral support. If these political incentives and individual traits remain unobserved, they are absorbed into the error term, causing it to correlate with the grant variable. Consequently, a simple OLS framework fails to isolate the true behavioral response to the transfer, erroneously attributing the impact of these omitted political characteristics to the grants themselves and ultimately yielding biased and inconsistent estimates.

In this paper, I exploited a reform introduced in Italian municipal grants assignment system to derive an instrument for an instrumental variable approach in order to mitigate potential reverse-causality biases and omitted variable biases derived from local politicians confounders.

Starting with a decentralization reform in 2009, which effectively affected central transfers redistribution from 2015 onward, the Italian government introduced a new system, defined as a combination of the previous historical criterion and a new one so-called standardized criterion. The latter follows a similar idea of con-

sidering spending needs and tax capacity, but it introduced new methodologies to measure these two figures instead of looking at past budgets. Standardized spending needs are estimated by modeling historical expenditures across fundamental municipal functions using cross-sectional regression analyses.¹³ These models identify and assess the impact of a comprehensive set of structural, demographic, and socio-economic cost drivers. To calculate the final standardized spending need for a subsequent period, the estimated coefficients—which capture the marginal effect of each specific driver—are multiplied by the municipality’s updated, time-varying characteristics. This procedure yields the theoretical expenditure required by the municipality to provide a standard level of public services at an average level of efficiency.

Conversely, standardized tax capacity measures a municipality’s intrinsic ability to generate revenue, independent of its discretionary local tax policies. This metric is computed by applying standard, national average tax rates to the municipality’s objective local tax bases for the most significant municipal taxes (property and income surtaxes).

Municipalities with higher standardized spending needs and higher historical expenditures received more transfers, while municipalities with larger standardized tax capacity and historical tax revenues received less. The standardized and historical components of the grants are weighted using a specific policy parameter, or coefficient, established by the central government. This coefficient gradually increases over the period of interest.¹⁴ Importantly, this parameter can be con-

¹³The methodological framework and empirical estimations for municipal standard needs were developed by SOSE (*Soluzioni per il Sistema Economico*), a public company operating under the Ministry of Economy and Finance, and subsequently validated by the Technical Commission for Standard Needs (CTFS).

¹⁴Details on the evolution and the use of the coefficient in the allocation formula are explained

sidered exogenous to the specific characteristics of individual municipalities and their local politicians.

Although the legislative framework for this reform was initiated in 2009, the precise annual value of the coefficient remained entirely unknown at the time. Furthermore, local administrators could not foresee its actual impact on their specific budgets, given that the final transfer amounts simultaneously depended on the standardized measurements of needs and tax capacities, which were only computed and disclosed from 2015 onward. The mathematical complexity of the allocation formula—designed and calibrated by an independent technical committee—effectively shielded the distribution mechanism from targeted political manipulation. An individual mayor, regardless of their political alignment or bargaining power, could neither predict nor influence how the coefficient would alter their municipality’s specific grant allocation.

Moreover, the planned trajectory of this coefficient was not fixed; rather, its growth pattern was subject to annual revisions. These temporal fluctuations were driven entirely by aggregate, macro-level dynamics—specifically, ongoing political bargaining between the central government and the collective association of municipalities (ANCI in Italy), as well as shifting macroeconomic constraints and national budgetary necessities. Consequently, from the perspective of any single municipality, the annual variation in this coefficient acts as an exogenous, aggregate shock that is entirely independent of local budget decisions or the idiosyncratic characteristics of local incumbents.

Lastly, the 2009 reform established a new fund to support current transfers, the “Fondo di Solidarietà Comunale” (hereafter FSC). The FSC was primarily in Appendix [A](#).

ily financed by each municipality by a fixed amount calculated as a percentage of standardized measurements of two Italy’s most significant municipal taxes in 2015, the IMU and TASI, along with a minor contribution from the central government to limit variations in transfers compared to prior levels. Current transfers received from the FSC are not tied to any specific purpose and can be used freely by local governments without limitations.

4.2 Identification

The proposed identification strategy relies on two key elements derived from the 2015 institutional reform to construct the instrument for current central transfers. Crucially, the mathematical design of this formula ensures that the instrument specifically drives variations only within the allocation of unconditional equalization grants (the FSC), rather than affecting the broader aggregate of all central transfers.¹⁵ Specifically, the instrument is defined as the interaction between the municipal standardized spending needs—strictly fixed at 2015 level, one year prior to the period of analysis—and the time-varying statutory policy weight,. To this interaction term, I add the fixed per-capita tax quota originally retained to finance the FSC, yielding the following equation:

$$Z_{i;t} = \mu_t F_{std,i,2015} + quota_{i;2015} \quad (1)$$

¹⁵Isolating the exogenous variation specifically within unconditional grants provides a significant methodological advantage. It ensures that the second-stage estimates capture the pure local fiscal response to an unconstrained revenue windfall, remaining structurally independent of the mechanical effects and spending constraints tied to other specific, conditional, or earmarked grants the municipality might simultaneously receive.

In Equation 1, $Z_{i,t}$ is the final instrument employed in the first stage regression for municipality i in year t ; μ_t is the policy parameter introduced in the previous section, which represents the weight given to the standardized component in the grants calculation formula; $F_{std,i,2015}$ represents the standardized spending needs in 2015; $quota_{i,2015}$ represents the participation quota of each municipality in the FSC.¹⁶ As discussed in the previous section, while the policy weight coefficient μ_t is determined at the national level and is exogenous to individual municipalities, it only varies over time. Consequently, its standalone effect is absorbed by year fixed effects and is insufficient to explain cross-sectional differences in the transfers received. To construct a relevant instrument for the 2016-2019 analysis period, μ_t is interacted with the municipality's standardized spending needs, which are fixed at their 2015 baseline. Because the statutory formula implies that spending needs are multiplied by this coefficient, this interaction closely follows the actual grant calculation, thereby improving the explanatory power of the instrument. In this setup, the 2015 spending needs provide cross-sectional variation, while the coefficient μ_t provides temporal variation, allowing the model to retain year fixed effects. Using spending needs, however, introduces a potential identification challenge, as the underlying components of local spending demand could be correlated with the fiscal outcomes of interest. To address this, the empirical strategy relies on two main steps. First, fixing the standardized spending needs at the year prior to the analysis period (2015) helps mitigate contemporaneous endogeneity concerns. Second, the underlying variables used by the technical committee to estimate these needs are observable. By including almost the entire set of these time-varying determinants for the 2016-2019 period as control

¹⁶Details on the grants calculation formula are explained in Section [A](#)

variables,¹⁷ alongside municipal and year fixed effects, the model accounts for the local characteristics driving the demand for spending. This set of controls helps address potential confounding pathways, mitigating concerns regarding omitted variable bias.

Crucially, the inclusion of all determinants of the instrument improves the robustness of the Exclusion Restriction, as it accounts for the impact of the 2015 standardized demand across potential alternative channels directly related to the calculation of FSC transfers. This guarantees that the interaction of μ with standardized needs affects the outcomes primarily through its influence on horizontal transfers received by each municipality.

Moreover, the large number of determinants creates a “worst-case scenario” for estimation, wherein a great part of the variability in the model is absorbed by the control variables. This strengthens the robustness of statistically significant results, enhancing confidence in the reliability of the findings.

Importantly, the rigorous methodology underlying the standardized criteria significantly limits the scope for direct manipulation by either local or central governments. Consequently, it is reasonable to assume that discretionary political decisions made prior to the reform are unlikely to systematically bias the computation of standardized needs. This feature addresses one of the primary methodological challenges in evaluating intergovernmental transfers: the threat of reverse causality. For example, when analyzing current expenditures, the historical component of the FSC implies that grant allocations are positively tied to past municipal spending. Consequently, higher transfers stimulate local expenditures, which mechanically drive up the transfers received in subsequent years,

¹⁷Out of 35 determinants of standardized spending, 33 are available in the final dataset.

creating an endogenous feedback loop.

A similar reverse causality issue arises in models where local taxes serve as the dependent variable, as local taxation can endogenously affect transfers through two distinct but interconnected channels: (i) directly, local fiscal policies can alter the municipal tax base, which feeds into the calculation of the standardized tax capacity—a measure that negatively impacts the computation of FSC grants; (ii) indirectly, higher local taxes generate more municipal revenue, facilitating increased local spending, which in turn triggers higher future transfers via the historical allocation criterion.¹⁸

Employing the 2015 standardized spending needs as an instrument, alongside municipal fixed effects, may mitigate both types of simultaneity bias. Because the instrument is anchored to a baseline year prior to the period of analysis and is constructed using a broad set of structural local characteristics, it remains reasonably independent of prior local spending decisions and discretionary tax policies.

Finally, in an instrumental variable framework, the monotonicity assumption dictates that the instrument must affect the endogenous variable in the same direction for all observations, effectively ruling out the existence of “defiers.” This crucial condition is structurally satisfied by the institutional design of the reform. Because the policy weight μ consistently increases over time, and a municipality’s standardized spending needs mathematically exert a strictly positive contribution to the statutory grant calculation, the instrument’s effect operates uniformly. Consequently, an increase in the instrument mechanically drives up

¹⁸The theoretical and empirical evidence collected thus far does not allow for the formulation of a clear *a priori* hypothesis regarding the net direction of these concurrent biases.

the standardized component of the transfers for all municipalities, ensuring that the direction of the effect remains unambiguously positive across the entire sample.

To estimate the impact of transfers on current revenues, current expenditures, capital expenditures, and their composition within municipal budgets, I employed a Two-Stage Least Squares (2SLS) approach. The final specification of the model is:

$$X_{i,t} = \alpha_0 + \alpha_1 Z_{i,t} + M_{i,t} + I_{i,t} + T_t + D_i + \epsilon_{i,t} \quad (2)$$

$$Y_{i,t} = \beta_0 + \beta_1 \hat{X}_{i,t} + M_{i,t} + I_{i,t} + T_t + D_i + u_{i,t} \quad (3)$$

All monetary values in the regression are measured in log Euro per capita.¹⁹ $Y_{i,t}$ represent the outcome variable of interest for municipality i in year t . T_t and D_i are year and municipal fixed effects, respectively. $M_{i,t}$ is a matrix containing 33 different covariates used to determine the standardized needs. $I_{i,t}$ is another matrix related to individual characteristics of mayors.²⁰ The excluded instrument $Z_{i,t}$ is defined as in equation 1. The endogenous explanatory variable $X_{i,t}$ is the

¹⁹Almost all relevant variables in Euro per capita are non-zero out of a full sample of 25,770 observations. The instrument presents no cases with zero values. The endogenous variable is equal to zero only in 19 cases. Capital expenditures are equal to zero in 139 cases. See Appendix C for a discussion on linear specifications of the model.

²⁰Specifically, gender, age when elected, a dummy accounting for graduated mayors, years of experience, votes received in previous election and a dummy equal to one when the mayor is born in the same city where is elected.

sum of all vertical and horizontal current transfers received from the central government, either via the FSC or through other channels unrelated to the 2015 reform criteria.²¹

To assess the robustness of the estimates with respect to the historical component of expenditures (and consequently to potential reverse causality biases), I tested a second specification of the main model that includes two instruments, with the second being an additional exogenous excluded instrument capturing the effect of historical spending on transfers. Specifically, I adopted the strategy proposed by Chiades et al. (2019), which demonstrates that, due to the historical criteria, Italian intergovernmental transfers are closely related to their levels in 1979. Given that part of the computation of FSC-related transfers still depends on historical spending weighted by one minus the policy coefficient μ , I introduced as a second instrument the interaction term between (one minus) the policy coefficient and the 1979 per capita level of transfers. The new first-stage specification is as follows:

$$X_{i;t} = \alpha_0 + \alpha_1 Z_{i;t} + \alpha_2 H_{i;t} + M_{i;t} + I_{i;t} + T_t + D_i + \epsilon_{i;t} \quad (4)$$

with $H_{i;t} = (1 - \mu_t) * X_{i;1979}$ being the additional excluded instrument.²²

²¹The latter are all current transfers from the central government that are not tied to pre-defined purposes, cannot be used for investments or to mitigate financial distress, and are determined by specific cases of law. Current transfers from the FSC are on average 66% of current central transfers, with 25% of the sample displaying more than 90% of transfers received from the FSC. Current transfers from regions, firms, families, and the European Union are excluded from this measurement.

²²In Appendix B, I conduct a similar analysis, substituting the historical instrument in Equation 4 with the standardized tax capacity, $C_{std;i,t}$. Alongside spending needs, standardized tax capacity serves as the other fundamental variable utilized within the statutory formula to compute the standardized component of the transfers received by municipalities. However, because

5 Results

In this section I presents main results from the empirical analysis. Section 5.1 displays first stage regressions. Section 5.2 shows the results for current revenues, current expenditures and capital expenditures. Section 5.3 discuss potential treats to the exclusion restriction and provide some suggestive evidence in support of the identification strategy. Section 5.4 discuss heterogeneity in the effect on current expenditures. In Section 5.5 I analyze the effect of central current transfers on expenditure's budget composition. In Section 5.6, I included main estimates computed with the additional instrument controlling for historical expenditures.

5.1 First Stage

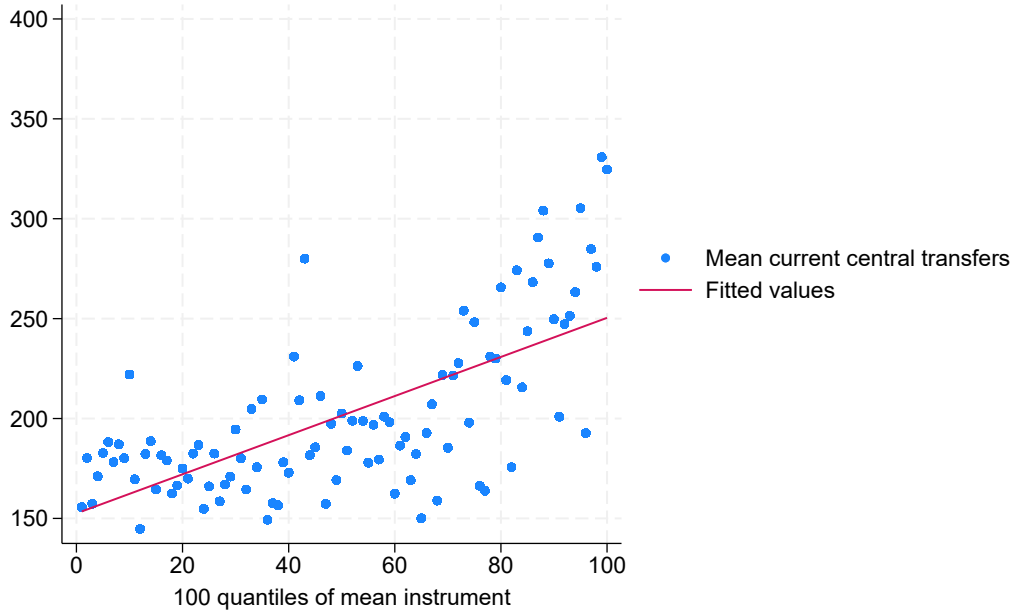
Firstly, to better visualize the relationship between the instrumental variable and the grants received by each municipality, in Figure 6 I present the linear correlation between the instrument, the standardized spending needs multiplied by the weight coefficient, and the endogenous variable, the total amount of current intergovernmental transfers received.²³ As expected, the instrument appears to be positively correlated with current central transfers supporting the validity of the relevance assumption.

Table 3 presents the first-stage fixed-effects regressions of total current trans-

I lack complete data on all the specific local tax bases employed in its official calculation, I opted to exclude this variable from the main baseline model. Nevertheless, the primary results presented in Section 5 remain highly consistent with these supplementary estimates.

²³For visual clarity, I first computed the mean value of the instrument for each municipality over the entire period. Next, I sorted the municipalities in ascending order based on this mean value and partitioned the sample into 100 equal-sized groups, effectively representing the percentiles of the instrument's distribution. Finally, I plotted the average current central transfers corresponding to each of these 100 groups.

Figure 6: Instrument correlation with central current transfers



Notes. The graph reports the average current central transfers in Euro per-capita for 100 equally-spaced groups (quantiles). To obtain each quantiles, I computed the mean value of the instrument for each municipality over the entire period. Next, I sorted the municipalities in ascending order based on this mean value and partitioned the sample into 100 equal-sized groups, effectively representing the percentiles of the instrument's distribution.

fers on the instrument, estimated both with and without the inclusion of the municipal covariates used to calculate standardized spending needs, as well as mayor-level controls. In both specifications, the estimated coefficients are positive and statistically significant at the 1% level.

Specifically, the point estimate indicates that a 1% increase in a municipality's standardized spending needs translates into a 1.25% increase in total current transfers received. This magnitude is plausible and consistent with the institutional design of the equalization formula, which links higher recognized local needs to larger grant allocations.

Furthermore, the estimated coefficients remain stable upon the inclusion of the

full set of covariates. This suggests that the first-stage estimates are robust, even in a specification where the control variables absorb a substantial portion of the variation stemming from standardized spending needs. Overall, the instrument appears relevant in explaining variations in the endogenous regressor. In all subsequent 2SLS estimations, I report the Kleibergen-Paap rk Wald F-statistic for the excluded instrument. These statistics consistently exceed the Stock-Yogo critical values, mitigating concerns regarding weak instruments and supporting the relevance assumption.

Table 3: First Stage regressions

Variables	(1) Log of Current	(2) Transfers
Log of 2015 Std. Needs $\times$ Policy coefficient	1.292*** (0.181)	1.249*** (0.209)
Observations	25,737	25,725
Number of municipalities	6,478	6,475
Estimator	FE OLS	FE OLS
Year FE	YES	YES
Municipal FE	YES	YES
Municipality controls	NO	YES
Mayor controls	NO	YES

Notes. The table reports fixed effects (FE) estimates for the log of total current central transfers per capita on the log of the instrument. Column (1) controls for year and municipal fixed effects. Column (2) controls for year and municipal fixed effects, all determinants of the standardized demand, and mayor-level controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Expenditures and revenues

Tables 4 to 7 present the estimated coefficients of the endogenous regressor, the instrument in its reduced form, and the 2SLS estimates on current expenditures, capital expenditures, current revenues from taxes, and current revenues from sources unrelated to taxes. Even-numbered columns include all control variable which has been employed for the calculation of standardized spending needs, as

well as mayor-level controls. Estimates for the main model displayed in Section 4 are reported in Column (6).

Firstly, the estimated coefficients of current transfers on current expenditures in Table 4 are all positive and statistically significant. Estimates from the main model indicate an effect four times larger compared to the simple effect measured by the endogenous regressor. In contrast, results for capital expenditures in Table 5 are less conclusive. The instrument predicts a negative and statistically significant effect when only year and municipal fixed effects (FEs) are accounted for. However, when all covariates are included, the effect remains negative but becomes much smaller and insignificant.

Table 4: Current intergovernmental transfers on current expenditures

Variables	(1) <i>Endogenous</i>	(2) <i>OLS</i>	(3) <i>Reduced form</i>	(4)	(5) <i>2SLS</i>	(6)
	Log of Current Expenditures					
Log of Current transfers	0.046*** (0.006)	0.045*** (0.007)			0.094*** (0.029)	0.207*** (0.044)
Log 2015 Std. Needs $\times$ Policy coeff.			0.120*** (0.036)	0.253*** (0.043)		
Observations	25,769	25,757	25,769	25,757	25,769	25,757
Number of municipalities	6,486	6,483	6,486	6,483	6,486	6,483
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES
K-P rk Wald F					50.989	36.804

Notes. The table reports FE estimates for log total current expenditures per capita. Columns (1) and (2) report Fixed-Effects OLS estimates with the endogenous independent variable, without and with controls for standardized demand and mayor-level characteristics, respectively. Columns (3) and (4) report Fixed-Effects estimates in reduced form, without and with the same sets of controls. Columns (5) and (6) report Fixed-Effects 2SLS estimates, without and with controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Current intergovernmental transfers on capital expenditures

Variables	(1) <i>Endogenous</i>	(2) <i>OLS</i>	(3) <i>Reduced form</i>	(4)	(5) <i>2SLS</i>	(6)
	Log of Capital Expenditures					
Log of Current transfers	0.028 (0.024)	0.039 (0.026)			-0.703*** (0.255)	-0.435 (0.294)
Log 2015 Std. Needs $\times$ Policy coeff.			-0.925*** (0.297)	-0.554 (0.343)		
Observations	25,631	23,557	25,631	23,557	25,631	23,557
Number of municipalities	6,486	6,483	6,486	6,483	6,486	6,483
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES
K-P rk Wald F					51.737	37.687

Notes. The table reports FE estimates for log total capital expenditures per capita. Columns (1) and (2) report Fixed-Effects OLS estimates with the endogenous independent variable, without and with controls for standardized demand and mayor-level characteristics, respectively. Columns (3) and (4) report Fixed-Effects estimates in reduced form, without and with the same sets of controls. Columns (5) and (6) report Fixed-Effects 2SLS estimates, without and with controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 shows that while baseline fixed-effects regressions report a negative correlation between tax revenues and intergovernmental grants, this relationship is no longer significant in the reduced-form and 2SLS estimations. This discrepancy suggests that the initial negative correlation might reflect the mechanics of the grant allocation process rather than a genuine behavioral response. Specifically, since the equalization formula assigns fewer transfers to municipalities with greater fiscal capacity, uninstrumented models tend to overestimate the negative association. When instrumenting grants with standardized needs, which nets out the influence of actual fiscal capacity, the results do not support the presence of a crowding-out effect on local taxation.²⁴

²⁴In the uninstrumented regressions, the negative association likely captures the institutional design of the FSC: although higher tax revenues may be associated with higher spending levels, their role as a measure of fiscal capacity automatically reduces the equalization grants received by the municipality.

Table 6: Current intergovernmental transfers on local taxes

Variables	(1) <i>Endogenous OLS</i>	(2) <i>Endogenous OLS</i>	(3) <i>Reduced form</i>	(4) <i>Reduced form</i>	(5) <i>2SLS</i>	(6) <i>2SLS</i>
	Log of Local Taxes					
Log of Current transfers	-0.045*** (0.006)	-0.045*** (0.006)			0.028 (0.041)	0.001 (0.041)
Log 2015 Std. Needs $\times$ Policy coeff.			0.037 (0.051)	0.002 (0.052)		
Observations	25,769	23,687	25,842	23,786	25,729	23,648
Number of municipalities	6,486	6,151	6,484	6,157	6,470	6,112
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES
K-P rk Wald F					50.989	36.804

Notes. The table reports FE estimates for log per capita revenues from local taxes. Columns (1) and (2) report Fixed-Effects OLS estimates with the endogenous independent variable, without and with controls for standardized demand and mayor-level characteristics, respectively. Columns (3) and (4) report Fixed-Effects estimates in reduced form, without and with the same sets of controls. Columns (5) and (6) report Fixed-Effects 2SLS estimates, without and with controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Current intergovernmental transfers on no-tax revenues

Variables	(1) <i>Endogenous OLS</i>	(2) <i>Endogenous OLS</i>	(3) <i>Reduced form</i>	(4) <i>Reduced form</i>	(5) <i>2SLS</i>	(6) <i>2SLS</i>
	Log of No-Tax Revenues					
Log of Current transfers	-0.002 (0.007)	-0.003 (0.008)			0.051 (0.067)	0.099 (0.084)
Log 2015 Std. Needs $\times$ Policy coeff.			0.066 (0.085)	0.119 (0.101)		
Observations	25,769	25,757	25,842	25,830	25,729	25,717
Number of municipalities	6,486	6,483	6,484	6,481	6,470	6,467
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES
K-P rk Wald F					50.989	35.757

Notes. The table reports FE estimates for log per capita revenues unrelated to taxes. Columns (1) and (2) report Fixed-Effects OLS estimates with the endogenous independent variable, without and with controls for standardized demand and mayor-level characteristics, respectively. Columns (3) and (4) report Fixed-Effects estimates in reduced form, without and with the same sets of controls. Columns (5) and (6) report Fixed-Effects 2SLS estimates, without and with controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Crucially, the absence of a downward adjustment in local taxation is not

mechanically driven by the institutional framework in place during the period of analysis. Although the Italian central government imposed a strict statutory freeze preventing municipalities from increasing local tax rates between 2016 and 2019, local administrations retained full legal autonomy to reduce them. Because the theoretical crowding-out effect precisely predicts a reduction in local taxation in response to positive grant shocks, this institutional constraint does not preclude the possibility of observing such tax cuts. Finally, there is no evidence that increased transfers induced any significant changes in non-tax current revenues (Table 7).

Taken together, the main model estimates align with previous studies indicating no crowding-out effect on taxes in the short run²⁵ with local governments increasing current spending rather than adjusting tax rates or increasing capital investments when receiving more current intergovernmental transfers.²⁶

5.3 Exclusion Assumption

A fundamental threat to the validity of the instrumental variable strategy is a potential violation of the exclusion restriction—namely, the risk that the instrument might affect the fiscal outcomes of interest through channels other than current municipal transfers. This concern is particularly salient given the inherent nature of standardized spending needs. Because this variable serves as a proxy for the effective demand for local public goods, it is intrinsically correlated with endogenous structural factors that can directly influence overall expenditure levels, tax collection, and budget composition.

²⁵Helm and Stuhler (2024); Leduc and Wilson (2017); Dahlberg et al. (2008)

²⁶Bianchi et al. (2023); Leduc and Wilson (2017); Meloni (2016)

For instance, the age distribution of the local population is a primary determinant in the calculation of standardized spending needs. Municipalities with a higher proportion of young residents naturally face greater demand for educational expenditures and may rely on a smaller tax base compared to cities with a larger working-age population. To effectively address this issue and isolate the grant channel, the empirical model incorporates the comprehensive set of local characteristics used in the official computation of standardized spending needs, alongside both municipal and year fixed effects. Nevertheless, while this rigorous set of controls substantially mitigates omitted variable bias, it cannot absolutely guarantee that the instrument does not inadvertently capture the lingering effects of earlier structural differences.

In principle, there is no direct empirical test to definitively prove the validity of the exclusion restriction. A common approach in the empirical literature involves conducting balance tests on pre-treatment municipal characteristics to support both the independence and exclusion assumptions.²⁷ However, applying this strategy is structurally constrained in the specific context of standardized spending needs. A core element of the present identification strategy relies on conditioning the analysis on the comprehensive set of the instrument’s determinants. While this rich set of variables is fully observed during the analysis period, a complete and uniform collection of these specific data points is not readily available prior to the reform’s implementation.²⁸ Similarly, conducting a

²⁷See, for example, Altonji et al. (2005), Oster (2019), or Goldsmith-Pinkham et al. (2020).

²⁸Although some standard covariates—such as demographic age brackets or local employment rates—can be reconstructed using alternative historical sources, a significant portion of the determinants were surveyed exclusively for the reform’s implementation. Highly specific variables, such as the monthly frequency of open-air markets, the exact number of municipal parking stalls, or the share of disabled students utilizing public transportation, were simply not uniformly tracked across all municipalities in the pre-reform era. Consequently, while a full

standard pre-trend analysis is structurally precluded. As previously discussed, the full implementation of the FSC coincides with the comprehensive 2015 municipal budget reporting reform. Because this accounting overhaul fundamentally altered how local revenues and expenditures are classified and recorded, attempting to trace consistent fiscal trajectories across this institutional threshold would yield inherently unreliable comparisons. One viable alternative to provide suggestive evidence supporting the exclusion restriction is the so-called “zero-first-stage” test.²⁹ This empirical strategy involves isolating a specific subset of observations within the main sample for which the instrument demonstrably has no effect on the endogenous regressor. If, within this sub-sample of municipalities where the primary grant channel is effectively “shut down,” a reduced-form regression of the fiscal outcomes on the instrument still yields statistically significant coefficients, it would strongly imply that the instrument is operating through alternative pathways. Such a finding would indicate a violation of the exclusion restriction, as the instrument would be affecting the outcomes independently of the intergovernmental transfers.

Although the FSC reform applies to all municipalities within Ordinary Statute Regions, the statutory formula itself provides a mechanism to identify a specific subset of observations where the effect of standardized spending needs is effectively neutralized. For instance, if a municipality’s standardized spending needs are nearly identical to its standardized tax capacity, the standardized component

reconstruction of the pre-treatment baseline is currently limited, future extensions of this research will aim to at least partially assemble the available historical covariates from alternative sources to perform exploratory balance tests on previous outcomes or municipal characteristics not directly employed in the standardize spending need calculation.

²⁹This approach was popularized by Angrist et al. (2010) and later formalized by Van Kippersluis and Rietveld (2018).

of the allocation—which is defined as the difference between these two values—approaches zero. In such instances, the effect of the instrument on the final grant allocation is mechanically removed. Furthermore, even when this standardized component is non-zero, there are cases where its magnitude is negligible relative to the historical component. To systematically identify this subset of municipalities, I computed the ratio between the standardized and historical components, each weighted by its respective statutory coefficient, in absolute value, using the following formula:

$$Ratio_{i,t} = \left| \frac{\mu_t(F_{i,t} - C_{i,t})}{(1 - \mu_t)(H_{2015;i} - TC_{2015;i})} \right| \quad (5)$$

where $F_{i,t}$ denotes the standardized spending needs for municipality i in year t ; $C_{i,t}$ represents the standardized tax capacity; and $H_{i,2015}$ and $TC_{i,2015}$ indicate the historical spending and historical tax capacity, respectively, strictly fixed at their 2015 levels as dictated by the statutory grant allocation formula. As this ratio approaches zero, it mathematically implies that the numerator (the standardized component) is either inherently close to zero or its magnitude is completely negligible compared to the denominator (the historical component).

Based on the empirical distribution of this ratio, I identified the bottom 5th, 10th, and 25th percentiles, corresponding to values of 0.03, 0.06, and 0.13, respectively. The sub-samples where the instrument is expected to have a negligible effect are defined by municipalities whose ratios fall below these specific thresholds. To mitigate concerns regarding imprecise estimates arising from small sample sizes, I conducted the zero-first-stage test across these three successively

larger groups.³⁰

Table 8 reports the first-stage regression estimates, where the log of current transfers is regressed on the log of the instrument. These specifications include municipal and year fixed effects, alongside the full set of standardized spending need determinants and mayor-level control variables. Across all three restricted groups, the estimated coefficients on the instrument are small in magnitude and statistically insignificant. This confirms that, within these specific sub-samples, the instrument has no discernible impact on the endogenous variable, successfully satisfying the prerequisite for the zero-first-stage test.³¹

Table 8: Zero-First Stage

Variables	Log of Current Transfers		
	(1)	(2)	(3)
Log of 2015 Std. Needs $\times$ Policy coefficient	-1.155 (1.686)	-0.886 (0.978)	0.118 (0.419)
Observations	1,283	2,560	6,557
Number of municipalities	733	1,284	2,976
Estimator	FE OLS	FE OLS	FE OLS
Year FE	YES	YES	YES
Municipal FE	YES	YES	YES
Municipality controls	YES	YES	YES
Mayor controls	YES	YES	YES
Ratio upper bound	0.03	0.06	0.13

Notes. The table reports FE estimates for the log of total current central transfers per capita on the log of the instrument. Column (1) reports estimates for the subsample where the ratio between the standardized component and the historical component is below 0.03. Column (2) reports estimates for the subsample where the ratio is below 0.06. Column (3) reports estimates for the subsample where the ratio is below 0.13. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

³⁰The 5th percentile was selected as the most restrictive threshold, as smaller cutoffs would not yield a sufficient number of observations to ensure adequate statistical power.

³¹Importantly, because the identification of the near-zero sub-sample partially depends on the level of standardized spending needs, this restriction inherently introduces a certain degree of sample selection. Municipalities located in the lower tail of this ratio's distribution tend to exhibit systematically lower standardized spending needs, higher tax capacities, and larger historical grant components. Nevertheless, the inclusion of municipal and year fixed effects, combined with the comprehensive set of municipal and mayor-level controls, helps mitigate concerns regarding potential selection bias within these restricted estimations.

Tables 9 and 10 report the reduced-form estimates regarding the direct effect of the instrument on municipal expenditures and revenues, respectively. Across all three restricted sub-samples, the estimated coefficient for standardized spending needs is statistically insignificant for all considered outcomes—namely, current and capital expenditures, as well as tax and non-tax revenues.

Overall, while this zero-first-stage test cannot definitively rule out all potential violations of the exclusion restriction, it provides compelling suggestive evidence supporting the identifying assumption: the instrument affects the fiscal outcomes of interest exclusively through its formula-induced impact on intergovernmental grants.

Table 9: Placebo test on current and capital expenditures

Variables	<i>Log of Current Expenditures</i>			<i>Log of Capital Expenditures</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Log of 2015 Std. Needs $\times$ Policy coefficient	-0.214 (0.338)	-0.225 (0.226)	0.133 (0.143)	0.404 (3.950)	-3.913 (2.822)	-1.346 (1.299)
Observations	1,290	2,572	6,571	1,281	2,558	6,528
Number of municipalities	736	1,286	2,979	733	1,279	2,964
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor controls	YES	YES	YES	YES	YES	YES
Ratio upper bound	0.03	0.06	0.13	0.03	0.06	0.13

Notes. The table reports FE estimates for the log of total current and capital expenditures per capita on the log of the instrument. Column (1) reports estimates on current expenditures for the subsample where the ratio between the standardized component and the historical component is below 0.03. Column (2) reports estimates on current expenditures for the subsample where the ratio is below 0.06. Column (3) reports estimates on current expenditures for the subsample where the ratio is below 0.13. Columns (4), (5), and (6) report estimates on capital expenditures following the same sample restrictions. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Placebo test on tax and no-tax revenues

Variables	<i>Log of Tax Revenues</i>			<i>Log of No-Tax Revenues</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Log of 2015 Std. Needs $\times$ Policy coefficient	0.347 (0.709)	0.032 (0.466)	-0.239 (0.217)	1.014 (1.346)	-0.571 (1.146)	0.155 (0.375)
Observations	1,290	2,572	6,571	1,290	2,572	6,571
Number of municipalities	736	1,286	2,979	736	1,286	2,979
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS
Year FE	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor controls	YES	YES	YES	YES	YES	YES
Ratio upper bound	0.03	0.06	0.13	0.03	0.06	0.13

Notes. The table reports FE estimates for the log of tax and no-tax revenues per capita on the log of the instrument. Column (1) reports estimates on tax revenues for the subsample where the ratio between the standardized component and the historical component is below 0.03. Column (2) reports estimates on tax revenues for the subsample where the ratio is below 0.06. Column (3) reports estimates on tax revenues for the subsample where the ratio is below 0.13. Columns (4), (5), and (6) report estimates on no-tax revenues following the same sample restrictions. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.4 Heterogeneity analysis

As discussed in the review of the literature, "second-generation" theories of fiscal federalism emphasize that local fiscal responses to central grants are not merely mechanical. Instead, they are deeply shaped by the political economy environment and the principal-agent dynamics between voters and local administrators (Oates, 2005; Inman, 2009). To explore these mechanisms and understand whether the observed flypaper effect is mediated by the individual traits and political constraints of local policymakers, this section presents a heterogeneity analysis based on the mayors' political experience, electoral power, and education level.³²

³²The analysis focuses on heterogeneity effects on current expenditures. The other main outcomes, namely capital expenditures, tax revenues and non-tax revenues do not presents different results with respect to the main analysis.

Table 11 investigates whether the spending response to unconditional transfers varies with the mayor's prior political experience. To achieve this goal, I compute the average years of experience of each mayor, and split the sample in two groups, with the average years of experience in politics above or below the median in the national distribution. The 2SLS estimates indicate that the expansionary effect on current expenditures is larger for less experienced mayors compared to their highly experienced counterparts. This finding aligns with the hypothesis that newly elected or less established politicians might face stronger incentives to use unconstrained grant windfalls to quickly build a political base and signal competence to voters, whereas entrenched mayors with established reputations may feel less pressure to immediately expand current spending.

Table 11: Heterogeneity with years of experience

Variables	<i>First Stage on transfers</i>		<i>Current Expenditures</i>	
	<i>Low experience</i> (1)	<i>High experience</i> (2)	<i>Low experience</i> (3)	<i>High experience</i> (4)
Log of 2015 Std. Needs $\times$ Policy coeff.	1.069*** (0.333)	1.326*** (0.316)		
Log of Current transfers			0.253*** (0.091)	0.170*** (0.058)
Observations	12,116	11,571	11,892	11,201
Number of municipalities	3,978	3,886	3,754	3,516
F statistic	5.684	4.496		
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor controls	YES	YES	YES	YES
K-P rk Wald F			10.283	17.560

Notes. The table reports first stage FE estimates and 2SLS estimates for the effect of log total current central transfers per capita on log current expenditures. Column (1) reports first stage estimates for the subsample of mayors with average years of experience in politics below the median. Column (2) reports first stage estimates for the subsample of mayors with average years of experience above the median. Column (3) reports 2SLS estimates of the effect of current transfers on current expenditures for the subsample of mayors with average years of experience below the median. Column (4) reports 2SLS estimates for the subsample of mayors with average years of experience above the median. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 12 explores the role of political power, measured by the average number of votes received by the mayor, splitting the sample at the median. The results suggest a stronger expenditure response among mayors with higher political power relative to those with lower consensus. From a political economy perspective, a larger electoral mandate might reduce the perceived threat of electoral accountability. Consistent with the literature highlighting how reduced political competition can foster budgetary expansions or opportunism (Brollo et al., 2013; Curto-Grau et al., 2018), mayors who feel politically secure may be more inclined to exploit fiscal illusion, channeling external revenues into highly visible current expenditures without the fear of immediate electoral backlash.

Table 12: Heterogeneity with political power

Variables	<i>First Stage on transfers</i>		<i>Current Expenditures</i>	
	<i>Low pol. power</i> (1)	<i>High pol. power</i> (2)	<i>Low pol. power</i> (3)	<i>High pol. power</i> (4)
Log of 2015 Std. Needs $\times$ Policy coeff.	1.353*** (0.290)	0.993*** (0.307)		
Log of Current transfers			0.214*** (0.056)	0.288*** (0.102)
Observations	11,797	11,890	11,115	10,890
Number of municipalities	3,877	4,039	3,195	3,039
F statistic	6.340	4.940		
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor controls	YES	YES	YES	YES
K-P rk Wald F			21.743	10.470

Notes. The table reports first stage FE estimates and 2SLS estimates for the effect of log total current central transfers per capita on log current expenditures. Column (1) reports first stage estimates for the subsample of mayors with average votes received below the median. Column (2) reports first stage estimates for the subsample of mayors with average votes received above the median. Column (3) reports 2SLS estimates of the effect of current transfers on current expenditures for the subsample of mayors with average votes received below the median. Column (4) reports 2SLS estimates for the subsample of mayors with average votes received above the median. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Finally, Table 13 examines heterogeneity based on the mayors' educational

attainment, dividing the sample between graduated and non-graduated administrators. The estimates reveal that graduated mayors exhibit a significantly larger propensity to spend out of central transfers compared to non-graduated ones. This difference points toward the role of administrative capacity. Higher formal education may proxy for a greater ability to navigate municipal bureaucracy, allowing these mayors to more efficiently and rapidly absorb external funds and deploy them into local public programs. Alternatively, graduated administrators might simply pursue more complex, and consequently more costly, policy agendas.

Table 13: Heterogeneity with education

Variables	<i>First Stage on transfers</i>		<i>Current Expenditures</i>	
	<i>Non-graduated</i> (1)	<i>Graduated</i> (2)	<i>Non-graduated</i> (3)	<i>Graduated</i> (4)
Log of 2015 Std. Needs $\times$ Policy coeff.	1.236*** (0.329)	1.188*** (0.257)		
Log of Current transfers			0.171*** (0.058)	0.324*** (0.088)
Observations	12,991	10,696	12,824	10,557
Number of municipalities	3,926	3,331	3,759	3,192
F statistic	5.075	6.628		
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor controls	YES	YES	YES	YES
K-P rk Wald F			14.102	21.361

Notes. The table reports first stage FE estimates and 2SLS estimates for the effect of log total current central transfers per capita on log current expenditures. Column (1) reports first stage estimates for the subsample of non-graduate mayors. Column (2) reports first stage estimates for the subsample of graduate mayors. Column (3) reports 2SLS estimates of the effect of current transfers on current expenditures for the subsample of non-graduate mayors. Column (4) reports 2SLS estimates for the subsample of graduate mayors. All regressions include all available controls. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Taken together, these heterogeneity results confirm that the "flypaper effect" is not a uniform behavioral anomaly. Rather, the extent to which grant money "sticks where it hits" is significantly mediated by the personal characteristics

and political constraints of the local policymakers. By highlighting the role of political experience, electoral strength, and administrative capacity, this analysis provides further empirical support for the theoretical framework in which local fiscal decisions are deeply embedded in political economy incentives.

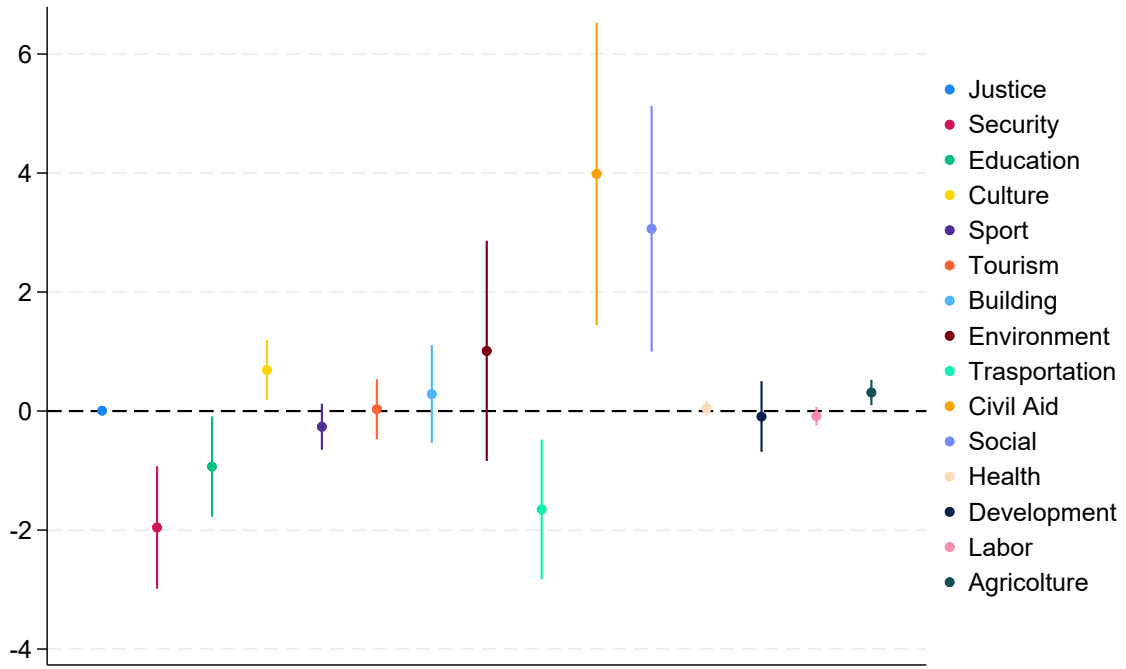
5.5 Budget Composition

Figures 7 and 8 present the estimated coefficients from the main model specification, with 2SLS regressions and all standardized spending's determinants included, on the share of current and capital expenditures for the so-called *Missions*, which represent each type of public service provided by municipalities.³³

When examining budget composition of current expenditures in Figure 7, some small but significant differences emerge. In particular, municipalities receiving higher transfers allocate less resources to the management of public order and safety, for education, as well as to transportation services. These expenditures all together accounts, on average, for 20% of total current expenditures. On the other hand, they allocate a higher share of their budgets to the protection and administration of cultural heritage, spend more in civil aid, disaster prevention, relief, and recovery, and support policies aimed at marginalized and vulnerable social groups, such as the elderly, minors, and people with disabilities. On the other hand, there is no significant impact on budget composition when examining detailed capital expenditures, as shown in Figure 8. The only significant coefficient is the share of investment dedicated to administration and operation of sports, recreational, and youth activities.

³³Shares of expenditures are measured in percentage points and are not log-transformed, while both the instrument and the endogenous variables are in log per capita. Thereby, the two figures report estimates of semi-elasticities of a linear-log specification of the main model.

Figure 7: Log of Current transfer on Current Expenditures shares, 2SLS estimates

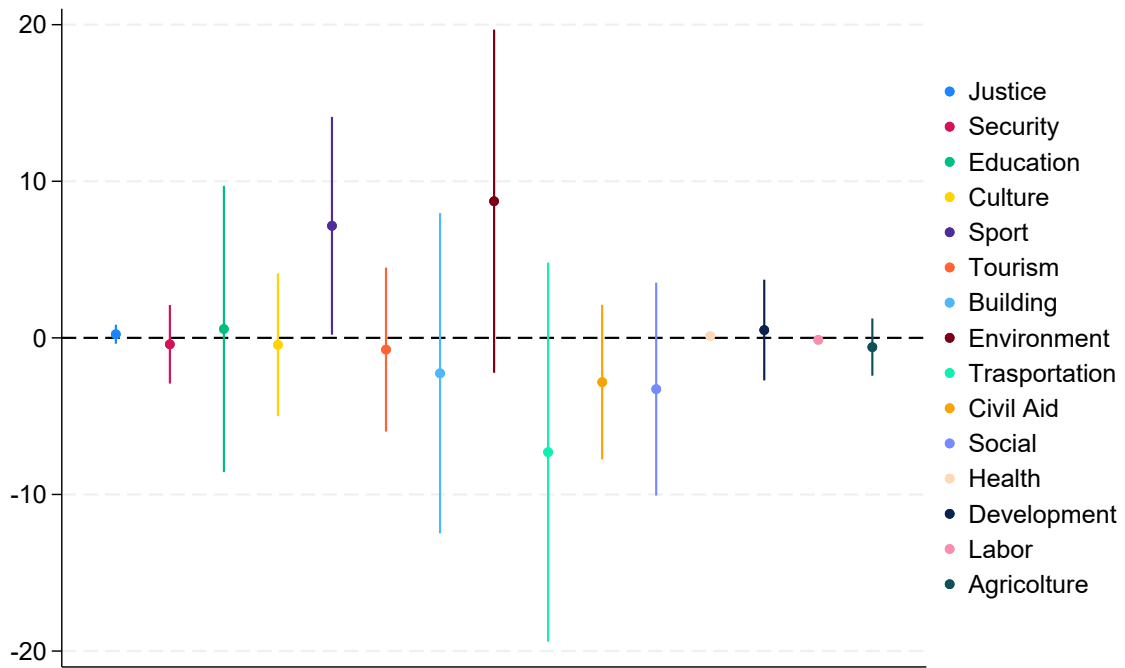


Notes. The graph reports the 2SLS point estimates of the effect of current transfers on current expenditures share of municipal budget Mission, with 95% confidence intervals, using standardized spending needs as instrument. All available controls are included in the regression. Robust standard errors clustered at municipal level.

Estimated coefficient for both current and capital expenditures decomposition are robust in terms of magnitude, sign and significance to those obtained with Fixed-effects regressions using the endogenous variable, as shown in Figure [D.1](#) to [D.4](#) in Appendix [D](#). In general, it is clear that intergovernmental transfers does have a non-negligible effect on current spending preferences of local governors, influencing allocation of resources in favor of some locally-provided public services.^{[34](#)}

³⁴One possible explanation to this difference follows the accountability arguments. If local politicians are less efficient when more dependent on external sources of revenues, and have relatively higher degree of discretionary power in some types of current expenditures, it could be the case that current expenditures rise mostly for those types, leaving the others unchanged.

Figure 8: Log of Current transfer on Capital Expenditures shares, 2SLS estimates



Notes. The graph reports the 2SLS point estimates of the effect of current transfers on capital expenditures share of municipal budget Mission, with 95% confidence intervals, using standardized spending needs as instrument. All available controls are included in the regression. Robust standard errors clustered at municipal level.

Compared to [Bianchi et al. \(2023\)](#), the study most closely related to mine, which explores the impact of fiscal decentralization on expenditure composition through the lens of municipal taxation¹ find fewer differences in overall budget composition and no evidence of an increased share of expenditures dedicated to revenue-generating policies. Specifically, [Bianchi et al. \(2023\)](#) provide evidence that an exogenous increase in municipal fiscal autonomy induced local administrators to prioritize revenue-generating and customer-facing services. Furthermore,

¹This can in fact cause a shift in terms of shares. Information in the current dataset are insufficient to tests for this hypothesis. Future version of this paper will include an analysis tackling potential mechanism explaining budget composition differences.

they show that this decentralization expanded the provision of local welfare services, such as public nursery schools, which ultimately led to a substantial increase in female labor-force participation and a reduction in the gender employment gap.

5.6 Historical component of intergovernmental transfers

A core premise of the primary identification strategy is that the main instrument—standardized spending needs—remains independent of historical local expenditures. To empirically test this assumption, I verify whether the baseline results hold when augmenting the 2SLS model with a second, additional instrument, used simultaneously alongside the spending needs to exogenously capture the historical component of the FSC transfers. Because the actual historical baseline employed in the official FSC formula is endogenous to past local fiscal policies, I adapt the approach proposed by Chiades et al. (2019), who successfully instrumented Italian municipal grants using historical per-capita transfers from 1979. However, employing this static 1979 baseline directly is insufficient in the current context due to the evolving nature of the reform. By law, the total grant allocation is a weighted average, where the historical component is explicitly weighted by $(1 - \mu)$. Because the policy parameter μ strictly increases over time to progressively favor the standardized criteria, the actual influence of the historical component mechanically shrinks each year. Therefore, to accurately replicate this statutory mechanism, the additional instrument is constructed by interacting the time-invariant 1979 transfers with the time-varying statutory weight $(1 - \mu)$. This interaction ensures that this second instrument precisely proxies the dynamically declining impact of historical criteria on modern grant allocations.

Table 14: Main results with additional instrument for historical transfers

Variables	First Stage (1)	(2)	Current Exp. (3)	(4)	Capital Exp. (5)	(6)	Tax Revenues (7)	(8)	No-Tax Rev. (9)	(10)
Log 2015 Std. Needs $\times$ Policy coeff.	1.355*** (0.180)	1.254*** (0.208)								
Log 1979 Transfers $\times$ (1 - Policy coeff.)	-0.490*** (0.185)	-0.094 (0.204)								
Log of Current Transfers			0.127*** (0.029)	0.207*** (0.044)	-0.549** (0.234)	-0.429 (0.292)	0.029 (0.040)	-0.000 (0.042)	0.092 (0.066)	0.108 (0.084)
Observations	25,569	23,667	25,561	23,628	25,416	23,488	25,561	23,628	25,561	23,628
Number of municipalities	6,436	6,146	6,428	6,107	6,421	6,096	6,428	6,107	6,428	6,107
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
K-P rk Wald F			32.333	18.766	32.882	19.252	32.333	18.766	32.333	18.766
Hansen J p-value			0.000	0.933	0.017	0.672	0.762	0.698	0.008	0.029

Notes. The table reports FE estimates for the first stage and main results with the inclusion of the additional instrument measuring historical transfers. Columns (1) and (2) represent the first stage. Columns (3) and (4) report 2SLS coefficients on current expenditures. Columns (5) and (6) report 2SLS coefficients on capital expenditures. Columns (7) and (8) report 2SLS coefficients on current tax revenues. Columns (9) and (10) report 2SLS coefficients on current no-tax revenues. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 14 presents the first-stage and 2SLS estimates for the main fiscal outcomes using the overidentified model. The first-stage coefficients of the main instrument remain stable when this second instrument is added to the first-stage equation. The historical instrument itself is significant in explaining current transfers only in the specification that excludes the standardized spending determinants.

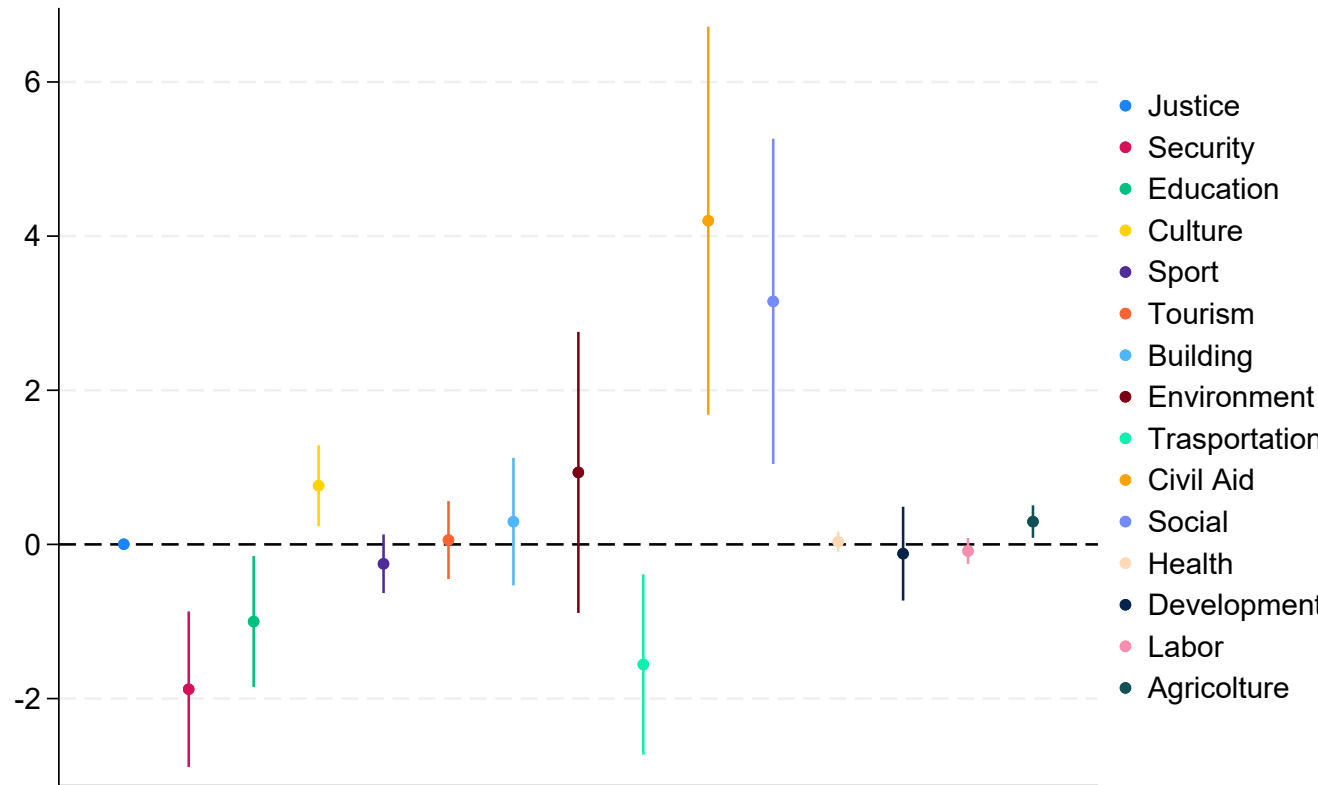
Furthermore, the 2SLS estimates for current and capital expenditures, as well as tax and non-tax revenues, are consistent in sign, magnitude, and significance with those from the baseline exactly identified model.³⁵ Overall, the stability of the estimates in this multiple-instrument setting suggests that the baseline results are not driven by the endogenous historical component, and consequently, by potential reverse-causality bias.

In Figure 9 and 10 I conduct the analysis with two instrumental variables on the share of current and capital expenditures. Results align with those estimated in the main model specification.

Following a similar logic, Appendix B presents an additional robustness check that estimates an alternative overidentified model. In this supplementary specification, the second excluded instrument is constructed as the interaction between the statutory policy coefficient and the municipality's standardized tax capacity, fixed at its 2015 level. The main 2SLS results remain robust to the inclusion of this alternative instrument.

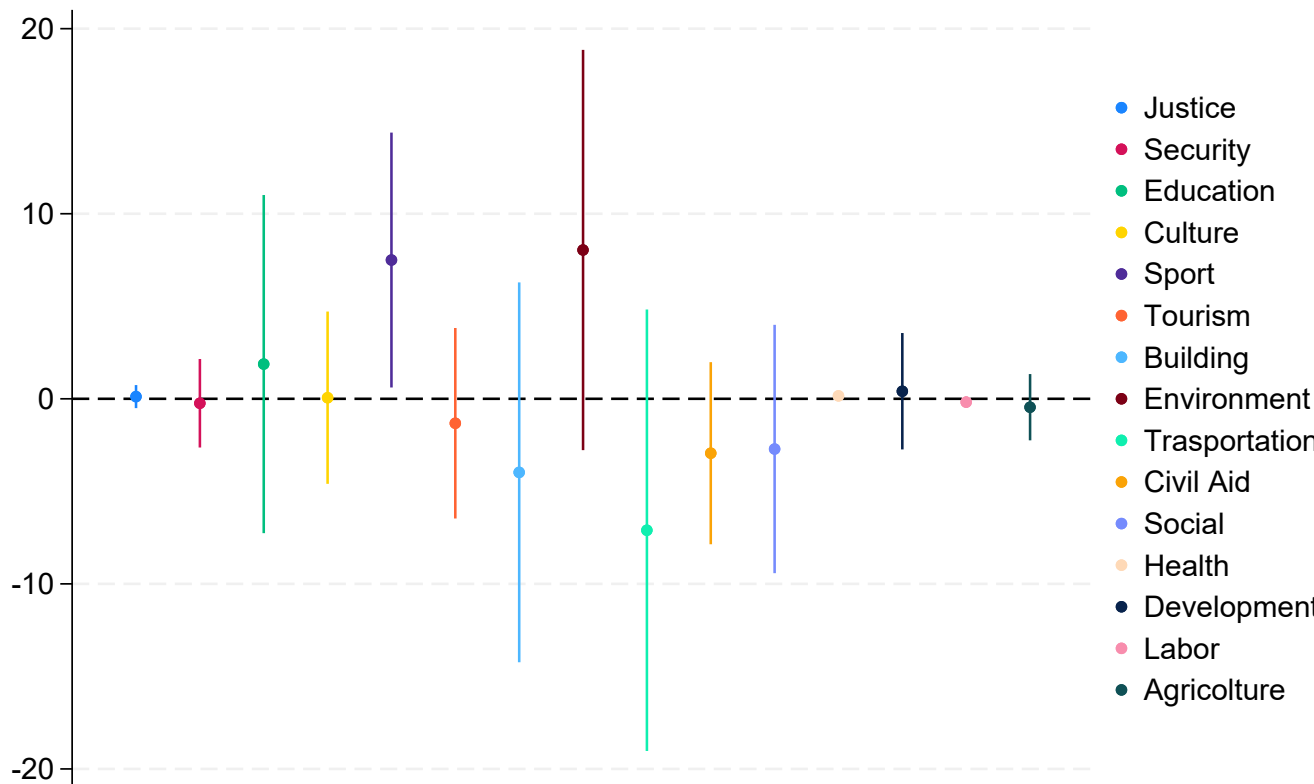
³⁵It should be noted that the two instruments do not pass the Hansen J-test for overidentification when time-varying controls are omitted.

Figure 9: Log of Current transfer on Current Expenditures shares, 2SLS estimates with additional historical instrument



Notes. The graph reports the 2SLS point estimates of the effect of current transfers on current expenditures share of municipal budget Mission, with 95% confidence intervals, using both standardized spending needs and current expenditures in 1979 as instruments. All available controls are included in the regression. Robust standard errors clustered at municipal level.

Figure 10: Log of Current transfer on Capital Expenditures shares, 2SLS estimates with additional historical instrument



Notes. The graph reports the 2SLS point estimates of the effect of current transfers on capital expenditures share of municipal budget Mission, with 95% confidence intervals, using both standardized spending needs and current expenditures in 1979 as instruments. All available controls are included in the regression. Robust standard errors clustered at municipal level.

6 Concluding remarks

This paper has provided a comprehensive review of the theoretical and empirical literature on fiscal federalism, focusing on the complex dynamics introduced by intergovernmental grants. As discussed, the literature has evolved from first-generation models, which posit revenue neutrality and benevolent policymaking,

to second-generation theories that emphasize political economy frictions. A central finding in this body of work is the “flypaper effect”-the empirical regularity that central transfers stimulate local public spending significantly more than an equivalent increase in private income. Rather than a mere statistical artifact, the literature suggests this phenomenon is driven by behavioral factors such as fiscal illusion and the opportunistic behavior of local politicians, who may exploit asymmetric information to expand budgets without bearing the political cost of raising local taxes. Furthermore, external dependency can weaken local accountability and structurally reshape the composition of public expenditures.

To complement this literature review, the paper includes an empirical exercise designed to explore these theoretical dynamics within the context of Italian municipalities. Using an instrumental variable approach based on the 2015 institutional reform, the estimates are consistent with the main findings of the existing literature. Specifically, the analysis suggests that higher current central transfers in the short run lead to increased current expenditures, without exhibiting significant crowding-out effects on local taxation. Furthermore, the findings provide suggestive evidence that failing to account for reverse-causality bias may lead to an underestimation of the impact of current transfers on current expenditures, and an overestimation of their negative effects on taxes.

Overall, these empirical results lend additional support to the presence of a short-run flypaper effect. As highlighted in the theoretical framework, increased external dependency can generate space for political opportunism, as local administrators might increase expenditures without directly harming the local tax base, potentially exercising less caution when implementing costly local policies. Additionally, the empirical evidence supports the hypothesis that varying levels

of reliance on central transfers systematically influence the allocation of resources across different types of public expenditures.

While the results clearly indicate an increment in local expenditures following an increase in grants, the present analysis is not sufficient to definitively disentangle the underlying mechanisms driving this response. On the one hand, this expansion could reflect a physiological 'catching-up' effect: municipalities previously constrained by a lack of resources might be utilizing these transfers to finally meet the baseline demand for essential public services. On the other hand, the increase in spending could be symptomatic of fiscal inefficiencies. As local institutions become more reliant on external funding, the diminished political cost of spending—compared to raising local taxes—might relax local accountability and foster suboptimal resource allocation. Therefore, determining whether this increment represents a welfare-enhancing correction or a distortional effect of external dependency remains a critical avenue for future research. Further extensions of this research will aim to incorporate downstream indicators, such as municipal employment and specific measures of service provision quality, to properly assess these real-world impacts. Nevertheless, the combined theoretical review and empirical findings indicate that policymakers must carefully consider the trade-off between equalization goals and behavioral distortions. Designing effective decentralization policies requires striking a delicate balance between resources collected locally and central government transfers allocated to municipalities.

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APPENDIX

A The FSC formula

The 2015 reform introduced a methodology based on standardized measurements of local spending needs and tax capacity, with the goal of equalizing resources among sub-national governments.

Resources from the FSC were allocated to municipalities using a linear combination of the new standardized criteria and the old historical criteria (the latter fixed at 2015 values). The weight assigned to the standardized methodology gradually increased over time, with the goal of making transfers fully dependent on the new criteria by 2029.

During the period of interest, 2016 to 2019, the formula used to distribute funds among municipalities was as follows:

$$FSC_{i;t} = \mu_t \Delta_{std;i;t} + (1 - \mu_t) \Delta_{his;i;2015} + Adj_{i,t} + quota_{i;2015} \quad (6)$$

In Equation [6](#), $FSC_{i;t}$ represents the total amount of transfers received by municipality i in year t from the FSC; $\Delta_{std;i;t}$ is the portion of transfer calculated using the standardized criteria; $\Delta_{his;i;2015}$ denotes the share of transfer computed based on the historical criteria fixed at 2015 level; the $Adj_{i,t}$ is a statistical adjustment mechanism designed to limit the impact of the reform by constraining the variation in transfers over time within fixed boundaries; $quota_{i;2015}$ is the participation fee for financing the total resources of the FSC at 2015 level; μ_t

denotes the weight assigned to each criterion.³⁶ In particular, the standardized component $\Delta_{std;i;t}$ is defined as:

$$\Delta_{std;i;t} = F_{std,i;t} - C_{std;i;t} \quad (7)$$

where $F_{std,i;t}$ represents the effective spending needs of a municipality, essentially reflecting the demand for locally provided public services, and $C_{std;i;t}$ measures the standardized fiscal capacity of each municipality.

³⁶It is equal to 0.0916 in 2015, 0.1374 in 2016, 0.2 in 2017 and 0.275 in 2018 and 2019

B Tax capacity Instrument

In principle, it would be possible to increase the explanatory power of the instrument by including the effect of the 2015 standardized tax capacity, interacted with the policy coefficient. While the standardized measurement can be fixed at 2015 levels and its calculation is carried out by the technical committee independently of local political choices, the Opencivitas dataset does not include the local tax bases used to calculate standardized tax capacity, as it does for standardized demand. Therefore, I cannot guarantee the exclusion assumption of such an instrument. Nevertheless, if the estimated coefficients remain consistent when including this new instrument, it would provide supporting evidence that the main model is sufficiently precise in accounting for differences in tax capacity. Table [B.1](#) shows the estimated coefficients for the first stage and the main results on expenditures and revenues. The 2SLS estimated coefficients are similar in sign, magnitude, and significance to those obtained in the main model specification.

Table B.1: Main results with additional instrument for 2015 Standard Tax Capacity

Variables	First Stage (1)	(2)	Current Exp. (3)	(4)	Capital Exp. (5)	(6)	Tax Revenues (7)	(8)	No-Tax Rev. (9)	(10)
Log 2015 Std. Needs $\times$ Policy coeff.	0.835*** (0.318)	0.253 (0.329)								
Log 2015 Std. Tax Cap. $\times$ Policy coeff.	-0.782 (0.507)	-1.890*** (0.593)								
Log of Current Transfers			0.077*** (0.029)	0.207*** (0.045)	-0.812*** (0.262)	-0.494* (0.284)	0.036 (0.041)	0.013 (0.039)	0.068 (0.067)	0.150* (0.082)
Observations	25,737	23,687	25,729	23,648	25,584	23,508	25,729	23,648	25,729	23,648
Number of municipalities	6,478	6,151	6,470	6,112	6,463	6,101	6,470	6,112	6,470	6,112
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
K-P rk Wald F			25.601	19.497	25.986	20.104	25.601	19.497	25.601	19.497
Hansen J p-value			0.000	0.977	0.002	0.541	0.151	0.396	0.140	0.106

Notes. The table reports FE estimates for the first stage and main results with the inclusion of the additional instrument measuring 2015 standardized tax capacity. Columns (1) and (2) represent the first stage. Columns (3) and (4) report 2SLS coefficients on current expenditures. Columns (5) and (6) report 2SLS coefficients on capital expenditures. Columns (7) and (8) report 2SLS coefficients on current tax revenues. Columns (9) and (10) report 2SLS coefficients on current no-tax revenues. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

C Linear regressions

Table [C.1](#) presents regressions for the first stage and main results on revenues and expenditures in a linear specification of the model. In the linear specification, the instrument is statistically significant in the first stage only when not accounting for all 33 determinants of standardized demand. Moreover, the instrument fails to pass the Stock-Yogo test for weak instruments, with the Wald F statistic on the excluded instrument dropping to 8.9 when additional covariates are not included. Nevertheless, in these latter cases, the sign and significance of the estimated coefficients are similar to those observed with the logarithmic specification in the main model and are consistent with estimates from other research in the field, such as those by [Helm and Stuhler \(2024\)](#) or [Leduc and Wilson \(2017\)](#). Further research is required to better understand why the linear specification performs poorly in terms of instrument relevance. One potential explanation is the presence of multiple extreme outliers in both the excluded instrument and the current intergovernmental grants. Table [C.2](#) presents estimated coefficients of a linear specification of the model, directly employing the endogenous explanatory variable, current central transfers, in the regressions on revenues and expenditure. Results are consistent with log specifications of the model.

Table C.1: Main results with linear specification of main models

Variables	First Stage (1)	(2)	Current Exp. (3)	(4)	Capital Exp. (5)	(6)	Tax Revenues (7)	(8)	No-Tax Rev. (9)	(10)
Euro per-capita 2015 Std. Needs $\times$ Policy coeff.	1.701*** (0.571)	-0.620 (0.540)								
Euro per-capita Current Transfers			1.919** (0.782)	-1.874 (2.892)	3.736 (3.279)	2.264 (11.580)	0.482* (0.264)	-1.616 (1.549)	0.472 (0.323)	-1.371 (1.462)
Observations	25,842	23,786	25,840	23,754	25,840	23,754	25,840	23,754	25,840	23,754
Number of municipalities	6,484	6,157	6,482	6,125	6,482	6,125	6,482	6,125	6,482	6,125
Estimator	FE OLS	FE OLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS	FE 2SLS
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
K-P rk Wald F			8.867	1.320	8.867	1.320	8.867	1.320	8.867	1.320

Notes. The table reports FE estimates for the first stage and main results of a fully linear specification of the main model. Columns (1) and (2) represent the first stage. Columns (3) and (4) report 2SLS coefficients on current expenditures. Columns (5) and (6) report 2SLS coefficients on capital expenditures. Columns (7) and (8) report 2SLS coefficients on current tax revenues. Columns (9) and (10) report 2SLS coefficients on current no-tax revenues. Robust standard errors clustered at the municipal level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.2: Main results with linear specification and endogenous explanatory variable

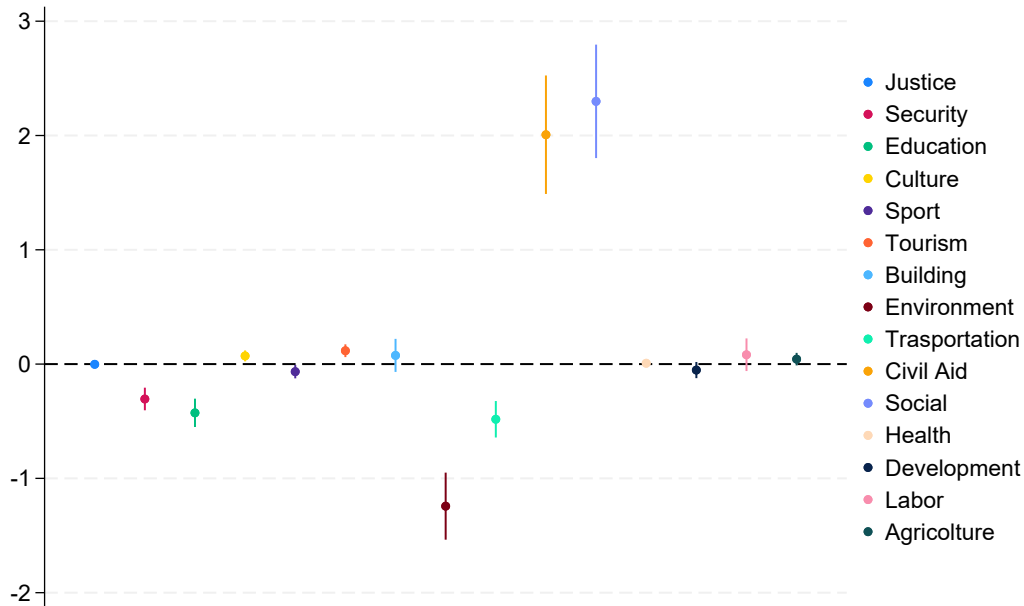
Variables	Current Exp. (1)	Capital Exp. (2)	Capital Exp. (3)	Tax Revenues (4)	Tax Revenues (5)	No-Tax Rev. (6)	No-Tax Rev. (7)	No-Tax Rev. (8)
Current Transfers	0.753*** (0.082)	0.755*** (0.106)	0.183* (0.110)	0.234** (0.102)	-0.046*** (0.015)	-0.032*** (0.011)	-0.012 (0.014)	-0.006 (0.013)
Observations	25,874	23,786	25,874	23,786	25,874	23,786	25,874	23,786
Number of municipalities	6,492	6,157	6,492	6,157	6,492	6,157	6,492	6,157
Estimator	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS	FE OLS
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipality controls	NO	YES	NO	YES	NO	YES	NO	YES
Mayor controls	NO	YES	NO	YES	NO	YES	NO	YES

Notes. The table reports FE estimates for linear regressions of the endogenous explanatory variable and main outcomes of interest. Columns (1) and (2) report estimated coefficients for the effect of central grants on current expenditures. Columns (3) and (4) report FE estimated coefficients on capital expenditures. Columns (5) and (6) report FE estimated coefficients on current tax revenues. Columns (7) and (8) report FE estimated coefficients on current no-tax revenues. Robust standard errors clustered at the municipal level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

D Budget Composition with endogenous explanatory variable

Figures [D.1](#) to [D.4](#) show estimates of the endogenous variable, log-transformed and in linear form respectively, with all determinants of standardized spending and mayor-level controls included, on the share of current and capital expenditures for each Mission.

Figure D.1: Log of Current transfer on Current Expenditures composition, FE OLS estimates



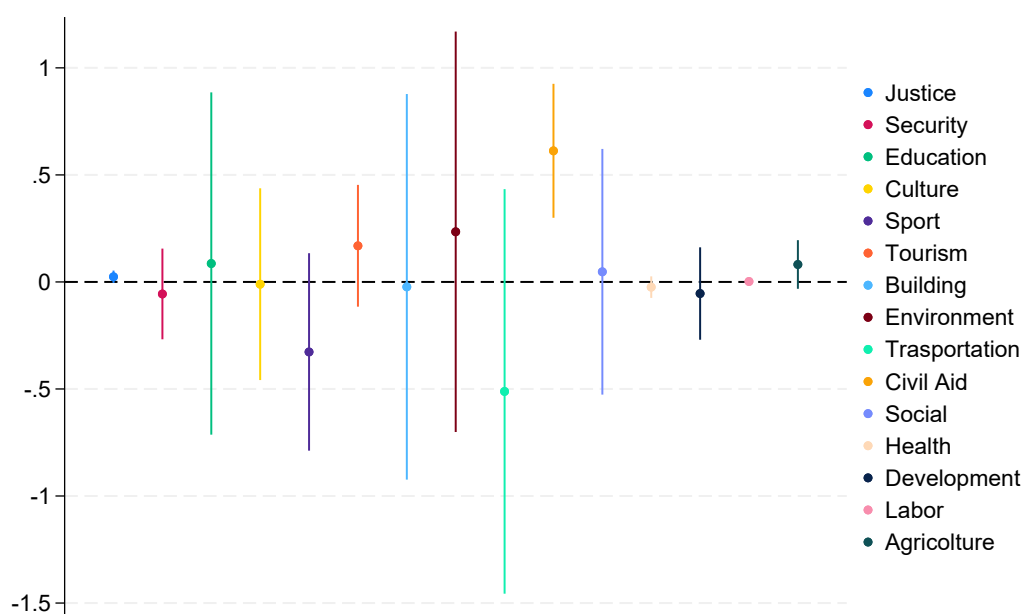
Notes. The graph reports the OLS FE point estimates of the log of current transfers on current expenditures share of municipal budget Mission, with 95% confidence intervals. All available controls are included in the regression. Robust standard errors clustered at the municipal level.

Figure D.2: Current transfer on Current Expenditures composition, FE OLS estimates



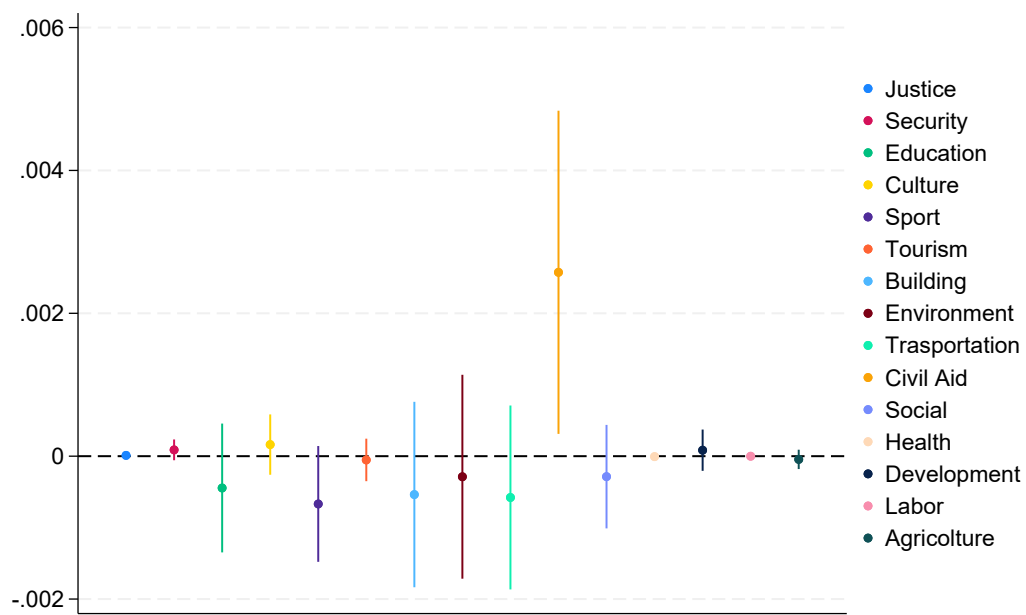
Notes. The graph reports the OLS FE point estimates of linear current transfers on current expenditures share of municipal budget Mission, with 95% confidence intervals. All available controls are included in the regression. Robust standard errors clustered at the municipal level.

Figure D.3: Log of Current transfer on Capital Expenditures composition, FE OLS estimates



Notes. The graph reports the OLS FE point estimates of the log of current transfers on capital expenditures share of municipal budget Mission, with 95% confidence intervals. All available controls are included in the regression. Robust standard errors clustered at the municipal level.

Figure D.4: Current transfer on Capital Expenditures composition, FE OLS estimates



Notes. The graph reports the OLS FE point estimates of linear current transfers on capital expenditures share of municipal budget Mission, with 95% confidence intervals. All available controls are included in the regression. Robust standard errors clustered at the municipal level.

E Additional Summary Statistics

Table E.1: Summary on budget shares

	Current (1)	Capital (2)
Management	38.87 (12.35)	17.21 (22.20)
Justice	0.0278 (0.235)	0.0406 (0.879)
Security	3.923 (3.259)	1.384 (5.079)
Education	8.988 (5.101)	13.33 (20.20)
Culture	1.590 (1.899)	2.982 (9.304)
Sport	1.229 (1.466)	5.261 (11.71)
Tourism	0.684 (1.714)	1.321 (7.178)
Building	1.778 (3.591)	12.53 (21.55)
Environment	20.04 (8.865)	10.96 (18.84)
Transportation	8.711 (5.163)	25.82 (25.25)
Civil Aid	0.792 (4.889)	2.246 (10.96)
Social	9.804 (8.034)	5.687 (12.28)
Health	0.109 (0.584)	0.0393 (0.971)
Development	0.669 (2.617)	0.930 (5.934)
Labor	0.0648 (0.795)	0.00609 (0.284)
Agriculture	0.0681 (0.472)	0.258 (2.950)
Energy	0.722 (4.684)	3.109 (122.5)
Local Relationship	0.153 (2.204)	0.0550 (2.639)
International Relationship	0.00212 (0.0598)	0.00134 (0.141)
Funds	0.0312 (0.434)	0.0214 (1.267)
Debt	1.571 (2.505)	- -
Financial Advances	0.174 (2.663)	- -
Observations	25814	25727

Table E.2: Summary on Std. Needs determinants (1)

	(1)
Open markets, days	4.427 (4.818)
Parking stalls	1.369 (6.720)
Employees in the events and accomodation sectors	7.129 (7.911)
kindergarten	11.81 (14.83)
Incoming commuters net of outgoing ones	0.368 (1.481)
Diseconomies of scale	68.22 (112.1)
Macro-area of intervention in Structures	1.987 (2.238)
Real estate	54.55 (27.03)
Population above 65	61.75 (23.92)
Population between 3 and 14	29.89 (10.31)
Population density	1.247 (3.674)
Streets of municipality	14.26 (33.86)
Area of municipality	35.69 (77.10)
Museum visitors	0.277 (2.807)
Students in public schools	0.398 (2.562)
Students with disabilities	0.313 (2.639)
Observations	25960

Table E.3: Summary on Std. Needs determinants (2)

	(1)
Students in private schools	1.686 (2.748)
Users in pre and post school centers	0.907 (1.435)
Schools in municipal complexes	11.68 (10.84)
Schools in municipal complexes, mq2	2.904 (2.314)
Public transports for students with disabilities	0.789 (1.538)
Public transports for students	8.327 (9.077)
Real estate rental costs for office use	-2.044 (20.61)
Other Exogenous output	0.894 (1.398)
Tourists	3.895 (16.04)
Average expenditures for software and hardware	-0.936 (1.551)
Police veicles related costs	-0.281 (5.082)
Schools	6.345 (4.841)
High seismic risk	3.730 (11.43)
Road accidents	3.919 (4.816)
Complaints	0.532 (1.571)
Sorted wastes	31.37 (14.20)
Extended-time classes	1.533 (2.000)
Observations	25960
	166

Chapter 3: Public Events and Political Returns. The Electoral Impact of Patron Saint Celebrations

1 Introduction

When considering the factors that explain the probability of a candidate's election in municipal governments, holding the position of incumbent mayor offers clear advantages in the final electoral outcome. Such a causal effect has been estimated at both national (Lee, 2008) and local level (Trounstein, 2011; Krebs, 1998). Between 1994 and 2019 in Italy, approximately 50% of mayors sought a second term, with 77% of them winning re-election.¹ The literature provides different factors potentially explaining the incumbency advantage. On one hand, some authors stated that part of the effect could be explained by incumbent politicians being selected as higher quality candidates in previous elections. Therefore increased probability of re-election is a reflection of electoral selection based on individual characteristics (Fowler, 2016; Ashworth and Bueno de Mesquita, 2008; Hirano and Snyder, 2009), following the Zaller (1998) example on the "Prize

¹Source: own calculations using electoral data from the Italian Ministry of Interior.

Fighter". On the other hand, empirical analysis provide evidences that holding an office has a causal impact on the probability of re-election even when discounting the effect related to party affiliation and incumbents quality (Fowler, 2016; Fowler and Hall, 2014; Hirano and Snyder, 2009; Ansolabehere et al., 2000). The incumbent candidate had already secured a majority in previous elections, thereby establishing a loyal base of voters that was consolidated throughout the duration of the term (Fowler, 2018; Ansolabehere et al., 2000). Moreover, incumbency makes it harder for voters to distinguish between lower and higher quality candidates (Gordon and Landa, 2009). Uninformed voters can consider incumbency as a signal of higher quality over opposition, thereby increasing probability of re-election of the incumbent (Fowler, 2018). Additionally, the existence of an incumbency advantage could discourage higher quality opposing candidates to participate in the electoral competition (Ashworth and Bueno de Mesquita, 2008; Goodliffe, 2005). In addition to the accumulation of political power, incumbents can strategically leverage their position to build broader support. The political economy literature has examined in depth the presence of a budget cycle linked to the timing of elections (Repetto, 2018; Alesina and Paradisi, 2017; Roubini et al., 1997). Incumbents running for a second mandate tend to reduce expenditures and increase taxes in the year following their first election (Revelli and Zotti, 2019), while increasing short-term investments and current expenditures in the year preceding the second election (Alesina and Paradisi, 2017). In doing so, they anticipate policies that may reduce consensus in the first year of their mandate, while appearing more active and effective as the next election approaches. This timing offers incumbents an opportunity to improve their public image in ways that opposing candidates cannot easily replicate. Furthermore, an-

other explanation of the incumbency advantage relates to name recognition and media coverage. Incumbent politicians are more capable of attracting resources for political campaigning (Fouirnaies and Hall, 2014), and can exploit the work done during mandate as a stronger argument during such campaigns (Druckman et al., 2020). Moreover, they receive greater media coverage (Prior, 2006) and receive greater returns in term of probability of re-elections from increased media exposure (Snyder Jr and Strömberg, 2010).² The familiar, more famous name of an incumbent can represents a great advantage in electoral term which can boost electoral results for the incumbent (Kam and Zechmeister, 2013; Krebs, 1998). When considering the effect of visibility and exposure to the general population in local elections, the office of mayor can potentially provide another strategic opportunity to gain additional advantages over opponents. Mayors may be involved in organizing local events and community activities, in which they are often invited to participate as hosts, thereby increasing their visibility. As elections approach, such signal can eventually impact the probability of securing victory at the polls. That is exactly the causal effect this paper aims at identifying. Specifically, the goal is to measure whether there is an advantage enjoyed by incumbent candidates in relation to public events held in proximity to local elections. To this end, I exploit a quasi-natural experiment occurring in Italian cities, which allows for the identification of the causal effect of public events and community activities on the probability of re-election, following the example of Revelli and Zotti (2019), which tested the impact of Patron Saint day on voters turnout and quality of candidates elected, as well as Atkinson and Fowler (2014)

²But see also Ansolabehere et al. (2006), which contested the effect of television coverage in explaining incumbency advantage.

which similarly studied the effect of Mexican Saint Day on voters turnout. In particular, I compare cities in which the Patron Saint Day occurs before and near the election date with those in which the event is temporally distant or happened after elections, focusing on a subsample of elections where incumbent mayors are running for re-election.

The Patron Saint Day is a significant event in Italian municipalities, widely attended by local communities as a long-standing tradition, valued not only for its religious significance.³ A survey conducted in 2016 by Confesercenti, an Italian association formed by entrepreneurs, reported that at least 11% of the Italian population consider the Patron Saint festivity as one of the most important holidays in Italy, above other Italian secular holidays such as the 25th of April (on this day, Italians celebrate the end of the Nazi occupation and the fall of the Fascist regime in 1945) and the 1st of May.⁴ In each municipality, it is held annually on a fixed date, typically established centuries ago.⁵ Municipal election dates, on the other hand, are determined by the central government independently of local administrations, are generally held on the same day (with limited exceptions) and are staggered across different years.⁶ Assignment to treatment is based on

³See, for example, the work of Ivona and Privitera (2023) on the value of these festivity for local population and tourism attraction, or Rossi (1969), who associate Patron Saint days with folkloristic events representing a key element in the definition of local cultural identity independently from religious influences.

⁴Source: Confesercenti (2016)

⁵The date is usually tied to the corresponding day in the Catholic calendar dedicated to that Saint, to the supposed day of the Saint's death, or to a miracle attributed to the Saint in that city.

⁶While there is a fixed duration of each mandate and the majority of elections happen in the same year, this is not always true for all local elections. Some elections could be anticipated or delayed for different reasons, such as historical effects, legal issues or premature resignations. Consequently, Italian municipal elections' structure provides an empirical setting where relevant features such as the political budget cycle do not result collinear with global time-fixed confounding factors.

the proximity between the election date and the Patron Saint Day. As both sources of variation in dates of elections and Patron Saint celebrations are exogenous with respect to time-varying characteristics of municipalities and incumbent candidates, assignment to treatment can be considered as-good-as random.

The analysis shows that when the Patron Saint Day occurs near the election, incumbent mayors have approximately a 12% higher probability of being re-elected. The results are robust to the inclusion of geographical fixed effects, measures of local religiosity, the impact of rainfall on Patron Saint Days (which could compromise the event's success), and a variety of municipal and individual controls and model specifications.

The heterogeneity analysis reveals that this electoral advantage is largely homogeneous across geographical areas and mayor characteristics. I find some suggestive, albeit weaker, evidence indicating that the effect is stronger in more religious municipalities. However, the most notable exception emerges with respect to gender: female mayors appear to benefit significantly more from the festivities compared to their male counterparts.

Finally, regarding voter participation, I document a positive and statistically significant effect on turnout, specifically for incumbent mayors seeking a second term. While the magnitude of this increase is relatively contained given the high baseline turnout, these findings support the hypothesis that incumbents strategically leverage these community events to mobilize voters, overcoming the potential organizational costs of the festivity suggested by previous literature (Atkinson and Fowler, 2014).

Three potential mechanisms may explain the increased probability of re-election for incumbent mayors following Patron Saint celebrations. First, such

events may generate a short-term boost in subjective self-perceived happiness (Campante and Yanagizawa-Drott, 2015; Deaton and Stone, 2013; Dolan et al., 2008). Increased happiness could lead to a shift of preferences in favor of the incumbent candidate (Ward et al., 2021). Second, incumbents benefit from enhanced visibility and opportunities to promote their agenda during the celebrations, increasing their support (Druckman et al., 2020; Kam and Zechmeister, 2013; Snyder Jr and Strömberg, 2010). Third, successful festivities may serve as a signal of incumbent valence. Imperfectly informed voters may shift their support toward a candidate with higher perceived quality (Fowler, 2018). This effect is potentially magnified in communities that have recently experienced heightened social cohesion due to Patron Saint celebrations, which appear to increase the salience of candidate quality relative to ideological positioning (Revelli and Zotti, 2019).

This paper contributes to the incumbency advantage literature by expanding possible opportunities exploitable by incumbent politicians to gather greater consensus in second-mandate elections. While the Patron Saint day is a specific event occurring mostly in states with strong christian traditions, there are many examples of this type of events dedicated to a religious Patrons or Protectors of local communities in nations of different religions.⁷ The causal relationship between local events and the incumbency advantage could be potentially extended to any type of local event where incumbent mayors have an opportunity to participate, with its effectiveness and magnitude depending on how much the event is broadly attended by the local population and the effective proximity to elections.

⁷For example, celebrations dedicated to Mumbadevi in Mumbai or Ganesh in Nagpur for the Hinduism, or Japanese local "matsuri", celebrations dedicated to Kami, for the Shintoism

This paper is organized as follow: Section 2 summarizes literature related to religious and community activity, electoral outcomes and incumbency advantages; Section 3 explains the Italian institutional framework in which the analysis is conducted; Section 4 reports dataset sources and construction process; Section 5 demonstrate the validity of the identification assumption; Section 6 shows the results of the main analysis; Section 7 reports final concluding remarks.

2 Literature review

This section reviews the literature at the intersection of sociology, political science and political economy, focusing on how community-based religious festivals, such as Patron Saint Days, may affect electoral outcomes.

Religious festivals often serve as focal points for community interaction and identity formation, consequently affecting preferences of voter and political outcomes. Revelli and Zotti (2019) show that Patron Saint Days in Italy influence fiscal timing and candidate selection: tax hikes are postponed when elections and festivals coincide, and such concurrence favors the election of better-educated, high-skilled candidates. The authors interpret this shift as a temporary transformation in voter preferences, from individualistic to community-oriented interests. However, they do not find evidence that increased social capital from religious celebrations affects voter turnout.

Analogous effects are observed in other contexts. Atkinson and Fowler (2014) document that Mexican Saint Day Fiestas events culturally similar to Italian Patron Saint celebrations foster social capital but simultaneously reduce voter turnout. The authors argue that the high cost of participation (in terms of time

and effort) may dis-incentivize electoral participation.

The mechanism through which community participation affects political behavior has been also previously studied. [Krishna \(2007\)](#) highlight that social capital can be generated internally through local leadership and self-organized groups. [Gerber et al. \(2008\)](#) show that social pressure significantly boosts turnout in large-scale field experiments, indicating that civic norms can be powerful motivators. Patron Saint celebrations represent an opportunity to meet the local population, exchange ideas and perceive such social pressure ([Revelli and Zotti, 2019](#)), increasing social capital and potentially affecting political outcomes.

[Prete and Revelli \(2017\)](#) offer a model where costly voting leads to lower turnout but potentially higher quality candidate selection, as those who do vote are more ideologically aligned or informed. This complements findings by [Revelli and Zotti \(2019\)](#), where voters shift preferences toward higher-quality candidates during Patron Saint Days.

Access to information can modify electoral dynamics. [Repetto \(2018\)](#) find that transparency reforms in Italian municipalities reduced opportunistic pre-electoral spending, particularly in areas with higher newspaper readership. [Alesina and Paradisi \(2017\)](#) confirm the existence of political budget cycles in Italy, showing that municipalities reduce tax rates before elections, especially in smaller towns. Community events provide a chance to increase sharing of information and to reduce the amount of uninformed voters, consequently, reducing the impact of politicians' strategic behaviors.

This paper also relates to several studies documenting strong incumbency advantages in local elections. [Freier \(2015\)](#) report a substantial incumbency effect in German municipalities, amplified by local spending and unaffected by polit-

ical affiliation. [Frank et al. \(2023\)](#) show that higher turnout increases incumbent vote share in Bavarian elections, exploiting a natural experiment involving postal voting during the COVID-19 pandemic. Institutional settings also matter. [Trounstein \(2013\)](#) find that low-turnout environments increase the electoral success of incumbents and skew spending toward organized interest groups. [Fowler \(2018\)](#) provide a Bayesian learning model, where voters interpret incumbency as a positive signal of candidate quality, especially under conditions of informational asymmetry. In Indonesia, [Lewis et al. \(2020\)](#) show that incumbents are more likely to win reelection when local service delivery improves. However, second-term incumbents tend to reduce spending on education and health, possibly due to reduced electoral accountability. [De Benedetto \(2019\)](#) use Italian municipal data to separate personal from partisan incumbency effects, finding that re-election is primarily driven by personal advantages, particularly in Southern Italy.

Overall, the interaction between religious celebration, social behavior, and electoral dynamics provides fertile ground to explore how civic rituals influence democratic outcomes at the local level. This paper contributes to the literature by identifying a positive causal relationship between community events and the probability of elections. In addition, it provides another additional explanation for the incumbency advantage specific to local elections.

3 Institutional framework

3.1 Municipal elections

The Italian institutional context represents an ideal setting for political scientists and political economists interested in understanding the dynamics of local elections. As in many other countries, municipal elections in Italy are generally held every five years. However, since the establishment of the Italian Republic, mayoral mandates have often been interrupted prematurely for various reasons. For example, mayors may resign for personal reasons, they may pass away during their term, or the local administration may be dissolved by the central government due to historical reasons, legal or regulatory violations, severe budgetary crises, or systemic corruption.⁸ When such events occur, municipal elections are typically anticipated, except in exceptional circumstances.

As a result, municipal elections are staggered across different years and municipalities. This structure allows researchers to better exploit variation within and between municipalities across different years in order to identify effects of interest, in contrast to countries where local elections are held simultaneously every 5 years.⁹

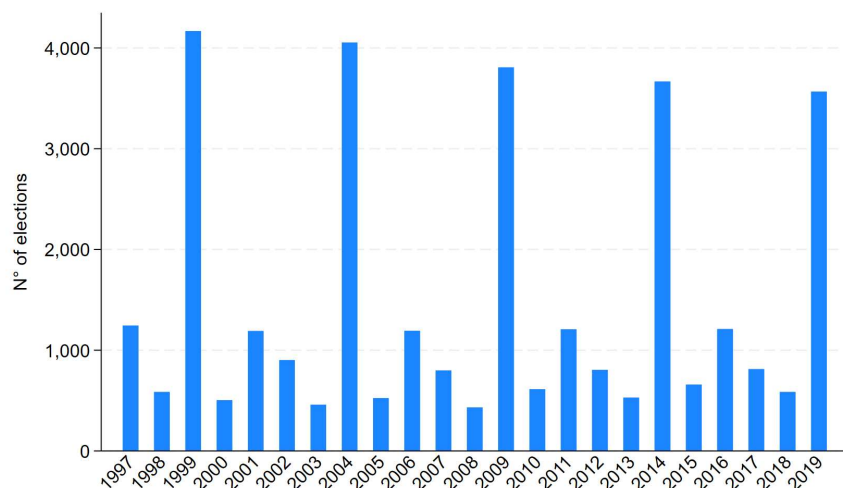
This structure is summarized in Figure 1, which shows the frequency of elections by year between 1997 and 2019. While the majority of elections occurred

⁸See Coviello and Gagliarducci (2017) for a discussion on the timing of local elections in Italy.

⁹This institutional setting is crucial for identifying political budget cycles, as it enables comparisons both between mayors at different distances from the next election and within the same administration over time, thereby isolating the effect of electoral proximity from aggregate time trends. See, for example, Alesina and Paradisi (2017) or Repetto (2018). Nonetheless, even in different studies, the staggered structure presents clear advantages in controlling for potential confounding factors related to year and location fixed effects.

at five-year intervals, numerous exceptions fill the gaps between regular election years.

Figure 1: Number of elections by year



Notes. The figure reports the frequency of elections held for each year

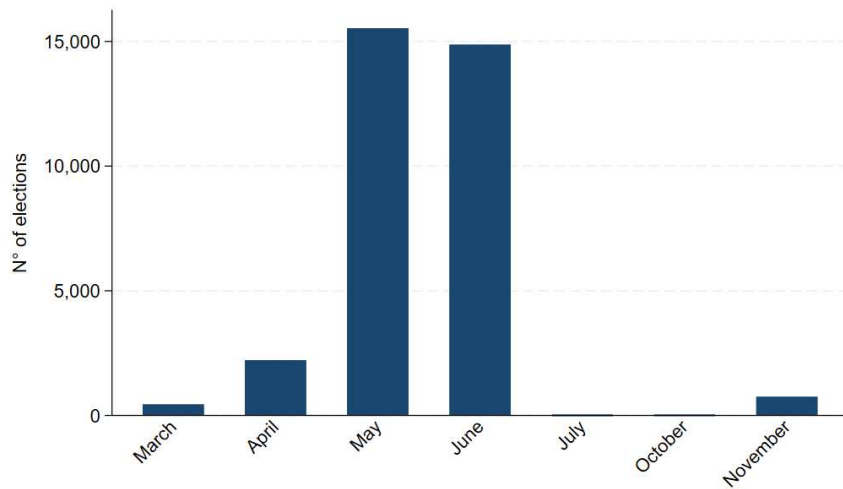
Moreover, municipal elections are typically held between April 15 and June 15 of each year, with the exact date determined by the central government in such a way that most of local elections in a given year occur on the same day.¹⁰ Aside from rare exceptions, approximately 96% of the 34,000 Italian municipal elections were held in accordance with legal provisions. This scheduling aims to prevent local electoral outcomes from influencing voters in other municipalities, as well as exploiting potential economies of scale. As a result, incumbent candidates cannot influence the election date and do not accidentally benefit from timing advantages conferred by politically aligned central governments.

This information is crucial for the identification strategy presented in Sec-

¹⁰Municipal elections scheduled within the same year may diverge from the national calendar due to the legislative autonomy of certain regions or the premature dissolution of local councils (e.g., following a vote of no confidence or state-mandated receivership), which triggers “out-of-election-day” polling dates.

tion 5. Since assignment to treatment partially depends on the election date, it is reasonable to assume its independence from individual characteristics of the mayors, thereby reducing concerns about potential biases arising from such characteristics in the main analysis. Figure 2 displays the frequency of elections by month between 1994 and 2019.

Figure 2: Number of elections by month



Notes. The figure reports the frequency of elections held for each month

3.2 Patron Saint day

In many countries with Catholic origins, a long-standing tradition has developed over centuries that involves associating a specific Saint, recognized by the Catholic Church, as the “protector” of a city, and performing rituals to honor this Saint in order to invoke protection and blessings for the local population. Countries such as Spain, Italy, and Mexico have developed strong traditions in honoring these Patron Saints, which continue to this day. In Italy in particular, the Patron Saint has become a deeply rooted feature of most cities, contributing over the

centuries to the formation of a shared cultural identity among local populations (Gencarella, 2010; Rossi, 1969).

Antonio Gramsci, one of the most important political thinkers in Italian history, recognized as early as 1929 the significance of local religious practices and religious folklore in shaping cultural and individual differences across the Italian peninsula.¹¹

The Patron Saint Day is recognized by the government as an official holiday; every Italian city has a legal right to held its celebration on the traditional date of each year. On that day, all local public offices are closed, as is the case on national holidays. Many private entities also suspend regular activities to allow workers to participate in the celebrations. Patron Saint festivities can be a simple one-day holiday or span several days and involve city-specific rituals alongside common practices (Revelli and Zotti, 2019). These celebrations can be accompanied by public events, traditional dances, fireworks, music, and unique performances. Temporary amusement rides are often installed throughout the city, alongside fairs offering typical local foods and artisanal products.

A key feature of Patron Saint holidays relevant to the analysis in this paper is that the specific date of celebration is independent of any decision made by local institutions. The date is usually tied to the corresponding day in the Catholic calendar dedicated to that Saint, to the supposed day of the Saint's death, or to a miracle attributed to the Saint in that city. In all cases, the specific dates on which local Patron Saints are celebrated were established long before Italy became a unified country, with the origins of these traditions tracing back centuries.

¹¹ *Observation on Folklore* is one of the many books written by Gramsci during his imprisonment under the Fascist regime and published posthumously. For a modern critical analysis of this book, see Gencarella (2010)

Patron Saint celebrations are organized months in advance. Typically, they are managed by local community groups, such as parishes or associations that promote local culture and tourism,¹² but local governments can participate in various ways, depending on the local tradition.

Municipal budgets may be used to support related expenditures. Local institutions are responsible for ensuring safety and security during the events,¹³ and mayors are often directly invited to participate and deliver speeches during the celebrations. In some cases, mayors are directly involved in the local organizing committee for the Patron Saint Day.¹⁴

As local elections approach, these opportunities can potentially represent a unique advantage for the incumbent over other candidates. Mayors can appear in public, engage in direct conversations with citizens, and deliver speeches to a heterogeneous audience, increasing their name recognition. They can also influence the success of the event by allocating a larger share of the municipal budget to the celebrations. All these elements can eventually work as signals of the incumbent administration's valence-whether realistic or not-which may, in turn, shift voters' preferences in favor of the mayor. In this paper, I present evidence that such efforts do, in fact, have a significant impact on the probability of re-election for incumbent candidates.

¹²"Pro-loco" associations, a typical form of local cultural organizations in Italian municipalities.

¹³The Ministry of the Interior outlines this responsibility in Ministerial Circular No. 11464 of 19 June 2017.

¹⁴For example, in the city of Legnano, the mayor is designated as the "supreme magistrate" and plays an active role in organizing the "Palio of Legnano", a horse race held during the Patron Saint celebrations.

4 Data sources

4.1 Elections

Municipal electoral data are provided by the Ministry of the Interior,¹⁵ and include information on all candidates' identities and election results from 1989 to 2023. I used personal information on candidates to identify mayors running for a second mandate, determine their age, and assess whether they were born in the municipality where they were seeking election. This dataset is complemented using the registry of local politicians, provided by the same source, which includes information on political experience and politicians education.

4.2 Patron Saint dates

The majority of information on Patron Saint dates comes from the *Database Comuni*, a free online platform that collects general information on Italian municipalities, sourced from the Italian National Institute for Statistics (ISTAT), and referring to municipalities as of 2024.¹⁶

Generally, the Patron Saint day may occur on a fixed calendar date (e.g., 15th of June or 9th of May), on a specific weekday within a given month (e.g., the third Monday of July), or be tied to a movable religious holiday (e.g., the second Sunday after Easter). I combined this information with the electoral dataset to establish the exact date of the Patron Saint day for each year considered in the

¹⁵Specifically, from one of its online platforms, called *Eligendo*: <https://elezioni.interno.gov.it/>

¹⁶The platform is managed by the private consulting company Desdinova, which provide data collection services to public and private firms. The dataset is the same used, for example, by Revelli and Zotti (2019) in its analysis of Patron Saint Celebrations. Available at: <https://www.databasecomuni.it/>

analysis.

For some municipalities, information on the Patron Saint is not available in the dataset. This may occur when two municipalities merged and have not yet designated an official Patron Saint for the newly formed entity. It also occurs in cases where the Saint is celebrated twice in a year-once on the official date from the Catholic calendar, and another on a date meaningful only to the local community.¹⁷ For all such cases, I reconstructed the Patron Saint day using a variety of supplementary sources, including official municipal websites and local news articles referring to the celebrations. However, it should be noted that this additional information is subject to potential errors, as it was not always possible to determine with certainty the exact date on which the Saint is officially celebrated.¹⁸ Consequently, I excluded these municipalities from the main analysis. Out of the 7,896 Italian municipalities in 2024, 498 were excluded from the main sample due to this anomaly, accounting for approximately 6% of the total.

4.3 Religiosity measurements

Even if the Patron Saint day can be considered a holiday that transcends its religious origins, with the population participating regardless of personal beliefs, religious practices remain an important component of the traditional celebrations, and the cult of the Saint is still present in some form. Consequently, to increase the precision of the estimates, I included a measure of local religiosity as a control

¹⁷For example, the date of a Saint miracle, or the date of his death.

¹⁸For example, official records report that Bari celebrates Saint Nicola both on the 6th of December-the official Catholic date-and on the 9th of May. However, in this specific case public events and festivities are held exclusively around the latter date.

variable.¹⁹

However, to the best of my knowledge, no standardized indicators of religiosity are available at the municipal level for Italy.²⁰ Therefore, I adopted the strategy proposed by [Oto-Peralías \(2018\)](#), which employs the toponymy of street names to construct an indicator of religious relevance for Spanish cities, showing that such a measure is strongly correlated with other religiosity indicators used in prior studies.

The underlying rationale of this approach is that, when local democratic institutions are free to choose, street names reflect the commemorative heritage of a given municipality. Hence, the proportion of street names with religious references serves as a useful proxy for the cultural importance of religion in that locality.²¹

In Italy, street names are decided by municipal governments with only minimal restrictions and require formal approval by a local representative of the central authority. As a result, elected local administrations enjoy considerable discretion in naming streets—a crucial condition for the validity of this indicator as a proxy for political or cultural signaling. While the administrative process ensures a degree of stability, the ultimate power to initiate toponymic changes remains firmly in the hands of the local executive.²²

¹⁹The focus of this measurement is on catholic religiosity, which is the one relevant for analysis related to Patron Saint celebrations, a catholic tradition. To simplify the discussion, for the reference to the paper, I used the word “religiosity” to identify the level of Catholic religious beliefs

²⁰Alternative proxies, such as census data on religious affiliation or marriage statistics, are often inadequate for measuring actual religiosity in the Italian context. Census data fail to capture the intensity of belief, while marriage rates (civil vs. religious) are increasingly influenced by secular socio-economic factors and legal incentives rather than purely religious commitment.

²¹A similar strategy has also been employed in gender studies, see [Caballero-Cordero et al. \(2025\)](#) and [Gutiérrez-Mora and Oto-Peralías \(2022\)](#).

²²It is worth noting that street names are generally considered “slow-moving” indicators.

I recovered Italian street names from the public 2025 road map sourced from ISTAT.²³ The dataset also includes the number of addresses (access points) associated with each street. I utilized this additional information to enhance the validity of the indicator. In particular, longer streets may hold greater cultural relevance than shorter ones. This suggests that they may better reflect contemporary religiosity. It should be noted that this adjustment comes with the drawback of underestimating the relevance of shorter streets in more central neighborhoods. In the main analysis, I considered the advantage of capturing the relevance of longer streets greater than the potential underestimation of shorter streets. Nevertheless, main results are robust to the inclusion of the unweighted religious street index as shown in Appendix E.

Consequently, I modified the indicator by computing the share of addresses located on streets with religious names over the total number of addresses in all streets of a given municipality. Following the methodology proposed by [Oto-Peralías \(2018\)](#), I first analyzed the 200 most frequently recurring words in Italian street names in each region.²⁴ I developed this exercise for all 20 Italian regions, to take into account of potential-more localized-terms related to vernacular languages and local dialect, scanning around 1000 different words. Within this subset, I identified terms associated with Christianity, including their abbreviations, and verified that these terms were not commonly used in non-religious contexts

Although local governments have the authority to rename streets, such changes are not implemented as a matter of routine with every new administration. Instead, they represent deliberate and significant acts of symbolic positioning that persist over time.

²³Made available from the *Archivio Nazionale dei Numeri Civici delle Strade Urbane*, the National Archive for Civic Numbers and Urban Streets, from the following link: <https://www.anncsu.gov.it/it/consultazione-dellarchivio/open-data/Accedi-ai-servizi-di-download-massivo-in-Open-data/>

²⁴Common terms such as “street”, “road” or “square” were excluded from the list for clarity.

and avoid potential overlap with synonyms.

Since the term “Papa” (the Italian word for Pope) was among the most frequent, I also included all streets named after official Popes. The complete list of selected terms, along with details on the criteria used to identify religious references, is available in Appendix [A](#). Notably, the term “Santo” (Saint in Italian), including all of its variants, emerged as by far the most common word in street names, reflecting the enduring influence of Christian traditions in Italian culture. Around 9% of all street names contains Christian references.

There are some limitations regarding the precision of this index. In some cases, street names may have remained unchanged for long periods, thereby reflecting a historical cultural heritage that does not necessarily align with contemporary individual beliefs during the period of analysis. Moreover, certain streets may bear religious names due to the symbolic relevance of a particular Saint or Pope to the local tradition of a city, rather than for broader religious significance. This may lead to an overestimation of the actual religiosity of a given municipality.

To corroborate the validity of the religiosity index based on street toponymy, I constructed two additional indexes at province level: the share of students attending religious classes in Italian elementary, middle, and high schools, and the share of catholic weddings over total weddings. Details on the construction of these indexes are available in Appendix [B](#).

The final source of evidence used to evaluate the validity of my index is the 2019 “Aspects of Daily Life” survey conducted by ISTAT, which includes a question regarding regular weekly attendance at religious services. Although this survey is only representative at the regional level and therefore cannot be used in the main analysis, it serves as a useful benchmark for validating the religious

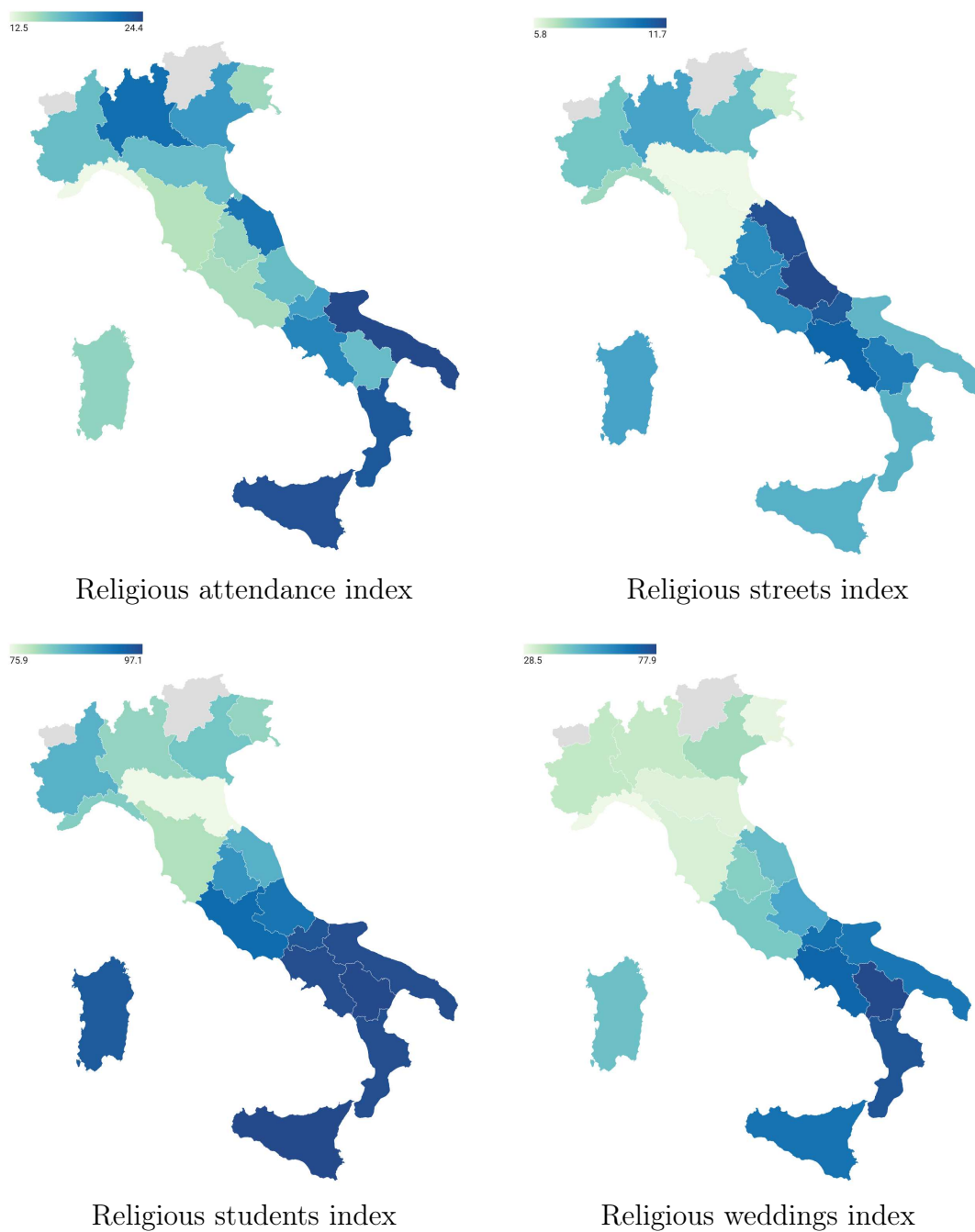
street index.

Figure 3 presents the regional distribution of religiosity according to all the measurements introduced thus far.

All indexes indicate that regions in Southern Italy are generally more religious compared to those in the North, which is consistent with expectations, given that Southern regions have more deeply rooted Catholic traditions. Religious attendance appears to be more evenly distributed across the country, whereas the other indicators highlight a clearer distinction between Northern and Southern regions. The religious streets index assigns higher levels of religiosity to central-southern regions such as Marche, Abruzzo, Molise, Lazio, and Campania. In contrast, all other indicators rank regions like Sicily, Calabria, Campania, and Apulia at the top of the religiosity distribution. Conversely, Liguria, Tuscany, and Emilia-Romagna consistently appear at the bottom.

Overall, the three indexes used in the analysis appear to be plausible when compared with survey data and among themselves, with only minor discrepancies. These are particularly evident when considering the religious wedding index in Northern regions and the religious streets index in central-southern regions. The religious students index and the religious weddings index exhibit a correlation of 83%, whereas the religious streets index correlates at 40% and 43% respectively with the other two measures (at province level). This weaker correlation may be due to the fact that the streets index reflects long-term cultural religious heritage, while the other two indexes capture religiosity at a specific point in time.

Figure 3: Regional religiosity indexes



Notes. The figure reports the density of religiosity indexes in Italian regions. The religious attendance index measures the weakly participation to religious functions at regional level. The religious streets index measures the religiosity by the percentage of catholic streets at municipal level. The religious student index measures the share of students attending catholic teaching in public school at province level. The religious wedding index measures the share of religious weddings at province level.

4.4 Rain data

Ideally, to better identify the impact of Patron Days on electoral returns, a measure of how appreciated and participated the event is from the local population would be required. Unfortunately, there is no clear data usable to construct a clear indicator of the success of a given Patron Saint celebration. Specific information on the costs incurred by each municipality is not available,²⁵ nor is it possible to precisely measure the share of the local population that participated in the event each year. Moreover, it may happen that the festivity is postponed due to external factors, such as the COVID-19 pandemic or adverse weather conditions.

To address this issue and increase the precision of the estimates, I incorporated information on daily precipitation into the main data set during the relevant period. Heavy rain could serve as a proxy controlling for effective participation, as one can safely assume that in rainy days fewer individuals are willing to participate in the festivity. Rain can reduce the enjoyment of ritualistic and celebratory aspects of the holiday, and can even cause delays or cancellation of the event. I retrieved daily rainfall data from Project Copernicus,²⁶ a European initiative aimed at harmonizing weather information across the continent starting from 1950. In its most granular resolution, the dataset divides Italy into approximately 3,500 grid cells of 10 km², with an average of 2.3 municipalities per cell.

I used geographic coordinates to match each municipality to its corresponding

²⁵Granular budgetary data regarding these celebrations are not explicitly isolated in Italian municipal accounts. Instead, these expenditures are typically aggregated under broader accounting categories—such as cultural services, public security, or urban maintenance—making it nearly impossible to disentangle the specific financial burden of the Patron Saint festivities from general administrative costs.

²⁶Available at: https://surfobs.climate.copernicus.eu/dataaccess/access_eobs_chunks.php

grid cell. Subsequently, I computed the mean precipitation in the celebration day and the 7 days following the Patron Saint day. It is important to consider some day after the Patron Saint day, since, if the celebration is delayed, it may be moved to a date away from bad weather. If heavy rain continues for repeated days, the effective date of the celebration may be moved after elections, or canceled. In both cases, the Patron Saint holiday would no longer be meaningful in terms of political outcomes. Finally, I created a dummy variable equal to 1 if mean precipitations exceed 2 mm/day, following the thresholds used in existing literature for meaningful rainfall.²⁷

4.5 Other control variables

Since social capital may play a role in explaining both individuals' responses at the polls and their participation in religious events,²⁸ I included two proxies in the dataset to control for this potential bias. Specifically, I collected voter turnout data from two Italian referenda: the divorce referendum (1974) and the abortion referendum (1981).²⁹

As a mean of testing random assignment in a balance test and to further increase the precision of the estimated effect, I also incorporated general information on the local population, including the old-age dependency ratio and average education levels, using census data from 1991, 2001, 2011, and 2021, as well as geographical characteristics such as altitude, surface area, and proximity to the

²⁷See, for example, Drago et al. (2025) and Madestam et al. (2013).

²⁸Higher social capital may indicate that the population is more willing to participate to a community-base, traditional local events. On the other hand, higher social capital can be associated with a more secularized population. In celebrations where the religious aspect of the festivity prevails, social capital can be correlated with lower participation.

²⁹This approach has been widely applied in the modern literature. See, for example, Crescenzi et al. (2015).

seashore.³⁰

Finally, I included a dummy variable equals to one when the precipitation is above 2mm on the day of election, to avoid that the rainfall dummy on Patron Saint day affects voting behavior through channels other than the quality of PSD events.

5 Identification Strategy

The main sample includes all municipal elections held between 1994 and 2019 in which an incumbent mayor decided to run for another term.³¹

The main identification strategy employed in this paper exploits the quasi-random timing of local elections and Patron Saint days to determine treatment assignment. Local election dates are determined by the central government, typically constrained within the interval between April 15th and June 15th, and generally occur simultaneously across the country. Consequently, there appears to be limited scope for the central government to strategically align election dates to favor specific local administrations. Furthermore, given this centralized and synchronized framework, it is highly unlikely that incumbent mayors can exert any meaningful influence over the timing of their own re-election bids.

In contrast, the Patron Saint Day represents a long-standing tradition. Its specific date was established in the past—often centuries ago—based either on the universal Catholic liturgical calendar or on particular miracles and historical events tied to the municipality. (Revelli and Zotti, 2019). Thus, from the perspective

³⁰All these information are sourced from ISTAT, available at: <https://esploradati.istat.it/databrowser/#/en>

³¹Years after 2019 have been excluded, as all public celebrations were banned during the COVID-19 pandemic.

of an incumbent mayor, the date is fully exogenous. As a result, the distance between local elections and Patron Saint days can be considered as-good-as randomly assigned, allowing for the identification of the causal effect of Patron Saint celebrations on electoral outcomes.

Assignment to treatment is defined as having the Patron Saint day occur within a short window prior to the election. In the main specification of the model, the treatment group includes all municipalities in which the Patron Saint day falls between 0 and 10 days before the local election. I also included dummy variables for intervals between 11 to 20 days and 21 to 30 days prior to the election in other specifications, to investigate the distance at which the effect remains relevant. All municipalities in which the Patron Saint day falls after the election or more than 30 days before the election are assigned to the control group.³²

I defined the 10 days interval for the treatment in the main analysis following a specification of the model in which I included the difference in days between Patron Saint day and the date of elections as a continuous variable. Since the treatment is expected to be relevant only for the days leading to the election and not for days after the election, I measured the difference in days in absolute values and included a dummy accounting for the Patron Saint days happening before elections (when the distance is negative). Thus, the model includes a triple interaction, considering also the rainfall dummy, in the following form:

³²In Appendix F, I also tested larger treatment windows of 15 and 20 days and showed that the effect is greater in proximity to the election date.

$$\begin{aligned}
Y_{i,t} = & \alpha_0 + \alpha_1 Before_{i,t} + \alpha_2 Days_{i,t} + \alpha_3 Rain_{i,t} + \alpha_4 Before_{i,t} * Days_{i,t} + \alpha_5 Before_{i,t} * Rain_{i,t} \\
& + \alpha_6 Days_{i,t} * Rain_{i,t} + \alpha_7 Before_{i,t} * Days_{i,t} * Rain_{i,t} + \alpha_8 Rel_i + \alpha_9 V_{i,t-1} + C_{i,t} + Y_t P_j + \epsilon_{i,t}
\end{aligned}
\tag{1}$$

where $Y_{i,t}$ is a dummy variable equal to one if the incumbent mayor is re-elected in municipality i in year t ; $Before_{i,t}$ is a dummy equal to one when the Patron Saint day happened before elections, $Days_{i,t}$ is the absolute value of the difference in days between Patron Saint and the date of elections; $Rain_{i,t}$ is a dummy equal to 1 if rainfall exceeds 2 mm/day on average on the 7 days after Patron Saint day; Rel_i controls for the degree of religiosity at municipal level using the street index; $V_{i,t-1}$ represents the percentage of votes received by the incumbent in the previous election, used as a proxy for political strength; $C_{i,t}$ is a matrix of municipal and individual covariates, listed in the balance test in Table [1](#); and $Y_t P_j$ includes all interaction terms between year t and province j fixed effects; standard errors are clustered at province level. To take into account potential non-linearity of the effect for different days, I also tested additional specification of this model with transformation of the difference in days using two functions: (i) the natural logarithm of the absolute value plus one, and (ii) the inverse hyperbolic sine (IHS) of the nominal value, to account for negative and zero values in difference in days without applying the absolute value.^{[33](#)}

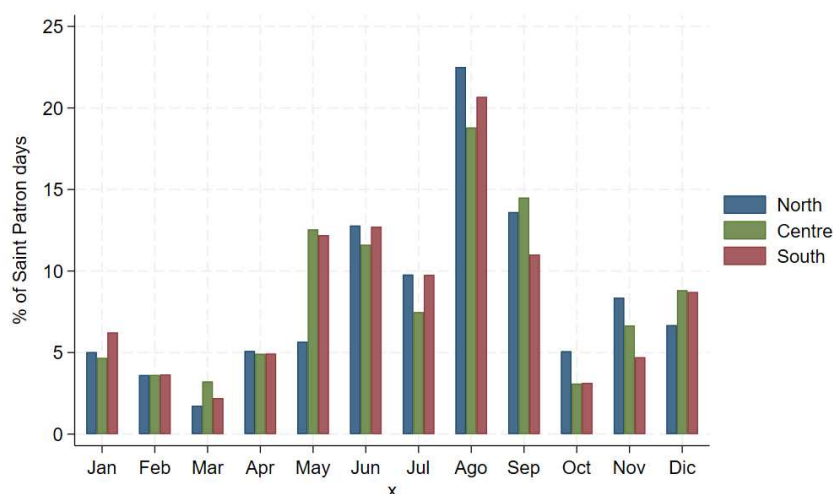
It is important to note that the date of each Patron Saint day should be interpreted as identifying an "intention to treat". It is not possible to determine

³³See, for example, [Norton \(2022\)](#) and [Bellemare et al. \(2013\)](#) for an application of the IHS in empirical research.

with certainty when or which part of a certain Patron Saint celebration has an impact on electoral outcomes, as it could potentially spans over several days. It could be the celebration in its entirety, it could be a specific more important day in all the festivity, or it could be the day a mayor has the opportunity to give a public speech.

One important caveat in the randomization process must be considered. While treatment assignment is independent of local government characteristics, it may still carry biases related to location fixed effects. This concern is illustrated in Figure 4, which plots the distribution of Patron Saint days by month and standard Italian macro-areas (North, Center, and South).

Figure 4: % of Patron Saint days by month and macroarea



Notes. The figure reports percentage of Patron Saint Days for each month by three macro-area of the Italian peninsula.

Most Patron Saint days are concentrated in the summer months, immediately following the expected interval designated for local elections. As a result, the number of treated municipalities is relatively limited compared to the control

group.³⁴

Moreover, the monthly distribution of festivities reveals that Northern Italy is significantly underrepresented during May. Since local elections are concentrated in the April-June interval, this seasonal pattern results in a lower share of Northern municipalities within the treatment group. This is confirmed by column (1) in Table 1.

In Table 1, I tested for mean differences in various control variables between the 10-day window treatment group and the control group, using a simple OLS regression with the treatment dummy as independent variable. In the first column, I reported all individual outcomes for which this test has been conducted. In principle, if the randomization is effective, there should be no statistically significant differences between the two groups. However, in the basic regressions shown in column (1), some differences emerge, particularly with respect to macro-areas.

Two strategies have been implemented to address these concerns. First, I restrict the main sample to a window of 80 days centered around the election date.³⁵ This approach restricts the control group to a narrower temporal window, thereby enhancing the comparability between treated and untreated units and mitigating potential unobserved confounding factors, as municipalities closer to the election date are expected to share more similar characteristics.³⁶

³⁴Approximately 7% of all municipalities have the Patron Saint day occurring within 30 days before elections, with only 2% falling within the 10-day window.

³⁵The 80-day window is computed using the optimal bandwidth approach described in Calonico et al. (2020), including province*year interactions as additional covariates. Results are robust to alternative bandwidth choices, as shown in Appendix G. See Appendix D for a discussion of RDD implementation in this context.

³⁶If unobserved heterogeneity threatens the exogeneity of treatment assignment, it is likely driven by seasonal patterns. For instance, municipalities in Northern Italy may historically prefer to celebrate their Patron Saint during the summer to benefit from more favorable weather, rather than strictly adhering to the liturgical calendar. In such cases, focusing on a narrow time window ensures that small differences in the timing of the festivity—just before or just after the

Table 1: Balance test

Variables	(1)	(2)	(3)	(4)
<i>Outcome variable</i>				
Re-election	0.013	0.018	0.043*	0.073**
<i>Mayor's characteristics</i>				
Female	-0.017	-0.022	0.006	0.002
Age	-0.860	-0.998*	-0.628	-0.330
Bachelor degree	0.115***	0.100***	0.063**	0.050
Years of experience	0.043	-0.129	0.197	0.155
Born locally	0.067**	0.066**	0.002	0.009
<i>Municipal characteristics</i>				
Religiosity (streets)	0.472	0.216	-0.041	-0.278
Religiosity (students)	3.397***	3.718***		
Religiosity (weddings)	6.661***	5.967***		
Pop. below 1000	-0.051*	-0.030	0.012	0.046*
Pop. btw. 1000-3000	-0.015	-0.013	-0.048*	-0.058*
Pop. btw. 3000-5000	0.001	-0.014	-0.011	-0.031
Pop. btw. 5000-15000	0.035	0.025	0.024	0.034
Pop. btw. 15000-50000	0.025	0.025	0.020	0.006
Pop. above 50000	0.005	0.006	0.003	0.004
% female inhabitants	0.000	0.000	-0.001	-0.002
% graduated inhabitants	0.005**	0.004*	0.004**	0.004*
% pop. above 64 y.o.	0.001	0.001	0.003	0.006
Surface	13.521***	12.385***	5.285*	6.055*
Altitude	25.216	19.801	-0.432	14.017
Proximity to seashore	0.026	0.014	0.000	-0.022
% votes prev. election	-0.011	-0.013	0.005	0.003
Turnout abortion ref.	-4.021***	-3.671***	-0.146	-0.423
Turnout divorce ref.	-2.670***	-2.501***	-0.034	-0.194
North	-0.269***	-0.237***		
Center	0.101***	0.092***		
South	0.168***	0.145***		
Observations	15,149	3,260	15,149	3,260
Interaction Prov*Year	NO	NO	YES	YES
Restricted sample	NO	YES	NO	YES

Notes. The table reports estimates of the difference in means for various control variables between the control group and the 10-day window treatment group, when the Patron Saint Day happened from 0 to 10 day before elections. Column (1) presents the baseline test without sample restrictions; column (2) restricts the sample to observations within 80 days of the election date. Columns (3) and (4) report the same specifications including province×year interaction terms. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Second, while the inclusion of municipality fixed effects would ideally strengthen the analysis by absorbing all time-invariant unobserved heterogeneity, this approach is not feasible in the current setting. Consequently, we account for election-remain as-good-as-random, thereby preventing confounding bias in the estimated effect.

location-related differences by including province-by-year interaction terms in the main specification. The reliance on this alternative strategy is dictated by the specific structure of the data: since the sample is restricted to incumbent mayors seeking re-election, municipalities are observed, on average, only 1.7 times. Furthermore, because treatment assignment is determined by fixed calendar dates (the Patron Saint day and the election window), the treatment exhibits very limited within-municipality variation over time. Specifically, only 5% of municipalities switch treatment status during the observation period. Implementing municipality fixed effects would therefore force the model to rely exclusively on the variation provided by this small subset of switchers, leading to highly sensitive estimates and drastically inflated standard errors. Column (4) of Table [1](#) shows that, after restricting the sample and including province*year interaction terms, no statistically significant differences remain across additional covariates, both at individual and municipal levels. Some control variables, namely the two population groups below 3,000 inhabitants, the share of graduated inhabitants and the municipal surface, exhibit a 10% significance in means' difference, but all those differences are relatively small compared to the sample average. All these covariates have been included in the main model as control variables, with the exception of voters turnout on divorce referendum, redundant with respect to the turnout on abortion referendum, and the religiosity measurements related to students taking IRC classes and to weddings, which are only available at province level.

One further test is required to support a causal interpretation of the effect of Patron Saint day on electoral outcomes. Since the main sample is restricted to incumbent mayors running for re-election, I test whether treatment status

correlates with the probability of an incumbent seeking re-election. The results, presented in Table [C.1](#) in Appendix [C](#), show no evidence of such correlation.

The final analysis is conducted using a Linear Probability Model estimated via standard OLS according to the following equation:

$$Y_{i,t} = \beta_0 + \beta_1 T_{i,t} + \beta_2 Rainfall_{i,t} + \beta_3 Rainfall_{i,t} * T_{i,t} + \beta_4 Rel_i + \beta_5 V_{i,t-1} + C_{i,t} + Y_t P_j + \epsilon_{i,t} \quad (2)$$

where $Y_{i,t}$ is a dummy variable equal to 1 if the incumbent mayor is re-elected in municipality i in year t ; $T_{i,t}$ is a treatment dummy equal to 1 if the Patron Saint day occurs within 0 to 10 days before the election, and all other variables are defined as in Equation (1).

6 Results

6.1 Probability of re-election

This section presents the empirical findings regarding the impact of Patron Saint festivities on electoral outcomes, starting with the baseline OLS estimates. Table [2](#) reports the results from a regression based on Equation (1), where the treatment is measured by a dummy for Patron Saint days occurring before the elections and the temporal distance in days.

Specifically, *Distance in days* is defined as the absolute difference between the date of the Patron Saint day and the election date in Columns (1) and (2); as the log-transformation of this absolute value plus one in Column (3); and as the inverse hyperbolic sine transformation in Column (4). In Column (1), I

exclude all controls except for the *Rainfall* dummy and its interactions, as well as all province-year fixed effects. Columns (2), (3), and (4) include the full set of control variables described in the previous section.

Focusing on Column (2), the coefficients align with the main hypotheses. The dummy for Patron Saint days occurring before elections—which represents the peak effect when the distance is zero and there is no rain³⁷—is approximately 5 percentage points. The coefficient for the distance in days alone, when both *Rainfall* and *Before* dummies are zero, captures the effect of festivities occurring after the elections. This estimate is near zero and statistically insignificant, suggesting that celebrations following the vote do not influence electoral results. This finding further implies that organizational activities leading up to the festivity have no meaningful impact on the incumbent’s performance.

Conversely, the interaction term between the *Before* dummy and the distance in days is negative and statistically significant: the further the Patron Saint day precedes the election, the smaller the impact on electoral outcomes. The precision of these estimates is further supported by the interaction terms with the *Rainfall* dummy. While rain is irrelevant for festivities occurring after elections, its impact is significant when it occurs beforehand. Specifically, when heavy rain occurs before the elections, the coefficient is statistically significant and carries the opposite sign of the *Before* indicators, with a magnitude similar enough that confidence intervals largely overlap. These results confirm that when celebrations are disrupted or canceled due to inclement weather, the electoral return for the incumbent vanishes. Finally, the results remain robust across the parsimonious

³⁷That is, the effect when the Patron Saint day coincides with the election day under favorable weather. 92 observations falls under this category.

specification in Column (1) and the non-linear transformations in Columns (3) and (4).

Table 2: Probability of re-election with continuous days treatment

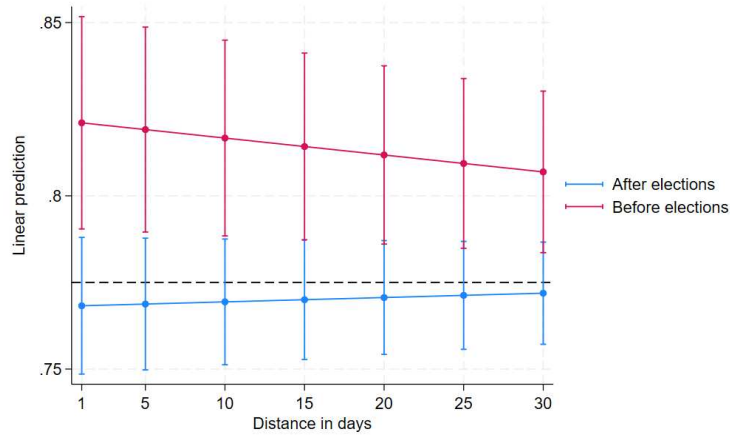
Variables	(1)	(2) =1 if re-elected	(3)	(4)
Before	0.043** (0.018)	0.053*** (0.018)	0.140*** (0.045)	0.152*** (0.049)
Distance in days	0.000 (0.000)	0.000 (0.000)	0.007 (0.007)	0.007 (0.007)
Before $\times$ Distance	-0.001*** (0.000)	-0.001*** (0.000)	-0.034*** (0.010)	0.017* (0.010)
Rainfall	0.022 (0.017)	0.013 (0.018)	0.022 (0.053)	0.025 (0.058)
Before $\times$ Rainfall	-0.076*** (0.028)	-0.078*** (0.029)	-0.175*** (0.078)	-0.187** (0.085)
Rainfall $\times$ Distance	-0.000 (0.000)	-0.000 (0.000)	-0.005 (0.012)	-0.005 (0.011)
Before $\times$ Rainfall $\times$ Distance	0.001*** (0.000)	0.001*** (0.000)	0.044** (0.018)	-0.031** (0.015)
Observations	16,370	15,149	15,149	15,149
Interaction Prov*Year	YES	YES	YES	YES
Population groups	NO	YES	YES	YES
Municipality controls	NO	YES	YES	YES
Mayor's controls	NO	YES	YES	YES
Religiosity control	NO	YES	YES	YES
Social capital control	NO	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Function	Linear	Linear	Log	IHS
Joint P-value	0.020	0.009	0.021	0.028

Notes. The table reports OLS estimates for the probability of re-election using a continuous treatment variable for days. Column (1) includes only the Rainfall dummy, its interactions, and province $\times$ year fixed effects. Columns (2), (3), and (4) add the full set of controls. The functional form of the distance variable varies across columns: absolute value in column (2), logarithmic transformation ($\log(1+x)$) in column (3), and inverse hyperbolic sine (IHS) transformation in column (4). Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

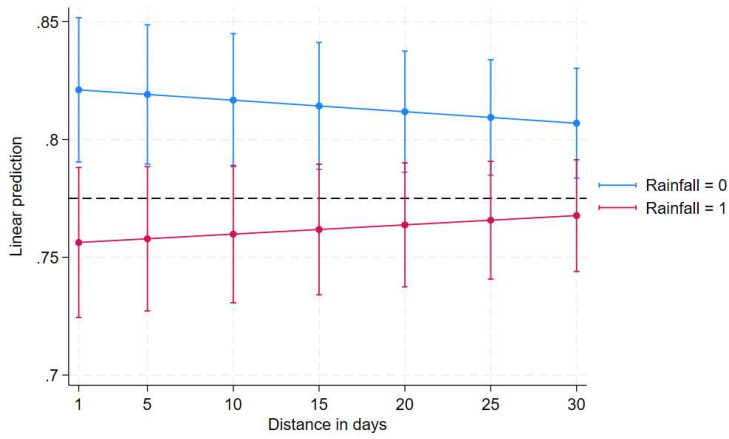
Figure 5 plots the linear predicted probabilities of re-election based on the model specification reported in Column (2) of Table 2. The left panel displays the estimated probabilities for Patron Saint days occurring at varying distances from the election date (e.g., at 5, 10, or 20 days), explicitly comparing the levels for festivities held before versus those held after the vote (assuming no rainfall). The right panel focuses exclusively on the pre-election period, contrasting the

predicted re-election probabilities in the presence of rainfall versus the absence of rainfall across the same time horizon. The dotted line represent the average probability of re-election for incumbent mayors, around 77.5%. In both cases, when the Patron Saint day happened before election, the probability of re-election is consistently higher than average even 30 days before elections happened. When the Patron Saint day celebration took place after the electoral date, or when the mean precipitation is above 2 mm/day in the 7 days after celebrations, the effect disappears. These two groups confidence intervals start to overlap with the treatment group around 10 days in terms of absolute distance, which serves as justification to expand the analysis to the model described in Equation (2). This specification, alongside additional robustness checks, allows to better understand the distance from which the treatment effect is relevant.

Figure 5: Estimated probability of re-election



Patron Saint before and after election



7 days mean rainfall above 2mm/day

Notes. The figure plots the predicted linear probability of re-election of the incumbent. The left panel shows the estimated probability at various days before or after the election in absence of heavy rainfall. The figure on the right shows the probability at various days before the elections with and without heavy rainfall

Table 3 reports the results of the regression estimating the probability of re-election of incumbent mayors on treatment dummies defined for the 10-day window, as outlined in the previous section. The main specification is presented

in columns (1) and (4), using the full sample and the sample restricted to 80 days around the election date, respectively. Estimates in Column (2) and (5) include also a dummy for the 11-to-20 days window. Column (3) and (6) add another dummy for the 21-to-30 days windows. In the main model in Column (1) and (4), municipalities identified by those dummies are assigned to the control group.

Across all model specifications and sample definitions, the coefficient on the 0-to-10 day treatment dummy is positive and statistically significant. The estimated effect measures around 9 percentage points. Given that the average probability of re-election for an incumbent mayor is approximately 77%, the average treatment effect corresponds to an increase of roughly 12% in the likelihood of re-election when there are low mean precipitations around the Patron Saint day. This suggests that the celebration of the Patron Saint day shortly before the election provides a substantial opportunity for incumbent mayors to boost their electoral prospects.

In contrast, the estimated coefficients for treatment dummies corresponding to earlier time windows (11 to 20 and 21 to 30 days before the election) are close to zero and not statistically significant. This indicates that the treatment effect is concentrated in the immediate proximity of the election date and dissipates when the Patron Saint day occurs too far in advance.

Table 3: Probability of re-election with 10-day treatment dummies

Variables	(1)	(2)	(3)	(4)	(5)	(6)
			=1 if re-elected			
0-10 Days	0.086*** (0.033)	0.086*** (0.033)	0.087*** (0.033)	0.118*** (0.040)	0.116*** (0.041)	0.117*** (0.041)
Rainfall	0.003 (0.008)	0.004 (0.008)	0.006 (0.008)	-0.009 (0.021)	-0.020 (0.025)	-0.018 (0.027)
0-10 Days $\times$ Rainfall	-0.086* (0.046)	-0.087* (0.047)	-0.090* (0.047)	-0.089 (0.054)	-0.077 (0.057)	-0.079 (0.058)
11-20 Days		0.014 (0.025)	0.015 (0.025)		-0.015 (0.031)	-0.014 (0.032)
21-30 Days			0.031 (0.034)			0.003 (0.034)
Observations	15,149	15,149	15,149	3,260	3,260	3,260
Interaction Prov*Year	YES	YES	YES	YES	YES	YES
Population groups	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES	YES	YES
Restricted sample	NO	NO	NO	YES	YES	YES

Notes. The table reports OLS estimates for the linear probability of re-election using 10-day window dummies. Columns (1), (2), and (3) include all available control variables for the full sample. Columns (4), (5), and (6) report estimates for the restricted sample, which includes only elections held within an 80-day window. When the 11-20 days and 21-30 days dummies are not explicitly included in the regression, those periods are absorbed into the control group. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To further validate these findings, I perform a series of robustness checks, starting with the sensitivity of the results to the definition of the treatment window. Tables [F.1](#) and [F.2](#) in Appendix [F](#) present the estimates using 15-day and 20-day treatment dummies, respectively. While the coefficients remain positive and statistically significant, their magnitude gradually decreases as the window expands, consistent with the decay of the effect over time. Crucially, the results are not driven by the specific threshold used to assign municipalities to the control group.

Second, I verify that the results are not contingent on the sample selection. As reported in Table [G.1](#) (Appendix [G](#)), the estimates remain robust when employing broader definitions of the restricted sample.

Finally, I test the sensitivity of the model to functional form and inference specifications. The main findings are confirmed when using Probit and Logit models (Appendix [H](#)) and remain consistent across different clustering levels for the standard errors (Appendix [I](#)).

6.2 Heterogeneity in religiosity

In Table [4](#), I report the estimates for sub-samples below and above the median of religiosity as measured by the religious street index, as well as a model specification that includes interaction term between the measure of religiosity and the treatment dummy. In all reported cases, the average treatment effect is jointly significant, and the coefficients on the treatment dummy are consistent with those in the main model. Notably, the effect does not seem to differ when accounting for different level of religiosity, both in sample-split and looking at interaction terms.

When looking at the specification of the model of Equation (2) in Table [J.1](#) in Appendix [J](#), with continuous days treatment variable, the result appear similar, with the exception of the sample-split analysis. In this case, the effect disappear for low level of religiosity and it is higher than baseline when focusing on more religious municipalities.

Table 4: Heterogeneity in religiosity with 10-day treatment dummy

Variables	(1) <i>Rel. (streets) = 0</i>	(2) <i>Rel. (streets) = 1</i> =1 if re-elected	(3)	(4)
0-10 Days	0.113* (0.058)	0.075* (0.042)	0.077 (0.048)	0.141** (0.059)
Religiosity			-0.000 (0.001)	0.001 (0.001)
0-10 Days $\times$ Religiosity			0.001 (0.003)	-0.002 (0.003)
Rainfall	0.009 (0.012)	0.001 (0.010)	0.003 (0.008)	-0.009 (0.021)
0-10 Days $\times$ Rainfall	-0.131** (0.066)	-0.072 (0.056)	-0.087* (0.046)	-0.087 (0.054)
Observations	7,261	7,382	15,149	3,260
Interaction Prov*Year	YES	YES	YES	YES
Population groups	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES
Religiosity control	NO	NO	YES	YES
Social capital control	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Restricted sample	NO	NO	NO	YES
Joint P-value			0.008	0.015

Notes. The table reports OLS estimates for the linear probability of re-election using a 10-day treatment dummy. Columns (1) and (2) present estimates for the subsamples below and above the median of the religiosity (streets) index, respectively. Columns (3) and (4) include interaction terms between the treatment variable and the religious streets index. Column (4) restricts the sample to elections held within an 80-day window. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Considering all the results, two possibilities emerge. On one hand, it may be the case that different levels of religiosity do not impact the overall treatment effect. On the other hand, the sample-split analysis with continuous treatment suggests that the effect is concentrated in municipalities with higher level of religiosity. Despite the first case seems more likely, all in all evidences reported are not sufficient to give a definitive answer.

6.3 Heterogeneity in gender

We further explore the heterogeneity of the Patron Saint Day effect by conditioning on the mayor's gender. Table 5 presents estimates from both subsample

analyses (male vs. female) and a fully interacted model that includes an interaction term between the treatment and a binary indicator for female mayors. In the interacted specification, the estimated effect for female mayors is approximately double that observed for their male counterparts. The coefficients for the 0-10 days window and the rainfall interaction term remain consistent with the patterns observed in the main model. However, the coefficient on the female dummy reveals a negative baseline association with re-election probability, indicating that female incumbents face a lower likelihood of securing a second term.³⁸ The results from the interaction term and sample split analysis suggest that the occurrence of the Patron Saint festival near the election allows female mayors not only to offset this baseline disadvantage but also to leverage the event more effectively, generating higher electoral returns. These findings are robust to using the alternative specification defined in Equation 1, as reported in Table J.2 in the Appendix.

One potential explanation is that female mayors may be more effective at leveraging this opportunity due to higher average political quality. Selection bias stemming from gender discrimination suggests that women who enter the electoral race often face higher entry barriers, meaning that those who do run tend to be among the most qualified and ambitious candidates. This selective process can lead to a higher average quality among elected female mayors compared to their male counterparts (Baltrunaite et al., 2014; Anzia and Berry, 2011). Consequently, female mayors may possess superior political skills or have stronger policy achievements to showcase during the Patron Saint festivities.

³⁸This finding is consistent with the existing literature. See, for example, Brollo and Troiano (2016).

Table 5: Heterogeneity in gender with 10-day treatment dummy

	(1)	(2)	(3)	(4)
	<i>Male mayors</i>	<i>Female mayors</i> =1 if re-elected		
Variables				
0-10 Days	0.087** (0.035)	0.182** (0.089)	0.074** (0.033)	0.103** (0.039)
Female			-0.070*** (0.011)	-0.114*** (0.028)
0-10 Days $\times$ Female			0.148** (0.066)	0.194** (0.087)
Rainfall	0.001 (0.008)	0.003 (0.026)	0.003 (0.008)	-0.010 (0.021)
0-10 Days $\times$ Rainfall	-0.109** (0.052)	-0.179 (0.139)	-0.088* (0.047)	-0.092* (0.055)
Observations	13,615	1,130	15,149	3,260
Interaction Prov*Year	YES	YES	YES	YES
Population groups	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES
Religiosity control	NO	NO	YES	YES
Social capital control	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Restricted sample	NO	NO	NO	YES
Joint P-value			0.008	0.006

Notes. The table reports OLS estimates for the linear probability of re-election using a 10-day treatment dummy. Columns (1) and (2) present estimates for the subsamples of male and female mayors, respectively. Columns (3) and (4) include interaction terms between the treatment variable and the female gender dummy. Column (4) restricts the sample to elections held within an 80-day window. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.4 Heterogeneity in other characteristics

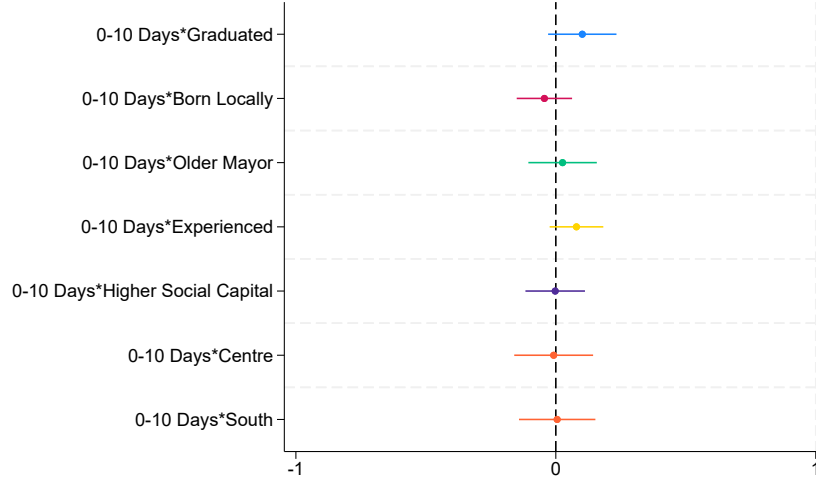
Finally, we extend our heterogeneity analysis to explore whether the electoral return of the Patron Saint Day varies according to other observable characteristics of the mayor (level of education, political experience, local roots) or the municipality (level of social capital and geographical location). Figure 6 reports the coefficients of the interaction terms between the treatment window (0-10 days) and these variables.

Overall, the analysis reveals no other noteworthy sources of heterogeneity. The interaction terms related to the mayor's characteristics-such as holding a univer-

sity degree, being born in the municipality, or having previous administrative experience-do not display a statistically significant differential effect compared to the baseline. Similarly, the level of social capital does not appear to significantly moderate the impact of the festivities.

Of particular importance for our identification strategy is the absence of differential effects across geographical macro-areas. As shown in Figure 6, the estimated coefficients for the interaction terms with Central and Southern Italy (using Northern Italy as the reference category) are statistically indistinguishable from zero. This finding is particularly reassuring given the geographical imbalance discussed in the descriptive statistics, where the treated sample is characterized by a lower prevalence of municipalities from Northern Italy compared to the control group. The evidence that the effect of the Patron Saint Day is homogeneous across macro-regions suggests that our main results are not driven by unobserved time-invariant regional confounders or by the specific geographical composition of the treated sample.

Figure 6: Heterogeneity Analysis



Notes. The figure reports the coefficient of the interaction term between the 0-10 Days treatment dummy and each variable of interest. Graduated is a dummy variable equal to one if the mayor holds a bachelor's degree or higher, and zero otherwise. Born Locally is a dummy variable equal to one if the mayor is born in the city where he run for re-election, and zero otherwise. Older Mayor is a dummy variable equal to one when mayors are above median in the age national distribution, and zero otherwise. Experienced is a dummy variable equal to one when mayors are above median of the years of experience national distribution, and zero otherwise. Higher Social Capital is a dummy variable equal to one when municipalities are above median of the divorce referendum turnout national distribution, and zero otherwise. Centre is a dummy variable equal to one if the municipality is located in the Central Italy macro-area, and zero otherwise. South is a dummy variable equal to one if the municipality is located in the Southern Italy macro-area, and zero otherwise.

6.5 Voters turnout

Previous similar analyses have argued that community activities, such as traditional festivities like the Patron Saint day, can enhance the sense of community and provide a temporary boost to social capital, potentially leading to an increase in voter turnout on election day (Revelli and Zotti (2019)). On the other hand, the costs associated with participating and organizing such celebrations may reduce voters' willingness to go to the polls (Atkinson and Fowler (2014)).

Therefore, I tested whether Patron Saint day celebrations held in close proximity to elections lead to higher or lower voter turnout.

Table 6: Effect on voter turnout

Variables	(1)	(2)	(3)	(4)
	<i>Second-term elections</i>	<i>Second-term elections</i>	<i>All elections</i>	<i>All elections</i>
		Voter turnout		
0-10 Days	1.508** (0.684)	1.898** (0.862)	1.198* (0.626)	1.017 (0.645)
Rainfall	0.154 (0.128)	0.028 (0.261)	0.190** (0.094)	0.193 (0.201)
0-10 Days $\times$ Rainfall	-1.998** (0.986)	-2.541** (1.272)	-1.401** (0.671)	-1.328* (0.775)
Observations	15,149	3,260	30,039	6,722
Interaction Prov*Year	YES	YES	YES	YES
Population groups	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Restricted sample	NO	YES	NO	YES

Notes. The table reports OLS estimates for the treatment effect on voter turnout in percentage points. Columns (1) and (2) focus on the main sample of analysis, with incumbent mayors running for a second term. Columns (3) and (4) include all elections. Columns (2) and (4) restrict the sample to elections held within an 80-day window. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Results are reported in Table 6. We observe a positive and statistically significant effect of the festivities on electoral participation, particularly when focusing on the main sample of incumbent mayors running for a second term (Columns 1 and 2). In this subsample, the estimated coefficient is larger in magnitude (ranging between 1.5 and 1.9 percentage points) and statistically significant at the 5% level. Conversely, when considering the pool of all elections (Columns 3 and 4), the effect becomes smaller and marginally significant or insignificant. It is worth noting that while this effect is statistically distinguishable from zero, its magnitude remains relatively limited given the high average turnout rate of approximately 77% observed in our sample.

These findings suggest that the potential negative effect of participation costs (Atkinson and Fowler (2014)) is marginally offset by the mobilization efforts of the incumbent. The discrepancy between the two samples indicates that the boost in turnout is likely driven by the incumbent’s strategic incentive to ensure a more prominent and successful event when seeking re-election. While the festivities are traditionally managed by local committees or ecclesiastical authorities, incumbent mayors can influence the scale of the event by providing additional administrative support, public funding, or logistical coordination. By facilitating a more impactful celebration, incumbents may effectively leverage the festive atmosphere to maximize the social capital channel described by (Revelli and Zotti (2019)). This generates a small but significant increase in voter turnout—an effect that appears less prevalent in open races where the incumbent is not necessarily on the ballot and thus has weaker incentives to mobilize local resources for the event.

6.6 Mechanism

There are three possible channels that may explain the increased probability of re-election for incumbent mayors.

First, evidence from the literature suggests that this type of religious activity and community event generates a short-term boost in individuals’ self-perceived happiness (Campante and Yanagizawa-Drott (2015); Deaton and Stone (2013); Dolan et al. (2008)). (Ward et al. (2021)) argue that subjective well-being can be a strong determinant of electoral outcomes, comparable in importance to macroeconomic indicators. Unhappy voters tend to shift their support from incumbents

to opposition candidates and vice versa. If the Patron Saint celebrations generate a short-term boost in happiness, some voters may decide to support the incumbent. Unfortunately, to the best of my knowledge, it is not possible to test this hypothesis directly. Such an analysis would require individual-level data on self-reported well-being at the municipal level, with some surveys collected randomly immediately after the Patron Saint celebrations.

Second, Patron Saint celebrations may offer an additional opportunity for incumbent candidates, relative to their opponents, to promote their political agenda and emphasize the achievements of their first term. Mayors could be potentially invited to deliver public speeches during Patron Saint celebrations and actively participate in traditional events, effectively extending their electoral campaigns through highly visible appearances at gatherings attended by diverse segments of the local population, regardless of political affiliation.

Third, the success of the Patron Saint celebration may serve as a signal of the incumbent's valence. Incumbent mayors can allocate a larger share of public resources to celebrations held close to the election date, thereby increasing the likelihood of organizing a successful and satisfying event. In a setting where communities are exposed to social pressure (Gerber et al. (2008)) and communal values are emphasized over individual preferences (Revelli and Zotti (2019); Prete and Revelli (2017)), such events can be interpreted as a sign of competence or dedication on the part of the incumbent. This combination of a shift in individual preferences and the signaling of valence through successful celebrations may contribute to the observed increase in re-election probability associated with Patron Saint days. Unfortunately, municipal budgets are not sufficiently detailed to isolate expenditures specifically related to the Patron Saint day. Another possible

avenue is the use of data from the Italian Information System on Operations of Public Entities (SIOPE), which records payments in more specific details and at monthly level for municipalities between 2010 and 2023. However, there is no invoice in the SIOPE codification that specifically identifies expenditures related to Patron Saint day celebrations.³⁹ Expenditures for this type of events are divided between different invoices, and payments could potentially be sustained by each municipality during different months after the Patron Saint day.

Moreover, even with data available for specific expenditures, in many cases the organization of the Patron Saint celebrations are managed by local non-profit organization or local parishes, which means that the incumbent mayor can participate economically only with transfers to these groups, which do not necessarily cover the entire cost.⁴⁰ Further research is required in this direction.

7 Concluding remarks

This paper investigates a novel channel through which incumbents may gain an electoral advantage: the strategic proximity of major local celebrations to the election date. Exploiting the quasi-random variation in the timing of Patron Saint Days relative to municipal elections in Italy between 1994 and 2019, I provide evidence that incumbents benefit significantly when such events occur shortly before the vote. The estimated increase in the probability of re-election—approximately 12%—is both statistically and politically significant.

³⁹There is an invoice dedicated to generic expenditures for event organizations, but is not sufficient to isolate costs specifically related to Patron Saint celebrations.

⁴⁰While it is possible that individuals mistake greater expenditures in Patron Saint celebration with effective economic participation of the local government, the fact that in many cases costs and organizations are third-party responsibilities limits strategic opportunities for the incumbent mayor.

The results are robust to a wide array of controls and alternative specifications, including geographical fixed effects, measures of religiosity, and weather shocks. The analysis of heterogeneity indicates that the effect is largely homogeneous across different regions and municipal characteristics, with the notable exception of gender: female incumbents appear to leverage the festivities more effectively than their male counterparts.

Additionally, I document a positive and statistically significant effect on turnout, specifically for incumbent mayors seeking a second term. This finding highlights the role of strategic incentives: the effect on participation is amplified when an incumbent is on the ballot, suggesting that mayors seeking re-election invest more effort in leveraging the community event to mobilize the electorate compared to open races where such incentives are absent. While this paper cannot conclusively identify the precise mechanisms driving the observed effect, it outlines three plausible channels: (i) a short-term increase in individual happiness generated by the event leading to increased support for the incumbent, (ii) increased visibility and agenda-setting opportunities for the incumbent, and (iii) signaling of competence and valence through the successful organization of the festivity.

Finally, a promising avenue for future research would be to investigate whether the electoral advantage generated by local festivities creates spillover effects in regional or national elections. However, testing this hypothesis requires establishing a clear political alignment between the incumbent mayor's coalition and the parties competing at higher institutional levels. In the current data framework, such a connection is difficult to identify, as approximately 80% of the candidates in our sample run under 'civic lists' (*liste civiche*), which are not directly traceable to a specific national political ideology.

Overall, this research contributes to our understanding of how incumbents can leverage non-policy tools-such as cultural and religious events-to strengthen their electoral prospects. It implies the presence of an advantage for incumbent mayors, which are usually invited to participate to such events, while opposing candidates do not necessarily have the same opportunity.

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APPENDIX

A Toponymy of street names

To identify streets carrying over religious meaning in their names, I firstly applied the methodology described by [Oto-Peralías \(2018\)](#), by isolating the top 200 most frequent words occurring in street names, and repeated the same process for all 20 Italian Regions, in order to account for vernacular forms and identified a larger group of words. The national list is reported in in Table [A.1](#). I identified all relevant religious words in all 21 lists and check wether they overlap with other words irrelevant for the analysis. Some of these words are those highlighted in yellow in Table [A.1](#). Since there are many streets with names of popes without specifically using the Italian word "Papa", which appears in the list, I also included streets with all 267 pope names. The words "Croce" and "Chiesa" have not been used in the main analysis, because there are many different important surnames and places which uses these words independently from religious meaning (for example, the general Dalla Chiesa, or the politician Benedetto Croce). I included those words in a second version of the indicator, paying attention to avoid as many coincidental matches as possible. The analysis is unaffected by the differences in the two indicators, thus I decided to use the most restrictive one to avoid potential measurement errors. Finally, for the indicator used in the main analysis I also included streets that contains obviously religious words that do not appear in the Top 200 lists. The words used in the main specification of the indicators, including variations due to gender, plurals or abbreviations, are the following: "Santo" (Saint), "Beato" (Blessed), "Vescovo" (bishop), "Cardinale"

(cardinal), "Don" and "Prete" (priest), "Monsignore" (monsignor), "Madonna" (madonna), "Padre" (Father), "Frate" (friar), "Gesù" (Jesus), "Trinità" (trinity), "Crocefisso" (crucifix), "Cristo" (Christ), "Arcangelo" (archangel), "Parrocchia" (parish), "Annunciazione" (annunciation), "Suora" (nun), "Abate" (abbot), "Cappella" (chapel).

Table A.1: Top 200 most frequent words in street names

Word	Freq	Word	Freq	Word	Freq	Word	Freq	Word	Freq
S.	68745	GIUSEPPE	27500	GIOVANNI	24738	ANTONIO	15601	FRANCESCO	13584
DON	11297	MONTE	11152	LUIGI	10963	GIACOMO	10138	VITTORIO	9944
PODERE	9860	PIETRO	9597	CARLO	8880	ROMA	8226	MARIA	7423
CASE	6369	ALESSANDRO	6329	ENRICO	6287	GUGLIELMO	6129	PAOLO	6099
GARIBALDI	5939	VINCENZO	5505	CESARE	5478	CHIESA	5440	FRATELLI	5155
MARCONI	5096	CORTILE	4901	REGIONE	4701	CASA	4647	UMBERTO	4536
CA'	4533	MAZZINI	4259	DANTE	4191	DOMENICO	4015	VALLE	4010
COLLE	3947	CASTELLO	3926	ALDO	3896	SPARSE	3879	EMANUELE	3821
PAPA	3812	CROCE	3731	NICOLA	3708	MAGGIO	3650	MORO	3624
CAVOUR	3566	GIORGIO	3505	MARTIRI	3479	FILIPPO	3471	NOVEMBRE	3466
MATTEOTTI	3465	ANDREA	3453	VERDI	3423	ALIGHIERI	3417	ANGELO	3398
VENETO	3376	FONTANA	3360	MADONNA	3313	BORGATA	3300	LEONARDO	3231
MICHELE	3228	PONTE	3218	VECCHIA	3197	LIBERTA'	3065	LORENZO	3048
GAETANO	3045	ROCCO	3022	SALVATORE	3014	MARCO	3012	TORRE	2969
ALBERTO	2934	UGO	2924	MANZONI	2904	BATTISTI	2822	CADUTI	2818
COSTA	2806	GRAMSCI	2789	APRILE	2788	MARIO	2774	VILLA	2739
SOTTO	2736	MARTINO	2730	GASPERI	2695	BATTISTA	2666	ALCIDE	2655
PADRE	2648	BOSCO	2617	TRIESTE	2584	PIAVE	2569	NUOVA	2481
MOLINO	2469	PIANO	2367	CAMILLO	2290	ALPE	2284	POGGIO	2280
EUROPA	2259	LEOPARDI	2252	CORTE	2249	CARDUCCI	2210	CRISTOFORO	2204
PORTA	2179	TRENTO	2172	BRUNO	2165	BELVEDERE	2160	CAMPO	2155
REGINA	2130	SOPRA	2128	SETTEMBRE	2126	BENEDETTO	2100	GIOSUE'	2095
VOLTA	2092	COLOMBO	2084	VINCI	2019	PASCOLI	2014	FONTE	2002
TOMMASO	1990	SANDRO	1988	GUIDO	1988	LAGO	1967	PIO	1961
GALILEO	1958	GENERALE	1940	VENTICINQUE	1913	STAZIONE	1897	MARGHERITA	1861
FERMI	1839	REPUBBLICA	1816	SILVIO	1813	PERTINI	1802	GIULIO	1802
GALILEI	1795	FOSCOLO	1787	SEBASTIANO	1784	ANNA	1763	ROSSINI	1744
ARMANDO	1715	PUCCINI	1703	MICHELANGELO	1696	VECCHIO	1694	ROSA	1690
CONTE	1685	POZZO	1634	LUCIA	1622	RISORGIMENTO	1615	FALCONE	1609
SERRA	1601	FELICE	1601	FIUME	1575	MONTI	1568	EUGENIO	1567
ITALIA	1563	CASALE	1553	SA	1544	LAVORO	1541	DIAZ	1539
VAL	1536	GABRIELE	1527	BELLINI	1523	RONCO	1521	KENNEDY	1506
STEFANO	1502	VITO	1485	PACE	1484	VENTI	1479	PETRARCA	1471
EMILIO	1442	TORINO	1431	GIOACCHINO	1415	MILANO	1404	MUNICIPIO	1400
SALVO	1400	NINO	1390	NAZIONALE	1386	CATERINA	1376	GRAPPA	1375
DUE	1374	PIAN	1366	PASQUALE	1362	MONSIGNOR	1356	NICCOLO'	1355
BORSELLINO	1350	PELLICO	1339	SAURO	1327	NAZARIO	1319	NICOLO'	1314
AGOSTINO	1311	VITTORIA	1305	VENEZIA	1297	BARTOLOMEO	1285	FEDERICO	1277
G.	1271	RAFFAELLO	1259	BENSO	1243	ROCCA	1230	FORNACE	1228
MINZONI	1222	GRAZIA	1210	D'ACQUISTO	1203	MULINO	1202	NAPOLI	1201

B Additional religiosity indexes

First, I collected data on students attending Catholic religion classes (IRC hereafter) across all school levels. IRC is a standard component of the Italian school curriculum, from elementary through high school; however, Italian law permits families to exempt their children from attending this class by submitting a formal request at the beginning of each school year. The effective percentage of students attending IRC classes relative to the total number of students serves as an informative proxy for religious importance, as non-religious families must undertake an additional bureaucratic step—an informal cost—to exclude their children from IRC. I computed this percentage by aggregating 2023 school data at the municipal level.⁴¹ Unfortunately, the original dataset lacks information for the regions of Valle d’Aosta and Trentino-Alto Adige, as well as for municipalities without schools or where schools did not share data with the Ministry of Education. Consequently, it is hard to prove if the student index could be considered a complete representation of Italian municipalities. Therefore, I decided to aggregate the indicator at provincial level, hopefully limiting concerns regarding municipalities with no middle and high schools, as well as municipalities with low rate of reporting. In the final sample, the religious students index was computed for 13,539 observations, covering 5,446 municipalities, which is assumed to be sufficient to represent all 105 provinces. While the street index may lead to an overestimation of religious belief, the student index may suffer from the opposite issue. Non-religious families might forget or fail to submit the required docu-

⁴¹The dataset is provided by the Italian Ministry of Education and made available by the Union of Rationalist Atheists and Agnostics (UAAR) under the #datiBeneComune project: <https://www.uaar.it/non-frequenza-irc-nelle-scuole-italiane/>

mentation on time, or may simply prefer that their children attend IRC classes to avoid them spending an hour unoccupied, as alternatives to IRC are not centrally regulated and depend on individual school policies. This is especially true in smaller municipalities, where generally only elementary schools are available.⁴²

To further strengthen the analysis, following for example De la Croix et al. (2023) or Ruiiu and Gonano (2020), I included a third religiosity index based on weddings. Specifically, I computed the 2019 share of religious weddings over total weddings at the province level.⁴³ As with the other measures, this index is not without limitations, and the direction of the potential bias is uncertain. On one hand, getting married in a church is a deeply rooted tradition in Italy; mixed couples may opt for a religious ceremony to satisfy partners or familial expectations, potentially leading to an overestimation of true religious belief. On the other hand, same-sex marriages are not permitted by the Catholic Church. Yet individuals in the LGBTQ+ community—who may nonetheless be devout Catholics—can legally marry via civil ceremony (available in Italy since 2016), which may lead to underestimation of religiosity.

⁴²To the best of my knowledge, this is the first attempt to use this indicator for religiosity, likely because it can only be applied in context where religious classes are mandatory but could be exempted on special cases

⁴³This dataset is provided by ISTAT at the municipal level. However, due to the nature of the index, many municipalities reported no weddings in the reference year. Therefore, I aggregated the data at the province level. While this limits the granularity of the analysis—since the index is absorbed by province fixed effects—it still provides a useful measure to validate the other indicators and can be employed in heterogeneity analysis.

C Correlation with probability of candidacy

Table C.1: Correlation with probability of candidacy for incumbents

Variables	(1) =1 if runs for re-election	(2)
0-10 Days	0.021 (0.015)	0.022 (0.018)
Observations	32,421	7,290
Interaction Prov*Year	YES	YES
Restricted sample	NO	YES

Notes. The table reports OLS estimates for the effect of the Patron Saint day on the probability of candidacy using a 10-day window treatment. Column (1) presents the baseline OLS estimates. Column (2) restricts the sample to elections held within an 80-day window. Both regressions include province $\times$ year interaction terms. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D RDD setting

In principle, the treatment assignment based on the distance between election date and Patron Saint day lands perfectly to an RDD model. The distance can be used as running variable, and the treatment effect would be estimated with more weight given to observations in the proximity of the cutoff, the election date. The model does not presents problem related to external validity, as both theoretical prediction and statistical evidences presented in this paper indicates that the treatment effect is only local and concentrated right before the cutoff. However, there are two important factors which prevents to use the RDD as main identification strategy. First, the running variable is not independent with respect to location-related confounding covariates. As explained in Section 5, regions in North Italy are proportionally underrepresented to the right of the cutoff (before election happens). This means that some variables related to geographic location are discontinuous at the cutoff, introducing potential biases in the analysis.

Moreover, as previously noticed, the distance should be intended as an "intention to treat". It is not possible to exactly identify which day of the celebration surrounding Patron Saint day is the most important to be exploited in order to gather consensus, nor it is possible to know with certainty when and up to which degree the mayor participate at the Patron Saint celebrations. Finally, the interaction term with rainfall dummy is crucial to measure the treatment effect correctly, and can not be included directly in an RDD setting.

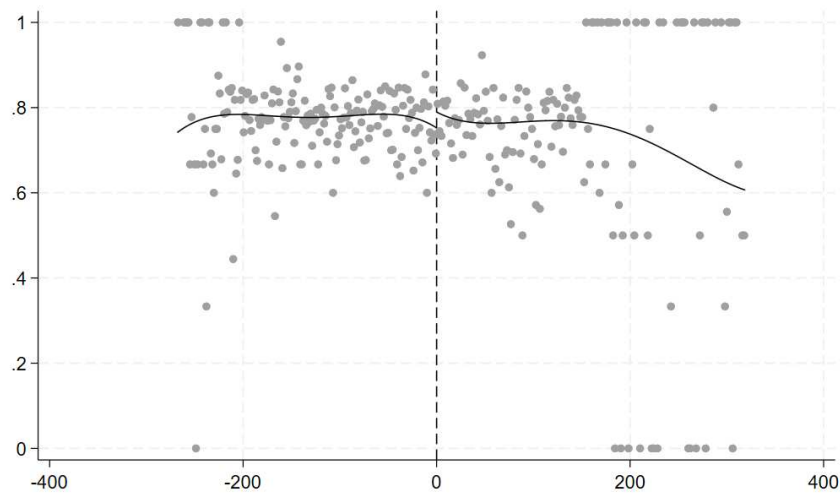


Figure D.1: RDD plot of probability of re-election on the distance from election date to Patron Saint day

Table D.1: Probability of re-election with RDD

Variables	(1)	(2) =1 if re-elected	(3)
RDD Estimate	0.016 (0.026)	0.076*** (0.021)	0.168*** (0.029)
Observations	16,356	16,374	3,260
Optimal bandwidth	65	32	6
Effective observations	6,511	3,008	380
Interaction Prov*Year	NO	YES	YES
Population groups	NO	NO	YES
Municipality controls	NO	NO	YES
Mayor's controls	NO	NO	YES
Religiosity control	NO	NO	YES
Social capital control	NO	NO	YES
Rain control	NO	NO	YES

Notes. The table reports RDD estimates for the probability of re-election using the distance between the election date and the Patron Saint's day as the running variable. Column (1) does not include any covariates. Column (2) includes only province×year interaction terms. Column (3) includes all additional covariates. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

No clear pattern emerges when looking at the classical RDD plot presented in Figure [D.1](#). Despite the estimated fitting line presents a small jump at the cutoff, there is no clear evidence of a discontinuity in the overall distribution of the sample bins. This is also confirmed in column (1) of Table [D.1](#), which reports RDD estimates for the treatment effect without additional covariates. Nonetheless, it is important to notice that in column (2), when including all province*year interactions, the estimated effect corresponds almost exactly to the OLS estimates on the 10 days window treatment, and is completely unaffected by the inclusion of all other control variables in column (3).

E Main result with unweighted religiosity measurement

Table E.1: Probability of re-election with 10-day treatment dummies and unweighted religiosity measurement

Variables	(1)	(2)	(3) =1 if re-elected	(4)	(5)	(6)
0-10 Days	0.086*** (0.032)	0.087*** (0.032)	0.088*** (0.032)	0.099** (0.042)	0.099** (0.044)	0.101** (0.043)
Rainfall	0.003 (0.007)	0.005 (0.007)	0.007 (0.007)	-0.010 (0.020)	-0.018 (0.024)	-0.014 (0.026)
0-10 Days $\times$ Rainfall	-0.086* (0.044)	-0.087* (0.045)	-0.090** (0.044)	-0.060 (0.054)	-0.052 (0.057)	-0.058 (0.059)
11-20 Days		0.027 (0.026)	0.028 (0.026)		-0.007 (0.031)	-0.004 (0.031)
21-30 Days			0.035 (0.034)			0.016 (0.033)
Observations	15,151	15,151	15,151	3,260	3,260	3,260
Interaction Prov*Year	YES	YES	YES	YES	YES	YES
Population groups	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES	YES	YES
Restricted sample	NO	NO	NO	YES	YES	YES

Notes. The table reports OLS estimates for the linear probability of re-election using 10-day window dummies and an unweighted street index of religiosity. Columns (1), (2), and (3) include all available control variables for the full sample. Columns (4), (5), and (6) report estimates for the restricted sample, which includes only elections held within an 80-day window. When the 11-20 days and 21-30 days dummies are not explicitly included in the regression, those periods are absorbed into the control group. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

F Wider windows treatment dummies

Table F.1: Probability of re-election with 15-day treatment dummies

Variables	(1)	(2)	(3) =1 if re-elected	(4)	(5)	(6)
0-15 Days	0.055* (0.028)	0.056** (0.028)	0.057** (0.028)	0.080** (0.037)	0.078** (0.038)	0.081** (0.038)
Rainfall	0.003 (0.008)	0.006 (0.008)	0.008 (0.008)	-0.014 (0.023)	-0.018 (0.027)	-0.012 (0.029)
0-15 Days $\times$ Rainfall	-0.053 (0.041)	-0.057 (0.041)	-0.059 (0.041)	-0.032 (0.055)	-0.027 (0.059)	-0.034 (0.061)
16-30 Days		0.028 (0.025)	0.029 (0.025)		-0.011 (0.027)	-0.007 (0.028)
31-45 Days			0.019 (0.025)			0.023 (0.028)
Observations	15,149	15,149	15,149	3,260	3,260	3,260
Interaction Prov*Year	YES	YES	YES	YES	YES	YES
Population groups	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES	YES	YES
Restricted sample	NO	NO	NO	YES	YES	YES

Notes. The table reports OLS estimates for the linear probability of re-election using 15-day window dummies. Columns (1), (2), and (3) include all available control variables for the full sample. Columns (4), (5), and (6) report estimates for the restricted sample, which includes only elections held within an 80-day window. When the 16-30 days and 31-45 days dummies are not explicitly included in the regression, those periods are absorbed into the control group. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F.2: Probability of re-election with 20-day treatment dummies

Variables	(1)	(2) =1 if re-elected	(3)	(4)
0-20 Days	0.044** (0.020)	0.046** (0.020)	0.039 (0.029)	0.044 (0.029)
Rainfall	0.004 (0.008)	0.008 (0.008)	-0.019 (0.024)	-0.012 (0.029)
0-20 Days $\times$ Rainfall	-0.055 (0.034)	-0.059* (0.034)	0.012 (0.049)	0.003 (0.053)
21-40 Days		0.027 (0.023)		0.017 (0.025)
Observations	15,149	15,149	3,260	3,260
Interaction Prov*Year	YES	YES	YES	YES
Population groups	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Restricted sample	NO	NO	YES	YES

Notes. The table reports OLS estimates for the linear probability of re-election using 20-day window dummies. Columns (1) and (2) include all available control variables for the full sample. Columns (3) and (4) report estimates for the restricted sample, which includes only elections held within an 80-day window. When the 21-40 days dummy is not explicitly included in the regression, that period is absorbed into the control group. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

G Other sample restrictions

Table G.1: Probability of re-election for different restricted samples

Variables	(1)	(2)	(3) =1 if re-elected	(4)	(5)	(6)
0-10 Days	0.086*** (0.033)	0.090*** (0.034)	0.097*** (0.035)	0.108*** (0.039)	0.147*** (0.044)	0.234*** (0.087)
Rainfall	0.003 (0.008)	0.005 (0.010)	-0.001 (0.013)	-0.011 (0.018)	0.015 (0.034)	-0.033 (0.078)
0-10 Days $\times$ Rainfall	-0.087* (0.047)	-0.092* (0.048)	-0.089* (0.051)	-0.083 (0.054)	-0.131* (0.070)	-0.211* (0.118)
Observations	15,149	11,084	6,721	4,184	1,338	351
Interaction Prov*Year	YES	YES	YES	YES	YES	YES
Population groups	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES	YES	YES
Sample Restriction	NO	250 days	150 days	100 days	40 days	20 days

Notes. The table reports OLS estimates for the linear probability of re-election using 10-day window dummies across different restricted samples. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

H Probit and Logit regressions

Table H.1: Probability of re-election with Probit and Logit models

Variables	(1)	(2)	(3)	(4)
		=1 if re-elected		
0-10 Days	0.319** (0.140)	0.495** (0.201)	0.571** (0.242)	0.899*** (0.339)
Rainfall	0.009 (0.033)	-0.035 (0.100)	0.016 (0.057)	-0.075 (0.175)
0-10 Days $\times$ Rainfall	-0.312 (0.198)	-0.338 (0.285)	-0.575* (0.341)	-0.630 (0.495)
Observations	13,788	2,394	13,788	2,394
Interaction Prov*Year	YES	YES	YES	YES
Population groups	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES
Restricted sample	NO	YES	NO	YES
Estimator	Probit	Probit	Logit	Logit

Notes. The table reports probit and logit coefficient estimates for the probability of re-election using a 10-day window dummy. Columns (1) and (2) report estimates from a probit model. Columns (3) and (4) report estimates from a logit model. Columns (2) and (4) report estimates for the restricted sample, which includes only elections held within an 80-day window. Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

I Other clusters

Table I.1: Probability of re-election with different clusters

Variables	(1)	(2)	(3) =1 if re-elected	(4)	(5)	(6)
0-10 Days	0.086*** (0.032)	0.099*** (0.038)	0.086*** (0.033)	0.099** (0.039)	0.086** (0.034)	0.099** (0.046)
Rainfall	0.003 (0.007)	-0.010 (0.019)	0.003 (0.007)	-0.010 (0.019)	0.003 (0.005)	-0.010 (0.023)
0-10 Days $\times$ Rainfall	-0.085* (0.044)	-0.060 (0.055)	-0.085* (0.045)	-0.060 (0.056)	-0.085** (0.037)	-0.060 (0.049)
Observations	15,862	3,418	15,862	3,418	15,862	3,418
Interaction Prov*Year	YES	YES	YES	YES	YES	YES
Population groups	YES	YES	YES	YES	YES	YES
Municipality controls	YES	YES	YES	YES	YES	YES
Mayor's controls	YES	YES	YES	YES	YES	YES
Religiosity control	YES	YES	YES	YES	YES	YES
Social capital control	YES	YES	YES	YES	YES	YES
Rain control and interaction	YES	YES	YES	YES	YES	YES
Restricted sample	NO	YES	NO	YES	NO	YES
Cluster	NO	NO	Municipality	Municipality	Region	Region

Notes. The table reports OLS estimates for the linear probability of re-election using a 10-day window dummy. Columns (1) and (2) report OLS estimates with heteroskedasticity-robust standard errors. Columns (3) and (4) report OLS estimates with robust standard errors clustered at the municipal level. Columns (5) and (6) report OLS estimates with robust standard errors clustered at the regional level. Even columns report estimates for the restricted sample, which includes only elections held within an 80-day window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

J Religious and gender heterogeneity with continuous days treatment

Table J.1: Heterogeneity in religiosity with continuous treatment

Variables	(1) <i>Rel. (streets) = 0</i>	(2) <i>Rel. (streets) = 1</i> =1 if re-elected	(3)
Before	0.016 (0.028)	0.088*** (0.026)	0.059*** (0.021)
Distance in days	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Before × Distance in days	-0.000 (0.000)	-0.001*** (0.000)	-0.001** (0.000)
Rainfall	0.021 (0.027)	0.020 (0.021)	0.018 (0.017)
Before × Rainfall	-0.037 (0.045)	-0.125*** (0.036)	-0.082*** (0.028)
Rainfall × Distance in days	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Before × Rainfall × Distance	0.000 (0.000)	0.002*** (0.000)	0.001*** (0.000)
Religiosity			0.001 (0.001)
Before × Religiosity			-0.001 (0.001)
Distance in days × Religiosity			-0.000 (0.000)
Before × Distance × Religiosity			-0.000 (0.000)
Observations	7,650	7,711	15,862
Interaction Prov*Year	YES	YES	YES
Population groups	YES	YES	YES
Municipality controls	YES	YES	YES
Mayor's controls	YES	YES	YES
Religiosity control	NO	NO	YES
Social capital control	YES	YES	YES
Rain control and interaction	YES	YES	YES
Restricted sample	NO	NO	NO

Notes. The table reports OLS estimates for the linear probability of re-election using a continuous treatment variable for days. Columns (1) and (2) present estimates for the sub-samples below and above the median of the religiosity index, respectively. Column (3) includes interaction terms between the treatment variables and the religious streets index. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table J.2: Heterogeneity in gender with continuous treatment

Variables	(1) <i>Male mayors</i>	(2) <i>Female mayors</i> =1 if re-elected	(3)
Before	0.046** (0.020)	0.104 (0.064)	0.046** (0.020)
Distance in days	0.000 (0.000)	0.001* (0.000)	0.000 (0.000)
Before × Distance in days	-0.001*** (0.000)	-0.001 (0.001)	-0.001*** (0.000)
Rainfall	0.011 (0.019)	0.011 (0.080)	0.013 (0.018)
Before × Rainfall	-0.080** (0.032)	-0.086 (0.099)	-0.079*** (0.029)
Rainfall × Distance in days	-0.000 (0.000)	-0.000 (0.001)	-0.000 (0.000)
Before × Rainfall × Distance	0.001*** (0.000)	0.001 (0.001)	0.001*** (0.000)
Female			-0.116*** (0.024)
Before × Female			0.076* (0.044)
Female × Distance in days			0.000** (0.000)
Before × Female × Distance			-0.001 (0.001)
Observations	13,615	1,130	15,149
Interaction Prov*Year	YES	YES	YES
Population groups	YES	YES	YES
Municipality controls	YES	YES	YES
Mayor's controls	YES	YES	YES
Religiosity control	NO	NO	YES
Social capital control	YES	YES	YES
Rain control and interaction	YES	YES	YES
Restricted sample	NO	NO	NO

Notes. The table reports OLS estimates for the linear probability of re-election using a continuous treatment variable for days. Columns (1) and (2) present estimates for the subsamples of male and female mayors, respectively. Column (3) includes interaction terms between the treatment variables and the female gender dummy. Robust standard errors clustered at the province level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.