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Pay Incentives to Run for Local Governments

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Abstract

Local governments in advanced democracies have increasingly struggled to attract political candidates, weakening electoral competition and accountability at the municipal level. While several factors may contribute to this trend, politicians' salaries represent one of the few policy levers that can be directly adjusted by policymakers. We study a large-scale reform that substantially increased local politicians' pay, exploiting quasiexperimental variation in election timing across municipalities. We find that higher salaries increase political entry, particularly among first-time candidates. Importantly, effects are heterogeneous across local contexts: in less affluent municipalities and in areas with lower entry barriers, higher pay also raises female candidacies and their probability of election. In the poorest areas, the reform further alters the composition of local political elites, shifting recruitment toward candidates with different educational and occupational backgrounds.

JEL classification

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Keywords

local governments, politicians' wages, time-shifted control design

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1 Introduction

In recent years, a growing number of developed democracies have witnessed a marked decline in individuals willing to pursue political careers at the local level. Civil society organizations and media outlets have highlighted notable examples in the United Kingdom (ERS, 2019; UK Parliament, 2019; BBC, 2022), Italy (Pagella Politica, 2021), Japan (Nikkei Asia, 2023; Nippon, 2024), Australia (ABC News, 2021), New Zealand (New Zealand Herald, 2022; The Spinoff, 2022), and the United States (New America, 2023). This contraction in the candidate pool has led to a shortage of mayoral hopefuls. The consequences for society are profound, as the competitive integrity of local elections is undermined for several reasons.

First, the erosion of electoral competition weakens accountability mechanisms, since incumbent office-holders face fewer credible challengers and thus feel less pressure to respond to constituent needs. Second, policy innovation stagnates: with a smaller talent pool, local governments struggle to attract individuals possessing the diverse expertise required to design and implement effective public services. Third, the shrinking of participatory pathways accelerates citizen disengagement, undermining social capital and trust both horizontally (among neighbors) and vertically (between citizens and institutions). Lastly, this vacuum can be exploited by well-resourced interest groups, heightening the risk of clientelism and corruption-trends that may ultimately compromise the legitimacy and resilience of the broader political system.

Some determinants of this decline include heightened public scrutiny, an increasing number of mayors experiencing harassment and threats, and waning civic engagement among younger cohorts (Bertoni *et al.*, 2023; Daniele *et al.*, 2023; Håkansson, 2021; Pulejo and Querubín, 2023). Another factor is the relatively low pay of local politicians in light of the significant responsibilities and duties that their roles entail. In both theoretical and empirical research in political economy, some argue that higher salaries for local politicians are necessary to attract a greater number of highly skilled candidates

(Besley, 2004; Ferraz and Finan, 2009; Dal Bó *et al.*, 2013; Gagliarducci and Nannicini, 2013). Conversely, others advocate reducing politicians' compensation to save costs, promote fiscal responsibility, and prevent potential rent-seeking behavior (Caselli and Morelli, 2004; Mattozzi and Merlo, 2008; Gagliarducci *et al.*, 2010).

Leveraging a significant reform in Italy that raised local politicians' salaries, this article examines whether increasing their remuneration not only raises the number of mayoral candidates but also improves their average quality (proxied by education and professional background), an essential factor for effective local governance.[1] To gauge political competition, we assess the total count of candidates, also distinguishing the "novel" contenders, who are those without any prior political experience. We evaluate vote concentration using the Herfindahl-Hirschman Index (HHI), computed by summing the squares of each mayoral candidate's share of votes. We explore the policy's influence on these educational and professional indicators within both the executive committee, appointed by the mayor, and the city council, directly elected by the voters.[2]

The reform led to a noteworthy upsurge in mayoral salaries, often exceeding 50%, rendering political careers more financially appealing in comparison to the average income in Italy. For instance, the mayor of a municipality with 2,500 inhabitants has seen her monthly salary increasing from 1,952 euros to 3,036. This is a significant rise, particularly when considering that the monthly gross average income in Italy is approximately 2,200 euros. Figure 1 provides a graphical evidence of the change in monthly wages imposed by the policy.[3] Importantly, the pay rise was not readily

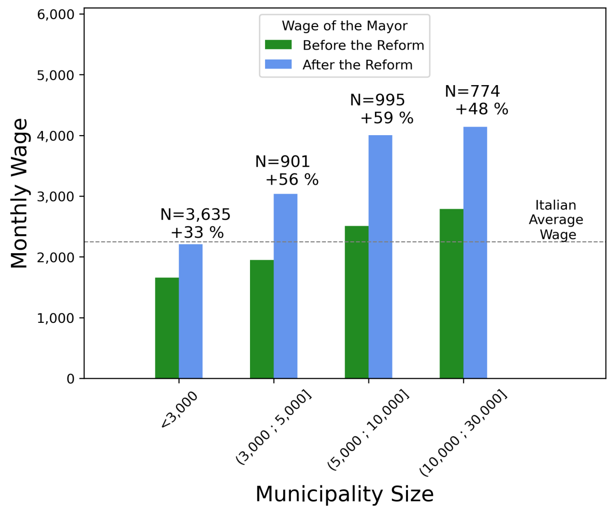
[1]This is important for society at large because local politics often serve as a gateway to a political career at the regional and national levels (Detterbeck, 2016; Einstein *et al.*, 2020). Prominent examples include Boris Johnson, who transitioned from his role as Mayor of London to become Prime Minister of the UK (Reuters, 2019); Olaf Scholz, who served as Mayor of Hamburg before becoming Chancellor of Germany (BBC, 2021); and Matteo Renzi, the former Mayor of Florence, who went on to become Prime Minister of Italy (BBC, 2014).

[2]See Section 2 for further details on the composition of these political institutions.

[3]Budget Law 234, December 30, 2021. Overall, the reform invests 220 million euros each year. The pay-rise involved all members of local government. The executive committee appointed by the elected mayor receives the equivalent of 45% of the mayor's salary, whereas the elected city council has a gross salary that is less than or equal to 25% of the mayor's wage.

anticipated, as it only appeared in the draft budget law transmitted by the government to Parliament on November 11, 2021 (Senate Act 2448, Volume 1, art. 175, p. 229).

Figure 1: Mayors' Wage Before and After the Reform



Notes: This figure shows the mayors' monthly gross salary in euro (vertical axes) before and after the reform for different population size groups, reported on the horizontal axes. It also reports the percentage increase in salary and the number of municipalities in each group (N). The dashed horizontal line corresponds to the Italian monthly gross average income, which is approximately equal to 2,200 euros.

As the reform affected all municipalities simultaneously and applied to all politicians in office as January 2022, regardless of when they were elected, there are no untreated cities in post-reform elections. This makes it impossible to adopt traditional policy evaluation methods such as difference-in-differences (DiD) or regression discontinuity designs (RDD), which rely on the contemporaneous availability of treated and untreated units.[4] We address this challenging empirical context by proposing the novel *time-shifted control design* (*TSCD*), which estimates treatment effects by exploiting misaligned election dates across cities. Most importantly, we formalize the *TSCD* within the potential outcomes framework, specify the assumptions that justify attributing the estimated impacts to the intervention, and demonstrate that, conditional on the underlying assumptions, the estimator yields an unbiased estimate of the average

[4]The reform was adopted on December 30, 2021. A pre-reform election was held on October 3, 2021. Post-reform elections occurred on June 12, 2022, and May 28, 2023.

treatment effect on the treated (ATT). In the Italian context, potential candidates have limited discretion in choosing where to run, and election timing varies across cities in a quasi-random manner. Indeed, since the late 1940s, municipal elections are held each year in a different cohort of municipalities, and each municipality schedules its elections at five-year intervals, unless the municipal council is dissolved early. This scenario prevents cities from choosing their election year, but allows for varied exposure to the reform at election time, creating distinct treatment and control groups for analysis.

Therefore, *TSCD* compares each treated city belonging to the cohorts holding the most recent local elections in 2022 or 2023 with the untreated cities belonging to the 2021 cohort, and having the most similar trends in terms of pre-2021 electoral outcomes. Essentially, the *TSCD* adapts the exact-matching non-parametric DiD estimator for aggregate panel data proposed by Imai *et al.* (2023) to the case of misaligned elections. Prior to conducting the empirical analyses, we confirmed that there were no differences in the pre-existing trends of the dependent variables between treated and control municipalities. With the *TSCD* approach, we can measure the direct reform's ATT as well as the conditional average treatment effects (CATEs).[5]

We find that the reform successfully increased the number of novel candidates in the political arena but only in elections that occurred in 2023, and it did not improve the overall quality of the political class. These outcomes possibly stem from the varying distribution of candidates whose candidacy decision is influenced by the reform (i.e., compliers), which may differ across diverse political and socio-economic contexts, alongside the timing of the elections with respect to the reform's implementation. Indeed, we find that in specific contexts with lower entry barriers and fewer appealing alternatives, such as in poorer areas, the reform succeeded in drawing more mayoral candidates,

[5] It is important to clarify that this framework diverges significantly from scenarios of staggered treatment adoption, wherein, over time, certain cohorts transition to treated status while others remain in the control group. In our case, given that post-reform elections were uniformly affected by the policy, the control group is defined exclusively by those municipalities that, by coincidence, held their elections immediately before the policy was enacted (see Appendix.2 for more details).

especially those novel to the political arena.[6]

Thus, we find an increase in mayoral competition within municipalities featuring open seats (i.e., when the incumbent is ineligible for re-election) and in those that did not attract more than two candidates pre-reform. These results also hold for female candidates, and we observe a rise in the number of elected female mayors in these contexts.[7] However, more competition has not been accompanied by significant changes in the educational or professional levels of the candidates elected. With respect to the economic context, we find that in less affluent municipalities the reform increased the proportion of executive committee members from white-collar professions, while it decreased the average education level of city council members. This result indicates that the increase in compensation may not have attracted the most qualified candidates. Instead, it appears to have drawn individuals primarily motivated by financial incentives, who may had limited opportunities in other professions (Messner and Polborn, 2004).

The paper is organized as follows. Section 2 describes the institutional background and the details of the reforms, while Section 3 the literature on the topic. Section 4 the data. Section 5 outlines the empirical strategy and describes the estimation procedure. Section 6 discusses the results. Finally, Section 7 concludes the paper.

2 Institutional Background

Local governments in Italy are administrated by a city council and an executive committee appointed by the elected mayor.[8] The council and the mayor are directly elected

[6] In Appendix.3, we provide several robustness checks and we implement a placebo analysis.

[7] For instance, in the UK, 80% of councilors elected in any given year are incumbents, which creates barriers for women entering local government (Maguire, 2018). In contrast, we do not find any evidence regarding the probability of re-election for incumbents in our setting, but we do find that the reform increases both the number of women running for local government and their probability of being elected.

[8] The executive committee is the *Giunta Comunale* whose members are the *Assessori* and the Mayor (*Sindaco*). One of the *Assessori* also performs the function of Deputy Mayor. When we examine the members of the executive committee we only refer to the *Assessori* as we examine the mayor separately. The city council (*Consiglio Comunale*) is made up of Councilors (*Consiglieri*), and it is chaired by the council chairperson.

for a five-year term. The elections for the mayor and the local council take place on the same day and with the same ballot. Voters must choose a mayoral candidate and can optionally cast a vote for a list and up to two preferences for council members. Executive committee members are not directly elected by voters but are appointed by the winning mayoral candidate after the election.[9] Therefore, the election of the three political roles are strongly intertwined.

The wage of local government politicians was established in 2000, reduced by 10% in 2006 (Article 1, paragraph 54), and slightly increased only for municipalities with up to 3,000 inhabitants in 2019 (Legislative Decree 124/2019, Article 57c.). The pay rise, approved on December 30, 2021, applies to the mayor of all ordinary regions' municipalities (6,562 out of 7,901 municipalities), and also increased the remuneration of other local politicians, such as deputy mayors and councilors. Importantly, the pay rise applies to all politicians in office from January 2022, regardless of when they were elected. This means that both those elected in 2021, as well as in previous or future years, will receive the new salary. However, the reform can only influence the decision to run for office among those seeking election after its implementation, given the content and timing of the reform were highly uncertain.

Some civil associations have accused the government of having adopted the pay rise in secrecy and without public debate (see Open Polis, May 24, 2022). However, there had been two similar proposals from left and right Members of Parliament that were published in June 2021. Senate Act No. 2266, June 8, 2021, first signatory Ignazio La Russa; Senate Act No. 2310, June 28, 2021, first signatory Luigi Zanda. Therefore, it is unlikely that local candidates were aware of an imminent pay rise before the municipal elections held in October 2021.

While a potential increase in politicians' salaries had been discussed for years, it is

[9]Executive committee members are generally chosen from the local council members who received the most votes within the governing parties. The number of available seats (and thus the likelihood of election) depends on the party's vote share and whether the linked mayoral candidate is elected. In addition, in municipalities with a population exceeding 15,000 inhabitants, the executive committee members can be appointed by the mayor, even from outside the members of the local council.

important to note that the exact timing of the reform's approval and implementation in 2021 can be considered a random event. Despite ongoing debates and proposals, the reform required substantial financing and had to follow the ordinary legislative process, being included in the budget law proposed in late October and typically approved at the end of December. Given that the 2021 elections occurred in early October, with candidacies finalized at least a month prior, candidates likely had no clear public indication that the reform would be enacted that year. Therefore, significant uncertainty surrounded the approval of the reform at the time when candidates made their decisions, minimizing the risk of an anticipation effect influencing electoral behavior.

The salary of local politicians is set according to the population class in which the municipality falls, according to the last official census.[10] The salary increase concerns several population thresholds, and we will analyze all those concerning small and medium-sized municipalities up to 30,000 inhabitants, which make up about 96% of Italian municipalities and 54% of the population.[11]

Table 1 demonstrates the shifts in mayors' monthly gross wages among various population size groups before and after a reform, where the salary hike often exceeds 50%, raising the wages substantially above the Italian average.

The examination of this reform is of particular significance in light of the growing disenchantment with politics and the declining interest in pursuing political careers. In 2021, seven Italian municipalities skipped local elections due to a lack of candidates, and 217 (i.e., 16% of total) municipalities saw elections with only a single candidate. During this time, the National Association of Italian Municipalities (ANCI) consis-

[10]Exceptions to this rule apply to provincial and regional capitals (*Capoluoghi di Provincia e di Regione*). Those municipalities are excluded from the analysis.

[11]Previous studies primarily targeted municipalities around the 5,000 inhabitant threshold (i.e., from 3,000 to 7,000), representing 21% of Italian municipalities and 13% of the population (Gagliarducci and Nannicini, 2013; Grembi *et al.*, 2016). In contrast, we only exclude the largest municipalities due to their limited number, which hinders a credible estimation of the counterfactual scenario. Electoral systems differ based on population size: smaller municipalities (below 15,000 inhabitants) adopt a single-round plurality system, while those above 15,000 inhabitants utilize a run-off system. Seats in the council predominantly align with the winning mayor's list(s): 60% in larger municipalities and two-thirds in smaller ones.

Table 1: Monthly Wage of the Mayor by Municipality Size

Municipality Size (1)	Monthly Wage Before the reform (2)	Monthly Wage After the reform (3)	Monthly Wage Increase (4)	N. of Municipalities (5)
$\leq$ 3,000	1,659	2,208	549 (+33%)	3,635
(3,000 ; 5,000]	1,952	3,036	1,084 (+56%)	901
(5,000 ; 10,000]	2,510	4,002	1,492 (+59%)	995
(10,000 ; 30,000]	2,789	4,140	1,351 (+48%)	774

Notes: This table shows the detailed changes in mayors' monthly salary before and after the reform for different population size groups. Column (1) reports the municipality size measured by the number of inhabitants. Columns (2) and (3) report the corresponding mayor's salary before and after the reform, respectively. Column (4) describes the post-reform wage increase, with the percentage increase shown in parentheses. Column (5) provides the number of municipalities belonging to each population size bracket. Monthly wages are reported in Euros and the number of municipalities refers to the 15 Italian ordinary status regions. We report the monthly wage changes only concerning municipalities up to 30,000 inhabitants, which are not provincial capitals (in Italy there are only 5 provincial capitals with up to 30,000 inhabitants and none of which held municipal elections in 2022). The pay rise is not fully instantaneous as it is applied 45% in 2022, 68% in 2023, and fully from 2024 onwards.

tently urged government intervention to shield mayors from undue responsibility and enhance financial incentives. ANCI warned that *"...if the trend persists, especially in smaller municipalities, there could soon be a shortage of citizens willing to take on the role of mayor"* (ANCI, 2021).[12] Nevertheless, uncontested elections and low compensation for local politicians are not unique to Italy (see, for instance, ERS (2019) and UK Parliament (2019)). Analyzing the impact of this remuneration policy can thus generate evidence relevant to other societies experiencing similar issues in local electoral dynamics.

3 Literature

This paper contributes to the existing body of literature that explores the influence of compensation on the competence and selection of local politicians. From a theoretical perspective, the effect of monetary incentives remains ambiguous and appears to vary depending on the specific institutional and socio-economic context (Caselli and Morelli, 2004; Besley, 2004; Messner and Polborn, 2004; Besley, 2005; Poutvaara and Takalo,

[12]See also the media articles (in Italian) by Corriere della Sera (2019) and la Repubblica (2021).

2007; Mattozzi and Merlo, 2008; Keane and Merlo, 2010; Dal Bó and Finan, 2018; Fedele and Giannoccolo, 2020). For instance, Caselli and Besley argue that higher salaries attract more capable individuals to political roles, whereas Messner and Mattozzi caution that increased compensation may unintentionally trigger negative selection effects by encouraging less-qualified individuals to enter politics.

Empirical research has also yielded mixed results (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Dal Bó *et al.*, 2017; Pique, 2019). For example, Ferraz's study of Brazilian local legislators finds that higher salaries can enhance electoral competition and improve the overall quality of politicians, while Pique's research in Peru reveals negative effects of increased wages on both selection and performance.[13]

The main focus of this literature is typically on the selection effect—how remuneration influences the type of individuals who run for office, particularly in the case of mayors or higher-level politicians. This interest is grounded in the widespread view that the quality of elected officials plays a critical role in shaping policy outcomes (Chattopadhyay and Duflo, 2004; Gagliarducci and Nannicini, 2013), although Freier and Thomasius (2016) presents a notable exception. As a result, most empirical studies have relied on settings that allow the estimation of what is known as the local average treatment effect (LATE).

Our study is rooted in a pressing social issue facing many local governments: uncontested elections and a general shortage of mayoral candidates. To address this, we exploit a large-scale natural experiment that introduced a substantial pay increase for all local politicians. This allows us to estimate the average treatment effect on the treated (ATT) across a broad sample of towns—representative for 96% of Italian municipalities—and to extend the analysis beyond mayors, encompassing both executive and legislative branches of local government.[14]

[13]Delfgaauw and Dur (2007) also investigate self-selection into the public sector, showing that when public sector wages are lower than in the private sector, more capable individuals tend to opt for the latter, while more motivated individuals choose public service.

[14]In the Italian context, Gagliarducci and Nannicini (2013) study municipal elections between 1993 and 2001, employing a sharp RDD based on salary thresholds tied to population size. They estimate

Our findings show that the reform increased political competition, particularly in municipalities with lower entry barriers or where alternative labor market opportunities are limited.[15] However, in these contexts, we also observe a decline in the average education level of elected city council members, which could have adverse long-term consequences for local governance and economic outcomes (Besley *et al.*, 2011).

Finally, we contribute methodologically by introducing the *time-shifted control design* (TSCD), which exploits natural variation in the timing of municipal elections. This approach offers a valuable alternative to standard evaluation methods (e.g., RDD and DiD) in settings where treatment timing is uniform but election timing is staggered. In our context, it enhances the credibility and external validity of our findings and may be applicable to other empirical studies with similar institutional features.

4 Data

The post-reform elections were held on June 12, 2022 and on May 14, 2023 in over 1,000 municipalities having at most 30,000 inhabitants. These towns will be compared to the municipalities that held elections right before the reform (i.e., October 3, 2021). The final sample is made up of 1,024 treated and 895 control municipalities and the details of its construction are described in Appendix.1.

We collected from the Ministry of the Interior the electoral results (e.g., number of candidates) and the information concerning the education levels and the professions of the candidates elected (i.e., mayor, executive committee, and city council members), along with data on municipal elections.[16] Population data for policy thresholds are sourced from the National Institute of Italian Statistics' (ISTAT) permanent census.

a LATE at the 5,000-inhabitant threshold and find that higher salaries attract more educated mayors who tend to perform better. Caria *et al.* (2023) use a similar approach but focus on earlier electoral cycles between 1985 and 1990.

[15] Low compensation may help explain the shortage of candidates. Bertoni *et al.* (2023) show that although winning a mayoral race (1993–2017) initially increases earnings, the wage premium turns negative after a decade.

[16] Electoral results come from "Eligendo" while information on politicians comes from the *"Anagrafe degli Amministratori"*.

We also acquired yearly log population data from ISTAT, and income per capita from the Ministry of Economy and Finance. These variables span the last five local electoral rounds per municipality, from 2001 to 2023. The availability of data for five consecutive electoral rounds enables the construction of a credible counterfactual scenario for each treated municipality, as discussed in section 5.1.

We use these data to create measures that serve as proxies for electoral competition, the educational attainment of local politicians, and their professional backgrounds.[17] To measure electoral competition, we consider the number of candidates and the HHI. Educational proxies gauge the mayor's years of education and the average years of education of the members of the executive committee and the city council.

4.1 Descriptive Statistics

Table 2 reports summary statistics for municipalities in the control group (G-2021) and in the two treated groups (G-2022 and G-2023), based on data from the four elections held prior to 2020. The table shows that treated municipalities are, on average, slightly larger in population and income per capita compared to control ones. Geographic distribution also differs somewhat, with a higher share of municipalities from Southern Italy in the treated groups. Political characteristics—including the number of mayoral candidates, market concentration (HHI), and average education levels—are broadly similar across groups, although treated municipalities tend to have slightly higher values on some dimensions, such as the share of candidates with white-collar backgrounds and education levels among mayors and council members. Overall, the balance across key covariates suggests a reasonable degree of comparability between treated and control groups prior to treatment. Nevertheless, to address residual imbalances and bolster the

[17] We collect job data from the Ministry of the Interior, classifying them into "white-collar" roles or not. Following Gagliarducci and Nannicini (2013), "white-collar" includes professions like physicians, lawyers, engineers, architects, managers, researchers, and professors, known as *"professionisti"* in Italy. This distinction relies on intellectual resource utilization, qualifications or registration in official registers (*"albi"* and *"esami di stato"*), and possession of higher university degrees. The complete list of white-collar jobs is available upon request from the authors.

validity of our comparisons, we employ a matching strategy in the empirical analysis.

Table 2: Summary Statistics

Variable	**Control**	**Treated**	
	G-2021	**G-2022**	**G-2023**
Population	4,358	5,579	5,323
	(4,946)	(6,068)	(5,669)
Income per capita	15,614	15,714	15,976
	(4,219)	(4,344)	(4,300)
Share Northern Italy	0.535	0.489	0.460
	(0.499)	(0.500)	(0.499)
Share Southern Italy	0.325	0.389	0.393
	(0.468)	(0.488)	(0.489)
Mayor:			
Number of candidates	2.665	2.946	2.955
	(1.059)	(1.277)	(1.296)
HHI	0.508	0.483	0.492
	(0.167)	(0.158)	(0.172)
Share of collars candidates	0.264	0.318	0.288
	(0.441)	(0.466)	(0.453)
Avg. years education	14.62	14.96	15.06
	(3.428)	(3.266)	(3.238)
Executive Committee:			
Share with collar workers	0.159	0.168	0.161
	(0.241)	(0.249)	(0.244)
Avg. years education	13.25	13.51	13.50
	(2.653)	(2.528)	(2.547)
City Council:			
Share with collar workers	0.147	0.162	0.156
	(0.133)	(0.146)	(0.141)
Avg. years education	12.998	13.289	13.350
	(1.591)	(1.543)	(1.592)
Observations	895	589	435

Notes: This table presents summary statistics for both the treated and control groups. Specifically, it reports the sample means of each variable for the four elections prior to 2020, with standard deviations in parentheses.

5 Identification Strategy

Most policy evaluation techniques based on the potential outcomes framework determine the causal effect by comparing post-treatment outcomes between treated and untreated groups. However, these methods cannot be adopted when all units receive the treatment simultaneously, leaving no untreated units for comparison in the post-treatment period. This is a significant constraint, especially when estimating the effects of simultaneous policy shifts, as in the case of a large-scale shock or a nationwide program with universal participation (Duflo, 2017), such as the reform under analysis. To address this challenge, we introduce a novel empirical design that exploits the natural misalignment of election dates across municipalities.[18] This is not the first study to exploit misaligned elections to estimate treatment effects—see, among others, Alesina and Paradisi (2017) and Repetto (2018). However, we are the first to explicitly recognize that this framework rules out the application of traditional methods and to formalize the assumptions underpinning the retrieval of the ATT, demonstrating that the resulting estimates are unbiased under these assumptions.

Define $Y^D_{ic_h,\tau}$ as the electoral outcome (e.g., the number of mayoral candidates) of the municipality i belonging to the cohort of municipalities c_h observed at time τ, which receives the binary treatment D. We can only observe Y intermittently (every five time periods), and the observation time τ is consistent within each cohort but varies across different cohorts. This means that Y is observed for cohort c_h at a time τ that differs from the observation time for another cohort c_j (with $j \neq h$). In this framework of

[18]Two potential evaluation strategies to appraise the policy's causal impact include population-based RDD at the 5,000 population threshold (as in Gagliarducci and Nannicini (2013)) and geographic RDD or DiD contrasting ordinary and special status regions' municipalities. The first approach, although feasible, presents serious limitations that it would hinder the relevance of the empirical analysis. Indeed, there are only a small number of municipalities around the 5,000 population threshold holding elections in 2022 and 2023 (178 municipalities had populations between 3,500 and 6,500 inhabitants). Moreover, as the reform affected all municipalities, the population-based RDD would only compare municipalities with small differences in the intensity of treatment rather than treated and untreated municipalities. The second strategy is impractical as special status regions implemented similar reforms in 2021 or 2022, making substantial wage increases universal among Italian local politicians.

misaligned election dates across municipalities, suppose that the population is divided into two groups: the treated with $D = 1$ and the untreated with $D = 0$ based on the time of the treatment. For example, if the treatment takes place between τ and $\tau + 1$, that is $D \in (\tau, \tau+1]$, we observe the post-treatment value $Y^1_{ic_2,\tau+1}$ for the treated cohort (c_2) and the pre-treatment value $Y^0_{jc_1,\tau}$ for the untreated cohort (c_1).
In this scenario, we make the following two assumptions to identify ATT_{c_2}:

Assumption ASS.1. $\nexists D' \neq D \in (\tau, \tau + 1]: \quad \mathbb{E}(Y^1_{ic_2,\tau+1}|D' = 1) - \mathbb{E}(Y^1_{ic_2,\tau+1}|D' = 0) \neq 0$

Assumption ASS.2. $\mathbb{E}(Y^0_{ic_2,\tau+1} - Y^0_{ic_2,\tau-4}) = \mathbb{E}(Y^0_{jc_1,\tau} - Y^0_{jc_1,\tau-5})$

ASS.1 states that between times τ and $\tau + 1$ there is no binary event D', which is different from the treatment D, that might influence the expected value of $Y^1_{ic_2,\tau+1}$. ASS.2 states that treated and untreated municipalities would have followed the same trend in electoral outcomes without the treatment. The latter assumption resembles the parallel trends assumption of the DiD estimator, but it applies to treatment and control groups observed in misaligned time periods. Under these assumptions, we retrieve ATT_{c_2} as follows:

$$\mathrm{A}TT_{c_2} = \left[\mathbb{E}\left(Y^1_{ic_2,\tau+1}\right) - \mathbb{E}\left(Y^0_{ic_2,\tau-4}\right)\right] - \left[\mathbb{E}\left(Y^0_{jc_1,\tau}\right) - \mathbb{E}\left(Y^0_{jc_1,\tau-5}\right)\right] \tag{1}$$

Proof. Starting from the definition of $\mathrm{A}TT_{c_2}$:

$$\mathrm{ATT}_{c_2} = \mathbb{E}\left(Y^1_{ic_2,\tau+1}\right) - \mathbb{E}\left(Y^0_{ic_2,\tau+1}\right) \tag{2}$$

we cannot directly observe the counterfactual outcome $\mathbb{E}\left(Y^0_{ic_2,\tau+1}\right)$. However, under ASS.2, we can express this unobserved expectation in terms of observable quantities. Specifically, ASS.2 states:

$$\mathbb{E}\left(Y^0_{ic_2,\tau+1} - Y^0_{ic_2,\tau-4}\right) = \mathbb{E}\left(Y^0_{jc_1,\tau} - Y^0_{jc_1,\tau-5}\right). \tag{3}$$

Rewriting Equation (3), we solve for the unobserved counterfactual:

$$\mathbb{E}\left(Y^0_{ic_2,\tau+1}\right) = \mathbb{E}\left(Y^0_{ic_2,\tau-4}\right) + \left[\mathbb{E}\left(Y^0_{jc_1,\tau}\right) - \mathbb{E}\left(Y^0_{jc_1,\tau-5}\right)\right]. \tag{4}$$

Substituting Equation (4) back into Equation (2), we obtain:

$$\begin{aligned} ATT_{c_2} &= \mathbb{E}\left(Y^1_{ic_2,\tau+1}\right) - \left\{\mathbb{E}\left(Y^0_{ic_2,\tau-4}\right) + \left[\mathbb{E}\left(Y^0_{jc_1,\tau}\right) - \mathbb{E}\left(Y^0_{jc_1,\tau-5}\right)\right]\right\} \\ &= \left[\mathbb{E}\left(Y^1_{ic_2,\tau+1}\right) - \mathbb{E}\left(Y^0_{ic_2,\tau-4}\right)\right] - \left[\mathbb{E}\left(Y^0_{jc_1,\tau}\right) - \mathbb{E}\left(Y^0_{jc_1,\tau-5}\right)\right] \end{aligned}$$

thus aligning with Equation (1) and completing the proof. □

Equation (1) represents a novel design for retrieving the ATT, that we label the *time-shifted control design* (*TSCD*). *TSCD* compares treated cohorts of municipalities with similar ones that are not observed in the treatment status because of the shifted observational time τ of the elections. As the pre/post-treatment periods are shifted between the treated and control groups, we cannot control for unexpected deviations from the trend in the shifted period (hence the need for ASS.1), but we can account for changes that follow the trend of previous years, under ASS.2.[19]

In many contexts, these assumptions are quite stringent. We contend that this design ought to be employed only in scenarios where both assumptions can be credibly met. In the Appendix.2, we will delineate the primary factors to evaluate these assumptions' credibility within our specific framework.

5.1 Estimation Procedure

Since the 1993 Italian local elections, each municipality has elected its council and mayor for a standard five-year term. However, premature term endings are not rare (about 14% of cases since 2000), necessitating new elections to be conducted at the earliest available opportunity.[20] In the context of our analysis, this implies that municipalities

[19] Assumption ASS.1 must hold not only over the post-treatment interval, but also over the shifted pre-reform periods used to construct counterfactual trends. In our empirical setting, treated and control municipalities are selected through a matching procedure (see Section 5.1) that conditions on both levels and trends of the outcome variables over four pre-reform (shifted) elections. While this procedure does not remove the need for Assumption ASS.1, it substantially reduces the likelihood that treated and control units differ because of systematic differential shocks or structural breaks in the shifted pre-reform periods, which would otherwise be reflected in diverging pre-treatment dynamics. Accordingly, while ASS.1 remains an identifying assumption, matching substantially reduces concerns about violations in the pre-reform shifted periods.

[20] Several factors can contribute to the curtailed tenure of local governments, including political disagreements within the majority, the passing of the mayor, or the infiltration of criminal elements

that underwent elections in 2021 share general similarities with those that did so in 2022 and 2023, and any possibility of self-selection into the treatment group is eliminated.

Despite this conceptual similarity, we shall ensure the establishment of genuine comparability by adapting the non-parametric DiD estimator for aggregate panel data proposed by Imai *et al.* (2023) to the case of shifted assignment to treatment. This methodology encompasses four distinct steps:

Step. 1 We implement a rigorous exact matching procedure, which compares each municipality with elections in 2022/2023 to those with elections in 2021, within identical geographical regions (North, Center, or South) and the same population categories ($\leq$ 3,000 inhabitants, between 3,001 and 5,000 inhabitants, between 5,001 and 10,000 inhabitants, between 10,001 and 30,000 inhabitants). Geographical matching ensures that treated and untreated municipalities share similar territorial and cultural characteristics (e.g., social capital), while population size matching accounts for the fact that mayoral wages vary across these population brackets.

Step. 2 We assign a positive weight to the three control municipalities that exhibit the closest similarity in pre-treatment values and trends across all dependent variables, as well as the other three covariates outlined in section 4, i.e., turnout, log-population, and income per capita. These control cities are selected by using the Mahalanobis distance matching. The pre-treatment data points are spaced five years apart and span the period 2001-2018, pertaining to the four pre-2021 municipal elections. Figure 2 demonstrates a high degree of covariate balancing between treated and matched control observations. Each line reports the standardized mean difference between treated and control municipalities with respect to each variable. It clearly emerges that the level of imbalance

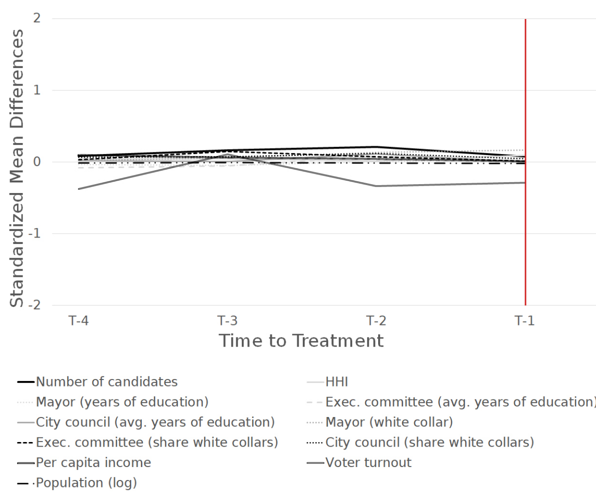
into the administration (Cerqua and Zampollo, 2022). For these reasons, the percentage of premature elections may vary from year to year without following any particular pattern. However, as explained in Appendix.1, we have excluded from the empirical analysis any municipalities that experienced early elections (less than five years) between the last and the second-to-last elections.

remains stable across the 4 pre-treatment data points and fully within the (-1, 1) range of the standard deviation. Treated and control municipalities exhibited very similar and balanced pre-reform trends.

Step. 3 We estimate the counterfactual outcome for each treated municipality based on the average of the three matched control units.

Step. 4 We employ the *TSCD* estimator to compute the treatment effect for each treated observation, subsequently averaging these effects across all treated observations to obtain the Average Treatment Effect on the Treated (ATT).

Figure 2: Covariate Balancing over the Pre-Treatment Time Period



Notes: This figure illustrates the covariate balancing between treated and matched control municipalities. Each line in the graph represents a different dependent variable, along with three covariates: turnout, log-population, and income per capita. The plot depicts the standardized mean difference between treated and control municipalities over the four pre-treatment data points reported on the horizontal axis. Pre-treatment data points are spaced five years apart, comparing shifted cohorts of municipalities over the period 2001-2018. For instance, at $T-1$, towns that held elections in 2017/2018 are compared to those that held elections in 2016, and so forth.

Formally, considering the cohort of towns treated in 2022 as an example, the ATT is computed as follows:

$$ATT_{2022} = \frac{1}{\sum_{i=1}^{N} D_i} \sum_{i=1}^{N} D_i \left\{ (Y_{i,2022} - Y_{i,2017}) - \sum_{i' \in M_i} \omega_i^{i'} \left(Y_{i',2021} - Y_{i',2016}\right) \right\} \quad (5)$$

In Equation (5), D_i is the treatment dummy; M_i denotes the matched set (i.e., the set of control units that share the same geographical area and population category as the treated unit, i); and $\omega_i^{i'}$ denotes the weight from Mahalanobis distance matching: each of the three control units most similar to unit i in the four-lag pre-treatment trends of all dependent and control variables receives a weight of 1/3, and all other control units receive a weight of zero.

6 Results

6.1 Reform Overall Effects

Table 3 presents estimates for all municipalities where we report the results with respect to competition and quality proxies. In Panel A, we report the results for the mayor, while, in Panel B, those related to the members of the executive committee and the city council.[21]

The estimates in both panels suggest that the reform did not result in any statistically significant effect on these outcomes.[22] We present results separately for mayors, executive committee members, and councilors because the incentives and mechanisms shaping each role differ. Mayors face electoral competition and make entry decisions under the risk of losing office, whereas executive committee members are appointed by (and typically pre-selected by) the mayor; nevertheless, their decision to accept the appointment can also depend on the economic incentive, which is proportional to their role relative to the mayor. Councilors generally combine public service with outside work and receive comparatively modest compensation, so changes in remuneration may have a distinct interpretation for them. Several reasons may explain why, in the context

[21]Please note that in assessing the executive committee and the city council, our focus is exclusively on evaluating the quality of elected officials. This is because measures of competition are either inapplicable or unsuitable in this context. For example, there are no executive committee "candidates" as they are chosen by the mayor after the election.

[22]Table 3 reports the ATT weighted by treated-cohort size: G-2022 carries a weight of 0.575 (589/1,024) and G-2023 a weight of 0.425 (435/1,024). Section 6.4 presents ATT estimates separately for each cohort.

under analysis, no effects were observed at the aggregate level.

Table 3: Reform Overall Effects

Panel A: Mayor

	No. of Candidates (1)	No. of Novel Candidates (2)	HHI (3)	Years of Education (4)	White-collar Worker (5)
Reform Effect	-0.033	0.151	-0.013	0.094	0.022
	(0.074)	(0.145)	(0.014)	(0.175)	(0.026)
No. of Treated	1,024	1,024	1,024	1,024	1,024
No. of Controls	895	895	895	895	895
Post-reform statistics for the treated group:					
Mean	2.466	1.057	0.592	15.175	0.299
SD	1.128	1.302	0.207	3.110	0.458

Panel B: Executive Committee & City Council

	Executive Committee		City Council	
	Average Years of Education (1)	Share of White-collar Workers (2)	Average Years of Education (3)	Share of White-collar Workers (4)
Reform Effect	-0.058	0.017	-0.024	-0.001
	(0.15)	(0.015)	(0.063)	(0.008)
No. of Treated	1,024	990	1,024	1,004
No. of Controls	895	891	895	895
Post-reform statistics for the treated group:				
Mean	14.018	0.163	13.702	0.134
SD	2.488	0.271	1.324	0.139

Notes: This table reports *TSCD* estimates on electoral outcomes related to the Mayor (Panel A), Executive Committee (Panel B, columns 1 and 2), and City Council (Panel B, columns 3 and 4), comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Ideally, with respect to the mayor, we can categorize post-reform mayoral candidates into two groups: "always-candidates" (ACs), who would run for the election regardless of the pay rise, and "comply-candidates" (CCs), who would run only because of the pay rise. The absence of effects on post-reform political competition can arguably be interpreted as a lack of CCs. In this regard, it is crucial to acknowledge that the motivation to comply varies between contexts and potential candidates.

Firstly, if the barriers to entry politics are too high, the incentives resulting from a salary hike might prove insufficient to surpass the pivotal point at which an individual opts to run. Consequently, given that we can only observe the action of running for the election, the inference that can be drawn is that the impact of the salary increase may be present but too subtle to discern in the available data. Secondly, it is vital to evaluate the incentive for compliance in connection with alternative career opportunities in the market (e.g., wages in non-political careers) that might depend on the local labor market context.[23] Finally, given the relative short time between the implementation of the reform and the election we are observing (from 6 to 17 months), it might be more challenging for CCs to enter the political market and participate in the election having only a few months to mature this decision.

To examine these three factors, we evaluate the effect of the reform with respect to: *i*) the distinct levels of barrier to entry in politics, specifically: the potential presence of an incumbent candidate and the degree of competition in previous elections; *ii*) the variety of the external opportunities by estimating treatment effects within various ranges of the local market wages; *iii)* the distance between the introduction of the policy and the election date.

[23] To clarify, when considering a fixed political career wage, a prospective candidate residing in an area with low market wages will display a stronger motivation to comply in comparison to a prospective candidate residing in an area with higher market wages.

6.2 Heterogeneity by Entry Barriers

Evaluating the effects of the policy in contexts with different entry barrier levels is particularly relevant for two main reasons. First, if the barriers are too high, even a significant increase in wages might not be enough to convince new candidates. Second, increasing politicians' wages could heighten the incumbents' willingness to seek re-election, potentially dissuading new challengers (Mattozzi and Merlo, 2008). From this perspective, a pay rise might even have the opposite effect, attracting fewer candidates. To assess these arguments, we examine whether the effects vary in environments where the incumbent is ineligible for re-election because of term limits: i.e., open seat elections.[24] In Table 4, Panel A displays the effect of the reform in municipalities without open seat elections, while Panel B focuses on cities with open seat elections, and Panel C on municipalities with at most two candidates in previous elections.

In Panel A, we find that the reform had no effects in municipalities without open seats. The results are not statistically significant and close to zero. On the contrary, in Panel B, we observe that the reform caused a significant surge in both the number of mayoral candidates and novel contenders in open seat elections. Compared to an average of 2.676, treated municipalities saw an increase in the number of candidates of 0.275 (that is an increase above 10% relative to the sample mean). The number of rookie candidates increases by 0.52 which is a 37% rise relative to the sample mean of treated after treatment. In terms of quality, our analysis reveals no statistically significant differences, regardless of the level of entry barriers. These results indicate that a salary increase can be effective in attracting new candidates in environments where the obstacles to entering politics are relatively low. Conversely, the flip side of the coin is that in settings where entry barriers are high, even a substantial wage increase may not suffice to influence the election's outcome.

[24] As part of our analysis on entry barriers to local politics, we re-ran the empirical analysis after splitting the sample between municipalities close to those with a local government ever dissolved due to mafia infiltration (not included in the main analysis, see Appendix.1) and those without such proximity. The analysis revealed no significant differences. Detailed results are available upon request.

Table 4: Reform Effects in Contexts with Different Entry Barriers

Panel A: Municipalities Without Open Seats					
Mayor	No. of Candidates (1)	No. of Novel Candidates (2)	HHI (3)	Years of Education (4)	White-collar Worker (5)
Reform Effect	-0.111	0.064	0.002	0.055	0.032
	(0.073)	(0.139)	(0.016)	(0.172)	(0.023)
No. of Treated	817	817	817	817	817
No. of Controls	719	719	719	719	719
Post-reform statistics for the treated group:					
Mean	2.412	1.009	0.605	15.129	0.307
SD	1.129	1.290	0.208	3.120	0.462
Panel B: Municipalities With Open Seats					
Reform Effect	0.275**	0.494**	-0.076***	0.248	-0.016
	(0.126)	(0.245)	(0.022)	(0.363)	(0.054)
No. of Treated	207	207	207	207	207
No. of Controls	176	176	176	176	176
Post-reform statistics for the treated group:					
Mean	2.676	1.246	0.538	15.357	0.266
SD	1.100	1.334	0.196	3.067	0.443
Panel C: Municipalities With at Most Two Candidates in Previous Elections					
Reform Effect	0.331***	0.487***	-0.049***	0.054	0.037
	(0.060)	(0.138)	(0.018)	(0.217)	(0.03)
No. of Treated	529	529	529	529	529
No. of Controls	499	499	499	499	499
Post-reform statistics for the treated group:					
Mean	2.089	0.807	0.645	15.023	0.274
SD	0.794	1.021	0.213	3.191	0.446

Notes: This table reports *TSCD* estimates on electoral outcomes related to the Mayor. Panel A illustrates the impact of the reform in municipalities where incumbents were eligible for re-election, while Panel B focuses on cities with open seat elections. Panel C shows result for municipalities having at most two mayoral candidates in the previous election. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Lastly, in Panel C of Table 4, we demonstrate that in municipalities that did not attract more than two mayoral candidates in the previous election, the impact on competition is both statistically significant and substantial in terms of magnitude. This effect seems to be driven by the increased number of rookies whereas the reform did not raise the probability of incumbents seeking re-election as it is shown in Table 5.

Based on these findings, the reform has proven effective in increasing competition in contexts where the discouragement stemming from incumbent presence was absent and where the appeal of holding the office was comparatively lower.[25]

The reform also had heterogeneous effects on female political participation depending on the electoral context. As shown in Table 6, the probability of a woman being elected mayor increases significantly by 13.7 percentage points in municipalities with open seats (Panel B). This effect is even more pronounced in municipalities that previously had low levels of competition (Panel C), where the number of female candidates and novel female candidates rises significantly, alongside a modest but statistically significant increase in the probability of electing a female mayor.

Table 5: Incumbent Mayors

	Incumbent Running Again (1)	Incumbent Winning Again (2)
Reform Effect	0.031	0.017
	(0.048)	(0.048)
No. of Treated	817	817
No. of Controls	719	719
Post-reform statistics for the treated group:		
Mean	0.802	0.639
SD	0.399	0.481

Notes: This table reports *TSCD* estimates on the probability of incumbent mayors to run again (column 1) and to win again (column 2). Only elections where the incumbent was eligible for re-election are considered. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

[25] We also investigated if these “different” effects hold on the quality of the members of the executive committee and the city council. However, we did not find significant differences. Results are available upon request.

Table 6: Reform Effects in Contexts with Different Entry Barriers - Female

Mayor	No. of Female Candidates (1)	No. of Female Novel Candidates (2)	Female Mayor (3)
Panel A: Municipalities Without Open Seats			
Reform Effect	-0.059	0.022	0.004
	(0.047)	(0.053)	(0.026)
Number of Treated	817	817	817
Number of Controls	719	719	719
Post-reform statistics for the treated group:			
Mean	0.334	0.192	0.130
SD	0.566	0.515	0.336
Panel B: Municipalities With Open Seats			
Reform Effect	0.042	0,0135	0.137***
	(0.093)	(0.095)	(0.050)
Number of Treated	207	207	207
Number of Controls	176	176	176
Post-reform statistics for the treated group:			
Mean	0.449	0.256	0.242
SD	0.658	0.589	0.429
Panel C: Municipalities With at Most Two Candidates in Previous Elections			
Reform Effect	0.097**	0.141***	0.051*
	(0.049)	(0.053)	(0.029)
Number of Treated	529	529	529
Number of Controls	499	499	499
Post-reform statistics for the treated group:			
Mean	0.308	0.163	0.153
SD	0.520	0.456	0.360

Notes: This table reports *TSCD* estimates on electoral outcomes related to the Mayor. Panel A illustrates the impact of the reform in municipalities where incumbents were eligible for re-election, while Panel B focuses on cities with open seat elections. Panel C shows result for municipalities having at most two mayoral candidates in the previous election. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.3 Heterogeneity by External Opportunities

In this section, we evaluate the effects of the reform across contexts with varying external opportunities, which we proxy using quartiles of the average municipal income distribution.[26] Accordingly, the first quartile includes the poorest municipalities, while the fourth comprises the richest. This taxonomy allows us to assess how the reform's effects vary with the availability of outside labor market options.

Table 7 reports summary statistics for treated municipalities after the reform, disaggregated by income quartile. While most indicators—such as the number of candidates, novel candidates, and average years of education—are broadly similar across groups, some clear patterns emerge. In the lowest income quartile (Panel 1), mayors tend to have slightly higher education levels and a greater incidence of white-collar backgrounds compared to middle-income municipalities, although the city council tends to be less qualified overall. In contrast, the highest-income municipalities (Panel 4) display the lowest vote concentration (HHI) and relatively higher educational and professional levels among local politicians, especially in the city council.

Figure 3 summarizes these heterogeneous effects graphically. Although not all estimates are statistically significant, several notable patterns emerge.[27] First, the reform increases the number of candidates significantly (at the 10% level) only in the poorest municipalities, while the effect is negative in all other groups (Panel A, top-left). A similar pattern holds for the number of novel candidates (Panel A, top-right). Second, the effect on the quality of mayoral candidates is negative in low-income municipalities but becomes increasingly positive in richer areas (Panel A, bottom). Finally, we find a significant positive effect on the share of executive committee members with white-collar backgrounds in poorer municipalities (Panel B, right). For city councils, the reform's effect on average education levels increases progressively from the lowest to the highest income quartile (Panel C, left).

[26] Although we lack systematic data on candidates' residences, a manual check of a sample revealed that over 90% live in or near the municipality in which they run.

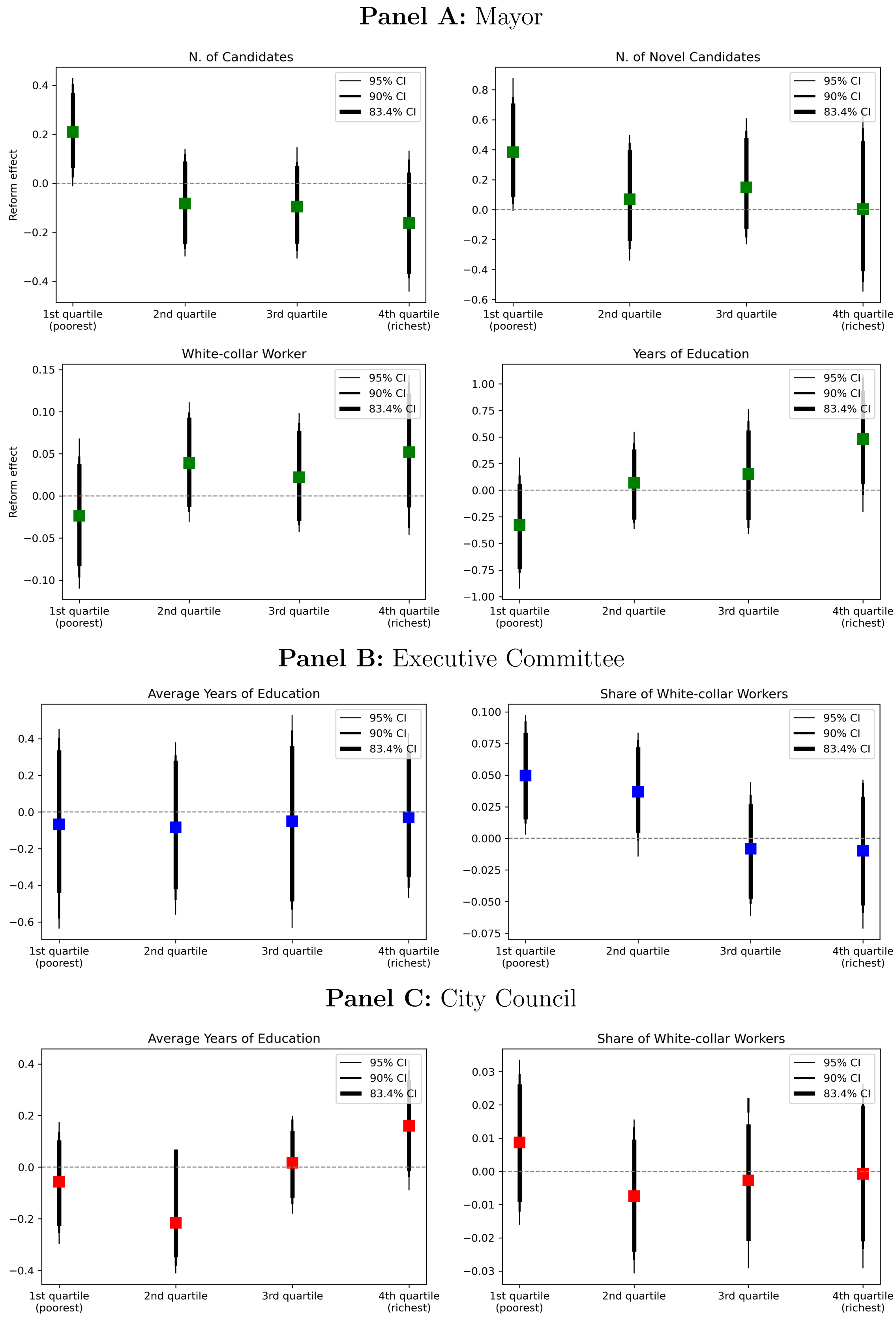
[27] When visually comparing two independent estimates, overlap of 95% confidence intervals does not correspond to a 5% test of equality, but instead reflects a substantially more conservative criterion. Under standard assumptions (see Knol *et al.* (2011)), using 83.4% confidence intervals provides a graphical comparison equivalent to a two-sided hypothesis test at the 5% significance level: non-overlapping 83.4% intervals indicate statistical significance at conventional levels. We adopt this convention to facilitate direct visual inference when contrasting estimates; for completeness, we also report the 90% and 95% confidence intervals in the figure.

Table 7: Summary Statistics - By Income Quartiles

Variable	**Mean**	**(Std. Dev.)**	N
Panel 1: Municipal Income Quartile 1			
Mayor:			
N. of Candidates	2.551	(1.282)	256
N. of Novel Candidates	1.195	(1.560)	256
HHI	0.604	(0.191)	256
White Collar Worker	0.336	(0.473)	256
Years of Education	15.473	(2.961)	256
Executive Committee:			
Share White Collar Workers	0.156	(0.304)	243
Avg. Years of Education	13.758	(2.595)	256
City Council:			
Share White Collar Workers	0.12	(0.137)	246
Avg. Years of Education	13.609	(1.257)	256
Panel 2: Municipal Income Quartile 2			
Mayor:			
N. of Candidates	2.449	(1.13)	256
N. of Novel Candidates	1.027	(1.330)	256
HHI	0.595	(0.203)	256
White Collar Worker	0.309	(0.463)	256
Years of Education	15.094	(3.019)	256
Executive Committee:			
share White Collar Workers	0.16	(0.273)	248
Avg. Years of Education	14.101	(2.583)	256
City Council:			
Share White Collar Workers	0.128	(0.149)	250
Avg. Years of Education	13.652	(1.404)	256
Panel 3: Municipal Income Quartile 3			
Mayor:			
N. of Candidates	2.289	(0.975)	256
N. of Novel Candidates	0.992	(1.148)	256
HHI	0.621	(0.221)	256
White Collar Worker	0.254	(0.436)	256
Years of Education	14.852	(3.271)	256
Executive Committee:			
Share White Collar Workers	0.142	(0.239)	249
Avg. Years of Education	13.88	(2.524)	256
City Council:			
share White Collar Workers	0.125	(0.134)	255
Avg. Years of Education	13.554	(1.358)	256
Panel 4: Municipal Income Quartile 4			
Mayor:			
N. of Candidates	2.574	(1.086)	256
N. of Novel Candidates	1.012	(1.222)	256
HHI	0.547	(0.206)	256
White Collar Worker	0.297	(0.458)	256
Years of Education	15.281	(3.162)	256
Executive Committee:			
Share White Collar Workers	0.192	(0.265)	250
Avg. Years of Education	14.334	(2.204)	256
City Council:			
Share White Collar Workers	0.164	(0.133)	253
Avg. Years of Education	13.994	(1.231)	256

Notes: This table presents summary statistics of outcome variables for treated municipalities in post-reform elections, segmented into four panels that correspond to municipalities' income quartiles.

Figure 3: Reform Effects by Income Quartiles



Notes: This figure shows *TSCD* estimates on split-samples by municipality's income quartiles. Panel A reports results on outcome variables measured for mayors, Panel B relates to the executive committee, and Panel C to the city council. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Coefficients are reported on the vertical axes with 83.4, 90, and 95% confidence intervals obtained via block-bootstrapping standard errors.

This evidence indicates that while the political pay rise did not enhance the quality of local politicians in medium to high-income municipalities, it did attract individuals with less education but experience in white-collar positions in mid to low-income areas.

Combining these results, it is evident that in the poorest areas, the reform drew more candidates and altered the quality mix of the political class. It reduced the average education level of city council members and, to a lesser extent, the mayors while increasing the presence of white-collar professionals in the executive committee.

Conversely, in wealthier areas, the reform did not produce significant enhancements, although there was a slight decline in the number of candidates and a rise in their average quality. The economic rationale behind these effects aligns with models proposed by Messner and Polborn (2004) and Mattozzi and Merlo (2008). Specifically, the salary increase may have appealed more to individuals with limited prospects in other professions or lacking the typical skills and abilities sought in political leadership. Consequently, the rise in compensation might not have attracted the most qualified candidates but rather those primarily enticed by financial incentives, potentially resulting in a negative selection effect.

6.4 Heterogeneity by Election Time to Reform

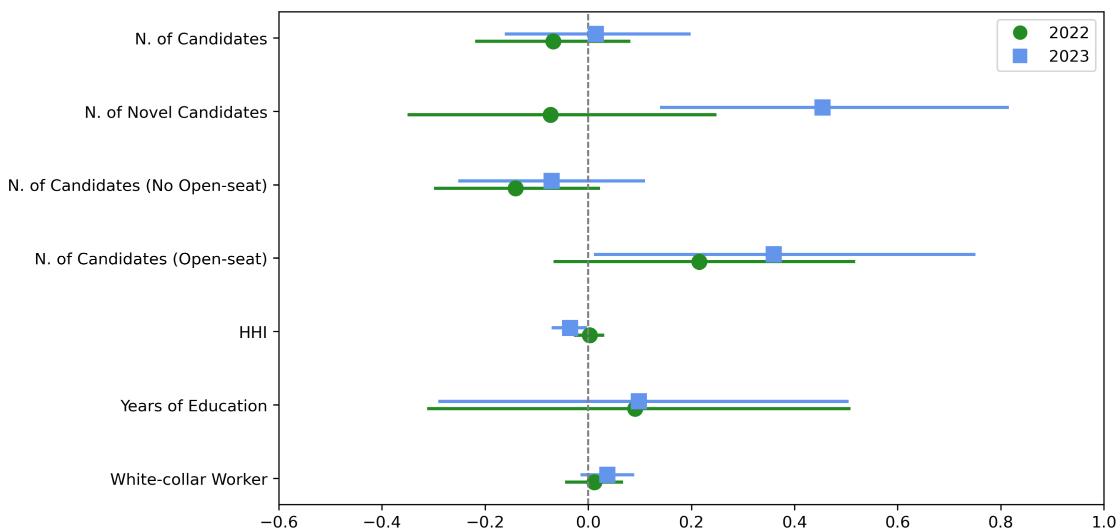
In this policy evaluation, given that only 6 months passed between the adoption of the reform and the first post-reform election, it might be that CCs had not enough time to respond to the reform. This could have happened for a number of reasons, for example: i) the time needed to spread the news of the reform (the reform did not garner widespread media attention); ii) the time required to prepare the candidacy.

In Figure 4, we evaluate the effects of the policy on the separate treatment groups of municipalities belonging to the 2022 or 2023 cohorts, respectively.[28] The effects in 2023 seem to be more pronounced, possibly due to CCs having a relatively limited

[28]We also implemented this analysis on the quality of the members of the executive committee and the city council. However, we did not find significant differences. Results are available upon request.

time-frame to respond to the reform in 2022. In support of this thesis, we observe that in 2023 the reform sharply increased the number of novel candidates–those who had never held a political office–compared to the elections in 2022, and who are likely less informed than experienced politicians.

Figure 4: Reform Effects in 2022 and 2023



Notes: This figure shows *TSCD* estimates on reform's effects in 2022 and 2023, separately. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control). For each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize the Mahalanobis distance criterion to identify the three untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Outcome variables are reported on the vertical axes, while standardized coefficients with 95% confidence intervals obtained via block-bootstrapping standard errors.

7 Conclusions

Across Europe and other advanced democracies such as the United Kingdom, Italy, and the United States, a growing number of local governments are struggling to attract candidates for local office. This shortage of political supply raises concerns not only about the health of local democracy, but also about broader governance capacity and representation at higher institutional levels. Local governments play a key role in delivering public services and cultivating future political leaders.

This paper investigates whether higher remuneration for local politicians can revitalize political participation and improve the quality of elected officials. Drawing on a large-scale natural experiment in Italy, we find that financial incentives matter, but their effects vary across contexts. The reform increased candidate entry, especially among newcomers and in areas with weaker labor markets or few mayoral contenders. However, the effects on candidate quality are mixed: while more candidates are attracted, the average qualifications of elected officials do not necessarily improve. These findings challenge the assumption that higher pay automatically yields better-quality governance and suggest that the broader socio-economic environment mediates the effectiveness of such policies.

These insights point to the limits of one-size-fits-all solutions in addressing the political recruitment crisis. While financial incentives can lower barriers to entry, especially in underserved areas, they are unlikely to foster better governance outcomes unless accompanied by broader institutional and civic support structures. Future research should explore how monetary and non-monetary levers interact in shaping both the quantity and quality of political engagement.

One limitation of our analysis is that it focuses solely on entry decisions and does not examine how enhanced remuneration shapes the behavior, decision-making, or governance practices of local politicians once in office. Future research should therefore assess whether—and through which mechanisms—higher pay translates into improved policy outcomes, administrative effectiveness, and accountability at the local government level, and to what extent these channels are shaped by *novel* politicians.

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Appendix

Appendix.1 Description of the Sample of Analysis

In the analysis, we only consider municipalities belonging to the regions with ordinary statute and having at most 30,000 inhabitants. The municipal elections of 2021 were held on October 3, 2021 in 1,109 municipalities having these characteristics, while those in the post-reform periods were held on June 12, 2022 (May 14, 2023) in 703 (557) municipalities having these characteristics. Therefore, the starting point was to consider 1,260 municipalities as treated and 1,109 municipalities as controls. However, before running the analysis we removed some of these municipalities due to the following reasons:

i. municipalities that changed their administrative boundaries (e.g., no mergers) in the period under analysis (2001-2023);

ii. municipalities with multiple elections in the post-COVID period;

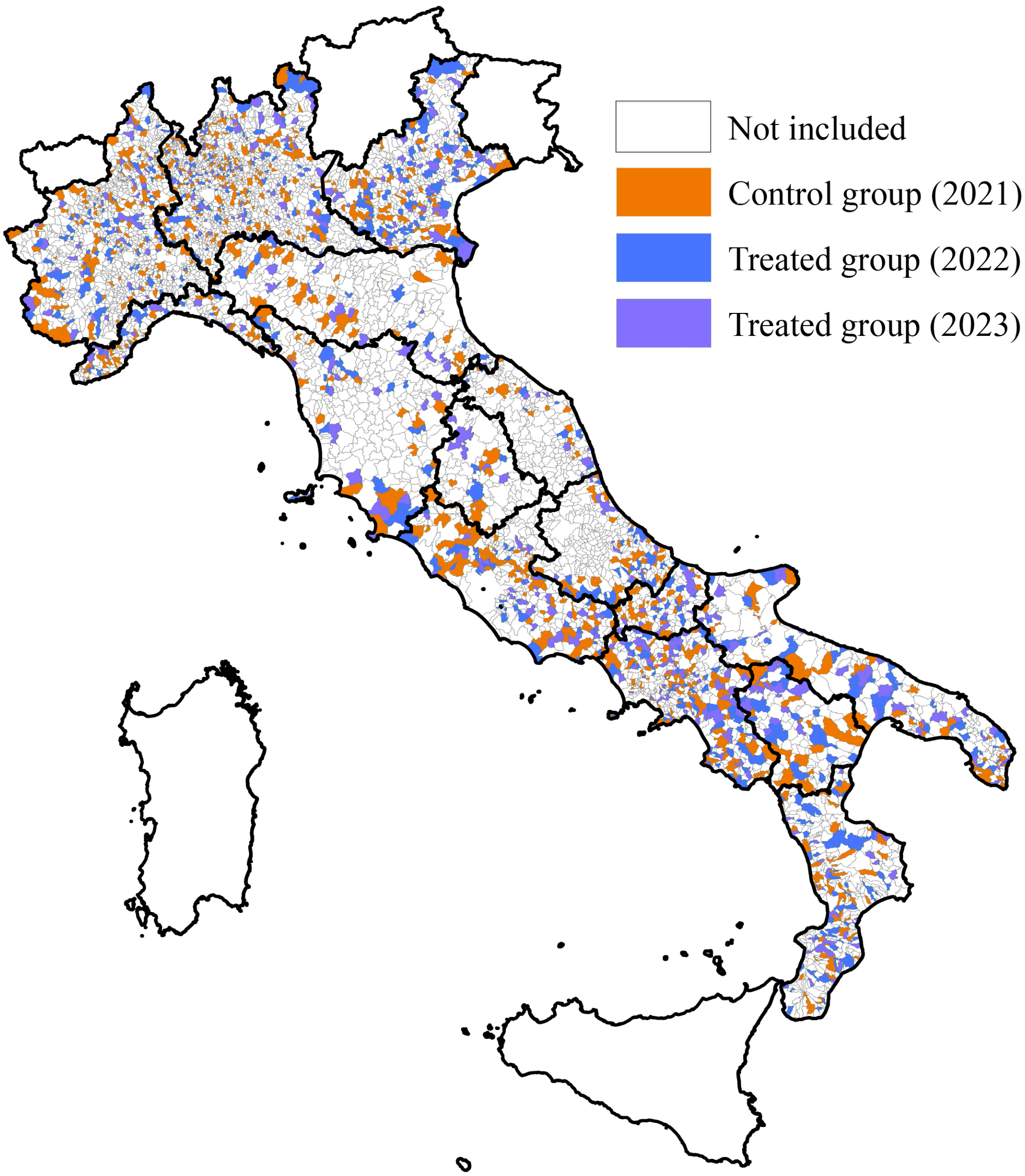
iii. municipalities with local governments dissolved due to mafia infiltration;

iv. municipalities severely hit by at least one of the three destructive earthquakes occurred in Italy in the period under analysis (2001-2023). In particular, L'Aquila earthquake in 2009, Emilia earthquake in 2012, and Center Italy earthquakes in 2016;

v. municipalities with early elections (less than five years) between the last and the second to last elections.

After this cleaning process, we were left with 1,919 municipalities, 1,024 of which make up the treated group (589 in 2022 and 435 in 2023) and 895 make up the control group. The geographic distribution of this sample is reported in Figure A.1.

Figure A.1: Treated and Control Municipalities Included in the Analysis



Notes: This figure reports Italian municipalities included or not in the empirical analysis and treatment status. Municipalities belonging to special status regions are excluded from the analysis.

Appendix.2 Validity of the Assumptions

Regarding ASS.1, the initial step involves examining the Italian political landscape to assess potential alternative policies or exogenous shocks that may have influenced the outcomes of interest between 2021 and 2022/2023. To the best of our knowledge, there were no other reforms that changed the structure, the remuneration, or the incentives of local politics (e.g., no changes in the accountability rules).[29] Consequently, we compare cohorts c_2 and c_3, respectively treated in 2022 and 2023, with cohort c_1 of untreated municipalities that held elections in 2021.

We recognize that the validity of this assumption diminishes with an increasing time gap between the elections of treated and untreated cohorts. This is because other unforeseen economic shocks could occur over time, which could influence the decision to enter local politics. Thus, the *TSCD* requires careful analysis of the context and of any significant omitted factors before being implemented.

A notable event after the reform was the Russian invasion of Ukraine, triggering an energy crisis and fostering inflation. However, the energy crisis did not cause strong repercussions in the Italian labor market in the first place (Etica Economica, 2023) and we deem it unlikely that this event could have significantly affected the incentives to run for local government positions, especially in the small- and medium-sized municipalities that make up the population of interest for our analysis.

Regarding ASS.2, while the policy shift had been under consideration for several years, its magnitude and timing were highly uncertain. This implies that both incumbent mayors and prospective candidates could not have readily anticipated such a salary increase. Moreover, municipalities cannot self-select the year of the election: hundreds or thousands of municipalities conduct local elections every year in Italy. The

[29] The only relevant policy change concerning the validity of this assumption occurred in April 2022 with the introduction of the Law 35/2022, which gave the possibility to the incumbent to run for three consecutive terms in municipalities up to 5,000 inhabitants (a possibility that was already warranted in municipalities up to 3,000 inhabitants). In section Appendix.3, we show that results are robust when dropping from the sample all municipalities affected by this policy change.

key aspect to take into account is that over the last seven decades, Italian municipal elections have been dispersed temporally, and each municipality schedules its municipal elections at five-year intervals. Indeed, although all municipalities initially held elections in 1946, subsequent elections frequently diverged from the five-year cycle because of premature council dissolutions. These dissolutions, triggered by factors related or unrelated to administrative quality (e.g., political contrasts in the majority or the death of the mayor), introduce significant unpredictability into the current composition of cohorts. Consequently, the allocation of municipalities into the five cohorts resembles a random assignment. Rather than analyzing all cohorts, in the empirical analysis we will consider only the three cohorts that held elections in the post-COVID era. The rationale is that the arrival of COVID-19 in 2020 delayed the 2020 municipal elections and could have modified the voting preferences of the citizens and the incentives to enter local politics (Picchio and Santolini, 2022; Bordignon *et al.*, 2023). The particulars of this scenario are delineated in Table A.1. As the policy was adopted at the close of 2021, the 2021 cohort (c_1) remained untouched by the treatment, whereas the 2022 (c_2) and 2023 (c_3) cohorts were subjected to it.[30]

Table A.1: Treated and Control Cohorts based on the Timing of the Elections

...	$Y^0_{c_1,2016}$			$Y^0_{c_1,2021}$		
...		$\mathbf{Y^0_{c_2,2017}}$			$\mathbf{Y^1_{c_2,2022}}$	
...			$\mathbf{Y^0}_{\mathbf{c_3,2018}}$			$\mathbf{Y^1_{c_3,2023}}$

Notes: This table shows how the timing of the elections relative to the enactment of the reform defines treated and control cohorts. c_1, c_2, and c_3 are three different cohorts of municipalities. Each cohort held election each five years, but at different points in time. For instance, c_1 had election in 2016 and 2021, c_2 in 2017 and 2022, and c_3 in 2018 and 2023. The policy was adopted at the close of 2021; therefore, the 2021 cohort (c_1) was not affected by the treatment, whereas the 2022 (c_2) and 2023 (c_3) cohorts were subjected to it. The cells shaded in grey denote the onset of the salary increase, while outcomes of treated cohorts pre/post-reform are reported in bold.

These characteristics ensure that there exists no substantive reason for municipalities

[30] It is important to emphasize again that *TSCD* does not require the outcomes of treated and control municipalities to be identical in absolute terms; it provides unbiased estimates as long as the outcome trends of c_2 and c_3 would have been similar to that of c_1 in the absence of the policy change.

conducting elections in 2022/2023 to inherently differ from those holding elections in 2021. To further address potential concerns regarding ASS.2, we adopt a modified version of the non-parametric DiD estimator for aggregate panel data proposed by Imai *et al.* (2023) to guarantee the comparison of treated municipalities with untreated counterparts within the same geographical area and falling within the same population size bracket. Moreover, only control municipalities exhibiting the closest similarity in pre-treatment values and trends across all dependent variables, as well as the other covariates outlined in the data section will receive a positive weight. We carefully describe the estimation procedure in section 5.1 and we propose some robustness checks and placebo analyses in section Appendix.3.

Appendix.3 Robustness Checks and Placebo Analyses

We conducted an extensive set of robustness checks (RC hereafter) to test the sensitivity of our primary results, with key findings summarized in Table A.2.[31]

First, we varied the dimension of the matched set, using 2 and 5 neighbors instead of the 3 we used in the main analysis (RC1 and RC2). Additionally, we examined the robustness of our primary analysis by employing different weighting and matching techniques to refine our control unit selection. This included the covariate balancing inverse propensity score weighting method proposed by Imai and Ratkovic (2014) and propensity score matching (PSM) as in Rosenbaum and Rubin (1983) (RC3 and RC4). We have also replicated the analysis without adopting any matching or rebalancing procedure (RC5) to assess the impact of matching and rebalancing on the main estimates. We have then assessed the potential influence of Law 35/2022, which allows incumbents in municipalities with up to 5,000 inhabitants to seek three consecutive terms—a provision already in place for municipalities up to 3,000 inhabitants. To address this, in RC6, we excluded all municipalities affected by this policy change from our sample.

[31] For the sake of simplicity, we present the robustness check analyses and the placebo analysis for the outcome measuring the number of candidates. Additional analyses on other outcomes are available upon request to the authors.

Table A.2: Robustness Checks and Placebo Analysis - No. of Candidates

	Overall	No Open Seats	Open Seats	Max 2 Candidates in previous Elections	Municipal Income Quartile 1	Elections 2022	Elections 2023
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Basiline Estimates:							
Reform Effect	-0.033	-0.111	0.275**	0.331***	0.210*	-0.068	0.015
	(0.074)	(0.073)	(0.126)	(0.060)	(0.115)	(0.078)	(0.093)
Rob. Check 1: Mahalanobis with 2 Neighbors							
Reform Effect	-0.041	-0.126	0.297**	0.327***	0.217*	-0.068	-0.003
	(0.076)	(0.077)	(0.138)	(0.068)	(0.120)	(0.082)	(0.097)
Rob. Check 2: Mahalanobis with 5 Neighbors							
Reform Effect	-0.036	-0.096	0.198*	0.329***	0.202*	-0.080	0.023
	(0.070)	(0.072)	(0.117)	(0.060)	(0.110)	(0.075)	(0.088)
Rob. Check 3: Covariate Balancing Inverse Probability Weighting							
Reform Effect	0.025	-0.066	0.381**	0.447***	0.232**	-0.024	0.179
	(0.107)	(0.101)	(0.177)	(0.088)	(0.118)	(0.089)	(0.161)
Rob. Check 4: Propensity Score Matching with 3 Neighbors							
Reform Effect	0.121	0.033	0.585**	0.500***	0.189*	0.057	0.334*
	(0.131)	(0.113)	(0.244)	(0.083)	(0.114)	(0.111)	(0.194)
Rob. Check 5: No Matching							
Reform Effect	-0.007	-0.054	0.177*	0.152***	0.128	-0.044	0.043
	(0.066)	(0.069)	(0.104)	(0.056)	(0.100)	(0.073)	(0.092)
Rob. Check 6: Removing Municipalities according to the Law 35/2022							
Reform Effect	-0.045	-0.136*	0.297**	0.321***	0.231**	-0.077	-0.001
	(0.077)	(0.078)	(0.142)	(0.065)	(0.117)	(0.080)	(0.099)
Rob. Check 7: Removing Municipalities according to the Law 124/2019							
Reform Effect	-0.032	-0.148	0.345**	0.504***	0.304*	-0.054	0.002
	(0.094)	(0.095)	(0.152)	(0.090)	(0.159)	(0.104)	(0.122)
Placebo Analysis: Fake Treatment in the Pre-reform Elections							
Reform Effect	-0.049	-0.059	0.021	-0.049	-0.018	-0.043	-0.057
	(0.068)	(0.071)	(0.146)	(0.089)	(0.117)	(0.084)	(0.086)

Notes: This table reports *TSCD* estimates on the number of candidates. Each column refers to a specific restriction of the estimation sample, and each row refers to a specific robustness check analysis, as detailed in Appendix.3. In robustness check number 6, we exclude municipalities impacted by Law 35/2022, which ruled out the possibility of a third mandate for municipalities up to 5,000 inhabitants, from the estimation sample. In robustness check number 7, we remove municipalities with up to 3,000 inhabitants as in 2019 they experienced an increase of about +15% in the mayor's salary (Law 124/2019). The estimates at the bottom of the table pertain to the placebo analysis. Estimates are obtained by comparing elections that occurred in 2022 and 2023 (treated) to those that occurred in 2021 (control), except for columns (6) and (7), which analyze treated cohorts in 2022 and 2023 separately, respectively. In all analyses except for RC5, for each treated municipality, we initially generate a matched set comprising only untreated municipalities that are within the same geographical region and population bracket. We then utilize a matching or rebalancing criterion to identify the untreated cities that exhibit the most similar pre-2021 trends in relation to: the number of candidates, the Herfindahl-Hirschman Index (HHI), years of education and white-collar status (for mayors, executive committees, and city council members), voter turnout, the logarithm of population size, and per capita income. Finally, we calculate the individual treatment effect for each treated municipality and aggregate these effects to derive the ATT. Section 5.1 and Appendix.1 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Furthermore, RC7 reports the estimates after removing municipalities with up to 3,000 inhabitants as in 2019 they experienced an increase of about +15% in the mayor's salary (Law 124/2019). The results from these robustness checks consistently align with those of our main analysis, reinforcing the reliability of our empirical analysis.

Finally, we conducted a placebo test by backdating the treatment year by five years, simulating the treatment's occurrence in the pre-reform elections. As shown in the final row of Table A.2, this placebo effect was statistically non-significant across all analyses, confirming that the significant findings of our main analysis are genuinely attributable to the reform.