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Pay Incentives in Politics: Evaluating a Large-scale Salary Increase for Local Politicians*

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Abstract

We evaluate the impact of a recent reform that sharply increased the salaries of Italian local politicians on electoral competition and the valence attributes of the candidates elected. Exploiting misaligned election dates across Italian cities, we propose a novel methodology, the shifted difference-in-differences design (*Sh-DiD*), to estimate the reform's impact on municipalities up to 30,000 inhabitants, representative of almost the entire universe of Italy's local administrative units. We find a boost in the entry of new political candidates after the first post-reform electoral round, with no significant enhancement in the overall quality of the political class. These outcomes possibly stem from the varying distribution of compliers—whose candidacy decision is influenced by the reform—across diverse political and economic contexts. Thus, we find that in less affluent areas or those with fewer entry barriers, the pay rise drew a larger number of mayoral candidates, encouraging individuals from outside the political sphere to enter the competition. In the poorest contexts, we also observe a shift in the profile of councilors and members of the mayor's executive committee, where the pay rise attracted individuals with lower educational levels but with experience in white-collar positions.

Keywords: local governments, politicians' wages, shifted difference-in-differences

JEL Classification: D04, D72, J45, C13

AEA Word Count: 5,883

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1 Introduction

The international debate over the appropriate compensation for politicians is a central and dynamic issue within the realm of political economics. This debate revolves around a fundamental question: should politicians receive higher or lower remuneration for their public service? The answer to this question carries far-reaching implications, touching on critical concerns related to governance effectiveness, fiscal responsibility, and public accountability.[1]

Remuneration policies hold particular importance for local politicians from different perspectives. First, local politics might serve as the gateway to a political career at the regional and national levels (Detterbeck, 2016; Einstein *et al.*, 2020).[2] Second, compared to holding a seat in the national parliament, the non-monetary benefits of a career in local politics are significantly more limited, while the cost and the risks associated with it can be disproportionately high (Bertoni *et al.*, 2023; Daniele *et al.*, 2023; Håkansson, 2021; Pulejo and Querubín, 2023). This situation could result in a significant democratic failure, characterized by a shortage of citizens willing to run for local elections.

Thus, our study adds to this ongoing discussion by leveraging a significant reform, adopted at the end of 2021, which raised the salaries of all local politicians in Italy, offering an ideal natural experiment to evaluate how monetary incentives affect electoral outcomes: namely, the number of candidates (i.e., electoral competition) and their educational and professional background.[3] We also explore the policy's influence on

[1]In both theoretical and empirical research, some argue that higher salaries for local politicians are necessary to attract competent individuals to public service, fostering a more capable leadership (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).

[2]Prominent examples in this scenario include Matteo Renzi, the mayor of Florence, who became the Italian Prime Minister (BBC, 2014), and Boris Johnson, who transitioned from his role as the Mayor of London to become the Prime Minister of the UK (Reuters, 2019).

[3]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 also evaluate vote

these educational and professional indicators within both the executive committee, appointed by the mayor, and the city council, directly elected by the voters.[4] This is a comprehensive policy evaluation, which considers its overall impact and how the effect varies across political and economic contexts.

The examination of this reform holds particular importance in light of the growing disenchantment with politics and the declining interest in pursuing political careers.[5] 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) consistently urged government intervention to shield mayors from undue responsibility and enhance financial incentives. ANCI warned that *"...if the trend persists, there could soon be a shortage of citizens willing to take on the role of mayor"* (ANCI, 2021).[6]

At the end of 2021, the Italian government approved a new reform that significantly increased the office allowances of local politicians. This 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.[7] Importantly, the pay rise

concentration using the Herfindahl-Hirschman Index (HHI), computed by summing the squares of each mayoral candidate's share of votes.

[4]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.

[5]Recent examples of this phenomenon at an international level include, among others, Japan (Nikkei Asia, 2023); Australia, particularly in the Northern Territory (ABC News, 2021); and New Zealand, where Northland regions report a scarcity of individuals willing to stand for election (New Zealand Herald, 2022).

[6]See also the media articles (in Italian) by Corriere della Sera (2019) and la Repubblica (2021). Uncontested elections and low-income compensations for local politicians are not unique to Italy; similar issues have been observed in the UK and New Zealand. For instance, refer to UK Parliament (2019) and The Spinoff (2022).

[7]Law 234 of 30 December 2021 (budget law). Overall, the reform invests 220 million euros each

was difficult to anticipate by local candidates, as it first 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).

As the reform affected all municipalities at the same time, there are no untreated cities in post-reform elections.[8] We address this challenging empirical context by proposing the novel shifted difference-in-differences design (Sh-DiD), which estimates treatment effects by exploiting the misaligned election dates across municipalities. 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. This scenario allows for varied exposure to the reform at election time, creating distinct treatment and control groups for analysis. Therefore, Sh-DiD compares each treated municipality belonging to the cohorts holding the most recent municipal elections in 2022 or 2023 with the untreated municipalities belonging to the 2021 cohort, and having the most similar trends in terms of pre-2021 electoral outcomes. We ensure such comparison by adopting an exact-matching non-parametric extension of the DiD estimator as proposed by Imai *et al.* (2023). With the Sh-DiD approach, we can measure the direct reform's average treatment effect on the treated (ATT) as well as the conditional average treatment effects (CATEs).[9]

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

year (comma 586). 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. Appendix Table A.1 reports in more detail the change in monthly wages.

[8]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.

[9]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. In Italy, municipal elections are held each year in a different cohort of municipalities, and each municipality schedules its elections at five-year intervals (see Section 3.2 for more details).

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 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, especially those novel to the political arena.[10]

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. 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 had limited opportunities in other professions (Messner and Polborn, 2004).

This work contributes significantly to the existing body of literature that explores the influence of compensation on the competence of local politicians. When examined through a theoretical lens, the answer to this question remains inconclusive and appears to vary based on the specific context of analysis (Caselli and Morelli, 2004; Besley, 2004; Messner and Polborn, 2004; Besley, 2005; Poutvaara and Takalo, 2007; Mattozzi and Merlo, 2008; Keane and Merlo, 2010; Dal Bó and Finan, 2018; Fedele and Giannoccolo, 2020).[11] However, empirical research in this area has also yielded mixed findings (Ferraz

[10]In Appendix.4, we provide several robustness checks and we implement a placebo analysis. These estimates confirm the robustness of our findings.

[11]For instance, Caselli and Besley argue that higher salaries should attract more capable individuals to political roles, whereas Messner and Mattozzi suggest that increasing compensation might lead to unintended negative selection effects by encouraging less-qualified individuals to run for office.

and Finan, 2009; Gagliarducci and Nannicini, 2013; Dal Bó *et al.*, 2017; Pique, 2019).[12]

The paper most closely related to our study is Gagliarducci and Nannicini (2013)'s investigation into Italian municipal elections from 1993 to 2001, where they employ a sharp Regression Discontinuity Design (RDD) based on wage schemes tied to municipal population thresholds.[13] Specifically, they estimate a local average treatment effect (LATE) for Italian municipalities at the 5,000-inhabitant threshold, and conclude that higher salaries attract more educated mayors who also tend to perform better. Our study differs in several respects.

Primarily, we delve into a large-scale natural experiment that entailed a substantial pay rise for all local politicians. Consequently, we analyze the average treatment effect on the treated (ATT) for towns that are representative of 96% of Italian municipalities, extending the examination to all local politicians, rather than focusing only on mayors. This policy evaluation exercise allows a comprehensive understanding of the reform's impact across various levels of local governance and diverse local contexts.

Secondly, our study investigates the years from 2001 to 2023, providing a comprehensive update on the evolving political landscape. Indeed, we provide evidence that the increase in electoral competition post-reform is notably driven by contexts with lower entry barriers or where alternative employment options are less attractive.[14] Furthermore, with respect to Gagliarducci and Nannicini (2013), in these areas, we also find a fall in the level of education of the elected city council, which might lead to a drop in the future economic performance of these places (Besley *et al.*, 2011).

[12]For instance, Ferraz's analysis of Brazilian local legislators suggests that higher salaries can enhance competition and improve the overall quality of local politicians, while Pique's research in Peru uncovers negative effects of wages on politician selection and performance. Likewise, one prevailing consensus is the pivotal role that the quality of politicians plays in shaping political outcomes (Chattopadhyay and Duflo, 2004; Gagliarducci and Nannicini, 2013), although Freier and Thomasius (2016) present a notable exception to this consensus.

[13]Caria *et al.* (2023) also study monetary incentives for local politicians; they use an approach similar to Gagliarducci and Nannicini (2013), but look at periods between 1985 and 1990.

[14]The shortage of mayoral candidates may be linked to low economic earnings, as also indicated by Bertoni *et al.* (2023). Their study reveals that winning a mayoral election (1993-2017) initially boosts earnings but the positive wage premium turns negative after a decade.

Lastly, we introduce the novel Sh-DiD design, leveraging natural variations in election schedules across municipalities. This methodological approach can be used by empirical researchers in several other contexts, conditional to the underlying assumptions. In the framework of our analysis, this enhances the external validity of the findings compared to prior studies with a similar analytical nature.

All in all, the results of this policy evaluation furnish valuable evidence for policymakers seeking to design targeted reforms tailored to specific local contexts, thereby enhancing their effectiveness.

2 Institutional Background and Data

In Italy, municipal governments are administrated by a city council and an executive committee appointed by the elected mayor. The council and the mayor are directly elected for a five-year term. 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.[15] The pay rise, approved on December 30, 2021, applies to the mayor of 6,562 out of 7,901 municipalities, and also increased the remuneration of other local politicians, such as deputy mayors and councilors.

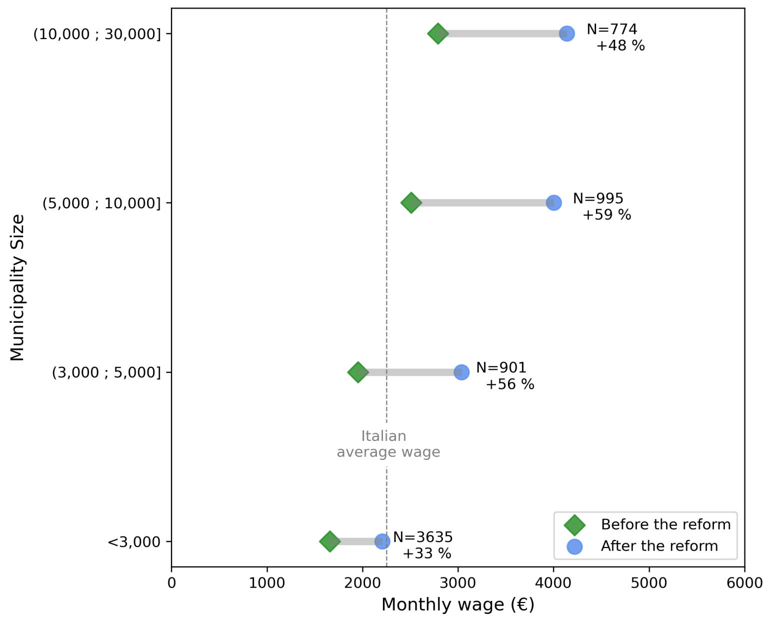
The salary of local politicians is set according to the population class in which the municipality falls, according to the last official census.[16] The salary increase concerns several population thresholds, and we will analyze all those concerning small and medium-sized municipalities up to 30,000 inhabitants, which make up about 96% of

[15]Legislative Decree 124/2019, Article 57c. 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.

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

Italian municipalities and 54% of the population.[17] Figure 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, marked by a dashed vertical line.

Figure 1: Mayors' Wage Before and After the Reform



Note: This This figure shows the mayors' monthly gross salary before and after the reform for different population size groups. It also reports the percentage increase in salary and the number of municipalities in each group (N). The dashed vertical line corresponds to the Italian monthly gross average income, which is approximately equal to 2,200 euros. Appendix Table A.1 provides more detail on the change in monthly wages.

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).

[17]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.

The final sample is made up of 1,024 treated and 895 control municipalities and the details of its construction are described in Appendix.2.

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.[18] Population data for policy thresholds are sourced from the National Institute of Italian Statistics' (ISTAT) permanent census. 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 Appendix.3.

We use these data to create measures that serve as proxies for electoral competition, the educational attainment of local politicians, and their professional backgrounds.[19] 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.

3 Empirical 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-

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

[19]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.

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). To address this challenge, we introduce a novel empirical design that exploits the natural misalignment of election dates across municipalities.[20]

3.1 The Shifted Difference-in-Differences Design

Define $Y_{ic_h,\tau}$ 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. We can only observe Y intermittently (e.g., every five time periods), and the observational interval of time τ is identical within groups but differs across groups. In this framework of misaligned election dates across municipalities, suppose that the population is divided into two groups: the treated and the untreated based on the time of the treatment. For example, if the treatment takes place between τ and $\tau+1$, we observe the post-treatment value $Y_{ic_2,\tau+1}$ for the treated cohort (c_2) and the pre-treatment value $Y_{jc_1,\tau}$ for the untreated cohort (c_1).

In this scenario, we make the following two assumptions to identify ATT_{c_2}:

Assumption AS.1. $\nexists D' \in (\tau, \tau+1) : \mathbb{E}(Y_{ic_2,\tau+1}|D') \neq 0$

Assumption AS.2. $\mathbb{E}(Y_{ic_2,\tau}|\tau+1) - \mathbb{E}(Y_{jc1,\tau}|\tau+1) = \mathbb{E}(Y_{ic_2,\tau}|\tau) - \mathbb{E}(Y_{jc_1,\tau}|\tau)$

AS.1 states that between times τ and $\tau+1$ there is no event D' that might influence $Y_{ic_2,\tau+1}$, apart from the treatment being analyzed. AS.2 states that treated and untreated municipalities would have followed the same trend in electoral outcomes

[20] 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 is not feasible due to few municipalities around the 5,000 population threshold holding elections in 2022 and 2023. Moreover, as the reform affected all municipalities, the population-based RDD would only compare municipalities with different intensities 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.

without the treatment. Under these assumptions, we retrieve ATT_{c_2} as follows:

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

Equation 1 represents a novel design for retrieving the ATT, that we label the *shifted difference-in-differences (Sh-DiD)*. Sh-DiD 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. Basically, this is a 2x2 DiD estimator in which pre/post-treatment periods are shifted between treated and control groups.

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 next section, we will delineate the primary factors to evaluate these assumptions' credibility within our specific framework.

3.2 Validity of the Assumptions

Regarding AS.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).[21] 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, potentially impacting the assessment of the policy. A notable event after the reform was the Russian invasion

[21]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 Appendix.4, we show that results are robust when dropping from the sample all municipalities affected by this policy change.

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 AS.2, while the policy shift had been under consideration for several years, its magnitude and timing were rather unforeseen. 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 key aspect to take into account is that, since the conclusion of World War II, Italian municipal elections have been dispersed temporally, and each municipality schedules its municipal elections at five-year intervals. A municipality can move from one cohort to another only in case of premature dissolution of the municipal council. These aspects impart a strong degree of unpredictability to the composition of the cohorts, making the assignment of the municipalities into the five cohorts akin to 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 Appendix Table A.2. 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.

These characteristics ensure that there exists no substantive reason for municipalities conducting elections in 2022/2023 to inherently differ from those holding elections in 2021. To further address concerns regarding AS.2, we adopt the non-parametric extension of the DiD estimator as proposed by Imai *et al.* (2023). This will 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. Therefore, each treated municipality will be paired with untreated municipalities having similar or identical relevant characteristics. We carefully describe the estimation procedure in Appendix.3 and we propose some robustness checks and placebo analyses in Appendix.4.

4 Policy Evaluation

4.1 Overall results

Table 1 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.[22] The estimates in both panels suggest that the reform did not result in any significant effect on these outcomes.

Several reasons may explain why, in the context under analysis, no effects were observed at the aggregate level. Ideally, we can categorize post-reform mayoral candidates into two groups: "always-candidates" (ACs), who would run for the election regardless of the reform (e.g., a 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

[22] 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.

Table 1: Reform Overall Effects

Panel A: Mayor

	N. of Candidates (1)	N. 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)
N. of Treated	1,024	1,024	1,024	1,024	1,024
N. of Controls	895	895	895	895	895
Statistics of treated in the treatment year:					
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

	Average Years of Education (1)	Share of White-collar Workers (2)	Average Years of Education (3)	Share of White-collar Workers (4)
	Executive Committee		City council	
Reform Effect	-0.058	0.017	-0.024	-0.001
	(0.15)	(0.015)	(0.063)	(0.008)
N. of Treated	1,024	990	1,024	1,004
N. of Controls	895	891	895	895
Statistics of treated in the treatment year:				
Mean	14.018	0.163	13.702	0.134
SD	2.488	0.271	1.324	0.139

Note: This table reports Sh-DiD 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. Appendix.2 and Appendix.3 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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 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.

4.2 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. In Table 2, Panel A displays the effect of the reform in municipalities without open seat elections, while Panel B focuses on cities with open seat elections.

[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.

Table 2: Reform Effects in Contexts with Different Entry Barriers

Panel A: Municipalities Without Open Seats					
Mayor	N. of Candidates (1)	N. 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)
N. of Treated	817	817	817	817	817
N. of Controls	719	719	719	719	719
Statistics of treated in the treatment year:					
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)
N. of Treated	207	207	207	207	207
N. of Controls	176	176	176	176	176
Statistics of treated in the treatment year:					
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)
N. of Treated	529	529	529	529	529
N. of Controls	499	499	499	499	499
Statistics of treated in the treatment year:					
Mean	2.089	0.807	0.645	15.023	0.274
SD	0.794	1.021	0.213	3.191	0.446

Note: This table reports Sh-DiD 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. Appendix.2 and Appendix.3 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

In Panel A, we find that the reform had no effects in municipalities without open seats. The results are not statistically significant and economically 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.

Lastly, in Panel C of Table 2, 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.[24]

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]

[24]For the sake of simplicity, this latter result is reported in Appendix Table A.3.

[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.

4.3 External Opportunities

We evaluate the reform in scenarios with different external opportunities that we measure by using the quartiles of the average income distribution at the municipal level.[26] Accordingly, we have the poorest municipalities in the first quartile and the richest ones in the fourth quartile. Such a taxonomy allows us to disentangle the effect of the reform on politics in terms of outside options.

In Figure 2, we graphically represent the results of this investigation.[27] Although the estimates are often not statistically significant, we can observe some clear patterns. First, the effect of attracting more candidates is positive and significant (at the 10% level) only in the poorest areas, while it is negative in all other areas (Panel A, top-left graph). A similar pattern emerges when considering the effects on competition measured by the number of rookies (Panel A, top-right graph). Second, the effect on the quality of mayoral candidates is negative in municipalities with a low income level, but gradually becomes positive in richer areas (Panel A, bottom graphs). Finally, upon examining other local politicians, our findings indicate a significant positive effect on the proportion of executive committee members in white-collar professions in less affluent municipalities (Panel B, right graph). We observe a positive trend of the effects of the reform on the average years of education among city council members, from the lower to the higher income bracket (Panel C, left graph). 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.

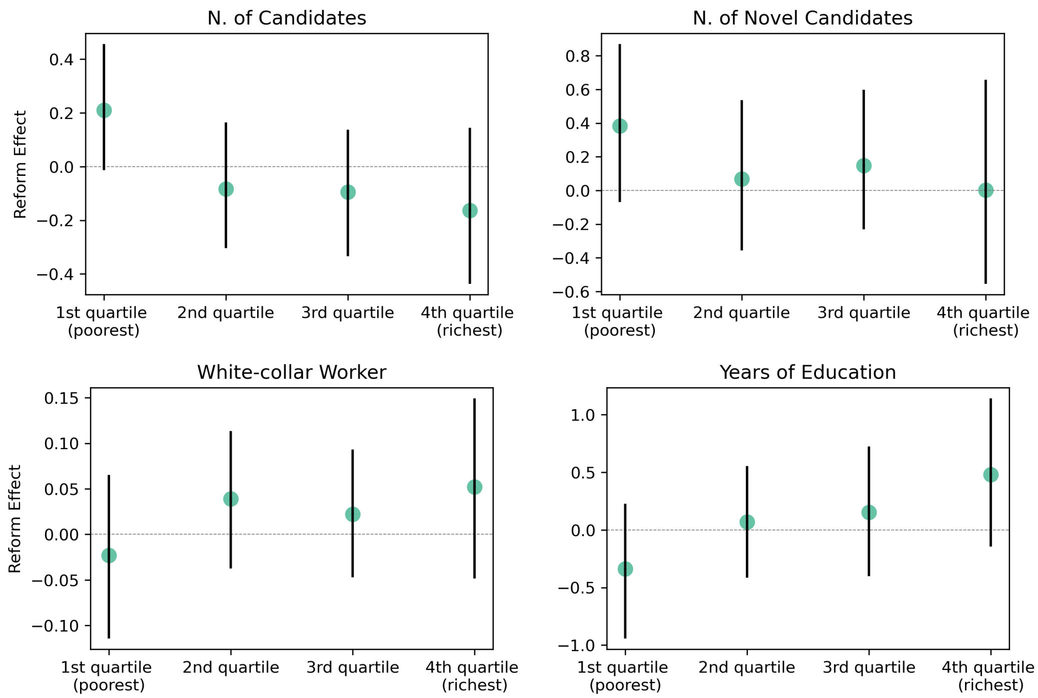
Combining these results, it is evident that in the poorest areas, the reform drew more candidates and altered the quality mix of the political class. It reduced the average education level of city council members and, to a lesser extent, the mayors while increasing the presence of white-collar professionals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

[26] Even if we do not have data on the residence of the candidates, we checked a sample of candidates and found that the vast majority of them live in (or near) the municipality in which they are running.

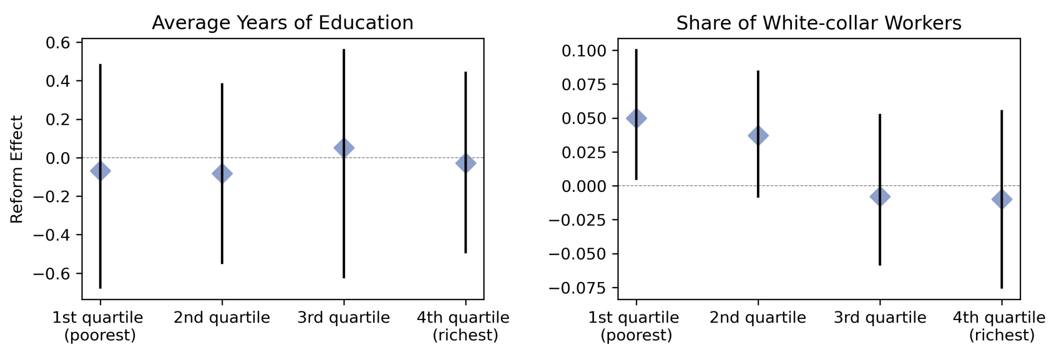
[27] Appendix Table A.4 reports summary statistics of the outcomes by municipal income quartiles.

Figure 2: Reform Effects by Income Quartiles

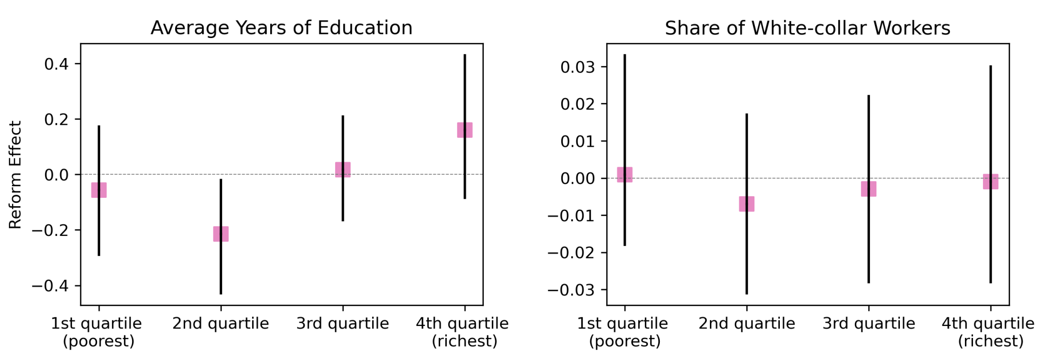
Panel A: Mayor



Panel B: Executive Committee



Panel C: City Council



Note: This figure shows Sh-DiD 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. Appendix.2 and Appendix.3 provide a detailed description of these procedures. Coefficients are reported on the vertical axes with 95% confidence intervals obtained via block-bootstrapping standard errors.

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.

4.4 Election Time to Reform

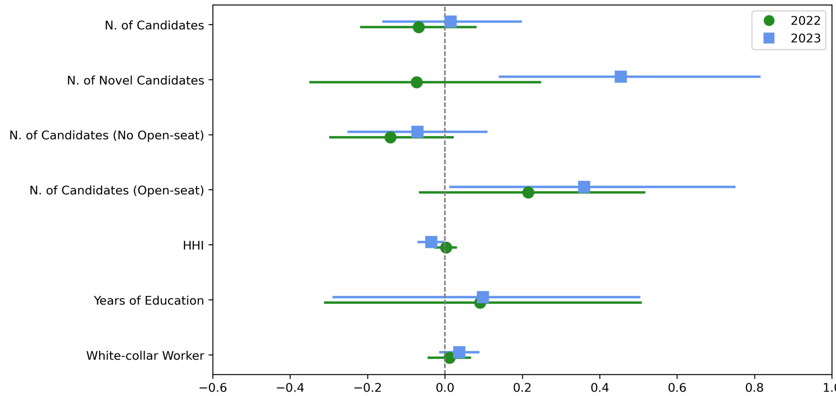
In this policy evaluation, given that only 6 months passed between the adoption of the reform and the first post-reform election, it might be that CCs had not enough time to respond to the reform. This could have happened for a number of reasons, for example: i) the time needed to spread the news of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

In Figure 3, 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 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.

[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.

Figure 3: Reform Effects in 2022 and 2023



Note: This figure shows Sh-DiD 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. Appendix.2 and Appendix.3 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.

5 Concluding Remarks

In our evaluation of an Italian reform that substantially increased the salaries of all local politicians, we find that the reform successfully boosted the number of candidates new to the political sphere, notably in the 2023 elections, without enhancing the caliber of the political class. These outcomes are attributed to the heterogeneous impact of the reform across various political and economic contexts, where the policy's incentive is perceived differently among compliers of the reform. Indeed, we find that these results are primarily driven by contexts with lower barriers to entry or fewer attractive career alternatives. Furthermore, in less wealthy municipalities, the reform shifted the composition of executive committees towards individuals from white-collar backgrounds but lowered the average educational attainment among city council members, potentially decreasing the future economic performance of these places (Besley *et al.*, 2011).

In conclusion, this study enriches the understanding of the intricate interplay between compensation policies, political incentives, and the composition of local governments. Our findings offer valuable insights for policymakers to tailor reforms, considering local nuances—in terms of political competition, income levels, and cities' living costs—to enhance their effectiveness.

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Appendix

Appendix.1 Additional Tables and Figures

Table A.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

Note: 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.

Table A.2: Treated and Control Cohorts based on the Timing of the Elections

...	$Y_{c_1,2016}$			$Y_{c_1,2021}$		
...		$\mathbf{Y_{c_2,2017}}$			$\mathbf{Y^T_{c_2,2022}}$	
...			$\mathbf{Y_{c_3,2018}}$			$\mathbf{Y^T_{c_3,2023}}$

Note: This table shows how the timing of the elections relative to the enactment of the reform defines treated and control cohorts. The policy was adopted at the close of 2021; therefore, the 2021 cohort (c1) was not affected by the treatment, whereas the 2022 (c2) and 2023 (c3) 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.

Table A.3: Incumbent Mayors

	Incumbent Running Again (1)	Incumbent Winning Again (2)
Reform Effect	0.031	0.017
	(0.048)	(0.048)
N. of Treated	817	817
N. of Controls	719	719
Statistics of treated in the treatment year:		
Mean	0.802	0.639
SD	0.399	0.481

Note: This table reports Sh-DiD 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. Appendix.2 and Appendix.3 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: 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

Note: 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.

Appendix.2 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;

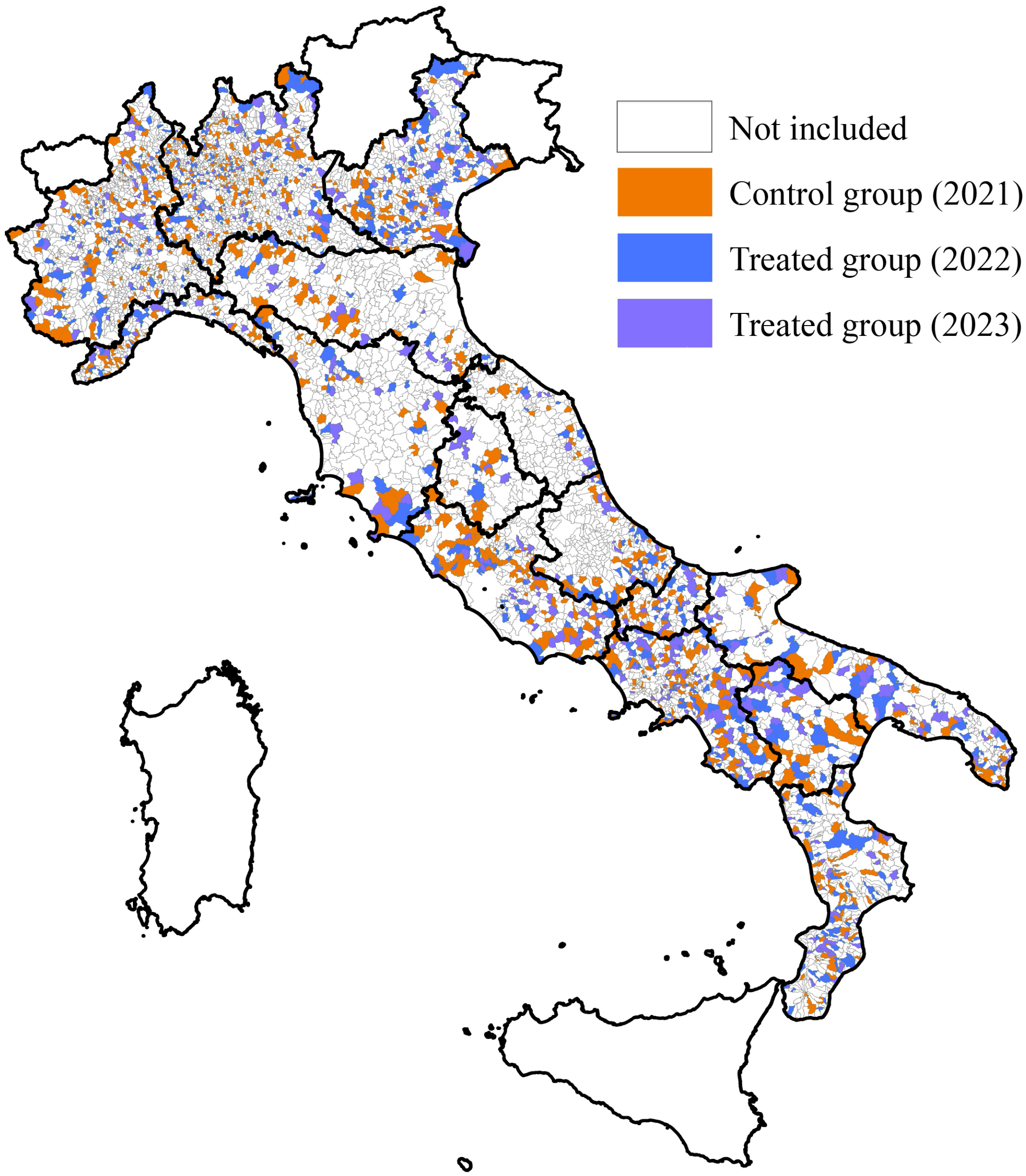
iii. municipalities with local governments dissolved due to mafia infiltration;

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



Note: 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.3 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.[29] In the context of our analysis, this implies that municipalities 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 adopting the non-parametric extension of the DiD estimator as proposed by Imai *et al.* (2023). This methodology encompasses four distinct steps:

Step. 1 We implement a rigorous exact matching procedure to ensure that each municipality holding elections in 2022/2023 is systematically compared to municipalities within the same geographical area (North, Center or South) and falling within the same population bracket ($\leq$ 3,000 inhabitants, between 3,001 and 5,000 inhabitants, between 5,001 and 10,000 inhabitants, between 10,001 and 30,000 inhabitants).

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 the Data section, i.e., turnout, log of the population and income per capita. These control municipalities are selected by using the Mahalanobis distance matching. The pre-treatment data points pertain to the four municipal elections conducted prior to 2021. Figure A.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

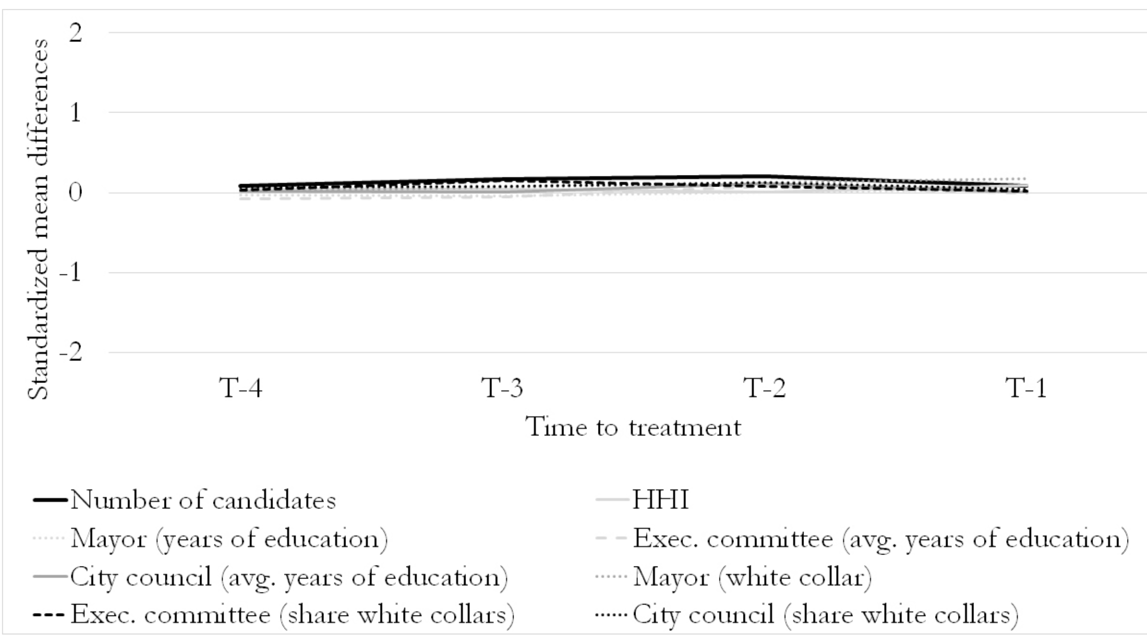
[29] Several factors can contribute to the curtailed tenure of local governments, including political disagreements within the majority, the passing of the mayor, or the infiltration of criminal elements into the administration (Cerqua and Zampollo, 2022).

dependent variable. It clearly emerges that the level of imbalance remains stable across the 4 pre-treatment data points and fully within the (-1, 1) range of the standard deviation.

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

Step. 4 We employ the *Sh-DiD* 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 A.2: Covariate Balancing



Note: This figure shows the covariate balancing between treated and matched control observations. Each line reports the standardized mean difference between treated and control municipalities with respect to each dependent variable.

Appendix.4 Robustness Checks and Placebo Analysis

We conducted an extensive set of robustness checks (RC hereafter) to test the sensitivity of our primary results, with key findings summarized in Appendix Table A.5.[30] 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). Furthermore, we 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 RC5, we excluded all municipalities affected by this policy change from our sample.

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.5, 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.

[30]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.5: Robustness Checks and Placebo Analysis - N. 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 with 3 Neighbors							
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: 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)
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)

Note: This table reports Sh-DiD estimates on the number of candidates. Each column of this table refers to a specific restriction of the estimation sample. 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.4. In robustness check number 5, 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. 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. 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. Appendix.2 and Appendix.3 provide a detailed description of these procedures. Block-bootstrapped standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$