When Women Take All: Direct Election and Female Leadership*

Davide Cipullo†

This version: July 30, 2026

Abstract

This paper investigates how direct election regimes (*à la* presidential democracy) affect the gender composition of political leaders compared to indirect appointment regimes (*à la* parliamentary democracy). I study a 1993 reform that replaced mayoral appointment by councilors with direct election of mayors by popular vote in Italian municipalities. Exploiting the staggered adoption of the reform across municipalities between 1993 and 1997, I find that direct election raises the probability that a municipality is led by a woman by about 2.5 percentage points, from a pre-reform baseline of 3.5 percent to approximately 6 percent. The effect is stronger in municipalities with a greater pre-reform female representation in municipal councils and is driven by more educated officials replacing less-educated incumbents.

JEL Classification: D02, D72, J16
Keywords: Political selection, Voting systems, Gender gaps, Female representation

*I would like to thank the editor Lata Gangadharan, two anonymous referees, Carmela Accettura, Michael Blanga-Gubbay, Massimo Bordignon, Enrico Cantoni, Alessandra Casarico, Tommaso Colussi, Jesper Eriksen, Matteo Gamalerio, Mounir Karadja, Arizo Karimi, Barton E. Lee, Torben Mideksa, Mariona Mas Montserrat, Eva Mörk, Marco Ovidi, Maria Petrova, Luca Repetto, Anna Thoresson, Ekaterina Zhuravskaya and the participants in the seminars held at Harvard University, Bocconi University, University of Barcelona, Uppsala Centre for Fiscal Studies (UCFS), Uppsala University, Università Cattolica del Sacro Cuore, the 78th Annual Congress of the International Institute of Public Finance (IIPF), and the 34th Annual Congress of the Italian Society of Public Economics (SIEP).

†Department of Economics and Finance, Università Cattolica del Sacro Cuore; CESifo; CIFREL; UCFS. Address: Largo A. Gemelli 1, 20123 Milan (Italy). E-mail: davide.cipullo@unicatt.it.

1 Introduction

Women remain substantially underrepresented in political office. Although their share in national parliaments has increased markedly over recent decades, women still account for only 27 percent of parliamentarians worldwide (World Bank, 2024), and the gender gap is even wider among political leaders. According to the REIGN database (Bell et al., 2021), only 12 of 199 national leaders were women, while 140 countries had never been governed by a woman. Existing research attributes women's political underrepresentation to barriers arising at different stages of the selection process, including attitudes toward political competition (e.g., Fox and Lawless, 2004), voter bias (e.g., Baskaran and Hessami, 2018; Le Barbanchon and Sauvagnat, 2022), and party leadership bias (e.g., Esteve-Volart and Bagues, 2012; Huidobro and Falcó-Gimeno, 2023).[1] Gender quotas have substantially increased women's representation in legislative assemblies, but their spillover effects onto executive leadership appear limited, at least in the short run (Bagues and Campa, 2021; Baltrunaite et al., 2019; De Paola et al., 2010; O'Brien and Rickne, 2016; Spaziani, 2022). Moreover, quotas are difficult to apply to positions in which a single individual must be selected.[2]

These limitations raise the question of whether the institutional process through which political leaders are chosen can itself affect women's likelihood of entering and surviving executive office.[3] In this paper, I answer this question by studying a 1993 reform of Italian municipal government that replaced the appointment of mayors by municipal councils with their direct election by voters. Because municipalities historically held elections in different years, the reform was implemented gradually between 1993 and 1997, allowing me to compare municipalities already exposed to direct election with municipalities not yet treated in a staggered Difference-in-Differences framework. I find that direct election raises the probability that a municipality is governed by a woman by approximately 2.5 percentage points, relative to a pre-reform mean of 3.5 percent. Although the effect appears large in relative terms, the resulting level of female leadership remains far from gender parity. It is nevertheless comparable to the very limited presence of women in

[1] See Hessami and da Fonseca (2020) for a comprehensive review.

[2] India is a remarkable and, to my knowledge, unique example of a country that adopted gender quotas targeting top positions. Specifically, only women can compete for the position of leader in a randomly selected fraction of villages (Beaman et al., 2012; Chattopadhyay and Duflo, 2004). However, it must be noted that preventing all male individuals from competing in elections just because of their gender may conflict with the general principle that all citizens—apart from extreme circumstances—should be allowed to compete for democratic political office.

[3] The substantive importance of identifying institutions that facilitate women's access to executive leadership extends beyond descriptive representation. A growing literature shows that female political representation can affect the allocation of public goods and the provision of services such as child care, although the magnitude and direction of these effects vary across institutional settings and policy domains (Baskaran and Hessami, 2025; Chattopadhyay and Duflo, 2004; Ferreira and Gyourko, 2014). Understanding which institutional arrangements affect the selection of female political leaders may therefore also be relevant for the policies implemented by governments.

executive political office during the mid-1990s.[4]

Direct-election regimes—of which presidential democracies are one prominent example—allow voters to choose among alternative candidates for the executive office and to evaluate their individual characteristics and expected performance alongside their ideological positions and policy platforms. Under parliamentary regimes, by contrast, voters elect the members of the legislative assembly, while the identity and tenure of the political leader are subsequently determined by elected politicians. The leader of the most-voted party is not necessarily appointed to lead the government and may be unseated and replaced during the term.[5] The reform that this paper studies closely reflects this distinction. Before the reform, voters elected the municipal council, whose members subsequently appointed one of them as the mayor. The mayor was not necessarily the leader of the most-voted party and could be replaced before the end of the council term. After the reform, mayors are instead elected directly by voters: in small municipalities, the candidate receiving the largest number of votes is elected, while in large municipalities a runoff takes place if no candidate obtains more than 50 percent of valid votes. The reform therefore shifted substantial control over both the initial selection and the subsequent survival of the municipal executive from elected politicians to voters.

The central hypothesis is that elected politicians and voters place different relative weights on political experience and competence when selecting a leader. Members of a municipal council may value experience not only because it can improve policymaking, but also because it signals party embeddedness, adherence to shared political objectives, and reliability in sustaining political agreements. These attributes are directly valuable to fellow politicians, even when they do not translate into better policy outcomes. Voters, by contrast, have no incentive to value this insider political capital beyond its contribution to expected government performance. Direct electoral competition may therefore increase the relative return to observable indicators of competence, such as education.[6]

Because local politics had historically been heavily male-dominated, men entered the reform period with substantially more political experience and were therefore more likely to possess the insider political capital rewarded under council-based appointment. Women were fewer and less experienced, but the women who had entered local politics

[4]For instance, women represented 7.5 percent of mayors in France in 1995 (Council of Europe, 2002), while only 12 women served as elected heads of state or government worldwide in the same year (Inter-Parliamentary Union, 2006).

[5]Examples of prime ministers in parliamentary democracies who were not the winning party's frontrunner at the preceding election abound. For instance, Italy experienced seven consecutive governments between 2011 and 2022—Monti, Letta, Renzi, Gentiloni, Conte I, Conte II, and Draghi—led by prime ministers who were not their party's leader at the previous election. Similarly, in the United Kingdom, Prime Ministers May, Truss, Sunak, and Burnham were not party leaders at the time of the previous election.

[6]In Italian municipalities, electing more educated mayors leads to more public investment, particularly in education, without compromising fiscal stability (Mitra, 2026). Related evidence associates politicians' education and competence with government efficiency, economic performance, and fiscal sustainability (see, e.g., Besley et al., 2011; Meriläinen, 2022; Sørensen, 2023).

were more positively selected than men on formal education. Direct election could consequently increase female leadership where a female political pipeline already existed, by changing the returns to accumulated insider experience relative to observable candidate qualifications.

The preceding arguments identify an underlying mechanism that finds strong empirical support in the data. First, the results are substantially stronger in municipalities where women were more strongly represented in the municipal council before the reform, a proxy for the pool of potential female candidates for the position of mayor. More specifically, the causal effect of the reform on the probability of electing a female mayor is one percentage point and statistically insignificant in municipalities where no women were serving in the municipal council before the reform. Among municipalities where at least one woman was serving in the municipal council before the reform, the effect is small and statistically insignificant in the least gender-balanced quartile and reaches approximately 5 percentage points in the most gender-balanced quartile. Second, I document that female mayors elected after the reform mainly replace incumbents lacking higher education and that the reform raises the proportion of both female and male mayors with at least a high-school degree.

This study makes a number of contributions to the literature on the political economy of institutions and gender. First, this paper is among the first to leverage a quasi-experimental setting to compare the two most widely implemented democratic forms of government. Previous empirical evidence on parliamentary versus presidential democracies falls short of providing conclusive causal estimates because it either relies on cross-country comparisons (e.g., Acemoglu, 2005; Persson and Tabellini, 2003) or on differences between the mayor-council system and the council-manager system (e.g., Coate and Knight, 2011; Enikolopov, 2014). To date, the only study that compares political leaders directly elected by voters with leaders selected by the majority of a legislative assembly in a quasi-experimental setting is Hessami (2018), who studies the introduction of direct mayoral elections in the German state of Hesse. Hessami (2018) finds that mayors directly elected by voters attract more national grants than mayors selected by the municipal council. Although her main focus is on the performance of politicians selected under alternative regimes, an appendix analysis also shows that direct election increases the proportion of female mayors and the educational attainment of mayors. Consistent with these findings, I estimate that direct election causes an increase in both female representation and the educational attainment of municipal leaders. I complement her evidence by documenting the underlying replacement pattern: direct election induces the replacement of less-educated incumbents—who were predominantly men because the pre-reform pool of experienced politicians was overwhelmingly male—with more educated mayors of both genders.

More broadly, these findings provide direct evidence on the political-selection channel

through which political institutions may affect government performance. Besley (2005) argues that the direct election of chief executives may change the attributes rewarded in attaining political leadership, while Persson and Tabellini (2003) document systematic differences in fiscal policy between presidential and parliamentary democracies.[7] While this paper does not directly estimate the effect of the reform on fiscal outcomes, it identifies a concrete change in the composition of the executive that may contribute to broader differences in government performance across institutional regimes. Importantly, the effects on political selection are not confined to the replacement of men by women. The reform reduces the representation of male mayors without a high-school degree and increases the proportion of more-educated male and female mayors. The results therefore extend the literature linking female representation to improvements in political selection (e.g., Baltrunaite et al., 2014; Besley et al., 2017) in two respects: they show that the improvement also operates through the selection of higher-quality men, and that it extends from legislative bodies to political executives.[8]

Second, this paper contributes to the literature on the determinants of gender gaps in political leadership. Existing research identifies barriers at several stages of the political-selection process, including gender differences in entry into political competition (e.g., Fox and Lawless, 2004), voter bias (e.g., Baskaran and Hessami, 2018; Cipullo, 2021; Le Barbanchon and Sauvagnat, 2022), and gatekeeping by parties and political elites (e.g., Esteve-Volart and Bagues, 2012; Gagliarducci and Paserman, 2012; Huidobro and Falcó-Gimeno, 2023). This paper contributes by comparing institutions that determine which actors exercise greater control over the selection of the executive. The finding that direct election fosters female leadership indicates that transferring influence over executive selection from political insiders to voters improves women's prospects of attaining the top municipal office.[9]

This evidence also extends the literature on how electoral institutions affect women's political representation. Existing research has primarily examined electoral rules governing the selection of multimember legislative bodies (Gonzalez-Eiras and Sanz, 2021; Le Barbanchon and Sauvagnat, 2022; Profeta and Woodhouse, 2021). I instead study the institutional rule used to select a single political executive. The results show that general institutional rules governing executive selection can affect women's advancement

[7] Persson and Tabellini (2003) find that presidential democracies have smaller public sectors than parliamentary democracies and are more likely to increase spending during economic downturns and reduce it during booms.

[8] Passarelli and Boto-García (2025) investigate how gender quotas affected the quality of elected politicians in Spanish municipal councils. The results document that gender quotas increased women's representation but had, on average, no statistically significant effect on the educational composition of elected politicians.

[9] It is important to note that this result does not require voters to be free from gender bias; rather, it shows that, in this setting, the barriers arising within the council-based appointment process were sufficiently important that direct electoral selection produced a more gender-balanced outcome despite women being potentially disadvantaged at elections (Cipullo, 2021; Savio, 2024).

into leadership even when they contain no provision explicitly targeting gender representation. They also reveal an important condition: direct election causes an increase in female leadership only where women had already entered local politics and accumulated relevant political experience.

2 Background

2.1 Context and data

Italy has been a parliamentary republic since the end of World War II. The territory is divided into three subnational administrative levels (Regions, Provinces, and Municipalities), which were all subject to parliamentary forms of government and proportional voting systems until the early nineties. Subsequently, a number of reforms implemented between 1993 and 1995 modified the voting rule for the national Parliament from a proportional system to a majoritarian system and introduced a presidential system featuring the direct election of executive officers at all sub-national levels.

The focus of this paper is on the reform of the municipalities' institutional regime, approved in 1993. Based on the election schedule, municipalities were exposed to the reform for the first time at various dates between 1993 and 1997. Municipalities vote on different dates for a combination of historical and political reasons. First, on the occasion of the first municipal elections after the end of WWII in 1946, several areas of the country were still devastated at the time of the vote, and the central government allowed municipalities to vote on different dates between March and late autumn. The majority of municipalities voted on five different dates during spring 1946, and more than 1,000 municipalities voted on eight dates during the autumn (Ballini, 2010). Second, during the post-war decades, successful votes of no confidence or other causes of early terminations were relatively common. The combination of these two factors generates a large variation in the timing of elections: as shown in Appendix Figure B.1, only 17 percent of municipalities voted in the same year as the implementation of the 1993 voting reform, while the first vote under the new regime took place in subsequent years in the remaining municipalities until 1997.

The main data source is the individual-level register of politicians holding any office at the municipality level, compiled every year since 1986 by the Italian Ministry for Internal Affairs (*Anagrafe degli Amministratori Locali*). The dataset reports characteristics such as gender, education, employment status and occupation, date and place of birth, as well as the description of the office, the party affiliation, and the election and nomination dates for all individuals holding a political office as of December 31.[10] I restrict the sample to

[10]Data coverage implies that individuals who leave office in the same year in which they obtained the position are not covered in the data.

municipalities belonging to the fifteen ordinary regions, as the so-called *special regions* (*Friuli-Venezia Giulia, Sardegna, Sicilia, Trentino-Alto Adige* and *Valle d'Aosta*) are guaranteed the autonomy to determine a different electoral system for their municipalities. Overall, I have information from 22,171 unique mayors serving in 6,322 municipalities between 1986 and 2008 for a total of more than 137,700 municipality-year observations.

In the years immediately preceding the first direct mayoral elections, women accounted for only about 3.5 percent of mayors and 8 percent of municipal councilors. Female leadership was rare across territories and across the whole political spectrum. Among the main parties active before the reform, women accounted for about 3 percent of mayors affiliated with the Christian Democrats (DC), 4 percent of mayors affiliated with the Communist Party (PCI), and 1.7 percent of mayors affiliated with the Socialist Party (PSI). The share was somewhat higher among independent mayors and mayors affiliated with civic lists, where women represented 6 percent of office-holders. However, these groups accounted for only 12 percent of pre-reform mayors, and therefore represented a limited share of the overall pool of local executives. Geographic differences were also substantial. Before the reform, women accounted for 4.5 percent of mayors in the North, compared with 2.9 percent in the Centre and only 1.4 percent in the South.

Yet, the data reveal a sharp gender difference in the type of political capital accumulated by local politicians before the reform. Conditional on reaching the mayoralty, female mayors differed systematically from male mayors. Table 1 shows that female mayors had accumulated significantly less political experience than male mayors, while being significantly more educated. These differences are visible in unconditional comparisons and survive controlling for age and for year, municipality, and party fixed effects. Female mayors had served on average fewer previous terms; they were also more educated, with an adjusted difference equal to approximately one extra year of schooling. This evidence documents that the reform occurred in a setting in which women were fewer and less experienced than men, but relatively more positively selected on formal education.

2.2 Mayor selection and survival under the parliamentary system

Before the reform, municipalities were akin to small parliamentary democracies. Voters elected the members of the municipal council (i.e., the legislative assembly). Subsequently, the council elected the mayor (head of the executive political power) from among its own members. At any time, the council had the power to unseat the incumbent mayor via a vote of no confidence and to try to form a new majority for the remaining part of the term. Terms lasted five years, although early terminations were allowed by law and were relatively common. Mayors were not term-limited.

Figure 1 reconstructs the dynamics of government formation and stability for mu-

Table 1: Gender differences in experience and education among pre-reform mayors

	(1)	(2)	(3)	(4)
	Mayor's Experience (terms)		Mayor's Education (years)	
Female Mayor	–0.049***	–0.056**	0.824***	0.918***
	(0.017)	(0.026)	(0.209)	(0.281)
Observations	50,254	50,250	50,254	50,250
R^2	0.000	0.696	0.002	0.787
Year Effects		✓		✓
Municipality Fixed Effects		✓		✓
Age		✓		✓
Party Fixed Effects		✓		✓

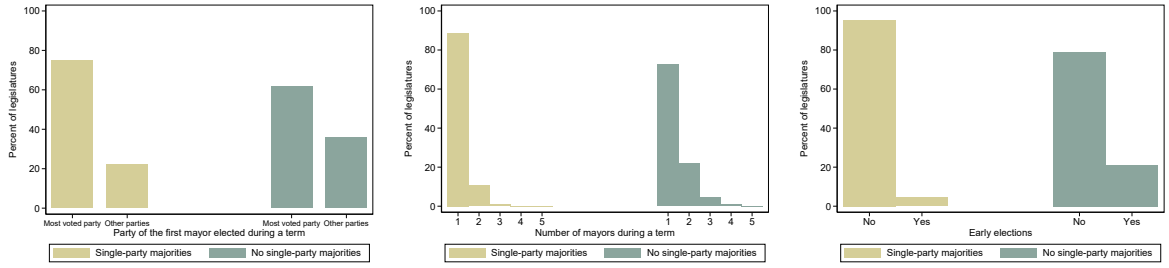
Notes: The table reports descriptive associations between mayor gender and observable characteristics of mayors serving before the introduction of direct mayoral elections. All mayors serving in municipalities belonging to ordinary regions elected between 1985 and 1992. The unit of observation is a municipality-year, and the sample includes only observations for which a mayor is in office. In columns (1) and (2), the dependent variable is the number of previous terms served by the incumbent mayor. In columns (3) and (4), the dependent variable is the mayor's years of schooling, reconstructed by converting the available information on completed degrees into years of schooling. Female mayor is an indicator equal to one if the mayor in office is a woman. Columns (1) and (3) report unconditional differences between female and male mayors. Columns (2) and (4) include age and year, municipality, and party fixed effects. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

nicipal elections held before the reform. To ease interpretation, all panels distinguish between legislatures with a single-party majority—that is, cases in which the largest party controlled more than 50 percent of the seats in the municipal council—and legislatures without a single-party majority (where forming a post-election coalition was necessary to control the council; see, e.g., Carozzi et al., 2024).

Panel (a) reports whether the mayor belonged to the largest party in the council or to another party.[11] The panel shows that municipal councils frequently selected mayors who did not belong to the largest party. This occurred in approximately 25 percent of legislatures with a single-party majority and in approximately one-third of legislatures without a single-party majority.[12] This evidence indicates that post-election bargaining among parties represented in the municipal council was an important component of mayoral selection under the pre-reform regime. It also suggests that voters could not perfectly anticipate which individual would eventually become mayor when casting their vote for the municipal council.

[11] When more than one individual served as mayor during the same term, the panel refers to the mayor elected at the beginning of the term.

[12] The data identify the party affiliation of mayors elected by municipal councils, but not whether these mayors were party leaders. I therefore cannot measure the incidence of cases in which the council selected a mayor who was not the leader of her party. Party leaders also cannot be inferred from list ranks because this information is not available in the Italian data. Moreover, the mayoral election did not feature a formal candidacy procedure; therefore, it is not possible to compare the characteristics of mayors with the characteristics of defeated mayoral candidates.



(a) Rank of mayor's party (b) Number of mayors (c) Early terminations

Figure 1: Government formation and government instability under the parliamentary system

Notes: Elections held in the fifteen ordinary regions between 1985 and 1992. The unit of observation is a term. In all panels, yellow bars represent single-party majorities while green bars represent legislatures in which no party held the majority of the seats. Approximately 80 percent of legislatures featured a single-party majority. Panel (a) shows the proportion of legislatures in which the mayor that was elected at the beginning of the term belonged to the most-voted party (resp., to any other party). Panel (b) shows the proportion of legislatures that elected one mayor to serve during the whole term (resp., more than one mayor during the term). Panel (c) shows the proportion of legislatures that remained in power for the whole five-year period stated by the law (resp., those that experienced an early termination). Information on early terminations comes from the digitalization of the universe of Decrees of the President of the Republic published between 1995 and 1997.

Panel (b) reports the number of mayors who served during the same council term. Approximately 15 percent of legislatures with a single-party majority experienced at least one within-term replacement of the mayor initially elected by the council. The corresponding share was substantially higher, above 30 percent, among legislatures without a single-party majority. These patterns show that replacing the mayor without holding a new council election was common under the pre-reform system, especially when no party controlled a majority of council seats.

Panel (c) reports the proportion of council terms ending in early elections due to episodes of political instability. Early terminations were rare among councils with a single-party majority, occurring in less than 5 percent of terms. By contrast, early elections were called in more than 20 percent of terms in which no party held a majority of council seats.

The joint incidence of these three margins quantifies how often the identity of the governing mayor can, in fact, be inferred from the election result alone. The pre-reform mayor belonged to the most-voted party, served for the entire term, and experienced no early council termination in only 66 percent of legislatures with a single-party majority. Among legislatures without a single-party majority, this share falls to 38.5 percent. In other words, for more than a third of single-party-majority terms and for most terms in which no party controlled the council, the identity of the mayor serving at some point during the term differed from what a voter could have anticipated from the election result alone.

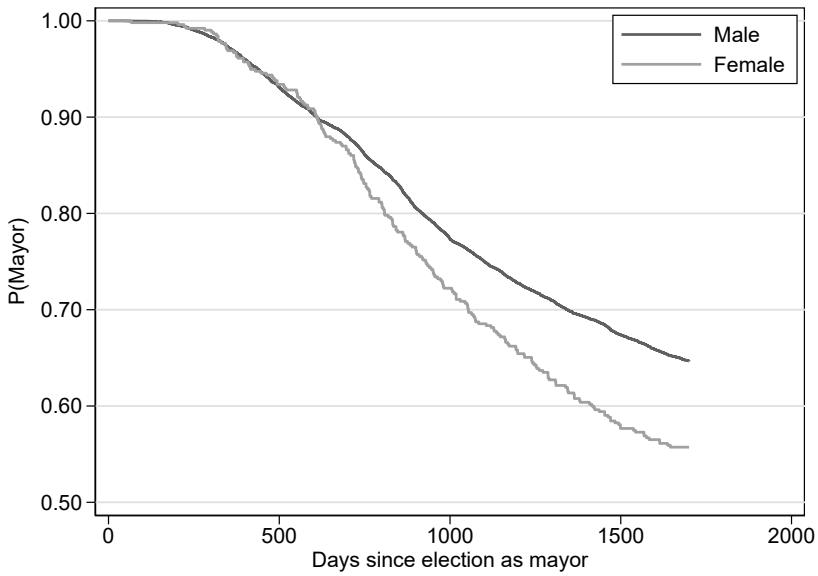


Figure 2: Gender gap in the survival rate in office as mayor (before the reform)

Notes: All mayors serving in municipalities belonging to ordinary regions elected between 1985 and 1992. The figure reports estimated survival functions for remaining in office as mayor as a function of the number of days since appointment. The dependent variable is an indicator equal to 1 if the individual is still serving as mayor. The darker line refers to male mayors while the lighter line denotes female mayors.

The gap between the election result and who is actually in office also interacts with gender. In Figure 2, I show the survival rates in office of male and female mayors elected by municipal councils during the same period (only 3.5 percent of mayors who served during the period were women). The figure shows that less than seventy percent of male mayors served for the full natural five-year cycle (the average length of mayoral terms was 3.8 years). Figure 2 also shows that, conditional on becoming mayor, women were considerably less likely than men to retain office for the whole term. The two lines overlap for less than two years and then become increasingly divergent over time within the term. Appendix Table B.1 confirms the presence of such a large gender gap in a formal regression framework: on average, women's mandates as mayor lasted approximately 100 fewer days than men's. The gap persists when controlling for municipality fixed effects, year of election fixed effects, the share of women sitting in the municipal council, and observable individual characteristics of the mayor such as education, age, and previous experience.

Taken together, this evidence indicates that, under the pre-reform regime, the election result was frequently not sufficient to determine who governed a municipality throughout a term. Two-thirds of single-party-majority terms and fewer than two-fifths of terms without a single-party majority satisfied all the conditions under which the election result alone determined who governed for the whole term. In the remaining terms, mayoral

identity was instead the product of post-election bargaining, replacement, or early termination. Moreover, this margin was not gender-neutral: conditional on reaching office, women were systematically less likely than men to remain mayor for the full term. For these reasons, the empirical analysis that follows measures the gender of the mayor in every year of the term rather than only at the moment of the election, so as to capture both the initial selection and the subsequent survival of municipal leaders.

2.3 The institutional reform

Since the reform, municipalities have been comparable to small presidential democracies. Mayors are directly elected under majoritarian rules, and a large majority premium of seats in the council is assigned to the mayor's coalition to enhance the stability of local governments. The reform also assigned to mayors the power to dismiss members of the executive office, required early termination of the term and new elections in the event of the resignation of the mayor or a vote of no confidence, and introduced a term limit of two consecutive terms for mayors.[13]

On the one hand, the reform changed the electoral object observed by voters. Under the pre-reform system, parties and lists competed for seats in the municipal council, but there was no official mayoral ballot and therefore no formal set of mayoral candidates. After the reform, by contrast, mayoral candidates appear directly on the ballot and are linked to one or more supporting parties or civic lists.[14]

On the other hand, the reform changed the relationship between the mayor and the council. If one reproduced the same statistics presented in Figure 1 and in Figure 2 for elections held after the reform, one would obtain a very different picture. First, legislatures without a single-party majority are virtually impossible.[15] Second, the mayor must be the leader of the most-voted party or pre-election coalition. Third, only one individual serves as mayor for the whole term because early elections are mandatory if the mayor is forced out of office. Fourth, early elections become less common because

[13]The introduction of the term limit did not prevent mayors who had been in office before the reform from running in the first direct election. Consider the example of a mayor elected in 1983 and confirmed in 1988. According to the law, she had the right to run for office in 1993 and 1997 but, in the case of two victories, not in 2002. The voting system reform also introduced gender quotas in all municipalities. More specifically, each party was required to have at least one-third or one-fourth women on its list of candidates for the municipal council, depending on the municipality's population. Italy's Constitutional Court later abolished this portion of the reform in 1995 due to constitutional concerns. Lastly, the reform reduced the natural length of mayors' and councilors' terms from five to four years – this portion of the reform was later repealed in 1998.

[14]This institutional feature is important for the interpretation of the empirical analysis. Variables such as the number of mayoral candidates, the gender composition of mayoral candidates, or the presence of independent mayoral candidates are well defined only after the reform. For this reason, the analysis cannot construct a symmetric pre/post comparison of these outcomes and instead focuses on the gender of the office-holder and on pre-reform characteristics of the local political pool.

[15]The law features a knife-edge case in municipalities with more than 15,000 in which this may not occur. However, this knife-edge case is extremely uncommon in the data.

all councils now feature a single-party or single-coalition majority. Consequently, for elections held after the reform, the survival rate in office throughout the full natural cycle exceeds ninety percent.[16]

3 Empirical Strategy

3.1 Setup

The reform was phased in gradually: because municipalities vote at different points in time, they were exposed to the presidential system for the first time at different dates over the period 1993–1997. This staggered implementation is the source of identifying variation exploited in this paper, and it is useful to describe it before turning to the construction of the estimation sample.

Table 2 illustrates the underlying logic. Suppose that Municipality A holds elections in 1988, 1993, and 1998, and therefore belongs to the 1993 treatment cohort, whereas Municipality B holds elections in 1987, 1992, and 1997, and belongs to the 1997 cohort. During their first terms, both municipalities are governed under the pre-reform indirect-appointment regime. During their second terms, Municipality A has switched to direct election following its 1993 election, while Municipality B remains under indirect appointment after its 1992 election. These two pairs of terms generate a standard 2×2 Difference-in-Differences contrast: both cohorts are observed before treatment, and in the subsequent period only Municipality A is treated. In the final pair of terms, both municipalities are governed under the direct-election regime, as Municipality B also becomes treated following its 1997 election, so no further comparison is feasible.[17] The staggered implementation of the reform extends the same logic to every pair of treatment cohorts between 1993 and 1997.

Although the Difference-in-Differences logic can be formulated at the level of terms, the object of interest in this paper varies more granularly. As documented in Section 2, under the pre-reform regime the mayor initially selected by the council frequently did not belong to the most-voted party, was often replaced during the term, and the term itself frequently ended early: all three conditions under which the election result determines who governs for the entire term held jointly in only 66 percent of legislatures with a single-party majority, and in 38.5 percent of those without one. This margin was moreover not gender-neutral, as women who reached the mayoralty were substantially less likely than

[16] Gagliarducci and Paserman (2012) document that female mayors elected in a close election are more likely to be unseated during the term than male mayors elected in a close election. Acconcia and Ronza (2026) find that gender quotas reduce episodes of government instability.

[17] As documented in Appendix Figure B.1, years after 1997 do not contribute to the identification of the average effect of the reform. Nevertheless, they are kept in the analysis to perform the placebo test discussed in Appendix A.

Table 2: Intuition of the empirical strategy

Municipality	Election year	$PostReform$	Treatment cohort
A	1988	0	1993
B	1987	0	1997
A	1993	1	1993
B	1992	0	1997
A	1998	1	1993
B	1997	1	1997

Notes: Municipality A holds elections in 1988, 1993, and 1998 and therefore belongs to the 1993 treatment cohort. Municipality B holds elections in 1987, 1992, and 1997 and belongs to the 1997 treatment cohort. In the first pair of terms, both municipalities are governed under the indirect-appointment regime. In the second pair, Municipality A has switched to direct election, whereas Municipality B remains under the indirect-appointment regime. These first two pairs of observations therefore generate a 2×2 Difference-in-Differences comparison. In the final pair, both municipalities are governed under the direct-election regime, so no comparison is feasible.

men to retain it for the full term. A term-level outcome, such as the gender of the mayor selected immediately after each election, would treat a mayor unseated after a few months identically to one governing for five years, and would therefore discard precisely the post-election appointment, replacement, and survival margins along which the two institutional regimes differ.

Accordingly, the goal of this paper is to estimate the effect of the institutional reform on the probability that a municipality is governed by a woman at a given point in time. I therefore construct a municipality-year panel containing information on the individual characteristics of the mayor in office at the end of the year, the members of the executive office, and the members of the municipal council, using the most granular level of disaggregation available in the *Anagrafe degli Amministratori Locali*.[18] Under the assumption that municipalities exposed to the reform in different years would have evolved following parallel trends in its absence, a difference-in-differences model of the form

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \beta PostReform_{i,t} + u_{i,t}, \tag{1}$$

identifies the causal effect of the institutional reform on the probability that the mayor holding office in municipality i during calendar-year t is a woman.

In equation (1), η_i represents the municipality fixed effects, δ_t represents the calendar-year fixed effects, and $PostReform_{i,t}$ is an indicator equal to 1 if the previous election took place after the implementation of the reform (notice that the $PostReform_{i,t}$ indicator is analogous to an indicator for the previous election being held in or after 1993, i.e. $\mathbb{1}(ElYear_{i,t} \geq 1993)$).

Two additional advantages follow from this structure. First, because the identity of the governing mayor is observed in every calendar year for every municipality, I observe

[18] Appendix Table B.2 reports the descriptive statistics of the variables used in the analysis.

in the same calendar year both mayors serving under a parliamentary system and mayors serving under a presidential system. This within-year variation in treatment status permits the inclusion of calendar-year fixed effects, ensuring that the results are not confounded by aggregate shocks that hit Italy in or around 1993 – a period of major fiscal and political reforms. Such controls would not be available in a design based on election years alone, since municipalities voting in the same year share the same treatment status by construction. Second, because all mayors are observable, including those selected by the municipal council later during the term, the two institutional regimes can be compared along all the dimensions discussed in Section 2: the propensity to choose the leader of the winning party as mayor, the propensity to replace the mayor during the term, and the propensity for early elections.

3.2 Identifying assumptions

The setup described above is intuitively appealing but deserves a careful discussion of the threats to the validity of the empirical design.

First, to assess the plausibility of the parallel-trends assumption, I estimate dynamic models of the form:

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \sum_{k\in[-5,5],k\neq-1} \alpha_k \mathbb{1}(ElYear_{i,t} - 1993 = k) + \varepsilon_{i,t}. \qquad (2)$$

The validity of the empirical design would be supported if the coefficients α_k are indistinguishable from zero for all $k < 0$. An important threat to the parallel-trends assumption is that the empirical strategy compares municipalities that, in the same year, are at different points of their electoral cycle (in the 1993 vs. 1997 example outlined before, equation (2) compares a municipality in the first year of the term and a municipality in the final year of the term). To ensure that the results of this paper are not mechanically confounded by this comparison, I also produce results in which I control for cohort-specific linear time trends (i.e., linear trends common for all municipalities exposed to the reform for the first time in the same year).

Second, recent research has shown that estimating Difference-in-Differences models using a two-way fixed effects (TWFE) model as in the equations specified above might result in biased estimates of the average treatment effect on the treated when different units are treated at different points in time (e.g., Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). To ensure the results of this paper are robust, I also estimate the empirical model using the estimators proposed by Sant'Anna and Zhao (2020), Callaway and Sant'Anna (2021), and De Chaisemartin and d'Haultfoeuille (2020).

Third, the reform evaluated in this paper also introduced gender quotas in the voting system to select members of the municipal council (later abolished in 1995). There are several reasons why this potential confounder does not invalidate the empirical exercise. On the one hand, previous research has documented, both in Italy and in other contexts, that gender quotas for selecting members of legislative bodies do not systematically affect the selection of women into leadership (i.e., mayoral) positions. More specifically, Spaziani (2022) finds that neither gender quotas implemented in Italy between 1993 and 1995 (i.e., the same period considered here) nor later experiments of gender quotas (introduced in 2013 and 2014) have an impact on the probability of electing a female mayor, while De Paola et al. (2010) find that the 1993–1995 instance of gender quotas increased the proportion of female mayors elected in later years. Baltrunaite et al. (2019) do not find any effect of the most recent implementations of gender quotas in Italy. Bagues and Campa (2021) find, in the context of municipalities in Spain, that gender quotas do not have any impact on the probability of electing a female mayor or on the probability that a woman is appointed as her party's leader. To my knowledge, the only evidence in support of the hypothesis that gender quotas may spill over to affect the probability that women are elected to leadership positions is in O'Brien and Rickne (2016).[19] Studying a gender quota introduced by one party in Sweden, O'Brien and Rickne (2016) find that the decision increased the proportion of women who become mayors. On the other hand, if my empirical results were driven by the temporary introduction of gender quotas rather than by direct election, the effects would die out after the quota's abolition in 1995 and would be driven by municipalities where the pre-reform share of female councilors was lower than the quota.[20] In the next sections, I provide strong empirical evidence against both of these possibilities.

Fourth, the reform also introduced a limit of two consecutive terms for directly elected mayors. This restriction did not prevent any incumbent from participating in the first direct election, nor did it prevent a mayor elected under the new regime from seeking immediate re-election. Its mechanical effect arose only at the third post-reform election, approximately eight to ten years after a municipality's first exposure to direct election. Thus, the term-limit provision could explain the immediate treatment effect only if prospective politicians altered their decision to seek the mayoralty because they anticipated being required to leave office after two future mandates. Evidence from a later Italian reform of term-limit policies suggests that this anticipatory channel is unlikely to be an important mechanism. Kansikas and Bagues (2026) study a reform that extended

[19] Casas-Arce and Saiz (2015) finds, in the context of municipalities in Spain, that gender quotas increase the support for parties most affected by quotas, suggesting that parties were not maximizing their expected vote share absent the quotas. This result has been recently challenged by Bagues and Campa (2020), who instead document a lack of significant effects of gender quotas on voting outcomes in the same context.

[20] Equations (2) and (5) are also important to separate the effect of the 1993 reform from the potential effect of gender quotas, abolished in 1995.

mayoral term limits from two to three terms and focus on the same outcome considered in this paper: whether the mayor is a woman. The authors distinguish the mechanical replacement effect from changes in the expected career horizon of prospective politicians. The effects on female mayoral representation are concentrated in municipalities where the incumbent was completing a second term and the reform therefore directly changed eligibility to run again. Where the incumbent was still in the first term, so that the reform affected only the potential length of a future political career, they find no effect on the gender or other characteristics of the elected mayor. This evidence is consistent with term limits affecting political selection primarily when they mechanically bind rather than reshaping the overall incentive trajectory several years in advance. Because direct elections and term limits were introduced by the same law, the design cannot formally exclude every possible anticipatory response to the future term limit. The timing of the estimated effect and the evidence from Kansikas and Bagues (2026) make an anticipatory response an implausible explanation for the estimated effect.

Lastly, government stability, which in turn may affect the timing of exposure to the reform, is potentially endogenous to the voting system (Bordignon et al., 2016; Carozzi et al., 2022) and the gender of political leaders (Gagliarducci and Paserman, 2012; Huidobro and Falcó-Gimeno, 2023).[21] For instance, incumbent mayors elected under the old rule might have advocated for early elections under the newly introduced presidential system to secure a more stable hold on power. Conversely, members of the municipal council might have unseated the incumbent mayor just before the phase-in of the reform to secure another term subject to the parliamentary regime. There are numerous anecdotes, including in Rome and Milan, of councils and mayors who resigned with the explicit objective of holding early elections just after the adoption of the institutional reform.

To deal with the potential endogeneity in the timing of exposure to the reform, I use an instrumental variables approach in which I predict the first year in which each municipality should have been exposed to the presidential system had the previous term lasted for its natural five-year cycle.[22] Formally, I instrument $PostReform_{i,t}$ with its five-year lagged counterpart $\mathbb{1}(ElYear_{i,t-5} \geq 1988)$. The instrument should not be affected by any endogeneity concerns because the parliamentary discussion about the reform started well after 1988. This instrumental variable strategy implies that the compliers in year

[21] Carozzi et al. (2022) find in Spanish municipalities that, in the presence of indirect election of mayors, an increase in the number of parties entering the municipal council reduces the stability of incumbent governments while Bordignon et al. (2016) document that the number of parties admitted to the municipal council is endogenous to the voting system. Gagliarducci and Paserman (2012) find in Italian municipalities that, under a direct election scheme, female mayors are more likely to be unseated via a vote of no-confidence than male mayors while Huidobro and Falcó-Gimeno (2023) find in Spanish municipalities, under an indirect election scheme, that parties that win the majority of seats in the local council are less likely to appoint the new mayor if the party leader is a woman.

[22] A similar instrument has been adopted by Repetto (2018).

t are the municipalities subject to the reform in the same year in which the previous election took place in year $t-5$. Hence, the compliers are municipalities whose actual exposure followed the scheduled electoral calendar rather than being brought forward by an early termination between 1988 and 1992.

I estimate the following Difference-in-Differences regression using 2SLS:

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \beta \widehat{PostReform}_{i,t} + v_{i,t}, \tag{3}$$

where

$$PostReform_{i,t} = \eta_i + \delta_t + \phi \mathbb{1}(ElYear_{i,t-5} \geq 1988) + \epsilon_{i,t}, \tag{4}$$

and the dynamic specification

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \sum_{k \in [-5,5], k \neq -1} \alpha_k \mathbb{1}(ElYear_{i,t-5} - 1988 = k) + \varepsilon_{i,t}. \tag{5}$$

4 Results

4.1 Main results

Table 3 reports the estimated impact of the introduction of a direct election regime on the probability that the mayor is a woman. I show the coefficients obtained both using OLS as in equation (1) and 2SLS as in equations (3) and (4). Appendix Table B.3 reports the first-stage and reduced-form relationship associated with columns (4) – (6) of Table 3. The first-stage F-statistics are far above the conventional weak-instrument benchmark of 10, as expected in this setting. Scheduled exposure strongly predicts actual exposure, with departures from a perfect mapping arising only from early terminations or other disruptions of the normal electoral calendar.

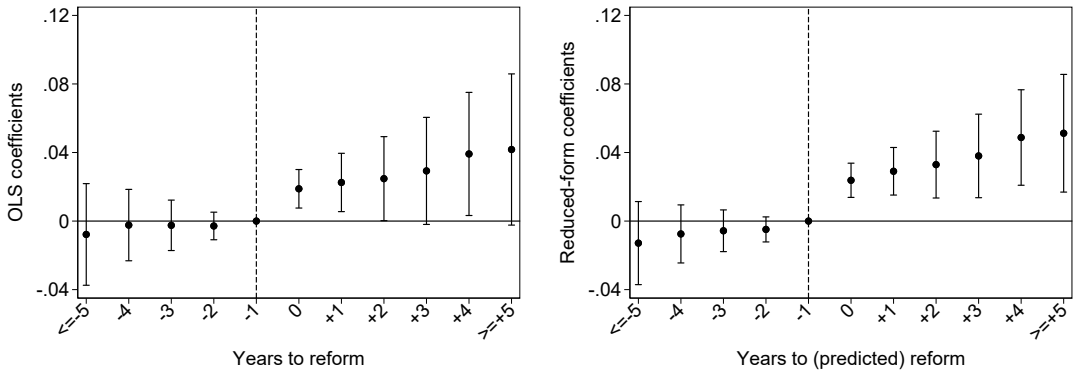
Across all specifications, the results document that the introduction of the direct election of mayors raises the probability of selecting a female mayor by 1.5–3.5 percentage points, depending on the specification. The coefficients are sizable in magnitude and very precisely estimated: taking into consideration that a woman was the mayor only in 3.5 percent of pre-reform municipality-year observations, the results imply that the reform increases the proportion of female mayors by 40–100 percent. Table 3 also shows that the estimates are robust to the introduction of cohort-specific linear time trends and additional covariates interacted with year dummies. Indeed, the coefficients estimated in columns (1), (2), and (3) and the coefficients estimated in columns (4), (5), and (6) lie within each other's confidence intervals, respectively. However, the specifications in columns (2) and (5) appear to be more conservative than the specifications in columns (1) and (4). Therefore, the analysis conducted in the rest of the paper relies on the same

Table 3: Direct election of leaders and proportion of female mayors

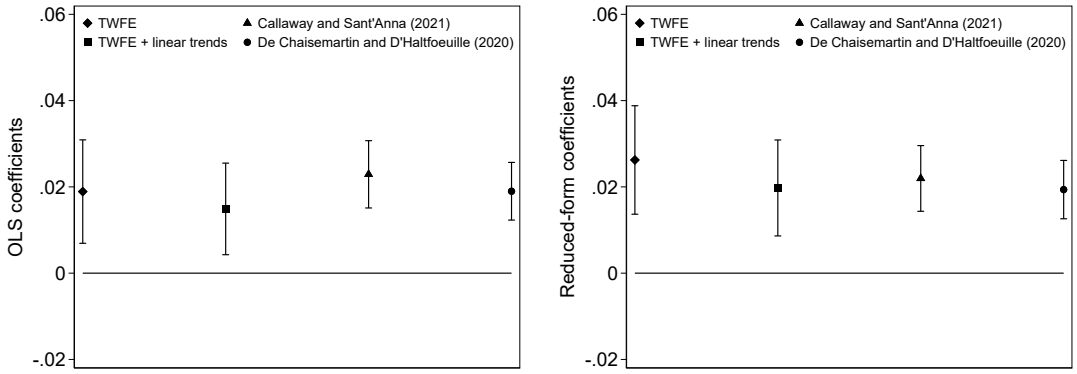
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.019***	0.015***	0.015***	0.035***	0.024***	0.024***
	(0.006)	(0.005)	(0.005)	(0.009)	(0.007)	(0.007)
Observations	137,718	137,718	137,718	137,718	137,718	137,718
R^2	0.345	0.346	0.348	0.000	0.000	0.004
Year Effects	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort × linear trends		✓	✓		✓	✓
Time-varying covs			✓			✓
Pre-determined covs × Year			✓			✓
Control mean	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350
First-stage F				3553	6180	6180

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. In column (1), the estimated equation is (1). Column (2) augments the specification in column (1) with linear trends specific to each cohort of municipalities. Column (3) augments the specification in column (2) with controls for each municipality's surface, population density, gender differences in educational attainment, population, sex ratio, female employment rate (all measured in the 1991 population census) as well as the pre-reform share of women in the municipal council interacted with year dummies, educational attainment of the pre-reform incumbent mayor interacted with year dummies, and vote share of the main parties in 1992 national elections interacted with year dummies. In column (4), the estimated equations are (3) and (4), using 2SLS. Column (5) augments the specification in column (4) with linear trends specific to each cohort of municipalities. Column (6) augments the specification in column (5) with controls for each municipality's surface, population density, gender differences in educational attainment, population, sex ratio, female employment rate (all measured in the 1991 population census) as well as the pre-reform share of women in the municipal council interacted with year dummies, educational attainment of the pre-reform incumbent mayor interacted with year dummies, and vote share of the main parties in 1992 national elections interacted with year dummies. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

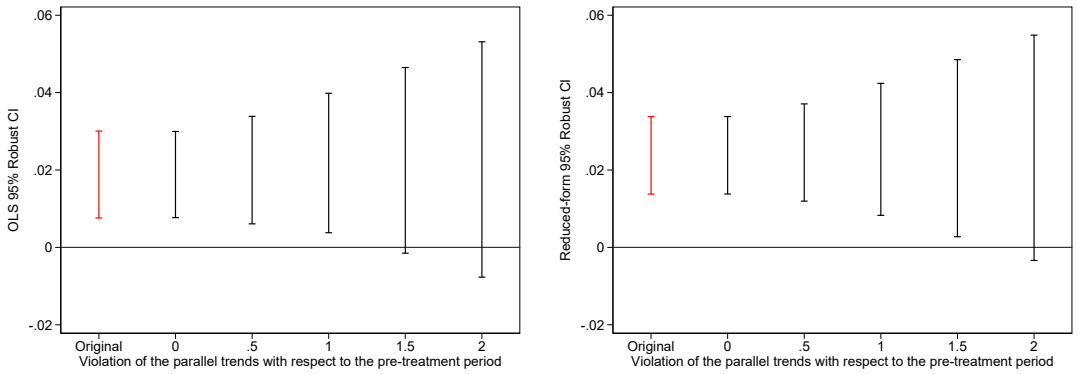
specifications as in columns (2) and (5). The preferred specification is the one presented in column (5), according to which direct election increases the probability of selecting a female mayor by 2.5 percentage points (i.e., approximately 70 percent relative to the pre-reform mean of the dependent variable).



(a) Dynamic specifications



(b) Alternative Difference-in-Differences estimators



(c) Robustness to violations of parallel trends assumption (Rambachan and Roth, 2023)

Figure 3: Dynamic specifications and alternative estimators

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. Panel (a) reports the results from estimating equations (2) and (5), respectively. Panel (b) plots the results from estimating the dynamic counterparts of the OLS coefficients reported in Table 3, columns (1) and (2) and the reduced-form coefficient reported in Table B.3, columns (4) and (5) using the estimators proposed by Callaway and Sant'Anna (2021), Sant'Anna and Zhao (2020), and De Chaisemartin and d'Haultfoeuille (2020). Panel (c) performs the Rambachan and Roth (2023) test for robustness to violations of the parallel trends assumption of equations (2) and (5), respectively. 95% confidence intervals are based on standard errors robust to clustering at the municipality level.

Figure 3 confirms the overall credibility of the estimates. First, in panel (a) I estimate the OLS model in equation (2) and the reduced-form model in equation (5). The results strongly suggest that, absent the voting system reform, the probability of having a female mayor in municipalities treated at different points in time would have evolved following parallel trends. All estimated coefficients relative to the years preceding the (predicted) reform exposure are small and not significantly different from zero at conventional levels; on the contrary, coefficients estimated after the introduction of the reform are sizable and consistently significant at the 5 percent level. In addition, coefficients estimated in subsequent years appear very stable, suggesting that the 1995 abolition of gender quotas does not affect the results. Second, in panel (b) I show the results obtained by using alternative Difference-in-Differences estimators that account for the issues of TWFE regressions discussed in Section 3. The results indicate that TWFE estimators perform adequately in this context since the estimated coefficients are virtually equivalent across a variety of estimators. This result is not unexpected due to the treatment assignment procedure: as pointed out by Athey and Imbens (2022), TWFE estimators consistently identify the Average Treatment Effect on treated units if the treatment timing is randomly assigned, as it is likely in this context – at least when adopting the 2SLS specification. Third, in panel (c) I perform the test suggested by Rambachan and Roth (2023) to assess the robustness of the estimates to potential violations of the parallel trends assumption. The results document that estimates of equations (2) and (5) would remain positive and statistically significant when allowing for violations up to 1.5 times the largest violation observed in the pre-reform period.

Finally, Appendix Table B.4 reports estimates obtained after collapsing the panel to one observation per municipality and electoral term and controlling for the position of the term in each municipality's electoral sequence instead of calendar-year fixed effects. The resulting estimates are very similar to the baseline findings. The results closely resemble those from the municipality-year panel whether the outcome is the share of years within the term in which the mayor is a woman, the share of mayors serving during the term who are women, or an indicator for the first (respectively, last) mayor of the term.

4.2 Robustness checks

4.2.1 Robustness to the introduction of gender quotas

As discussed in Section 2, the main threat to the validity of the empirical strategy is that the same law that modified the institutional regime also introduced gender quotas that all lists of candidates to the municipal council had to satisfy. The possibility that the empirical results presented in Table 3 reflect the effect of gender quotas – and not the effect of direct election – finds limited support in previous research (Bagues and Campa, 2021; Baltrunaite et al., 2019; Spaziani, 2022). Nevertheless, in this section I propose a

battery of formal tests in which I control for each municipality's quasi-random exposure to the gender quotas reform. If the main results are not confounded by the introduction of gender quotas, one should detect a positive and statistically significant coefficient on the $PostReform_{i,t}$ indicator, which captures the effect of direct election in municipalities not affected by gender quotas.

First, in Column (1) of Table 4, I leverage the sudden abolition of the gender quotas by the Constitutional Court at the end of 1995 by augmenting the specification in equations (3) and (4) with an indicator equal to 1 for elections held during the time window in which gender quotas were prescribed by the law. The introduction of the term $PostReform_{i,t} \times BeforeAbolition_{i,t}$ ensures that the coefficient on $PostReform_{i,t}$ is identified based on a staggered comparison of municipalities that had their first post-reform election after the abolition of gender quotas. The results presented in Column (1) of Table 4 show that the estimated effect of the introduction of direct election remains statistically significant and is not altered by controlling for the term $PostReform_{i,t} \times BeforeAbolition_{i,t}$.

Second, in Columns (2) and (3) of Table 4, I exploit the observation that municipalities were differentially exposed to gender quotas depending on their population size and their pre-existing representation of women in the municipal council. On the one hand, in municipalities with more than 15,000 inhabitants, the gender quota law required all parties to include at least 33 percent female candidates in their list for the municipal council. In smaller municipalities, parties were required to assign at least 25 percent of positions to women. On the other hand, each municipality was differentially exposed to the introduction of quotas depending on whether the proportion of women serving in a municipal council before the introduction of gender quotas was close to (or above) the requirement or smaller than the required share.[23]

Formally, I construct a measure of municipality i's predetermined exposure to gender quotas, $Exposure_i$, defined as the difference between the requirement and the pre-reform share of female councilors (i.e., the share of female councilors in 1992):

$$Exposure_i = \begin{cases} 0.25 - FemaleCouncil_{i,1992} \text{ if } Population_i < 15,000 \\ 0.33 - FemaleCouncil_{i,1992} \text{ if } Population_i \geq 15,000. \end{cases} \quad (6)$$

In Column (2), I then augment the regression models in equations (3) and (4) with the interaction term $PostReform_{i,t} \times Exposure_i$. The results show that the estimated effect of the introduction of direct election remains positive and statistically significant. The

[23] Measuring exposure using the incumbent share of councilors instead of the share of women who ran for the position of council member in the last election prior to the reform is necessary because data on unelected candidates for the municipal council are not available in Italy – therefore, it is not possible to measure the share of female candidates for the position of councilor before the reform. Nevertheless, it is reasonable to assume that the pre-reform share of female councilors is highly correlated with the pre-reform share of female candidates.

negative coefficient on the term $PostReform_{i,t} \times Exposure_i$ suggests that the effect of direct election is smaller in municipalities more exposed to gender quotas. A rationale behind this evidence (carefully investigated in Section 5) is that the supply of potential female candidates was low in municipalities with few (or no) female councilors just before the reform.

In Column (3), I augment the specifications in (3) and (4) with the binary indicator $PostReform_{i,t} \times \mathbb{1}(Exposure_i \geq 0)$. Controlling for $PostReform_{i,t} \times \mathbb{1}(Exposure_i \geq 0)$ ensures that the coefficient on $PostReform_{i,t}$ is identified only in the municipalities for which the share of female councilors was already above the quota requirement prior to the reform. The coefficient on $PostReform_{i,t}$ remains positive – and increases in magnitude compared to Table 3 and to Columns (1) and (2) of Table 4 – although it becomes statistically insignificant since the variable $\mathbb{1}(Exposure_i \geq 0)$ takes the value 0 only in 89 municipalities.

Lastly, in Columns (4) and (5), I jointly exploit the unexpected abolition of gender quotas and the heterogeneity in exposure across municipalities. The coefficient on the $PostReform$ indicator remains positive and statistically significant in both columns.

In Appendix Table B.5, I further refine the $Exposure_i$ measure by leveraging the fact that the effective gender quota requirement – although formally set at 25 percent or 33 percent by the law – in practice varied quasi-randomly across municipalities when the total number of seats in the municipal council was not a multiple of 0.25 or 0.33, respectively. For instance, the effective quota for municipal councils of 40 members is 32.5 percent, while the effective gender quota for municipal councils of 50 members is 34 percent.[24]

Formally, I replicate the analysis presented in Table 4 by replacing $Exposure_i$ with

$$\overline{Exposure_i} = EffectiveQuota_i - FemaleCouncil_{i,1992}, \tag{7}$$

where $EffectiveQuota_i$ is the exact quota requirement to which municipality i was subject. The results presented in Appendix Table B.5 are qualitatively analogous to the results presented in Table 4.

4.2.2 Alternative empirical strategy

In this section, I replicate the results obtained in Table 3 by adopting an alternative empirical strategy. Specifically, I limit the sample to years in which an election occurred and estimate an Interrupted Time Trend model (i.e., a model akin to a regression-discontinuity design suited for exploiting time as the running variable). All terms resulting from elections held in or after 1993 appear to the right of the threshold, while all terms governed

[24] If a municipal council has 40 seats, the quota requirement of 33% amounts to 13.3 seats, rounded at 13. In the case of 50 seats, the quota requirement amounts to 16.6, rounded at 17.

Table 4: Robustness to the introduction of gender quotas

	2SLS				
	(1)	(2)	(3)	(4)	(5)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.025***	0.050***	0.068	0.050***	0.107**
	(0.007)	(0.012)	(0.043)	(0.013)	(0.054)
Post Reform × Before Abolition	0.006			0.004	–0.057
	(0.006)			(0.012)	(0.049)
Post Reform × Exposure		–0.141***		–0.139***	
		(0.047)		(0.053)	
Post Reform × Exposure × Before Abolition				–0.016	
				(0.057)	
Post Reform × 1(Exposure ≥ 0)			–0.045		–0.028
			(0.043)		(0.046)
Post Reform × 1(Exposure ≥ 0) × Before Abolition					–0.063
					(0.049)
Observations	137,718	137,718	137,718	137,718	137,718
R^2	0.001	0.001	0.001	0.001	0.001
Year Effects	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓	✓
Control mean	0.0350	0.0350	0.0350	0.0350	0.0350

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. The estimated equations are (3) and (4) augmented with linear trends specific to each cohort of municipalities. Column (1) includes the interaction term $PostReform_{i,t} \times BeforeAbolition_{i,t}$. Column (2) includes the interaction term $PostReform_{i,t} \times Exposure_i$. Column (3) includes the interaction term $PostReform_{i,t} \times \mathbb{1}(Exposure_i \geq 0)$. Column (4) includes the interaction terms $PostReform_{i,t} \times BeforeAbolition_{i,t}$, $PostReform_{i,t} \times Exposure_i$, and $PostReform_{i,t} \times BeforeAbolition_{i,t} \times Exposure_i$. Column (5) includes the interaction terms $PostReform_{i,t} \times BeforeAbolition_{i,t}$, $PostReform_{i,t} \times \mathbb{1}(Exposure_i \geq 0)$, and $PostReform_{i,t} \times BeforeAbolition_{i,t} \times \mathbb{1}(Exposure_i \geq 0)$. $BeforeAbolition_{i,t}$ is an indicator variable taking values 1 for all elections held in 1993 or after 1993 and before September 6, 1995. $Exposure$ is defined as in (6) and $\mathbb{1}(Exposure_i \geq 0)$ takes the value 1 if $Exposure_i \geq 0$. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

under the previously adopted indirect-election regime lie to the left of the threshold. Compared to the main identification strategy presented in this paper, this alternative comes with two main caveats that motivate the decision not to adopt it as the main strategy. First, this strategy would not allow me to control for calendar-year fixed effects: that is, if other shocks or national reforms hit all municipalities at the same time as this institutional reform, the results would be biased as they would reflect the effects of compound treatments. Second, this strategy allows me to evaluate the impact of the reform on the gender distribution of mayors selected immediately after a new election: for this reason, this approach would underestimate the underlying causal effect of the reform if female mayors were more likely than male mayors to be replaced during the term by a challenger of opposite gender under the pre-reform regime, as the evidence presented

in Figure 2 documents. Yet, documenting whether the effects persist when focusing only on the first mayor elected during a term is informative about the mechanisms underlying the main result.

After restricting the sample to election years, I obtain a sample of approximately 32,500 municipality-term observations, and estimate the following model:

$$FemaleMayor_{i,t} = \beta PostReform_{i,t} + \gamma_1 DistToReform_{i,t} + \gamma_2 PostReform_{i,t} \times DistToReform_{i,t} + \varepsilon_{i,t}. \quad (8)$$

In this context, $FemaleMayor_{i,t}$ takes the value 1 if the first mayor appointed after the election held in municipality i at time t is a woman and zero otherwise, while $DistToReform$ is the distance (in calendar years) between the election year and 1993. The linear terms on either side of the 1993 threshold absorb the secular evolution in the probability of electing a female mayor, which increased gradually over the sample period. The parameter of interest is β, which captures the discontinuous change at the threshold after accounting for the underlying pre- and post-reform trends.

The results are summarized in Figure 4. Each marker represents the share of women among first mayors selected following elections held in each year, while marker size reflects the number of elections held in each year (see Appendix Figure B.1). There is a sizable and statistically significant jump coinciding with the 1993 threshold. Formal estimates of equation (8) show that, according to this approach, the reform increases the proportion of female mayors by approximately 1.3 percentage points (statistically significant at the 1% level). The estimated effect is smaller than the estimate from the most conservative OLS specification in Table 3: this evidence is consistent with the observation that, under the old institutional regime, female mayors were relatively more likely to be unseated and replaced by the municipal council during the term.[25]

4.2.3 Additional robustness checks

In Appendix A, I address several other empirical concerns. First, in Appendix Figure A.1, I perform a placebo test in which I assume a reform took place every year between 1990 and 2000. The estimated coefficients are never statistically significant for the placebo reforms, whereas the only positive and significant estimate corresponds to the actual reform. This evidence provides further reassurance against the possibility that the main effects are confounded by gender quotas, since the abolition of quotas in 1995 is not associated with any statistically significant effect. Second, in Appendix Table A.1 and Appendix Table A.2, I show that the results are not confounded by common shocks that

[25] This observation is consistent with the results obtained by Huidobro and Falcó-Gimeno (2023) in the context of Spain and by Gagliarducci and Paserman (2012) in the context of post-reform Italian municipalities.

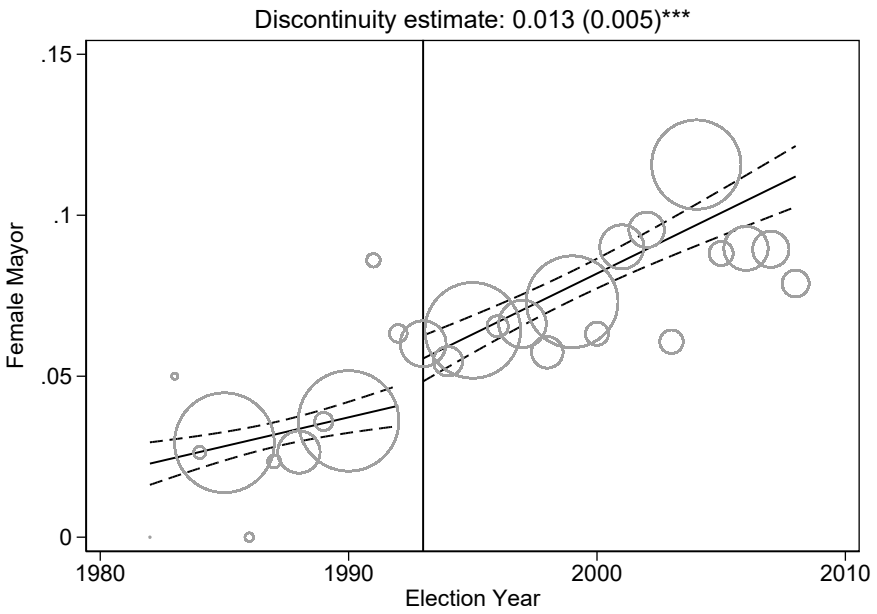


Figure 4: Alternative empirical strategy: Interrupted Time Trend

Notes: The dependent variable is an indicator equal to 1 if the first mayor selected following the municipal election is a woman. Estimation method: OLS as in equation (8). 95% confidence intervals are based on standard errors robust to clustering at the municipality level.

affected all municipalities at the same time around the implementation of the reform such as the introduction of a local property tax and the dissolution of Italy's largest parties.

5 Mechanisms

Besley et al. (2017) and Baltrunaite et al. (2014) show that policies increasing women's political representation can improve political selection by displacing lower-quality male politicians.[26] This "crisis of the mediocre man" is consistent with a broader competence–loyalty trade-off in political selection (e.g., Egorov and Sonin, 2011; Zakharov, 2016): political organizations value competence, but may also reward politicians who are embedded in the organization, adhere to its objectives, and can be relied upon to sustain political agreements. In this section, I examine whether a related selection mechanism helps explain why the transition from council appointment to direct election raises female leadership in Italian municipalities.

In this setting, accumulated political experience can be seen as an observable form of insider political capital. Both voters and elected politicians may value experience insofar as it improves expected performance in office. Council members, however, may

[26] Passarelli and Boto-García (2025) find that gender quotas increased women's representation but had, on average, no statistically significant effect on the educational composition of elected politicians in Spanish municipal councils.

value it for an additional reason: a long political career reveals party embeddedness, adherence to shared political objectives, and reliability in council bargaining. These attributes are directly valuable to fellow politicians even when they do not translate into better policy outcomes. Voters, by contrast, have no direct reason to reward this insider political capital beyond its contribution to expected government performance. Consistent with this distinction, evidence from Norwegian local governments suggests that political experience has limited effects on government efficiency (Sørensen, 2023).

Direct election may consequently reduce the relative advantage associated with accumulated insider experience and increase the return to observable indicators of competence, such as education. Because local political careers had historically been heavily male-dominated, men entered the reform period with substantially more political experience and were therefore more likely to possess the insider political capital rewarded under council-based appointment. Women were fewer and less experienced, but the women who had entered local politics were more positively selected on formal education.

The preceding argument yields three complementary empirical implications. First, the institutional reform cannot increase female leadership where no women are available to benefit from the change in relative returns. The effect should therefore be concentrated in municipalities where women had already entered local politics and accumulated sufficient experience to be plausible candidates for the mayoralty. I test this implication using the pre-reform representation of women in municipal councils as a proxy for the local pool of potential female leaders.

Second, if direct election reduces the relative advantage associated with insider political capital and increases the return to observable competence, its effect should depend on the type of incumbent selected under the pre-reform regime. Educational attainment can be interpreted as a proxy for the mayor's productivity in office and for voters' perceptions of her ability. Where the incumbent was already highly educated, the council-based system had selected a politician who also possessed the observable qualification that voters might reward under direct election, leaving comparatively little scope for changing the type of mayor in office. Where the incumbent had lower formal education, by contrast, the reform creates greater scope for replacing a politician whose attractiveness to council members may have rested relatively more on accumulated experience and insider political capital. I therefore test whether the increase in female leadership is larger where the pre-reform incumbent was less educated.

Third, the proposed mechanism implies that the reform should affect political selection more broadly rather than merely inducing a mechanical substitution of women for men. If direct election raises the relative return to observable competence, it should increase the representation of more-educated mayors among both women and men and reduce the representation of less-educated mayors.

5.1 Reform and the local pool of potential female leaders

I first examine whether the effect of direct election depends on the presence of women who had already entered local politics. Because formal mayoral candidacies did not exist under the pre-reform regime, I cannot directly observe the pool of women who might have competed for the mayoralty under direct election. I therefore use the pre-reform representation of women in the municipal council as a proxy for the local political pipeline of potential female mayoral candidates.[27] Council members were plausible candidates for the mayoralty because they had already received individual preference votes, accumulated political experience, and demonstrated their abilities to parties and voters.[28]

In panel (a) of Figure 5, I compare municipalities where at least one woman was serving as a member of the municipal council before the reform with municipalities where only men served in the municipal council. The results show that the introduction of a direct election regime increases the proportion of female mayors especially in municipalities where at least one woman was a member of the municipal council before the reform. The results of this exercise document that direct election contributes to the election of female mayors mainly in municipalities where a pool of politically experienced women from which parties and voters could select already existed. By contrast, in municipalities where no women were active in the political arena, the reform does not lead to any statistically significant increase in the proportion of female mayors.

In panel (b) of Figure 5, I further explore this mechanism by comparing, among municipalities where at least one woman was serving as a member of the council before the reform, municipalities where the pool of councilors was relatively more vs. less gender-balanced. Specifically, I interact the baseline regression models with three indicators that measure the proportion of female members of municipal councils before the reform. Formally, I estimate

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \sum_{k \in [1,3]} \alpha_k PostReform_{i,t} \times \mathbb{1}(FemaleCouncil_{i,1992} = k) + \varepsilon_{i,t}, \tag{9}$$

where $\mathbb{1}(FemaleCouncil_{i,1992} = 1)$ takes the value 1 for municipalities in the bottom 25 percent of the pre-reform female representation in the council, $\mathbb{1}(FemaleCouncil_{i,1992} = 2)$ takes the value 1 for municipalities in the middle 50 percent of the distribution of pre-

[27] Greater female representation in the council indicates that more politically experienced women were available to advance to the mayoralty, but it may also reflect stronger demand for female politicians among voters and parties, or more favorable gender norms. The heterogeneity analysis therefore tests whether direct election is more effective where a female political pipeline already existed; it does not disentangle whether that pipeline arose from the supply of women entering politics or from the willingness of voters and parties to support them.

[28] Franzoni (2023) documents that 82 percent of mayors had previously held lower-ranked positions within the municipal administration.

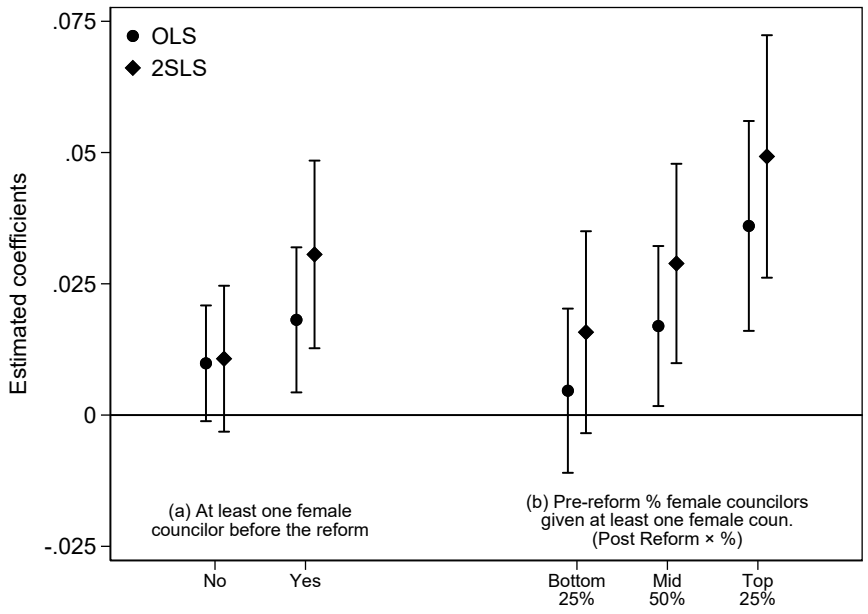


Figure 5: Heterogeneous effects by pre-reform share of women in the municipal council

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. Panel (a): coefficients and confidence intervals are obtained by estimating the OLS model in equation (1) and the 2SLS model in equations (3) and (4) in the subsamples of municipalities where, before the reform, i) no women were serving as members of the municipal council and ii) at least one woman was serving as a member of the municipal council. Panel (b): coefficients and confidence intervals are obtained by estimating the model in equations (9) using OLS and 2SLS, respectively. The specifications include linear time trends specific to each cohort of municipalities as in columns (2) and (5) of Table 3. Circle markers represent OLS estimates while diamond markers represent 2SLS estimates. 95% confidence intervals are based on standard errors robust to clustering at the municipality level.

reform female representation, and $\mathbb{1}(FemaleCouncil_{i,1992} = 3)$ takes the value 1 for municipalities in the top quartile of pre-reform female representation. The results presented in panel (b) of Figure 5 document that the reform increases the proportion of female mayors significantly more in municipalities where the pre-existing pool of politically experienced women was larger. Specifically, the results show that direct election has a small and statistically insignificant impact in municipalities belonging to the bottom 25 percent, while it raises the probability of selecting a female mayor by 2.5 percentage points in municipalities in the middle part of the distribution and by approximately twice as much in municipalities in the top quartile.[29]

Appendix Table B.7 provides complementary evidence on the local conditions under which direct election increases female leadership. The effect of the reform is significantly smaller in municipalities characterized by more traditional pre-reform gender norms. Across alternative proxies, a one-standard-deviation increase in traditional gender

[29]The results presented in Figure 5 are also useful for separating the effect of the direct election regime from any confounding effect induced by the introduction of gender quotas. Specifically, the effect is statistically insignificant in municipalities most affected by the introduction of quotas and largest in municipalities arguably less affected by quotas.

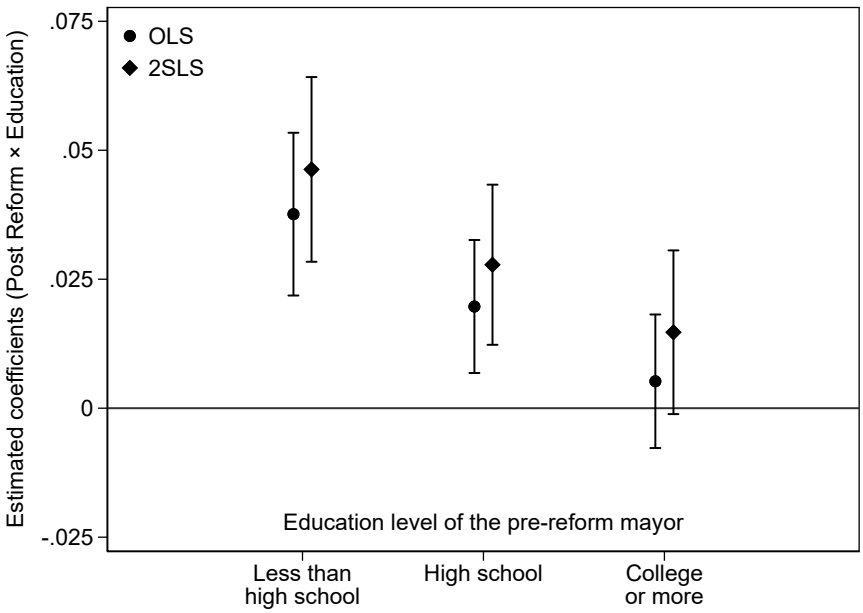


Figure 6: Heterogeneous effects by pre-reform incumbent mayor's educational attainment

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. Coefficients and confidence intervals are obtained by estimating the model in equations (10) using OLS and 2SLS, respectively. The specifications include linear time trends specific to each cohort of municipalities as in columns (2) and (5) of Table 3. Circle markers represent OLS estimates while diamond markers represent 2SLS estimates. 95% confidence intervals are based on standard errors robust to clustering at the municipality level.

norms reduces the estimated effect by approximately 0.7–1.0 percentage points.

5.2 Incumbent education and the selection of female mayors

In Figure 6, I exploit heterogeneity across municipalities in the pre-reform educational attainment of mayors. Formally, I estimate using OLS and 2SLS

$$FemaleMayor_{i,t} = \eta_i + \delta_t + \sum_{k\in[1,3]} \alpha_k PostReform_{i,t} \times \mathbb{1}(Education_{i,1992} = k) + \varepsilon_{i,t}, \tag{10}$$

where $\mathbb{1}(Education_{i,1992} = 1)$ takes the value 1 in municipalities where the pre-reform incumbent mayor did not complete high-school (and zero otherwise), $\mathbb{1}(Education_{i,1992} = 2)$ takes the value 1 if the pre-reform incumbent held a high-school degree as the highest educational attainment, and $\mathbb{1}(Education_{i,1992} = 3)$ takes the value 1 if the pre-reform incumbent mayor held at least a college degree.

The results presented in Figure 6 are consistent with the mechanism proposed at the beginning of this section. The voting system reform has a small and statistically

insignificant effect on the probability of electing a female mayor in municipalities where the incumbent mayor in 1992 held a college degree. Conversely, the treatment effect of the reform is driven by municipalities in which, before the reform, a mayor without a college degree was in office. More specifically, in municipalities where the pre-reform incumbent held a high-school degree as the highest educational attainment, direct election increases the probability of electing a female mayor by 3 percentage points. In municipalities where the pre-reform incumbent had not completed high-school, the reform increases the probability of electing a female mayor by 5 percentage points. The estimated coefficients for $PostReform_{i,t} \times \mathbb{1}(Education_{i,1992} = 2)$ and $PostReform_{i,t} \times \mathbb{1}(Education_{i,1992} = 1)$ are significantly different from each other and both significantly different from zero.[30]

Appendix Table B.8 provides complementary evidence based on the pre-reform educational attainment of the local population. A one-standard-deviation increase in the share of the population holding a high-school degree as of the 1991 census raises the estimated effect of direct election by approximately 0.6 percentage points. The gradient is largest where the pre-reform incumbent had not completed high-school, consistent with observable candidate qualifications becoming particularly consequential where there was greater scope for replacing a less-educated incumbent.[31]

5.3 Direct election improves the quality of mayors

In Figure 7, I estimate the impact of the reform on the educational attainment of the mayor, separately by gender. More specifically, in Panel (a) I estimate the model presented in equation (5) using as the dependent variables an indicator equal to 1 if the mayor is a man holding at least a high-school degree (and zero otherwise) – black coefficients and confidence intervals – and an indicator equal to 1 if the mayor is a woman holding at least a high-school degree (and zero otherwise) – red coefficients and confidence intervals. Similarly, in Panel (b) I estimate (5) using as the dependent variables an indicator taking the value 1 if the mayor is a man without a high-school degree, and an indicator equal to 1 if the mayor is a woman without a high-school degree, respectively.

[30] A growing literature documents gender differences in political persistence and the role of incumbency in shaping women's access to office (Baskaran and Hessami, 2022; Lippmann, 2023; Nzabonimpa, 2023; Wasserman, 2023). In the present setting, however, the pattern cannot be explained by selective withdrawal of pre-reform incumbents from the first direct election alone. On average, 48.6 percent of pre-reform incumbent mayors decided to run for office in the first post-reform term. However, male incumbents were slightly more likely to recontest than female incumbents, with rates of 49.2 and 40.5 percent, respectively. Moreover, recontest rates were highest among the less-educated incumbents for whom the estimated effect is largest: 56.5 percent of incumbents without a high-school degree reran, compared with 50.7 percent of those with a high-school degree and 45.0 percent of college graduates.

[31] Appendix Table B.6 reports larger effects in Northern municipalities than in Central and Southern Italy. I interpret this pattern as descriptive geographic heterogeneity rather than as evidence of an autonomous regional mechanism, since macro-regions differ along the same predetermined dimensions examined more directly in this section, including the proportion of women who were active in politics, the educational attainment of the population and incumbent mayors, and local gender norms.

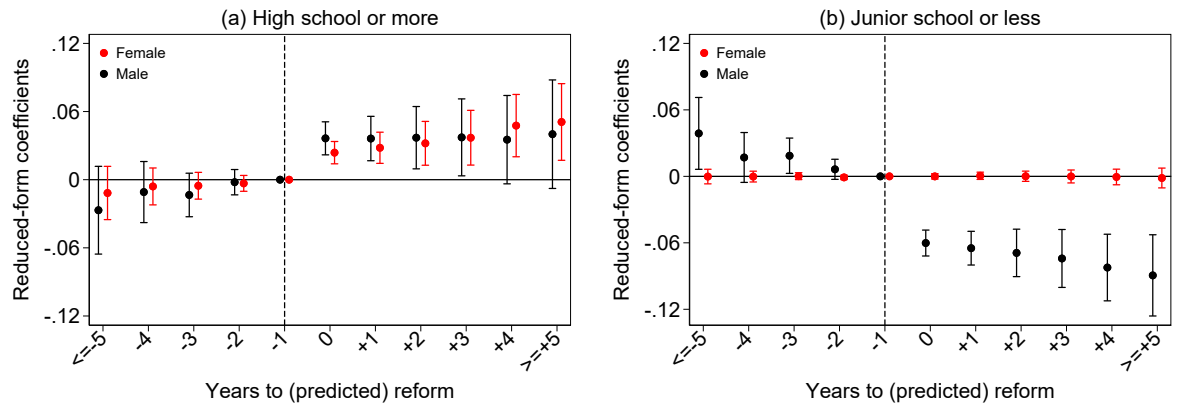


Figure 7: Direct election of leaders and educational attainment of male and female mayors

Notes: This figure reports the results from estimating equation (5) using as dependent variables an indicator equal to 1 if the mayor is a female politician who holds at least a high-school degree (panel (a), red markers); an indicator equal to 1 if the mayor is a male politician who holds at least a high-school degree (panel (a), black markers); an indicator equal to 1 if the mayor is a female politician who neither holds a high-school degree nor a college degree (panel (b), red markers); an indicator equal to 1 if the mayor is a male politician who has not completed high-school. 95% confidence intervals are based on standard errors robust to clustering at the municipality level.

The results presented in Figure 7 show that the reform significantly increases the representation of both female and male mayors with at least a high-school degree. Conversely, the reform significantly reduces the probability of electing a male mayor without a high-school degree.[32] Although coefficients estimated before the reform in Panel (b) of Figure 7 show a minor violation of the parallel trends assumption, the jump in the outcome exactly in the year of exposure to the reform is sizable and much larger than any pre-reform imbalance.

In Appendix Table B.9, I also document how the reform affected other individual characteristics of mayors as well as their survival rate in office. Specifically, I document that mayors elected after the reform are more likely to have previously worked in high-skill occupations and to be re-elected after a first term in office.[33]

6 Concluding remarks

This paper investigates the impact of direct election of political leaders on the selection of female leaders by exploiting the 1993 reform of the voting regime to select mayors in Italian municipalities. The results document that the introduction of a direct election scheme increased the proportion of female mayors from around 3.5 percent to approximately 6

[32]The reform does not have any effect on the probability of electing a less educated female mayor because virtually all women who were in office before or after the reform held at least a high-school degree.

[33]This evidence provides further reassurance against the possibility that the main results merely reflect the reform weakening the incumbency advantage.

percent. The effect is not uniform across municipalities: it is small and statistically insignificant in municipalities where women were least represented in the municipal council before the reform, and it increases monotonically with pre-reform female council representation. More broadly, the results of this paper show that institutional reforms governing how leaders are selected appear to act as a complement to, rather than a substitute for, policies aimed at increasing the representation of women at lower tiers of political office, such as gender quotas in multi-member assemblies.

Moreover, newly elected female mayors are more likely to replace less-educated incumbents, while direct election has no statistically significant effect in municipalities where the pre-reform mayor held a college degree. Male and female mayors entering office after the reform were, on average, better educated than mayors serving before the reform. The reform also increased incumbent re-election, providing complementary evidence that it did not operate through a generalized weakening of political persistence. This pattern is consistent with the two selection regimes rewarding different attributes. Council-based appointment places relatively greater weight on accumulated insider political capital, which men had disproportionately acquired because local political careers had long been dominated by men; direct election raises the relative return to qualifications that voters can observe and reward. Notably, this mechanism does not require any intrinsic gender difference in party loyalty or commitment but only the historically unequal accumulation of political experience.

The results of this paper also highlight a political selection channel that may help explain documented fiscal policy differences between presidential and parliamentary democracies. Earlier studies (Persson, 2002; Persson and Tabellini, 2003) focus mainly on the impact of the different incentives that the two constitutional regimes give to otherwise similar agents, while this paper documents that the two regimes also select different individuals into political leadership. While I do not estimate the effect of the reform on fiscal outcomes directly, the compositional change is substantial and affects both genders: the reform increased the share of educated mayors among women and among men alike and reduced the share of male mayors without a high-school degree by 6 percentage points.

The results presented in this paper relate to the existing debate in the literature about the determinants of gender gaps in politics, with explanations that range from gender differences in attitudes towards competition to party leadership bias and voter bias. The evidence that direct election improves the representation of women indicates that the barriers arising with council-based selection (Esteve-Volart and Bagues, 2012; Huidobro and Falcó-Gimeno, 2023) were sufficiently important that transferring control over executive selection from political insiders to voters produced a more gender-balanced outcome, even though women may still face disadvantages at the ballot box (Cipullo, 2021; Savio, 2024). Identifying the institutional arrangements that ease women's access to executive office matters beyond descriptive representation, as the gender composition of

political office can affect the policies that governments implement (Baskaran and Hessami, 2025).

References

Acconcia, A. and Ronza, C. (2026). The Stabilizing Effect of Elected Women and Political Seniority. *European Journal of Political Economy*, page 102820.

Acemoglu, D. (2005). Constitutions, Politics, and Economics: a Review Essay on Persson and Tabellini's The Economic Effects of Constitutions. *Journal of Economic Literature*, 43(4):1025–1048.

Athey, S. and Imbens, G. W. (2022). Design-Based Analysis in Difference-in-Differences Settings with Staggered Adoption. *Journal of Econometrics*, 226(1):62–79.

Bagues, M. and Campa, P. (2020). Women and Power: Unpopular, Unwilling, or Held Back? A Comment. *Journal of Political Economy*, 128(5):2010–2016.

Bagues, M. and Campa, P. (2021). Can Gender Quotas in Candidate Lists Empower Women? Evidence from a Regression Discontinuity Design. *Journal of Public Economics*, 194:104315.

Ballini, P. L. (2010). La Rifondazione della Democrazia nei Comuni: la Legge Elettorale Amministrativa e le Elezioni Comunali del 1946. In *Le Autonomie Locali dalla Resistenza alla I Legislatura della Repubblica*, pages 369–432. Rubbettino.

Baltrunaite, A., Bello, P., Casarico, A., and Profeta, P. (2014). Gender Quotas and the Quality of Politicians. *Journal of Public Economics*, 118:62–74.

Baltrunaite, A., Casarico, A., Profeta, P., and Savio, G. (2019). Let the Voters Choose Women. *Journal of Public Economics*, 180:104085.

Baskaran, T. and Hessami, Z. (2018). Does the Election of a Female Leader Clear the Way for More Women in Politics? *American Economic Journal: Economic Policy*, 10(3):95–121.

Baskaran, T. and Hessami, Z. (2022). The Gender Recontest Gap in Elections. *European Economic Review*, 145:104111.

Baskaran, T. and Hessami, Z. (2025). Women in Political Bodies as Policymakers. *The Review of Economics and Statistics*, 107(6):1501–1517.

Beaman, L., Duflo, E., Pande, R., and Topalova, P. (2012). Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India. *Science*, 335(6068):582–586.

Bell, C., Besaw, C., and Frank, M. (2021). The Rulers, Elections, and Irregular Governance (REIGN) Dataset.

Besley, T. (2005). Political selection. *Journal of Economic Perspectives*, 19(3):43–60.

Besley, T., Folke, O., Persson, T., and Rickne, J. (2017). Gender Quotas and the Crisis of the Mediocre Man: Theory and Evidence from Sweden. *American Economic Review*, 107(8):2204–42.

Besley, T., Montalvo, J. G., and Reynal-Querol, M. (2011). Do Educated Leaders Matter? *The Economic Journal*, 121(554):F205–F227.

Bianchi, N., Giorcelli, M., and Martino, E. M. (2023). The Effects of Fiscal Decentralization on Publicly Provided Services and Labor Markets. *Economic Journal*, 133(653):1738–1772.

Bordignon, M., Gamalerio, M., and Turati, G. (2020). Manager or Professional Politician? Local Fiscal Autonomy and the Skills of Elected Officials. *Regional Science and Urban Economics*, 83:103529.

Bordignon, M., Nannicini, T., and Tabellini, G. (2016). Moderating Political Extremism: Single Round versus Runoff Elections under Plurality Rule. *American Economic Review*, 106(8):2349–2370.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics*, 225(2):200–230.

Carozzi, F., Cipullo, D., and Repetto, L. (2022). Political Fragmentation and Government Stability: Evidence from Local Governments in Spain. *American Economic Journal: Applied Economics*, 14(2):23–50.

Carozzi, F., Cipullo, D., and Repetto, L. (2024). Powers That Be? Political Alignment, Government Formation, and Government Stability. *Journal of Public Economics*, 230:105017.

Casas-Arce, P. and Saiz, A. (2015). Women and Power: Unpopular, Unwilling, or Held Back? *Journal of Political Economy*, 123(3):641–669.

Chattopadhyay, R. and Duflo, E. (2004). Women as Policy Makers: Evidence from a Randomized Policy Experiment in India. *Econometrica*, 72(5):1409–1443.

Cipullo, D. (2021). Gender Gaps in Political Careers: Evidence from Competitive Elections. CESifo Working Paper No. 9075.

Coate, S. and Knight, B. (2011). Government Form and Public Spending: Theory and Evidence from US Municipalities. *American Economic Journal: Economic Policy*, 3(3):82–112.

Council of Europe (2002). Women in Local and Regional Political Life.

Daniele, G., Galletta, S., and Geys, B. (2020). Abandon Ship? Party Brands and Politicians' Responses to a Political Scandal. *Journal of Public Economics*, 184:104172.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–96.

De Paola, M., Scoppa, V., and Lombardo, R. (2010). Can Gender Quotas Break Down Negative Stereotypes? Evidence from Changes in Electoral Rules. *Journal of Public Economics*, 94(5-6):344–353.

Egorov, G. and Sonin, K. (2011). Dictators and their Viziers: Endogenizing the Loyalty–Competence Trade-off. *Journal of the European Economic Association*, 9(5):903–930.

Enikolopov, R. (2014). Politicians, Bureaucrats and Targeted Redistribution. *Journal of Public Economics*, 120:74–83.

Esteve-Volart, B. and Bagues, M. (2012). Are Women Pawns in the Political Game? Evidence from Elections to the Spanish Senate. *Journal of public Economics*, 96(3-4):387–399.

Ferreira, F. and Gyourko, J. (2014). Does Gender Matter for Political Leadership? The Case of US Mayors. *Journal of Public Economics*, 112:24–39.

Fox, R. L. and Lawless, J. L. (2004). Entering the Arena? Gender and the Decision to Run for Office. *American Journal of Political Science*, 48(2):264–280.

Franzoni, F. (2023). Essays on Political Economics. Ph. D. Thesis, Università Cattolica del Sacro Cuore.

Gagliarducci, S. and Paserman, M. D. (2012). Gender Interactions within Hierarchies: Evidence from the Political Arena. *The Review of Economic Studies*, 79(3):1021–1052.

Gonzalez-Eiras, M. and Sanz, C. (2021). Women's Representation in Politics: The Effect of Electoral Systems. *Journal of Public Economics*, 198:104399.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics*, 225(2):254–277.

Hessami, Z. (2018). Accountability and Incentives of Appointed and Elected Public Officials. *Review of Economics and Statistics*, 100(1):51–64.

Hessami, Z. and da Fonseca, M. L. (2020). Female Political Representation and Substantive Effects on Policies: A Literature Review. *European Journal of Political Economy*, 63:101896.

Huidobro, A. and Falcó-Gimeno, A. (2023). Women Who Win but do not Rule. The Effect of Gender in the Formation of Governments. *Journal of Politics*, 85(4):1562–1568.

Inter-Parliamentary Union (2006). Women in Politics: 1945–2005.

Kansikas, C. and Bagues, M. (2026). Gender Equality through Turnover: Quasi-Experimental Evidence from Term Limit Reforms in Italy. *Journal of Public Economics*, 259:105667.

Le Barbanchon, T. and Sauvagnat, J. (2022). Electoral Competition, Voter Bias, and Women in Politics. *Journal of the European Economic Association*, 20(1):352–394.

Lippmann, Q. (2023). Persistence of Incumbents and Female Access to Political Positions. *Journal of Economic Behavior & Organization*, 206:327–349.

Meriläinen, J. (2022). Political Selection and Economic Policy. *The Economic Journal*, 132(648):3020–3046.

Mitra, A. (2026). Should You Want an Educated Mayor? Evidence from Close Elections in Italy. *European Journal of Political Economy*, 92:102636.

Nzabonimpa, M. (2023). Gender Differences in Politician Persistence and Incumbency Advantage. *European Journal of Political Economy*, 79:102404.

O'Brien, D. Z. and Rickne, J. (2016). Gender Quotas and Women's Political Leadership. *American Political Science Review*, 110(1):112–126.

Passarelli, F. and Boto-García, D. (2025). Gender Quotas and Politicians' Education. *Journal of Economic Behavior & Organization*, 239:107260.

Persson, T. (2002). Do Political Institutions Shape Economic Policy? *Econometrica*, 70(3):883–905.

Persson, T. and Tabellini, G. (2003). The Economic Effects of Constitutions: What Do the Data Say. MIT press.

Profeta, P. and Woodhouse, E. F. (2021). Electoral Rules, Women's Representation and the Qualification of Politicians. *Comparative Political Studies*, 55(9):1471–1500.

Rambachan, A. and Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, 91(2):743–747.

Repetto, L. (2018). Political Budget Cycles with Informed Voters: Evidence from Italy. *The Economic Journal*, 128(616):3320–3353.

Sant'Anna, P. H. and Zhao, J. (2020). Doubly Robust Difference-in-Differences Estimators. *Journal of Econometrics*, 219(1):101–122.

Savio, G. (2024). Beyond the Party Push: Gender Differences in Voters' Persuasion. *European Journal of Political Economy*, 85:102613.

Sørensen, R. J. (2023). Educated Politicians and Government Efficiency: Evidence from Norwegian Local Government. *Journal of Economic Behavior & Organization*, 210:163–179.

Spaziani, S. (2022). Can Gender Quotas Break the Glass Ceiling? Evidence from Italian Municipal Elections. *European Journal of Political Economy*, 75:102171.

Wasserman, M. (2023). Gender Differences in Politician Persistence. *The Review of Economics and Statistics*, 105(2):275–291.

World Bank (2024). Proportion of Seats Held by Women in National Parliaments (%). World Bank Gender Data Portal.

Zakharov, A. V. (2016). The Loyalty-Competence Trade-off in Dictatorships and Outside Options for Subordinates. *The Journal of Politics*, 78(2):457–466.

Appendix

A Robustness checks

A.1 Placebo reforms in any year between 1990 and 2000

This section documents that no other statistically significant effects can be estimated by applying the same regression model to other *placebo* reforms that would have occurred in any year between 1990 and 2000. The results in Appendix Figure A.1 also confirm that the estimates are not confounded by gender quotas for selecting members of the municipal council since their abolition (after the 1995 election but before the 1996 election) does not cause any statistically significant effect. The coefficient for the placebo reform occurring in 1996 is indeed precisely estimated at zero.

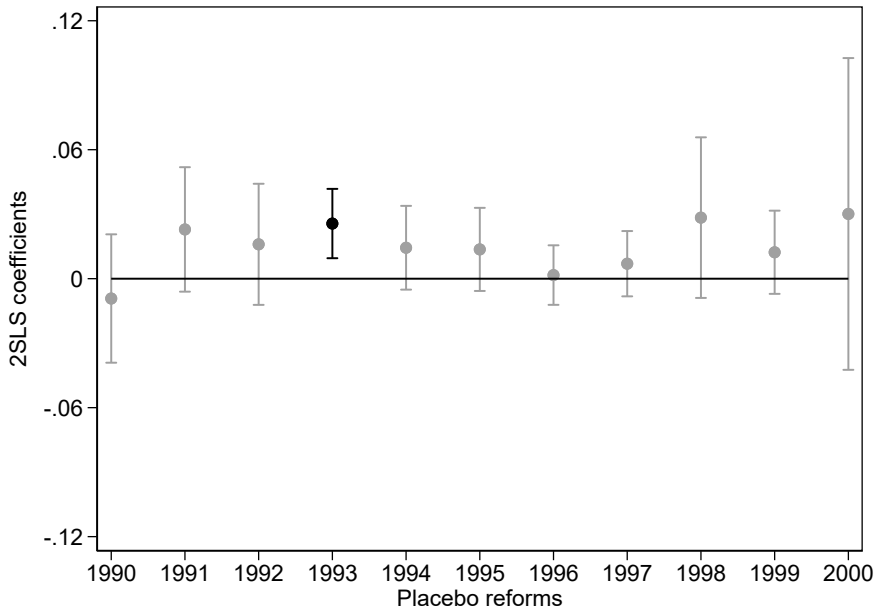


Figure A.1: Placebo coefficients

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. Estimation method: 2SLS as in equations (3) and (4) where, at each iteration, the treatment indicator $PostReform_{i,t}$ has been replaced with an indicator $\mathbb{1}(\widehat{ElYear}_{i,t} \geq k)$, where each $k \in \{1990, \ldots, 2000\}$ is specified in the horizontal axis label. Consistently, the instrument $\mathbb{1}(ElYear_{i,t-5} \geq 1988)$ has been replaced with an indicator $\mathbb{1}(ElYear_{i,t-5} \geq k-5)$. The specifications include linear time trends specific to each cohort of municipalities as in columns (2) and (5) of Table 3. 95% confidence intervals are based on standard errors robust to clustering at the municipality level. The coefficient for 1993 represents the estimated effect of direct election presented in column (5) of Table 3.

A.2 Robustness to other shocks that hit municipalities around the timing of the reform: property tax

This section excludes the possibility that the results are driven by the concurrent introduction of a municipal property tax that has previously been shown to affect political selection (Bordignon et al., 2020). The introduction of the municipal property tax ICI should not affect the estimates since it was introduced in 1993 throughout the country. Nevertheless, in Table A.1, I augment the specification with controls for predictors of each municipality's exposure to the newly-introduced local property tax used in the literature (Bianchi et al., 2023; Bordignon et al., 2020) interacted with time dummies. Specifically, I add controls for i) the pre-reform transfers received by the municipality, ii) the share of buildings constructed before WWII, iii) the provincial GDP in 1992, and iv) the revenues from a temporary property tax levied in 1992, all interacted with time dummies.

Table A.1: Robustness to introduction of local property tax

	2SLS					
	(1)	(2)	(3)	(4)	(5)	(6)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.024***	0.024***	0.025***	0.023***	0.025***	0.023***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	116,671	116,707	137,065	137,632	134,451	116,192
R^2	0.001	0.001	0.001	0.003	0.001	0.004
Year Effects	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓	✓	✓
Transfers1992 × Year	✓					✓
Yearly Transfers		✓				✓
Share Pre-WW2 Buildings × Year			✓			✓
Province GDP × Year				✓		✓
ISI1992 × Year					✓	✓
Control mean	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. In column (1), the specification is augmented with the interaction between the total amount of transfers received by the municipality in 1992 and year dummies; in column (2), the specification is augmented with controls for the yearly total amount of transfers received by the municipality; in column (3), the specification is augmented with the interaction between the share of housing built before WWII and year dummies; in column (4), the specification is augmented with the interaction between provincial GDP and year dummies; in column (5), the specification is augmented with the interaction between the amount of municipality tax revenues from the 1992 version of the property tax and year dummies. Finally, in column (6), the specification is augmented with all the controls included in columns (1)–(5). Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

A.3 Robustness to other shocks that hit municipalities around the timing of the reform: dissolution of largest parties

This section excludes the possibility that the empirical results are driven by the quasi-concurrent dissolution of Italy's largest parties. Three of the main parties that shared government responsibilities at the national or local level in post-World War II Italy dissolved after the end of the Soviet Union and a major corruption scandal that hit many national political leaders in the early 1990s. Daniele et al. (2020) estimate that mayors belonging to parties hit by the corruption scandal were more likely to be unseated and replaced than mayors belonging to other parties. The dissolution of the major parties should not affect the estimates since none of them were dissolved exactly at the same time as the voting system reform (i.e., after the 1992 local election and before the 1993 local election).

Table A.2: Robustness to dissolution of major parties

	2SLS			
	(1)	(2)	(3)	(4)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.030***	0.027***	0.024***	0.027***
	(0.008)	(0.007)	(0.007)	(0.008)
Post Reform × DC Mayor 1990	–0.006			–0.004
	(0.006)			(0.007)
Post Reform × PCI Mayor 1990		–0.015		–0.015
		(0.015)		(0.015)
Post Reform × PSI Mayor 1990			0.015**	0.012
			(0.007)	(0.009)
Observations	136,457	136,457	136,457	136,457
R^2	0.001	0.001	0.001	0.001
Year Effects	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓
Control mean	0.0340	0.0340	0.0340	0.0340

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. In column (1), the specification is augmented with the interaction between an indicator equal to 1 if the mayor serving during the year 1990 belonged to the Christian Democratic party (DC) and the $PostReform_{i,t}$ dummy; in column (2), the specification is augmented with the interaction between an indicator equal to 1 if the mayor serving during the year 1990 belonged to the Italian Communist Party (PCI) and the $PostReform_{i,t}$ dummy; in column (3), the specification is augmented with the interaction between an indicator equal to 1 if the mayor serving during the year 1990 belonged to the Italian Socialist Party (PSI) and the $PostReform_{i,t}$ dummy. All interaction terms are instrumented as in the main specification. Finally, in column (4), the specification is augmented with all the controls included in columns (1)–(3). Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

Nevertheless, in Table A.2, I interact the model with an indicator equal to 1 if the incumbent mayor in 1992 was a member of the Christian Democrats (DC), the Communist

Party (PCI), or the Socialist Party (PSI), respectively, to provide reassurance that the results are not driven by the replacement of incumbent mayors that belonged to parties that were dissolved during their period in office.

B Additional figures and tables

Table B.1: Length of mayor's office by gender

	(1)	(2)	(3)	(4)
	Length of mayoral term			log(Length)
Female	–83.292***	–134.475***	–94.156***	–0.079***
	(26.218)	(29.450)	(27.578)	(0.029)
Observations	13,800	13,800	13,800	13,800
R^2	0.001	0.768	0.796	0.745
Year Effects		✓	✓	✓
Municipality Fixed Effects		✓	✓	✓
Age and education			✓	✓
Control mean	1481	1481	1481	7.204

Notes: The table reports descriptive associations between the length of mayors' spell in office and observable characteristics of mayors serving before the introduction of direct mayoral elections. The unit of observation is a term. Column (1) reports the results from a regression in which the length (in days) of the mayoral term is regressed against the gender of the mayor. Column (2) augments the specification in column (1) with municipality and election-year fixed effects. Column (3) augments the specification in column (2) with controls for the share of women sitting in the municipal council as well as for age, education dummies, and experience dummies of the mayor. Column (4) replicates the specification in column (3) using the logarithmic transformation of the length (in days) of the mayoral term as the dependent variable. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

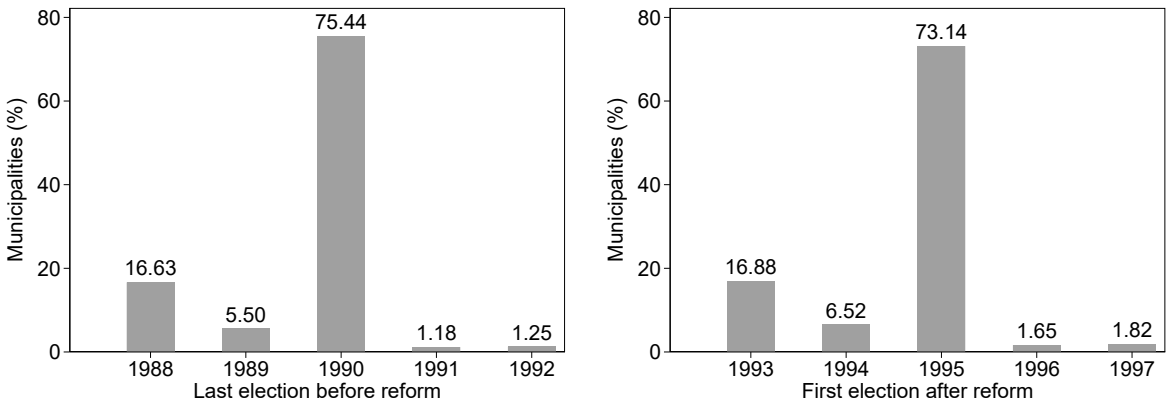


Figure B.1: Distribution of elections around the reform and 5 years prior to the reform

Notes: Distribution of elections held in each year during the 1988–1992 cycle (panel (a)) and during the 1993–1997 cycle (panel (b)).

Table B.2: Descriptive Statistics

	(1)	(2)	(3)
Variable	Mean	St. Dev.	Obs.
(a) Mayor and Council characteristics			
Female Mayor	0.064	0.246	137,718
College Mayor	0.407	0.491	137,718
Age Mayor	46.572	9.858	137,718
Female Councilors	0.148	0.112	137,718
High-skill job Mayor	0.138	0.345	137,718
Re-elected Incumbent	0.331	0.471	137,718
Same Party	0.866	0.340	131,483
(b) Election and City characteristics			
Post Reform	0.635	0.481	137,718
Instrument	0.641	0.480	137,718
First year of treatment	1,994.674	0.829	137,718
First year of predicted treatment	1,994.538	1.080	137,718
Year	1,997.176	6.490	137,718
Population	6,624.680	43,208.263	137,718
Surface (km^2)	33.070	45.941	137,718
Population density (inh./km^2)	278.498	629.428	137,718
Gender gaps in educ. attainment	112.131	26.847	137,718
Sex ratio	96.487	6.128	137,718
Female employment rate	29.547	8.612	137,718
(c) Pre-reform characteristics			
Female Councilors (1992)	0.082	0.066	137,718
Female Councilors in majority party (1992)	0.078	0.101	137,718
Female Councilors in minority parties (1992)	0.083	0.099	137,718
College Mayor (1992)	0.371	0.483	137,718
High School Mayor (1992)	0.437	0.496	137,718
Junior School Mayor (1992)	0.193	0.395	137,718
Vote share of DC (1992)	0.343	0.129	137,718
Vote share of PDS (1992)	0.138	0.102	137,718
Vote share of PSI (1992)	0.138	0.080	137,718
Vote share of LEGA (1992)	0.126	0.113	137,718
Vote share of RC (1992)	0.053	0.037	137,718
Vote share of MSI (1992)	0.040	0.030	137,718

Notes: All mayors serving in municipalities belonging to ordinary regions between 1986 and 2008.

Table B.3: First stage and reduced form

	First-stage			Reduced form		
	(1)	(2)	(3)	(4)	(5)	(6)
	Post Reform	Post Reform	Post Reform	Female Mayor	Female Mayor	Female Mayor
Instrument	0.748***	0.811***	0.811***	0.026***	0.020***	0.019***
	(0.013)	(0.010)	(0.010)	(0.006)	(0.006)	(0.006)
Observations	137,718	137,718	137,718	137,718	137,718	137,718
R^2	0.978	0.982	0.982	0.000	0.000	0.004
Year Effects	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort × linear trends		✓	✓		✓	✓
Time-varying covs			✓			✓
Pre-determined covs × Year			✓			✓
Control mean	0.00300	0.00300	0.00300	0.0340	0.0340	0.0340
F-stat	3553	6180	6180			

Notes: In columns (1)–(3), the dependent variable is $PostReform_{i,t}$, an indicator equal to 1 if the municipality is subject to the direct election regime. In columns (4)–(6), the dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. In column (1), the estimated equation is (4). Column (2) augments the specification in column (1) with linear trends specific to each cohort of municipalities. Column (3) augments the specification in column (2) with controls for each municipality's surface, population density, gender differences in educational attainment, population, sex ratio, female employment rate (all measured in the 1991 population census) as well as the pre-reform share of women in the municipal council interacted with year dummies, educational attainment of the pre-reform incumbent mayor interacted with year dummies, and vote share of the main parties in 1992 national elections interacted with year dummies. In column (4), the estimated equations are the reduced form associated with the 2SLS specification in equations (3) and (4). Column (5) augments the specification in column (4) with linear trends specific to each cohort of municipalities. Column (6) augments the specification in column (5) with controls for each municipality's surface, population density, gender differences in educational attainment, population, sex ratio, female employment rate (all measured in the 1991 population census) as well as the pre-reform share of women in the municipal council interacted with year dummies, educational attainment of the pre-reform incumbent mayor interacted with year dummies, and vote share of the main parties in 1992 national elections interacted with year dummies. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

Table B.4: Difference-in-Differences estimates at the municipality-term level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				2SLS			
	Share of term with a female mayor	Female first mayor	Female last mayor	Share of female mayors	Share of term with a female mayor	Female first mayor	Female last mayor	Share of female mayors
Post Reform	0.016**	0.018**	0.012	0.015**	0.026***	0.029***	0.022**	0.025***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)
Observations	32,541	32,541	32,541	32,541	32,541	32,541	32,541	32,541
R^2	0.337	0.332	0.337	0.337	0.000	0.000	0.000	0.000
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Term-position Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.0344	0.0330	0.0365	0.0348	0.0344	0.0330	0.0365	0.0348
First-stage F					7257	7257	7257	7257

Notes: The unit of observation is a municipality–electoral-term. In columns (1) and (5), the dependent variable is the share of years within the term in which the mayor in office is a woman. In columns (2) and (6), the dependent variable is an indicator taking the value 1 if the first mayor observed during the term is a woman, and zero otherwise. In columns (3) and (7), the dependent variable is an indicator taking the value 1 if the last mayor observed during the term is a woman. In columns (4) and (8), the dependent variable is the share of mayors observed during the term who are women. Columns (1)–(4) report OLS estimates while Columns (5)–(8) report 2SLS estimates in which $PostReform$ is instrumented using scheduled exposure to the reform. All specifications include municipality fixed effects and fixed effects for the position of the term in each municipality's observed electoral sequence. Standard errors robust to clustering at the municipality level are reported in parentheses. *, **, and *** represent the 10%, 5%, and 1% significance levels.

Table B.5: Robustness to introduction of gender quotas – Refined definition of exposure

	2SLS				
	(1)	(2)	(3)	(4)	(5)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.025***	0.049***	0.068	0.048***	0.107**
	(0.007)	(0.012)	(0.043)	(0.013)	(0.054)
Post Reform × Before Abolition	0.006			0.005	–0.057
	(0.006)			(0.012)	(0.049)
Post Reform × $\overline{\text{Exposure}}$		–0.135***		–0.135**	
		(0.046)		(0.052)	
Post Reform × $\overline{\text{Exposure}}$ × Before Abolition				–0.011	
				(0.057)	
Post Reform × 1($\overline{\text{Exposure}} \geq 0$)			–0.045		–0.028
			(0.043)		(0.046)
Post Reform × 1($\overline{\text{Exposure}} \geq 0$) × Before Abo					–0.063
					(0.049)
Observations	137,718	137,718	137,718	137,718	137,718
R^2	0.001	0.001	0.001	0.001	0.001
Year Effects	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓	✓
Control mean	0.0350	0.0350	0.0350	0.0350	0.0350

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. The estimated equations are (3) and (4) augmented with linear trends specific to each cohort of municipalities. Column (1) includes the interaction term $PostReform_{i,t} \times BeforeAbolition_{i,t}$. Column (2) includes the interaction term $PostReform_{i,t} \times \overline{Exposure_i}$. Column (3) includes the interaction term $PostReform_{i,t} \times \mathbb{1}(\overline{Exposure_i} \geq 0)$. Column (4) includes the interaction terms $PostReform_{i,t} \times BeforeAbolition_{i,t}$, $PostReform_{i,t} \times \overline{Exposure_i}$, and $PostReform_{i,t} \times BeforeAbolition_{i,t} \times \overline{Exposure_i}$. Column (5) includes the interaction terms $PostReform_{i,t} \times BeforeAbolition_{i,t}$, $PostReform_{i,t} \times \mathbb{1}(\overline{Exposure_i} \geq 0)$, and $PostReform_{i,t} \times BeforeAbolition_{i,t} \times \mathbb{1}(\overline{Exposure_i} \geq 0)$. $BeforeAbolition_{i,t}$ is an indicator variable taking values 1 for all elections held in 1993 or after 1993 and before September 6, 1995. $\overline{Exposure_i}$ is defined as in (7) and $\mathbb{1}(\overline{Exposure_i} \geq 0)$ takes the value 1 if $\overline{Exposure_i} \geq 0$. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

Table B.6: Heterogeneity by macro-region

	North-West	North-East	Central Italy	South
		2SLS		
	(1)	(2)	(3)	(4)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.032***	0.058**	0.023	0.005
	(0.012)	(0.024)	(0.019)	(0.011)
Observations	62,972	19,870	20,844	34,032
R^2	0.000	0.003	0.008	0.002
Year Effects	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓
Control mean	0.0440	0.0460	0.0290	0.0130

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. Estimation method: 2SLS as in equations (3) and (4). In column (1), the sample is restricted to municipalities belonging to Piedmont, Lombardy, and Liguria regions. In column (2), the sample is restricted to municipalities belonging to Veneto and Emilia-Romagna regions; in column (3), the sample is restricted to municipalities belonging to Tuscany, Umbria, Lazio, Marche, and Abruzzo regions; in column (4), the sample is restricted to municipalities belonging to Molise, Campania, Basilicata, Calabria, and Puglia regions. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.

Table B.7: Heterogeneous effects by pre-reform gender norms

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor	Female Mayor
Post Reform	0.015***	0.014**	0.015***	0.015***	0.028***	0.027***	0.027***	0.027***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)
Post Reform × Diff. employment rate (std.)	–0.007***			–0.007***	–0.007***			–0.007***
	(0.002)			(0.002)	(0.002)			(0.002)
Post Reform × share against abortion (std.)		–0.009***		–0.004		–0.009***		–0.005
		(0.003)		(0.005)		(0.003)		(0.005)
Post Reform × share against divorce (std.)			–0.010***	–0.006			–0.010***	–0.006
			(0.003)	(0.004)			(0.003)	(0.004)
Observations	137,718	134,555	134,375	134,375	137,718	134,555	134,375	134,375
R^2	0.346	0.347	0.347	0.348	0.000	0.000	0.001	0.001
Year Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350	0.0350

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. The table examines heterogeneity in the effect of direct election with respect to three predetermined proxies for local gender norms. The gender gap in employment rates is measured using the 1991 Population Census. The shares against abortion and divorce are municipal vote shares in favor of the restrictive 1981 abortion referendum and in favor of the 1974 referendum to repeal the right to divorce law. All three variables are standardized to have mean zero and standard deviation one, and are oriented so that higher values indicate more traditional gender norms. Columns (1)–(3) report OLS specifications that interact $PostReform_{i,t}$ with each measure separately, while column (4) includes all three interactions jointly. Columns (5)–(8) report the corresponding 2SLS specifications. In the 2SLS models, $PostReform_{i,t}$ and its interactions with the measures of gender norms are instrumented using scheduled reform exposure and the corresponding interactions with the same predetermined municipal characteristics. All specifications include municipality fixed effects, calendar-year fixed effects, and treatment-cohort-specific linear trends. Standard errors robust to clustering at the municipality level are reported in parentheses. *, **, and *** represent the 10%, 5%, 1% significance levels.

Table B.8: Heterogeneous effects by pre-reform educational attainment of the local population

	OLS		2SLS			
	(1)	(2)	(3)	(4)	1	1
	Female Mayor	Female Mayor	Female Mayor	Female Mayor	2	2
Post Reform	0.014***		0.027***		15	3
	(0.005)		(0.008)		16	4
Post Reform × share population with high-school degree (std.)	0.006**		0.006**		17	5
	(0.003)		(0.003)		18	6
Post Reform × College		0.005		0.017*	3	7
		(0.007)		(0.009)	4	8
Post Reform × High-school		0.020***		0.030***	5	9
		(0.007)		(0.009)	6	10
Post Reform × Less than high school		0.039***		0.050***	7	11
		(0.008)		(0.010)	8	12
Post Reform × College × share pop. with high-school degree (std.)		0.003		0.003	9	13
		(0.004)		(0.004)	10	14
Post Reform × High-school × share pop. with high-school degree (st		0.007		0.007	11	15
		(0.005)		(0.005)	12	16
Post Reform × Less than high school × share pop. with high-school		0.014**		0.014*	13	17
		(0.007)		(0.007)	14	18
Observations	137,718	137,718	137,718	137,718	19	19
R^2	0.346	0.347	0.000	0.001	20	20
Year Effects	✓	✓	✓	✓	21	21
Municipality Fixed Effects	✓	✓	✓	✓	22	22
Cohort × linear trends	✓	✓	✓	✓	23	23
Control mean	0.0350	0.0350	0.0350	0.0350	24	24

Notes: The dependent variable is $FemaleMayor_{i,t}$, an indicator equal to 1 if the mayor in office is a woman. The measure of local educational attainment is the share of the population holding at least a high-school degree, measured in the 1991 Population Census and standardized to have mean zero and standard deviation one. Columns (1) and (3) interact $PostReform_{i,t}$ with local educational attainment. Columns (2) and (4) allow this gradient to vary with the educational attainment of the mayor serving before the reform, following the three categories defined in Section 5. The three categories identify municipalities in which the pre-reform incumbent held at least a college degree, held a high-school degree as the highest educational attainment, or had not completed high-school. Columns (1) and (2) report OLS estimates, while columns (3) and (4) report 2SLS estimates. In the 2SLS specifications, $PostReform_{i,t}$ and all its interactions are instrumented using scheduled reform exposure and the corresponding interactions with the predetermined education measures. All specifications include municipality fixed effects, calendar-year fixed effects, and treatment-cohort-specific linear trends. Standard errors robust to clustering at the municipality level are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table B.9: Impact of the reform on the characteristics of mayors

	(1)	(2)	(3)	(4)	(5)	(6)
	Age Mayor	College Mayor	High school Mayor	No High sch. Mayor	High-skill job Mayor	Win next
Post Reform	0.520*	0.098***	–0.037**	–0.061***	0.056***	0.256***
	(0.309)	(0.014)	(0.015)	(0.009)	(0.011)	(0.018)
Observations	137,718	137,718	137,718	137,718	137,718	137,718
R^2	0.000	0.000	0.000	0.000	0.001	0.004
Year Effects	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
Cohort × linear trends	✓	✓	✓	✓	✓	✓
Control mean	44.926	0.355	0.428	0.217	0.100	0.400

Notes: In column (1), the dependent variable is the age of the mayor. In column (2), the dependent variable is an indicator equal to 1 if the mayor holds a college degree. In column (3), the dependent variable is an indicator equal to 1 if the mayor holds a high-school degree as the highest educational attainment. In column (4), the dependent variable is an indicator equal to 1 if the mayor did not complete high-school. In column (5), the dependent variable is an indicator equal to 1 if the mayor worked in a high-skill profession before appointment. In column (6), the dependent variable is an indicator equal to 1 if the mayor is re-elected after the next election. Standard errors robust to clustering at the municipality level are in parentheses. *,**,*** represent the 10%, 5%, 1% significance levels.