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Gender quotas and politicians' education

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Abstract:

What are the effects of gender quotas on the educational composition of municipality councils? Despite abundant research on the effects of gender quotas on political outcomes, it is yet unclear how they affect the educational attainment of elected male and female councilors. This paper provides novel evidence on this matter for Spain using data for 119,567 elected municipal councilors in the local elections of 2003, 2007, 2011 and 2015. We adopt a difference in discontinuities research design that compares outcome values before and after the quota implementation in municipalities below and above the quota population threshold. Our results show that the quota has not had a significant effect on the average years of schooling of male and female municipal councilors, on average. While ensuring greater female representation, gender quotas are thus found to be neutral with respect to politicians' competence.

Keywords: gender quota; municipality councils; gender differences; difference in discontinuities

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1. INTRODUCTION

The selection and recruitment of competent politicians remains a major concern for modern societies. In general, better-educated politicians induce higher levels of council efficiency (Gavoille & Verschelde, 2017; Sørensen, 2023) and generate higher economic growth (Besley et al., 2011). Depending on the electoral system, voters typically face a tradeoff between competent candidates and those with policy preferences similar to their own (Beath et al., 2016). For this reason, political parties may deliberately choose to recruit mediocre but homogeneous politicians, discriminating against some groups to maximize collective effort (Mattozi & Merlo, 2015).

A growing body of research has documented relevant differences in policy making depending on the gender of the politician, which arise as a mixture of differences in policy priorities (Besley & Coate, 1997) and a potential better qualification of females that achieve power positions (Kotakorpi & Poutvaara, 2011), which depend, among others, on outside options and political wages (Gagliarducci & and Nannicini, 2013).[1] Despite important progresses in recent years, females are still underrepresented in power positions. Seniority bias within party organizations (Cirone et al., 2023), social roles (Teele et al., 2018) and voter bias against females (Le Barbanchon & Sauvagnat, 2022) have been shown to dampen women's career advancement in politics.[2] As a result, females are typically nominated at poorer positions on

[1] A large literature has shown that female legislators are efficient at managing municipal administrations (Braga & Scervini, 2017), less corrupt and vulnerable to political opportunism (Baskaran et al., 2023), adopt more female-oriented policies (Chattopadhyay & Duflo, 2004; Clots-Figueras, 2011; Lippman, 2022), and devote a large share of spending to the environment (Casarico et al., 2022), childcare policies and services (Baskaran & Hessami, 2023; Bhalotra & Clots-Figueras, 2014), education (Bhalotra et al., 2023; Svaleryd, 2009) and local security (Andreoli et al., 2022). Moreover, increasing female political representation exerts positive effects on educational outcomes in urban areas (Clots-Figueras, 2012) and the likelihood of public schools receiving grants (Priyanka, 2022), increases female political participation (Maitra & Rosenblum, 2022), and incentivizes more female candidates to run for office (Baskaran & Hessami, 2018). Nonetheless, other studies do not find significant differences in political outcomes based on female leadership (Carozzi & Gago, 2023; Ferreira & Gyourko, 2014). The reader is referred to Hessami and Lopes da Fonseca (2020) for a review of this literature.

[2] Females obtain fewer votes in municipalities with higher gender gaps due to voter bias against them (Le Barbanchon & Sauvagnat, 2022). Females are also less likely to recontest in elections due to family obligations related to motherhood and male dominance in politics (Baskaran & Hessami, 2022; Fiva & King, 2024; Lassébie, 2022). This gender recontest gap is larger in cases when they lose as compared to males (Peveri & Sangnier, 2023). Furthermore, female mayors exhibit a higher likelihood of early

the ballot (Esteve-Volart & Bagues, 2012), and less likely to be reelected as members of regional councils (Cellini & Cuccia, 2024).

To circumvent females' underrepresentation, in recent years different types of gender quota policies that require a minimum percentage of females in party lists have been implemented worldwide, particularly in Western societies. Empirical evidence shows that they increase electoral participation (De Paola et al., 2014), the pool of female candidates (Lassébie, 2022; O'Brien & Rickne, 2016), votes for female candidates (Bonomi et al., 2013), and female representation among elected politicians (Bagues & Campa, 2021; Baltrunaite et al., 2019; Cavallini et al., 2023).[3] Despite this abundant literature, there remains little knowledge about whether quotas affect political quality selection *by gender*.

The goal of this article is to study the effects of gender quotas on the educational level of selected politicians at the municipal level. We examine how imposing a gender quota on party lists affects the years of schooling of selected male and female politicians as a proxy for their quality. The enforcement of gender quotas has been shown to increase the share of elected females in political councils and correspondingly decrease that of males (Bagues & Campa, 2021; Cavallini et al., 2023; De Paola et al., 2014; Lassébie, 2020; Spaziani, 2022). However, as theoretically characterized by Júlio and Tavares (2017), the impact on the competence of those elected depends on the selection of educated individuals from each group. In the presence of supply constraints on candidates, critics of quota designs argue that they reduce the scope of political competition, fostering the replacement of competent males with mediocre females. A distinct viewpoint, however, posits that the imposition of gender quotas expels mediocre male

termination of the legislature, particularly in regions with less favorable attitudes towards working women (Gagliarducci & Paserman, 2012).

[3] Notwithstanding this, the effects of quotas seem to be country-specific and interact with cultural factors and electoral systems. Bagues and Campa (2021) do not detect significant effects of gender quotas on the probability that women reach powerful positions or the size and composition of public finances. For the case of French parliamentary elections, Lippman (2021) shows that main political parties protect incumbents by nominating women in less winnable districts, particularly the right-wing party. Spaziani (2022) and Lassébie (2022) do not find evidence that the quotas improve female mayoral candidacies or female list leaders.

leaders (Besley et al., 2017). Accordingly, the final effect is undetermined a priori and requires empirical testing.[4]

We leverage the quasi-experimental introduction in 2007 of gender quotas in party lists in Spain that required at least 40% of candidates of each gender in the ballot in municipalities with more than 5,000 inhabitants. To avoid females being placed at the bottom of the list, the Spanish quota applies to each five-position bracket. The quota was subsequently extended from 2011 onwards to municipalities over 3,000 inhabitants. We exploit both the discontinuity in the application of the quota based on population thresholds in a pre-post setting and adopt a difference in discontinuities research design à la Grembi et al. (2016). Our identification strategy compares outcome trajectories before and after the quota implementation below and above the population threshold.

We use data on 119,567 elected municipality councilors in Spain for the election years 2003, 2007, 2011 and 2015. Based on their attained educational level, we compute the equivalent years of schooling and use it as our dependent variable. Our results show that the quota has not had a significant effect on the educational compositions of municipality councils by gender; no effect is neither found when focusing on mayors. Our findings imply that while increasing female representation in party lists and therefore in local councils, gender quotas have neutral effects on the competence of elected politicians. Therefore, this result does not support the theoretical predictions by Júlio and Tavares (2017), who argue that gender quotas should increase high-ability female candidacies without discouraging high-ability male ones. Our results are instead compatible with those females entering municipality councils following the quota enforcement attaining about the same educational level than that of pre-quota incumbent females. At the same time, the exclusion of males in exchange of females does not appear to obey to educational attainment.

We contribute to the growing literature on the effects of gender quotas in political outcomes (Baltrunaite et al., 2019; Bonomi et al., 2013; Lassébie, 2022; Lippman, 2021; Spaziani, 2022; O'Brien & Rickne, 2016), and particularly to research on their impact on the quality of selected politicians. Baltrunaite et al. (2014) document an increase that ranges from 0.12 to 0.24 in the

[4] On the one hand, these quotas could attract more qualified and educated women, thus raising the overall educational level of politicians. On the other hand, if the selection of candidates is based on other criteria, such as experience or political affiliation, the impact on education could be null.

average years of education of elected politicians caused by the setting of a gender quota in local elections in Italy. Besley et al. (2017) evaluate a zipper quota in Sweden that required local parties to alternate men and women on the ballot, showing that the quota raised the competence of male politicians. Aldrich and Daniel (2024) similarly document that quotas increase the number of educated males and females in European Parliament delegations. However, other studies document null effects. For the case of the Italian parliament, Weeks and Baldez (2015) do not find that quotas affect the qualification of elected politicians. In Spain, Bagues and Campa (2021) do not observe significant variations in the average years of schooling of municipal councils because of the quota implementation.

The paper complements this literature by offering novel evidence on the potential heterogeneous impact of quotas on politicians' schooling by gender. All the abovementioned studies have considered the municipality as the unit of analysis and the average schooling of all politicians as the outcome measure, without examining its compositional effects. Instead, we look at how quotas affect males' and females' schooling. Exploring the existence of a potential differential impact depending on the gender is important because, if more education is an indicator of better competence and male and female politicians make different policy decisions, this could influence political decisions and the quality of governance. In this way, we provide the first medium-to-long-term estimation of compositional effects of gender quotas on the educational level of politicians, which offers relevant implications for group decision-making settings and policy implementation.

Our study has also a relevant methodological contribution. Most related works have relied on difference-in-differences (e.g., Casas-Arce & Saiz, 2015; Baltrunaite et al., 2014) or regression discontinuity designs (e.g., Bagues & Campa, 2021) to identify the effect of the quota on various economic outcomes. As illustrated by Bagues and Campa (2020), the former strategy could produce misleading estimates when treated and non-treated municipalities are on different trends. Bagues and Campa (2021) argue in favor of the use of regression discontinuity designs that compare outcome values immediately below and above the population threshold that determines the policy implementation. However, this strategy could produce biased estimates when the quota population cutoff coincides with other policies that allegedly affect the outcome, as discussed in Grembi et al. (2016). We therefore implement a difference-in-discontinuities approach that compares the change in politicians' schooling before and after the quota in municipalities that locate closely below and above the population threshold.[5] As we illustrate in the paper, canonical difference-in-differences regressions misleadingly suggest that the quotas have had a negative effect on females' schooling. We show that this result comes from a drop in the gender schooling gap over time as population grows but not due to the quota implementation. Accordingly, our study makes a cautionary advice on the use of difference-in-differences research designs for policy interventions that are defined based on population thresholds.

The remainder of the paper proceeds as follows. Section 2 provides some background for the analysis, describing the functioning of the Spanish electoral system and gender quotas design and implementation. Section 3 presents the dataset and reports some descriptive statistics. Section 4 describes the three identification strategies used in the empirical analysis. Section 5 presents the main estimation results together with some heterogeneity analyses and robustness checks. Finally, Section 6 concludes with the main implications and policy recommendations that can be drawn from our findings.

2. INSTITUTIONAL BACKGROUND

2.1. Spanish local government

Spain is characterized by a robustly decentralized system, organized into 17 Autonomous Communities (NUTS 2) and 50 provinces (NUTS3). This administrative structure further extends to 8,132 municipalities, serving as the fundamental territorial units. Endowed with legal personality, as well as their own territory, population, and administrative organization, each municipal government operates autonomously and exercises its competences according to the principle of subsidiarity. Each municipality ensures the management of services and needs closest to citizens. This entails a broad spectrum of responsibilities, including tax regulation (such as property and vehicle taxes), urban planning, water supply, waste management, local policing, or social services, among others. Furthermore, municipalities actively engage in cultural, tourism, and sporting initiatives. Generally, the number of functions that local authorities are legally obliged to provide increases with population size.

[5] This strategy has been recently adopted in related studies including Grembi et al. (2016), De Benedetto and De Paola (2019) and Cavallini et al. (2023).

The governance structure at the municipal level comprises the mayor and the municipal council, with the former holding significant executive authority and managing a substantial portion of the municipal budget. The mayor’s functions include, by law, directing municipal services, representing the municipality, serving as the chief of staff, overseeing the local police, authorizing expenditures below certain limits, presiding over the full council and cabinet, and appointing the mayor’s deputies. The remuneration of local representatives is variable, contingent upon factors such as municipality size and budget allocation. For instance, mayors’ salaries in the 2003-2015 period average 34,380 euros per year (adjusted to 2014 Consumer Price Index), although they can reach up to 109,939 euros. Councilor salaries are generally lower and can reach a maximum of 98,193 euros per year (ISPA, 2019).[6]

2.2. *Spanish electoral system at the local level*

The electoral system at the local level in Spain depends on the population size of the municipality in the year before the elections. In municipalities with a population exceeding 250 inhabitants, council members are elected directly by citizens through a single-district election employing a closed-list proportional representation system.[7] Each political party presents a list of candidates, and voters indicate their preference by selecting the corresponding party-list ballot, which contains as many candidates as there are seats in the municipal council.[8] Females in Spanish local councils have been shown to be better represented under the closed-list than under the open-list system (Gonzalez-Eiras & Sanz, 2021), which is due to a mixture of differences in the supply of female candidates, voter bias and party bias.

Local elections in Spain are held simultaneously across all municipalities, typically on the last Sunday of May every four years. The allocation of seats is determined using the D'Hondt

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[6] The salary information of mayors and councilors comes from the Information System for Administration Job Salaries (ISPA), covering the remuneration of mayors in over 6,700 municipalities, constituting 90% of the Spanish population in 2019.

[7] The institutional characteristics of Spanish local government align with the key features of parliamentary systems, where the executive leader is elected by a collective legislative body through a proportional system.

[8] In municipalities with 250 or fewer inhabitants there are open lists in which voters can vote for up to four individual candidates from the same or different parties. Moreover, municipalities with under 100 inhabitants act as an open council, with a directly elected mayor and an assembly of neighbors. Since 2011 these small municipalities are also allowed to use a closed list system.

method, which distributes councilors among the party candidacies according to the number of votes received.[9] To prevent excessive fragmentation, parties failing to meet a minimum of 5% of votes are not represented in the municipality council. Councilors are appointed from their respective lists in the order in which candidates are listed.[10] For example, if a party obtains four seats in the council, then the four candidates at the top of the list become councilors. Therefore, the design of list order underscores the importance of party performance: candidates positioned lower on the list may succeed or fail to secure seats depending on party success.

The selection of the mayor among the top-listed candidates of each party is done by majority rule, i.e., the councilor who obtains a majority of the votes is selected as mayor. If a single party secures a majority of seats, its candidate is usually directly elected mayor; otherwise, the candidate from the most voted party assumes the mayoral role. Once elected, the mayor serves a four-year term unless removed through a "motion of censure" by the full council. In practice, the council operates as a small representative democracy, requiring a majority vote to enact regulations proposed by the mayor.

2.3. Gender quotas in Spain

On March 22, 2007, the Law for the Equality of Women and Men was passed by the Spanish Parliament.[11] This law required candidate lists in all elections to include at least 40% of candidates from each gender. Moreover, to prevent parties from placing all female candidates at the bottom of the list, this gender parity quota-imposed restrictions on the candidates' order; the gender balance had to be maintained for every bracket of five positions. Quotas were implemented for the first time in 2007 elections, but they only applied for municipalities over 5,000 inhabitants (as measured in January of the previous year). In the following local elections, quotas were extended to all municipalities with more than 3,000 inhabitants.

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[9] The composition of municipal councils varies according to population size (Article 179 of *Ley Orgánica 5/1985, de 19 de junio, de Régimen Electoral General*), with an average of approximately 10 members. It ranges from 7 in smaller localities (municipalities between 250 and 1,000 inhabitants) to 57 in major cities like Madrid.
[10] Typically, the provincial management of political parties is responsible of making the lists.
[11] The Law had popular support. A poll conducted in September 2007 by the Spanish Centre for Sociological research (CIS) showed that two out of three Spaniards were in favor of the introduction of gender parity in candidacy lists.

Some prior studies have investigated the effects of gender quotas in Spain on different outcomes. Casas-Arce and Saiz (2015) report that electoral parties most affected by the quota (i.e., those that were forced to increase more the share of female candidates relative to the baseline) obtain better electoral results.[12] Esteve-Volart and Bagues (2012) find that parties nominate female candidates to worser positions on candidate lists in Senate and House elections, a result that holds independently of candidates' experience. Their analysis suggests that parties adopt gender parity whenever it is not costly for male candidates. More recently, Bagues and Campa (2021) do not find evidence that gender quotas affect the mean educational attainment of elected councilors or municipal budget allocation policies. They nonetheless show that quotas increase the share of females in candidate lists by around 8 percentage points and among council members by 4 percentage points.

Since female politicians tend to be more educated than their male peers (e.g., Baltrunaite et al., 2014), and the quotas change the gender composition of municipality councils, it seems relevant to examine whether quotas affect the educational attainment of elected politicians. While Bagues and Campa (2021) look at average schooling, in what follows we investigate the potential educational composition changes by gender predicted by Júlio and Tavares (2017).

3. DATA

3.1. Dataset

We utilize administrative data on local councils' composition derived from four consecutive municipal elections (2003-2015) held in Spain. The data comes from the *Portal de Transparencia* and consists of 286,845 councilors in office for local legislative bodies (municipalities) during four legislature periods (2003-2007; 2007-2011; 2011-2015 and 2015-2019) corresponding to 8,135 Spanish municipalities. We have information about the municipality for whom he/she is elected, the appointment date, the political party, age (in years), role (distinguishing between mayor, vice-mayor or council) and attained level of education. While the gender of the politician is not available in the original source, we obtain it from the

[12] Bagues and Campa (2020) conduct a replication study and cast serious doubts about whether the effects documented by Casas-Arce and Saiz (2015) are statistically significant, arguing that the estimates are sensitive to accounting for municipality size.

dataset used by Bagues and Campa (2021). These data are merged with information about population size during the election year and also one year lagged. This information is obtained from the local Census and provided by the Spanish Statistics Institute.

For the empirical analysis, we first exclude municipalities with less than 250 inhabitants. This is because, as abovementioned, in those municipalities people vote for candidates under an open-list system. We also exclude municipalities over 10,000 inhabitants, as done by Bagues and Campa (2021); since the gender quota was imposed on municipalities over 5,000 inhabitants in 2007 and over 3,000 inhabitants in 2011 and 2015, we need to restrict the group of treated municipalities (i.e., affected by the quota) to those that are more similar to the untreated ones. We further exclude politicians that were appointed in a different year from the election date, mostly as a replacement of another councilor who leaves his/her office prior to the end of the legislature. This leaves us a total of 170,088 elected members in local councils pertaining to 4,978 municipalities.[13]

3.2. Dependent variable: politicians' years of schooling

Our outcome of interest is politicians' quality, understood as his/her ability to serve the public interest with competence. However, this is a fuzzy concept that is quite difficult to measure. Because there is a positive correlation between education and government quality (Besley et al., 2011; Gavoille & Verschelde, 2017; Sørensen, 2023), many scholars measure the quality of politicians in terms of their human capital (De Benedetto & De Paola, 2019; Baltrunaite et al., 2014; Galasso & Nannicini, 2011). We follow this approach and proxy politicians' competence by their years of schooling.

In the original dataset, we have information about the attained educational level (i.e., educational cycle completed). Based on this, we computed the minimum equivalent years of schooling needed to complete such level of education in the Spanish education system using ISCED 2011 criteria. The corresponding equivalence is shown in Appendix, Table A1. Importantly, we only avail data on attained education for 72% of the restricted sample (123,043 observations). To inspect whether this could induce selection biases, Figures A1 and A2 in

[13] Because the number of municipality councils is proportional to population size, excluding large municipalities results in a large drop in the number of observations.

Appendix present histograms of population size depending on whether the politician's education is reported or not. As illustrated there, missing data appears to be independent from the population size. As regards the gender distribution, we run the following linear probability regression:

$$\begin{aligned} I(education \neq .)_{ijt} \\ &= \alpha_i + P_{ijt} + Year_t + \psi Female_i + \sum_{t=2}^{T} \psi_t Female_i \times Year_t \\ &+ \lambda_1 Treated_{jt} + \lambda_2 Treated_{jt} \times Female_i + \zeta R_i + \varsigma_{ijt} \end{aligned} \tag{1}$$

where *i* indexes politicians, *j* municipalities and *t* election years; $I(education \neq .)$ is a binary indicator for whether we observe the level of education of politician *i* in municipality *j* in period *t*; α_i, P_{ijt} and $Year_t$ are municipality, party and year fixed effects; $Female_i$ is a dummy for being a female; $Treated_{jt} = 1(pop_{jt} > pop^*{}_t) \times Post_t$ is a binary indicator for whether the quota applies to the municipality in election year *t*, where $pop^*{}_t$ is the corresponding population threshold in each period that determines the gender quota implementation and $Post_t$ a binary indicator that takes value 1 for the years 2007, 2011, and 2015; R_i are dummy indicators for the role of the politician (mayor and councilor, leaving vice-mayor as the reference category) and ς_{ijt} is the error term.

Table A2 in Appendix reports the regression output. We document that education observability decreases over time but increases with the politician's relevance in the council. Mayors are 3.9 percent more likely to report their education than vice-mayors, which are also 6.6 percent more likely to report their education than councilors. Importantly, no differences are detected based on gender, and this does not vary by election year. Most notably, no differences in schooling observability are found depending on whether the municipality is affected by the gender quota. Therefore, since the observability of our outcome variable appears to be unrelated to the treatment of interest, we do not expect that excluding those for whom schooling information is unavailable from the sample would significantly distort the empirical analysis. We nevertheless acknowledge this as a limitation of the dataset.

3.3. Control variables

Alongside politicians' educational attainment, we also have information about other relevant councilors' characteristics. We define *Female* as a dummy variable that takes value 1 if the elected councilor is a women. Based on politicians' age (in years), we define four dummies for the following age intervals: *Less than 30*, *Between 31-50*, *Between 51-65*, and *Over 66*. We also avail information about the office position that each municipal council member holds, distinguishing between mayor, vice-mayor and councilor. We therefore define three corresponding dummies for each of them. We also know the political party affiliation of each council member.

After excluding some additional missing values in some of the controls, our sample for the empirical analysis is finally composed of 119,624 councilors elected in local legislative bodies in 4,921municipalities. Figure 1 illustrates our data coverage in terms of observations per year. Consistent with Table A2 in Appendix, there is a lower number of observations in the last elections due to the increase in the share of politicians that do not report their attained level of education over time. Figure A3 in Appendix plots the number of female politicians in the sample by population size and election year. Table 1 presents proportion tests on the share of female councilors in the sample in municipalities affected by the quota before and after. Consistent with Bagues and Campa (2021) for Spain and other studies for France (Lassébie, 2020) and Italy (Cavallini et al., 2023; Spaziani, 2022), there is an increase in female representation in those municipalities under the quota.

FIGURE 1 HERE

TABLE 1 HERE

3.4. Descriptive statistics

Table 2 shows descriptive statistics on the average years of schooling of elected politicians, for the pooled sample and separately by gender. Panel A displays the statistics for the full sample period, while Panel B distinguishes by election year. In Panel C, we present summary statistics depending on municipality size and election period. In the last column, we present the statistic for a t-test of mean male-female comparison. The mean schooling of municipality councilors is 11.68 years. On average, females are better educated than males. This gap remains statistically significant in all election periods. Interestingly, this gap in schooling becomes smaller, albeit still significant, in larger municipalities. This is partially explained by the comparatively higher female representation in larger municipality councils. As presented in Table A3 in Appendix, the lower percentage of female councilors in small municipalities likely forces them to attain high education to enter those councils. For illustration purposes, Figure 2 plots the mean years of schooling for males and females by population size. We see the gender gap narrows as population grows.

TABLE 2 HERE

FIGURE 2 HERE

Table 3 presents descriptive statistics of the control variables to be used in the empirical analysis, both for all councilors and separately by gender. The proportion of female mayors is significantly lower than that of males (7% versus 13%). Whereas no differences are found in the role of vice-mayors, females tend to occupy the position of councilor. Furthermore, females are younger; around 83% of female councilors are under 51 (17% under 30), as compared to 68% of males. Concerning differences by municipality size, there is a significantly higher proportion of males in municipalities below 3,000 inhabitants. On the contrary, there are more female councils in municipalities over 5,000 inhabitants.

TABLE 3 HERE

4. IDENTIFICATION STRATEGIES

4.1.Difference-in-differences (DID)

To study the effects of the quota on the years of schooling of municipality councilors, we start with a difference-in-differences (hereafter DID) research design where we compare the outcome values of treated municipalities with those unaffected by the quota before and after its implementation, as done by Casas-Arce and Saiz (2015) and Baltrunaite et al. (2014). The regression equation to be estimated is:

$$Y_{ijt} = \alpha_j + year_t + \beta_1 Treated_{jt} + \beta_2 Female_{it} + \beta_3 Treated_{jt} \times Female_{ijt} + \theta X_{it} + \varepsilon_{ijt} \quad (2)$$

where Y_{ijt} are the years of schooling of politician *i* in municipality *j* in election year *t*, α_j are municipality fixed effects capturing any time-invariant differences in the average educational attainment of councilors across municipalities, $year_t$ are election year fixed effects, $Treated_{jt} = 1(pop_{jt} > pop^*{}_t) \times Post_t$, where $pop^*{}_t$ is the corresponding population threshold in each period and $Post_t$ a binary indicator that takes value 1 for the years 2007, 2011, and 2015, and X_{it} are a set of control variables that include the charge of the politician (mayor and councilor, leaving vice-mayor as the reference category), his/her age interval (leaving less than 30 years old as the reference category) and party fixed effects.

The parameter β_1 quantifies overall differences in councilors' education in municipalities affected by the quota before and after whereas β_2 captures generic differences in schooling based on gender. The key parameter of interest is β_3, which measures the potential change in educational attainment for female councilors in treated municipalities after the quota implementation.

One first concern could be bias from negative weights arising from the quotas being implemented in different municipalities in different periods, so that municipalities between 3,000-5,000 are used as control units in 2007 but as treated units since 2011 onwards (e.g., de Chaisemartin & d'Haultfoeuille, 2020). Moreover, the effect we intend to estimate might be heterogeneous over time for the following reason: the supply of qualified females willing to run for office in 2007 (among which to select female candidates) might be lower than in the following election cycles, thereby reflecting in potential distinct effects on educational attainment of elected politicians over time. To address this issue, we also consider separate 2x2 regression designs where we compare outcome values in each election year under the quota relative to the untreated baseline 2003 (i.e., 2003 vs 2007; 2003 vs 2011; 2003 vs 2015). Note that in doing so β_3 will be capturing short and long-distance outcome differences.

4.2.Regression discontinuity (RD)

As argued by Bagues and Campa (2020; 2021), a DID design might be inappropriate in this setting because small and large municipalities are likely to be on differential trends in educational requirements. Bagues and Campa (2021) use instead a sharp Regression

Discontinuity (RD) design on the cross-sectional dimension, comparing outcome values below and above the population threshold determining the quota implementation for each year. We follow their approach and estimate the subsequent equation for each election year *t* under the quota implementation:

$$Y_{ij_{2007}} = \alpha + \gamma_1 I\big(population_{j,2006} > 5000\big) + \gamma_2 f(population)_{j,2006} + \gamma_3 Female_i + \vartheta X_{ij} + \epsilon_{ij} \tag{3}$$

$$Y_{ij_{2011}} = \alpha + \gamma_1 I\big(population_{j,2010} > 3000\big) + \gamma_2 f(population)_{j,2010} + \gamma_3 Female_i + \theta X_{ij} + \epsilon_{ij} \tag{4}$$

$$Y_{ij_{2015}} = \alpha + \gamma_1 I\big(population_{j,2014} > 3000\big) + \gamma_2 f(population)_{j,2014} + \gamma_3 Female_i + \vartheta X_{ij} + \epsilon_{ij} \tag{5}$$

where $I(.)$ is an identity function that takes value 1 if politician *i* belongs to the council of municipality *j* that is above the corresponding population threshold in each year. Here $population_{jt}$ is the 'running' variable and $f(.)$ is a local polynomial around the threshold point. Following Calonico et al. (2019), we include the charge of the politician (mayor and councilor) and the age interval as additive control variables.

4.3. Difference-in-discontinuities (DIF-IN-DISC) approach

However, in our view, the standard RD in Equations (3) to (5) could be problematic due to differences in unobservables at both sides of the threshold. As discussed by Eggers et al. (2018), one problem of RD based on population thresholds is that they typically coincide with other policy changes, which potentially introduces confounding factors. In Spain, according to Law 7/1985 *Ley de Bases de Régimen Local*, municipalities over 5,000 inhabitants are required to provide a public library, local police, a public park, and waste treatment. They also receive a higher transfer from the central government. Most notably, the maximum annual wage of a majors is €46,464 for municipalities between 1,000 and 5,000 inhabitants but €52,272 in municipalities between 5,000 and 10,000 inhabitants.[14] Previous research has theoretically and

[14] Ley 31/2022, de 23 de diciembre, de Presupuestos Generales del Estado, 2023.

empirically shown that higher transfers increase corruption and reduce the average education of candidates for mayor (Brollo et al., 2013). These regulations thus likely impose distinct educational requirements that could confound the effect of the quota if we adopt a canonical RD design.

To address this concern, we finally implement a difference-in-discontinuities (DIF-IN-DISC) design that exploits two sources of variation: 1) the before-after 2007 (2011) and 2) the below-above the 5,000 (3,000) inhabitants' thresholds. In this way, we evaluate the differences in the years of schooling between the pretreatment and the posttreatment discontinuity at the corresponding threshold. The regression equation to be estimated has the following form:

$$Y_{ijt} = \alpha + \mu_1 I\left(population_{ijt} \in (3{,}000 - 5{,}000)\right) + \mu_2 I(population_{ijt} \geq 5{,}000) + Year_t + \delta_1 Treated_{jt} + \delta_2 Female_{ijt} + \delta_3 ln\, pop_{jt} + \delta_4 Treated_{jt} \times Female_{ijt} + \delta_5 Female_{ijt} \times ln\, pop_{jt} + \delta_6 Treated_{jt} \times Female_{ijt} \times ln\, pop_{jt} + \pi X_{ijt} + \omega_{ijt} \tag{6}$$

where Y_{ijt} are again the years of schooling of politician *i* in municipality *j* in election year *t*, $I\left(population_{ijt} \in (3{,}000 - 5{,}000)\right)$ and $I(population_{ijt} \geq 5{,}000)$ are dummy variables that take value 1 if the municipality has between 3,000 and 5,000 inhabitants or more than 5,000 inhabitants, respectively, $year_t$ are election year fixed effects, $Treated_{jt} = 1(pop_{jt} > pop^*{}_t) \times Post_t$, where $pop^*{}_t$ is the corresponding population threshold in each period and $Post_t$ a binary indicator that takes value 1 for the years 2007, 2011, and 2015, $ln\, pop_{jt}$ is the log of population the year before (running variable) and X_{ijt} are the same control variables as in Equations (3)-(5).[15]

The parameter δ_1 quantifies overall differences in councilors education in municipalities affected by the quota before and after, δ_2 captures generic differences in schooling based on gender; and δ_3 measures the potentially non-linear relationship between population size and

[15] Equation (6) resembles the difference in discontinuities estimator proposed by Grembi et al. (2016) and subsequently applied by Alpino et al. (2022), De Bendetto and De Paola (2019) and Cavallini et al. (2023) in related studies. We use an OLS (linear) regression, which can be seen as a local linear regression of order 0, and deal with the potential non-linearity of the running variable using the log transformation.

years of schooling. δ_4 is the key parameter of interest for us: it measures the potential change in educational attainment for female councilors in treated municipalities. δ_5 measures a potential distinct relationship in educational requirements as the population grows for males and females. Finally, the parameter δ_6 captures potential differential effects of the quota on females' education depending on population size.

Because the regression includes $ln\ pop_{jt}$ and we only have 4 election years, we do not consider here municipality fixed effects to avoid collinearity problems. We instead control for broad differences between treated and control units by including $I\left(population_{ijt} \in (3{,}000 - 5{,}000)\right)$ and $I(population_{ijt} \geq 5{,}000)$.

The appropriate identification of Equation (6) relies on the following three assumptions. First, municipalities cannot manipulate population counts to circumvent the quota implementation. To test this, we implemented McCrary (2008) test for sorting of the population variable around 5000 inhabitants in year 2007 and 3000 inhabitants in year 2011 as proposed by Cattaneo et al. (2018). The output from this manipulation test is shown in Appendix A, Table A4 and Figures A4 and A5. In both cases, the density tests suggest there is no significant jump at the threshold. Second, potential outcomes are continuous at the cutoff, which we consider a plausible assumption in this setting. Finally, the effect of confounding policies below and above the population threshold are constant over time (also known as local parallel trends).

5. RESULTS

5.1. Baseline DID analysis

Table 4 presents the estimation results of the DID regression in equation (2). In column 1, we present the results for municipalities in the 250–10,000 inhabitant interval. Columns 2, 3, 4 and 5 report the estimates for three subsamples that sequentially narrow the upper and lower bounds by 250 inhabitants (i.e., 500–9750; 750–9500; 1000–9250; 1250–9000). In all cases, we use two-way clustered standard errors at the municipality and party level to deal with cross-correlation in the residuals along the lines of Cameron et al. (2011). For illustration purposes,

Figure 3 plots the coefficient estimates and confidence intervals from each regression for the key variables of interest.

TABLE 4 HERE

FIGURE 3 HERE

The years of schooling of municipality councilors are 0.14 units higher in those municipalities under the quota system, *ceteris paribus*. Interestingly, while females are on average more educated (+0.81 years), female's schooling is -0.29 years lower after the quota implementation in treated municipalities according to the regression estimates. Accordingly, this result seems to suggest that the gender quota generates positive selection into education for male politicians only. The quota raises (decreases) the proportion of female (male) elected within councils (see Appendix, Table A3 and Table 1). Because now males compete for fewer positions, the selection process seems to favor highly educated males. In line with the findings presented by Besley et al. (2017), despite attaining lower education at the baseline, it seems that the gender quota raises the average years of schooling of males while decreasing that of females, resulting in a narrower gender gap in education.

Importantly, according to the estimates in Column 1 of Table 3, the average marginal effect of schooling with respect to the quota is 0.149 for males and -0.140 for females, with the overall marginal effect not resulting statistically significant. This implies that the quota does not change the average level of competence of municipality councilors, in line with Bagues and Campa (2021). The results suggest, instead, a compositional effect by gender.

As regards the control variables, mayors are around 1.2 years more educated than vice-councilors, which at the same time are 0.18 years more educated than councilors. This pattern may reflect a combination of factors related to parties' internal processes, such as eligibility requirements, competition, role responsibilities, and perceptions of prestige associated with each of the different roles in office. It is possible that the internal selection process for holding the top position on the party's list is more competitive, resulting in a higher proportion of individuals with higher education aspiring to become mayors compared to the rest of the councilors. As described in Section 2.2, only councilors occupying the top position on their party's list are eligible for mayoral candidacy, which further emphasizes the importance of the internal party processes in shaping the educational backgrounds of mayors.[16] We also document a clear negative correlation between years of schooling and age. Those over 66 attain 3.6 less years of education than those under 30.

The results remain similar when considering narrower samples. Nonetheless, we find that the estimates of Treated and Treated×Female become smaller in magnitude as we restrict the range of population size for the comparison, as illustrated in Figure 3. Table 5 reports the estimates from 2x2 clean comparisons that are not affected by the negative weighting issue (see Figure B1 in Appendix for a graphical illustration). In these regressions, we again find that female councilors' education is significantly lower in municipalities under the quota, with the magnitude of the effect becoming smaller over time. Concerning the rest of controls, the results remain consistent with Table 4.

TABLE 5 HERE

In Appendix B, we present additional results considering (i) separate regressions per age intervals of the politician, (ii) only mayors, and (iii) only parties that contested in the four elections. From these analyses, we document that the negative effect of the quota on females' schooling mainly holds for politicians aged 31 to 50. For those over 50, no significant effects are detected. Interestingly, no effect is found on mayors, suggesting that, if there is any impact, the gender quota affects the schooling of council members but not that of mayors. The negative effect nonetheless remains significant for the subsample of parties that contested in all elections.

5.2. Cross-sectional RD results

Table 6 reports the RD estimates according to Equations (3)-(5).[17] The first two columns present placebo estimates for the year 2003 considering both the 3,000 and 5,000 population threshold points, when the quota was not implemented. As expected, no significant differences are

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[16] Additionally, the position of mayor may be perceived as more prestigious or have higher social status, leading parties to strategically nominate individuals with higher levels of education as heads of the list, which may be more attractive to and instill greater confidence in the electorate.

[17] We use *rdrobust* (Calonico et al., 2017) in Stata 17. The bandwidth is chosen using the data-driven procedure proposed by Calonico et al. (2014). We use a local linear polynomial (p=1) with a triangular kernel function for the point estimates and a local quadratic polynomial for the bias correction. This follows recommendation by Gelman and Imbens (2019), who warn about the use of high-order polynomials.

detected before the quota implementation. Columns (3)-(5) report the results for the years 2007, 2011, and 2015. We present the results under (i) a conventional variance estimator (*conventional*), (ii) a bias-corrected RD conventional variance estimator (*bias-corrected*), and (iii) a bias-corrected robust variance estimator (*robust*). In all cases, standard errors are clustered at the municipality level following Calonico et al. (2017). In Panel A, we report the results for all the sample, whereas Panels B and C present the estimates separately for females and males, respectively. For illustration purposes, Figures C1 to C9 in Appendix present RD plots on the relationship between schooling and population at both sides of the corresponding threshold per election year considering the optimal bandwidth.

TABLE 6 HERE

There is a positive and significant effect of the quota on politicians' education in 2007. On average, an average politician in a municipality above 5,000 inhabitants attains 0.685 years of schooling more than a comparable one in a municipality below such threshold. Looking at the separate regressions by gender, this educational gap comes from an increase in females' competence. In particular, female councilors in the first election under the gender quota have 0.974 years of schooling more than comparable female politicians in municipalities not subject to the quota. In contrast, males' schooling does not differ at both sides of the threshold. This finding is quite puzzling and contradicts evidence from the DID analysis.

The positive effect on females' schooling vanishes in the 2011 and 2015 elections: no significant differences are found at both sides of the population threshold, with the only exception that elected males are more educated in municipalities over 3,000 inhabitants in 2015. Importantly, the point estimates and standard errors remain consistent when we do not consider covariates and run the simplest version of the RD estimator (Appendix, Table C1) and when we consider distinct MSE-optimal bandwidths for below and above the cutoff instead of a common one (Appendix, Table C2).

How can we reconciliate these results from those in Tables 4 and 5? Bagues and Campa (2021) argue that the RD results should be more reliable because it is unlikely that the parallel trends assumption holds. Nonetheless, one concern about the RD results is that municipalities at both sides of the border might differ in unobservables that influence politicians' schooling other than the quota (e.g., Brollo et al., 2013).

5.3. Difference in discontinuities regression results

Table 7 presents the estimation results from the DIF-IN-DISC strategy in Equation (6).[18] Figure 5 plots the coefficient estimates and confidence intervals for the variables of interest. As in Tables 4 and 5, female politicians are on average more educated. However, their years of schooling decrease with population size, implying that the gender gap in politicians' education in the favor of females becomes smaller in large municipalities. This result is consistent with descriptive evidence in Table 2 and Figure 2. The average marginal effect for female in Column (1) is 0.93 for a municipality of 1,000 inhabitants but 0.36 in a municipality with 9,500 inhabitants. Politicians' years of schooling are also increasing in population size, being the semi-elasticity at the sample means equal to 0.56.[19]

TABLE 7 HERE

FIGURE 4 HERE

Crucially, neither Treated, Treated×Female or Treated×Female×Ln Pop are now statistically significant. Contrary to the baseline DID and RD results, these regressions indicate that there are no differences in the average years of schooling of elected politicians in municipalities affected by the gender quota, neither on average nor by gender, once we condition on differences in schooling associated with population size.[20] That is, the negative effect we documented in the baseline DID regressions was misleadingly pointing to quotas decreasing (increasing) the competence of elected female (male) politicians. Instead, it seems that such negative effect was capturing the fact that the positive schooling gap in the favor of females becomes smaller as population grows over time, as visually presented in Figure 2.

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[18] As in the DID regressions, standard errors are two-way clustered at the municipality and party level. Results are nonetheless robust to clustering only at the municipality level (available upon request).

[19] Note the regressions include dummies for medium-sized and large municipalities, which already control for broad size differences. As such, the coefficient for the log of population measures variation in schooling per one percent increase in population within municipality size categories.

[20] As illustrated in Figure D1 in Appendix, there is high imprecision in the estimation of Treated×Female, which partially emerges due to collinearity with Treated×Female×Ln Pop. Nonetheless, the non-significance remains when we run alternative specifications omitting one of the two terms (available upon request).

Table 8 reports the results from separate 2x2 difference in discontinuity regressions, analogous to Table 5, comparing each election year under the quota with the baseline year 2003. Figure 5 compares the coefficients of interest. Again, we find that there are no significant differences in the years of schooling of elected politicians in municipalities affected by the quota as compared to 2003.

TABLE 8 HERE

FIGURE 5 HERE

To better illustrate this result, Figures 5, 6 and 7 depict binscatterplots (Cattaneo et al., 2024) of the revisualized cross-sectional relationship between years of schooling and population for each year as compared to 2003 after controlling for the charge of the politician (mayor and councilor), his/her age interval and party fixed effects. Panel A presents the results for females and Panel B for males. These Figures show that, both for males and females, the relationship between schooling and population size is very similar before and after the quota implementation at both sides of the threshold. The lack of statistical significance of the coefficient for Treated×Female×Ln Pop is consistent with the fact that the change in slope after the threshold is similar for males and females in each year as compared to 2003.

FIGURE 5 HERE

FIGURE 6 HERE

FIGURE 7 HERE

In Appendix D, we present additional results considering (i) separate regressions per age intervals of the politician, (ii) only mayors, and (iii) only parties that contested in the four elections. The results remain consistent with Table 7, and the estimate for Treated×Female is never found to be significant at conventional levels.

5.4. Robustness checks and additional analyses

A first robustness check exercise involves re-estimating our 2x2 DID equations in Table 5 but considering the average of the optimal bandwidths from the RD regressions in Table 6. Table E1 in Appendix reports the results. Consistent with the results in Table 7, now *Treated* and *Treated×Female* are not significant. Unlike Table 5, when the analysis is done for a subset of municipalities that lie in a narrow bandwidth around the population threshold, no significant differences in politicians' schooling are found.

The channel through which the quota allegedly affects politicians' education is through the gender composition effects it generates among elected councils. In municipalities with gender parity pre-treatment, the policy is unlikely to produce significant effects. On the contrary, the quota potentially triggers larger male for female substitutions in municipality with a high male-female imbalance in 2003. To inspect this, we have re-estimating our 2x2 DID equation considering the average of the optimal bandwidths from the RD regressions and interacting *Treated*×*Female* with the share of females in the council in the municipality in 2003. In this way, we test whether the effect depends on the municipality exposure to the policy. This interaction term, shown in Table E2 in Appendix, is not statistically significant. Therefore, the effect of quotas on politicians' competence is not contingent on their pre-policy degree of feminization.

In a similar fashion, one could consider that the effects of quotas on politicians' schooling could be contingent on their levels of education pre-treatment. We also run 2x2 DID regressions restricting the sample to observations that lie within the average optimal bandwidth from Table 6 and interacting *Treated*×*Female* with the average schooling of municipality councilors in 2003. As presented in Table E3 in Appendix, this interaction term is not significant. This check therefore confirms that gender quotas have no effect on the educational attainment of municipality councilors, and this null effect is not driven by the exposure to the policy.

Because our dataset is a repeated cross-section, the number of observations per municipality (councils) is proportional to its population size. To examine the potential confounding effect of the imbalance in sample observations per municipality to each side of the threshold, we have run RD regressions in first differences by averaging schooling at the municipality level, as done by Bagues and Campa (2021). That is, we first average the years of schooling of councilors (males, females, and both) for each municipality and period and collapse the data at the municipality-period level. Next, we take differences in the mean years of schooling of each election year (also for males, females and both) always relative to the pre-treatment year (2003). Table E4 in Appendix reports the estimation results. Consistent with Table 8, no significant effects are found for the years 2007 and 2011. For 2015, though, the RD estimator in first differences detects a lower average level of education as compared to 2003 but no effect on males’ and females’ education.

6. CONCLUDING REMARKS

This paper is among the first medium-to-long-term analyses of the compositional effects of gender quotas on the educational level of politicians. We have used a rich dataset on elected councilors in Spanish municipalities over four legislative periods (2003-2015). We have evaluated the changes in the educational attainment of politicians caused by the quasi-experimental introduction of gender quotas in two distinct but consecutive electoral periods, each with different population thresholds. Our empirical strategy has exploited the discontinuity in the application of the quota based on population thresholds in a pre-post setting using a difference in discontinuities research design à la Grembi et al. (2016). The effect of the quota on politicians’ education is identified by comparing outcome trajectories before and after the quota implementation in municipalities below and above the population threshold.

Our analysis does not yield evidence of quotas having significant impact on the educational composition of municipality councils. No effect is neither found when focusing on mayors. These findings reveal that gender quotas are neutral to politicians’ quality: while increasing female representation in local councils, gender quotas have no effect on the competence of elected politicians. Our results suggest that those females entering municipality councils following the quota enforcement attain about the same educational level than that of pre-quota incumbent females. At the same time, the replacement of males in exchange of females does not obey to educational attainment. These results remain robust to a battery of checks.

The null effect of quotas on politicians’ competence aligns with previous evidence for Spain presented by Bagues and Campa (2021). While these authors use data on politicians’ average education at the municipality level, our analysis shows that no compositional effects by gender are present: the introduction of the gender quota in local elections does not increase nor decrease the educational attainment of male and female politicians.

Overall, our results contribute to a more nuanced understanding of how gender quotas influence the composition and competence of elected officials. This has broader implications for policymakers aiming to design effective gender parity interventions. It suggests that the success of such policies may be contingent on local demographic and political environments, rather than the quotas themselves. Thus, future policies should be designed to account for these varying local conditions to achieve desired outcomes in gender representation and competence among elected officials.

Our findings are subject to some limitations that should be acknowledged, which could constitute valuable avenues for future research. The effectiveness of gender quotas may vary across different political contexts, and our findings may not generalize to other countries with different local legislative bodies. Additionally, we have used the educational level of elected councilors as our measure of political competence. However, politicians' quality is multidimensional and could be alternatively assessed using other individual traits that we do not avail in our data. In this respect, we lack information on other relevant characteristics at the individual like political experience or the opportunity cost of remaining in politics (e.g., wage in the labor market). This is left as an avenue for future work.

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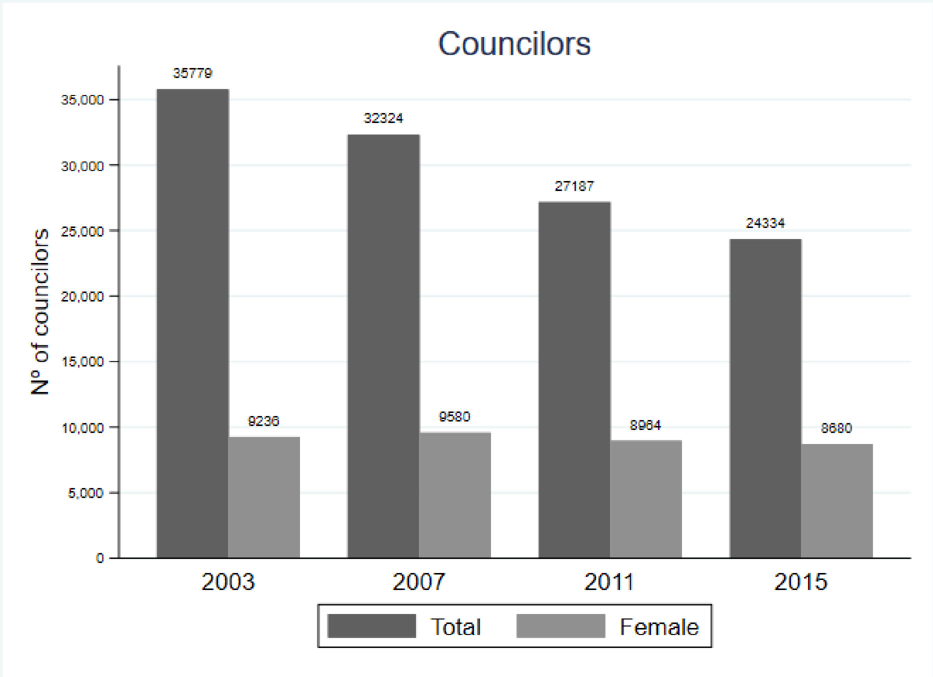

Figure 1. Data coverage

Table 1. Proportion tests of females before and after the quota implementation

	Less than 5,000 inhabitants		More than 5,000 inhabitants		
	Mean	SE	Mean	SE	Diff
Share females 2003	0.250	0.002	0.301	0.006	-0.051***
Share females 2007	0.281	0.002	0.373	0.006	-0.091***
	Less than 3,000 inhabitants		More than 3,000 inhabitants		
	Mean	SE	Mean	SE	Diff
Share females 2003	0.253	0.002	0.293	0.007	-0.039***
Share females 2011	0.319	0.003	0.392	0.008	-0.072***
Share females 2015	0.347	0.003	0.412	0.008	-0.064***

Table 2. Descriptive statistics of years of schooling

		All		Female		Male		t-test
		Mean	SD	Mean	SD	Mean	SD	
Panel A: All Sample		11.68	3.72	12.53	3.55	11.31	3.74	-52.38***
Panel B: Election periods								
	2003	11.25	3.73	12.11	3.64	10.96	3.72	-25.88***
	2007	11.59	3.68	12.34	3.53	11.27	3.70	-24.14***
	2011	11.88	3.72	12.70	3.50	11.47	3.75	-25.90***
	2015	12.22	3.68	12.98	3.43	11.80	3.76	-24.29***
Panel C: Population thresholds by election periods								
Less than 3,000 inhabitants	2003	10.85	3.66	11.78	3.63	10.55	3.62	-23.50***
	2007	11.15	3.63	11.95	3.52	10.85	3.62	-20.62***
	2011	11.43	3.70	12.34	3.53	11.04	3.70	-22.38***
	2015	11.76	3.68	12.60	3.48	11.34	3.71	-21.19***
Between 3,000-5,000 inhabitants	2003	11.95	3.74	12.46	3.63	11.74	3.78	-5.79***
	2007	12.22	3.67	12.74	3.60	11.97	3.69	-6.44***
	2011	12.56	3.65	13.10	3.47	12.23	3.74	-7.01***
	2015	12.92	3.60	13.42	3.41	12.58	3.69	-6.69***
More than 5,000 inhabitants	2003	12.60	3.69	13.08	3.51	12.39	3.74	-6.54***
	2007	12.97	3.49	13.30	3.31	12.77	3.59	-5.38***
	2011	13.12	3.48	13.49	3.28	12.88	3.58	-5.94***
	2015	13.61	3.32	13.96	3.03	13.36	3.49	-5.70***

Notes: The Table presents descriptive statistics of politicians' years of schooling, for all the sample and separately for female and male councilors. In Panel A we report statistics for all the sample period. Panel B provides information for each election year. In Panel C we further differentiate by municipality size. A t-test for mean difference between males and females is also reported in the last column. *** p<0.01, ** p<0.05, * p<0.1.

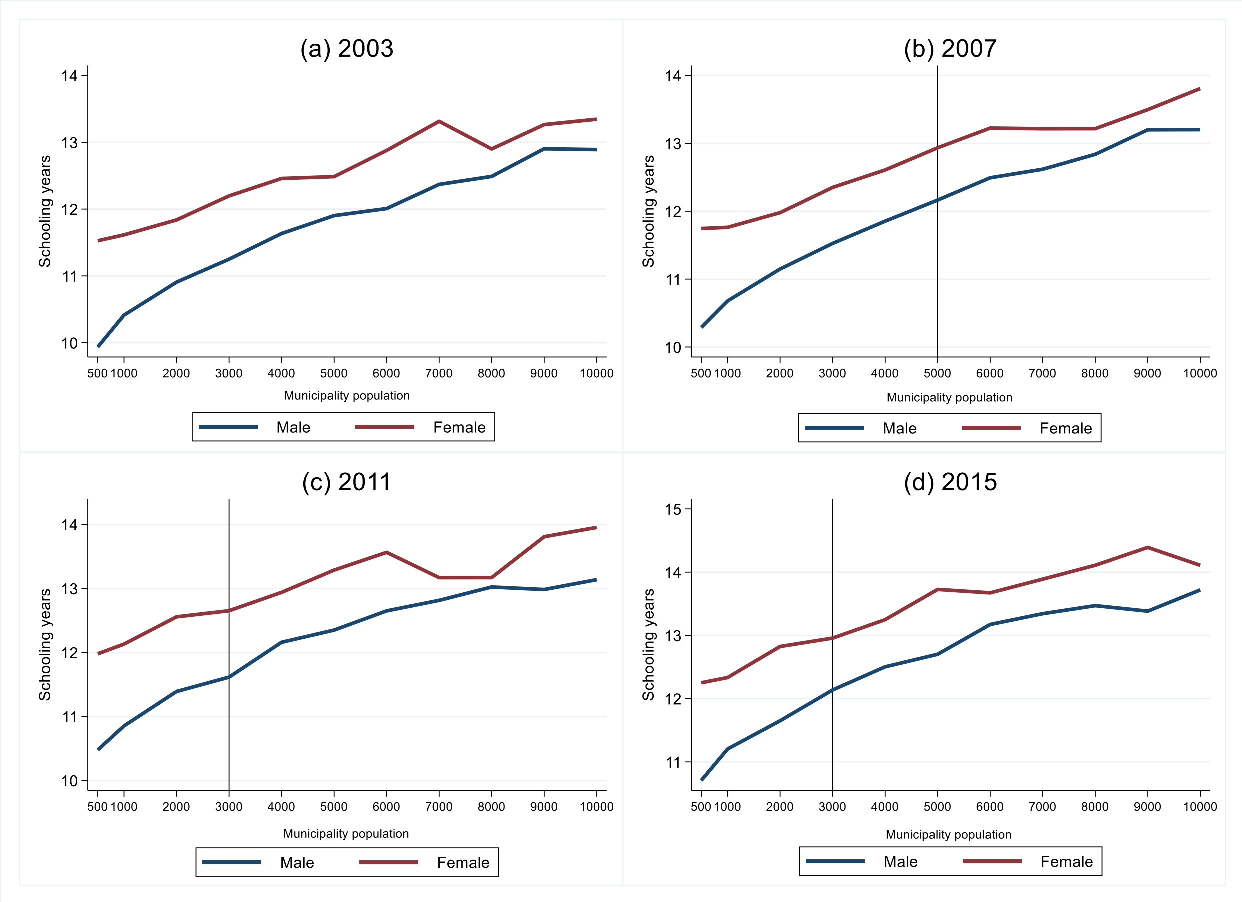

Figure 2. Average years of schooling for males and females by population size and election year

Table 3. Summary statistics of control variables

	All councilors (N=119,624)		Females (N=36,460)		Males (N=83,164)		
Variable	Mean	SD	Mean	SD	Mean	SD	t-test
Politician's office							
Mayor	0.13	0.33	0.07	0.25	0.15	0.36	-41.72***
Vice-Mayor	0.16	0.36	0.16	0.36	0.16	0.36	0.47
Councilor	0.72	0.45	0.78	0.42	0.69	0.46	30.23***
Age							
Less than 30	0.12	0.33	0.17	0.37	0.10	0.30	32.57***
Between 31-50	0.60	0.49	0.66	0.47	0.58	0.49	26.61***
Between 51-65	0.24	0.43	0.16	0.37	0.27	0.45	-42.39***
Over 66	0.04	0.19	0.01	0.12	0.05	0.22	-29.49***
Municipality size							
Less than 3,000 inhabitants	0.71	0.46	0.65	0.48	0.73	0.44	-26.54***
Between 3,000-5,000 inhabitants	0.13	0.34	0.15	0.36	0.12	0.33	13.49***
More than 5,000 inhabitants	0.16	0.37	0.20	0.40	0.15	0.36	20.35***

Table 4. Baseline difference-in-differences regression results

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.149***	0.109**	0.105*	0.094*	0.104**
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
Female	0.816***	0.748***	0.725***	0.686***	0.680***
	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Treated×Female	-0.290***	-0.200***	-0.191***	-0.158***	-0.147***
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
Mayor	1.267***	1.381***	1.414***	1.404***	1.440***
	(0.08)	(0.09)	(0.10)	(0.11)	(0.12)
Councilor	-0.183***	-0.206***	-0.213***	-0.237***	-0.245***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Between 31-50	-1.088***	-1.114***	-1.093***	-1.104***	-1.104***
	(0.07)	(0.09)	(0.09)	(0.12)	(0.11)
Between 51-65	-2.457***	-2.484***	-2.491***	-2.506***	-2.506***
	(0.05)	(0.07)	(0.08)	(0.10)	(0.11)
Over 66	-3.607***	-3.522***	-3.614***	-3.625***	-3.623***
	(0.14)	(0.19)	(0.22)	(0.24)	(0.26)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,864	3,480	2,826	2,417	2,079
Num. Parties	36	35	34	34	34
Observations	119,567	95,521	81,947	73,128	64,530
Mean dep. variable	11.69	11.91	12.05	12.15	12.26

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, vice-mayor and less than 31 years old.

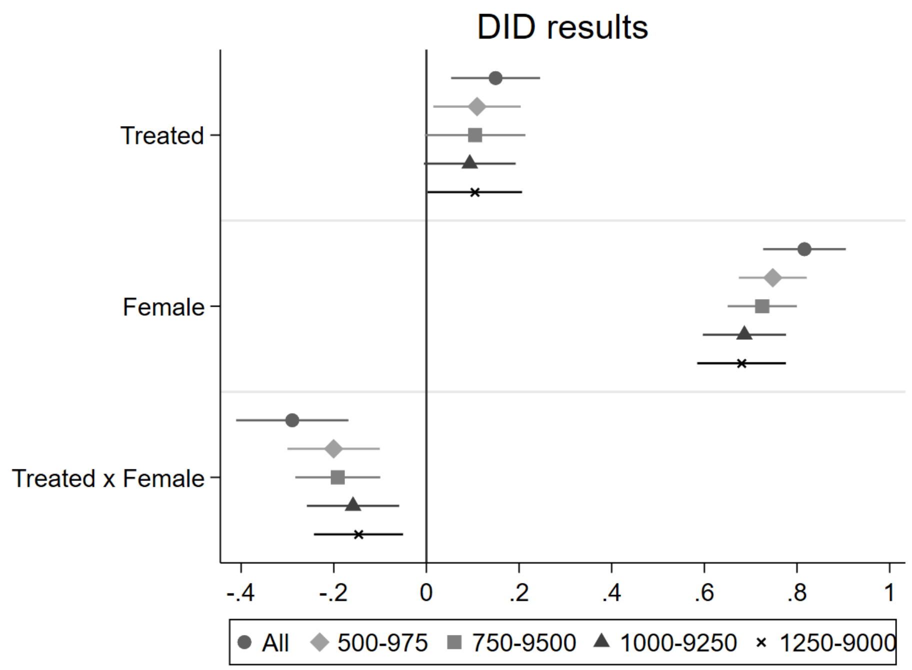

Figure 3. DID results for key variables of interest per subsamples

Table 5. Baseline difference-in-differences regression results (2x2 comparison)

	(1)	(2)	(3)
	2003 vs 2007	2003 vs 2011	2003 vs 2015
	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.143***	0.148	0.152**
	(0.05)	(0.10)	(0.06)
Female	0.720***	0.823***	0.810***
	(0.05)	(0.07)	(0.05)
Treated×Female	-0.378***	-0.289***	-0.202***
	(0.10)	(0.10)	(0.06)
Mayor	1.262***	1.338***	1.339***
	(0.08)	(0.07)	(0.07)
Councilor	-0.175***	-0.157***	-0.133***
	(0.04)	(0.04)	(0.02)
Between 31-50	-1.155***	-1.220***	-1.196***
	(0.08)	(0.08)	(0.05)
Between 51-65	-2.581***	-2.644***	-2.702***
	(0.08)	(0.07)	(0.05)
Over 66	-3.684***	-3.897***	-3.715***
	(0.21)	(0.13)	(0.12)
Year FE	YES	YES	YES
Muni FE	YES	YES	YES
Party FE	YES	YES	YES
Num. Municipalities	4,678	4,753	4,723
Num. Parties	21	25	33
Observations	68,010	62,874	60,000
Mean dep. variable	11.42	11.53	11.65

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(4) report the estimates considering the years 2003-2007, 2003-2011, and 2003-2015, respectively. In all the regression, municipalities with more than 250 and less than 10,000 inhabitants are considered. The reference categories are males, vice-mayor, less than 31 years old and less than 3,000 inhabitants.

Table 6. Regression Discontinuity (RD) estimates

	(1)	(2)	(3)	(4)	(5)
	Year 2003 (placebo)	Year 2003 (placebo)	Year 2007	Year 2011	Year 2015
Panel A: All	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Conventional	0.033	-0.189	0.561*	0.104	-0.051
	(0.324)	(0.244)	(0.310)	(0.226)	(0.241)
Bias-corrected	0.012	-0.205	0.685**	0.116	-0.084
	(0.324)	(0.244)	(0.310)	(0.226)	(0.241)
Robust	0.012	-0.205	0.685*	0.116	-0.084
	(0.389)	(0.290)	(0.361)	(0.270)	(0.284)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	35,779	35,779	32,324	27,187	24,334
Opt. Bandwidth	813.10	1,968.45	1,495.54	1,477.25	1,170.45
Num. observations	3,373	4,338	2,759	5,713	3,758
(municipalities) left of cutoff	(650)	(848)	(639)	(1,529)	(974)
Num. observations	2,216	3,113	2,364	3,086	2,257
(municipalities) right of cutoff	(318)	(352)	(342)	(497)	(411)
Panel B: Only females					
Conventional	0.238	0.205	0.811*	-0.171	-0.594
	(0.422)	(0.396)	(0.416)	(0.393)	(0.441)
Bias-corrected	0.173	0.239	0.974**	-0.266	-0.773*
	(0.422)	(0.396)	(0.416)	(0.393)	(0.441)
Robust	0.173	0.239	0.974**	-0.266	-0.773
	(0.506)	(0.467)	(0.485)	(0.463)	(0.519)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	9,236	9,236	9,580	8,964	8,680
Opt. Bandwidth	983.08	1,669.07	1,690.10	873.13	681.36
Num. observations	1,309	7,609	1046	1,071	724
(municipalities) left of cutoff	(763)	(559)	(736)	(592)	(444)
Num. observations	739	1,627	980	772	615
(municipalities) right of cutoff	(338)	(300)	(348)	(327)	(263)
Panel C: Only Males					
Conventional	-0.050	-0.340	0.310	0.232	0.626*
	(0.342)	(0.268)	(0.332)	(0.316)	(0.353)
Bias-corrected	-0.059	-0.370	0.414	0.258	0.774**
	(0.342)	(0.268)	(0.332)	(0.316)	(0.353)
Robust	-0.059	-0.370	0.414	0.258	0.774*
	(0.409)	(0.319)	(0.394)	(0.378)	(0.404)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	26,543	26,543	22,744	18,223	15,654
Opt. Bandwidth	817.96	1,928.58	1,568.75	1,088.82	640.11
Num. observations	2,391	2,980	1,959	2,671	1,062
(municipalities) left of cutoff	(656)	(813)	(619)	(884)	(521)
Num. observations	1,585	2,151	1,538	1,464	800
(municipalities) right of cutoff	(319)	(348)	(329)	(404)	(306)

Note: The table presents RD estimates using a local linear polynomial (p=1) for the point estimates and a local quadratic regression for the bias correction (q=2) under a triangular kernel function. Standard errors are clustered at the municipality level.

Table 7. Difference-in-discontinuity (linear) regression results

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.019	0.069	0.070	0.088	0.131**
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
Female	2.757***	2.347***	2.648***	2.258***	2.263***
	(0.28)	(0.22)	(0.25)	(0.36)	(0.65)
Treated×Female	-1.491	-0.859	-0.779	-0.185	-0.298
	(1.03)	(1.04)	(1.04)	(1.00)	(0.95)
Ln Pop	0.611***	0.714***	0.787***	0.802***	0.746***
	(0.03)	(0.04)	(0.05)	(0.08)	(0.09)
Female×Ln Pop	-0.264***	-0.208***	-0.248***	-0.199***	-0.198**
	(0.04)	(0.03)	(0.04)	(0.05)	(0.09)
Treated×Female×Ln Pop	0.179	0.099	0.093	0.021	0.033
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)
Major	1.244***	1.363***	1.408***	1.419***	1.456***
	(0.08)	(0.08)	(0.10)	(0.10)	(0.11)
Councilor	-0.229***	-0.246***	-0.243***	-0.251***	-0.253***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Between 31-50	-1.079***	-1.108***	-1.090***	-1.105***	-1.108***
	(0.06)	(0.08)	(0.08)	(0.09)	(0.09)
Between 51-65	-2.461***	-2.483***	-2.507***	-2.519***	-2.513***
	(0.04)	(0.06)	(0.06)	(0.06)	(0.08)
Over 66	-3.658***	-3.552***	-3.658***	-3.663***	-3.670***
	(0.12)	(0.19)	(0.17)	(0.19)	(0.21)
Between 3,000-5,000 inhabitants	0.231***	0.110*	0.061	0.029	0.040
	(0.05)	(0.06)	(0.07)	(0.08)	(0.08)
More than 5,000 inhabitants	0.540***	0.348***	0.263**	0.210*	0.229*
	(0.08)	(0.09)	(0.11)	(0.11)	(0.12)
Year FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,921	3,589	2,921	2,499	2,163
Num. Parties	36	35	34	34	34
Observations	119,624	96,796	83,149	74,257	65,820
Mean dep. variable	11.69	11.92	12.06	12.15	12.25

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, vice-mayor, less than 30 years old and less than 3,000 inhabitants.

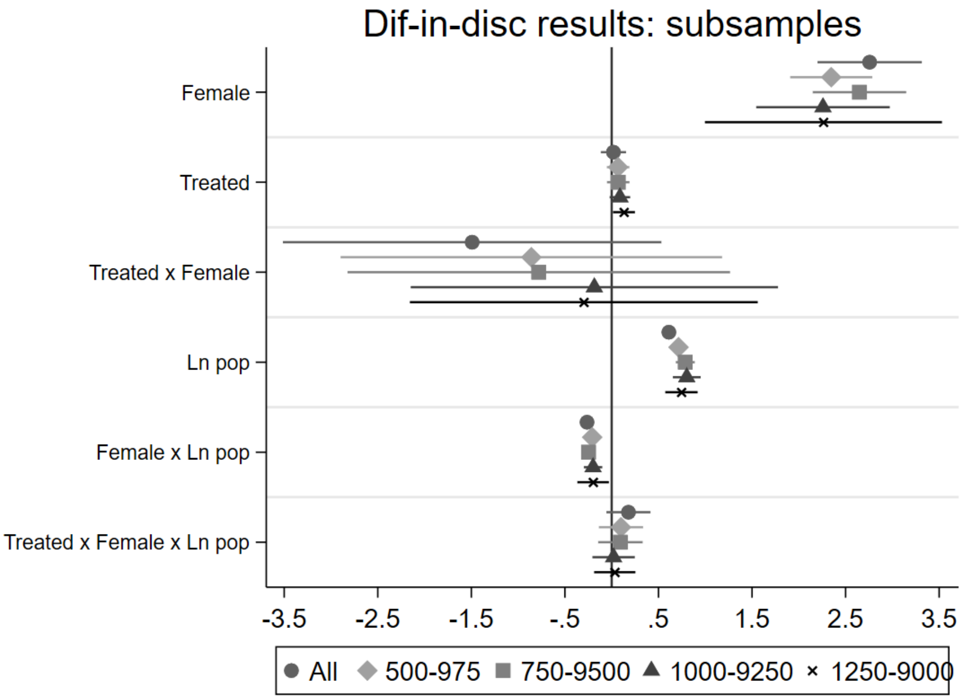

Figure 4. Difference in discontinuity results for key variables of interest per subsamples

Table 8. Difference-in-discontinuity (linear) regression results (2x2 comparison)

	(1)	(2)	(3)
	2003 vs 2007	2003 vs 2011	2003 vs 2015
	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.116	-0.034	-0.031
	(0.09)	(0.11)	(0.09)
Female	2.634***	2.694***	2.730***
	(0.34)	(0.22)	(0.28)
Treated×Female	-1.992	-1.117	-2.002
	(2.56)	(1.41)	(1.25)
Ln Pop	0.633***	0.607***	0.629***
	(0.03)	(0.03)	(0.03)
Female×Ln Pop	-0.253***	-0.251***	-0.259***
	(0.04)	(0.03)	(0.03)
Treated×Female×Ln Pop	0.221	0.129	0.246*
	(0.29)	(0.16)	(0.14)
Major	1.251***	1.329***	1.331***
	(0.07)	(0.06)	(0.06)
Councilor	-0.210***	-0.194***	-0.168***
	(0.04)	(0.03)	(0.03)
Between 31-50	-1.177***	-1.209***	-1.194***
	(0.06)	(0.07)	(0.05)
Between 51-65	-2.606***	-2.641***	-2.704***
	(0.05)	(0.04)	(0.05)
Over 66	-3.794***	-3.942***	-3.733***
	(0.17)	(0.12)	(0.13)
Between 3,000-5,000 inhabitants	0.202***	0.260***	0.200**
	(0.06)	(0.07)	(0.09)
More than 5,000 inhabitants	0.525***	0.564***	0.533***
	(0.09)	(0.08)	(0.09)
Year FE	YES	YES	YES
Party FE	YES	YES	YES
Num. Municipalities	4,771	4,845	4,836
Num. Parties	21	25	33
Observations	68,103	62,966	60,113
Mean dep. variable	11.42	11.53	11.65

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(4) report the estimates considering the years 2003-2007, 2003-2011, and 2003-2015, respectively. In all the regression, municipalities with more than 250 and less than 10,000 inhabitants are considered. The reference categories are males, vice-mayor, less than 30 years old and less than 3,000 inhabitants.

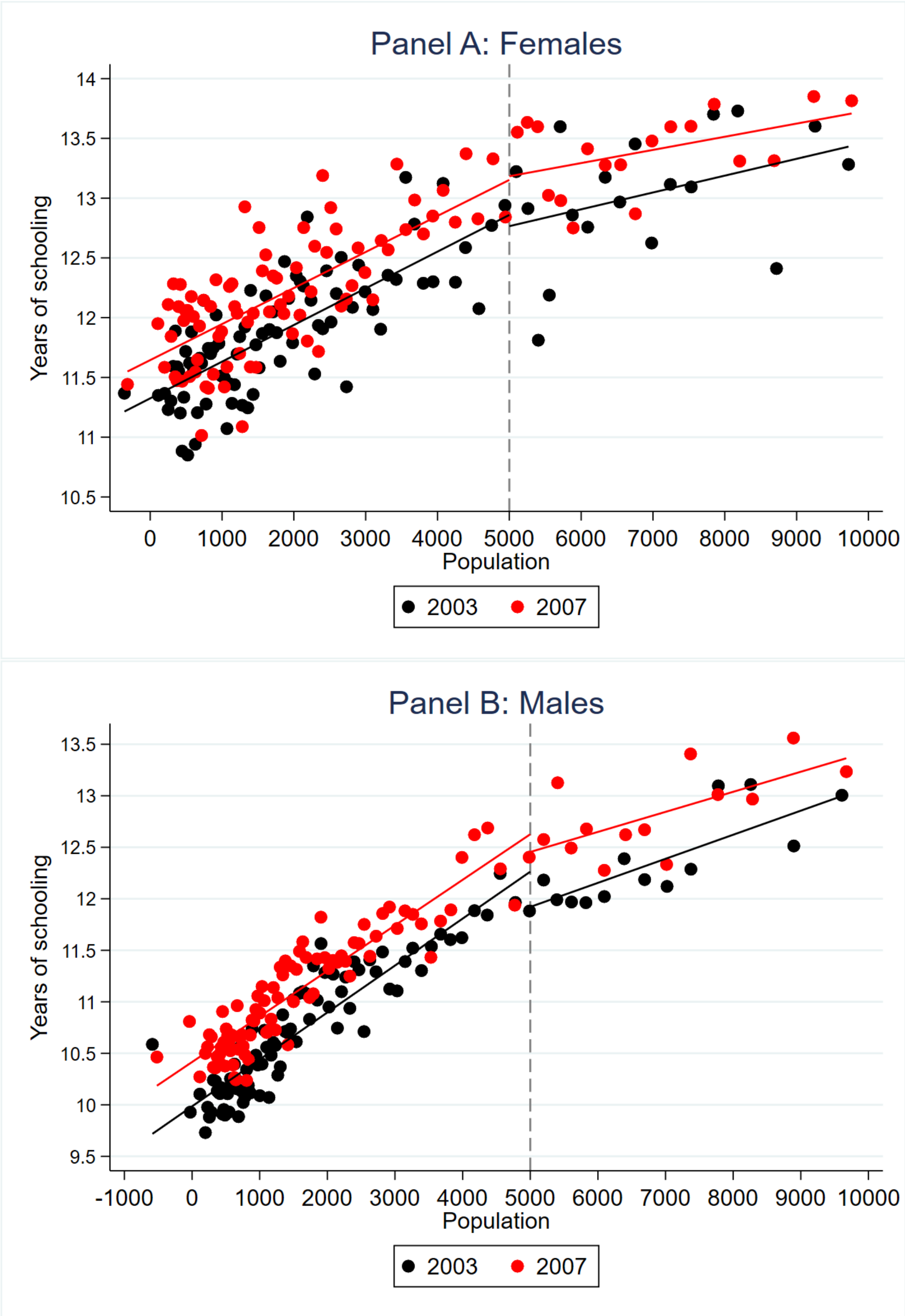

Figure 5. Binscatterplot of the residualised relationship between years of schooling and population size by gender in years 2003 and 2007

*Note: the two variables have been residualised by Mayor, Councilor, Between 31-50, Between 51-65, Over 66, and party fixed effects

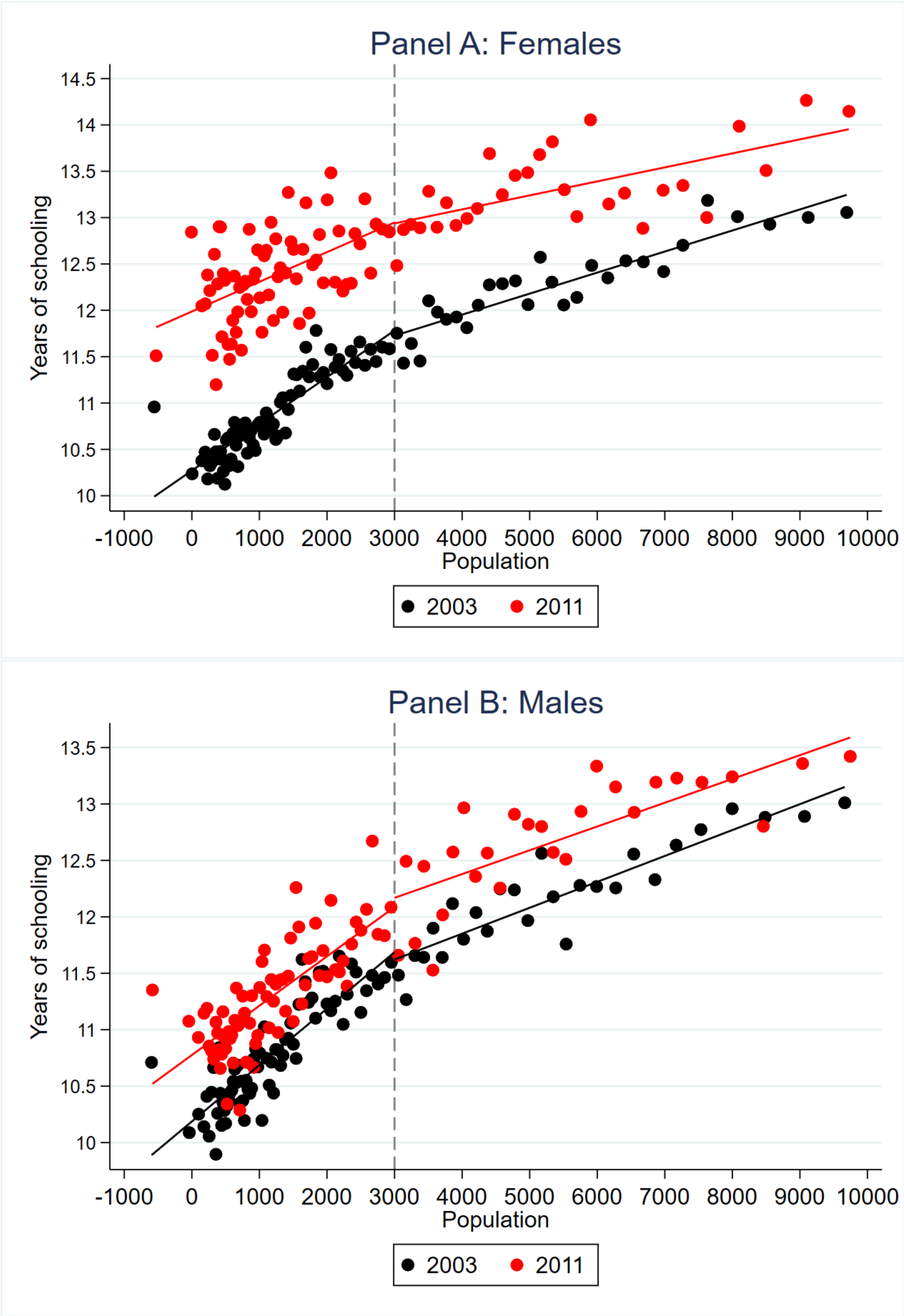

Figure 6. Binscatterplot of the residualised relationship between years of schooling and population size by gender in years 2003 and 2011

*Note: the two variables have been residualised by Mayor, Councilor, Between 31-50, Between 51-65, Over 66, and party fixed effects

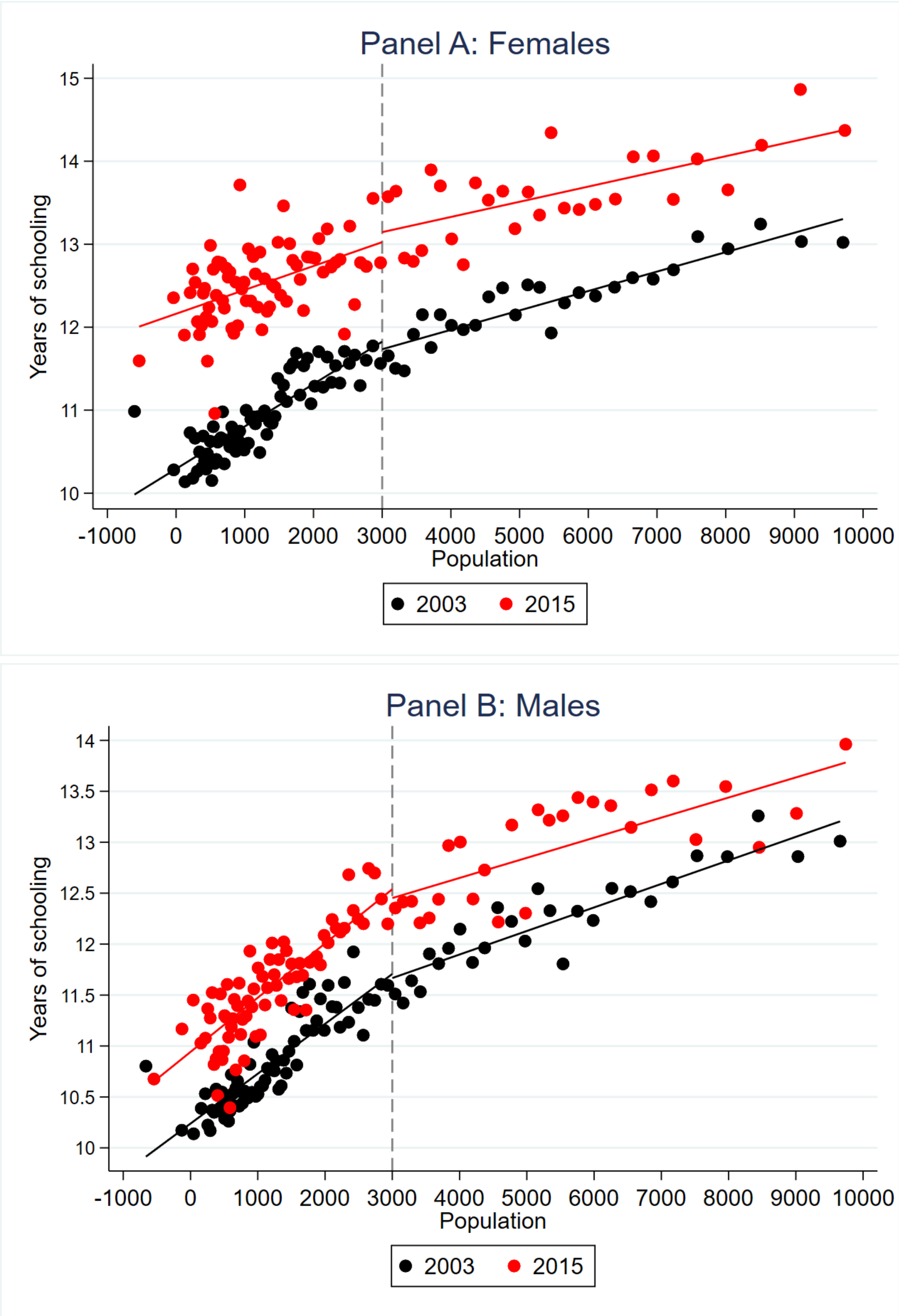

Figure 7. Binscatterplot of the residualised relationship between years of schooling and population size by gender in years 2003 and 2015

*Note: the two variables have been residualised by Mayor, Councilor, Between 31-50, Between 51-65, Over 66, and party fixed effects.

ONLINE APPENDIX

A. **DESCRIPTIVE STATISTICS**
B. **DIFFERENCE-IN-DIFFERENCES RESULTS**
C. **REGRESSION DISCONTINUITY RESULTS**
D. **DIFFERENCE IN DISCONTINUITY RESULTS**
E. **ADDITIONAL ROBUSTNESS CHECKS**

APPENDIX A. DESCRIPTIVE STATISTICS

Table A1. Equivalence of attained educational level and equivalent years of schooling (obs=123,043)

Equivalent years of schooling	Attained educational level	%	% Males	% Females
2	No studies	0.65	0.79	0.34
6	Primary studies: incomplete	7.15	8.72	3.53
8	Primary studies: completed (*Bachillerato elemental, EGB, ESO*)	32.16	34.95	25.75
12	Secondary studies (*Bachillerato superior, BUP, COU*)	16.21	15.97	16.75
13	Vocational training: medium (*Formación Profesional Grado Medio*)	5.54	5.50	5.64
14	Vocational training: superior (*Formación Profesional Grado Superior*)	8.25	8.26	8.23
15	Bachelor's degree (*Diplomatura*)	15.06	12.46	21.05
16	Bachelor's degree (*Grado*)	0.13	0.08	0.23
17	Master/second cycle university studies (*Licenciatura, Arquitectura, Ingeniería Superior*)	14.20	12.59	17.89
20	PhD	0.66	0.69	0.60

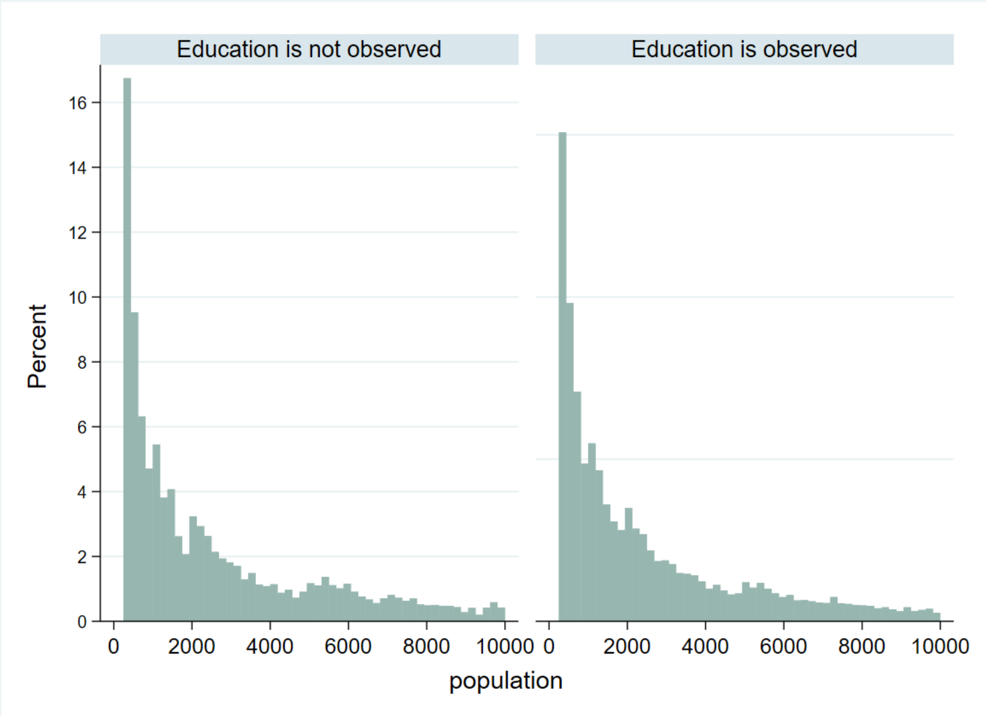

Figure A1. Histogram of population size depending on whether education level of the politician is observed or not

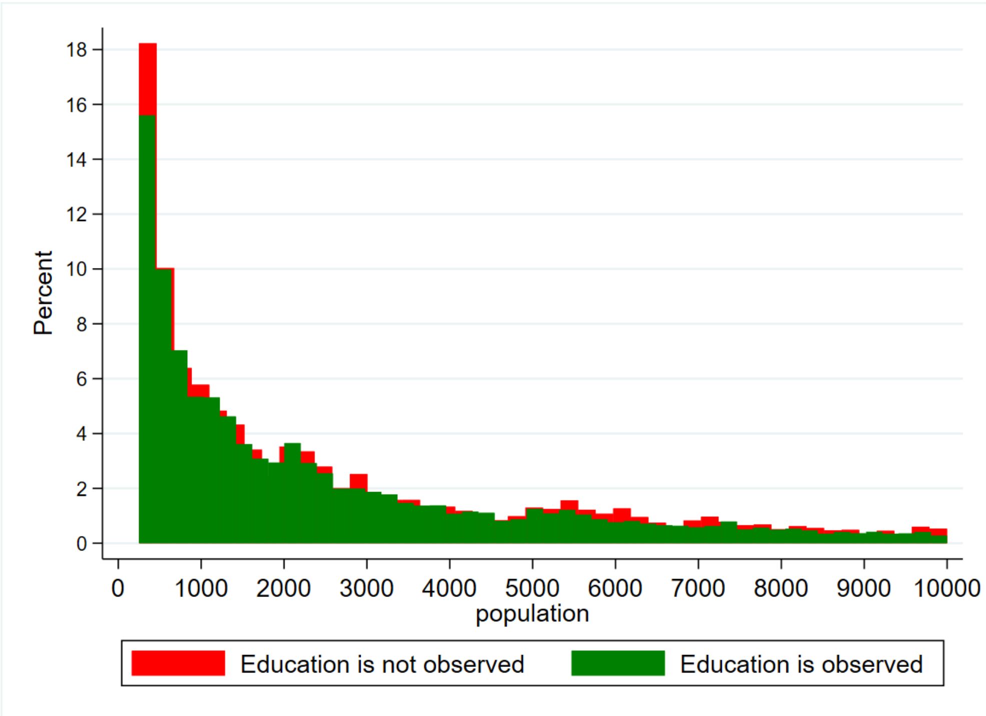

Figure A2. Overlayed histogram of population size depending on whether education level of the politician is observed or not

Table A2. Linear probability regression of the binary indicator for education observability following Equation (1)

Variables	Coef.
	(SE)
Female	-0.005
	(0.00)
Year 2007	-0.117***
	(0.01)
Female ×Year 2007	-0.001
	(0.01)
Year 2011	-0.207***
	(0.01)
Female ×Year 2011	-0.001
	(0.01)
Year 2015	-0.278***
	(0.01)
Female × Year 2015	-0.009
	(0.01)
Treated	0.007
	(0.01)
Treated × Female	0.001
	(0.01)
Mayor	0.039***
	(0.00)
Councillor	-0.066***
	(0.00)
Municipality FE	YES
Party FE	YES
Num. Municipalities	4,977
Observations	170,087
Mean dep. variable	0.72

Note: Standard errors are clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1.

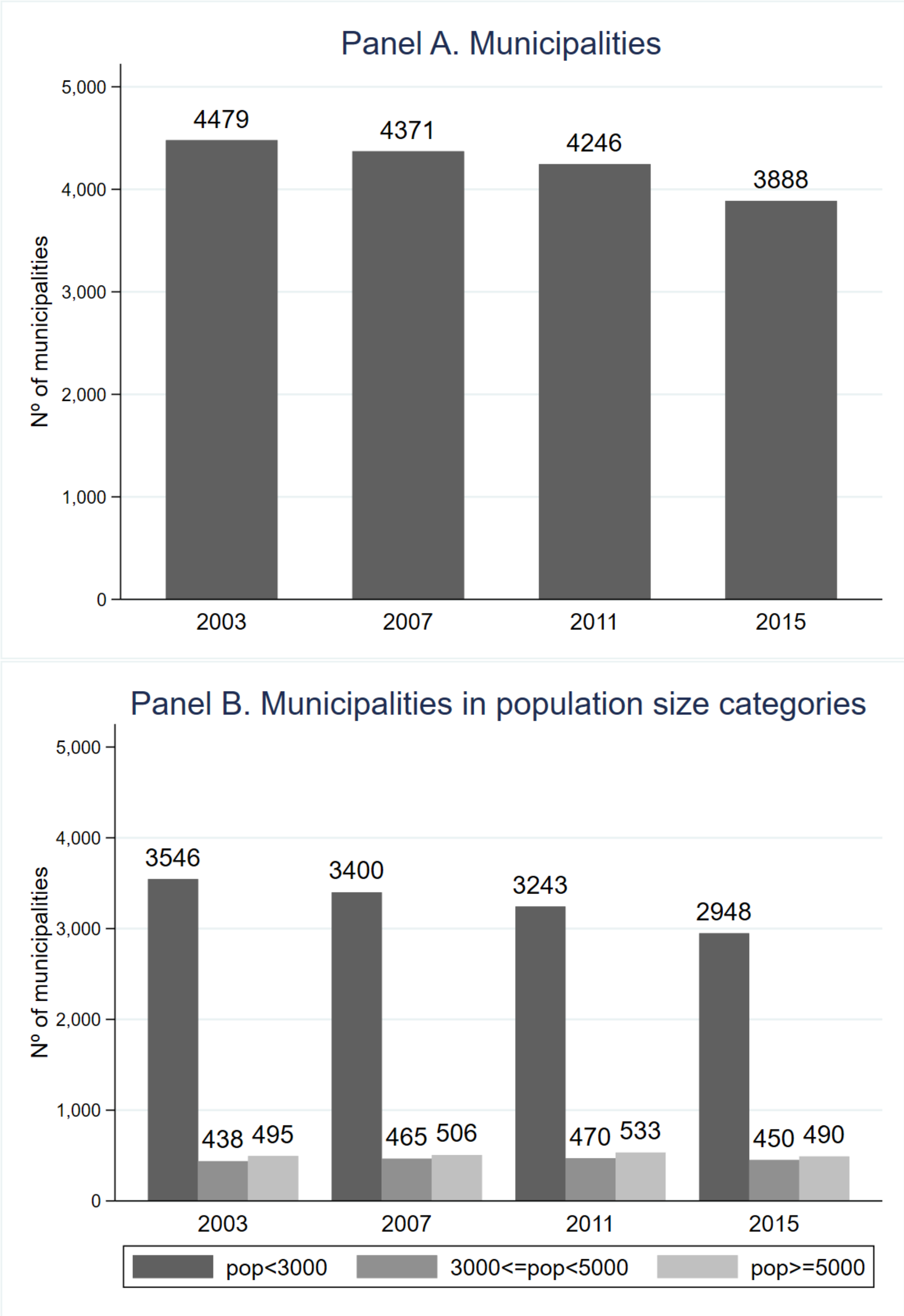

Figure A3. Number of municipalities per election year (Panel A) and number of municipalities by population size per year (Panel B)

Table A3. Number of female politicians in the sample per election year and municipality size

		Obs	%
All Sample		36,460	30.48
Election years	2003	9,236	25.81
	2007	9,580	29.64
	2011	8,964	32.97
	2015	8,680	35.67
Less than 3,000 inhabitants	All	23,800	28.20
	2003	6,291	24.30
	2007	6,210	27.31
	2011	5,628	30.04
	2015	5,671	33.32
Between 3,000-5,000 inhabitants	All	5,502	35.10
	2003	1,254	29.31
	2007	1,414	32.56
	2011	1,474	39.22
	2015	1,360	41.25
More than 5,000 inhabitants	All	7,158	36.60
	2003	1,691	30.15
	2007	1,956	37.32
	2011	1,862	39.69
	2015	1,649	41.03

Table A4. RD manipulation test using local polynomial density estimation following Cattaneo et al. (2018)

Population threshold	5000	3000
Year	2007	2011
Num. obs left cutoff	882	925
Num. obs right cutoff	1481	783
Optimal bandwidth (h) left cutoff	584.78	313.65
Optimal bandwidth (h) right cutoff	779.14	321.09
T-statistic	-0.503	-1.619
p-value	0.614	0.105

The manipulation test uses triangular kernel for a quadratic local polynomial with asymptotic plug-in standard errors. The optimal bandwidth is chosen as a combination of MSE of sum and the difference of two density estimators using *bddensity* module in Stata (Cattaneo et al., 2018)

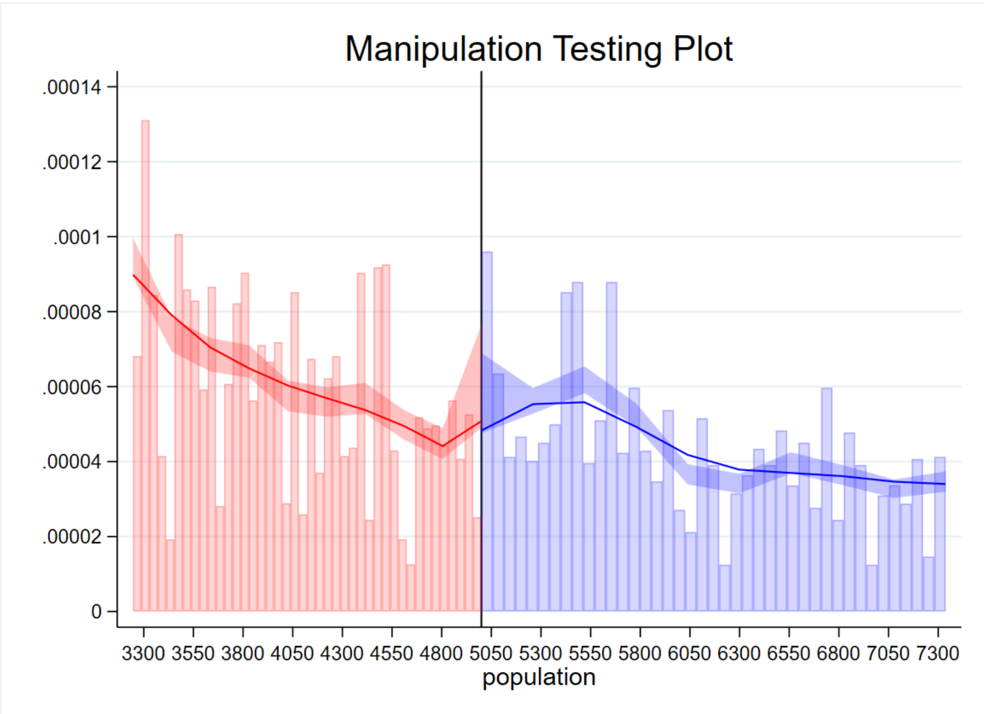

Figure A4. Manipulation testing plot following Cattaneo et al. (2018) for year 2007

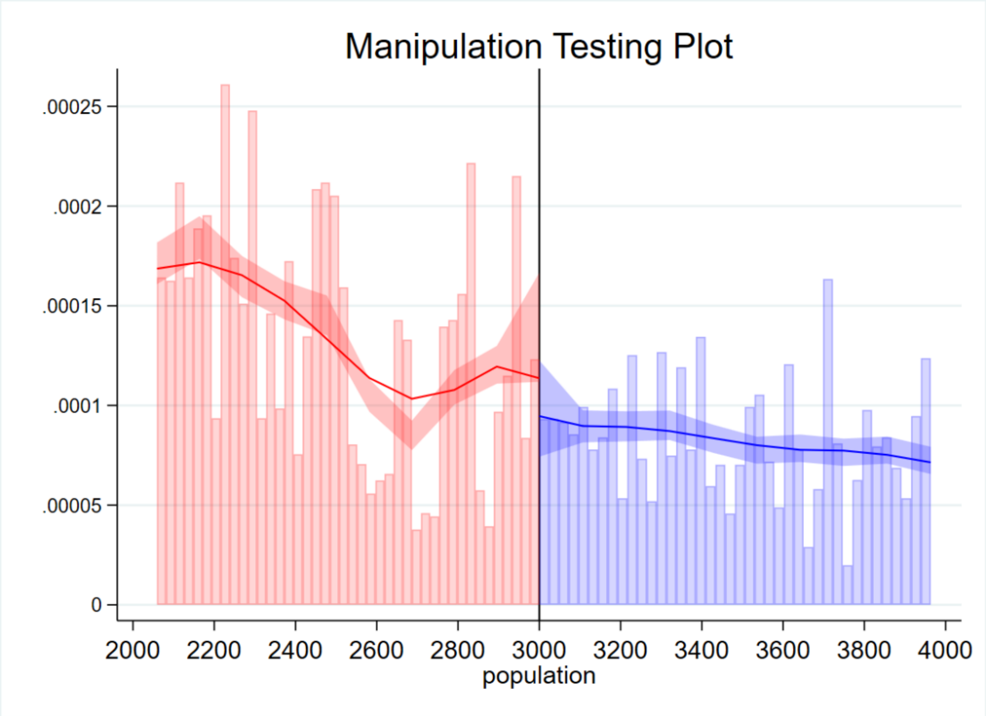

Figure A5. Manipulation testing plot following Cattaneo et al. (2018) for year 2007

APPENDIX B. DIFFERENCE-IN-DIFFERENCES RESULTS

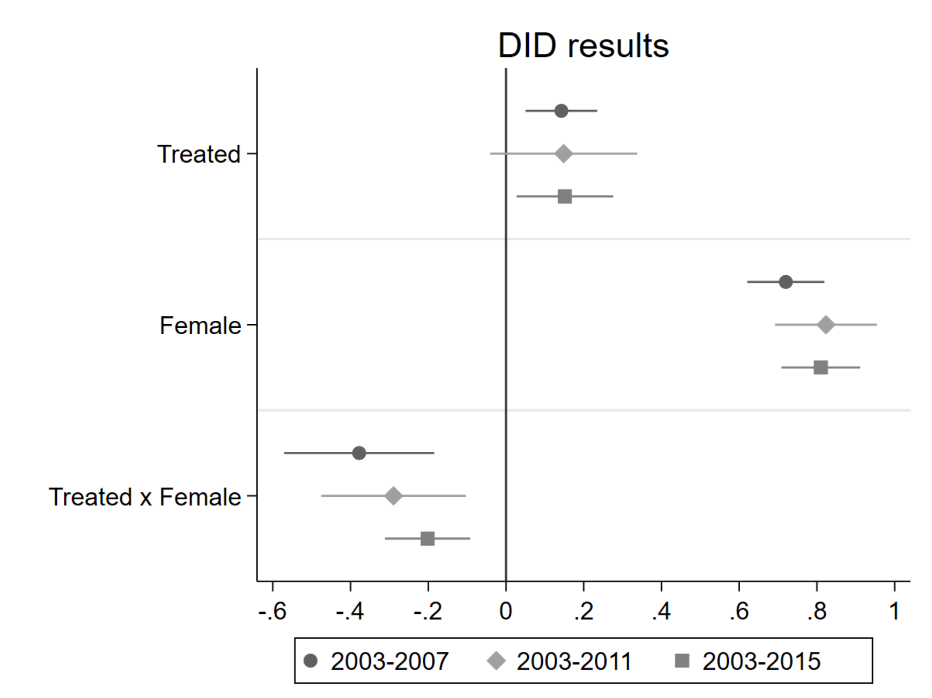

Figure B1. DID results for key variables of interest: 2x2 comparison (Table 5)

Table B1. Baseline difference-in-differences regression results: Less than 31

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.170**	0.131	0.048	-0.036	-0.071
	(0.09)	(0.11)	(0.11)	(0.17)	(0.14)
Female	1.034***	0.928***	0.885***	0.809***	0.758***
	(0.09)	(0.09)	(0.08)	(0.11)	(0.09)
Treated×Female	-0.345***	-0.219**	-0.185**	-0.111	-0.027
	(0.10)	(0.10)	(0.08)	(0.10)	(0.06)
Mayor	0.950***	1.017***	1.014***	0.946***	1.007***
	(0.28)	(0.26)	(0.23)	(0.22)	(0.23)
Councillor	-0.626***	-0.649***	-0.615***	-0.693***	-0.694***
	(0.17)	(0.17)	(0.15)	(0.15)	(0.15)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	3,221	2,507	2,118	1,857	1,627
Num. Parties	29	27	27	26	25
Observations	13,653	11,207	9,808	8,795	7,879
Mean dep. variable	12.97	13.15	13.27	13.38	13.45

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, and vice-mayor.

Table B2. Baseline difference-in-differences regression results: Between 31 and 50

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.215***	0.172***	0.143***	0.144***	0.168***
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Female	0.844***	0.769***	0.747***	0.710***	0.710***
	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
Treated×Female	-0.348***	-0.257***	-0.232***	-0.199***	-0.205***
	(0.06)	(0.06)	(0.05)	(0.06)	(0.05)
Mayor	1.245***	1.340***	1.365***	1.377***	1.386***
	(0.08)	(0.10)	(0.09)	(0.09)	(0.10)
Councillor	-0.184***	-0.204***	-0.216***	-0.240***	-0.245***
	(0.04)	(0.05)	(0.06)	(0.05)	(0.06)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,831	3,470	2,819	2,411	2,073
Num. Parties	35	34	33	33	33
Observations	71,825	58,278	50,428	45,216	40,000
Mean dep. variable	12.01	12.19	12.32	12.41	12.50

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, and vice-mayor.

Table B3. Baseline difference-in-differences regression results: Between 51 and 65

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.093	0.081	0.111	0.109	0.081
	(0.15)	(0.13)	(0.15)	(0.16)	(0.13)
Female	0.437***	0.420***	0.360***	0.335***	0.299***
	(0.07)	(0.08)	(0.08)	(0.09)	(0.11)
Treated×Female	-0.054	-0.008	0.049	0.064	0.140
	(0.15)	(0.16)	(0.16)	(0.16)	(0.18)
Mayor	1.529***	1.604***	1.693***	1.695***	1.766***
	(0.14)	(0.15)	(0.16)	(0.18)	(0.23)
Councillor	0.080	0.061	0.084	0.105	0.093
	(0.08)	(0.09)	(0.09)	(0.12)	(0.11)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,315	3,190	2,612	2,229	1,934
Num. Parties	32	32	31	31	30
Observations	28,318	22,361	18,923	16,809	14,743
Mean dep. variable	10.66	10.87	10.99	11.08	11.19

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, and vice-mayor.

Table B4. Baseline difference-in-differences regression results: More than 65

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.068	-0.110	0.253	0.390	0.457*
	(0.38)	(0.43)	(0.42)	(0.29)	(0.24)
Female	0.706**	1.030**	2.393***	2.473***	3.079***
	(0.34)	(0.49)	(0.60)	(0.62)	(0.57)
Treated×Female	0.472	0.225	-1.063	-1.217	-1.899***
	(0.52)	(0.52)	(0.72)	(0.75)	(0.57)
Mayor	1.374***	1.976***	1.763***	1.928***	1.535***
	(0.16)	(0.16)	(0.21)	(0.29)	(0.24)
Councillor	0.023	-0.085	-0.188	-0.386	-0.272
	(0.13)	(0.09)	(0.21)	(0.25)	(0.23)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	1,112	719	541	460	375
Num. Parties	22	22	22	22	20
Observations	3,441	2,135	1,572	1,315	1,073
Mean dep. variable	8.87	9.40	9.48	9.62	9.73

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, and vice-mayor.

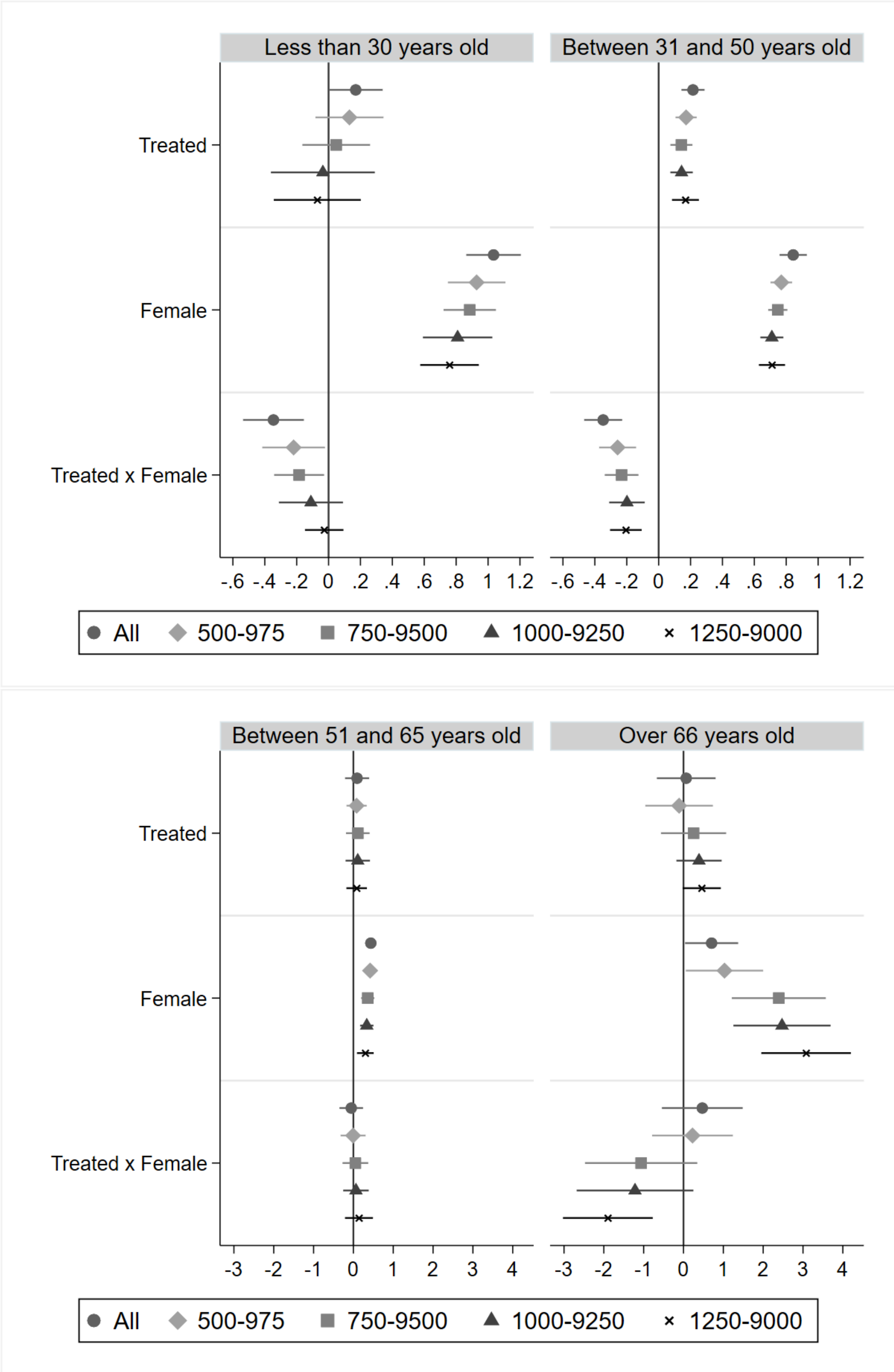

Figure B2. DID results for key variables of interest per age interval

Table B5. Baseline difference-in-differences regression results: only mayors

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.077	-0.102	-0.062	-0.031	0.034
	(0.07)	(0.09)	(0.11)	(0.12)	(0.12)
Female	1.055***	0.920***	0.839***	0.950***	1.064***
	(0.14)	(0.11)	(0.11)	(0.12)	(0.15)
Treated×Female	-0.278**	-0.157	-0.115	-0.226*	-0.317*
	(0.14)	(0.11)	(0.12)	(0.12)	(0.18)
Between 31-50	-1.052***	-1.074***	-1.019***	-0.953***	-0.955***
	(0.08)	(0.06)	(0.13)	(0.17)	(0.15)
Between 51-65	-2.167***	-2.191***	-2.136***	-2.080***	-2.048***
	(0.11)	(0.08)	(0.19)	(0.19)	(0.21)
Over 66	-3.147***	-2.706***	-2.799***	-2.836***	-2.892***
	(0.18)	(0.27)	(0.33)	(0.37)	(0.33)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,432	3,307	2,685	2,299	1,976
Num. Parties	37	34	31	29	29
Observations	14,479	11,017	8,964	7,650	6,601
Mean dep. variable	12.43	12.81	13.02	13.17	13.33

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males and less than 30 years old.

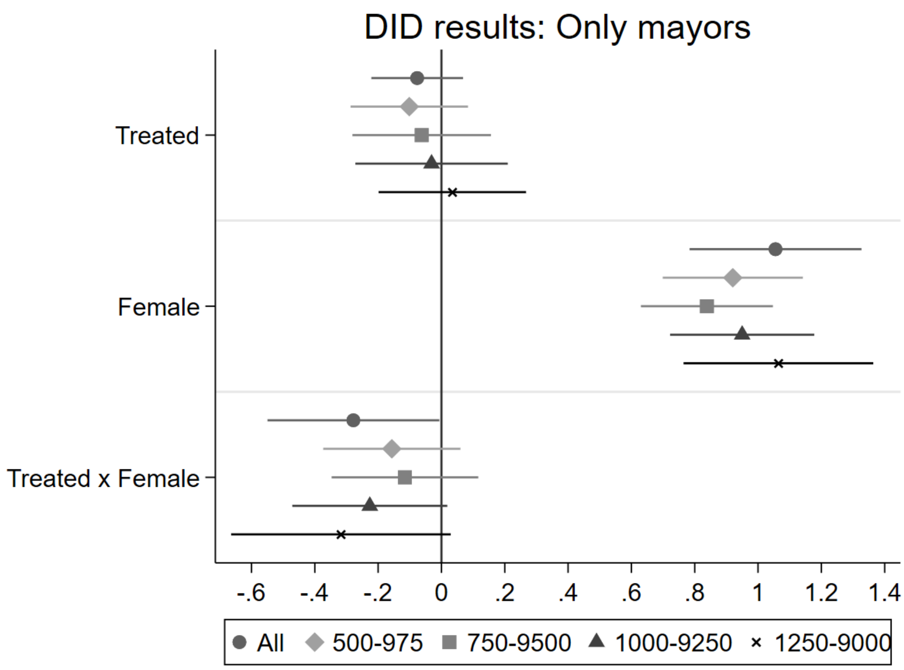

Figure B3. DID results for key variables of interest: only mayors

Table B6. Baseline difference-in-differences regression results: only parties that contested in all elections

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.178***	0.136***	0.131**	0.112**	0.125**
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
Female	0.817***	0.749***	0.732***	0.695***	0.695***
	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Treated×Female	-0.290***	-0.198***	-0.192***	-0.161***	-0.157***
	(0.07)	(0.06)	(0.05)	(0.06)	(0.05)
Mayor	1.318***	1.438***	1.485***	1.482***	1.522***
	(0.07)	(0.08)	(0.09)	(0.09)	(0.10)
Councillor	-0.192***	-0.222***	-0.225***	-0.245***	-0.262***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Between 31-50	-1.093***	-1.127***	-1.106***	-1.112***	-1.115***
	(0.08)	(0.10)	(0.10)	(0.13)	(0.13)
Between 51-65	-2.458***	-2.491***	-2.498***	-2.516***	-2.525***
	(0.06)	(0.08)	(0.09)	(0.11)	(0.13)
Over 65	-3.628***	-3.539***	-3.663***	-3.687***	-3.688***
	(0.15)	(0.22)	(0.25)	(0.28)	(0.31)
Year FE	YES	YES	YES	YES	YES
Muni FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,808	3,453	2,806	2,404	2,070
Num. Parties	12	11	11	11	11
Observations	104,924	83,416	71,275	63,681	56,162
Mean dep. variable	11.58	11.82	11.96	12.07	12.17

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, vice-mayor and less than 31 years old.

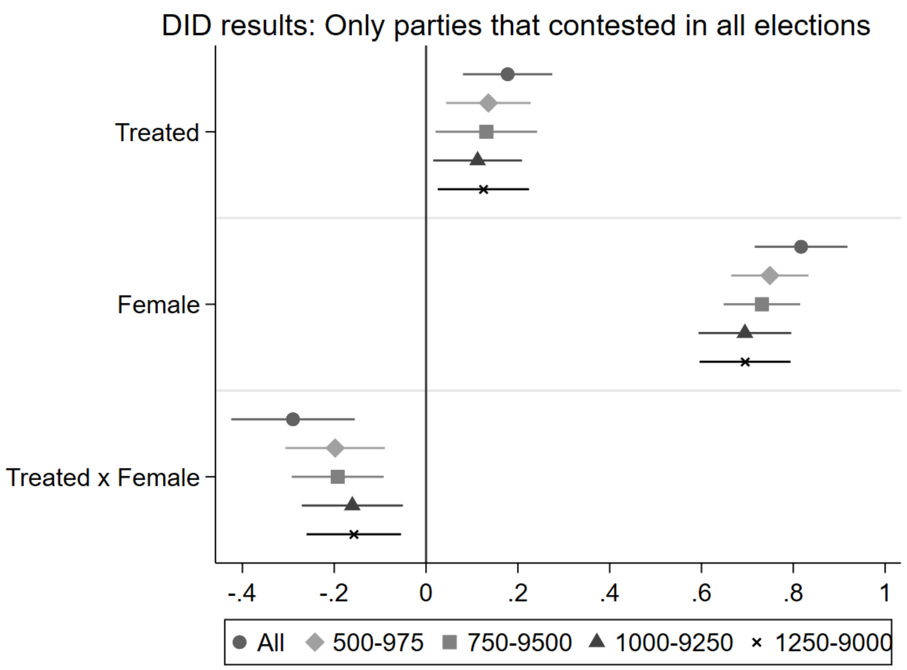

Figure B4. DID results for key variables of interest: only parties that contested in all elections

APPENDIX C. REGRESSION DISCONTINUITY RESULTS

Table C1. Regression Discontinuity (RD) estimates: without covariates

	(1)	(2)	(3)	(4)	(5)
	Year 2003 (placebo)	Year 2003 (placebo)	Year 2007	Year 2011	Year 2015
Panel A: All	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Conventional	-0.103	-0.106	0.768**	0.021	-0.105
	(0.324)	(0.241)	(0.343)	(0.244)	(0.265)
Bias-corrected	-0.160	-0.096	0.908***	-0.007	-0.168
	(0.324)	(0.241)	(0.343)	(0.244)	(0.265)
Robust	-0.160	-0.096	0.908**	-0.007	-0.168
	(0.385)	(0.287)	(0.393)	(0.290)	(0.312)
Covariates included	NO	NO	NO	NO	NO
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	35,779	35,779	32,324	35,779	35,779
Opt. Bandwidth	837.20	1,953.51	1,254.99	1,311.07	976.96
Num. observations	3,457	4,261	2,217	5,025	2,955
(municipalities) left of cutoff	(695)	(834)	(522)	(1,254)	(779)
Num. observations	2,281	3,112	2,062	2,813	1,999
(municipalities) right of cutoff	(332)	(351)	(305)	(464)	(373)
Panel B: Only females					
Conventional	0.297	0.233	0.962**	-0.288	-0.601
	(0.447)	(0.417)	(0.418)	(0.409)	(0.425)
Bias-corrected	0.246	0.257	1.125***	-0.388	-0.781*
	(0.447)	(0.417)	(0.418)	(0.409)	(0.425)
Robust	0.246	0.257	1.125**	-0.388	-0.781
	(0.539)	(0.490)	(0.482)	(0.484)	(0.495)
Covariates included	NO	NO	NO	NO	NO
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	9,236	9,236	9,580	8,964	9,236
Opt. Bandwidth	989.07	1,517.66	1,629.36	859.20	733.90
Num. observations	1,328	899	976	1,050	771
(municipalities) left of cutoff	(758)	(474)	(723)	(592)	(503)
Num. observations	2,915	773	945	765	648
(municipalities) right of cutoff	(336)	(279)	(346)	(327)	(286)
Panel C: Only Males					
Conventional	-0.214	-0.232	0.474	0.133	0.586
	(0.347)	(0.263)	(0.373)	(0.325)	(0.381)
Bias-corrected	-0.268	-0.224	0.607	0.117	0.740*
	(0.347)	(0.263)	(0.373)	(0.325)	(0.381)
Robust	-0.268	-0.224	0.607	0.117	0.740*
	(0.413)	(0.312)	(0.437)	(0.390)	(0.436)
Covariates included	NO	NO	NO	NO	NO
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	26,543	26,543	22,744	18,223	26,543
Opt. Bandwidth	819.85	2,001.65	1,335.45	1,116.91	646.24
Num. observations	2,398	3,117	1,568	2,756	1,071
(municipalities) left of cutoff	(678)	(895)	(512)	(922)	(526)
Num. observations	1,585	2,181	1,345	1,480	805
(municipalities) right of cutoff	(324)	(352)	(295)	(406)	(310)

Note: The table presents RD estimates using a local linear polynomial (p=1) for the point estimates and a local quadratic regression for the bias correction (q=2) under a triangular kernel function. Standard errors are clustered at the municipality level.

Table C2. Regression Discontinuity (RD) estimates: alternative bandwidth

	(1)	(2)	(3)	(4)	(5)
	Year 2003 (placebo)	Year 2003 (placebo)	Year 2007	Year 2011	Year 2015
Panel A: All	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Conventional	0.072	-0.188	0.641*	0.097	-0.025
	(0.282)	(0.242)	(0.330)	(0.215)	(0.221)
Bias-corrected	0.017	-0.212	0.790**	0.074	-0.041
	(0.282)	(0.242)	(0.330)	(0.215)	(0.221)
Robust	0.017	-0.212	0.790**	0.074	-0.041
	(0.333)	(0.290)	(0.386)	(0.257)	(0.262)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	35,779	35,779	32,324	35,779	24,334
Opt. Bandwidth left of cutoff	682.11	2,170.67	1,322.98	1,272.44	1,108.98
Opt. Bandwidth right of cutoff	1,870.89	1,778.27	1,443.50	2,268.42	1,950.54
Num. observations	2,706	4,889	2,326	4,853	3,540
(municipalities) left of cutoff	(536)	(885)	(538)	(1,059)	(907)
Num. observations	4,191	2,896	2,290	4,189	3,203
(municipalities) right of cutoff	(626)	(347)	(300)	(721)	(622)
Panel B: Only females					
Conventional	0.328	0.169	1.023**	-0.154	-0.476
	(0.365)	(0.368)	(0.471)	(0.345)	(0.379)
Bias-corrected	0.324	0.210	1.226***	-0.255	-0.614
	(0.365)	(0.368)	(0.471)	(0.345)	(0.379)
Robust	0.324	0.210	1.226**	-0.255	-0.614
	(0.435)	(0.440)	(0.553)	(0.407)	(0.448)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	9,236	9,236	9,580	8,964	8,680
Opt. Bandwidth left of cutoff	867.92	2,430.21	1,407.93	758.04	607.08
Opt. Bandwidth right of cutoff	2,246.38	1,464.87	1,252.18	2,106.84	1,742.47
Num. observations	1,054	1,704	827	868	621
(municipalities) left of cutoff	(652)	(1,015)	(513)	(507)	(360)
Num. observations	1,441	762	772	1,517	1,206
(municipalities) right of cutoff	(626)	(279)	(259)	(624)	(501)
Panel C: Only Males					
Conventional	-0.085	-0.334	0.281	0.239	0.498
	(0.305)	(0.266)	(0.315)	(0.279)	(0.334)
Bias-corrected	-0.162	-0.373	0.390	0.259	0.613*
	(0.305)	(0.266)	(0.315)	(0.279)	(0.334)
Robust	-0.162	-0.373	0.390	0.259	0.613
	(0.357)	(0.319)	(0.381)	(0.335)	(0.381)
Covariates included	YES	YES	YES	YES	YES
Cutoff threshold	3,000	5,000	5,000	3,000	3,000
Num. observations	26,543	26,543	22,744	18,223	15,654
Opt. Bandwidth left of cutoff	676.07	2,068.87	1,692.39	1,95.22	569.81
Opt. Bandwidth right of cutoff	1,715.38	1,769.09	1,790.16	2,262.57	1,487.23
Num. observations	1,921	3,255	2,158	2,677	940
(municipalities) left of cutoff	(527)	(817)	(648)	(841)	(460)
Num. observations	2,754	2,008	1,726	2,559	1,575
(municipalities) right of cutoff	(592)	(343)	(355)	(705)	(486)

Note: The table presents RD estimates using a local linear polynomial (p=1) for the point estimates and a local quadratic regression for the bias correction (q=2) under a triangular kernel function. Standard errors are clustered at the municipality level

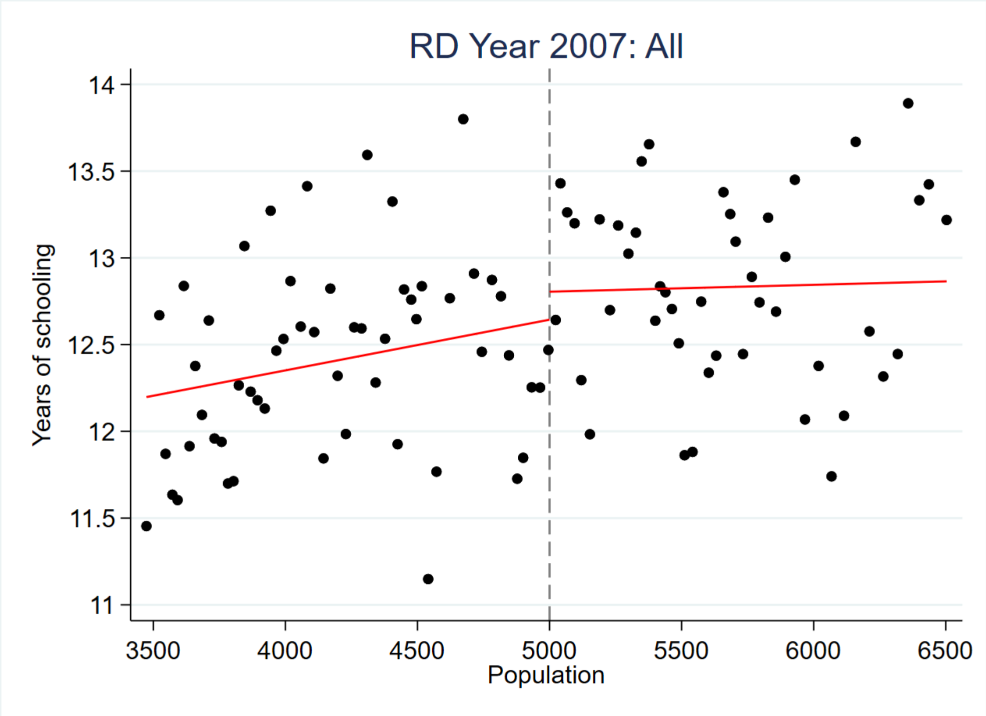

Figure C1. RD Plot: politicians' schooling in 2007

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

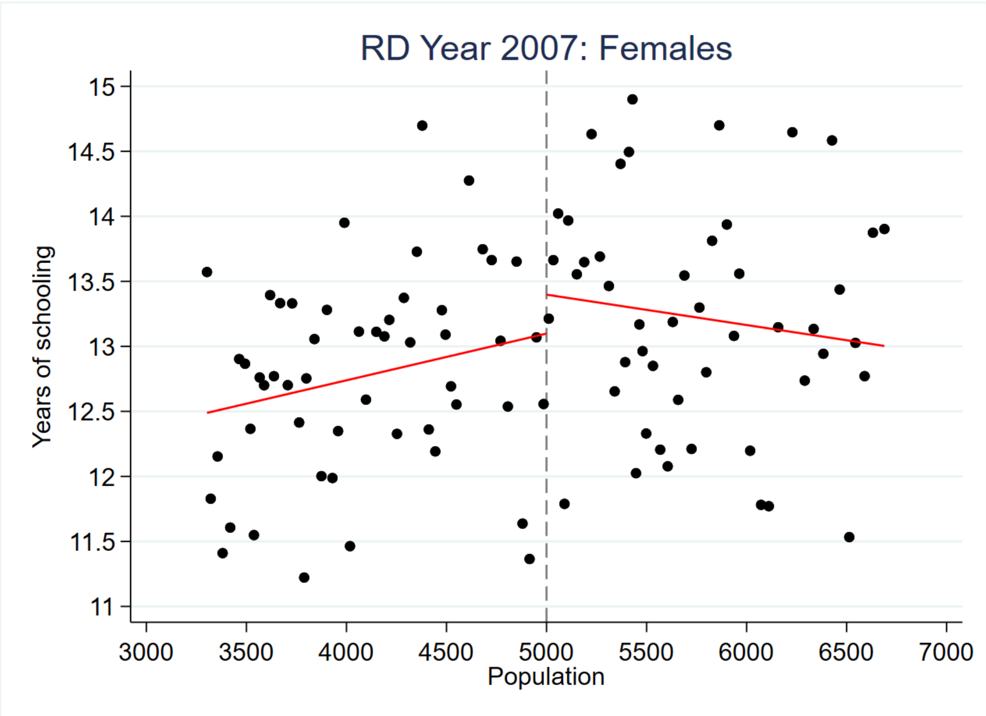

Figure C2. RD Plot: female politicians' schooling in 2007

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

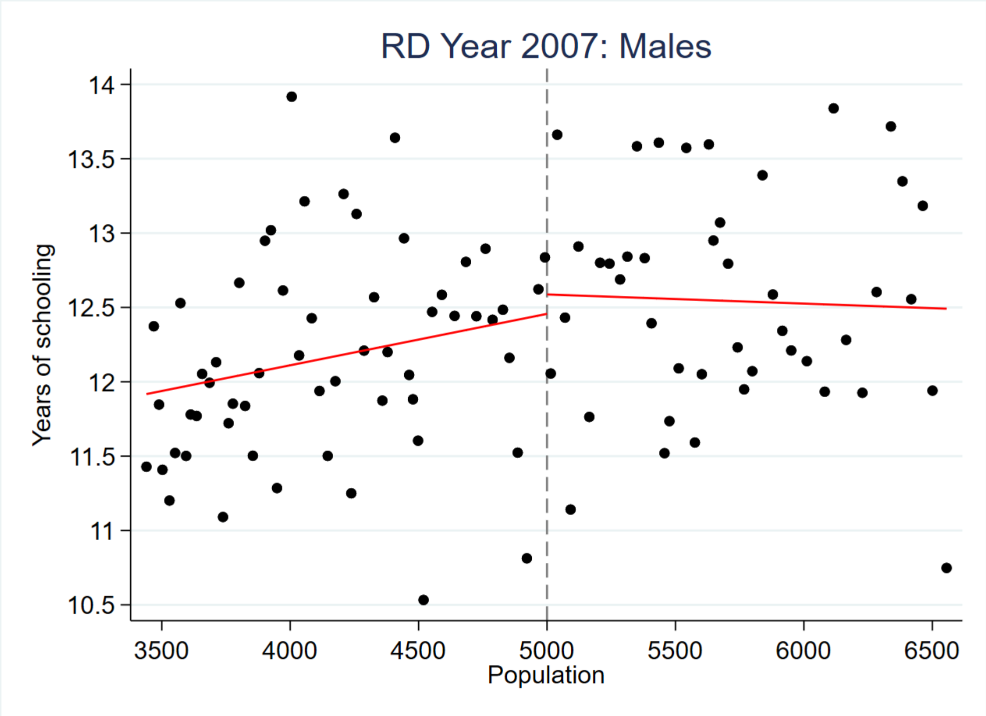

Figure C3. RD Plot: female politicians' schooling in 2007

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

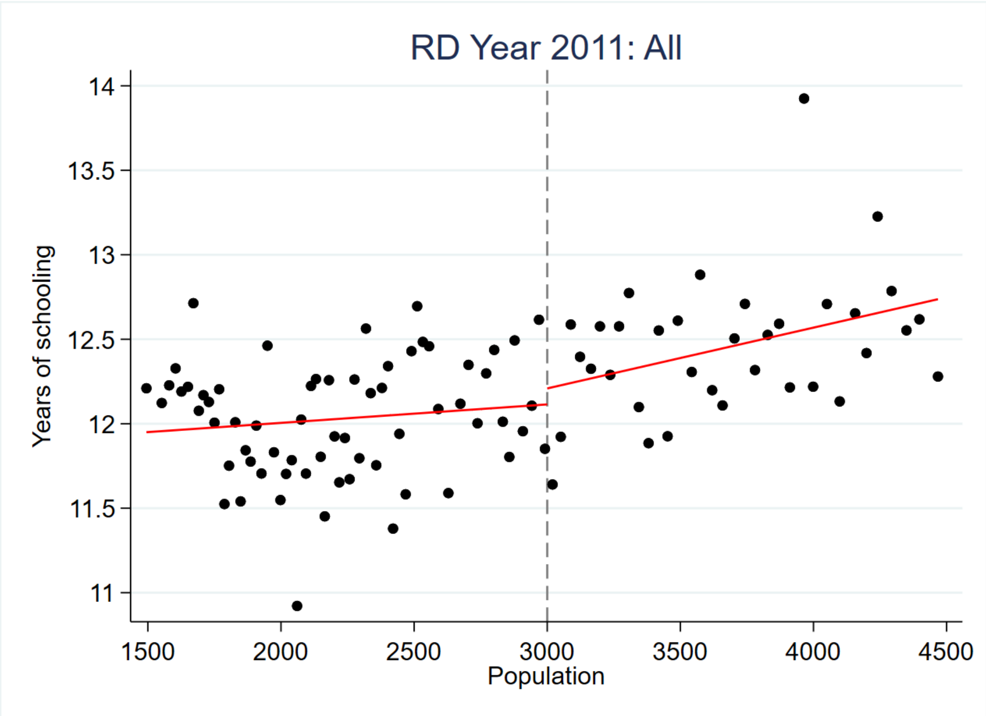

Figure C4. RD Plot: politicians' schooling in 2011

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

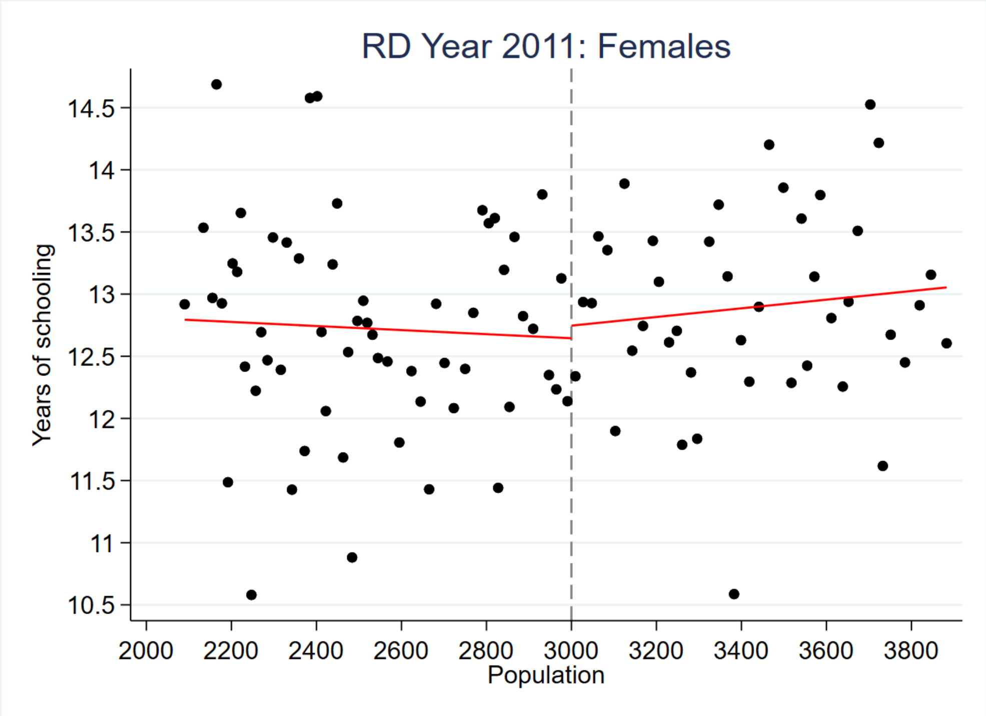

Figure C5. RD Plot: female politicians' schooling in 2011

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

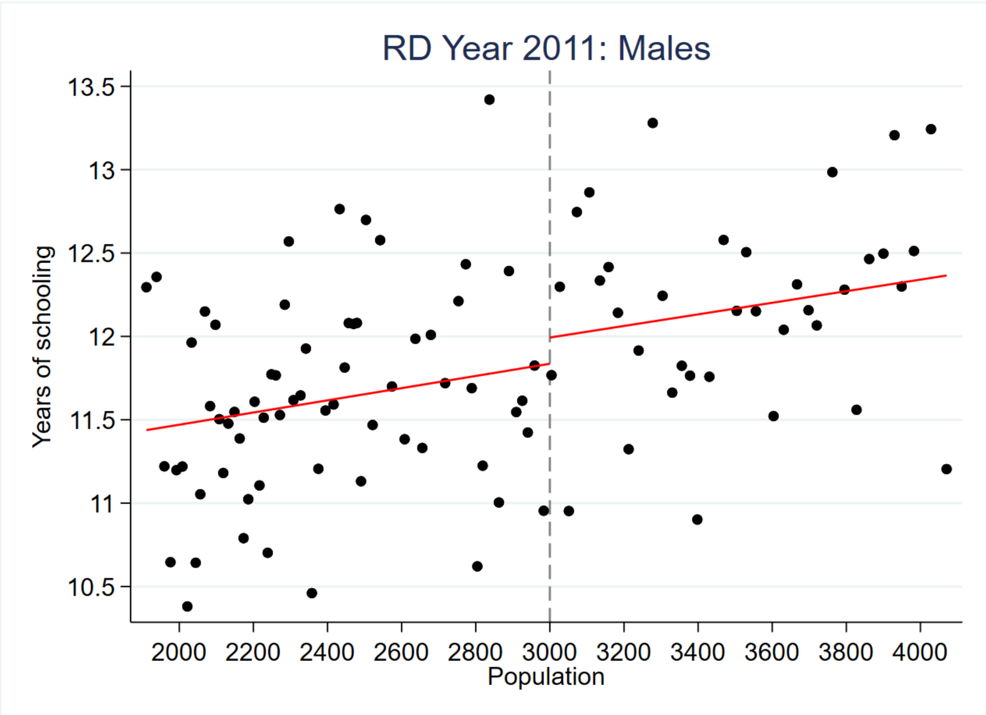

Figure C6. RD Plot: male politicians' schooling in 2011

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

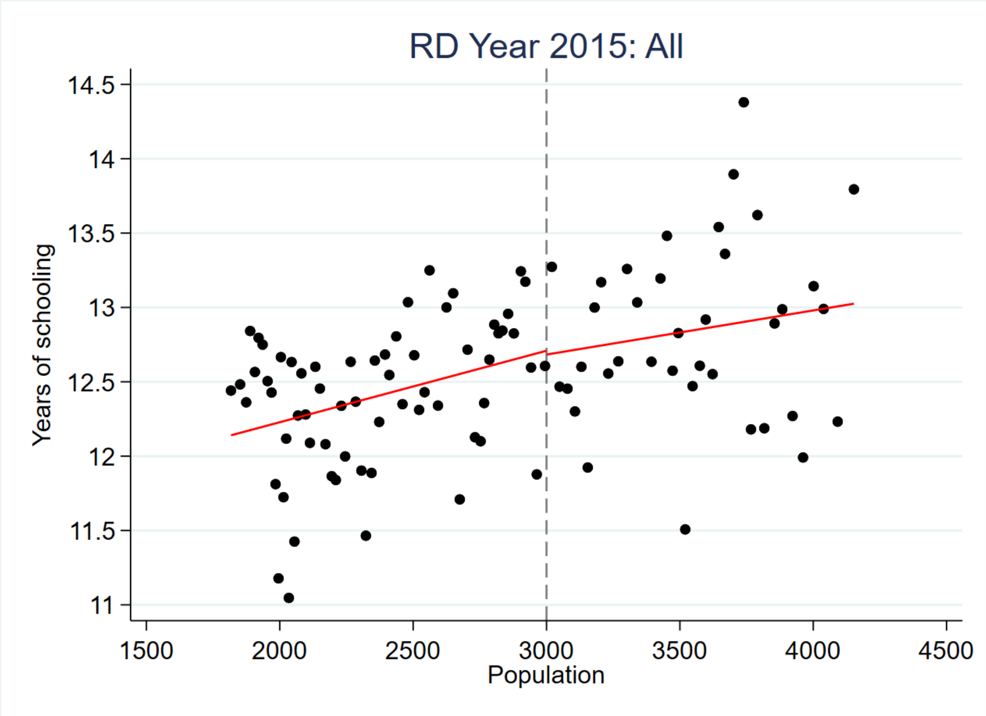

Figure C7. RD Plot: politicians' schooling in 2015

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

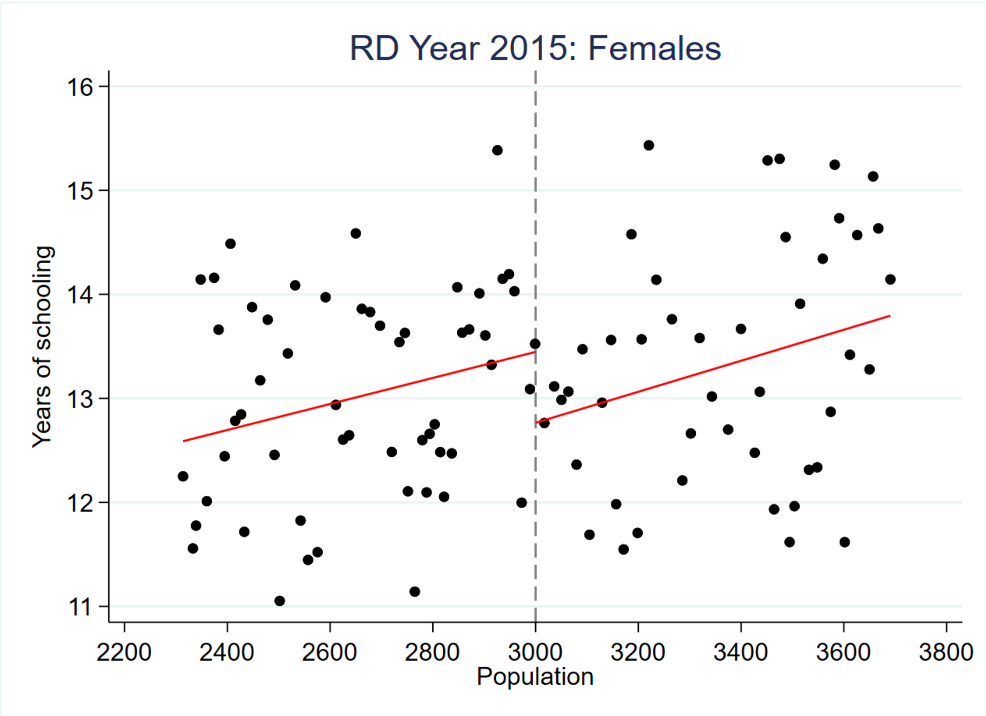

Figure C8. RD Plot: female politicians' schooling in 2015

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

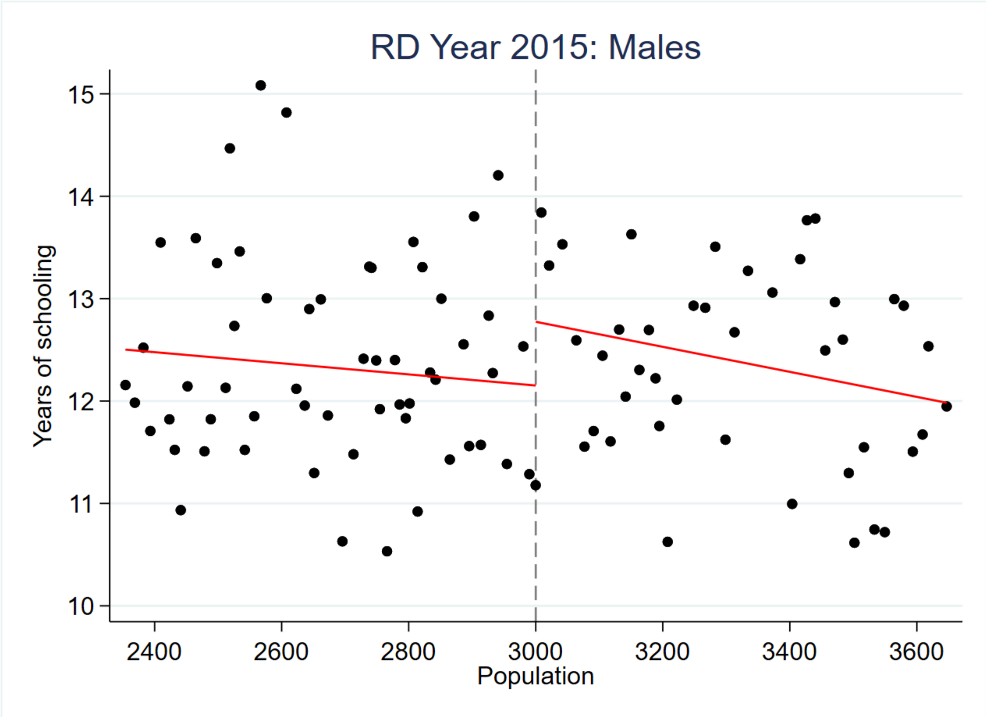

Figure C9. RD Plot: male politicians' schooling in 2015

*The graph presents the sample average within 100-quantile bins from a linear regression that controls for politicians' gender, age and charge considering an optimal symmetric bandwidth to both sides of the threshold

APPENDIX D. DIFFERENCE IN DISCONTINUITIES RESULTS

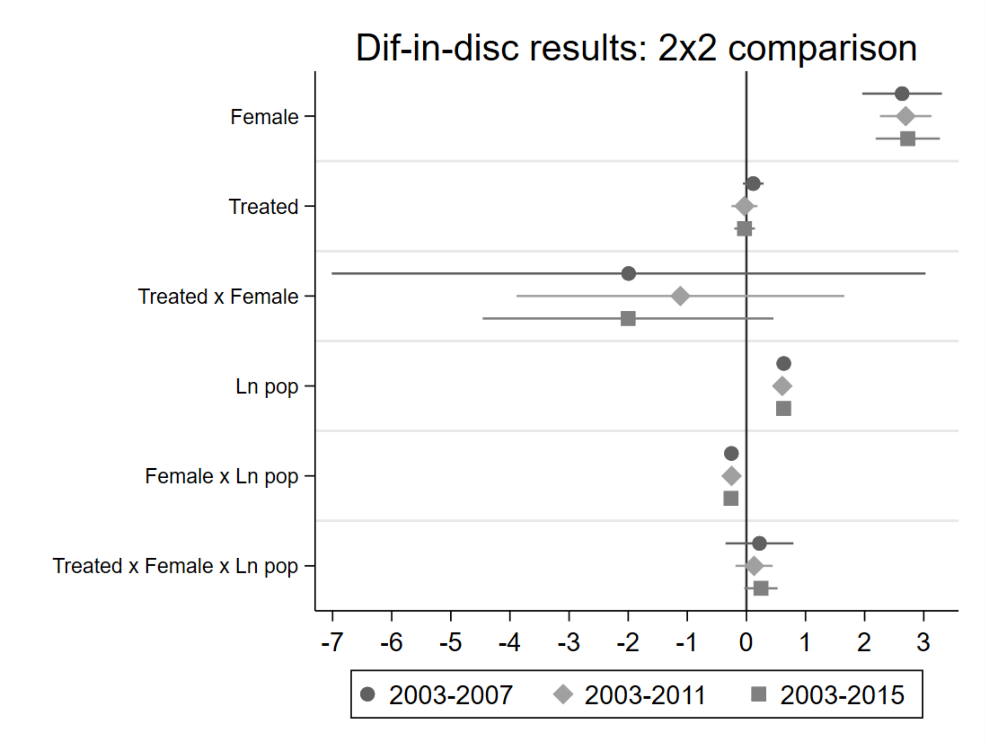

Figure D1. Difference in discontinuity results for key variables of interest (2x2 comparison)

Table D1. Difference-in-discontinuity (linear) regression results: Separate regressions per age interval of the politician

	(1)	(2)	(3)	(4)
	Less than 30	Between 31 and 50	Between 51 and 65	More than 66
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.099	0.069	-0.111	0.096
	(0.13)	(0.06)	(0.14)	(0.21)
Female	3.708***	2.977***	2.829***	-3.480
	(0.39)	(0.26)	(0.36)	(2.63)
Treated×Female	1.401	-2.067	-2.693	2.736
	(1.82)	(1.26)	(2.55)	(10.42)
Ln Pop	0.696***	0.600***	0.616***	0.619***
	(0.08)	(0.03)	(0.08)	(0.14)
Female×Ln Pop	-0.358***	-0.290***	-0.323***	0.642
	(0.05)	(0.03)	(0.05)	(0.39)
Treated×Female×Ln Pop	-0.148	0.245*	0.363	-0.370
	(0.21)	(0.14)	(0.29)	(1.19)
Major	1.084***	1.251***	1.411***	1.075***
	(0.14)	(0.08)	(0.12)	(0.22)
Councillor	-0.636***	-0.221***	-0.031	-0.184
	(0.11)	(0.05)	(0.09)	(0.14)
Between 3,000-5,000 inhabitants	0.218**	0.260***	0.164	0.137
	(0.09)	(0.07)	(0.10)	(0.24)
More than 5,000 inhabitants	0.372**	0.575***	0.563***	0.209
	(0.19)	(0.11)	(0.16)	(0.39)
Year FE	YES	YES	YES	YES
Province FE	YES	YES	YES	YES
Party FE	YES	YES	YES	YES
Num. Municipalities	4,083	4,888	4,662	2,219
Num. Parties	29	35	32	25
Observations	14,515	71,882	28,665	4,550
Mean dep. variable	12.95	12.01	10.66	9.01

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(4) report the estimates considering politicians with less than 30 years old, between 31 and 50, between 51 and 65 and over 66, respectively. In all the regression, municipalities with more than 250 and less than 10,000 inhabitants are considered. The reference categories are males, vice-mayor and less than 3,000 inhabitants.

Table D2. Difference-in-discontinuity (linear) regression results: only mayors

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.107	-0.076	-0.020	0.009	0.054
	(0.09)	(0.08)	(0.10)	(0.11)	(0.12)
Female	2.574***	2.313**	3.129***	2.181*	4.349***
	(0.61)	(0.92)	(1.11)	(1.13)	(1.53)
Treated×Female	-5.386	-4.531	-5.498	-4.135	-6.811
	(5.12)	(5.11)	(5.30)	(5.44)	(6.38)
Ln Pop	0.968***	0.992***	1.047***	1.017***	0.814***
	(0.07)	(0.07)	(0.11)	(0.14)	(0.15)
Female×Ln Pop	-0.202**	-0.166	-0.271*	-0.150	-0.421**
	(0.09)	(0.13)	(0.15)	(0.15)	(0.19)
Treated×Female×Ln Pop	0.603	0.496	0.617	0.448	0.778
	(0.60)	(0.59)	(0.61)	(0.63)	(0.75)
Between 31-50	-1.276***	-1.353***	-1.153***	-1.090***	-1.022***
	(0.10)	(0.12)	(0.16)	(0.17)	(0.19)
Between 51-65	-2.616***	-2.722***	-2.564***	-2.489***	-2.354***
	(0.12)	(0.11)	(0.14)	(0.15)	(0.19)
Over 65	-3.924***	-3.747***	-3.817***	-3.690***	-3.686***
	(0.14)	(0.20)	(0.22)	(0.20)	(0.24)
Between 3,000-5,000 inhabitants	-0.128	-0.160*	-0.223*	-0.225*	-0.129
	(0.09)	(0.10)	(0.11)	(0.13)	(0.12)
More than 5,000 inhabitants	0.002	-0.044	-0.164	-0.158	0.052
	(0.20)	(0.18)	(0.18)	(0.23)	(0.25)
Year FE	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,862	3,559	2,899	2,482	2,146082
Num. Parties	30	29	28	26	26
Observations	15,007	11,383	9,273	7,928820	6,876
Mean dep. variable	12.41	12.79	13.01	13.16	13.31

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, less than 30 years old and less than 3,000 inhabitants.

Table D3. Difference-in-discontinuity (linear) regression results: Only parties that contested in all elections

	(1)	(2)	(3)	(4)	(5)
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	0.030	0.080	0.080	0.093	0.132**
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
Female	2.709***	2.278***	2.511***	2.094***	2.166***
	(0.28)	(0.21)	(0.27)	(0.38)	(0.70)
Treated×Female	-1.710	-0.964	-0.849	-0.288	-0.460
	(1.13)	(1.11)	(1.10)	(1.05)	(1.01)
Ln Pop	0.613***	0.720***	0.790***	0.811***	0.755***
	(0.03)	(0.04)	(0.05)	(0.08)	(0.09)
Female×Ln Pop	-0.257***	-0.199***	-0.229***	-0.178***	-0.185**
	(0.03)	(0.03)	(0.04)	(0.05)	(0.09)
Treated×Female×Ln Pop	0.203	0.110	0.098	0.030	0.050
	(0.13)	(0.13)	(0.13)	(0.12)	(0.12)
Major	1.267***	1.396***	1.451***	1.469***	1.508***
	(0.07)	(0.08)	(0.09)	(0.09)	(0.10)
Councillor	-0.230***	-0.247***	-0.243***	-0.250***	-0.254***
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Between 31-50	-1.092***	-1.126***	-1.108***	-1.120***	-1.124***
	(0.07)	(0.08)	(0.08)	(0.10)	(0.09)
Between 51-65	-2.475***	-2.504***	-2.534***	-2.545***	-2.546***
	(0.04)	(0.06)	(0.06)	(0.07)	(0.08)
Over 65	-3.680***	-3.575***	-3.683***	-3.695***	-3.701***
	(0.13)	(0.20)	(0.18)	(0.20)	(0.22)
Between 3,000-5,000 inhabitants	0.249***	0.125**	0.077	0.042	0.056
	(0.05)	(0.06)	(0.07)	(0.08)	(0.08)
More than 5,000 inhabitants	0.550***	0.352***	0.268**	0.209*	0.231*
	(0.08)	(0.09)	(0.11)	(0.12)	(0.13)
Year FE	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES
Party FE	YES	YES	YES	YES	YES
Num. Municipalities	4,915	3,586	2,919	2,498	2,163
Num. Parties	15	14	14	14	14
Observations	113,585	91,526	78,440	70,094	62,170
Mean dep. variable	11.62	11.85	12.00	12.09	12.19

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Column (1) reports the estimates considering municipalities with more than 250 and less than 10,000 inhabitants. Columns (2)-(5) consider municipalities with 500–9750, 750–9500, 1000–9250, and 1250–9000 inhabitants, respectively. The reference categories are males, vice-mayor, less than 30 years old and less than 3,000 inhabitants.

APPENDIX E. ADDITIONAL ROBUSTNESS CHECKS

Table E1. Baseline difference-in-differences regression results (2x2 comparison) using average optimal bandwidth

	(1)	(2)	(3)
	2003 vs 2007	2003 vs 2011	2003 vs 2015
	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.028	0.165	-0.156
	(0.15)	(0.11)	(0.21)
Female	0.557***	0.772***	0.657***
	(0.11)	(0.15)	(0.11)
Treated×Female	-0.090	-0.235	0.064
	(0.20)	(0.25)	(0.18)
Mayor	1.354***	1.607***	1.574***
	(0.12)	(0.16)	(0.13)
Councillor	-0.401***	-0.182**	-0.242***
	(0.06)	(0.07)	(0.08)
Between 31-50	-1.149***	-1.361***	-1.340***
	(0.16)	(0.15)	(0.10)
Between 51-65	-2.676***	-2.772***	-2.811***
	(0.14)	(0.22)	(0.12)
Over 66	-3.411***	-4.234***	-4.007***
	(0.35)	(0.52)	(0.40)
Cutoff threshold	5,000	3,000	3,000
Average optimal bandwidth	1731.99	1145.17	991.77
Year FE	YES	YES	YES
Muni FE	YES	YES	YES
Party FE	YES	YES	YES
Num. Municipalities	714	1,055	913
Num. Parties	19	24	28
Observations	12,439	14,989	12,169
Mean dep. variable	12.36	11.88	12.01

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(3) report the estimates considering the years 2003-2007, 2003-2011, and 2003-2015, respectively. The regressions consider only the municipalities that lie within the optimal bandwidth in each case. Such bandwidth comes as the average of the optimal bandwidths from RD regressions per year in Table 6 in the text. The reference categories are males, vice-mayor, and less than 31 years old.

Table E2. Baseline difference-in-differences regression results (2x2 comparison) using average optimal bandwidth interacting treatment with pre-treatment share of female councilors

	(1)	(2)	(3)
	2003 vs 2007	2003 vs 2011	2003 vs 2015
	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.045	0.181	-0.163
	(0.15)	(0.11)	(0.20)
Female	0.541***	0.776***	0.652***
	(0.10)	(0.14)	(0.12)
Treated×Female×Share fem 03	-0.157	-0.887	0.256
	(0.59)	(0.83)	(0.59)
Mayor	1.353***	1.595***	1.593***
	(0.13)	(0.16)	(0.13)
Councilor	-0.397***	-0.192**	-0.231***
	(0.06)	(0.07)	(0.08)
Between 31-50	-1.146***	-1.374***	-1.361***
	(0.17)	(0.14)	(0.10)
Between 51-65	-2.711***	-2.745***	-2.845***
	(0.14)	(0.23)	(0.14)
Over 66	-3.456***	-4.241***	-4.078***
	(0.39)	(0.52)	(0.42)
Cutoff threshold	5,000	3,000	3,000
Average optimal bandwidth	1731.99	1145.17	991.77
Year FE	YES	YES	YES
Muni FE	YES	YES	YES
Party FE	YES	YES	YES
Num. Municipalities	674	1,008	876
Num. Parties	19	23	28
Observations	12,020	14,604	11,858
Mean dep. variable	12.33	11.87	11.99

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(3) report the estimates considering the years 2003-2007, 2003-2011, and 2003-2015, respectively. The regressions consider only the municipalities that lie within the optimal bandwidth in each case. Such bandwidth comes as the average of the optimal bandwidths from RD regressions per year in Table 6 in the text. The reference categories are males, vice-mayor, and less than 31 years old.

Table E3. Baseline difference-in-differences regression results (2x2 comparison) using average optimal bandwidth interacting treatment with pre-treatment average schooling of municipality councilors

	(1)	(2)	(3)
	2003 vs 2007	2003 vs 2011	2003 vs 2015
	Coef. (SE)	Coef. (SE)	Coef. (SE)
Treated	-0.006	0.209*	-0.088
	(0.15)	(0.11)	(0.20)
Female	0.566***	0.793***	0.684***
	(0.11)	(0.15)	(0.12)
Treated×Female×Av. Schooling 03	-0.013	-0.029	-0.009
	(0.02)	(0.02)	(0.02)
Mayor	1.352***	1.595***	1.591***
	(0.13)	(0.16)	(0.13)
Councilor	-0.397***	-0.193**	-0.232***
	(0.06)	(0.07)	(0.08)
Between 31-50	-1.145***	-1.372***	-1.358***
	(0.17)	(0.14)	(0.10)
Between 51-65	-2.712***	-2.746***	-2.844***
	(0.14)	(0.23)	(0.13)
Over 66	-3.459***	-4.242***	-4.077***
	(0.39)	(0.52)	(0.42)
Cutoff threshold	5,000	3,000	3,000
Average optimal bandwidth	1731.99	1145.17	991.77
Year FE	YES	YES	YES
Muni FE	YES	YES	YES
Party FE	YES	YES	YES
Num. Municipalities	674	1,008	876
Num. Parties	19	23	28
Observations	12,020	14,604	11,858
Mean dep. variable	12.33	11.87	11.99

Note: Standard errors are two-way clustered at the municipality and party level. *** p<0.01, ** p<0.05, * p<0.1. Columns (1)-(3) report the estimates considering the years 2003-2007, 2003-2011, and 2003-2015, respectively. The regressions consider only the municipalities that lie within the optimal bandwidth in each case. Such bandwidth comes as the average of the optimal bandwidths from RD regressions per year in Table 6 in the text. The reference categories are males, vice-mayor, and less than 31 years old.

Table E4. Regression Discontinuity (RD) estimates (first-differences) at the municipality level

	(1)	(2)	(3)
	Year 2007-Year 2003	Year 2011-Year 2003	Year 2015-Year 2003
Panel A: All	Coef. (SE)	Coef. (SE)	Coef. (SE)
Conventional	0.338	-0.367	-0.719*
	(0.304)	(0.357)	(0.373)
Bias-corrected	0.425	-0.442	-0.825**
	(0.304)	(0.357)	(0.373)
Robust	0.425	-0.442	-0.825*
	(0.359)	(0.421)	(0.429)
Cutoff threshold	5,000	3,000	3,000
Num. observations	4,079	3,880	3,531
Opt. Bandwidth	1,916.06	1,109.08	1,220.51
Num. observations left of cutoff	401	493	504
Num. observations right of cutoff	252	287	297
Panel B: Only females			
Conventional	-0.047	-0.803	-0.399
	(0.465)	(0.606)	(0.562)
Bias-corrected	0.009	-0.686	-0.397
	(0.465)	(0.606)	(0.562)
Robust	0.009	-0.686	-0.397
	(0.568)	(0.728)	(0.671)
Cutoff threshold	5,000	3,000	3,000
Num. observations	3,049	2,736	2,473
Opt. Bandwidth	1,889.98	887.94	1,162.54
Num. observations left of cutoff	332	309	383
Num. observations right of cutoff	227	204	233
Panel C: Only Males			
Conventional	-0.047	-0.341	-0.788
	(0.465)	(0.405)	(0.487)
Bias-corrected	0.009	-0.425	-0.849*
	(0.465)	(0.405)	(0.487)
Robust	0.009	-0.425	-0.849
	(0.568	(0.473)	(0.580)
Cutoff threshold	5,000	3,000	3,000
Num. observations	3,049	3,774	3,410
Opt. Bandwidth	1,889.98	1,067.52	1,005.77
Num. observations left of cutoff	332	465	385
Num. observations right of cutoff	227	278	251

Note: The table presents RD estimates using a local linear polynomial (p=1) for the point estimates and a local quadratic regression for the bias correction (q=2) under a triangular kernel function. Standard errors are clustered at the municipality level.