DISCUSSION PAPER SERIES

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ABSTRACT

The Long-Run Earnings Effects of Winning a Mayoral Election*

We estimate the effect of winning a mayoral election on long-run licit earnings, which plays a key role in the selection of local political leaders. We use Italian administrative social security data from 1995 to 2017 and a sharp regression discontinuity design based on close elections. Over a 15-year horizon, the average present discounted value of winning an election is equal to 35,000€, or 85 percent of the annual labor and social security earnings for the average candidate in our sample, a modest effect driven by the compensations for political service and concentrated during the first five years after the election. Net of compensations for service, this effect is negative during the first ten years after the election, and almost fades away afterwards. Differences in the political careers of winners and runners-up and a two-term limit rule on mayors' office contribute to explain our results.

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1. Introduction

In deciding whether to run for office, potential politicians consider the expected returns of alternative choices. Pecuniary returns include direct remunerations, earnings opportunities during and after leaving office, as well as illicit rents. Non-pecuniary rewards derive instead from service motivation, social status, and power. Pecuniary and non-pecuniary returns affect both the number of candidates and their quality, because high skilled individuals require adequate payoffs to counterbalance their higher opportunity costs.[1] Higher returns also make re-election more appealing and provide incentives to act as voters wish (Besley, 2004, Di Tella and Fisman, 2004). Whether these returns derive from licit or illicit activities is also relevant, as the decision of honest citizens to enter politics will not be affected by illegitimate rents.

Understanding the returns to leading political positions, which include presidents, governors and mayors, is particularly relevant because the winning candidate is endowed with greater decisional power than an ordinary member of either the national parliament or the local council, and the candidate's behavior while in office exerts a first-order impact on the political performance of the national or local legislature. While political leaders have greater salience than other politicians, which may increase their returns from office, they also face higher responsibilities and more demanding tasks. Even in small towns, mayors face enormous expectations and pressure. For instance, with reference to Italy, in an interview to the Huffington Post (May 13, 2016), the former mayor of Venice, Massimo Cacciari, declared that "To be mayor today you have to be crazy, an endless martyrdom".

To the best of our knowledge, however, the existing literature is silent about the returns to salient political positions, both in national and in local elections, and our paper intends to fill this gap. Using Italian administrative electoral and social security data and a sharp regression discontinuity design (RDD), we contribute to the literature on the pecuniary returns to political office by providing the first empirical evidence on the long-term effect on earnings of being elected as mayor, the leading political position in municipal councils.

Although several studies – including Eggers and Hainmueller (2009) for English MPs, Querubin and Snyder (2013) for members of the US Congress between 1850 and 1880, Fisman et al. (2014),

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[1] Several papers find that higher remuneration increases the quality of candidates (Ferraz and Finan, 2009; Kotakorpi and Poutvaara 2011; Gagliarducci and Nannicini, 2013).

for Indian politicians, Kotakorpi et al. (2017) for members of the Finnish parliament and Berg (2020a) for Swedish MPs[2] – estimate large monetary returns to obtaining a seat in national parliaments, there is no evidence to date on the returns accruing to prime ministers or presidents.

Similarly, the small literature on the private pecuniary returns to local politics looks at politicians in general (including ordinary council members) rather than at mayors, with mixed results (see Dal Bó and Finan, 2018, for a review). While Berg (2020b) and Kotakorpi et al. (2017) estimate small or null returns for members of local councils that were elected with a small margin compared to candidates who barely lost, Cingano and Pinotti (2013) show that the earnings of private sector employees holding a local political office grow faster in the ten years following election than the earnings of non-elected employees. Akicgit et al. (2022) also find substantial positive returns from local political service in terms of weekly wages when comparing elected and non-elected workers employed in the same firm.

In the last two papers, more than 90% of the pool of politicians-employees are ordinary members of local councils rather than mayors. Since political tasks and responsibilities absorb more time from mayors than from council members, the latter have ampler opportunities to keep on working while in office than the former, without facing the same degree of career interruptions, skills depreciation, or weakened labor market ties. For council members, political rents are likely to add to an almost intact stream of market incomes, contributing to explain the positive returns uncovered in the literature.

Compared to this literature, our research investigates the pecuniary returns from service accruing to mayors. Focusing on local political leaders, as we do in this paper, is novel and relevant for at least four reasons. First, local administrators are responsible both for the municipal budget and for several important activities, including the management of public utilities such as local roads, garbage collection, transportation and nursery schools, which have a great impact on citizens' daily lives. Second, they often act in closer contact with local stakeholders than national politicians, and may therefore have a more direct impact on citizens' welfare. Third, local politics may serve as a stepping-stone into political careers at higher levels of government. Finally, by focusing on Italian

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[2] Gagliarducci, Nannicini and Naticchioni (2010) show that most members of the Italian parliament still earn a considerable amount of money by working in the private sector after the election. There is also evidence of a comparative advantage for high-ability citizens in terms of outside income, which might explain why they decide to enter politics.

mayors we also shed light on whether poor economic returns may be a reason for the shortage of candidates that has been recently pointed out by many Italian commenters,[3] including the president of the National Association of Italian Municipalities.[4] This shortage does not seem to be consistent with the gains by local politicians pointed out in the literature discussed above, and may be the reason why the Italian government in 2022 substantially increased the compensation of mayors.

In our analysis, we combine Italian individual-level information on annual earnings and sector of employment from the administrative archives of the Italian Social Security Institute (INPS) with information on elected and non-elected candidates from detailed electoral data on the universe of municipal elections. These data cover the population of mayor candidates who ran for office between 1993 and 2017 and earnings for the years from 1995 to 2017.

Although we use the most comprehensive available data on earnings in Italy, we do not have information on earnings from capital assets, illicit sources, or accruing to family members or relatives. An important part of the returns to politics might be illegitimate, or take place either as increased earnings for close family members (see Gagliarducci and Manacorda, 2020) or as benefits deriving from the manipulation of local fiscal policy (see Giommoni, 2019).[5] Nevertheless, personal licit returns remain an important parameter to inform the decision of honest and law-abiding candidates, who may be less interested in joining politics to earn returns in the illicit market or by undertaking nepotistic actions in favor of their relatives.

Following most of the empirical literature, our identification strategy exploits close elections and relies on a sharp regression discontinuity design (RDD) to estimate the earnings effects associated

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[3] See also https://www.repubblica.it/politica/2021/06/12/news/inchiesta_sindaci_caccia_ai_candidati-305683761 and https://www.socialbg.it/ma-quanto-e-difficile-trovare-un-sindaco-per-i-piccoli-comuni/. The low compensation of mayors is not only specific to Italy and has been mentioned also in countries such as New Zealand and the UK. See for instance https://thespinoff.co.nz/local-elections-2022/10-08-2022/not-enough-people-are-standing-for-local-elections and https://commonslibrary.parliament.uk/uncontested-elections-where-and-why-do-they-take-place/.

[4] Giuseppe Sala, the current mayor of Milan, has recently argued that the compensation received by mayors for their service is too low. https://www.telesettelaghi.it/2021/05/26/sala-difficile-trovare-candidati-perche-sindaci-pagati-poco/. The difficulty to find candidates is at the basis of a 2020 law proposal that increases the maximum number of mandates from two to three for municipalities with less than 5,000 inhabitants http://documenti.camera.it/leg18/pdl/xhtml/leg.18.pdl.camera.2600.18PDL0110180.html. Since 2014 mayors of municipalities with less than 3,000 inhabitants are allowed to re-run for a third term (Law April 2014 no.56).

[5] The 2021 Corruption Perceptions Index by Transparency International ranks Italy 42nd out of 180 countries, much higher than in Finland and Sweden, which rank 1st and 4th, respectively. Also, many transactions in Italy occur in informal markets that escape taxation. According to the Italian National Statistical Institute, ISTAT, in 2020 about 11% of the national GDP was either under-reported or produced by undeclared work (see https://www.istat.it/it/archivio/275914).

with winning an election as mayor. By comparing mayoral candidates who won or lost by small margins, we uncover credible causal effects of electoral wins on earnings gained during the political mandate and after leaving office. As in other countries, mayors in Italy can keep their job during their mandate and cumulate earnings with office-related compensations, which allows us to investigate the effects of winning an election on different sources of earnings and whether returns to political power occur during the mandate or after leaving office. An advantage of the Italian local electoral system is that the election of the mayor is based on a single winner single- or dual-ballot system – depending on population size. Hence, electoral closeness is straightforwardly observed without requiring the application of bootstrap approaches, as it happens in the case of proportional open list systems.[6]

Considering total earnings, which include labor market earnings, pensions, social security payments and compensations from holding office, we find that winning an election initially increases earnings by 12,000€ on average, a 30 percent premium in terms of annual average total earnings. This premium, however, vanishes 5 years after the first election and becomes negative after 10 years. Overall, the present discounted value of the average gains accumulated over 15 years is close to 35,000€, or roughly 85 percent of average annual labor and social security earnings, a modest effect over such a long horizon.

When we then break down total earnings into office-related compensations and labor and social security earnings, we find that winning an election increases compensations by roughly 16,000€ per year, but only during the first electoral term. In later terms, consistent with the presence of a two-term limit, many initial winners leave politics while some runners-up win elections. Being elected does not help instead labor and social security earnings. In this case, the gap between mayors and runners-up is close to -3,000€ per year, or 15 percent of runners-ups earnings.

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[6] While in single winner elections electoral closeness is measured by the difference in vote shares between winners and losers, the vote share dividing winners from losers is not observed in proportional elections with multi-member districts, where seats are allocated to parties based upon their total vote shares and candidates compete within parties. In these elections, electoral success depends both on a candidate's votes and on the total votes received by the party. Kotakorpi et al., 2017, introduce a heuristic-based bootstrap method to measure electoral closeness in these cases, which resamples votes from the observed vote share distribution across candidates while keeping the electoral rules fixed. This method generates variation in the vote share in each election while maintaining the expected value of the vote shares across elections equal to the observed one, and can be interpreted as a probabilistic voting model, where each candidate has a set of supporters who only turn out with some probability. Close winners and losers are then identified from the probability of getting elected in a series of simulated elections. The use of this probability for an RDD requires a further normalization, so that all losers are below and all winners are above a given threshold.

Although smaller, this gap remains negative and significant even after 10 to 14 years from the election.

The negative impact on labor and social security earnings during the first electoral term (0 to 4 years since election) is larger for private than for public sector workers, most likely because the latter find it easier to take time off from work and dedicate it to politics without sacrificing earnings. It is also more persistent for the self-employed than for employees, as the latter may return to their initial jobs with fewer penalties to earnings, after completing their mandate, than the former.

We also find that, while the negative impact on earnings during the first electoral terms (0 to 4 and 5 to 9 years) is comparable between the more economically developed North and the less developed South, the negative effect detected 10 to 14 years after election is much larger and only statistically significant in the South. Within Southern Italy, this effect is only observed in local labor markets characterized by high unemployment, where it is difficult to return to work after completing the mandate. We find no heterogeneity, instead, with respect to the local share of illegal employment estimated by the Italian National Statistical Office (ISTAT, 2005), suggesting that engagement in the illegal labor market is not a main driver of the negative persistence.

Although office-related compensations grow with the size of the municipality, our results show that the positive effect of winning a mayoral election on total earnings during the first electoral term is larger in small municipalities. This happens because the bigger compensations received by mayors in larger municipalities are more than compensated by the larger negative effects of holding office on labor and social security earnings. These negative effects are also more persistent in larger municipalities, probably because the higher effort spent by mayors while in office may harm their return to work after completing their mandate.

2. Data

a. Data sources

Our analysis relies on three data sources. First, we use electoral data from the administrative archives of the Italian Ministry of Interior. The data cover the universe of municipal elections that took place in Italy between 1993 and 2017 and provide information on candidates' name and surname, demographics, and party affiliation, as well as on electoral outcomes. The election of the

mayor is based on a single winner plurality vote system. It relies on single ballot rule for municipalities with a population below 15,000 inhabitants and on a dual ballot system for those with a population above this threshold, where the second ballot takes place only if no candidate obtains the absolute majority in the first one.[7] For each election, we construct the margin of victory of the winner vs. the runner-up as the difference in the vote share of the former vs. the latter. In case of a ballot, we use the vote shares at the ballot. We also reconstruct the history of all political appointments held by mayor candidates in our data using the census of local administrators (municipal, provincial, and regional councils) provided by the Ministry of Interior (up to 2017) and data from Fondazione de Benedetti on the members of the Italian parliament (up to 2007).

Second, we merge the electoral data with the administrative archives from the Italian Social Security Institute – INPS. These archives provide individual-level information on annual labor earnings, pension earnings and social-security payments for maternity leave, unemployment benefits and short-term work between 1995 and 2017.[8] We measure all monetary variables in Euros (€) and in real terms, using 2010 prices.

The information on earnings is based on individual pension files, that are used by INPS to compute both pension eligibility and pension benefits at the end of a worker's career. They contain annual information on gross earned labor income from any source that generates social security contribution payments. The individual pension files also include information on payments made to workers during maternity leave, unemployment or for short-time work episodes (*Cassa Integrazione Guadagni*). We include them in our measure of labor and social security earnings. Finally, we reconstruct pension earnings using dedicated archives, that cover the universe of pensions paid by INPS from 1995 to 2017. For all these sources, the coverage of the data is close to universal, and only very few occupations (such as journalists) are excluded because their pensions were not managed – until very recently – by INPS.

By looking at the different funds to which individuals pay social security contributions, we also recover their prevalent sector of employment in each year. We adopt the following classification: *i)* public employees; *ii)* private employees; *iii)* self-employed professionals, who pay contributions

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[7] A detailed presentation of the Italian electoral system for municipal election is reported in the Appendix.
[8] We start from 1995 because data on pensions are only available from 1995 onwards. Moreover, there may be gaps in the records on social security contributions paid by public employees before 1994, when INPDAP – a public institution dedicated at the management of public employees' pensions – was established.

to dedicated occupational funds (e.g., lawyers, engineers, accountants, architects, psychologists, notaries, physicians, pharmacists,…); *iv)* other self-employed, who pay contributions directly to INPS and include mostly craftsmen and retailers; *v)* fixed-term collaborators, who pay contributions to a separate fund within INPS; *vi)* retirees, who do not pay contributions and receive a pension as their main source of earnings; *vii)* individuals for whom we see no social security payment and who receive no pension.

In several countries, including Italy, both national and local politicians holding office can keep working in the private sector after election. Their current income includes both the compensation associated with office and the income earned in the labor market. The former component is generally not available in the INPS database, because it is exempt from social security contributions. To recover this information, we turn to a third dataset, which includes the rules governing both eligibility and the compensations received by politicians at the municipal, provincial, and regional levels of government. We do not reconstruct data on the compensations of Members of Parliament, because our data for this group end in 2007, and only 70 candidates in our records become MP within this time span. To compile the third dataset, we hired a lawyer expert in public law who consulted all the relevant national and local sources of legislation. We report detailed information on the institutional rules governing compensations in the Appendix.

We use these data to impute the compensation earned by politicians holding office. The INPS dataset allows us to keep track of each candidate's employment status and occupation, and we use this information to halve or double the mandated compensations, as prescribed by the law. Unfortunately, the reconstruction of data on the compensation of members of the regional councils of Basilicata, Molise and Umbria was not possible as it is regulated by internal documents that we could not retrieve. In addition, regulation for regional compensation is highly heterogeneous depending on the region and the year, generating some issues of comparability. As a result, we exclude regional compensations from our main measure of office-related rewards, and only include them in a robustness check. Furthermore, when we include them, we drop Basilicata, Molise, and Umbria from the estimation sample.

b. Sample selection

Our electoral data include all candidates at mayoral elections held in Italy from 1993 to 2017. Since Italian regions working under a special status (Sicilia, Sardegna, Valle d'Aosta/Vallée

d'Aoste, Trentino-Alto Adige/Südtirol, and Friuli Venezia Giulia) enjoy higher levels of autonomy in setting political compensation – including those of mayors – we exclude elections held in these regions from our analysis. Our initial sample includes close to 44,700 candidates who either won or were runners-up in these elections. From this sample, we drop candidates with missing information on some of the variables used to compute the social security number (name, surname, gender, and date and municipality of birth), that we use to merge electoral and social security records, ending up with roughly 42,800 candidates.

Due to privacy restrictions, we were able to import in the INPS servers only the election year, the margin of victory rounded to the closest multiple of 1%, macro-area dummies (North, Centre, South), a dummy for having had a previous mayoral appointment, and a winner dummy.[9] Furthermore, we could import only data on "close elections" – and were required to define a threshold on the margin of victory before importing the data. We select this threshold as the optimal bandwidth obtained using the data-driven approach proposed by Calonico et al. (2014), when estimating the effect of winning a mayoral election on the probability of winning a subsequent mayoral election– one of the few outcomes of interest that we observed in our data before accessing the social security files. The resulting threshold is 0.1, and there are 16,500 candidates with a margin of victory below this number (in absolute value). Due to restrictions imposed by INPS, we also dropped the cells defined in terms of the retained variables that contained less than 5 observations.

As a result, we were able to include in our data approximately 14,600 candidates. Table A1 in the Appendix compares the full sample against the included candidates along a set of demographic characteristics. Although the differences are not very large, we observe that the included candidates are less likely to be females (11.6 vs. 12.8%) and are more likely to have a tertiary education degree (47.6 vs. 44.5%). They are also more likely to run for elections in Southern Italy (34.5 vs. 28.5%) and in smaller municipalities (6,254 vs. 7,635 inhabitants on average).

For each candidate, we construct the timing of the first election at which he/she is observed and drop candidates who already served as mayors before the time span of our electoral data, because

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[9] Some of the variables used in the paper – including non-rounded margin of victory, anonymized municipality and election identifiers, candidates' demographics, their political careers, imputed office – related compensations, and municipal population – were also imported in the server later but are not accessible to us due to restrictions imposed by INPS. The INPS IT staff run our codes for any analysis involving these data.

for them we cannot compute the margin of victory at their first mayoral election. The resulting sample includes 13,691 candidates. The INPS IT staff merged the electoral dataset with the INPS social security data and replaced the social security codes with anonymized candidate identifiers. A total of 746 candidates were not present in the INPS archives, while for 920 candidates that were present in the INPS archives we retrieved empty records. Reassuringly, the estimate of the sharp regression discontinuity effect of winning a mayoral election on being missing in this sample is equal to 0.008, with a standard error of 0.009, suggesting that selection is orthogonal to electoral outcomes (see Section 3.b for details on estimation). Our final working sample consists of 12,025 candidates.

For each candidate, we reconstruct the labor and social security earnings history, the political career and office-related compensations for a maximum time span ranging from 5 years before to 14 years after the first election – or the year of death, if this event happens first – for a total of 180,192 candidate-year observations. Given that data on earnings are limited to the period 1995/2017, candidates contribute with observations to different years to/from the election depending on the year when they were first elected.[10]

c. Descriptive statistics

Table 1 displays the descriptive statistics for the key variables used in the analysis. The average candidate was born in 1955 and was 48 at the first mayoral election in which he/she either won or was a runner-up. This first election took place in 2003, on average. Only 11.7 percent of candidates is female, 99.4 percent were born in Italy, and 46 percent have a tertiary education degree. According to the OECD,[11] in 2003 this percentage in the Italian population aged 25-64 was equal to 10.5 percent, suggesting substantial positive selection into politics with respect to education (see Dal Bò et al., 2017).

Table 1 also shows that data on annual labor and social security earnings are available for 86.8 percent of candidate-year observations. Assuming completeness of the archives, we interpret missing observations as an indicator of non-participation in the labor market. For observations with non-missing data, average gross annual labor and social security earnings net of office-related

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[10] For instance, for a candidate elected in 1993 we only observe data between 2 and 14 years after the election, while for a candidate elected in 2009 the observation period ranges between 5 years before the election and 8 years after. Figure A1 in the Appendix reports the distribution of observations by year to/from the election.

[11] https://data.oecd.org/eduatt/adult-education-level.htm#indicator-chart

pay are equal to 40,672€. When we add office-related compensations for elected candidates, only 8.88 percent of observations have missing values, and average gross annual total earnings increase to 43,947€.[12]

3. Empirical methodology

a. Identification

The total earnings of winners in a mayoral election change because of office-related compensations and the labor and social security earnings losses or gains associated with winning the election. To identify these changes, one would like to compare the observed total earnings of mayors with the counterfactual total earnings had they not won the election. Our empirical strategy consists of approximating the unobserved counter-factual with the labor market earnings of runners-up in a closely contested election.

An experiment we could design to identify the long-run effects of winning a mayoral election would entail the random selection of a mayor from the population of each municipality, and the comparison of the earnings trajectories of mayors and non-elected citizens. This comparison would be informative about the average treatment effect of winning a mayoral election (in this case, a lottery for becoming a mayor) for a random person in the municipal population.

Unfortunately, our data are generated by a process where individuals self-select into politics based on their preferences and expected returns, which makes it harder to reach causal conclusions. A partial solution to this problem could be to limit the comparison of earnings profiles to those who choose to run for a mayoral office, who are likely to share similar unobservable traits with respect to selection into politics.[13] Even within the pool of mayor candidates, however, the comparison between winners and losers can hardly be taken as evidence of a casual effect, because electoral outcomes are not decided by the flip of a fair coin. Winning candidates most likely differ from runners-up along several observable and unobservable dimensions (such as skills, grit, and

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[12] In Table 1 we exclude compensations for appointments in regional institutions but include all Italian regions. If we also include compensations for regional appointments but exclude Basilicata, Molise and Umbria, for which these data are not available, we are left with 161,145 observations, earnings data are missing for 8.6 percent of candidate-year observations, and average total earnings are equal to 44,955€.
[13] An additional problem for the comparison of earnings profiles of election winners and subjects who never run for office is that this comparison would mix up the effect of winning the election with the effect of running for office, that includes for instance the time and monetary resources spent campaigning.

connections) that contribute to shape the electoral outcome and at the same time are among the determinants of their earnings capacity.

We overcome these empirical complications by leveraging the sharp regression discontinuity design that is generated by "close" municipal elections, where the outcome is decided by a handful of votes. In such cases, the quasi-randomness of the electoral outcome makes it such that winners should be, on average, comparable to runners-up along most observable and unobservable determinants of earnings. This design is used in most of the studies on returns to office mentioned in the introduction.

Let $Y_{i\tau}$ be the annual earnings of mayor candidate i observed τ years to/from e, the year of the first mayoral election in which he/she appears as either the winner or the runner-up.[14] The treatment of interest for our analysis is described by the dummy variable M_i, that is equal to 1 for winners at e, and to 0 for losers. Potential earnings at time τ are defined with respect to M_i and τ as $Y_{i\tau}(0)$ and $Y_{i\tau}(1)$, and we have that $Y_{i\tau} = Y_{i\tau}(0) + M_i \times [Y_{i\tau}(1) - Y_{i\tau}(0)]$. The causal parameters of interest for us are the τ-specific average effects of winning a mayoral election on winners, that is, $E(Y_{i\tau}(1) - Y_{i\tau}(0)|M_i = 1), \tau = 0, ..., \text{T}$.

For each candidate i, we let V_i be the difference in the share of votes between him/her and his/her best opponent. Given the system governing municipal election in Italy (see Section 2.a and Appendix C for details), we have that $M_i = 1(V_i > 0)$. This setup defines a sharp regression discontinuity design where V_i is the running variable and the cut-off point is at $V_i = 0$. Under a standard assumption on the continuity of the non-treatment potential outcome function at the cut-off $\left(\lim_{v \to 0^+} E(Y_{i\tau}(0)|V_i = v) = \lim_{v \to 0^-} E(Y_i(0)|V_i = v), \tau = 0, ..., \text{T} \right)$, the comparison of average earnings at time τ between candidates who barely won and lost their first mayoral election $\left(\lim_{v \to 0^+} E(Y_{i\tau}|V_i = v) - \lim_{v \to 0^-} E(Y_{i\tau}|V_i = v), \tau = 0, ..., T \right)$ identifies our parameter of interest for observations in a small neighborhood of $V_i = 0$ $(E(Y_{i\tau}(1) - Y_{i\tau}(0)|M_i = 1, V_i = 0), \tau = 0, ..., T)$.

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[14] The opponent of a first-time winner or runner-up may not be present in our data if he/she is not at his/her first election. While our estimates are based on all winners and runners-up that we observe in this pooled sample, in a robustness test we include election fixed effects and compare pairs of winners and runners-up who ran for the same seat.

Importantly, the longitudinal dimension of our data on earnings allows us to carry out placebo tests for the validity of the continuity assumption for time periods before e. We also provide evidence that this identification strategy delivers a set of winners and runners-up that are comparable along a set of demographic characteristics, and that the density of the score V_i is smooth at the cut-off, alleviating potential concerns about the possibility that electoral outcomes could have been manipulated.

Before moving to estimation, it is important to clarify that our parameter of interest does not refer to *serving* as mayor, but to *winning an election* for a mayoral seat. Figure 1 reports the share of mayors by treatment status and time. Consistent with the definition of our sample, none of our candidates served as mayor before election year e. The figure also illustrates that the distinction between winning an election and serving as mayor is virtually irrelevant in the year of the election, but it becomes substantial afterwards. On the one hand, roughly 20 percent of winners terminate their service before the legal duration of a mandate (5 years), and close to 40 percent of winners are re-elected and serve for a second mandate. Moreover, consistent with the two-term limit rule (see the Appendix for more details), only a minority of first-term winners – who were not initially re-elected for their second mandate – serve as mayors ten years after their first victory. On the other hand, we also see that, as time goes by, roughly 12 percent of the initial runners-up also serve as mayors, and that in most cases this happens after the first mandate of the winner is over (that is, in year 5). This figure helps us highlight that the dynamics of office-holding are an important mechanism behind the effects of winning an election on long-run earnings dynamics. We investigate this matter at length in Section 4.d.

b. Estimation

We jointly estimate the parameters of interest by stacking, for each individual i, the data for all time periods τ, and by using local linear regressions. We estimate with Ordinary Least Squares the following linear model:

$$Y_{it\tau} = \sum_\tau D_\tau \times [\beta_\tau M_i + \gamma_{0\tau} + \gamma_{1\tau} V_i + \gamma_{2\tau} M_i \times V_i] + \delta_t + \varepsilon_{it\tau} \qquad [1]$$

where D_τ is a set of period dummies and t is the calendar year, so that $\tau = t - e$. Coefficients in vector β_τ capture the sharp regression discontinuity effects of winning a mayoral election on earnings τ years after the election. We allow for period τ-specific piecewise linear trends in the margin of victory V_i with a single knot at $V_i = 0$ ($\gamma_{0\tau} + \gamma_{1\tau} V_i + \gamma_{2\tau} M_i \times V_i$), and include calendar

year fixed effects (δ_t) to account for aggregate shocks. Finally, $\varepsilon_{it\tau}$ is an error term that we cluster by candidate to allow for serial correlation.[15]

Equation [1] assumes that, on each side of the cut-off, potential earnings evolve linearly as a function of V_i, and uses a rectangular kernel to weight observations with different values of V_i. Moreover, as illustrated in Section 2, our sample is limited to observations with $V_i < 0.1$. Since this cut-off was decided ex-ante and without carrying out any data mining procedure, we do not select an optimal bandwidth in the final sample using data on earnings for every period τ. Even if we had the possibility to do so, however, we are not aware of methods that – in a panel data context – deliver a time-specific optimal bandwidth while allowing for clustering of standard errors at the unit level. To support our modelling choices, in Section 4.b we test the sensitivity of our estimated effects when we use a quadratic instead of linear functional form for the polynomial in V_i, a triangular instead of rectangular kernel to weight observations, and a smaller bandwidth (0.07 and 0.05).

4. Results

a. Internal validity checks

Table 2 reports the results of balancing tests after regressing pre-determined candidate-level characteristics on a binary variable equal to 1 for winners of a mayoral election and to 0 for runners-up, using one observation per candidate. The p-value of the test of no joint significance of all the estimated effects, obtained using seemingly unrelated estimation, fails to reject the null, as reported at the bottom of the table. The results confirm that our identification strategy delivers comparable samples of winners and runners-up.

We also include in all the tables reporting estimated treatment effects (Tables 3 to 6 and 8 to 11) the estimates of placebo treatments in pre-election periods. Overall, these results support the validity of the design for virtually all outcomes. We only detect slightly statistically significant, but nonetheless small, pre-treatment effects on the probability of being a member of a municipal council (higher for runners-up) and of being a member of a regional council (higher for winners) before the first mayoral election – see Table 6.

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[15] There are two advantages from joint estimation: *i)* testing the significance of the differences in the effects across different time periods; *ii)* allowing for within-candidate serial correlation in the error terms.

Finally, Figure 2 reports the density of the margin of victory and reveals no evidence of suspicious jumps or bunching at the cut-off, supporting the absence of manipulation in the electoral outcomes. This graphical evidence is confirmed by the p-value of a test for the continuity of the density of the margin of victory at the cut-off (see Cattaneo, Jansson and Ma, 2020), that is equal to 0.65.

b. Effects on total earnings trajectories

Figure 3 shows the estimates of parameter β_τ for the log of annual total earnings, that consist of labor earnings, pensions, other social security payments (namely unemployment insurance, short time work subsidies and maternity leave) and office-related compensations, after excluding observations with missing values. The figure shows that winners and runners-up follow comparable earnings trajectories before the election. From the election year, however, winners experience a substantial increase in total earnings (approximately equal to 30 log points) relative to runners-up. This effect becomes negligible and insignificant after the first electoral term is over (year 5) and turns negative in the last electoral term (from year 10).

Figure 4 reports an example of regression discontinuity plot for average log total earnings and for bins of the margin of victory of width 0.01 in the year after the first election. The figure shows that, on both sides of the cut-off, potential outcomes are remarkably flat functions of the running variable. This result holds more in general, as we almost always find very small and mostly insignificant coefficients associated with the trends in the margin of victory. As highlighted for instance by Angrist and Rokkanen (2015), the lack of correlation between the running variable and potential outcomes is reassuring for the extrapolation of our findings away from the regression discontinuity cut-off, thereby enhancing the external validity of our results also for elections where electoral competition was not extremely tight. Furthermore, this result weakens concerns about the relevance of bandwidth choice for estimation.

To gain statistical power, we constrain β_τ in Equation [1] to be constant within each electoral term, that is, for the 5 years before the first election and for years 0 to 4, 5 to 9, and 10 to 14, and report the corresponding estimates in Table 3. The results for the baseline specification – see column (1) – illustrate that the average premium for annual total earnings during the first electoral term (0 to 4) is equal to 26.7 percent and statistically significant at less than the 1 percent level. The premium fades away completely in the second term, and turns negative in the third term, when it is equal to -10.38 percent (and statistically significant).

The remaining columns of Table 3 assess the sensitivity of our main results when we adopt alternative specifications of Equation [1]. Column (2) shows that results are unaltered when we include the individual variables listed in Table 1 as controls. In column (3) we include election fixed effects and limit our comparisons of earnings trajectories within pairs of candidates that compete for a mayoral seat in the same municipality and the same election. Thus, this approach excludes candidates whose opponent is not present in our final sample. Next, given that the observed earnings distribution is positively skewed, column (4) reports the results that we obtain when, instead of using OLS regressions and modelling the impact on mean earnings, we use unconditional quantile regressions and model median earnings. Again, our results are stable. Furthermore, column (5) shows that results using earnings levels, instead of logs, are also comparable to our baseline – although less precisely estimated. Using the specification in column (5) and a 2 percent discount rate, we compute that the present discounted value of the effects over years 0 to 14 is equal to 34,568€, roughly 85 percent of the average annual labor and social security earnings observed in the data. Over a 15-year horizon, this is a modest effect.

Table 4 reports instead some standard specification tests for the sharp regression discontinuity design, that include the use of: *i)* a quadratic polynomial for the margin of victory; *ii)* a triangular kernel; *iii)* a bandwidth of 0.07 and 0.05; *iv)* a "donut hole" of 0.01, to eliminate observations close to the cut-off, that are more prone to potential issues of manipulation of the running variable. Consistent with the flat relationship between potential outcomes and the running variable exemplified in Figure 4, the results are rather stable across specifications. If anything, in some specifications the effect shrinks marginally and loses significance 10 to 14 years after election. Next, in Table A2 in the Appendix we show that the dynamics in the effects that we estimate by electoral term are stable for candidates elected in different years. This result suggests that our main results are not driven by the unbalanced nature of our panel (see Section 2.b). Finally, column (1) of Table A3 in the Appendix shows comparable effects even when we include in office-related compensations also those for members of regional councils, but exclude Basilicata, Molise and Umbria, for which these data are not available. We conclude that the patterns of results presented in column (1) of Table 3 are robust to a broad set of specification tests.

The results in Table 3 rely on a sample that excludes the share (8.88 percent) of observations with missing earnings records, that we interpret as nonparticipation in the labor or political markets. Column (1) of Table A4 in the Appendix reports the effects of winning the election on the

probability of observing any earnings or compensations and shows that winners are more likely than runners-up to report any earnings or compensations during the first electoral term. The opposite holds for the third term, when initial runners-up are more likely to be mayors than initial winners (see Figure 1). This motivates us to assess the sensitivity of our findings to the inclusion of observations with missing earnings data, after imputing the missing potential earnings of nonparticipants.

Columns (2)-(4) of Table A4 report the estimates that we obtain when we assign zero to the missing values of the log, the level, and the inverse hyperbolic sine of earnings. The effects increase substantially with respect to those in Table 3. This is not surprising as we estimate the effect on the mean of earnings, which is very sensitive to the presence of outliers, and we impute implausibly low values for nonparticipants' potential earnings. To overcome these concerns, in column (5) we follow Olivetti and Petrongolo (2008) and model the effects of winning on median rather than mean earnings, after imputing an arbitrarily low value (zero) to unobserved potential earnings. Unlike the mean, however, the median depends only on the relative position of observations, and not on the specific imputed value. Because of this, we consider this strategy as more credible than imputing zero to missing values and estimating treatment effects on the mean. Reassuringly, results in column (5) confirm both qualitatively and quantitatively those of reported in Table 3.

c. Effects on office-related compensations vs labor and social security earnings

To investigate the mechanisms behind our findings in Table 3, we distinguish between the effects on office-related compensations and political careers and the effects on the sum of labor and social security earnings.[16] Figure 5 reports the year-by-year estimates of the effects of winning a mayoral election on log labor and social security earnings, net of office-related compensations and after excluding observations with missing values. The figure shows that, starting from the election year, winners experience a penalty in their earnings with respect to runners-up, which persists until 10 years after the first election and fades away later.

Column (1) of Table 5 reports our estimates when we pool the data by electoral term. The penalty during the first term (years 0 to 4) is equal to 16.6 percent on average, and persists in the second

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[16] Results reported here exclude compensations for members of regional councils but include all regions. Columns (2)-(4) of Table A3 in the Appendix show that we obtain comparable results including those compensations but excluding Basilicata, Molise, and Umbria, for which these data are not available.

term, when it is equal to 13 percent, a value statistically indistinguishable from the one estimated for the first term. The negative gap declines substantially during the third term, when it is equal to 6.5 percent and statistically significant only at the 10 percent level. The differences between the effects during the first two and the third terms are significant at less than the 5 percent level. Column (2) of Table 5 reports comparable effects on labor and social security earnings levels, though less precisely estimated. These results suggest that working as a mayor significantly reduces labor and social security earnings from the election to 9 years after the election. The penalty remains, but declines in size, in the long run (from 10 to 14 years after election).

As shown by Figure 6 and column (3) of Table 5, earnings losses are compensated by office-related pay, which is equal on average to 16,641€ per year. Although smaller in magnitude, the compensatory role of this pay remains 5 to 9 years from election, when it is equal to 3,935€ per year, and turns negative and equal to -1,170€ per year 10 to 14 years after starting office.

d. Effects on career dynamics

We investigate career dynamics by looking at the evolution over time of the political careers of winners and runners-up – see Table 6.[17] Columns (1)-(3) report the effects of winning a mayoral election at time 0 on being a member of the city council (the legislative body of a city), a member of the *Giunta* (the executive committee of a city council), or a mayor at time τ, which takes values -1 to -5, 0 to 4, 5 to 9 and 10 to 14. We find that winning the election at time 0 has positive and significant effects on holding office at $\tau = 0$ to 4 and at $\tau = 5$ to 9, either as a council member, a member of the *Giunta*, or a mayor. Consistent with the two-term limit rule for mayoral appointments, we estimate that winning the first-term election reduces the probability of being a mayor at $\tau = 10$ to 14. This probability declines but remains positive for council members and presidents of the city council.

These findings contribute to explain why the negative effect of winning a mayoral election on earnings (net of office-related compensation) declines in absolute value as time goes by. First, some runners-up in the election at time 0 may win later elections for a mayoral seat (for instance at time 5 to 9 or 10 to 14), and therefore face a drop in earnings during their first electoral term.

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[17] As noted in Section 4.a, we detect some imbalances in the probability of being a city or regional council member before the election. Therefore, results for these outcomes shall be interpreted with caution.

Second, the lower involvement in municipal politics of winners at time 0 who are not re-elected is consistent with their progressive return to work and thus to higher earnings outside of politics.

Political careers do not necessarily end at the municipal level. In columns (4)-(8) of Table 6 we investigate whether winning a mayoral election at time 0 affects the probability of holding – later on – an office at higher levels of government. Compared to close runners-up, we find that winners at time 0 have a lower probability of being a regional council member or a member of the Italian Parliament during the first electoral term (τ=0 to 4) – when they are appointed as mayors. They have, however, a larger probability of being a provincial or regional council member at τ=10 to 14, when the two-term limit rule prevents them to be re-elected as mayors. These effects for higher levels of government are smaller in magnitude than the ones for municipal government (and less precisely estimated), possibly because of the more limited number of available seats. We conclude that, although holding office at higher levels of government 10 to 14 years after winning a mayoral election may imply less involvement in the labor market even after the experience as mayors is over, this channel is in place for a minority of candidates at best.

A relevant concern for the external validity of our findings is how the estimated effects in Table 3 would change if runners-up did not win a mayoral office after losing the first election. This concern was first voiced by Berg (2020a), who claims that "RD estimates fading out over time likely reflects that the differences in treatment (being elected) fade out over time. This is because many candidates in the control group—candidates who were close to being elected in a given election— often run again and indeed are elected in the subsequent election". An implication is that the total effect uncovered by our RD design can be decomposed in two parts: a direct effect, owing to winning the election at time *e*, and an indirect effect owing to the possibility that runners-up who lose at *e* may win at a later election. As shown in Figure 1, in our data close to 11 percent of initial runners-up win an election in the second term (years 5 to 9), and an additional 1.5 percent win for the first time in the third term (years 10 to 14).

Berg (2020a) addresses the issue by using a difference-in-differences design where the control group is composed of never-elected candidates. In our setup, this would be a debatable choice considering that we do not use difference-in-differences and electoral wins are generally not random. Following Cingano et al. (2022), we approach the problem under the assumption of stability of treatment effects across initial winners and initial runners-up who win an election at

later electoral terms. First, given that most runners-up start taking mayoral positions only from year 5 (see Figure 1), we notice that the total effect we identify for the first electoral term (years 0 to 4) coincides with the direct effect. Second, under the stability assumption, we subtract this effect from the observed earnings of the initial runners-up who won an election in years 5 to 9, and thus estimate the direct effect accruing to initial winners during the second electoral term (years 5 to 9). Third, we estimate the long-run direct effect for initial winners by iterating this step to the third electoral term (years 10 to 14). We do so by subtracting: *i)* the direct effect of a win 0 to 4 years after the election from the earnings of initial runners-up who first won an election during years 10 to 14; and *ii)* the direct effect of a win 5 to 9 years after the election from the earnings of runners-up who won an election during the years 5 to 9.

Our results – reported in Table 7 – show that, for total earnings, the total and direct effects have the same sign and are of comparable magnitude. In addition, the present discounted value of the stream of earnings for years 0 to 14 would change from 34,568€ (total) to 42,664€ (direct), not a substantial difference. Nevertheless, some qualitative differences emerge when we contrast estimated effects on labor and social security earnings with the ones of office-related compensations. On the one hand, since winning election decreases labor and social security earnings in the first (and second) term, long-run earnings of runners-up are negatively affected by later wins, and thus the negative total effects are smaller in absolute value than the negative direct effects. On the other hand, and consistent with the claim by Berg (2020a), winning an election increases compensations in the first (and second) electoral term, but since runners-up can win elections later while incumbents cannot, direct effects in the third term are positive despite negative total effects.

e. Heterogeneous effects

The withdrawal from the labor market during a political career that could last longer than 10 years after becoming mayor is associated with costs and benefits that may vary both with the occupation held before the political experience and with local labor market conditions. In this sub-section, we explore this variation. Unfortunately, our sample is not large enough to allow us to reach firm conclusions from a statistical viewpoint in all the sub-samples we consider. Therefore, we take this evidence as suggestive.

Previous employment

Table 8 reports the estimates of parameter β_τ for labor and social security earnings by candidates' sector of employment before the election. To have at least one year of pre-election data for all candidates, we include in the sample only elections held from 1996 onwards. There is suggestive evidence that the negative effects of the treatment on earnings during the first electoral term (0 to 4) are larger among private sector workers, which include both employees and self-employed. This could be explained with the lower variance of earnings and lower earnings growth in the public vs. private sector, implying that public employees among the runners-up gain little from dedicating more time and effort to the labor market. It is also possible that public sector workers find it easier than private sector employees and the self-employed to take time off work and dedicate it to politics without sacrificing earnings.

The negative effect that we find on private employees during the first electoral term is in contrast with the positive effects estimated by Cingano and Pinotti (2013) and Akcigit et al. (2022). As discussed in the introduction, several factors help explain these differences, including the political positions being considered (mayors vs. all politicians), the dependent variable (annual labor and social security earnings for all candidates vs. average weekly wages for candidates who work as private sector employees), the time span (1995-2017 vs. 1985-97 for Cingano and Pinotti and 1985-2014 for Akcigit et al.), the research design (regression discontinuity on close mayoral elections vs. difference-in-differences that compares local politicians with never appointed individuals or colleagues).

Table 8 also shows that the persistency of the negative effect of the treatment on earnings is more marked among the self-employed than among employees. This heterogeneity can be explained by differences in earnings growth, which may be higher for the self-employed, and by the possibility of fruitfully returning to the previous job after a political experience, that may be easier for employees. Rather unsurprisingly, we also find that retirees do not experience any significant earnings change because of their political experience. Finally, winners who were not employed before their political experience lose out substantially with respect to runners-up, who may have used their higher time availability to find better paying jobs while elected mayors were in office.

We also investigate whether winners and runners-up have different patterns of mobility across sectors after electoral outcomes at time 0. Results are reported in Tables B1 to B7 of Appendix B. Here, we summarize the main findings as follows:

i)   Winners who were private sector employees are more likely than runners-up to leave employment during the first (0 to 4) and second (5 to 9) term. This could indicate that – compared to public sector workers – private sector employees may find it more difficult to keep both their job and their political office.

ii)   Winners who were public employees postpone retirement to work as fixed-term collaborators after the mandate, although with no consequence on earnings (see Table 8).

iii) Self-employed professionals are more likely to leave employment, a persistent effect that may depend on the same mechanism discussed for private sector employees as well as on the difficulty to return to employment after the political service is over.

iv)   Not-employed winners are more likely to retire during the first electoral term than runners-up do, a result that may depend on the inability of winners to dedicate time to job search.

Overall, our results show limited evidence of mobility across sectors. While we see evidence of detachment from the labor market because of a political involvement, the estimates do not suggest that those involved in politics change sector of employment during their mandate or after their mandate is over. Our findings also fail to display positive earnings effects after the mandate. This pattern of results is in contrast with the idea of "revolving door" behavior (e.g., see Blanes-i-Vidal et al., 2012), as we do not find that the competences learned during the political service as mayors induce winners to exploit their new skills and connections and start jobs as self-employed or consultants to help firms greasing some wheels with their political connections.

Geographic area

Given the well-known North-South gap in social capital, informality, and political and labor market dynamics in Italy, Table 9 reports the heterogeneous effects of winning a mayoral election by area. We distinguish between the South (including Abruzzo, Basilicata, Calabria, Campania, Molise, and Puglia) and the Centre-North (including the other regions). Results show that effects on total earnings are rather comparable across areas. If anything, the negative effects detected in the pooled sample for the last electoral term (10 to 14) are much larger (15.9 vs. 8.1 percent) in the South.

The mechanisms behind this heterogeneity deserve further investigation. For instance, if incumbency effects were stronger in the South, where political budget cycles are more marked (see Alesina and Paradisi, 2017), then longer careers in politics could explain the stronger

23

persistence of negative effects on earnings. When we distinguish between office-related compensations and labor and social security earnings, we find that the negative and more persistent effects in the South are mostly due to labor and social security earnings. If anything, effects on compensations in the second term are more positive in the Centre-North than in the South. This evidence does not support longer political careers as a mechanism behind stronger persistence in the South. This is also confirmed by the evidence in Table 10, that reports the effects on careers in politics by area and shows that the effects of winning the first-term election on holding an office at a later term is larger in the Centre-North of Italy – especially at the municipal level.

After completing their service, winners of mayoral elections may find it more difficult to re-entry the labor market when there is high unemployment, or when work-related connections – that may be weakened during service - play an important role. On the other hand, winners may find it easier to enter the illegal labor market, especially if winning a mayoral election brings candidates in contact with criminal associations. High unemployment, the importance of social ties and illegal labor markets are more important in Southern Italy than in the more industrialized North (see Ponzo and Scoppa, 2010).

We investigate the "slack labor markets with higher re-entry costs" and the "informal labor market" hypotheses by focusing on the South of Italy and estimating parameters β_τ separately for provinces with a share of illegal employment above and below median and with above and below median unemployment rates. Data on illegal employment shares are from the Italian National Statistical Office – ISTAT – and refer to 2003, the only available year.[18] Data on local unemployment rates are also from ISTAT and refer to 2005, the central year of our sample. We show the classification of provinces by share of illegal employment and unemployment rate in Figure 7, and report in Table 11 our estimates of parameter β_τ by share of illegal employment (columns (1) and (2)) and by unemployment rate (columns (3) and (4)). On the one hand, earnings effects are comparable across provinces with illegal employment shares below and above the median, suggesting that different levels of informality in the local area do not contribute to explain the magnitude or persistence in the effects of winning an election. On the other hand, these effects

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[18] These shares are estimated by ISTAT by comparing the aggregate number of employed individuals in each province - obtained from firms' register data - with the same number obtained from representative household surveys (see ISTAT, 2005, for details).

are small and not statistically significant in areas with low unemployment, and substantially larger, more persistent, and statistically significant in areas with high unemployment.

Considering that Figure 7 shows a positive correlation between the illegal employment share and the unemployment rate, in Table A4 in the Appendix we report the estimates of parameter β_τ for separate groups, defined in terms of the joint (instead of the marginal) distribution of above/below median illegal employment and unemployment. Although the small sample size of the four sub-samples affects the precision of our estimates, we find that it is only in areas with above median unemployment – irrespective of the level of illegal employment share – that the labor market effects of winning the election are large, negative, and persistent. We conclude from this that the difficulty to return to work after serving as mayors in slack labor markets with high entry cost is the most plausible explanation behind the higher persistence of the negative effects of the treatment on earnings (net of office-related compensations) in the South.

Other heterogeneous effects

We discuss heterogeneous effects by gender, education, age at first election, level of earnings before the first election, and population size in the Appendix – see Appendix Tables C1 and C2. Here, we summarize our key findings as follows:

i) Compared to runners-up, female winners of a mayoral election have more to lose in terms of labor and social security earnings net of office-related compensations than male winners;

ii) Compared to runners-up, the highly educated, those with higher earnings before the election, and senior candidates are better at limiting labor and social security earnings losses net of office-related compensations than the low educated, those with lower earnings, and junior candidates;

iii) Although compensations for service increase with population size, the positive effect of winning a mayoral election on total earnings is larger in smaller municipalities, probably because the higher burden of responsibilities and duties faced by mayors in larger cities does not allow them to maintain a strong attachment to the labor market.

5. **Conclusions**

Using Italian data and a RDD identification strategy, we have provided the first available evidence on the long-term pecuniary licit returns to being elected as mayor, the leading political position in

municipal councils. We have shown that winners of mayoral elections initially earn on average 12,000€ per year more than runners-up, a 30 percent premium in terms of annual average total earnings. This positive premium, however, vanishes already 5 years after the first election and becomes negative after 10 years.

Breaking down total earnings into office-related compensations and labor and social security earnings, we have shown that winning an election increases compensations on average by roughly 16,000€ per year, but only during the first electoral term. In later terms, consistent with the presence of a two-term limit, many initial winners leave politics while some runners-up win elections. Being elected does not help instead labor and social security earnings, as mayors face on average a penalty close to 3,000€ per year with respect to runners-up, or 15 percent of runners-ups earnings. Although smaller, this penalty remains even after 10 to 14 years from the election. We have explained this result with differences in the political careers of winners and runners-up and with the two-term limit rule on mayoral elections.

Our empirical investigation of Italian local elections indicates that the present discounted value of the average gains accumulated over 15 years is close to 35,000€, or 85 percent of average annual labor and social security earnings, suggesting that over such a long horizon the expected pecuniary returns from winning a mayoral election are positive but rather modest. Since our analysis covers licit earnings and compensations for political service, and excludes capital gains, illicit personal earnings, or returns accruing to family members, our estimates are likely to be a lower bound of the overall average pecuniary returns. They remain, however, the relevant ones to inform honest citizens who are unwilling to engage in illicit activities or in nepotism.

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Figures and Tables

Figure 1. Share of mayors by time since the first election and electoral outcomes.
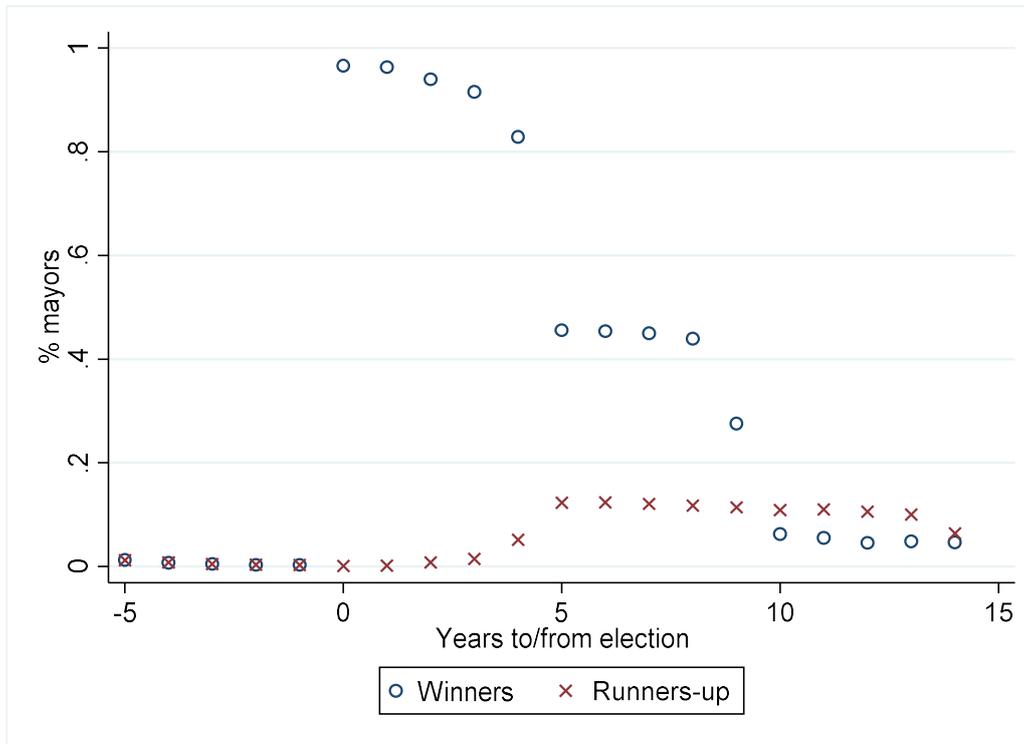
Notes: the sample includes 180,192 candidate-year observations for 12,025 candidates.

Figure 2. Density of the margin of victory
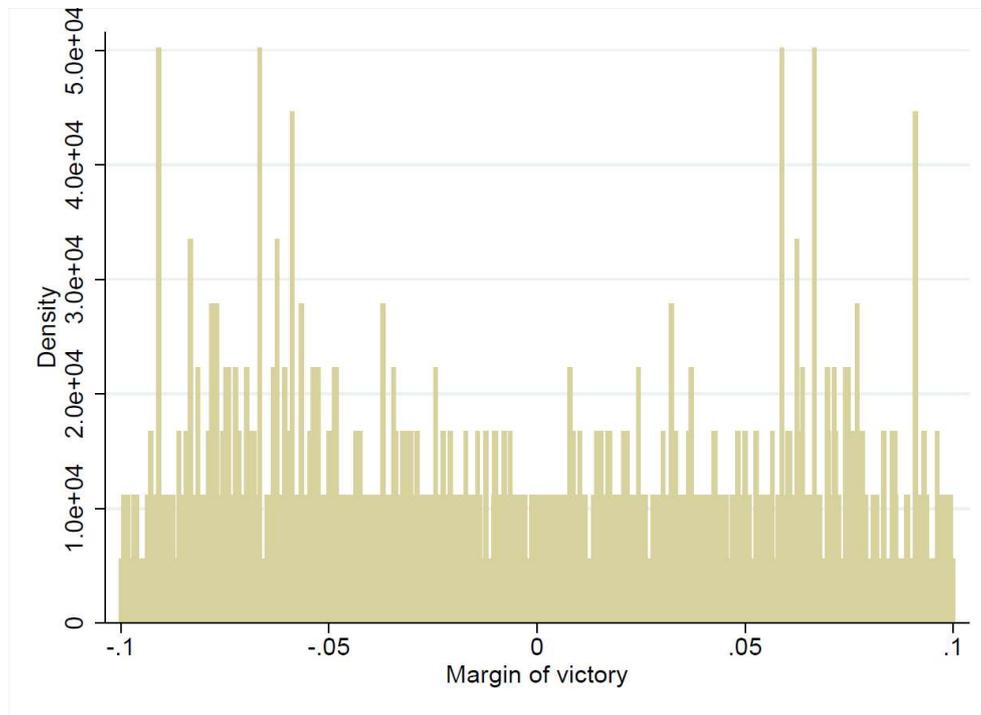
Notes: the sample includes 12,025 candidate-level observations.

Figure 3. The effects of winning a mayoral election on annual log (labor and social security earnings + political compensations).

Notes: the figure reports the local linear regression estimates of the effects of winning a mayoral election on annual log (labor and social security earnings + compensations), excluding observations with no earnings or compensations. Earnings and compensations are measured in 1,000€ at 2010 prices. Annual effects are reported from 5 years before to 14 years after the election. The model includes calendar year dummies, years to/from election dummies, and years to/from election-specific linear splines in the margin of victory. We use a bandwidth of 0.1 and a rectangular kernel. Standard errors are clustered by candidate. Confidence intervals at the 90, 95 and 99 percent level are also reported. The estimation sample includes 164,353 candidate-year observations for 12,025 candidates.

Figure 4. Average log (labor and social security earnings + compensations) in the year after the election, by margin of victory.
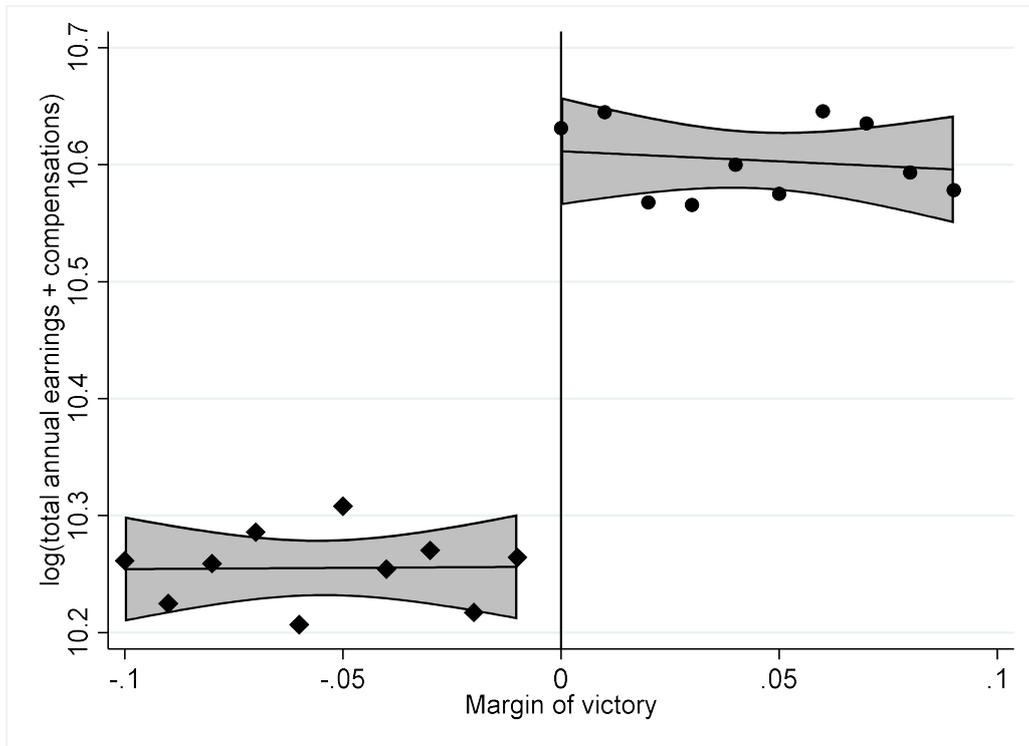
Notes: the figure reports the average log (labor and social security earnings + compensations) in the year after the election, excluding observations with no earnings, for margin of victory bins of width 0.01. We also report the local linear regression fit of the data with its 95 percent confidence interval, separately for observations with a positive and a negative margin of victory. Earnings are measured in 1,000€ at 2010 prices. The estimation sample includes observations for 12,025 candidates.

Figure 5. The effects of winning a mayoral election on annual log (labor and social security earnings).
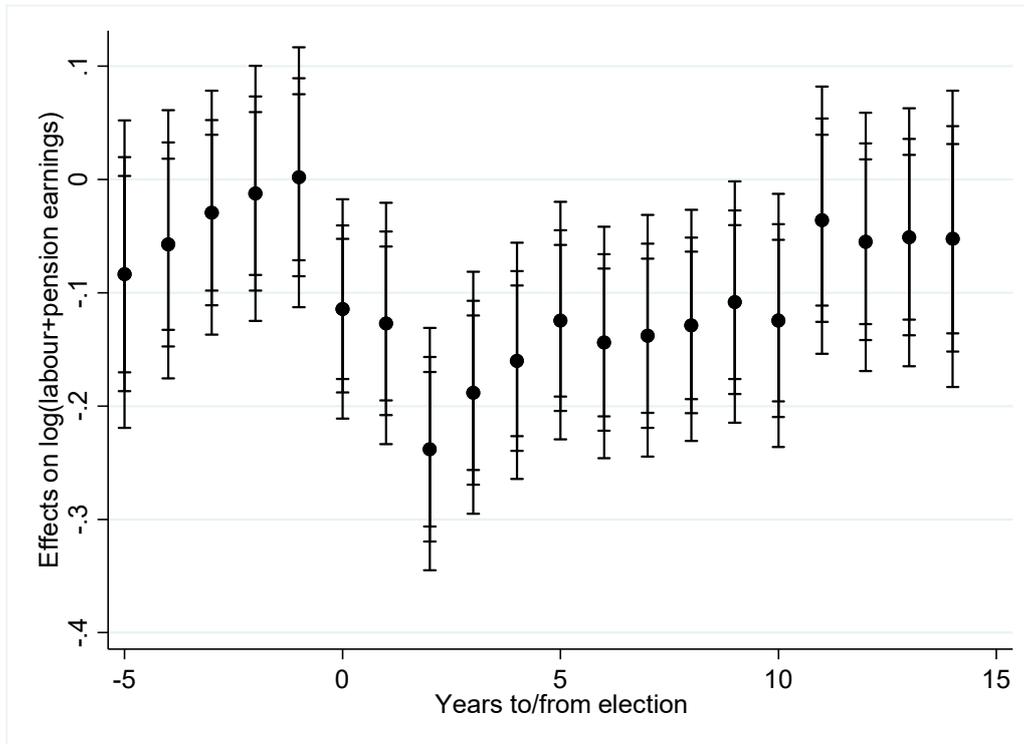
Notes: the figure reports the local linear regression estimates of the effects of winning a mayoral election on annual log (labor and social security earnings), excluding observations with no earnings. Earnings are measured in 1,000€ at 2010 prices. Annual effects are reported from 5 years before to 14 years after the election. The model includes calendar year dummies, years to/from election dummies, and years to/from election-specific linear splines in the margin of victory. We use a bandwidth of 0.1 and a rectangular kernel. Standard errors are clustered by candidate. Confidence intervals at the 90, 95 and 99 percent level are also reported. The estimation sample includes 164,353 candidate-year observations for 12,025 candidates.

Figure 6. The effects of winning a mayoral election on annual compensation levels (1,000€).
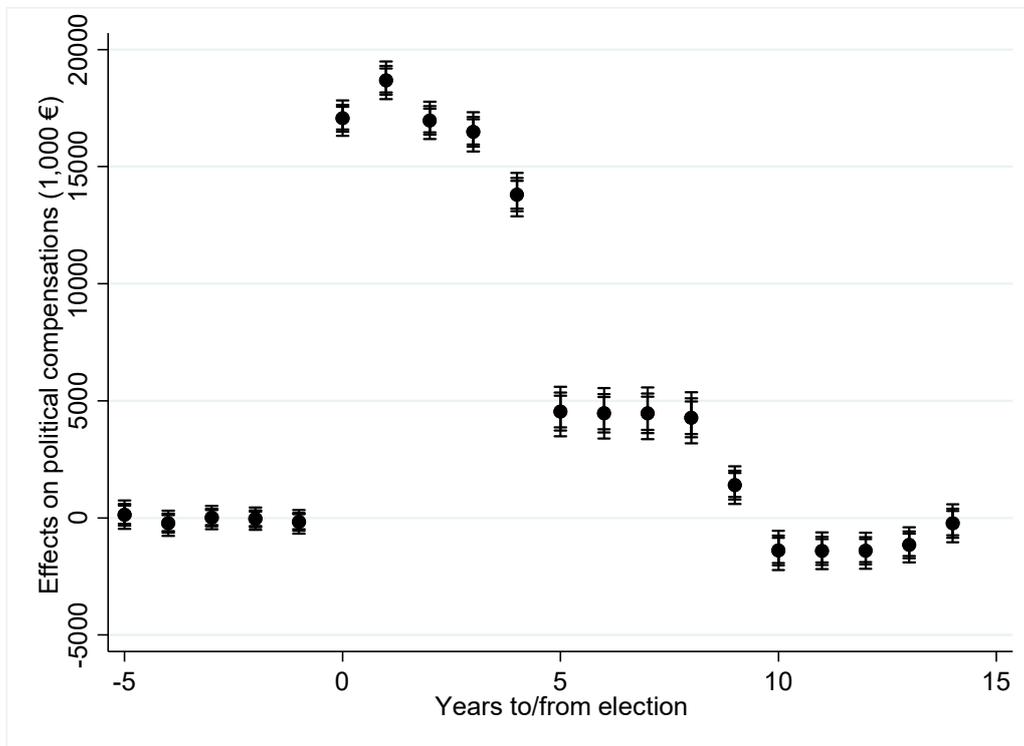
Notes: the figure reports the local linear regression estimates of the effects of winning a mayoral election on annual compensation levels. Compensations are measured in 1,000€ at 2010 prices. Annual effects are reported from 5 years before to 14 years after the election. The model includes calendar year dummies, years to/from election dummies, and years to/from election-specific linear splines in the margin of victory. We use a bandwidth of 0.1 and a rectangular kernel. Standard errors are clustered by candidate. Confidence intervals at the 90, 95 and 99 percent level are also reported. The estimation sample includes 180,192 candidate-year observations for 12,025 candidates.

Figure 7. Illegal employment share and unemployment across Southern-Italian provinces

Notes: sample limited to municipalities in Southern Italy. The illegal employment share by province is for 2003. The unemployment rate by province is for 2005.

Table 1. Descriptive statistics

Variable	Observations	Mean	Std. Dev.
Demographics			
Year of birth	12,025	1955	11.82
Age at first election	12,025	47.98	10.46
Year of first election	12,025	2,003	6.999
Female	12,025	0.117	0.321
Born in Italy	12,025	0.994	0.075
Has tertiary education	11,716	0.460	0.498
Population of candidate's municipality	12,025	6,203	26,406
Electoral outcomes			
Winner	12,025	0.478	0.500
Margin of victory	12,025	-0.003	0.056
Annual earnings			
Pr(any labor and social security earnings)	180,192	0.868	0.339
Labor and social security earnings – excluding missing values	156,346	40,672	39,895
Log labor and social security earnings – excluding missing values	156,346	10.274	0.955
Pr(any labor and social security earnings + compensations)	180,192	0.912	0.283
Labor and social security earnings + compensations, excluding missing values	164,353	43,947	39,978
Log labor and social security earnings + compensations, excluding missing values	164,353	10.39	0.870

Notes: Compensations: pay associated with holding office at the municipal or provincial level.

Table 2. Balancing tests

Dependent variable	(1) Year of birth	(2) Year of first election	(3) Age at first election	(4) Female	(5) Born in Italy	(6) Has tertiary education	(7) Population
Winner	-0.5410	-0.0893	0.4518	-0.0074	-0.0000	0.0129	-754
	(0.4263)	(0.2568)	(0.3782)	(0.0118)	(0.0027)	(0.0184)	(845)
Joint significance p-value	0.756						
Observations	12,025	12,025	12,025	12,025	12,025	11,716	12,025

Notes: the table reports the average effects of winning a mayoral election on the individual characteristics listed in the column headings. Estimates are based upon local linear regressions. Each regression includes a linear spline in the margin of victory. We use a bandwidth of 0.1 and a rectangular kernel. Robust standard errors are reported in parenthesis. The p-value for the joint significance of all the effects is also reported at the bottom of the table. ***: p<0.01; **: p<0.05; *: p<0.10.

Table 3. The effects of winning a mayoral election on annual labor and social security earnings + compensations, by years since/to election.

Dep. var.: log(labor and social security earnings + compensations)	(1)	(2)	(3)	(4)	(5) Baseline. Dep. Var.: labor and social security earnings + compensations levels (€)
Specification	Baseline	With individual demographic controls	With election fixed effects	Unconditional median regression	
Years to/since election					
-5 to -1	-0.0484	-0.0463	-0.0454	-0.0428	-1,008.5
	(0.0370)	(0.0345)	(0.0378)	(0.0306)	(1,548.2)
0 to 4	0.2670***	0.2604***	0.2748***	0.2561***	7,958.0***
	(0.0266)	(0.0254)	(0.0258)	(0.0247)	(1,392.5)
5 to 9	-0.0171	-0.0163	-0.0216	0.0078	204.9
	(0.0306)	(0.0299)	(0.0296)	(0.0299)	(1,539.6)
10 to 14	-0.1038***	-0.1003***	-0.1039***	-0.0889**	-1,162.5
	(0.0370)	(0.0365)	(0.0349)	(0.0355)	(1,749.3)
Observations	164,353	164,353	163,741	164,353	164,353
Candidates	12,025	12,025	12,001	12,025	1,2025

Notes: the table reports, by years since/to election, the average effects of winning a mayoral election on annual labor and social security earnings + compensations, excluding observations with no earnings or compensations. Earnings and compensations are measured in 1,000€ at 2010 prices. Unless otherwise noted in the column heading: i) estimates are based upon local linear regressions; ii) the dependent variable is measured in logs; iii) additional controls include calendar year dummies, years to/from election dummies, and years to/from election-specific linear splines in the margin of victory; iv) we use a bandwidth of 0.1 and a rectangular kernel. Demographic controls in column 2 include age, age squared, gender, being born in Italy, having tertiary education (and a dummy for missing education), and municipality population. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 4. The effects of winning a mayoral election on annual log(labor and social security earnings + compensations), by years since/to election. RD specification tests.

Dep. var.: log(labor and social security earnings + compensations)	(1)	(2)	(3)	(4)	(5)
Specification	Quadratic margin of victory	Triangular kernel	Bandwidth <0.07	Bandwidth <0.05	Donut hole of 0.01
Years to/since election					
-5 to -1	-0.0214	-0.0382	-0.0487	-0.0027	-0.0675
	(0.0550)	(0.0398)	(0.0434)	(0.0513)	(0.0462)
0 to 4	0.2813***	0.2726***	0.2655***	0.3140***	0.2557***
	(0.0406)	(0.0291)	(0.0318)	(0.0376)	(0.0326)
5 to 9	-0.0055	-0.0127	-0.0270	0.0205	-0.0464
	(0.0453)	(0.0329)	(0.0361)	(0.0424)	(0.0386)
10 to 14	-0.0798	-0.0947**	-0.1111**	-0.0688	-0.1412***
	(0.0552)	(0.0401)	(0.0435)	(0.0513)	(0.0465)
Observations	164,353	164,074	120,142	86,169	148,294
Candidates	12,025	12,025	8,789	6,310	10,847

Notes: the table reports, by years since/to election, the average effects of winning a mayoral election on annual log(labor and social security earnings + compensations), excluding observations with no earnings or compensations. Earnings and compensations are measured in 1,000€ at 2010 prices. Unless otherwise noted in the column heading: i) estimates are based upon local linear regressions; ii) the dependent variable is measured in logs; iii) additional controls include calendar year dummies, years to/from election dummies, and years to/from election-specific linear splines in the margin of victory; iv) we use a bandwidth of 0.1 and a rectangular kernel. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 5. Heterogeneous effects of winning a mayoral election on office-related compensations and on labor and social security earnings, by year to/since election and area.

Dependent variable	(1) Log (labor and social security earnings)	(2) Labor and social security earnings levels (€)	(3) Compensation levels (€)
Years to/since election			
-5 to -1	-0.0343	-825.0	-58.5
	(0.0372)	(1,578.1)	(180.2)
0 to 4	-0.1661***	-3,154.0**	16,641.2***
	(0.0328)	(1,474.7)	(285.1)
5 to 9	-0.1295***	-2,598.7*	3,934.5***
	(0.0343)	(1,576.0)	(382.7)
10 to 14	-0.0650*	-188.6	-1,169.8***
	(0.0385)	(1,767.9)	(270.8)
Observations	156,346	156,346	180,192
Candidates	12025	12025	12025

Notes: estimates based on the specification in Table 3, Column (1). Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 6. The effects of winning a mayoral election at time t=0 on the probability of holding political offices, by years since/to election.

Dependent variable	(1) City council member	(2) City council executive body member	(3) Mayor	(4) Province council member	(5) Province council executive body member	(6) Region council member	(7) Region council executive body member	(8) Member of the Italian parliament (until 2007)
Years to/since election								
-5 to -1	-0.0352*	-0.0200	-0.0026	0.0040	0.0001	0.0046**	0.0011	-0.0027
	(0.0203)	(0.0163)	(0.0026)	(0.0065)	(0.0027)	(0.0020)	(0.0009)	(0.0021)
0 to 4	0.0259***	0.8873***	0.8941***	0.0012	-0.0004	-0.0038***	-0.0004	-0.0035**
	(0.0082)	(0.0064)	(0.0062)	(0.0047)	(0.0017)	(0.0014)	(0.0006)	(0.0018)
5 to 9	0.1870***	0.1703***	0.2213***	0.0090	0.0009	-0.0003	-0.0004	-0.0015
	(0.0184)	(0.0172)	(0.0162)	(0.0058)	(0.0025)	(0.0023)	(0.0011)	(0.0032)
10 to 14	0.0587***	-0.0214	-0.0647***	0.0153**	0.0025	0.0049*	-0.0005	-0.0061
	(0.0197)	(0.0154)	(0.0115)	(0.0068)	(0.0027)	(0.0030)	(0.0012)	(0.0063)
Observations	180,192	180,192	180,192	180,192	180,192	180,192	180,192	97,803
Candidates	12,025	12,025	12,025	12,025	12,025	12,025	12,025	10,415

Notes: estimates based on the specification in Table 3, Column 1. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 7. The effects of winning a mayoral election on labor and social security earnings and on office-related compensations, by year to/since election, with and without correcting the earnings of initial runners-up who win a later election for the impact of later wins.

Dependent variable	(1) log(labor and social security earnings + compensations)		(2) log(labor and social security earnings)		(3) Compensations (€)	
Correction	No	Yes	No	Yes	No	Yes
Years to/since election						
5 to 9	-0.0171	0.0352	-0.1295***	-0.1610***	3,934.5***	7,044.4***
	(0.0306)	(0.0303)	(0.0343)	(0.0342)	(382.7)	(361.9)
10 to 14	-0.1038***	-0.0921**	-0.0650*	-0.0971**	-1,169.8***	482.2*
	(0.0370)	(0.0370)	(0.0385)	(0.0385)	(270.8)	(267.8)

Notes: each cell is from a different regression, following the specification in Table 3, Column (1). The estimates for periods 5 to 9 are obtained after subtracting the estimated effect of a win 0 to 4 years after the first election from the earnings of runners-up who win an election during this period. Similarly, the estimates for periods 10 to 14 are obtained after subtracting the estimated effect of a win 0 to 4 years after the first election from the earnings of runners-up who win an election during this period, and after subtracting the estimated effect of a win 5 to 9 years after the first election from the earnings of runners-up who win an election during the period 5 to 9. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$. The number of candidates is 12,025. The total number of observations is 47,331 for the period 5 to 9 and 34,056 for the period 10 to 14.

Table 8. The effects of winning a mayoral election at time t=0 on annual log(labor and social security earnings), by years to/since election and previous sector of employment

Dep. var.: log(labor and social security earnings)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Previous sector of employment	Private employees	Public employees	Self-employed professionals	Other self-employed	Fixed-term collaborators	Retirees	Not employed
Years to/since election							
-5 to -1	-0.0710	0.0487	-0.0130	-0.0649	-0.1203	0.0196	-
	(0.0705)	(0.0599)	(0.1020)	(0.0887)	(0.1942)	(0.0858)	
0 to 4	-0.1834**	-0.0464	-0.3339***	-0.1565*	-0.1636	0.0330	-0.3504
	(0.0801)	(0.0630)	(0.1061)	(0.0912)	(0.1710)	(0.0765)	(0.2170)
5 to 9	-0.1054	-0.0458	-0.1722	-0.2127*	-0.1277	-0.0600	-0.2783*
	(0.0821)	(0.0707)	(0.1306)	(0.1088)	(0.1830)	(0.0819)	(0.1537)
10 to 14	-0.0729	-0.0666	-0.1447	-0.2803*	0.2457	-0.0872	-0.0113
	(0.1013)	(0.0937)	(0.1615)	(0.1456)	(0.2404)	(0.1046)	(0.1485)
Observations	27,377	26,767	18,151	13,271	8,811	19,610	6,260
Candidates	1929	1888	1375	977	741	1322	863

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 9. The effects of winning a mayoral election on labor and social security earnings and compensations, for the full sample and by macro area, by year to/since election and area.

Dep. var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)			
	log(labor and social security earnings + compensations)			Compensation levels (€)			Labor and social security earnings levels (€)			log(labor and social security earnings)		
Sample:	Full sample	North	South	Full sample	North	South	Full sample	North	South	Full sample	North	South
Years to/ since election												
-5 to -1	-0.0484	-0.0741*	-0.0015	-58.5	-29.3	-104.9	-825.0	-1,034.4	-779.1	-0.0343	-0.0579	0.0095
	(0.0370)	(0.0433)	(0.0692)	(180.2)	(220.9)	(311.2)	(1,578.1)	(1,981.1)	(2,490.5)	(0.0372)	(0.0434)	(0.0704)
0 to 4	0.2670***	0.2497***	0.3083***	16,641.2***	16,695.3***	16,521.9***	-3,154.0**	-2,860.7	-3,957.2*	-0.1661***	-0.1537***	-0.2035***
	(0.0266)	(0.0316)	(0.0479)	(285.1)	(344.9)	(505.9)	(1,474.7)	(1,905.2)	(2,040.6)	(0.0328)	(0.0383)	(0.0621)
5 to 9	-0.0171	-0.0090	-0.0427	3,934.5***	5,035.4***	1,541.7**	-2,598.7*	-2,582.7	-2,661.3	-0.1295***	-0.1273***	-0.1373**
	(0.0306)	(0.0368)	(0.0538)	(382.7)	(470.4)	(643.7)	(1,576.0)	(2,016.3)	(2,318.0)	(0.0343)	(0.0410)	(0.0615)
10 to 14	-0.1038***	-0.0814*	-0.1591**	-1,169.8***	-1,460.8***	-578.7	-188.6	673.7	-2,237.9	-0.0650*	-0.0361	-0.1365**
	(0.0370)	(0.0441)	(0.0668)	(270.8)	(310.9)	(524.4)	(1,767.9)	(2,265.5)	(2,649.2)	(0.0385)	(0.0460)	(0.0691)
Observations	164,353	113,080	51,273	180,192	121,675	58,517	156,346	108,468	47,878	156,346	108,468	47,878
Candidates	12,025	8076	3949	12025	8076	3949	12025	8076	3949	12025	8076	3949

Notes: estimates based on the specification in Table 3, Column (1). Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table 10. The effects of winning a mayoral election on careers in politics, by electoral term and macroarea.

Dependent variable	(1) City council member	(2) City council executive body member	(3) Mayor	(4) Province councilor	(5) Province council executive body member	(6) Region councilor	(7) Region council executive body member	(8) Member of the Italian parliament (until 2007)
Years to/since election								
Panel a. Northern Italy								
-5 to -1	-0.0407	-0.0208		0.0003	0.0100	0.0011	0.0053**	-0.0039*
0 to 4	0.0324***	0.9062***	0.9113***	-0.0024	0.0016	-0.0007	-0.0024*	-0.0029
5 to 9	0.2287***	0.2272***	0.2833***	-0.0002	0.0077	-0.0000	0.0012	0.0009
10 to 14	0.0889***	-0.0088	-0.0644***	-0.0022	0.0089	0.0015**	0.0045	-0.0045
Panel b. Southern Italy								
-5 to -1	-0.0293	-0.0205		-0.0079	-0.0002	0.0034	0.0009	-0.0002
0 to 4	0.0115	0.8472***	0.8572***	0.0008	0.0041	-0.0065**	0.0003	-0.0048*
5 to 9	0.0958***	0.0465	0.0872***	0.0119	0.0033	-0.0034	-0.0012	-0.0062
10 to 14	-0.0041	-0.0481*	-0.0654***	0.0277**	0.0120**	0.0058	-0.0048	-0.0099

Notes: estimates based on the specification in Table 3, Column (1). The total number of observations is 58,517 for the South and 121,675 for the North. Standard errors not reported to save space. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table 11. The effects of winning a mayoral election annual log (labor and social security earnings) within Southern Italy, by years to/since election and municipality characteristics.

	(1)	(2)	(3)	(4)
	Illegal employment share		Unemployment rate	
Dep. var.: log(labor and social security earnings)	Below median	Above median	Below median	Above median
Years to/since election				
-5 to -1	0.0082	0.0263	0.0901	-0.0791
	(0.0809)	(0.1387)	(0.0943)	(0.1061)
0 to 4	-0.2232***	-0.1805	-0.1026	-0.3263***
	(0.0751)	(0.1105)	(0.0849)	(0.0913)
5 to 9	-0.1321*	-0.1462	-0.0650	-0.2084**
	(0.0740)	(0.1098)	(0.0847)	(0.0894)
10 to 14	-0.1471*	-0.1225	-0.0697	-0.2022**
	(0.0848)	(0.1186)	(0.0988)	(0.0961)
Observations	31,188	16,591	24,572	23,207
Candidates	2525	1415	1979	1961

Notes: estimates based on the specification in Table 3, Column (1). Sample limited to municipalities in Southern Italy. The illegal employment share by province is for 2003. The unemployment rate by province is for 2005. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Appendix – for online publication only

A. Appendix Figures and Tables

Figure A1. Distribution of years since the first election
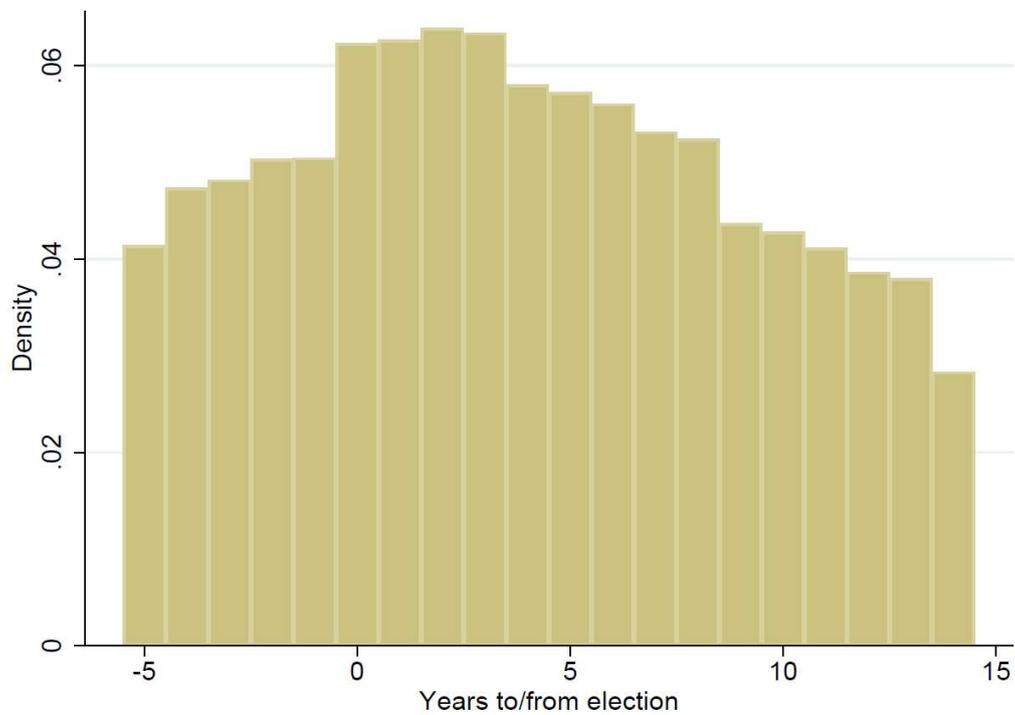
Notes: the sample includes 180,192 candidate-year observations for 12,025 candidates.

Table A1. Sample selection: descriptive statistics for the full sample and for candidates included in the INPS server

	Full sample	Included candidates
Variable		
Female	0.1282	0.1158
Has tertiary education	0.4446	0.4756
Year of birth	1955.7	1955.2
Municipality in Southern in Italy	0.2854	0.3449
Municipal population	7635.2	6253.9
Observations	42,785	14,580

Notes: time varying variables refer to the first election observed in the data for each candidate.

Table A2. Robustness: stability of the baseline estimates by election year.

Dep. var.: log (labor and social security earnings + compensations)	(1)	(2)	(3)	(4)	(5)	(6)
Election years	1993/2017	1996/2007	1996/2012	1993/2007	1996/2017	1993/2012
Years to/since election						
-5 to -1	-0.0484	-0.0502	-0.0367		-0.0370	
	(0.0370)	(0.0495)	(0.0411)		(0.0370)	
0 to 4	0.2670***	0.3597***	0.3375***	0.2293***	0.3482***	0.2497***
	(0.0266)	(0.0391)	(0.0323)	(0.0331)	(0.0298)	(0.0282)
5 to 9	-0.0171	0.0416	-0.0067	0.0114		-0.0175
	(0.0306)	(0.0420)	(0.0358)	(0.0341)		(0.0305)
10 to 14	-0.1038***	-0.1162**		-0.1042***		
	(0.0370)	(0.0478)		(0.0370)		
Observations	164,353	83,510	94,807	96,341	77,547	97,740
Candidates	12025	5026	7460	7940	8976	10301

Notes: estimates are based on the specification in Table 3, Column (1). Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table A3. Robustness: including political compensation at regional level, but excluding not Basilicata, Molise and Umbria, for which the data is not available.

Dep. Var.:	(1) Log (labor and social security earnings + compensations)	(2) Log (labor and social security earnings)	(3) Labor and social security earnings levels (€)	(4) Compensation levels (€)
Years to/since election				
-5 to -1	-0.0445	-0.0291	-929.2210	578.6150
	(0.0389)	(0.0395)	(1,683.9113)	(411.1997)
0 to 4	0.2474***	-0.1647***	-3,163.1241**	16,271.3397***
	(0.0279)	(0.0343)	(1,589.8885)	(364.3454)
5 to 9	-0.0263	-0.1306***	-2,869.9249*	4,253.3101***
	(0.0324)	(0.0362)	(1,684.6718)	(545.2514)
10 to 14	-0.0878**	-0.0598	-62.9337	-539.4361
	(0.0390)	(0.0404)	(1,900.7416)	(488.0651)
Observations	147,362	139,938	139,938	161,145
Candidates	10748	10748	10748	10748

Notes: the Table replicates the specifications in column (1) of Table 3 and columns (1)-(3) of Table 5. Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table A4. Robustness: handling missing earnings data.

Specification/dependent variable	(1) Pr(Any labor and social security earnings + compensations)	(2) log(labor and social security earnings + compensations), missing values = 0	(3) Labor and social security earnings + compensations levels (€), missing values = 0	(4) Inverse hyperbolic sine of labor and social security earnings + compensations, missing values = 0	(5) Unconditional median regression. log(labor and social security earnings + compensations), missing values = 0
Years to/since election					
-5 to -1	-0.0103	-0.1483	-1,319.2	-0.1557	-0.0521
	(0.0125)	(0.1362)	(1,456.2)	(0.1446)	(0.0356)
0 to 4	0.1041***	1.3347***	12,088.8***	1.4070***	0.3387***
	(0.0078)	(0.0860)	(1,359.8)	(0.0912)	(0.0248)
5 to 9	-0.0029	-0.0457	62.1	-0.0477	0.0043
	(0.0093)	(0.1034)	(1,504.3)	(0.1096)	(0.0303)
10 o 14	-0.0218**	-0.3229***	-2,051.1	-0.3380***	-0.1126***
	(0.0104)	(0.1172)	(1,698.2)	(0.1241)	(0.0363)
Observations	180,192	180,192	180,192	180,192	180,192
Candidates	12025	12025	12025	12025	12025

Notes: estimates in columns (1)-(4) are based on the specification in Table 3, column 1. Estimates in column (5) replicate the specification in Table 3, column (4). Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table A5. The effects of winning a mayoral election on annual log(labor and social security earnings), by electoral term and municipality characteristics

Dep. var.: log(labor and social security earnings)	(1)	(2)	(3)	(4)
Illegal employment share	Below median	Above median	Below median	Above median
Unemployment rate	Below median	Below median	Above median	Above median
Years to/since election				
-5 to -1	0.0453	0.3733	-0.0660	-0.0962
	(0.1016)	(0.2565)	(0.1334)	(0.1650)
0 to 4	-0.1396	0.1005	-0.3959***	-0.2825**
	(0.0908)	(0.2323)	(0.1327)	(0.1252)
5 to 9	-0.0976	0.0982	-0.1912	-0.2319*
	(0.0909)	(0.2250)	(0.1257)	(0.1257)
10 to 14	-0.1027	0.0824	-0.2199	-0.1946
	(0.1056)	(0.2531)	(0.1398)	(0.1313)
Observations	20,126	4,446	11,062	12,145
Candidates	1605	374	920	1041

Notes: estimates based on the specification in Table 3, Column (1). Sample limited to municipalities in Southern Italy. The illegal employment share by province estimated by the Italian National Statistical Office for 2003. The unemployment rate by province is for 2005. Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

B. The effects of winning a mayoral election on current sector of employment, by electoral term and sector of employment before the election.

Table B1. Previous sector: private employees

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	-0.1196***	0.0051	-0.0136*	0.0044	0.0025	0.0319	0.0893***
	(0.0345)	(0.0076)	(0.0073)	(0.0083)	(0.0124)	(0.0242)	(0.0228)
5 to 9	-0.0670	0.0192	-0.0127	-0.0023	0.0045	0.0014	0.0568**
	(0.0488)	(0.0146)	(0.0129)	(0.0152)	(0.0221)	(0.0415)	(0.0232)
10 to 14	-0.0837	0.0197	-0.0383**	0.0082	0.0289	0.0566	0.0085
	(0.0613)	(0.0196)	(0.0177)	(0.0195)	(0.0271)	(0.0592)	(0.0285)
Observations	20,444	20,444	20,444	20,444	20,444	20,444	20,444
Candidates	1929	1929	1929	1929	1929	1929	1929

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table B2. Previous sector: public employees

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	-0.0063	-0.0102	-0.0036	-0.0024	0.0092	0.0055	0.0078
	(0.0055)	(0.0262)	(0.0067)	(0.0033)	(0.0079)	(0.0139)	(0.0187)
5 to 9	-0.0022	0.0222	0.0160	-0.0049	0.0311***	-0.0538**	-0.0085
	(0.0093)	(0.0367)	(0.0110)	(0.0051)	(0.0114)	(0.0259)	(0.0233)
10 to 14	0.0031	0.0478	-0.0123	-0.0079	0.0392**	-0.0608	-0.0091
	(0.0111)	(0.0525)	(0.0135)	(0.0068)	(0.0178)	(0.0414)	(0.0296)
Observations	19,792	19,792	19,792	19,792	19,792	19,792	19,792
Candidates	1888	1888	1888	1888	1888	1888	1888

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table B3. Previous sector: professional self-employed

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	-0.0011	-0.0024	-0.0588**	0.0082	-0.0014	-0.0003	0.0559**
	(0.0030)	(0.0090)	(0.0290)	(0.0059)	(0.0065)	(0.0134)	(0.0237)
5 to 9	-0.0032	-0.0004	-0.0180	0.0065	-0.0058	-0.0536**	0.0746**
	(0.0088)	(0.0160)	(0.0453)	(0.0053)	(0.0157)	(0.0234)	(0.0334)
10 to 14	-0.0066	0.0001	-0.0165	0.0033	-0.0099	-0.0549	0.0846*
	(0.0108)	(0.0277)	(0.0650)	(0.0087)	(0.0142)	(0.0361)	(0.0492)
Observations	13,382	13,382	13,382	13,382	13,382	13,382	13,382
Candidates	1375	1375	1375	1375	1375	1375	1375

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table B4. Previous sector: other self-employed

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0/4	-0.0211	-0.0050	-0.0052	0.0251	-0.0076	-0.0099	0.0237
	(0.0193)	(0.0050)	(0.0098)	(0.0428)	(0.0189)	(0.0227)	(0.0245)
5/9	-0.0050	-0.0081	0.0023	0.0632	-0.0092	-0.0732*	0.0300
	(0.0220)	(0.0079)	(0.0122)	(0.0604)	(0.0308)	(0.0383)	(0.0375)
10/14	-0.0142	-0.0119	0.0084	0.1045	-0.0043	-0.0446	-0.0379
	(0.0269)	(0.0094)	(0.0259)	(0.0870)	(0.0457)	(0.0678)	(0.0536)
Observations	9,814	9,814	9,814	9,814	9,814	9,814	9,814
Candidates	977	977	977	977	977	977	977

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.01$.

Table B5. Previous sector: fixed-term collaborators

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	0.0020	-0.0058	-0.0817**	0.0026	0.0473	0.0216	0.0140
	(0.0260)	(0.0289)	(0.0340)	(0.0268)	(0.0612)	(0.0352)	(0.0512)
5 to 9	-0.0098	-0.0136	-0.0973**	0.0551	-0.0263	0.0029	0.0890
	(0.0340)	(0.0335)	(0.0447)	(0.0422)	(0.0700)	(0.0647)	(0.0638)
10 to 14	-0.0174	-0.0382	-0.0948	0.1017*	-0.0207	0.0226	0.0468
	(0.0478)	(0.0458)	(0.0626)	(0.0600)	(0.0886)	(0.0982)	(0.0747)
Observations	7,494	7,494	7,494	7,494	7,494	7,494	7,494
Candidates	741	741	741	741	741	741	741

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table B6. Previous sector: retirees

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	0.0008	0.0002	0.0005	0.0024	0.0046	-0.0155	0.0070
	(0.0006)	(0.0002)	(0.0050)	(0.0050)	(0.0083)	(0.0128)	(0.0068)
5 to 9	0.0051	0.0057	0.0005	-0.0058	-0.0078	0.0052	-0.0029
	(0.0034)	(0.0044)	(0.0082)	(0.0048)	(0.0181)	(0.0228)	(0.0070)
10 to 14	-0.0110	0.0000	-0.0090	-0.0002	-0.0062	0.0315	-0.0051
	(0.0112)	(0.0001)	(0.0055)	(0.0043)	(0.0204)	(0.0247)	(0.0063)
Observations	13,924	13,924	13,924	13,924	13,924	13,924	13,924
Candidates	1322	1322	1322	1322	1322	1322	1322

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

Table B7. Previous sector: not employed

Current sector of employment	(1) Private employees	(2) Public employees	(3) Self-employed professionals	(4) Other self-employed	(5) Fixed-term collaborators	(6) Retirees	(7) Not employed
Years to/since election							
0 to 4	-0.0335	-0.0062	-0.0093	-0.0224	-0.0421	0.1126***	0.0009
	(0.0229)	(0.0183)	(0.0255)	(0.0212)	(0.0269)	(0.0397)	(0.0505)
5 to 9	-0.0225	-0.0040	-0.0563	0.0009	-0.0362	0.0902	0.0279
	(0.0320)	(0.0242)	(0.0364)	(0.0263)	(0.0358)	(0.0606)	(0.0571)
10 to 14	-0.0097	-0.0006	-0.0309	-0.0117	-0.0116	0.0098	0.0548
	(0.0398)	(0.0313)	(0.0467)	(0.0300)	(0.0327)	(0.0755)	(0.0549)
Observations	10,806	10,806	10,806	10,806	10,806	10,806	10,806
Candidates	863	863	863	863	863	863	863

Notes: estimates based on the specification in Table 3, Column (1). The sample is limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: $p<0.01$; **: $p<0.05$; *: $p<0.10$.

C. Other heterogeneous effects

We report other heterogeneous effects in Tables C1 and C2. For these estimates, as in Table 8, the sample is limited to elections held from 1996 onwards to have some pre-election information for all candidates. Columns (1) and (2) of Table C1 show that during the first term males gain more than females from winning an election, and the same columns in Table C2 suggest that this likely happens because women experience bigger labor and social security earnings penalties.

Columns (3) and (4) distinguish instead between candidates with and without a tertiary education degree. The effect on log total earnings during the election is larger in percentage terms for the low educated, most likely because for this group comparable effects on compensation levels are added to lower labor and social security earnings. On the other hand, the negative long-term effect on labor and social security earnings is larger in magnitude and more persistent for candidates without a tertiary education degree. This pattern suggests that the highly educated may find it easier to return to work after political service.

Columns (5) and (6) of Tables C1 and C2 show that the negative effects of the treatment on labor and social security earnings are larger and more persistent for younger candidates, reflecting the higher share of retirees (with stable earnings) among senior candidates. Columns (7) and (8) of Table C1 indicate that the effects on log total earnings are initially larger for candidates with lower than median levels of total earnings before the election, a result due to differences in the levels of labor and social security earnings for constant political compensations across the two groups. Instead, Table C2 suggests that the low-earners lose more in terms of labor and social security earnings in the long run – suggesting that high-earners are better at finding earnings opportunities after the first mandate is over. These results mirror the ones we obtain from unconditional quantile regressions (not reported for brevity), showing that the positive effects on total earnings are smaller in percentage terms at the top of the earnings distribution, while the negative ones on labor and social security earnings are concentrated at the bottom of the earnings distribution.

Finally, columns (9) and (10) distinguish between municipalities with more or less than 5000 inhabitants. Since 2001, the former group is subject to stricter fiscal rules (see Grembi et al., 2016). Therefore, mayors in these municipalities have less room than elsewhere to use discretion and extract rents. In addition, the 5000-inhabitant threshold also determines a change in the compensation received by mayors for political service. This higher salary is expected to

compensate mayors for the heavier burden of responsibilities and duties connected with the government of a larger municipality (see Gagliarducci and Nannicini, 2013). Results in Table 12 display larger positive effects on total earnings during the first electoral term in small municipalities, and more negative long-run effects in large municipalities. Table 13 highlights that the negative effects in larger municipalities are due to labor and social security earnings, suggesting that the busier mayors of large municipalities face both better outside options and more difficulties to re-connect with their previous job after the political service.

Table C1. Heterogeneous effects of winning a mayoral election on annual log(labor and social security earnings + compensations) by years to / since election.

Dep. var.: log(labor and social security earnings + compensations)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Candidate's gender		Candidate's education		Candidate's age at election		Candidate's pre-election earnings including compensations		Municipality's population	
					Below median	Above median	Below median	Above median	Below 5000	Above 5000
	Male	Female	Secondary or lower	Tertiary						
Years to/since election										
-5 to -1	-0.0340	-0.1360*	-0.0360	-0.0580	-0.0392	-0.0449	-0.0353	-0.0033	-0.0264	-0.1657
	(0.0338)	(0.0812)	(0.0379)	(0.0554)	(0.0404)	(0.0487)	(0.0460)	(0.0350)	(0.0316)	(0.1079)
0 to 4	0.3415***	0.2003***	0.3888***	0.2013***	0.2679***	0.3613***	0.6277***	0.2551***	0.3102***	0.2181**
	(0.0308)	(0.0693)	(0.0343)	(0.0497)	(0.0347)	(0.0455)	(0.0354)	(0.0327)	(0.0296)	(0.0847)
5 to 9	0.0143	-0.0382	0.0104	-0.0215	-0.0033	0.0158	0.0314	-0.0008	0.0146	-0.1667
	(0.0385)	(0.0953)	(0.0418)	(0.0651)	(0.0446)	(0.0587)	(0.0441)	(0.0437)	(0.0370)	(0.1208)
10 to 14	-0.1259***	-0.1220	-0.1718***	-0.0148	-0.1245**	-0.0869	-0.1252**	-0.0182	-0.0852*	-0.2825*
	(0.0474)	(0.1089)	(0.0513)	(0.0847)	(0.0565)	(0.0670)	(0.0536)	(0.0576)	(0.0441)	(0.1713)
Observations	111,226	15,018	68,716	54,186	67,662	58,573	51,345	61,427	111,149	15,095
Candidates	7919	1178	4724	4083	4811	4284	3976	4132	7987	1110

Notes: estimates based on the specification in Table 3, Column (1). Sample limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.10.

Table C2. Heterogeneous effects of winning a mayoral election on annual log(labor and social security earnings + compensations) by years to / since election.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. var.: log(labor and social security earnings)	Candidate's gender		Candidate's Education		Candidate's age at election		Candidate's pre-election earnings, including compensations		Municipality's population	
					Below median	Above median	Below median	Above median	Below 5000	Above 5000
	Male	Female	Secondary or lower	Tertiary						
Years to/since election										
-5 to -1	-0.0229	-0.1376	-0.0145	-0.0354	-0.0394	-0.0548	0.0008	-0.0030	-0.0260	-0.0973
	(0.0340)	(0.0845)	(0.0380)	(0.0555)	(0.0407)	(0.0481)	(0.0490)	(0.0353)	(0.0320)	(0.1144)
0 to 4	-0.0679**	-0.2177**	-0.1043***	-0.1064*	-0.1478***	-0.0890*	-0.1250**	-0.1150***	-0.0861***	-0.2863**
	(0.0346)	(0.0888)	(0.0388)	(0.0569)	(0.0436)	(0.0458)	(0.0487)	(0.0394)	(0.0325)	(0.1258)
5 to 9	-0.0902**	-0.2139**	-0.1496***	-0.0769	-0.1450***	-0.0828	-0.0932*	-0.0790*	-0.0844**	-0.3547**
	(0.0408)	(0.0955)	(0.0424)	(0.0718)	(0.0524)	(0.0536)	(0.0485)	(0.0480)	(0.0376)	(0.1510)
10to 14	-0.0884*	-0.1676	-0.1647***	0.0667	-0.0978	-0.0651	-0.0717	0.0015	-0.0501	-0.2501
	(0.0482)	(0.1078)	(0.0522)	(0.0893)	(0.0610)	(0.0631)	(0.0544)	(0.0622)	(0.0446)	(0.1817)
Observations	106,209	14,047	66,108	50,935	63,923	56,324	46,779	60,671	106,063	14,193
Candidates	7919	1178	4724	4083	4811	4284	3976	4132	7987	1110

Notes: estimates based on the specification in Table 3, Column 1. Sample limited to elections held from 1996 onwards to observe at least one pre-election year for all candidates. Standard errors clustered by candidate are reported in parenthesis. ***: p<0.01; **: p<0.05; *: p<0.010.

D. Institutional details

a. Mayor election rules

There are over 8,000 municipalities in Italy, and municipal administrations are responsible of many public services.[19] Within the administration, Municipal Councils (*Consiglio Comunale*) are endowed with legislative powers, while executive authority is assigned to a Mayor (*Sindaco*) who heads an Executive Committee (*Giunta Comunale*).

The system currently regulating municipal elections in Italy has been introduced in 1993 (DL 25 March 1993, no. 81), and establishes the direct election of the mayor and the adoption of a single winner plurality vote rule, with some differences according to the size of the city. For municipalities with a population of fewer than 15,000 inhabitants, elections are held with a single ballot and the winning candidate is awarded a majority premium of at least two-thirds of the seats in the Council. For cities with a population above 15,000, elections are held using a dual ballot (where the second is held only if none of the candidates obtains an absolute majority of votes in the first ballot). In these larger municipalities, only the two leading candidates at the first round compete in the second ballot and the winning candidate is awarded a majority premium of at least 60 percent of the seats in the Council.[20]

Municipal elections are held every 5 years and municipal governments cannot choose the election schedule.[21] The election of city council members is carried out contextually with the election of the mayor. In municipalities with a population size below 15,000 inhabitants, each mayor candidate is connected to only one list.[22] Conversely, in municipalities with more than 15,000

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[19] These include the management of public utilities (local roads, water, sewage, garbage collection, etc.), the provision of public housing and transportation, nursery schools, and assistance to elderly people. Municipal administrators are also responsible of the municipal budget and define the municipal personal income tax rates (the so-called "addizionale IRPEF") as well as the garbage collection tax (TARI) and the municipal housing tax (IMU).

[20] The average population size of Italian municipalities is 7,018 and only 8% of municipalities have a population greater than 15,000 inhabitants.

[21] In certain circumstances, the legislature may not survive until the end of its legislative term, e.g. because of a mayor's early resignation. In these cases, elections are held before the natural schedule, and, consequently, all subsequent elections are held at different times from other municipalities that have completed the foreseen legislative term.

[22] Each electoral list must be composed by several candidates not higher than the number of councilors to be elected by law and not less than 3/4 of the total. Voters can express only one preference for the councilors of the list connected to the chosen mayor candidate by writing their last name under the list. Therefore, when a candidate is elected, the list of councilors connected to him/her is chosen to grant the majority premium of at least two-thirds of the seats in the council according to preferences expressed by voters.

inhabitants, each candidate can be connected to one or more lists and electors can express two choices: one for the mayor and one for the list of council members.[23]

Municipalities have a registry of eligible voters, which is revised whenever there is an election and all citizens aged 18 or above on the election date are automatically registered to vote. At the local level, population thresholds establish several institutional features (such as council and executive committee size, electoral rules, etc.) and a vast array of national policies (for instance, those concerning public transfers). Among these policies there is one defining the mayor's wage that sharply changes in the proximity of some population thresholds. Since 1993, mayors have been subject to a two-term limit, while the members of the *Giunta*, the executive body of the municipal council, and of the city council, endowed with a monitoring role and some residual powers, especially regarding budgetary controls, can be re-elected indefinitely.

b. Office-related compensation

Local public administration in Italy is organized in three hierarchical layers: regions, provinces, and municipalities. The compensation of mayors, council members, and presidents of the municipal council during the period of interest (1995-2017) is regulated by two national laws, Law 816 of 1985 and Decree 119 of 2000. These laws establish the maximum allowance. Although local municipalities can set lower allowances, this information is not available to us. We report the relevant data in Table D1.

The annual compensation of mayors depends on the municipality size. For example, during the period 2006-2017 it ranged between 13,944€ in small municipalities with less than 1,000 inhabitants and 84,224€ in municipalities with more than 500,000 inhabitants. Compensation is increased if the municipality is the provincial capital, and mayors are also entitled to receive a severance indemnity at the end of service, which corresponds to one month of pay for every 12 months of service.

The compensation received by members of the *Giunta* is determined as a fraction of the mayor's compensation. Council members instead only get a small fee for participating in the council meetings. An important feature of Decree 119 of 2000 is the introduction of the rule establishing that private and public sector employees who do not take leave from work to operate as mayors or

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[23] In these cities, it is possible to choose a different list from that in which the candidate to whom the elector has expressed his vote is grouped (*separate vote*).

members of the *Giunta* are entitled to 50 percent of the compensation reported by the law. Only those taking leave are entitled to full compensation. Before 2000, workers who took leave from work were entitled to twice the amount set by law, but only in municipalities with more than 10,000 inhabitants for mayors and above 50,000 for members of the *Giunta*.

The (maximum) compensation for provincial presidents, presidents of the provincial council and provincial council member is regulated by the same laws that discipline the compensation of mayors. Starting from 2014 (with Law 56/2014), however, provincial offices are filled by mayors only. Therefore, all compensations for provincial offices are set to zero for this period. Table D2 shows the distribution of allowances by province size and over time. The compensation for positions in regional councils is regulated by regional laws, with important variability both across regions and over time. In some cases, members of the regional council are also entitled to a refund of the expenses borne for the fulfilment of their mandate (travel tickets, secretary services, …). Refunds could be determined ex-post based on actual expenses or could be a lump-sum amount. Again, these choices are region-specific and time-varying. Because of this, the data is too cumbersome to be reported. However, it is available from the authors upon request.

Table D1. Appointments in mayoral councils.

a.1995/1999

Position	Population size	Standard compensation (€)	Compensation for province capital (€)	Workers on leave receive double amounts
Mayor	≤3000	5999.16		No
	3001-5000	8998.75		No
	5001-10000	11998.33		No
	10001-30000	11998.33	16497.70	Yes
	30001-50000	13498.12	16497.70	Yes
	50001-100000	16497.70	19497.28	Yes
	100001-250000	19497.28	19497.28	Yes
	250001-500000	22496.86	29995.82	Yes
	>500000	29995.82	29995.82	Yes
Members of the Giunta				
Senior member	5001-10000	5999.16		No
	10001-30000	6599.08	9073.73	No
	30001-50000	7423.96	9073.73	No
	50001-100000	12373.27	14622.96	Yes
	100001-250000	14622.96	14622.96	Yes
	250001-500000	16872.65	22496.86	Yes
	>500000	22496.86	22496.86	Yes
Standard member	5001-10000	5399.25		No
	10001-30000	5399.25	7423.96	No
	30001-50000	6074.15	7423.96	No
	50001-100000	9898.62	11698.37	Yes
	100001-250000	11698.37	11698.37	Yes
	250001-500000	14622.96	19497.28	Yes
	>500000	19497.28	19497.28	Yes

b. 2000-2017.

Position	Population size	Standard compensation (€)	Compensation, province capital (€)	Severance pay (€)	Severance pay, province capital (€)
Mayor	≤1000	15493.71		1291.14	
	1001-3000	17352.95		1446.08	
	3001-5000	26029.43		2169.12	
	5001-10000	33466.41		2788.87	
	10001-30000	37184.90	49579.86	3098.74	4131.66
	30001-50000	41523.13	49579.86	3460.26	4131.66
	50001-100000	49579.86	60115.58	4131.66	5009.63
	100001-250000	60115.58	69411.81	5009.63	5784.32
	250001-500000	69411.81	93581.99	5784.32	7798.50
	>500000	93581.99	93581.99	7798.50	7798.50
Vice-mayor	≤1000	2324.06			
	1001-3000	3470.59			
	3001-5000	5205.89			
	5001-10000	16733.20			
	10001-30000	20451.69	27268.92		
	30001-50000	22837.72	27268.92		
	50001-100000	37184.90	45086.69		
	100001-250000	45086.69	52058.86		
	250001-500000	52058.86	70186.49		
	>500000	70186.49	70186.49		
Member of the Giunta	≤1000	1549.37			
	1001-3000	2602.94			
	3001-5000	3904.41			
	5001-10000	15059.88			
	10001-30000	16733.20	22310.94		
	30001-50000	18685.41	22310.94		
	50001-100000	29747.92	36069.35		
	100001-250000	36069.35	41647.08		
	250001-500000	45117.67	60828.29		
	>500000	60828.29	60828.29		
Head of the council	≤1000	774.69			
	1001-3000	1735.30			
	3001-5000	2602.94			
	5001-10000	3346.64			
	10001-15000	3718.49			
	15001-30000	16733.20	22310.94		
	30001-50000	18685.41	22310.94		
	50001-100000	29747.92	36069.35		
	100001-250000	36069.35	41647.08		
	250001-500000	45117.67	60828.29		
	>500000	60828.29	60828.29		

Notes: for all positions, compensations for employees not on leave are halved. Compensations are reduced by 10% from 2006 onwards.

Table D2. Appointments in provincial councils.

a. 1995-1999.

Position	Population size	Compensation (€)
Head of the province	≤250000	16497.70
	250001-500000	19497.28
	500001-1000000	22496.86
	>1000000	26996.24
Members of the Giunta		
Senior member	≤250000	12373.27
	250001-500000	14622.96
	500001-1000000	16872.65
	>1000000	20247.18
Standard member	≤250000	10723.50
	250001-500000	12673.23
	500001-1000000	14622.96
	>1000000	17547.55

Notes: for all positions, workers on leave receive double amounts.

b. 2000-2017.

Position	Population size	Compensation (€)	Severance pay (€)
Head of the province	≤250000	49579.86	4.131.66
	250001-500000	60115.58	5.009.63
	500001-1000000	69411.81	5.784.32
	>1000000	83666.02	6.972.17
Vice-head of the province	≤250000	37184.90	
	250001-500000	45086.69	
	500001-1000000	52058.86	
	>1000000	62749.51	
Member of the Giunta	≤250000	32226.91	
	250001-500000	39075.13	
	500001-1000000	45117.67	
	>1000000	54382.91	
President of the council	≤250000	32226.91	
	250001-500000	39075.13	
	500001-1000000	45117.67	
	>1000000	54382.91	

Notes: for all positions, compensations for employees not on leave are halved. Compensations are reduced by 10% from 2006 onwards. Provincial council members do not receive any compensation from July 2014 onwards. Politicians in "metropolitan areas" (Torino, Milano, Venezia, Genova, Bologna, Firenze, Bari, Napoli) are rewarded according to the criteria in place for provinces with population >1000000.