Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection*

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Abstract

The wage paid to politicians affects both the choice of citizens to run for an elective office and the performance of those who are appointed. First, if skilled individuals shy away from politics because of higher opportunities in the private sector, an increase in politicians' pay may change their mind. Second, if the reelection prospects of incumbents depend on their in-office deeds, a higher wage may foster performance. We use data on all Italian municipal governments from 1993 to 2001 and test these hypotheses in a quasi-experimental framework. In Italy, the wage of mayors depends on population size and sharply rises at different thresholds. We apply a regression discontinuity design to the only threshold that uniquely identifies a wage increase: 5,000 inhabitants. Exploiting the existence of a two-term limit, we further disentangle the composition from the incentive component of the effect of the wage on performance. Our results show that a higher wage attracts more educated candidates, and that better paid politicians size down the government machinery by improving efficiency. Importantly, most of this policy effect is driven by the selection of competent politicians, rather than by the incentive to be reelected.

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1 Introduction

Paying politicians is a debated but elusive topic. Firms set the wage of workers to maximize their profits; politicians set the wage of bureaucrats to maximize either social welfare or their own interests. For the same reason, citizens—the principal—should set the optimal compensation of politicians—the agent—according to some welfare criteria. But this is rarely the case, as the public opinion swings from the complaint against the high salaries of the political elite to the acknowledgment that "if you pay peanuts you get monkeys" also in politics. No evidence unambiguously supports either claim.

According to the efficiency wage theory, workers' productivity is increasing in the wage they are paid. There are three main explanations for why this relationship should hold: (1) paying workers more reduces shirking because of the higher cost of being fired (Shapiro and Stiglitz, 1984); (2) it enhances the quality of applicants (Weiss, 1980); and (3) it improves motivation and group work norms (Akerlof, 1982). If we apply these insights to the labor market for politicians, we can conclude that a higher wage is likely to improve the performance of elected officials by increasing the incumbent's payoff from being reelected, by attracting more skilled individuals (that is, citizens with better outside opportunities in the private sector), and by improving the morale of politicians.

The efficiency wage theory, of course, does not consider many aspects that are specific to the political arena, such as party selection, campaigning, non-monetary incentives, and voters' preferences. Yet, various models in political economics build on the efficiency-wage intuitions to predict that a higher salary should increase the average quality of politicians (e.g., see Caselli and Morelli, 2004; Besley, 2004). This prediction, however, is not unanimously shared by the literature. Actually, other contributions suggest that the opposite may be true in all the circumstances in which high-quality citizens have alternative first-order incentives to enter politics, such as post-congressional returns or outside income: in similar cases, a higher remuneration has the indirect effect of making all the other (low-quality) candidates more willing to run (e.g., see Messner and Polborn, 2004; Mattozzi and Merlo, 2008; Gagliarducci et al., 2010). At the end of the day, the question of whether politicians' pay affects their selection and performance remains empirical.

In this paper, we use data on all Italian municipal governments from 1993 to 2001 and evaluate the impact of politicians' remuneration in a quasi-experimental framework. In

Italy, the wage of the mayor increases with the size of the resident population. The same rule applies to the other members of the municipal government, that is, the executive officers appointed by the mayor, although in this case the salary is significantly lower in absolute terms. The quasi-experimental framework arises because the wage does not increase monotonically, but sharply changes at 9 different thresholds. As long as population cannot be manipulated by politicians to sort above these thresholds and be paid more, the institutional setting delivers a clean source of (local) exogenous variation.

Politicians' pay is not the only policy decided by the number of resident inhabitants, however. The size of the municipal council, the electoral rule, and many other policies vary according to population brackets. After comparing different legislative sources and inquiring the Ministry of Internal Affairs, we discovered that only 3 out of the 9 original thresholds uniquely identify a wage increase: 1,000, 5,000, and 50,000 inhabitants. However, we cannot use the 1,000 threshold because of its late introduction (in 2000) and the 50,000 threshold because of sample size limitations. We hence focus on the 5,000 threshold, at which there is a sharp 33% increase in the mayor's gross wage. As of 2000, the wage rises from 2,169 to 2,789 euros per month, while the compensation for executive officers increases from 20% to 50% of the mayor's wage, although it remains as low as 1,394 euros. Compared to the rest of the population, the increase in the mayor's wage corresponds to moving from the 87^{th} percentile in the income distribution of cities below 5,000 (average of 1,444 euros) to the 91^{st} percentile of cities between 5,000 and 20,000 (2,211 euros).[1]

We therefore apply a Regression Discontinuity Design (RDD) at the 5,000 threshold to control for unobservable town characteristics and test whether a higher wage attracts individuals with higher opportunity costs (*effect of the wage on political selection*) and shapes the performance of elected politicians (*effect of the wage on performance*). Although the local effect identified at this threshold may not be easily generalized to higher population levels, cities below 10,000 inhabitants account for about 85% of all Italian municipalities and 35% of the entire population. Moreover, this provides us with a greater sample size than other RDD exercises that exploit policies varying with population size.

[1] Source: Bank of Italy, Survey of Household Income and Wealth (SHIW), wave 2000. Employed individuals between 18 and 65; gross labor income (as employee, self-employed, or entrepreneur) recovered by increasing the disposable income available in the SHIW by the corresponding tax rate, plus an additional 20% to account for under-reporting (see Brandolini, 1999).

As a matter of fact, the wage is not the only source of income for mayors, who can keep their job and cumulate earnings, the only restriction being that if they work as dependent employees they have to ask for a leave-of-absence, otherwise the salary is cut by half. In addition to this, under specific and documented circumstances, the executive committee can grant up to an additional 15% increase to the mayor, conditional on the approval of the Ministry of Internal Affairs. However, we conducted a phone interview survey of mayors in towns between 4,900 and 5,100 and found that the fraction of part-time mayors was almost identical for towns below and above 5,000, and that only very few municipalities introduced a wage raise, so that we can confidently conclude that we are estimating the impact of a 33% wage increase at 5,000.

Because the salary received by the members of the executive committee is relatively small, we mainly focus on mayors. As for selection, the results show that the 33% wage increase at 5,000 attracts more educated mayoral candidates (0.905 years of schooling more), which means an increase in education of about 6.4% (with respect to an average of 14 years of schooling in municipalities between 3,000 and 5,000). There is also evidence that the wage increase attracts more candidates employed in white-collar occupations, such as lawyers and managers. This translates into more educated (0.879 years of schooling more) and high-skilled elected mayors. Interestingly, if we had to apply the same income shift to cities below 5,000, this would correspond to 0.672 years of schooling more.

As for performance, following the literature on political accountability and political budget cycles (e.g., Besley and Case, 1995; List and Sturm, 2006; Akhmedov and Zhuravskaja, 2004; Brender and Drazen (2008); Whalley, 2010), we first look at policies of direct interest to voters, such as taxes, tariffs, and expenditures. We find that better paid politicians reduce the size of the municipal government. In particular, they lower tariffs per capita by about 86% and reduce the amount of investments and current expenditures by about 11% and 22%, respectively. These results are in line with previous empirical studies on the correlation between politicians' remuneration and in-office performance.[2]

[2]Di Tella and Fisman (2004) look at gubernatorial pay in the US from 1950 to 1990 and find that wages respond to changes in state income and taxes per capita. In particular, governors obtain a one percent pay cut for each ten percent increase in per capita taxation, and there is some evidence that this negative tax elasticity is an implicit form of performance pay. Besley (2004) analyzes the same data on US gubernatorial pay. He finds that the congruence between the ideological positions of the governor and citizens—as measured by established surveys—is positively associated with the governor's wage. Keane

The existing literature, however, suffers from a problem of endogeneity of politicians' wage formation, as more skilled politicians or politicians in places more difficult to manage might be granted a higher remuneration. The only exception is represented by Ferraz and Finan (2009), who—independently—presented an empirical exercise similar to ours. They implement an RDD exploiting a Brazilian constitutional amendment that introduced caps on the wages of municipal councillors (*vereadores*) according to population size. They show that a higher wage attracts more candidates and, in particular, more educated ones; they also find that legislative productivity—measured as the number of bills submitted and approved—increases with the salary. After controlling for observable characteristics, they conclude that part of the differential performance can be explained by the incentive to be reelected. Despite the similarity between the two approaches, however, they differ in many respects. First, we implement a sharp (instead of a fuzzy) RDD, because in Italy it is the statutory wage that varies with population. Second, we focus on the mayor as the chief executive of the municipality, and thus we look at budget indicators as performance outcomes. Third—and most importantly—we use the existence of two-term limits for Italian mayors to disentangle the composition and the (reelection) incentive effect of the wage on performance.

Indeed, the effect of the wage on performance might be driven by two distinct components: better paid politicians might act differently because of their higher skills (*composition effect of the wage on performance*) or because of enhanced reelection motives (*incentive effect of the wage on performance*), as those who are paid more should also be more willing to be reappointed by voters and therefore improve their in-office performance. Eventually, the composition effect of the wage on performance should also include a pure motivational incentive *à la* Akerlof (1982), if any. We discuss in Section 3.1 why we expect this effect to be relatively small, and therefore we will simply refer to a composition (or selection) effect.

To disentangle the above two channels, as mentioned, we exploit a feature of the Italian legislation: the existence of a two-term limit. Also mayors with a binding term limit have

and Merlo (2007) estimate a structural dynamic model of congressional careers in the US to evaluate the effect of reducing the relative wage of congressmen. They find that a wage reduction would induce more skilled politicians to exit Congress (where skills refer to the ability to win elections), but this is not true for "achievers," that is, for those who perform better in terms of legislative and policy goals.

a lot of incentives to perform well, including the desire to run for higher offices or to leave a positive legacy, but all of their motivations do not depend on the wage. As a result, mayors just below and just above the 5,000 threshold have identical incentives when they all face a binding term limit; on the contrary, when the term limit is not binding, their incentives diverge because of the different wage they can keep if reelected. We can thus apply a diff-in-diff strategy to the sample of mayors who were elected for two consecutive terms: first, we take the difference between the performance of the mayors in their first term just above and below the threshold, to estimate the overall *effect of the wage on performance* (which reflects the difference in composition and in reelection motives); second, we subtract from the latter the difference between the performance of the same mayors in their second term (when there are no reelection motives) just above and below the threshold, to remove any difference in observable and unobservable selection and therefore estimate the (reelection) *incentive effect of the wage on performance*.

Our results show that most of the effect is driven by the higher competence of elected mayors, rather than by the incentive to be reelected. Alternative explanations for the lack of a (reelection) incentive effect, including strong ideological preferences by voters, do not receive support from the data. This result is particularly interesting, because it undermines the importance of reelection as a disciplining device for incumbent politicians.

We discuss two different interpretations for our policymaking results. First, more skilled politicians may be better at making the government machinery more efficient, as long as they reduce current—instead of capital—expenditure, characterized by sizable passive waste in Italy (Bandiera, Prat, and Valletti, 2009). Second, the reduction in government size may reflect differences in preferences, with more educated mayors having weaker preferences for redistribution and public services (Alesina and Giuliano, 2009). We shed more light on these alternative explanations by looking at two efficiency indicators for the management of the municipal government: the speed of revenues collection (that is, the ratio between collected and assessed revenues within the year) and the speed of payment (that is, the ratio between paid and committed outlays within the year). Our results show that better paid mayors increase the former by 7% and the latter by 2%, and this supports the interpretation that they make the bureaucratic organization more efficient.

The paper proceeds as follows. In Section 2, we formalize our econometric strategy. In Section 3, we describe the institutional framework and the data. In Section 4, we present the estimation results and a number of robustness exercises. We conclude with Section 5.

2 Econometric framework

2.1 Identifying the effect of the wage

In this section, we formalize the evaluation framework that allows us to identify the effect of the wage on both the selection and the performance of politicians. In particular, we test the following hypotheses.

(H1) A higher wage attracts more citizens with high opportunity costs into politics, that is, more skilled individuals with high alternative remunerations in the private sector (*effect of the wage on political selection*).

(H2) A higher wage enhances the performance of elected officials (*effect of the wage on performance*). This may in turn be determined by two channels:

(H2.1) a higher wage attracts more skilled citizens into politics (*composition effect*);

(H2.2) a higher wage increases the cost of not being reelected (*incentive effect*).

To overcome the endogeneity problem of politicians' wage formation, we exploit the Italian policy of paying mayors according to the population size of the municipality. Define X_i as the characteristics of citizens who run for mayor in town i; Y_i as some performance indicator; P_i as the population size; and W_i as the wage paid to the mayor. By law, the wage sharply increases at the population threshold P_c. That is, if $P_i \geq P_c$, then $W_i = W_h$; if $P_i < P_c$, then $W_i = W_\ell < W_h$. To formalize the idea that both the characteristics of politicians and the performance of the mayor depend on the wage, we use a potential outcome framework. Define $X_i(W_k) \equiv X_{ik}$, with $k \in \{\ell, h\}$, as the potential characteristics of politicians in town i if the wage is equal to W_k. Similarly, $Y_i(W_k) \equiv Y_{ik}$, with $k \in \{\ell, h\}$, captures the potential performance of the mayor in town i if the wage is equal to W_k. In the following, we omit the subscript i, for all variables are town-specific.

The estimand of interest is the average treatment effect for a given subpopulation of cities (Ω): $E[X_h - X_\ell | i \in \Omega]$ and $E[Y_h - Y_\ell | i \in \Omega]$. The conditional comparison of X and Y in towns with $W = W_\ell$ against $W = W_h$ does not generally provide an unbiased estimate of the treatment effect, because towns with different unobservable characteristics may endogenously choose the mayor's remuneration. We thus make the following assumptions (see Hahn, Todd, and Van der Klaauw, 2001).

Assumption 1 *$E[X_\ell | P = p]$ and $E[X_h | P = p]$ are continuous in p at P_c.*

Assumption 2 *$E[Y_\ell | P = p]$ and $E[Y_h | P = p]$ are continuous in p at P_c.*

In other words, the potential characteristics of politicians and the potential performance of mayors, which may depend on the population size P, should not display any discontinuity at P_c. Although both assumptions are more than plausible in our setting, two caveats are in order. First, if mayors can manipulate population size and sort above the threshold, treatment assignment is no longer exogenous. Second, if there is another policy that depends on population size and shares the same threshold P_c, the effect of the wage is confounded with the effect of this additional policy and cannot be identified. It is thus important to check whether the data provide evidence of sorting around the threshold, and to be sure that other policies do not vary across the same threshold.

Under Assumption 1, $E[X_\ell | P = P_c] = \lim_{P \uparrow P_c} X$ and $E[X_h | P = P_c] = \lim_{P \downarrow P_c} X$. We can thus identify the effect of the wage on political selection as

$$\tau_{sel} \equiv E[X_h - X_\ell | P = P_c] = \lim_{P \downarrow P_c} X - \lim_{P \uparrow P_c} X. \tag{1}$$

Similarly, under Assumption 2, the effect of the wage on performance is

$$\tau_{per} \equiv E[Y_h - Y_\ell | P = P_c] = \lim_{P \downarrow P_c} Y - \lim_{P \uparrow P_c} Y. \tag{2}$$

2.2 Disentangling incentives from selection

To empirically disentangle (H2.1) and (H2.2) as alternative explanations of the impact of the wage on performance, we need to make further assumptions. Using an additive specification, as it is typical in diff-in-diff, we rewrite potential performance in the following form: $Y_k = S(X_k + v_k) + I_k$, where the function $S(.)$ captures the impact of the observable

characteristics X_k and the unobservable characteristics v_k on performance, and I_k represents the effect of the incentive to be reelected when the wage is W_k.[3] For example, if $v_h > v_\ell$, citizens attracted to politics by W_h are more skilled than citizens attracted to politics by W_ℓ. Based on this formulation, the effect of the wage on performance can be decomposed as $\tau_{per} = \sigma_{per} + \phi_{per}$, where:

$$\sigma_{per} \equiv E[S(X_h + v_h) - S(X_\ell + v_\ell)|P = P_c],$$

$$\phi_{per} \equiv E[I_h - I_\ell|P = P_c].$$

To identify these average treatment effects, we exploit an additional feature of the Italian institutional framework. Because of a term limit, mayors cannot spend more than two consecutive terms in office. We can thus introduce the following assumption.

Assumption 3 *The (reelection) incentive effect of the wage on performance is at work only when the term limit is not binding.*

Note that this assumption does not mean that mayors in the second term have no incentives to perform well. They may still want to do their best because they plan to run for higher offices; because they want to be remembered for their positive legacy; or simply because of intrinsic motivations. The important point is that all of these motivations do not depend on the wage, as reelection in the same town no longer belongs to the mayor's opportunity set. As a result, when the term limit is binding, incentives are identical for mayors just below or just above P_c. On the contrary, when the term limit is not binding, the (reelection) incentive is stronger (or simply different) for mayors just above P_c. Let TL be an index for the term limit, with $TL = 0$ when the term limit constraint is not binding (the mayor is in the first term), and $TL = 1$ when it is binding (the mayor is in the second term). Potential outcomes now depend not only on W, but also on TL, that is, Y_{kj}, with $j \in \{0, 1\}$. Under Assumption 3, they can be summarized as follows.

[3] One might think of alternative specifications for Y, e.g., including an interaction term between $(X_k + v_k)$ and I_k to capture the different outside opportunities of mayors attracted by different wage levels. However, as it will become clear in the rest of this section, we partial out any heterogeneity in the individual outside option when we compute the difference of Y over the terms for the same mayor.

	$W = W_\ell$	$W = W_h$
TL=0	$Y_{\ell 0} = S(X_{\ell 0} + v_{\ell 0}) + I_\ell$	$Y_{h0} = S(X_{h0} + v_{h0}) + I_h$
TL=1	$Y_{\ell 1} = S(X_{\ell 1} + v_{\ell 1}) + exp$	$Y_{h1} = S(X_{h1} + v_{h1}) + exp$

Here, exp stands for administrative experience, which we assume to affect performance independently of the wage schedule.[4] The above table shows that mayors in the first term and in the second term might have different skills. In particular, as long as performance is relevant for reelection, we expect mayors at $TL = 1$ to be more skilled according to both observable and unobservable characteristics. In general: $S(X_{k0} + v_{k0}) \neq S(X_{k1} + v_{k1})$. However, if we restrict the analysis to the sample of politicians who are elected for two consecutive terms, we have that: $S(X_{k0} + v_{k0}) = S(X_{k1} + v_{k1})$.

For two-term mayors, we can identify the overall effect of the wage on performance as:

$$\tau_{per} = E[Y_{h0} - Y_{\ell 0}|P = P_c] = \lim_{P\downarrow P_c|TL=0} Y - \lim_{P\uparrow P_c|TL=0} Y, \tag{3}$$

where the first equality follows from Assumption 3 and the sample restriction to politicians elected for two consecutive terms, while the second equality follows from Assumption 2.

Similarly, we can identify the composition effect and the (reelection) incentive effect of the wage on performance, respectively, as:

$$\sigma_{per} = E[Y_{h1} - Y_{\ell 1}|P = P_c] = \lim_{P\downarrow P_c|TL=1} Y - \lim_{P\uparrow P_c|TL=1} Y, \tag{4}$$

and

$$\begin{aligned} \phi_{per} &= E[(Y_{h0} - Y_{h1}) - (Y_{\ell 0} - Y_{\ell 1})|P = P_c] = \\ &= \left(\lim_{P\downarrow P_c|TL=0} Y - \lim_{P\uparrow P_c|TL=0} Y \right) - \left(\lim_{P\downarrow P_c|TL=1} Y - \lim_{P\uparrow P_c|TL=1} Y \right). \end{aligned} \tag{5}$$

In both equations, the first equality follows from Assumption 3 and the sample restriction to two-term mayors, while the second equality follows from Assumption 2.

Note that, to leave the framework as simple as possible, so far we have not contemplated the pure motivational effect of the wage on performance (Akerlof, 1982). Experimental evidence suggests this effect to be relatively small (see Gneezy and List, 2006). If there

[4] If experience enhanced performance more for high-skilled than for low-skilled mayors (that is, $exp_h > exp_\ell$), we could still identify the overall effect of the wage on performance, but we would overestimate (underestimate) the composition (incentive) component.

were any, the potential performance should be rewritten as: $Y_k = S(X_k + v_k) + I_k + M_k$, where M_k represents the *morale effect* associated with the wage W_k. It is easy to show that, while ϕ_{per} would still identify the incentive effect (M_k would cancel out in equation 5), the same would not be true for σ_{per} in equation (4), as it would contain both the composition and the motivational effect. In this case, σ_{per} should thus be interpreted as a broader complement of the (reelection) incentive effect. Keeping in mind this caveat, for simplicity we will still refer to σ_{per} as the composition (or selection) effect.

3 Italian municipalities

3.1 Institutional framework

The Italian municipal government (*Comune*) is composed by a mayor (*Sindaco*), an executive committee (*Giunta*) appointed by the mayor, and an elected council (*Consiglio Comunale*) that supervises the legislative activity of the mayor and endorses the proposed policies, including the annual budget, with majority rule. Since March 1993, mayors are directly elected by citizens with plurality rule (single round below 15,000 inhabitants and runoff above) and are subject to a two-term limit (unless one of the two terms lasted for less than two years). In 1993, the duration of a legislative term was reduced from five to four years, then restored to five years in 2000. Italian municipalities are in charge of a vast pool of services, from water supply to waste management, from municipal police to infrastructures, from housing to welfare policies.

The remuneration of the mayor depends on the size of the resident population, as measured by the national Census that takes place every 10 years, and sharply changes at 9 different thresholds. Nominal salaries have been adjusted almost every year to account for price inflation, so that real values within each population bracket have remained almost unchanged between 1993 to 2004, in line with the trend in national per capita income (see the Online Appendix Table A1). The average real disposable income remained almost unchanged from the beginning to the end of the 1990s in Italy, decreasing in the first half and going back to the initial level in the second half. Since adjustments were applied uniformly to all municipalities, the relative wage between different population brackets also remained identical across time.

The mayor's wage, however, is not the only policy varying with population size. In Table 1, we present a summary of all the policies based on population brackets, which we recovered from the Ministry of Internal Affairs and by comparing different legislative sources. Besides the salary of the mayor, population size also determines a vast array of policies: the compensation of the members of the executive committee (between 15 and 75 percent of the mayor's) and of the councillors (as of 2000, between 18 and 36 euros per each session attended); the size of the council (ranging from 12 to 60); the size of the executive committee (ranging from 4 to 16); the electoral rule (single round versus runoff); whether or not a municipality can have additional elective bodies in every neighborhood; whether or not a municipality can host hospital facilities, or organize a health-care district. All the other policies based on population change proportionally with the number of inhabitants (e.g., transfers from the central government).[5]

Table 1 shows that only three out of the nine wage thresholds determine a variation solely in the remuneration of the mayor (or of the other members of the municipal government): 1,000, 5,000, and 50,000. In all of the other cases, in fact, the wage increase overlaps with additional policies whose effect cannot be dismissed. Because the wage increase at 1,000 was only introduced in April 2000 and our dataset does not contain budget information after 2005, we cannot use this threshold in our analysis, as we are not able to calculate performance indicators for mayors elected for two consecutive terms after 2000. Because of the very small sample size around 50,000 inhabitants (only 31 terms between 49,000 and 51,000, and from 1993 to 2007), we cannot use this threshold either. As a result, we focus on the 5,000 threshold, with a remaining caveat: from 2001 to 2004, municipalities below 5,000 inhabitants were exempted from complying with the Internal Stability Pact, a set of rules decided by the national government to improve fiscal discipline at the municipality level. We therefore drop from our sample mayoral terms after 2001 to avoid this confounding policy.

As of 2000, the real gross wage of the mayor ranges from 1,291 euros per month for municipalities with less than 1,000 inhabitants up to 7,798 euros for those with more than 1,000,000 people. At the 5,000 threshold, the gross salary increases by 28.6% (from

[5]Legislative references in hierarchical and chronological order: Laws 816/1985, 81/1993, and 265/1999; *Decreti del Ministero dell'Interno* 11/4/1988, 2/4/1991, 4/7/1994, 12/3/1997, and 4/4/2000. For central transfers, see *Decreto Legislativo* 504/1992. For health-care administration, see Law 412/1991.

2,169 to 2,789 euros), while before 2000 (i.e., for the largest share of our sample period) the salary increase at 5,000 was even higher and equal to 33.3%.[6] These numbers are quite sizable if compared to the rest of the population. In 2000, the average gross labor income in Italian cities with less than 5,000 inhabitants was 1,651 euros per month for men and 1,281 for women, while in cities between 5,000 and 20,000 it was 1,762 and 1,363, respectively.[7] Especially in small cities, it thus seems that being appointed as mayor provides a significant source of income for a large fraction of the population.

Before moving to the data, it is worth addressing three specific aspects of the Italian institutional framework that, to a certain extent, might affect the interpretation of our results. First of all, the compensation of the members of the executive committee changes along with the compensation of the mayor at 5,000. Although the overall effect might be interesting *per se* (that is, the effect of an increase in the salary of all the members of the executive office), we cannot separately identify the effect of a change in the wage of the mayor. However, since the magnitude of the compensation of the executive committee is very small, it is plausible to assume that the effect of increasing the remuneration of elected officials is actually driven by the mayor being paid more, the compensation packages for the other executive officers being second-order.

Second, mayors can keep their job and cumulate earnings, the only restriction being that if they work as dependent employees they have to ask for a leave-of-absence, otherwise the salary is cut by half. The possibility of making outside income, however, only affects the external validity of our results. In other words, what we are estimating is the impact of politicians' wage in a situation where the elective office is compatible with outside work, as opposed to the situation in which it is not, and we know from Gagliarducci et al. (2010) that the impact of the wage on political selection may differ in the two cases. To assess the relevance of outside work in our data, we conducted a phone interview survey of mayors in towns between 4,900 and 5,100 inhabitants (in office on May 1, 2009). We obtained replies from 36 out of 57 mayors. The fraction of part-time mayors was 53%, with the others working full-time as mayor. Importantly, this fraction was almost identical for

[6] Assuming a taxpayer without dependents and other sources of income, the increase in the net salary is 23.1% after 2000 and 22.9% before 2000.

[7] An average mayor in our sample, based on his/her observable characteristics, would otherwise earn a gross labor income of about 2,264 euros in the private market (predicted using age, gender, education, and type of job). Source: see footnote 1.

towns below and above 5,000 (54% and 53%, respectively).[8] It seems therefore that the time devoted to office is relevant and associated to a sizable opportunity cost.

Finally, under specific and documented circumstances, the executive committee can grant up to an additional 15% increase to the mayor, conditional on the approval of the Ministry of Internal Affairs. If applied, this policy would simply change the (quantitative) interpretation of the estimated effects. Suppose for example that all towns above 5,000 chose to increase the salary, while all towns below did not. In this case, we would estimate the effect of a 48% wage increase. In the opposite case, if all towns above 5,000 increased the salary, while the others did not, there would be an 18% increase. According to the mentioned survey of mayors around 5,000, however, only very few municipalities (two out of 36, both above the threshold) introduced a wage raise, so that we can confidently conclude that we are estimating the impact of a 33% wage increase at 5,000.

3.2 Data

The original dataset contains the mayoral terms elected from 1993 to 2005 for all Italian municipalities. It carries information about gender, age, highest educational attainment (self-declared), political affiliation, and previous job (self-declared) of the elected mayor and the losing mayoral candidates, as well as yearly information at the municipality level about the budget components (i.e., subcategories of revenues and expenditures) and some administrative indicators (i.e., speed of revenues collection and payment). The individual-level data were provided by the Statistical Office of the Italian Ministry of Internal Affairs, while the town-level data by ANCI (*Associazione Nazionale Comuni Italiani*).

In Table 2, we pool together all the mayoral terms between 1993 and 2001 and summarize the characteristics of both the three best candidates (top panel) and the elected mayor (middle panel) for whom we have non-missing information—31,822 candidates and 16,393 mayors—by population size.[9] On average, 7% of the candidates are women, aged 46.6, and with about 13.8 years of schooling (i.e., high-school level). Almost 14% were not employed (either unemployed or out of the labor force) before the election, 27% were either self-employed or entrepreneurs, 30% were white collars (lawyers, professors, physi-

[8] The (self-declared) weekly working hours of full-time mayors were 38, those of part-time mayors 28.

[9] We could not recover information about any other candidate. However, only 2.79% of the electoral races had more than three candidates, 18.92% exactly three, 63.37% two, and 14.92% were uncontested.

cians, managers), and 20% were employed in low-skilled occupations (blue collars, clerks, and technicians), with other types of jobs in the residual category. As far as the population size increases, candidates are more educated, less likely to be non-employed or low-skilled, and more likely to be high-skilled. These patterns pass-through to the winner of the electoral race, as we also observe similar levels and trends for the elected mayors.

As budget indicators we use the following variables per capita (and per calendar year): total expenditure, total revenues, and deficit. The budget is decided at the end of every year by the mayor and his cabinet, and is subject to final approval by the municipal council. To assess budget management and priorities, we also look at the following items (in 2000 real per-capita values): i) expenditure for investments ("capital expenditure"), personnel and debt service ("rigid expenditure"), or goods and services ("current expenditure"); ii) revenues from transfers (from the European Union, the national, or the regional government), taxes (rates set by the central government), or tariffs on various services provided to citizens (e.g., renting public properties, or demographic services). All variables are averaged over the term, excluding election years to avoid the overlapping of different mayors over the same calendar year.[10] Because of missing observations, the budget sample is smaller: 14,115 mayoral terms.[11]

To further evaluate the efficiency of the municipal government, we look at two additional performance measures: the speed of revenues collection (i.e., the ratio between the collected revenues and the total amount of assessed revenues that the municipality should collect within the year) and the speed of payment (i.e., the ratio between the outlays actually paid and the outlays committed in the municipal budget within the year). The delay in tax collection and payment originates instead from the gap between the cash basis accounting and the accruals principle of accounting: some expenses and revenues, in fact, are recorded even if they may not have been actually paid or received in cash. And the timing of paying and collecting is also under the control of the mayor's cabinet.

The bottom panel of Table 2 contains descriptive statistics of the performance indicators. Both revenues and expenditure have a U-shaped relationship with population size:

[10] Municipal elections in Italy are usually held in the late Spring, so that the electoral and the calendar year do not coincide.

[11] We checked for the nonrandom presence of missing information on the mayors' characteristics, the candidates' characteristics, and budget outcomes, in a close interval around the discontinuity point (200 below and above 5,000), and could not find any statistically significant jump in missing values.

they decrease at first, possibly because of economies of scale in running the municipal administration, and then rise again for cities above 10,000, where more infrastructures are usually undertaken. When we look at the composition of revenues and expenditure, we see that 41% of the expenditure is used to cover investments, 29% to cover personnel costs and the debt service, and the remaining 30% to purchase goods and services. As for revenues, 70% are made of transfers, while 19% are local taxes, and 11% are tariffs for municipal services.

For the reasons discussed above, we restrict the analysis to the 5,000 threshold and the sample to cities between 3,250 and 6,750 inhabitants, to stay sufficiently away from other policy thresholds. This leaves us with a total of 3,039 mayoral terms: 858 within the range of 500 inhabitants (432 below and 426 above), 161 within the range of 100 (64 below and 97 above), and 94 within the range of 50 (42 below and 52 above).

4 Empirical results

4.1 The effect of the wage on political selection

In this section, we analyze whether paying politicians more affects the selection into politics. In Table 3, we look at whether a higher remuneration has an effect on the quality of both the best three candidates and the elected mayor. We present results using our preferred estimation method, Local Linear Regression (LLR, henceforth) with optimal symmetric bandwidth, which is rate optimal and has attractive bias properties (Porter, 2003).[12] In the Online Appendix, as a robustness check, we also provide the results with different (spline) polynomial approximations over the entire sample.

As we can see, the 33% wage increase at the 5,000 threshold has a sizable and statistically significant impact on the educational level of candidates. Candidates to a better paid office turn out to have 0.905 years of schooling more. This corresponds to a +6.4% increase with respect to the average value of 14.06 years in the 3,000-5,000 population bracket. This selection effect of the wage on the quality of candidates transfers almost one-to-one into the quality of elected mayors. The impact on the education of mayors is 0.879 years of schooling more (+6.2%). And the impact on the fraction of white-collar

[12] See the Online Appendix for a description of different estimation methods in RDD.

candidates is positive when we look at the mayoral candidates.[13]

These results are consistent with the descriptive plots in Figure 1 and Figure 2, where we draw scatters of the observed values, plus a running-mean smoothing performed separately on either side of the threshold, and the LLR fit from the estimation in Table A3 of the Online Appendix. The sharp jump when moving from the left to the right of the threshold is particularly evident for the years of schooling, but not for the other variables, where there is more noise.[14]

To assess the robustness of these (local) results, in Figure 5 we implement placebo tests by estimating the treatment effect at 500 fake thresholds below and above the 5,000 threshold (any point from 4,900 to 4,400, and from 5,100 to 5,600), where there should be no effect (see Della Vigna and La Ferrara, 2011). To preserve sample size, in this case we used a third-grade polynomial fit instead of the baseline LLR with optimal bandwidth. We do not find striking evidence of a significant effect outside the 5,000 threshold, as most of the placebo coefficients are either below or above our estimated coefficient (the vertical line), and most of the placebo coefficients are actually zero. In particular, on the left of the threshold all the placebo coefficients on candidates' years of schooling were below our estimated coefficient, and only 5.4% on the right of the threshold. The same numbers are 0% and 5.8% for the placebo coefficients on mayors' years of schooling (see the Online Appendix Figure A1). This evidence reinforces the robustness of out results on education, as they are not driven by random chance alone.

To sum up, the 33% wage increase at 5,000 attracts more educated candidates. Not surprisingly, this translates into more educated mayors. Indeed, if we take into account that in municipalities below 5,000 the average gross labor income per month for people

[13]Results are robust to the use of a (spline) third-grade and fourth-grade polynomial approximation over the maximum bandwidth, as well as to the inclusion of the available pre-determined variables (geographical size, sea level, and geographical location) as covariates in the baseline LLR specification (see the Online Appendix Table A2). Results are also robust to collapsing observations at the city level. In the collapsed sample, the impact of the wage on candidates' years of schooling is 1.064 (s.e. 0.435, obs. 1,484); the impact on mayors' years of schooling is 1.443 (s.e. 0.569, obs. 1,484). Full results available upon request.

[14]In the Online Appendix Table A3, we also report the estimated effect of the wage on the selection of executive officers (appointed by the mayor). As expected, we find a positive effect on years of schooling and a negative effect on age, but this is relatively smaller than the effect for the mayor, and not always statistically significant. We also performed the same estimates on the sample of non-winning candidates, and found very similar results as for the elected mayors. For instance, the impact of the wage on years of schooling is 0.832 (s.e. 0.497, obs. 3,505). Full results available upon request.

without (with) a high-school degree was 1,364 (1,629) euros in 2000, while people with college education earned 1,913 euros, the selection effect of a wage increase of 620 euros is hardly surprising. We also performed the RDD estimations separately for the North and the South of the country. The effect of the wage on years of schooling and high-skilled occupations remains statistically different from zero in both samples, but the point estimates are greater in the South, where the lower cost of living amplifies the impact of a wage increase (results available upon request). There is also some evidence that a higher wage attracts relatively more citizens employed in high-skilled occupations, pointing to the fact that the time devoted to the office is an important component of the opportunity cost of entering politics.[15]

4.2 The effect of the wage on performance

We now investigate whether the salary affects the way mayors run their municipality. As explained in Section 2.2, for each budget outcome we will estimate three separate parameters: the overall effect of the wage on performance (in the row "overall" of each table), identified on mayors with a non-binding term limit, who therefore still have a reelection incentive; the composition effect of the wage on performance (in the row "composition" of each table), identified on the same mayors but when facing the term limit, who therefore cannot be reelected; and, finally, the (reelection) incentive effect (in the row "reelection" of each table), identified as the difference between the previous two effects.[16] As for the selection results, the baseline estimation method is LLR with optimal bandwidth.

In Table 4, if we look at the overall effect, the first result to notice is that paying a

[15] In the Online Appendix Table A4, we ran the same exercise for the available data around the 1,000 threshold (from 2000 only), and found that the 12% wage increase at 1,000 is not enough to motivate highly educated citizens to enter politics. At the 1,000 threshold, we only observe a pale reduction in the percentage of candidates employed in low-skilled occupations. While we would be tempted to attribute the different effect between the 1,000 and 5,000 threshold to the intensity of the treatment, we have to acknowledge that the two (local) results refer to different time periods (2000-2007 and 1993-2001, respectively), and that the composition of the reference labor force might also differ greatly in the two situations (e.g., less high-skilled and college graduates in smaller cities). Furthermore, the wage might have a delayed effect on political selection, not captured in the exercise at 1,000 because this threshold was only introduced in 2000.

[16] For external validity, one might worry that mayors who survive two terms are different from the others (actually, on average, they are younger, and less likely to be non-employed or female). We thus estimated the effect of the wage on selection (as in Table 3) over the sample of mayors used in the estimation of the effect of the wage on performance, and found the same results. For instance, the impact of the wage on mayors' years of schooling is 1.612 (s.e. 0.659, obs. 578). Full results available upon request.

mayor 33% more reduces the size of the municipality budget, as both total expenditure and revenues per capita decrease by a significant amount (-228.00 and -223.19 euros, respectively, in both cases about -20% with respect to the 3,000-5,000 average values). Looking at expenditure subcategories, we can see that the budget reduction is mostly driven by a significant cut in expenditure for goods and services, equal to -86.46 euros (-21.8%). Investments are reduced by 65.75 euros (-15.8%), while for the expenditure on personnel and debt service the reduction is not statistically significant. As for collected revenues, there is a -121.85 euros reduction in tariffs (-75%), while there is no significant evidence of a decline in transfers from the other levels of government or in taxes.[17]

Although we had no prior on the direction of the effect on the budget, the heterogeneity of the results over the different subcategories is not surprising. Mayors, in fact, can easily cut on the purchase of goods (e.g., computers, cars, furniture), services (e.g., cleaning, general maintenance, and especially external consulting), and on investments (e.g., the construction of roads or public facilities). They might find instead more resistance in cutting on personnel (as a matter of fact, we do not find any effect over this dimension), except for the share of workers on a temporary basis (i.e., not covered by either unions or by the Public Administration national contract, which is indeed very protective). On the side of revenues, as long as tax autonomy was still limited over our sample period, and transfers are set by the central government, mayors might find it easier to cut on tariffs for services provided to citizens (e.g., renting public properties to private activities like sport facilities and community centers, or demographic services). Since revenues and expenditures move in the same direction, the effect on the deficit is not statistically different from zero.[18]

Looking at the other estimates in Table 4, it is clear, though, that most of the overall effect comes from the selection of different politicians, rather than from the interaction between a high wage and the willingness to be reelected.[19] As a matter of fact, the (re-

[17]Results are robust to the use of a (spline) third-grade and fourth-grade polynomial approximation over the entire sample, as well as to the inclusion of the available pre-determined variables (geographical size, sea level, and geographical location) as covariates in the baseline LLR specification (see the Online Appendix Table A5).

[18]All results are robust to the use of the budget variables as observed in the last year in office, instead of term averages. Results are also robust to collapsing observations by city and term limit. In the collapsed sample, the overall, composition, and selection effect are (respectively): total expenditure (-209.144, s.e. 107.641; -257.905, s.e. 128.139; 48.761, s.e. 82.574); total revenues (-201.863, s.e. 109.053; -272.057, s.e. 132.055; 70.194, s.e. 81.278); obs. 1,193. Full results available upon request.

[19]66% of mayors rerun for a second term, and 78% of them are reelected. As a matter of fact, we also

election) incentive effect is almost never significant, despite the relatively small standard errors, except for the budget deficit. In this latter case, it seems that mayors avoid fiscal imbalances when they have to run for another election, while they relax the budget constraint as soon as they cannot be reelected (indeed, the effect of the reelection incentive on the budget deficit is almost offset by the composition effect). Among mayors with a binding term limit (composition effect), instead, those who are paid more reduce expenditures, taxes, and tariffs.[20] In other words, for the reduction in tariffs, investments, and current expenditure, selection is clearly the driving force behind the overall effect.[21] This can also be seen by comparing Figure 3 (for two-term mayors without a binding term limit) and 4 (for two-term mayors with a binding term limit). In particular, there is a visible negative jump both in the total revenues and expenditures per capita, while for the rest of the figures the graphical evidence is less clear. Furthermore, the comparison of the two graphs confirms the absence of any incentive effect, as figures look almost the same irrespectively of whether the term limit is binding or not (indeed, the incentive effect is represented by the difference between the two).[22]

find that being paid more has an effect on the decision to run for reelection (8 percentage points more, significant at the 5% level), but not on reelection. This might be explained by the fact that, as we showed in the previous section, a higher wage also attracts a better pool of (losing) candidates.

[20] Also with the aim to disentangle (reelection) incentives from selection, Ferraz and Finan (2009) isolate the impact of incentives by controlling for politicians' observable characteristics X when estimating the overall effect of the wage on their performance indicators. Our approach, however, can account for both observables (X) and unobservables (v). As a matter of fact, finding that the inclusion of observable characteristics does not change the baseline estimates could mean two very different things: (i) that selection does not count (which is their conclusion); (ii) that the available observable characteristics of politicians are not enough to account for the selection effect (which is our conclusion). With this in mind, we replicated their strategy on our sample, estimating the effect of the wage on performance both without and with covariates, and found that our performance estimates are only slightly affected by the inclusion of observable covariates. In line with the results of our approach, we interpret this as evidence that unobservable self-selection is the main channel of the impact of the wage on performance.

[21] As discussed in Section 2.2, mayors in the second term might also have the incentive to perform well because they plan to run for higher offices. As these motivations do not depend on the wage, they should not affect our identification strategy, unless they were completely first-order. We actually observe that, in municipalities between 3,500 and 6,500, only 5.3% of the mayors were appointed in the provincial government, 1.8% in the regional government, and 0.4% in the national parliament. Importantly, we do not detect any difference in the career prospects of mayors above and below 5,000.

[22] An alternative explanation for the lack of any reelection incentive could be that Italian voters have strong ideological preferences ("party alignment"), which makes the threat of non-reelection less credible. To be sure that this is not the case, in the top panel of the Online Appendix Table A6, we run the same exercise as in Table 4 restricting the sample to mayors whose electoral margin in the first term was small, that is, mayors who obtained less than 55% of the votes. In this subsample, one can expect swing voters to be decisive and then the reelection motive to be stronger. Even in this case, there is no evidence that a

Finally, in the Online Appendix Figures A2 and A3, we test for the treatment effect at fake thresholds (where there should be no effect) as we did in Section 4.1 for the selection outcomes. We find that on the left (right) of the true threshold only 4% (2.6%) of the placebo coefficients on total expenditure are below our estimated coefficient in the case of a non-binding term limit; the same numbers are 0% and 0.6% when the term limit is binding. We also find that both on the left and on the right of the true threshold none of the placebo coefficients on total revenues is below our estimated coefficient when the term limit is not binding (again, the same numbers are 0% and 0.6% when the term limit is binding). We therefore conclude that the baseline results are robust to placebo testing.[23]

Although we cannot observe the quality of public goods and services provided at the municipality level, the above evidence on the reduction of government size is consistent with the fact that the 33% wage increase at 5,000 attracts skilled citizens, who then run the government body more prudently. In particular, they lower the tariff burden, maybe reducing sources of waste in current outlays, while leaving almost unchanged other sources of expenditure. Indeed, empirical evidence about Italy shows that passive waste—that is, inefficiency due to red tape—is concentrated on expenditures for goods and services at the local level (Bandiera, Prat, and Valletti, 2009). An alternative interpretation of our results, however, is that the reduction in government size reflects differences in preferences; that is, a higher wage attracts more educated individuals, who are generally more reluctant toward redistribution even after controlling for income (Alesina and Giuliano, 2009). This would be true, on average, for candidates of both the center-left and center-right coalition.[24] And

higher wage has an impact through the willingness to be reelected, which makes us think that our result simply reflects the strength of the composition effect over the (reelection) incentive effect of the wage.

[23] A final robustness check concerns administrative experience. In the framework outlined in Section 2.2, we assumed that all mayors without a binding term limit were in the first term, while all mayors with a binding term limit were in the second term. However, this is not always the case in the data. When the term limit was introduced, in fact, it only applied to terms elected after 1993, no matter the previous ones. For this reason, we observe some mayors in the third and fourth term. In the middle panel of the Online Appendix Table A6, we present the same estimates as in Table 4 but restricting the sample to mayors elected for the first time after March 1993. The results are almost unchanged. We conclude that differences in administrative experience do not bias our baseline results. This is also reassuring about our assumption that the effect of experience is the same above and below 5,000 inhabitants (see again Section 2.2): otherwise, if experience affected the performance of low-paid and high-paid mayors differently, the outcome of this robustness exercise would differ from the baseline results.

[24] Note that the political party affiliation of the mayor is also well balanced around the threshold. This rules out the possibility that the difference in policymaking was driven by the different political views on the way fiscal policy should be conducted (results available upon request).

voters could accept the implicit policy change in exchange for the greater competence of these politicians.

To shed more light on these two alternative explanations, in Table 5 we perform the RDD estimations on the speed of revenues collection and the speed of outlays' payment, which we take as a better proxy for administrative efficiency. Although the effect is significant only at the 10% level, there is some evidence that better paid mayors increase the speed of revenues collection (+4.5 percentage points, which is +6.9% with respect to the 3,000-5,000 average value), while the evidence of an effect on the speed of payment is significantly lower (1.6 percentage points, which is +2.0% with respect to the 3,000-5,000 average value).[25] This result is consistent with the interpretation that, while the reduction in the government size could still reflect some differences in the preferences of the elected mayors, at least part of this effect is driven by a general improvement in the efficiency of the bureaucratic organization. Finally, placebo tests in the Online Appendix Figures A2 and A3 further reassure against the presence of any effect beyond the policy threshold.

4.3 Validity tests

In this section, we assess the validity of the above results by indirectly assessing the plausibility of the RDD identification assumptions discussed in Section 2. First, to formally check for the absence of manipulation of the running variable at 5,000, we test the null hypothesis of continuity of the density of population size at 5,000. Second, we check whether invariant characteristics of the municipalities, such as area size and geographic location, are balanced in the neighborhood of 5,000.

The Italian territory is very fragmented, with the great majority of the municipalities having a population size below 10,000 (about 87.0% as of 1991, and 86.6% as of 2001), or even below 5,000 (72.7% as of 1991, and 72.2% as of 2001). It is also worth noticing that no much changed in the population distribution between the 1991 Census and the 2001 Census, which is reassuring against the presence of migration flows in reaction to policy changes at different population thresholds.[26]

[25] Results for speed of collection (but not speed of payments) are robust to the use of a (spline) third-grade and fourth-grade polynomial approximation over the entire sample, as well as to the inclusion of the available pre-determined variables (geographical size, sea level, and geographical location) as covariates in the baseline LLR specification (see the Online Appendix Table A7).

[26] Between 1991 and 2001, only 40 cities moved from above to below 5,000 and 105 from below to

In Figure 6, we plot the frequency of municipalities , using different binsizes (50, 100, and 200 inhabitants) for the 2001 Census. Although the Census is run independently by the National Statistical Office, so that false reporting should be ruled out, it could still be the case that municipalities succeed in sorting above those thresholds by attracting citizens to their territory from other towns. However, no evidence of manipulative sorting can be detected.[27] The evidence of no sorting above 5,000 is reassuring. Mayors alone are not able (or willing) to manipulate population size. In fact, even if they did it, they would not stay in power enough time (because of the two-term limit) to grasp the benefits of sorting above the threshold in the following Census.

In Table 6, we further check for manipulative sorting by performing balance tests on the available invariant town characteristics. If there were nonrandom sorting, we should expect some of these characteristics to differ systematically between treated and untreated municipalities around 5,000. The available pre-treatment characteristics are the size of the geographical area, the sea level, and the location (North vs. South), because all the other variables are endogenous to the policy. The balance tests are by LLR (with full, half, and one quarter of the optimal symmetric bandwidth), plus a third and fourth grade polynomial fit over the entire sample (3,250-6,750). No pre-treatment characteristics show a significant discontinuity at 5,000. In particular, the geographical location, which in Italy might be correlated with social capital and administrative culture, is perfectly balanced. The same is true in Figure 7, when we graphically inspect the behavior of the

above. Differences in population growth above and below the policy thresholds are never statistically significant. We also collected population data from the last pre-treatment Census (1951, the threshold was introduced in 1960), as well as from all the subsequent Censuses (1961, 1971, 1981, 1991, and 2001). Indeed, population changes across Censuses are smooth around 5,000. In particular, the jump at 5,000 in the 1951-1961 population variation amounts to 66.382 (s.e. 152.780); -103.047 (159.382) in 1961-71; 39.299 (182.082) in 1971-81; and -81.859 (144.4266) in 1981-91. This ensures that the estimation results are not driven by population sorting.

[27]We formally test for the presence of a density discontinuity at the 5,000 threshold in the Online Appendix Figure A4, where a McCrary test is performed by running kernel local linear regressions of the log of the density separately on both sides of the threshold (see McCrary, 2008). The log-difference between the frequency to the right and to the left of the threshold is not statistically significant, the point estimate being -0.007 (s.e. 0.236). In the Online Appendix Figure A5, we also plot the frequency of all municipalities with less than 20,000 inhabitants, using different binsizes (100, 250, and 500 inhabitants). Visual inspection does not reveal any clear discontinuity at the other wage threshold (1,000), although the same is not true for the other policy thresholds (3,000, 10,000, and 15,000), where it seems that cities managed to sort just above the policy cutoff. Figures for the 1991 Census are identical and are available upon request.

predetermined characteristics, which look very smooth around the 5,000 threshold.

5 Conclusions

In this paper, we have shown that paying politicians more has a positive effect on the quality of elected officials (that is, their education and professional background), and it also affects the way they manage public finance. In particular, better paid politicians lower the size of the municipal government, by reducing tariffs and current expenditure, as the result of an improvement in the efficiency of the bureaucratic organization. Results also show that this policy effect is due to the selection of more skilled mayors, rather than the incentive to be reelected.

It is important to stress that our empirical exercise—because of its local nature—cannot help determining the optimal wage level, that is, it cannot identify the upper limit over which the welfare benefit from paying politicians more is completely offset by the wage increase itself. Yet, it makes clear that the monetary remuneration is a relevant motivation for citizens willing to run for elective offices. While the obvious recommendation would be to increase the salary paid to politicians, our exercise also suggests that, in addition to population size, the salary could be linked to the private sector compensation for similar occupations, as it is already done in some countries (e.g., Singapore). By doing so, voters could effectively compete with the market in recruiting competent citizens.

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Tables and Figures

Table 1: Legislative thresholds for Italian municipalities

Population	Wage Mayor	Wage Ex. Com.	Fee Council	Ex. Com. Size	Council Size	Electoral Rule	Neighbor. Councils	Hospital/ Health
Below 1,000	1,291	15%	18	4	12	single	no	no/no
1,000-3,000	1,446	20%	18	4	12	single	no	no/no
3,000-5,000	2,169	20%	18	4	16	single	no	no/no
5,000-10,000	2,789	50%	18	4	16	single	no	no/no
10,000-15,000	3,099	55%	22	6	20	single	no	no/no
15,000-20,000	3,099	55%	22	6	20	runoff	no	no/no
20,000-30,000	3,099	55%	22	6	20	runoff	no	yes/no
30,000-50,000	3,460	55%	36	6	30	runoff	allowed	yes/no
50,000-60,000	4,132	75%	36	6	30	runoff	allowed	yes/no
60,000-100,000	4,132	75%	36	6	30	runoff	allowed	yes/yes
100,000-250,000	5,010	75%	36	10	40	runoff	yes	yes/yes
250,000-500,000	5,784	75%	36	12	46	runoff	yes	yes/yes
Above 500,000	7,798	75%	36	14-16	50-60	runoff	yes	yes/yes

Notes. *Population* is the number of resident inhabitants as measured by the last available Census. *Wage Mayor* and *Wage Ex. Com.* refer to the monthly gross wage of the mayor and the members of the executive committee, respectively; the latter is expressed as a percentage of the former, which refers to 2000 and is measured in euros. *Fee Council* is the reimbursement per session paid to councillors and is measured in euros. The wage thresholds at 1,000 and 10,000 were introduced in 2000; all of the others date back to 1960. *Ex. Com. Size* is the maximum allowed number of executives appointed by the mayor. *Council Size* is the number of seats in the City Council. All of the size thresholds were set in 1960. Since 1993, *Electoral Rule* can be either single round (with 60% premium) or runoff (with 66% premium) plurality voting. *Neighborhood Councils* are bodies that represent different neighborhoods within the city and are provided with independent budgets. *Hospital/Health* captures whether the municipality is allowed to have a hospital or a health-care district, respectively.

Table 2: Legislative thresholds: candidates and mayors characteristics, budget components per capita

Population	Female	Age	Years school	Not employed	Entrepreneur	White collar	Blue collar
			All candidates				
Below 1,000	0.07	47.00	12.24	0.19	0.25	0.25	0.30
1,000-3,000	0.07	46.26	13.58	0.15	0.27	0.37	0.21
3,000-5,000	0.06	46.27	14.06	0.13	0.27	0.42	0.18
5,000-10,000	0.08	46.22	14.36	0.12	0.27	0.45	0.16
Above 10,000	0.07	47.40	15.13	0.08	0.28	0.51	0.13
Total	0.07	46.59	13.76	0.14	0.27	0.39	0.20
Obs.	31,818	31,818	31,818	31,818	31,818	31,818	31,818
			Mayors				
Below 1,000	0.06	47.61	12.38	0.18	0.28	0.26	0.28
1,000-3,000	0.06	46.43	13.74	0.13	0.30	0.39	0.19
3,000-5,000	0.06	46.44	14.22	0.11	0.29	0.44	0.15
5,000-10,000	0.08	45.98	14.48	0.10	0.30	0.46	0.14
Above 10,000	0.06	47.05	15.23	0.06	0.30	0.53	0.12
Total	0.07	46.73	13.80	0.12	0.29	0.40	0.19
Obs.	16,393	16,384	16,384	16,384	16,384	16,384	16,384

	Deficit	Expenditure				Revenues				Efficiency	
		Total	Investments	Personnel and debt	Goods and services	Total	Transfers	Taxes	Tariffs	Speed of collection	Speed of payment
Below 1,000	27.02	2,068.94	1,011.14	594.03	465.85	2,041.92	1,556.05	280.13	205.75	65.92	80.07
1,000-3,000	17.21	1,325.88	547.10	379.57	399.70	1,308.66	924.39	242.43	141.85	65.67	80.36
3,000-5,000	20.22	1,134.89	416.51	322.63	395.75	1,114.67	732.24	246.93	135.50	65.32	79.45
5,000-10,000	13.57	1,026.93	310.14	303.99	412.79	1,013.36	603.30	274.93	135.12	65.26	78.49
Above 10,000	19.00	1,075.28	302.18	315.46	457.64	1,056.28	616.40	292.11	147.76	65.07	78.15
Total	19.74	1,402.03	570.11	404.30	428.29	1,382.29	961.11	265.02	156.16	65.51	79.47
Obs.	14,109	14,109	14,109	14,109	14,109	14,109	14,109	14,109	14,109	14,109	14,109

Notes. Terms from 1993 to 2001. *Population* is the number of resident inhabitants, as measured by the last Census. The other columns report average values. All individual variables are dummies, except *Age* and *Years school* (both measured in years). *Years school* is the number of years needed to complete the highest degree obtained. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *Entrepreneur* includes self-employed and entrepreneurs. *White collar* includes lawyers, professors, physicians, and managers. *Blue collar* includes blue collars, clerks, and technicians. All budget variables (i.e., deficit, types of expenditure, and types of revenues) are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). *Transfers* refer to external transfers from the central government, the regional government, or the European Union. *Efficiency* measures in percentage points: *Speed of collection* is the ratio between collected and assessed revenues; *Speed of payment* is the ratio between paid and committed outlays for public expenditure.

Table 3: Candidates and mayor selection, RDD estimates

Population	Female	Age	Years school	Not employed	Entrepreneurs	White collar	Blue collar
				All candidates			
Effect	0.005	-0.903	0.905***	-0.025	-0.037	0.082**	-0.018
	(0.018)	(0.587)	(0.279)	(0.025)	(0.028)	(0.039)	(0.025)
Δ	1,300	1,700	900	900	1,700	1,300	1,400
Obs.	4,805	6,405	3,295	3,295	6,405	4,805	5,191
				Mayors			
Effect	-0.014	-0.847	0.879**	-0.007	-0.023	0.074	-0.035
	(0.022)	(0.822)	(0.346)	(0.033)	(0.046)	(0.046)	(0.035)
Δ	1,700	1,700	1,100	1,000	1,400	1,700	1,400
Obs.	2,971	2,971	1,905	1,738	2,396	2,971	2,396

Notes. Effect of the 33% wage increase at the 5,000 threshold on the characteristics of the three best candidates (top panel) and of the elected mayor (bottom panel). Terms from 1993 to 2001. Cities with population between 3,250 and 6,750 inhabitants. Local Linear Regression (LLR) with optimal symmetric bandwidth Δ. *Age* and *Years school* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *Entrepreneur* includes self-employed and entrepreneurs. *White collar* includes lawyers, professors, physicians, and managers. *Blue collar* includes blue collars, clerks, and technicians. Standard errors robust to clustering at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 4: Budget components per capita, RDD estimates

	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and debt	Goods and services	Total	Transfers	Taxes	Tariffs
A. Overall (no term limit)	-2.121	-165.318**	-65.748*	-14.011	-86.455***	-223.187***	-17.272	-23.355	-121.854***
	(8.041)	(65.940)	(38.315)	(14.502)	(25.596)	(67.977)	(65.748)	(15.722)	(42.225)
B. Composition (term limit)	4.635	-202.773***	-65.631	-23.398	-95.301***	-243.303***	-46.472	-47.406**	-116.855***
	(4.859)	(65.834)	(42.787)	(19.497)	(22.781)	(81.588)	(53.795)	(19.882)	(44.992)
C. Reelection (A-B)	-6.755	37.455	-0.117	9.388	8.846	20.116	29.200	7.155	-4.999
	(8.478)	(54.759)	(36.776)	(16.300)	(15.357)	(50.558)	(63.103)	(8.787)	(19.529)
Δ	1,300	1,500	1,700	1,400	1,500	1,000	1,400	1,700	1,100
Obs.	880	1,016	1,168	950	1,016	696	950	758	758

Notes. Effect of the 33% wage increase at the 5,000 threshold on budget variables. Terms from 1993 to 2001; only mayors observed over two consecutive terms, with binding term limit in the second. Cities with population between 3,250 and 6,750 inhabitants. Local Linear Regression (LLR) with optimal symmetric bandwidth Δ. All variables are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). Standard errors robust to clustering at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 5: Efficiency measures, RDD estimates

	Speed of collection	Speed of payments
A. Overall (no term limit)	4.534*	1.636*
	(2.482)	(0.923)
B. Composition (term limit)	0.933	0.948
	(2.742)	(1.011)
C. Reelection (A-B)	3.601	0.688
	(3.597)	(1.066)
Δ	900	1,500
Obs.	624	1,016

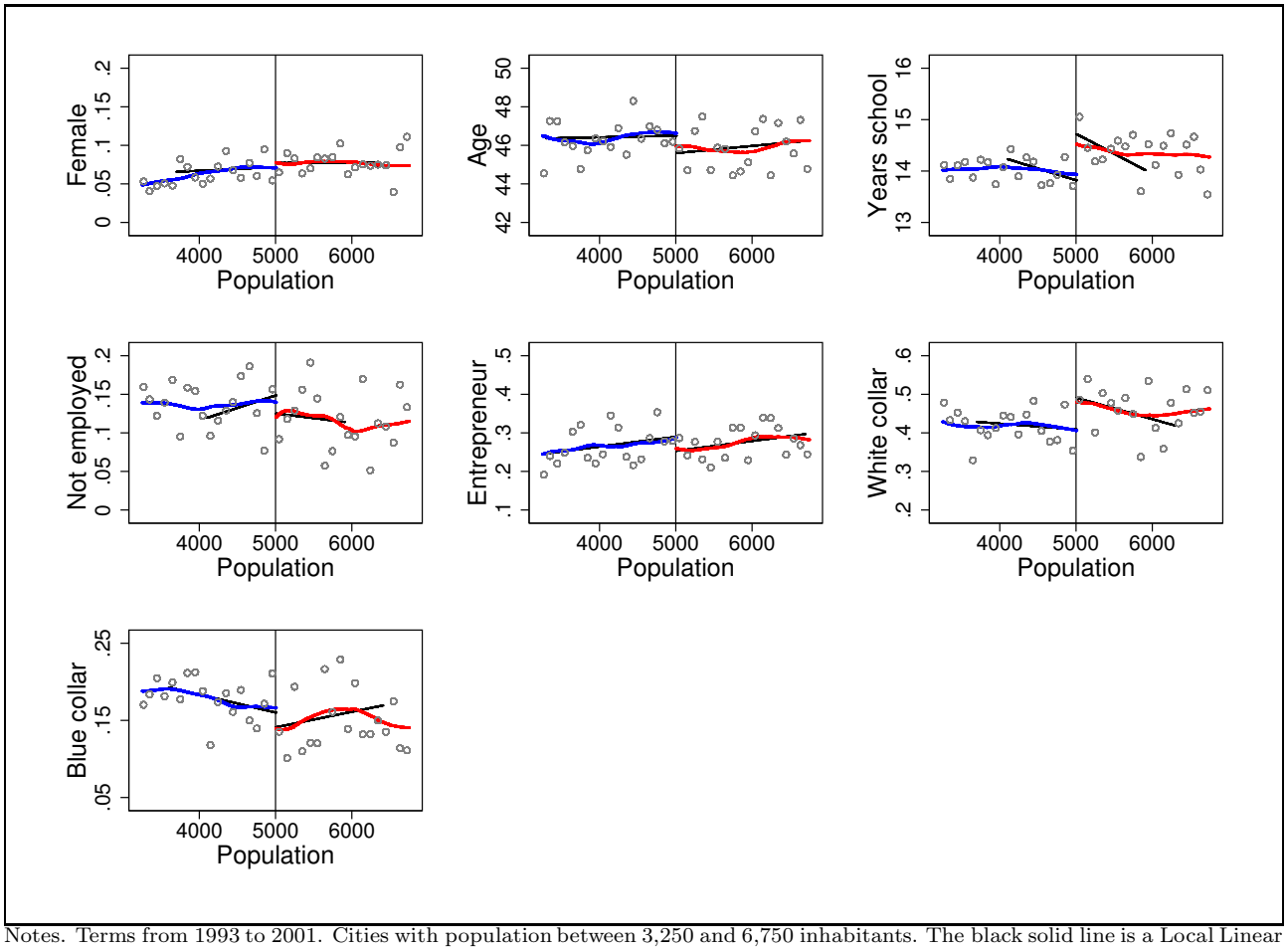
Notes. Effect of the 33% wage increase at the 5,000 threshold on efficiency measures. Terms from 1993 to 2001; only mayors observed over two consecutive terms, with binding term limit in the second. Cities with population between 3,250 and 6,750 inhabitants. Local Linear Regression (LLR) with optimal symmetric bandwidth Δ. All variables are in percentage points, and averaged over the mayoral term (election years excluded): *Speed of collection* is the ratio between collected and assessed revenues; *Speed of payment* is the ratio between paid and committed outlays for public expenditure. Standard errors robust to clustering at the municipality level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6: City pre-determined characteristics, RDD estimates

	Area	Sea level	North/South
	LLR with optimal bandwidth		
Discontinuity	-6.121	-1.803	-0.115
	(4.264)	(32.793)	(0.074)
Δ	1,700	1,000	600
Obs.	1,455	1,089	1,057
	LLR with 1/2 optimal bandwidth		
Discontinuity	-0.278	6.020	-0.128
	(5.757)	(45.744)	(0.089)
Obs.	694	419	838
Δ	850	500	300
	LLR with 1/4 optimal bandwidth		
Discontinuity	-1.401	43.418	-0.028
	(8.250)	(60.057)	(0.134)
Δ	425	250	150
Obs.	378	208	642
	3^{rd} spline polynomial approximation		
Discontinuity	2.333	25.434	-0.102
	(8.428)	(50.164)	(0.106)
Obs.	1,488	1,488	1,488
	4^{th} spline polynomial approximation		
Discontinuity	-5.589	2.994	-0.040
	(10.448)	(62.209)	(0.131)
Obs.	1,488	1,488	1,488

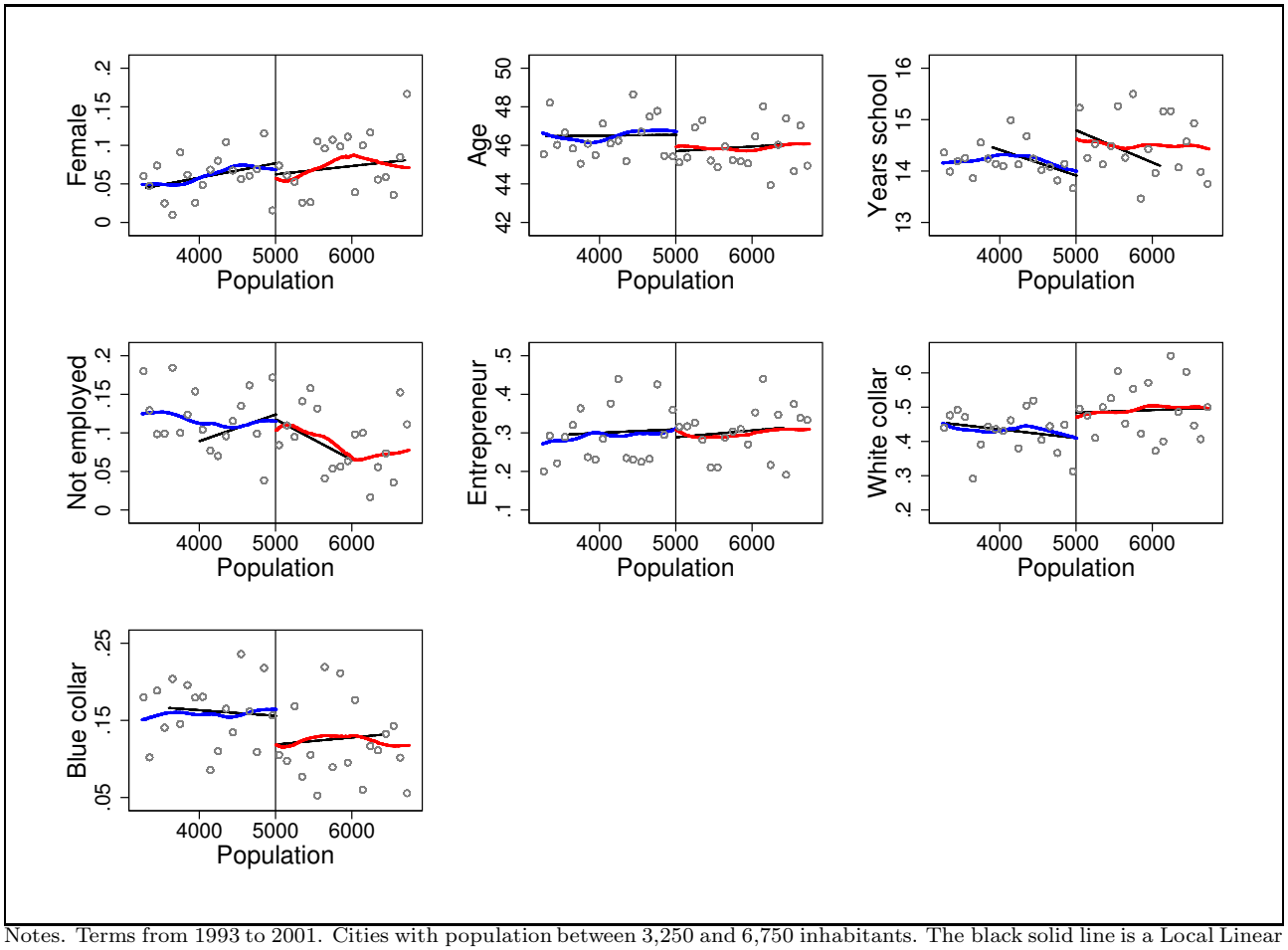
Notes. Cities with population between 3,250 and 6,750 inhabitants. First three estimates: Local Linear Regression (LLR) with optimal symmetric bandwidth Δ. Fourth estimate: 3^{rd} order polynomial approximation on either side of the 5,000 threshold. Fifth estimate: 4^{th} order polynomial approximation on either side of the 5,000 threshold. *Area* is measured in km^2; *Sea level* in meters, referred to the city administrative center; *North/South* is a dummy equal to 1 for Piemonte, Lombardia, Val d'Aosta, Veneto, Friuli-Venezia-Giulia, Trentino Alto-Adige, Veneto, Liguria and Emilia-Romagna, and 0 otherwise. Robust standard errors are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure 1: Candidates characteristics around 5,000



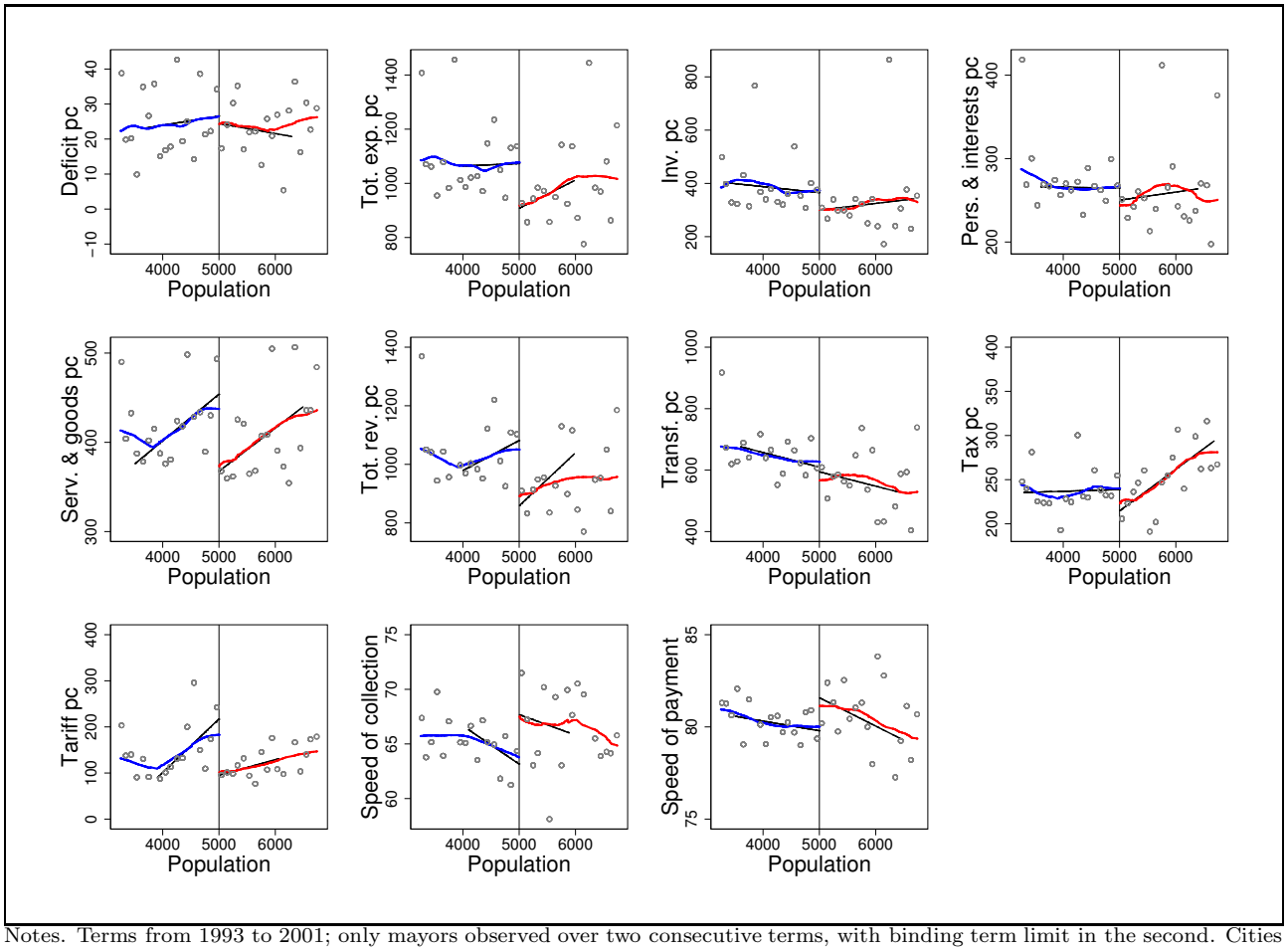
Notes. Terms from 1993 to 2001. Cities with population between 3,250 and 6,750 inhabitants. The black solid line is a Local Linear Regression (LLR) with optimal symmetric bandwidth Δ (see Table 3). The blue/red solid lines are running-mean smoothings of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The circles are the observed values averaged in intervals of 100 inhabitants. *Age* and *Years school* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *Entrepreneur* includes self-employed and entrepreneurs. *White collar* includes lawyers, professors, physicians, and managers. *Blue collar* includes blue collars, clerks, and technicians.

Figure 2: Mayor characteristics around 5,000



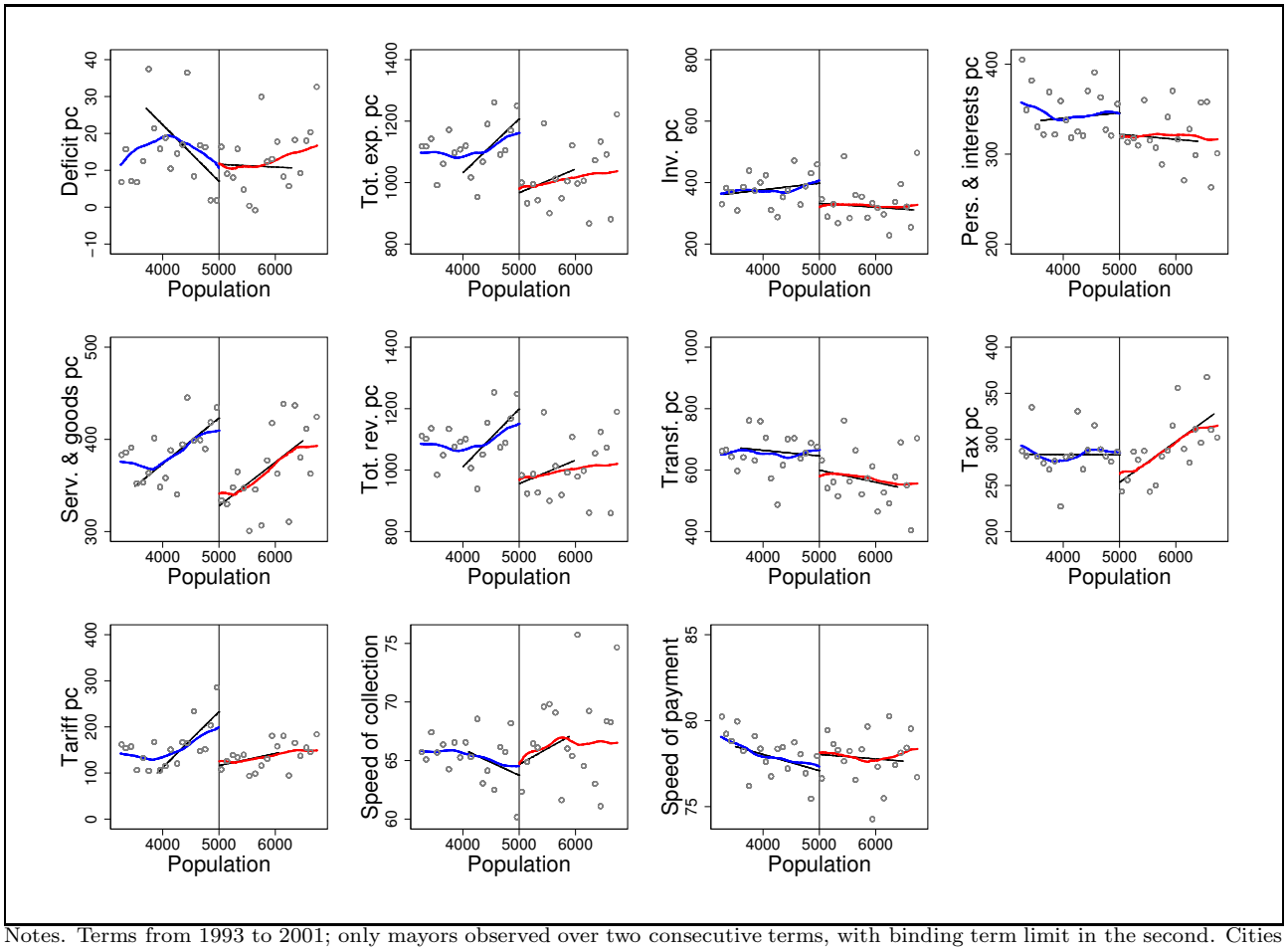
Notes. Terms from 1993 to 2001. Cities with population between 3,250 and 6,750 inhabitants. The black solid line is a Local Linear Regression (LLR) with optimal symmetric bandwidth Δ (see Table 3). The blue/red solid lines are running-mean smoothings of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The circles are the observed values averaged in intervals of 100 inhabitants. *Age* and *Years school* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *Entrepreneur* includes self-employed and entrepreneurs. *White collar* includes lawyers, professors, physicians, and managers. *Blue collar* includes blue collars, clerks, and technicians.

Figure 3: Budget performance and efficiency measures around 5,000, no term limit



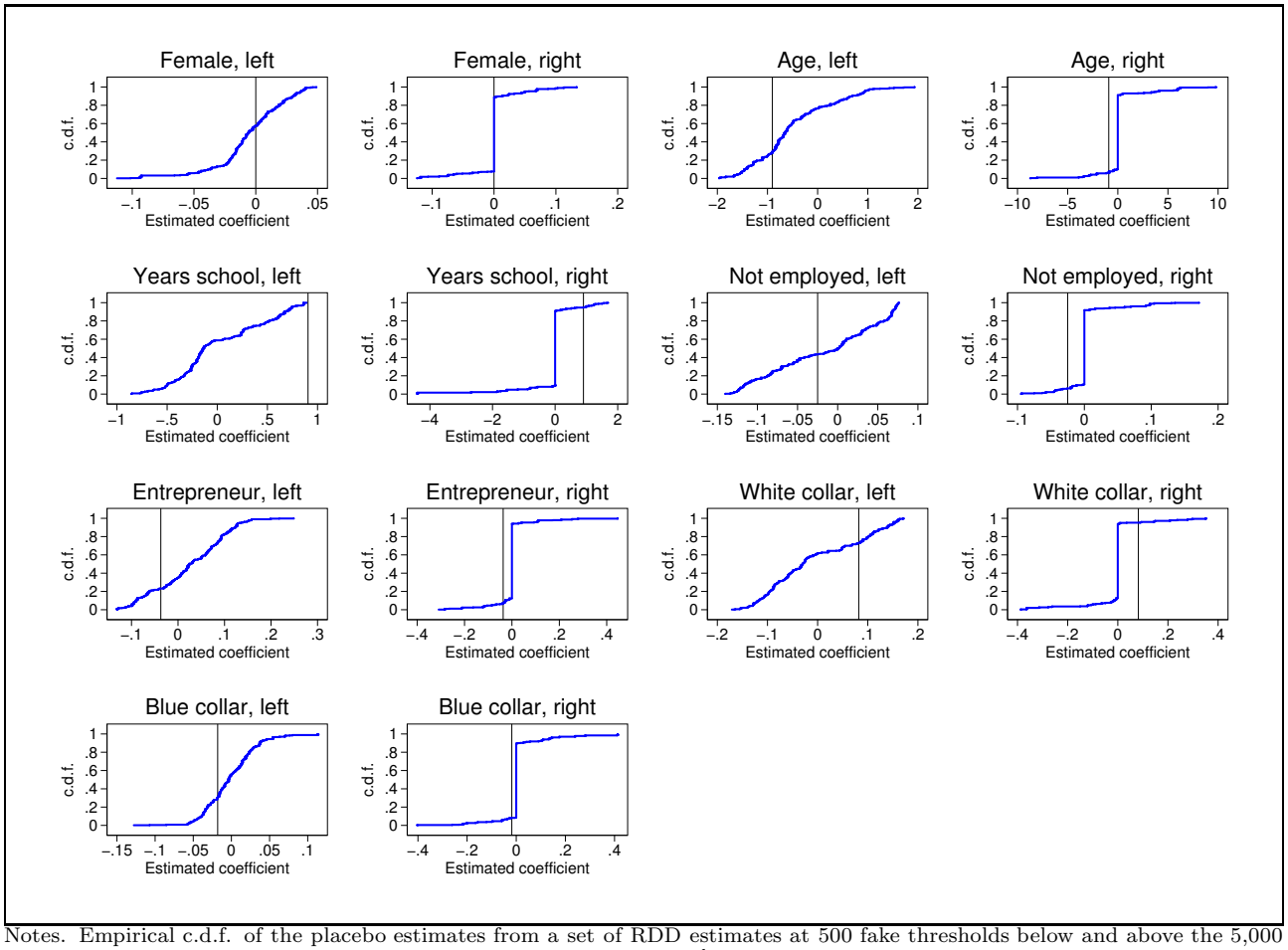
Notes. Terms from 1993 to 2001; only mayors observed over two consecutive terms, with binding term limit in the second. Cities with population between 3,250 and 6,750 inhabitants. The black solid line is a Local Linear Regression (LLR) with optimal symmetric bandwidth Δ (see Table 4). The blue/red solid lines are running-mean smoothings of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The circles are the observed values averaged in intervals of 100 inhabitants. All budget variables are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). All efficiency variables are in percentage points, and averaged over the mayoral term (election years excluded): *Speed of collection* is the ratio between collected and assessed revenues; *Speed of payment* is the ratio between paid and committed outlays for public expenditure. *TL* is an index for the term limit.

Figure 4: Budget performance and efficiency measures around 5,000, binding term limit



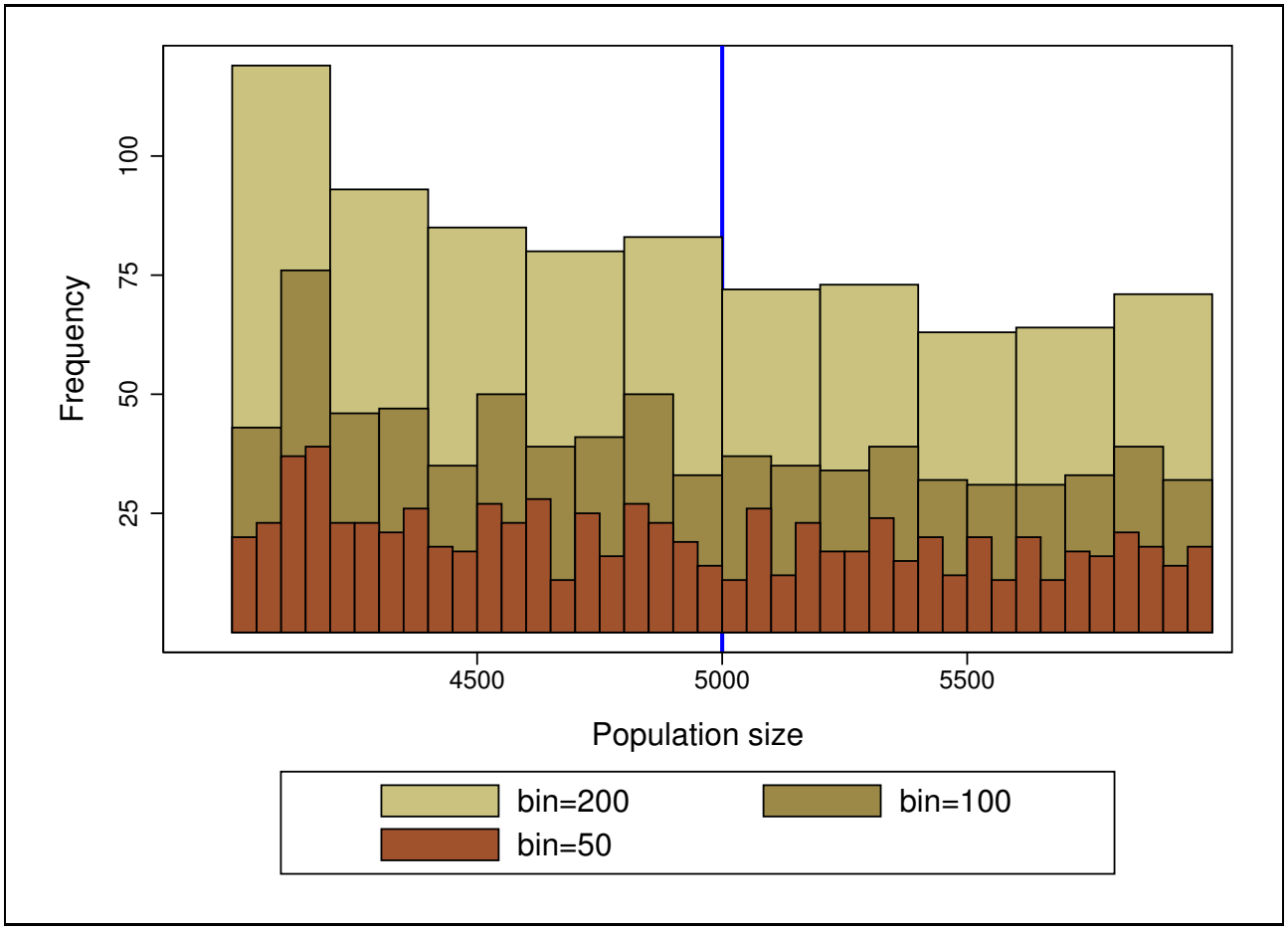
Notes. Terms from 1993 to 2001; only mayors observed over two consecutive terms, with binding term limit in the second. Cities with population between 3,250 and 6,750 inhabitants. The black solid line is a Local Linear Regression (LLR) with optimal symmetric bandwidth Δ (see Table 4). The blue/red solid lines are running-mean smoothings of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The circles are the observed values averaged in intervals of 100 inhabitants. All budget variables are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). All efficiency variables are in percentage points, and averaged over the mayoral term (election years excluded): *Speed of collection* is the ratio between collected and assessed revenues; *Speed of payment* is the ratio between paid and committed outlays for public expenditure. *TL* is an index for the term limit.

Figure 5: Candidates characteristics placebo estimates



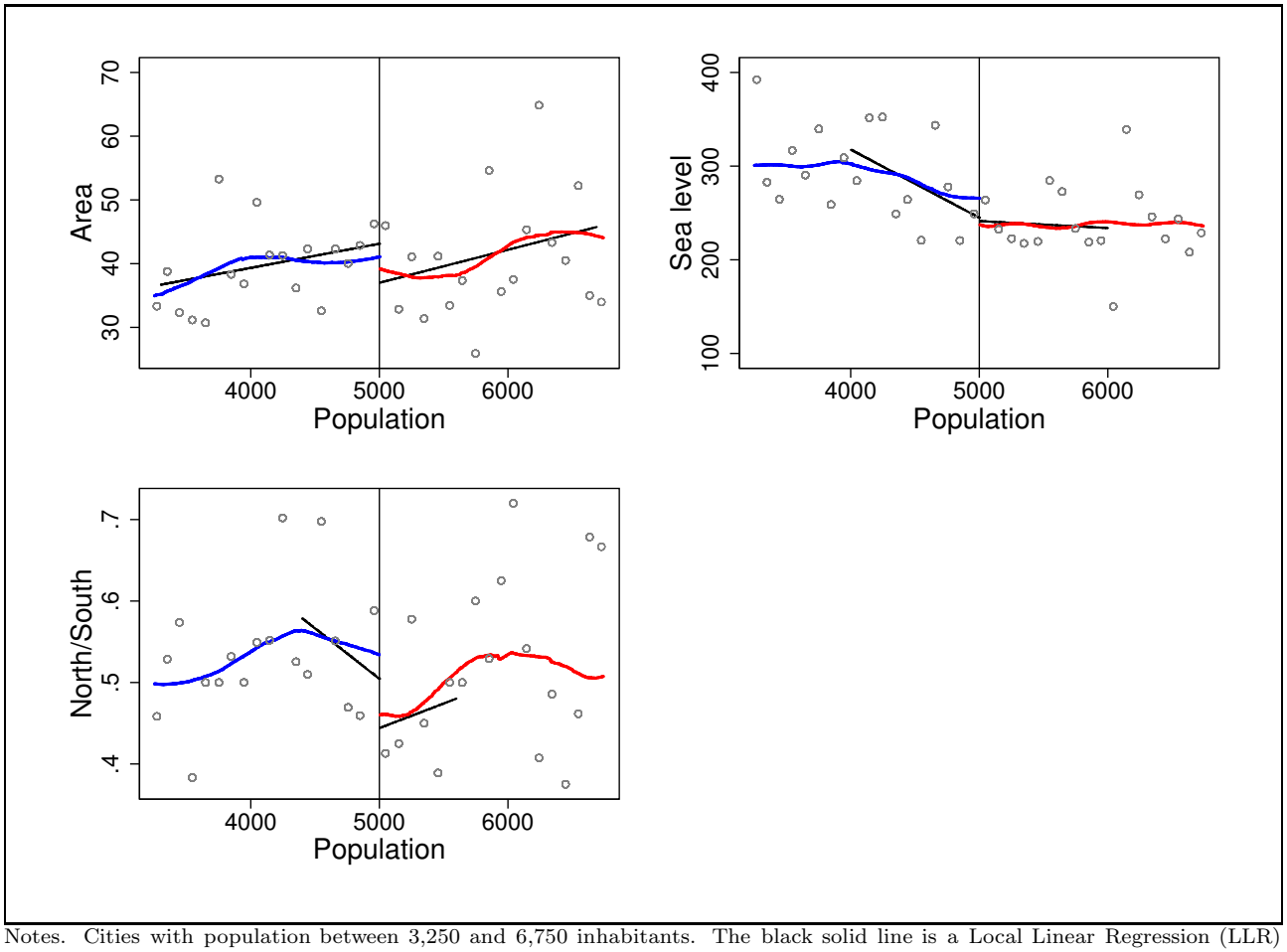
Notes. Empirical c.d.f. of the placebo estimates from a set of RDD estimates at 500 fake thresholds below and above the 5,000 threshold (any point from 4,900 to 4,400, and from 5,100 to 5,600); 3^{rd} order spline polynomial approximations. The vertical line indicates our benchmark estimate from Table 3. Terms from 1993 to 2001. *Age* and *Years school* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *Entrepreneur* includes self-employed and entrepreneurs. *White collar* includes lawyers, professors, physicians, and managers. *Blue collar* includes blue collars, clerks, and technicians.

Figure 6: Population density around 5,000



Notes. Frequency of cities around the 5,000 threshold (vertical line), according to population size in the 2001 Census.

Figure 7: City pre-determined characteristics around 5,000



Notes. Cities with population between 3,250 and 6,750 inhabitants. The black solid line is a Local Linear Regression (LLR) with optimal symmetric bandwidth Δ (see Table 6). The blue/red solid lines are running-mean smoothings of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The circles are the observed values averaged in intervals of 100 inhabitants. *Area* in km^2; *Sea level* in meters, referred to the city administrative center; *North/South* is a dummy equal to 1 for Piemonte, Lombardia, Val d'Aosta, Veneto, Friuli-Venezia-Giulia, Trentino Alto-Adige, Veneto, Liguria and Emilia-Romagna, and 0 otherwise.