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Stefano Gagliarducci and Tommaso Nannicini

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IGIER – Università Bocconi, Via Guglielmo Röntgen 1, 20136 Milano –Italy
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Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection*

Stefano Gagliarducci
CEMFI, Tor Vergata University, and IZA

Tommaso Nannicini
Bocconi University, IGIER, and IZA

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Abstract

The wage paid to elected officials affects both the choice of citizens to run for office and the performance of those who are appointed. On the one hand, if skilled individuals shy away from politics because of higher opportunities in the private sector, an increase in politicians' pay may change their mind. On the other hand, if the reelection prospects of incumbents depend on their in-office deeds, a higher wage may foster performance. We use data on all Italian municipalities from 1993 to 2007 to test these hypotheses in a quasi-experimental framework. In Italy, the wage of the mayor depends on population size and sharply increases at nine thresholds. We apply a regression discontinuity design to two thresholds that uniquely identify a wage increase (1,000 and 5,000 inhabitants) to control for unobservable town characteristics. Exploiting the existence of a two-term limit, we further disentangle the composition from the incentive component of the impact of the wage on performance. The empirical results show that a higher wage attracts more educated and high-skilled candidates, and that better paid politicians lessen the government machinery by reducing per-capita taxes, tariffs, and current expenditure, while leaving investments unchanged. Importantly, most of the performance effect is driven by the selection of better candidates, rather than the incentive to be reelected.

JEL codes: M52, D72, J45, H70.
Keywords: political selection, efficiency wage, term limit, local finance, regression discontinuity design.

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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. The wage of elected officials is decided (or follows rules decided by) politicians themselves. Public opinion swings from the complaint against the high salaries of the political elite, claiming that public-spirited officials should not be paid, to the acknowledgment that "if you pay peanuts you get monkeys" also in politics, or that a better pay might guarantee a broader representation of all social categories and reduce the incentive for corruption. No evidence unambiguously supports either claim.

In economics, it has long been recognized that politicians respond to incentives like all other agents, and that those incentives are a crucial determinant of economic outcomes (e.g., Brennan and Buchanan, 1980). Recent contributions also point to the impact of political selection on economic outcomes, for politicians may be heterogeneous in their competence and honesty (Besley, 2005). The wage paid to elected officials is an important element—although not the only one—in shaping both their decision to self-select into politics and their behavior once in office. According to the standard efficiency wage theory, a higher wage could both attract more skilled candidates (citizens with higher opportunity costs) and enhance performance (because of the higher cost of not being reelected). Although the efficiency wage theory leaves aside many elements that are peculiar to the labor market for politicians, influential models in political economics contain similar intuitions while adding more structure on the political side (e.g., Besley, 2004). Alternative models, however, lead to the opposite conclusion, that is, paying politicians more may decrease their average quality (e.g., Mattozzi and Merlo, 2008). Therefore, the question whether politicians' salary affects selection and performance remains empirical.

Despite one exception (Ferraz and Finan, 2008), there is no credible evidence on the causal effect of politicians' wage. In this paper, we use a data set on the mayors of all Italian municipalities from 1993 to 2007 to evaluate the impact of politicians' remuneration in a quasi-experimental framework. In Italy the wage of the mayor increases with the population living in the municipality, the motivation for this rule being that, as for

companies' executives, the amount of work and responsibility grows with the number of people managed. The quasi-experimental framework arises because the wage does not increase monotonically, but sharply changes at nine different thresholds. If population size cannot be manipulated by politicians to sort above these thresholds, the institutional setting delivers a clean exogenous variation in the mayor's remuneration.

Even though politicians' remuneration is not the only policy decided by population size, three out of the nine thresholds uniquely identify a wage increase: 1,000 inhabitants (introduced in 2000), 5,000 inhabitants (not overlapping with other policies until 2001), and 50,000 inhabitants. Because of sample size limitations, we focus on the first two thresholds: just above 1,000 inhabitants, mayors obtain a sharp 12% increase in the wage, while just above 5,000 inhabitants there is a 33% increase (28% after 2000). Albeit the local effects identified at these two thresholds may not be easily generalized to higher population levels, it should be noted that small cities (below 10,000 inhabitants) actually account for about 90% of all Italian municipalities. Furthermore, the availability of two thresholds with a different wage increase allows us, under specific assumptions, to obtain results for different treatment intensities.

We apply a Regression Discontinuity Design (RDD) to control for unobservable characteristics at the municipality level and test whether a higher wage attracts candidates with higher opportunity costs (*selection effect*) and improves the performance of elected officials over the term (*performance effect*). The last exercise is run on the 5,000 threshold only, because the late introduction of the 1,000 threshold does not deliver enough full-term budget observations.[1] As for selection, the empirical results show that a 12% wage increase at the 1,000 threshold is not enough to motivate highly educated citizens to enter politics. We only observe a pale reduction in the percentage of candidates employed in low-skilled occupations. The 33% wage increase at 5,000 proves instead to be more effective, as it attracts individuals who are more educated (from half to one year of schooling more) and employed in high-skilled occupations (such as lawyers, professionals, or entrepreneurs).

As for performance, measured with a number of indicators drawn from the municipality budget, we find that better paid politicians reduce government size. In particular, they

[1]We focus on budget performance because other dimensions of politicians' quality—such as honesty, congruence with voters, or electoral promises—are not available. For the same reason, we focus on the effect of the wage disregarding other possible incentives, such as ego rents or ideological goals.

lower taxes and tariffs per capita (by about 13% and 75%, respectively) and reduce the amount of expenditure in goods and services (by about 22%). At the same time, they leave the level of investments unchanged. We discuss two possible interpretations for these results. First, more skilled politicians are better at eliminating wastes and making the government machinery more efficient, as they reduce current—instead of capital—expenditure, where most of public spending inefficiencies at the local level are usually deemed to be (Bandiera, Prat and Valletti, 2009). Second, the reduction in government size reflects differences in preferences, with more educated—and eventually wealthier—mayors having weaker preferences for redistribution (Alesina and Giuliano, 2009).

The performance effect, in turn, might be driven by two distinct effects: better paid politicians acting differently because of their higher skills (*composition effect* on performance) or because of reelection motives (*incentive effect* on performance). To disentangle the two channels, we exploit another institutional feature of the Italian legislation, that is, the existence of a two-term limit. For mayors with a binding term limit, the incentive to perform well does not depend on the wage. As a result, if their performance were also affected by the remuneration, this could only be due to their different observable or unobservable characteristics. Our results show that indeed most of the performance effect is driven by the higher quality of the elected mayors, rather than by the incentive to be reelected. We take this as evidence of the strength of the composition effect. Alternative explanations for the lack of an incentive effect, including strong ideological preferences on the part of voters, do not receive support in the data.

The rest of the paper is organized as follows. In Section 2, we set the theoretical background and review the related literature. In Section 3, we formalize the evaluation framework used to identify and estimate the effects of interest. In Section 4, we describe the institutional framework and the data. In Section 5, we present the estimation results and a number of robustness exercises. We conclude with Section 6.

2 Related literature

2.1 Theoretical background

According to the efficiency wage theory, workers' productivity is increasing in the real wage they are paid.[2] There are three main explanations for why this relationship should hold: paying workers more reduces shirking because of the higher cost of being fired (Shapiro and Stiglitz, 1984); it enhances the quality of applicants (Weiss, 1980); and it improves motivation and group work norms (Akerlof, 1982). If we apply these insights to the market for politicians, we should conclude that a higher wage is likely to improve the performance of elected officials due to different reasons. First, a higher wage will attract more skilled individuals (that is, citizens with better outside opportunities in the private sector) into politics. Second, it will increase the incumbent's payoff from being reelected; and this, in turn, will make elected officials more disciplined (e.g., less inclined to extract rents). Third, it could improve 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. Various models in political economics, however, contain some intuitions and predictions of the efficiency wage hypothesis while providing specific insights on the political side. Besley (2004) builds an agency model with both unobserved heterogeneity in the congruence of politicians with voters (adverse selection) and unobserved action when in office (moral hazard). On the one hand, as reelection is the main incentive mechanism, a higher wage plays a discipline role, that is, it increases performance by forcing dissonant politicians to extract lower rents. On the other hand, a higher wage also increases the fraction of congruent politicians, who—unlike the dissonant type—earn no rents from entering politics. Caselli and Morelli (2004) present an adverse selection model where low-quality citizens ("bad politicians") have a comparative advantage in holding office, because their market wages are lower than those of more competent individuals, or because they extract more rents than more honest individuals. In this framework, a higher salary raises the average quality of the (self-selected) pool of politicians. Finally, Persson and Tabellini (2000) propose a career-concern model where forward-looking voters use past

[2]See Akerlof and Yellen (1986) or Yellen (1984) for a survey of the efficiency wage literature.

performance to estimate the ability of the incumbent. As a result, also low-ability officials have an incentive to cut rents and increase the political output in order to be reelected (signal jamming). Because the reelection incentive depends on the remuneration package, the higher the wage, the lower politicians' rents and the higher performance.

The prediction that the quality of politicians is increasing in their wage, however, is not unanimously shared by the literature. Actually, a number of models suggest that the opposite may be true in all those circumstances in which high-quality citizens have additional incentives to enter politics, and a higher remuneration has the indirect effect of making all the other, low-quality, candidates more willing to run (crowding-out effect). For example, Mattozzi and Merlo (2008) propose a dynamic model where there are both "career politicians", who stay in politics until retirement, and individuals with "political careers", who stay in politics for a while in order to signal their true ability to the private sector. In this framework, a wage increase lowers the average quality of citizens who have political careers (entry effect), because politics becomes a relatively more attractive option for all levels of skills, and it has an ambiguous effect on the average quality of career politicians, because also more high-ability incumbents are willing to remain in politics (retention effect).[3] Gagliarducci, Nannicini, and Naticchioni (2008) study the effect on political selection of allowing outside income: if politicians can keep their private business while appointed, and election boosts private returns especially for high-ability citizens, then outside income can induce equilibria with positive sorting, where a wage increase makes the public office relatively more attractive for low-ability citizens. Finally, Besley (2005) introduces one more argument that may explain a negative impact of the wage on politicians' quality: if public service motivations are strong, a higher remuneration lowers the relative attractiveness of politics for public-spirited individuals.

2.2 Empirical studies

Despite this rich set of theoretical predictions, there are only a few empirical studies on the impact of politicians' remuneration. 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

[3]Messner and Polborn (2004) come to a similar conclusion, although in their case the rationale is that competent candidates have an higher incentive to free-ride on mediocre candidates, under the assumption that the attractiveness of public life is low.

per capita income and taxes. 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 the citizens—as measured by established surveys—is positively associated with the governor's wage. Diermeier, Keane, and Merlo (2005) estimate a structural dynamic model of congressional careers in the US, finding that congressional experience significantly increases post-congressional wages in the private sector. Keane and Merlo (2007) use the same model 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, those who perform better in terms of legislative and policy goals.

An empirical exercise similar to ours was presented, independently, by Ferraz and Finan (2008). To the best of our knowledge, this is the only other paper that builds on a clear exogenous variation in the pay of elected officials. 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; and they find that legislative productivity—measured as the number of bills submitted and approved—increases with the salary. Despite the similarity between the two approaches, however, our paper is distinct 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 size. Second, we focus on the mayor as the chief executive of the municipality, and then we look at budget indicators as performance outcomes. Third—and most important—we disentangle between the *composition* and the *incentive* effect of the wage on performance, exploiting the existence of a two-term limit for Italian mayors.

To a lower extent, our paper also relates to other strands of the political economics literature. Some recent studies have implemented RDD exercises based on policies that vary with population size at the local level, in order to estimate the effect of the number of legislators on the size of government (Petterson-Lindbom, 2008), the effect of the electoral rule on economic policy (Bordignon and Tabellini, 2008; Chamon, de Mello, and Firpo,

2008), or the effect of direct versus representative democracy (Petterson-Lindbom and Tyrefors, 2009). Our results could also be compared with studies of the effect of pay in the civil service on corruption (Besley and McLaren, 1993; Van Rijckeghem and Weder, 2001), although we look at elected officials and focus on administrative IQ rather than honesty. Finally, as we make the assumption that a binding term limit wipes out the reelection incentive, our framework borrows insights from the vast literature on political accountability, term limits, and political budget cycles.[4]

3 Evaluation framework

3.1 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 political selection and the in-office performance of politicians. The theoretical background is the efficiency wage theory and the political economics literature reviewed in the previous section. In particular, we want to assess whether the simple efficiency-wage predictions—as opposed to some of the different views on paying politicians voiced in political economics—have an empirical counterpart in the labor market for politicians. To be more precise, we want to test the following hypotheses.

(H1) A higher wage attracts more citizens with high opportunity costs into politics, that is, more skilled individuals with lofty 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 different channels:

- (H2.1) a higher wage attracts more skilled individuals into politics (*composition effect of the wage on performance*);
- (H2.2) a higher wage increases the cost of not being reelected (*incentive effect of the wage on performance*).

[4]See—among others—Rogoff (1990), Besley and Case (1995), Maskin and Tirole (2004), List and Sturm (2006), Smart and Sturm (2006), Ferraz and Finan (2007), and Brender and Drazen (2008).

For the moment we disregard the pure motivational effect of the wage on performance, because it cannot be easily identified in our data. If there is any, as we discuss later in this section, it will complement the composition effect of the wage on performance. In the following, we show how to test (H1) and (H2), and disentangle (H2.1) from (H2.2).

A major empirical difficulty in identifying the effect of politicians' remuneration on their selection and performance is the absence of a truly exogenous variation in the amount they are paid. On the one hand, by comparing remunerations across countries—or across regions and cities within the same country—a number of place-specific observed and unobserved characteristics may confound the effect of the wage, like the size of the population, the size of the territory, labor market conditions, or any other historical heritage. On the other hand, remuneration changes across time may be endogenously associated with both political selection and the behavior of elected officials. It is not hard to imagine rent-seeking politicians raising their own salary, or righteous representatives giving up part of their remuneration to prove their public-spiritedness. Furthermore, high-quality politicians may be able to obtain higher wages (Di Tella and Fisman, 2004). To overcome these problems, we exploit the source of exogenous variation arising from the Italian policy of paying mayors according to the population size of their city.

Define X_i as the characteristics of citizens who run for mayor in town i; Y_i as some performance indicator for the mayor; 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 $W_i = W_l < W_h$. To formalize the idea that both the characteristics of the political elite 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 $k \in \{l, h\}$, as the potential characteristics of politicians in town i if the wage were equal to W_k. Similarly, $Y_i(W_k) \equiv Y_{ik}$, with $k \in \{l, h\}$, captures the potential performance of the mayor in town i if the wage were equal to W_k. As all of these variables are town-specific, in the following we omit the subscript i.

For each town, we either observe X_l and Y_l or X_h and Y_h, according to the wage schedule actually offered to the mayor. Potential outcomes, however, are defined for every town, and the estimand of interest is the average treatment effect for the entire population or for a subpopulation of cities (Ω): $E[X_h - X_l | i \in \Omega]$ and $E[Y_h - Y_l | i \in \Omega]$. The conditional

comparison of the observed X and Y in towns with $W = W_l$ against towns with $W = W_h$ does not generally provide an unbiased estimate of the average treatment effect, because towns with different unobservable characteristics may endogenously choose the mayor's remuneration as discussed above. The fact that in Italy the salary of mayors depends on the population size, however, can be exploited to implement a sharp RDD and estimate the causal effect of the wage on X and Y. We need to make the following assumptions.[5]

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

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

In other words, the potential characteristics of the political elite and the potential performance of the mayor, 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 other 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 use the same population threshold.

Under Assumption 1, it is straightforward to show that $E[X_l|P = P_c] = \lim_{P\uparrow P_c} X$ and $E[X_h|P = P_c] = \lim_{P\downarrow P_c} X$. We can thus identify the treatment effect of the wage on political selection as:

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

Similarly, under Assumption 2, we can show that $E[Y_l|P = P_c] = \lim_{P\uparrow P_c} Y$ and $E[Y_h|P = P_c] = \lim_{P\downarrow P_c} Y$, so as to identify the treatment effect of the wage on performance:

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

Both τ_{sel} and τ_{per} are defined as local effects, because they capture the impact of the wage only for towns around the threshold P_c. As usual in RDD, the gain in internal validity comes at the price of lower external validity.

[5]See Hahn, Todd, and Van der Klaauw (2001) for a discussion of identification assumptions in RDD.

3.2 Disentangling incentives from selection

To empirically disentangle (H2.1) and (H2.2) as alternative explanations of the impact of the wage on politicians' performance, we need to introduce further notation and assumptions. Rewrite potential performance in the following additive form: $Y_k = S(X_k) + v_k + I_k$, with $k \in \{l, h\}$, where the function $S(.)$ captures the impact of the potential observable characteristics X_k on performance, v_k the impact of potential unobservable characteristics, and I_k represents the incentive effect associated with the wage W_k. For example, if $v_h > v_l$, citizens attracted to politics by W_h have unobservable skills that enhance their in-office performance with respect to 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:

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

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

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. This implies that, for mayors at the second term, reelection is no longer a feasible goal. We can thus introduce the following assumption.

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

This does not mean that mayors at 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 crucial point is that all of these incentives do not depend on the wage, because reelection as a mayor no longer belongs to the opportunity set.

If T is the number of previous consecutive terms, at $T = 0$ the term limit constraint is slack, while at $T = 1$ it is binding. Potential outcomes now depend not only on W but also on T: Y_{kj}, with $k \in \{l, h\}$ and $j \in \{0, 1\}$. And, under Assumption 3, they can be summarized as follows.

	$W = W_l$	$W = W_h$
T=0	$Y_{l0} = S(X_{l0}) + v_{l0} + I_l$	$Y_{h0} = S(X_{h0}) + v_{h0} + I_h$
T=1	$Y_{l1} = S(X_{l1}) + v_{l1} + exp$	$Y_{h1} = S(X_{h1}) + v_{h1} + exp$

Here, exp stands for administrative experience and we are assuming that its effect on performance is independent of the wage schedule. The above table shows that the skills of politicians are different, if we compare mayors in their first term with mayors who have already been reelected once. In particular, as long as performance is relevant for reelection, we expect mayors at $T = 1$ to be more skilled according to both observable and unobservable characteristics. In general: $S(X_{k0}) \neq S(X_{k1})$ and $v_{k0} \neq v_{k1}$, with $k \in \{l, h\}$. If we restrict the analysis to a sample of politicians elected for two consecutive terms, however, we observe the same guys at both $T = 0$ and $T = 1$. In such a restricted sample, we have that indeed: $S(X_{k0}) = S(X_{k1}) = S(X_k)$ and $v_{k0} = v_{k1} = v_k$.

In this context, we can identify the overall effect of the wage on performance as:

$$\tau_{per} = E[Y_{h0} - Y_{l0}|P = P_c] = \lim_{P\downarrow P_c|T=0} Y - \lim_{P\uparrow P_c|T=0} Y, \tag{3}$$

where the first equality follows from Assumption 3 and the fact that we are only dealing with two-term politicians, while the second equality follows from Assumption 2.

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

$$\sigma_{per} = E[Y_{h1} - Y_{l1}|P = P_c] = \lim_{P\downarrow P_c|T=1} Y - \lim_{P\uparrow P_c|T=1} Y, \tag{4}$$

$$\begin{aligned} \phi_{per} &= E[(Y_{h0} - Y_{h1}) - (Y_{l0} - Y_{l1})|P = P_c] = \\ &= \left(\lim_{P\downarrow P_c|T=0} Y - \lim_{P\uparrow P_c|T=0} Y \right) - \left(\lim_{P\downarrow P_c|T=1} Y - \lim_{P\uparrow P_c|T=1} Y \right). \end{aligned} \tag{5}$$

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

To leave the framework as simple as possible, so far we have not contemplated the pure motivational effect of an increase in the salary on performance (Akerlof, 1982). Experimental evidence suggests this effect being relatively small (Gneezy and List, 2006). If there were any, however, 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 of the wage on performance. We might worry the latter effect being particularly important in the political arena, where work norms are more effective since mayors' finances are always under the spotlight. If so, σ_{per} should then be interpreted as a broader complement of the incentive effect.

3.3 Estimation and validity tests

In order to test (H1), (H2), (H2.1), and (H2.2), we need to implement equations (1), (3), (4), and (5) in some way. Basically, this is a problem of estimating the boundary points of two (or four) regression functions. Various semiparametric and nonparametric methods have been proposed for that purpose. We first apply the local linear regression approach as suggested by Imbens and Lemieux (2008). This method restricts the estimation to a compact support, and fits linear regression functions to the observations distributed within a distance h on either side of the threshold. In other words, to implement equation (1), we restrict the sample to towns in the interval $P_i \in [P_c - h, P_c + h]$ and estimate the model:

$$X_i = \delta_0 + \delta_1 P_i^* + D_i(\delta_2 + \delta_3 P_i^*) + \eta_i, \tag{6}$$

where X_i captures some observable trait of the mayor or candidates, D_i is a treatment dummy equal to one if $P_i \geq P_c$, and the normalized variable $P_i^* = P_i - P_c$ allows us to interpret δ_2 as the jump between the two regression lines at the threshold P_c (i.e., at $P_i^* = 0$). As a result: $\tau_{sel} = \delta_2$. We select the bandwidth h in two ways: applying cross-validation methods (Ludwig and Miller, 2007); choosing the maximum symmetric bandwidth forced by the fact that there are different policy thresholds below or above P_c and we do not want our sample to cross them.[6] As the same city is observed in different mayoral terms, we control for intra-city correlation in the error term η_i.

[6]The cross-validation method consists in choosing h so as to minimize the loss function: $CV_X(h) = \frac{1}{N}\sum_{i=1}^{N}(X_i - \hat{X}_h(P_i))^2$, where the predictions $\hat{X}_h(P_i)$ are retrieved as follows: for every P_i to the left (right) of the threshold P_c, we predict the value of X as if it were at the boundary of the estimation, using only observations in the interval $[P_i - h, P_i]$ ($[P_i, P_i + h]$). Following Imbens and Lemieux (2008), we discard 50% of the observations on either side of the threshold.

In a similar way, to implement equations (3), (4), and (5), we fit two different linear regression functions on both sides of the threshold P_c: one for politicians *without* a binding term limit ($T = 0$) and one for politicians *with* a binding term limit ($T = 1$). The jump in the regression lines for the subsample $T = 0$ can be interpreted as an estimate of τ_{per}, while the jump in the regression lines for the subsample $T = 1$ can be seen as an estimate of σ_{per}. The difference between the two jumps produces an estimate of ϕ_{per}. Formally, we define S_i as a dummy equal to one if the term limit constraint is slack ($T = 0$) and equal to zero if the term limit is binding ($T = 1$). We then restrict the sample to cities in the interval $P_i \in [P_c - h, P_c + h]$ and estimate the model:

$$Y_i = \delta_0 + \delta_1 P_i^* + D_i(\delta_2 + \delta_3 P_i^*) + S_i[\delta_4 + \delta_5 P_i^* + D_i(\delta_6 + \delta_7 P_i^*)] + \xi_i, \tag{7}$$

where Y_i is some performance indicator for the mayor, D_i the treatment, and P_i^* the normalized population size. Standard errors are clustered at the city level. It is straightforward to show that the overall effect of the wage on performance is $\tau_{per} = \delta_2 + \delta_6$ (when $S_i = 1$), while the composition effect on performance is $\sigma_{per} = \delta_2$ (when $S_i = 0$). It follows that the incentive effect on performance is $\phi_{per} = \tau_{per} - \sigma_{per} = \delta_6$.

An alternative to the local linear regression approach is to use the whole sample and choose a flexible functional form specification to fit the relationship between X_i and P_i on either side of the threshold. Specifically, we can replace equation (6)—and, accordingly, equation (8)—with the following:

$$X_i = (\delta_0 + \delta_1 P_i^* + ... + \delta_p P_i^{*p}) + D_i(\gamma_0 + \gamma_1 P_i^* + ... + \gamma_p P_i^{*p}) + \eta_i. \tag{8}$$

Usually, a third-grade polynomial ($p = 3$) is used in the empirical literature. This method is attractive for many reasons, although a possible concern is that it may be sensitive to outcome values for observations far away from the threshold. We will implement it as a robustness check of the local linear regression results.

A number of additional tests can evaluate the validity of the RDD assumptions. First, to formally check for the absence of manipulation of the running variable around P_c (violated if mayors were able to alter population size and sort above the threshold), we test the null hypothesis of continuity of the density of P at P_c, as proposed by McCrary (2008). Second, as treatment assignment should be as good as random at the threshold, we check

for the balancing of invariant city characteristics (such as area size and geographical location) just below and above P_c. Third, we perform placebo RDD estimations at fake discontinuity points, expecting to detect zero treatment effects.

4 Data

4.1 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 by voting with the majority rule. Since March 1993, mayors are directly elected with plurality rule (single ballot below 15,000 inhabitants and dual ballot 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 also reduced from five to four years, then restored to five years in 2000. Italian municipalities—alone or in association—are in charge of a vast number of services, from water supply to waste management, from municipal police to certain infrastructures, from housing to welfare policies.

The compensation of a mayor depends on the size of the resident population in the municipal area, as measured in the national Census that takes place every ten years, and sharply changes at nine different thresholds.[7] Nominal salaries have been adjusted almost every year to account for price inflation, so that real values within each class of population have remained almost unchanged from 1993 to 2007 (see Table 1). Since adjustments were applied uniformly to all municipalities, the relative wage between classes of population has also remained identical.[8] As of 2000, the real gross wage 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. Under specific and documented circumstances, the executive committee can increase the salary of the mayor by an additional 15%, subject to the approval of the Ministry of Internal Affairs. Importantly, mayors can keep their private

[7] For the period covered in our sample, the two reference Censuses were held in 1991 and 2001.

[8] Legislative references in hierarchical and chronological order: *Legge* 816/1985, *Legge* 81/1993, and *Legge* 265/1999; *Decreti del Ministero dell'Interno* April 11th 1988, April 2nd 1991, July 4th 1994, March 12th 1997, and April 4th 2000.

job while appointed. However, in case the mayor works as an employee, the salary is cut by half unless the mayor asks for a leave-of-absence for the duration of the mandate.[9]

In Table 2, we present a summary of all the policies varying with population size. Besides the salary of the mayor, resident population also determines 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, a fiche between 18 and 36 euros for 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 ballot or dual ballot); whether or not a municipality can have neighborhood councils (which mean additional rents for politicians); and whether or not a municipality is constrained by the Internal Stability Pact (a set of measures intended to restrain local public expenditure), which started in 2002.

As of 2000, the gross salary of the mayor increases at the 1,000 threshold from 1,291 euros to 1,446 euros per month (+12.0%), and from 2,169 euros to 2,789 euros (+28.6%, but +33.3% before 2000) at the 5,000 threshold. 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 1,375 euros per month for men and 1,067 for women, while in cities between 5,000 and 20,000 it was 1,468 and 1,135, respectively.[10] Especially in small cities, it seems that being appointed as mayor provides a significant additional source of income, at least in monetary terms, for a large fraction of the population.

Only three out of the nine wage thresholds determine a variation solely in the compensation 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 of the very small sample size around 50,000 inhabitants (see also Table 3), we will then focus on the 1,000 and 5,000 thresholds.

Before moving to the data, it is worth addressing three specific aspects of the Italian institutional framework that might change, to a certain extent, the interpretation of our results. First of all, the compensation of the members of the executive committee and the

[9]The rationale for this distinction resides in the different tax burden between dependent employment and self-employment. Before 2000, the possibility of having a higher salary for self-employed or on-leave employees was in place only for municipalities above 10,000 inhabitants.

[10]Source: Bank of Italy, Survey of Household Income and Wealth (SHIW), wave 2000. Employed individuals between 18 and 65; gross income (as employee, self-employed, or entrepreneur) recovered by increasing the disposable income available in the SHIW by the corresponding tax rate.

councillors changes along with the compensation of the mayor. Although this effect might be interesting *per se* (that is, the effect of an increase in the salary of all the members of a municipal government), we cannot separately identify the effect of a change in the compensation of the mayor only. However, the magnitude of the compensation of the executive committee and the councillors at the 1,000 and 5,000 thresholds is very small (between 15% and 20% of the mayor's salary for the members of the executive committee, and 18 euros per session for the councillors). Hence, it is plausible to assume that the main effect of increasing the remuneration of elected officials is actually driven by the mayor being paid more, rather than the rest of politicians in the municipal government.

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.[11] In this case, as we discussed in Section 2.1, top earners might find it profitable to enter politics in order to increase their visibility and boost private returns, and a wage increase might have the undesirable effect of crowding them out. Our estimate will therefore be the direct effect of making the office more profitable for citizens with higher opportunity costs, net of the indirect effect of making the public office relatively more attractive for low-skilled citizens. As far as heading a municipal government involves some minimum amount of time that cannot be otherwise dedicated to private activities, we might expect the indirect effect being relatively smaller than the direct effect in the present setting.

Finally, the executive committee can grant an additional 15% increase to the mayor. This, of course, would change the interpretation of the estimated effects. Suppose for example that all municipalities above the threshold chose (conditional to the approval of the Ministry of Interiors) to increase the salary, while all municipalities below the threshold did not. In this case, we would be estimating the effect of a 27% wage increase around 1,000 and 48% around 5,000. In the opposite case, where all the municipalities above the threshold increase the salary, while the others do not, there would be no wage increase around 1,000 and a 18% increase around 5,000. In the more realistic case in which the decision to increase the salary of a mayor is equally distributed above and below the

[11] Strict incompatibilities apply instead to any appointment in companies or entities under the control of the municipality (see *Decreto Legislativo* 267/2000).

thresholds, and determined by factors which are independent from population size, nothing would change.

4.2 Sample selection and variables of interest

The original data set contains all the mayoral terms elected from 1985 to 2007 for all the Italian municipalities. It carries information about gender, age, highest educational attainment (self-declared), political affiliation, and previous job (self-declared) of the elected mayor, the losing mayoral candidates, the members of the executive committee, and the members of the council. From 1993, it also provides yearly information at the municipality level about the budget components (i.e., subcategories of revenues and expenditure), as well as some financial and administrative indicators.[12]

We first dropped all mayoral terms elected before 1993, because we lack the budget information for those years. But also because the already mentioned electoral reform introducing direct election makes the post-1993 Italian mayors fit particularly well with Besley's (2004, p.210) recommendation that agency models on paying politicians "are most promising when applied in situations where there are directly elected chief executives with significant discretionary power." We then ended up with a sample of about 8,000 municipalities: 7,956 as of 1991 and 7,741 as of 2001 (see Table 3).[13] The territory is very fragmented, with the majority of the municipalities having a population between 1,000 and 3,000 (about 33.48% as of 1991, and 32.84% as of 2001), or below 1,000 (24.06% as of 1991, and 24.43% as of 2001). It is also worth noticing that no much changed in the population distribution moving from the 1991 to the 2001 Census data.

In Table 4, we pool together all the terms and summarize the characteristics of both elected mayors and the three best candidates (including the mayor) for whom we have non-missing information—23,946 mayors and 43,406 total candidates—by population size.[14] On average, 7% of the mayors are women, aged 47.4, and with about 13.9 years of schooling

[12]The individual-level data were provided by the Statistical Office of the Italian Ministry of Internal Affairs, while all the town-level data by ANCI (*Associazione Nazionale Comuni Italiani*).

[13]The number of municipalities repeatedly changed over time because of administrative agglomerations and separations.

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

(i.e., high-school level). Almost 13% were not employed (either unemployed or out of the labor force) before being appointed, while 46% were employed in high-skilled occupations (lawyers, professors, physicians, self-employed, and entrepreneurs), and 19% 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, mayors are more educated, less likely to be non-employed or low-skilled, and more likely to be high-skilled. These patterns are likely to be the result of a selection process over the pool of candidates. Accordingly, we also observe similar levels and trends for all the candidates in the electoral race.

As budget indicators we use the following variables per capita, only available until 2005: deficit, total expenditure, and total revenues. To assess budget management and priorities, we also look at the following items: 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, or tariffs. All variables are averaged over the term, excluding election years to avoid capturing a lame-duck effect or the confounding impact of political budget cycles. Since we could not compute the average for terms elected after 2001, and because of some missing observations, the sample for the budget information is sensibly smaller: 14,212 observations.[15]

Table 5 contains descriptive statistics of the budget indicators, in 2000 real terms. On average, total expenditure amounts to 1,402.61 euros per capita, total revenues to 1,382.95 euros, and the average deficit to 19.66 euros. Both revenues and expenditure have a U-shaped relationship with population size: they decrease at first, possibly because of economies of scale in running the administrative machine, and then rise again for cities above 50,000. When we look at the composition of revenues and expenditure, we can see that 41% of the expenditure is used to cover investments or other capital outlays (570.24), 29% to cover personnel costs and the debt service (404.89 euros), and the remaining 30% for goods and services (428.13). As for revenues, 70% are made of transfers (961.79 euros), while 19% are local taxes (265.16), and 11% are tariffs for municipal services (156.01).

[15] All the results we will present on budget performance are robust to the use of values for the last year fully in office.

For the reasons discussed in the previous section, we restrict our attention to the 1,000 and 5,000 thresholds. Since the 1,000 threshold was introduced only in 2000, we kept mayoral terms elected after 1999 and with population size below 2,000 (the maximum symmetric bandwidth around 1,000).[16] Similarly, because of the introduction of the Internal Stability Pact at the 5,000 threshold after 2001, we kept all the mayoral terms elected before 2002 and with population size between 3,500 and 6,500 (the maximum symmetric bandwidth around 5,000). The final sample is made of 6,395 cities around the 1,000 threshold and 2,589 cities around the 5,000 threshold.

5 Econometric results

5.1 Testing for nonrandom sorting above the threshold

In this section, we assess the validity of the identification strategy discussed in Section 3 with two different testing procedures. First, we investigate the smoothness of the running variable (that is, population size) around the 1,000 and 5,000 thresholds. This test is particularly relevant at 5,000 inhabitants, because the 1,000 threshold was only introduced in 2000 and it is unlikely that municipalities had time for manipulation (the last Census taking place in 2001). Second, we check whether all of the pre-treatment characteristics of the municipalities are balanced in the neighborhood of the two thresholds.

In Figure 1, we plot the frequency of municipalities with less than 20,000 inhabitants, using different binsizes (100, 250, and 500 inhabitants) for the 2001 Census.[17] We can see that the distribution is positively skewed, with a pick around 700. Visual inspection does not reveal any clear discontinuity around the 1,000 and 5,000 thresholds, 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. 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 the thresholds by attracting citizens to their territory from other towns (e.g., by means of urbanization

[16] We actually kept all mayoral terms starting 1999, because the policy was announced in April 1999 and might have had an effect on the 1999 elections too.

[17] We do not consider municipalities above 20,000 inhabitants because of the small sample size in that range. Figures for the 1991 Census are identical and are available upon request.

plans or tax rebates for owners who acquire official residence in the municipality). For this reason, in Figure 2 and Figure 3, we investigate more deeply the shape of the running variable around the 1,000 and 5,000 thresholds. Some variability can be detected, but this seems to be mostly sample noise rather than manipulative sorting.

We formally test for the presence of a density discontinuity at different policy thresholds in Figure 4, 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 (McCrary, 2008). As we can see from the figure, the log-difference between the frequency to the right and to the left of the threshold is not statistically significant at 1,000 and 5,000. In fact, the point estimate at 1,000 is -0.099 (standard error 0.135), and the point estimate at 5,000 is -0.001 (standard error 0.208).[18] We are aware that a density test may have low power if manipulation has occurred on both sides of the threshold. In that case the monotonicity assumption does not hold, and there might be nonrandom sorting around the threshold although this would not be detected in the distribution of the running variable. However, we do not know of any reason why mayors may want to sort just below 1,000 or 5,000, while the wage policy provides an incentive for mayors to sort above these thresholds. The evidence of no sorting just above 1,000 and 5,000 is thus reassuring: mayors are not willing or able to invest resources to manipulate population size. On the contrary, we may expect a broader interest for twisting the population size at other thresholds, because there the policy concerns the whole political elite instead of just the mayor. Indeed, Figure 4 highlights some manipulation at the 3,000 and 10,000 thresholds, where there are broader interests at stake, and local politicians may have coordinated their efforts to attract new residents in order to increase the number of political offices.

In Table 6 and Table 7, we further check for manipulative sorting by performing balance tests of the available pre-treatment characteristics. If there were nonrandom sorting, we should expect some of these characteristics to differ systematically between treated and untreated municipalities. The only available pre-treatment characteristics are the size of the geographical area and the location, because all the rest is endogenous to the policy. The balance tests are performed using the same procedure of the McCrary test, with

[18] The optimal bandwidth is 266.73 at 1,000 and 628.36 at 5,000, while the optimal binsize is 17.67 and 54.47. We thank Justin McCrary for providing us with the Stata codes to perform this test.

separate weighted kernel estimations on both sides of the discontinuity point.[19] No pre-treatment characteristics show a significant discontinuity at the threshold. In particular, the geographical location, which in Italy might be correlated with social capital and administrative culture, is perfectly balanced.[20] Importantly, even the political party affiliation of the mayor is well balanced around the threshold. Although this is not a pre-determined characteristic, it is reassuring to find that it is balanced as well, because it guarantees that the differences we may find in budget performance are not due to different political views on the way fiscal policy should be conducted. In the end, from the McCrary and balance tests, we confidently conclude that—around 1,000 and 5,000 inhabitants—being just below or above the threshold is purely random.

5.2 The effect of the wage on political selection

In this section, we analyze whether paying politicians more affects political selection. In Table 8 and Table 9, we look at whether a higher remuneration has an effect on the quality of the elected mayors and the other candidates standing for office. Specifically, we look at the characteristics of the mayor or the best three candidates, and estimate equation (6) with both the maximum and optimal bandwidth at the 1,000 and 5,000 thresholds.

As we can see in Table 8, the 12% wage increase at the 1,000 threshold does not have a strong effect on the quality of elected mayors. We only detect a reduction of 3.9 percentage points (significant at the 10% level) in the share of mayors who are employed in low-skilled occupations. This amounts to a reduction of about 13.4% for low-skilled occupations (with respect to the below-1,000 average of 0.29). The impact on the years of schooling of mayors, as well as their gender and age, is not statistically significant. Similar results hold when we restrict the sample to the optimal bandwidth (6.9 percentage points reduction in the fraction of low-skilled mayors, significant at the 5% level), and when we look at the best three candidates in the electoral race (5.4 percentage points reduction of low-skilled candidates, statistically significant at the 5% level but only with the optimal bandwidth). Note also the similarity with the plots in Figure 5 and Figure 6, where we

[19] In each test, we use a binsize equal to 10 and three different bandwidths (125, 150, and 175).

[20] Indeed, Nannicini (2009) finds that manipulative sorting at the 3,000, 10,000, 15,000, and 30,000 thresholds only take place in areas with low social capital. Also in those areas, however, there is no detected manipulation at 1,000 and 5,000.

draw scatters of the observed values, plus a running-mean smoothing performed separately on either side of the threshold. Also there, excluding low-skilled occupations, no relevant jump is visible, still remaining an overall increment in the fraction of educated and high-skilled candidates as far as the population size increases.

We find a more sizable effect when the salary of the mayor increases by 33% (28% after 2000) at the 5,000 threshold, where better paid mayors turn out to be more educated (0.581 or 0.879 years of schooling more, depending on the bandwidth). This amounts to an increase of about +4-6% with respect to the average value of 14.28 years in the 3,000-5,000 population bracket. With the optimal bandwidth, we also detect some positive effect on the proportion of high-skilled mayors (9.8 percentage points more, +19.6% with respect to the 3,000-5,000 average of 0.50). Similarly, the impact on candidates' years of schooling is positive and always significant at the 1% level (0.592 or 0.906 years more, depending on the bandwidth). And again the fraction of high-skilled workers is higher (although this effect is significant with the optimal bandwidth only). Candidates are also younger just above the threshold (1.2 years less, -2.5% with respect to the 3,000-5,000 average of 46.94 years). These results are consistent with the plots in Figure 7 and Figure 8, where the sharp jump in years of schooling is particularly evident when moving from the left to the right of the threshold.

In Table 10 and Table 11, we perform two types of robustness checks. First, we implement equation (8) and use a split polynomial function (third grade) over the sample included in the maximum symmetric bandwidth. This is a way to check whether results are sensitive to the use of a linear functional form. As we can see, estimates are almost identical to the ones presented in Table 8 and Table 9, both in terms of magnitude and statistical significance. Second, we include in the baseline local linear regression specification the available predetermined variables (that is, geographical size and location) as covariates. If these variables were balanced around the threshold, estimates should be insensitive to their inclusion. Even in this case, we do not find any difference with respect to the baseline estimates.

Finally, in Table 12 and Table 13, we implement placebo tests by estimating the treatment effect at fake thresholds, where there should be no effect. In particular, we look at points that are close to the true thresholds, that is, the median below and above the 1,000

and 5,000 cutoffs, excluding cities within 250 inhabitants from the original thresholds. We then estimate the treatment effect on several variables using local linear regression with the maximum symmetric bandwidth. With a very few exceptions (all at the 10% level), probably due to small sample noise, the effects at the fake thresholds are never significantly different from zero.

To sum up, while the 12% wage increase at 1,000 inhabitants seems not to have a relevant impact on the pattern of political selection, the 33% increase at 5,000 is able to attract more educated candidates, who—not surprisingly—translate into more educated mayors. The effect of the wage on education at the 5,000 threshold is always statistically significant, most of the time at the 1% level. Indeed, if we take into account that in 2000, in municipalities below 5,000 inhabitants, the gross labor income per month for people without (with) a high-school degree was on average 1,137 (1,357) euros, while people with college education earned 1,594 euros, the selection effect of a wage increase of 620 euros for the 5,000 threshold in 2000 is hardly surprising.[21] There is also some evidence that a higher wage attracts politicians previously employed in high-skilled occupations. While we would be tempted to attribute the different effect between the 1,000 and the 5,000 threshold to the diverse intensity of the treatment, we have to acknowledge that the two (local) results refer to different time periods (1999-2007 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 small cities).

5.3 The effect of the wage on budget performance: disentangling incentives from selection

We now investigate whether the remuneration of the mayor affects the way he or she manages the municipality. As outlined in Section 3, we estimate equation (7) to retrieve both the *overall* effect of the wage on performance (identified on mayors with a slack term limit) and the *composition* effect of the wage on performance (identified on mayors with a binding term limit), recovering at the same time the *incentive* effect as the difference between the two. All budget variables are averaged within the term, excluding election

[21] Source: Bank of Italy, Survey of Household Income and Wealth (SHIW), wave 2000. Employed individuals between 18 and 65; gross income (as employee, self-employed or entrepreneur) recovered by increasing the disposable income available in the SHIW by the corresponding tax rate.

years. We perform this exercise only at the 5,000 threshold, since we could not compute average values at the 1,000 threshold, which was only introduced in 2000.

If we look at the overall effect, the first result to notice in Table 14 is that paying a mayor 33% more reduces the size of the municipality budget, as both total expenditure and revenues per capita decrease by a significant amount (-167.252 and -163.619 euros, respectively, in both cases about -15% with respect to the 3,000-5,000 averages of 1,115.75 and 1,135.76 euros). Results are even stronger when we use the optimal bandwidth (-224.246 and -188.134 euros, respectively). Looking at expenditure subcategories, we can see that the budget reduction is mostly driven by a significant cut in expenditure for goods and services (-87.823, -22.2% with respect to the 3,000-5,000 average of 395.24), while for investment the reduction is never statistically significant. The same holds for personnel plus debt service outlays, which are indeed more rigid. As for collected revenues, there is a consistent reduction in taxes and, especially, tariffs (-30.990 and -101.540 euros, respectively, -12.6% and -75% with respect to the 3,000-5,000 averages of 246.56 and 135.09 euros), while there is no significant evidence of a reduction in transfers from other national or European institutions. We find very similar results when using the optimal bandwidth, although the reduction in taxes is no longer statistically significant. This is reassuring that our estimates are not too sensitive to the bandwidth choice. Since the revenue and expenditure forces go in the same direction, the effect on the deficit is not significantly different from zero. A graphical representation of the overall effect of the wage on performance can be found in Figure 9.

Looking at the other estimates in Table 14, 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. As a matter of fact, the incentive effect is never significant, both in size and in statistical terms, irrespectively of whether we use the maximum or the optimal bandwidth. Among mayors with a binding term limit (composition effect), instead, those who are paid more reduce all expenditure components, taxes, and tariffs. In other words, for the reduction in taxes, tariffs, and current expenditure, selection is clearly the driving force behind the overall effect.[22]

[22] As we argued in Section 3.2, mayors at the second term might also have the incentive to perform well because they plan to run for higher offices. In principle, as these other incentives do not depend on the wage anyway, they should not affect our identification strategy, unless they were completely first order.

Although we cannot observe the quality of public goods and services provided at the municipality level, the above evidence is consistent with the fact that the 33% wage increase at 5,000 attracts skilled citizens, who then run the government machine more cautiously: they lower the tax and tariff burden over their citizens, by reducing sources of waste in current outlays, while leaving investments unchanged (that is, increasing the ratio of capital over current expenditure). Empirical evidence about Italy shows that passive waste—that is, inefficiency to to red tape—is indeed concentrated on current outlays for goods and services at the local level (Bandiera, Prat and Valletti, 2009). The fact that personnel expenditures are not significantly affected might be related to the strict Italian regulation that makes firing public employees almost impossible. An alternative interpretation of the results is that the reduction in government size might reflect differences in preferences: 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 (see the balance tests in Table 7). And voters could accept the implicit policy change in exchange for the greater competence of these politicians.

Furthermore, we do not find evidence of any additional effect on first-term mayors willing to gain reelection. Actually, in the first term, better paid politicians seem to "pander" more to voters (Maskin and Tirole, 2004) by increasing both expenditure and transfers, but these effects are never statistically significant.[23] An alternative explanation of this result could be that Italian voters have strong ideological preferences ("party alignment"), and this makes the threat of non-reelection for the incumbent mayor less credible. To be sure that this is not the case, in the upper panel of Table 15 we run the same exercise as in Table 14 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 might expect swing voters to decide the electoral race. Even in this case, however, there

We actually observe that, among the mayors with a binding term limit and in municipalities between 3,500 and 6,500 inhabitants, only 5.3% of the mayors were then appointed in the provincial government after the term limit, 1.8% in the regional government, and 0.4% in the national parliament. Furthermore, we do not detect any difference above and below the 5,000 threshold.

[23]It is worth noticing that 66% of mayors rerun for a second term, and 78% of them are reelected. As a matter of fact, we also 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 the probability of being reelected.

is no evidence that a higher wage has an impact on the willingness to be reelected, which makes us think that our result simply reflects the strength of the composition effect over the incentive effect of the wage.

In the framework outlined in Section 3, we have assumed that all mayors without a binding term limit were at their first mandate, while all mayors with a binding term limit were at their second mandate. However, this is not always the case in the data. When the term limit was introduced, it was only applied to terms starting after 1993, no matter how many terms a mayor could have been in office before.[24] For this reason, in our sample, there are some mayors in their third or fourth consecutive term, and their different administrative experience could interact with the estimated effects. In the lower panel of Table 15, we present the same estimates as in Table 14 restricting the sample to mayors elected for the first time after March 1993. The results are almost unchanged. We therefore conclude that differences in experience do not bias our baseline results.

Finally, in Table 16, we perform the same additional robustness checks as for the estimates on political selection: using a split polynomial function (third grade) over the sample included in the maximum symmetric bandwidth; including in the local linear regression estimates the available predetermined variables as covariates. As we can see, estimates are almost identical in terms of magnitude to the ones presented in Table 14. The only difference we find is that results are less precise in statistical terms when using a split polynomial specification, and now there is a negative and significant incentive effect on budget deficit. In Table 17, we also test for the treatment effect at fake thresholds, where there should be no effect. With only one exception (at the 10% level), these jumps are never significantly different from zero.

6 Conclusions

In this paper, we have shown that paying politicians more has a positive effect on the observable skills (that is, education and professional background) of elected officials, and it also affects the way they manage public finance. In particular, better paid politicians lower the size of the municipal government, by reducing taxes and tariffs and, at the same

[24] Another reason for the term limit being binding at the third or following terms is that, because of early termination, some terms lasted less than two years and therefore did not enter the computation.

time, lowering current expenditure. Results also show that this performance 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—which is local in nature as any RDD—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. Still, 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. By doing so, voters could effectively compete with the market in recruiting the brightest citizens.

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Table 1: Mayor's gross monthly wage over time (in 2000 euros)

Year	Population brackets									
	Below 1,000	1,000-3,000	3,000-5,000	5,000-10,000	10,000-30,000	30,000-50,000	50,000-100,000	100,000-250,000	250,000-500,000	Above 500,000
1993	1,227	1,227	1,841	2,455	2,455	2,762	3,375	3,989	4,603	6,137
1994	1,306	1,306	1,959	2,612	2,612	2,939	3,592	4,245	4,898	6,531
1995	1,240	1,240	1,860	2,480	2,480	2,790	3,410	4,030	4,650	6,200
1996	1,190	1,190	1,785	2,381	2,381	2,678	3,273	3,869	4,464	5,952
1997	1,286	1,286	1,929	2,571	2,571	2,893	3,536	4,178	4,821	6,428
1998	1,262	1,262	1,892	2,523	2,523	2,838	3,469	4,100	4,731	6,308
1999	1,241	1,241	1,861	2,482	2,482	2,792	3,412	4,033	4,653	6,204
2000	1,291	1,446	2,169	2,789	3,099	3,460	4,132	5,010	5,784	7,798
2001	1,256	1,407	2,110	2,713	3,014	3,366	4,019	4,873	5,627	7,586
2002	1,226	1,373	2,060	2,648	2,943	3,286	3,924	4,757	5,493	7,406
2003	1,291	1,446	2,169	2,789	3,099	3,460	4,132	5,010	5,784	7,798
2004	1,263	1,415	2,122	2,728	3,031	3,385	4,042	4,901	5,659	7,629
2005	1,238	1,387	2,081	2,675	2,972	3,319	3,963	4,805	5,548	7,480
2006	1,396	1,563	2,345	3,015	3,350	3,741	4,466	5,415	6,253	8,430
2007	1,371	1,535	2,303	2,961	3,290	3,674	4,386	5,318	6,141	8,279

Notes. *Population* is the number of resident inhabitants as measured by the last available Census. The real monthly salary is computed using the OECD CPI index.

Table 2: Legislative thresholds for Italian municipalities

Population	Wage Mayor	Wage Ex. Com.	Fiche Council	Ex. Com. Size	Council Size	Electoral Rule	Neighbor. Councils	Stability Pact (2001)
Below 1,000	1,291	15%	18	4	12	single	no	no
1,000-3,000	1,446	20%	18	4	12	single	no	no
3,000-5,000	2,169	20%	18	4	16	single	no	no
5,000-10,000	2,789	50%	18	4	16	single	no	yes
10,000-15,000	3,099	55%	22	6	20	single	no	yes
15,000-30,000	3,099	55%	22	6	20	dual	no	yes
30,000-50,000	3,460	55%	36	6	30	dual	allowed	yes
50,000-100,000	4,132	75%	36	6	30	dual	allowed	yes
100,000-250,000	5,010	75%	36	10	40	dual	yes	yes
250,000-500,000	5,784	75%	36	12	46	dual	yes	yes
Above 500,000	7,798	75%	36	14-16	50-60	dual	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 the year 2000 and is measured in euros. The wage threshold at 1,000 and 10,000 were introduced in 1999; all of the others date back to 1960. *Fiche Council* is the reimbursement per session paid to councillors and is measured in euros. *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 (thresholds set in 1960). Since 1993, *Electoral Rule* can be either single-ballot (with 60% premium) or dual-ballot (with 66% premium) plurality voting. *Neighbor. Councils* are bodies that represent different neighborhoods within the city and are provided with independent budgets. *Stability Pact* refers to a set of rules decided by the central government to impose fiscal discipline on local authorities (introduced in 2001).

Table 3: Population distribution

Population	1991 Census	2001 Census
Below 1,000	1,914	1,891
	(24.06)	(24.43)
1,000-3,000	2,664	2,542
	(33.48)	(32.84)
3,000-5,000	1,202	1,156
	(15.11)	(14.93)
5,000-10,000	1,145	1,116
	(14.39)	(14.42)
10,000-15,000	406	427
	(5.10)	(5.52)
15,000-30,000	344	343
	(4.32)	(4.43)
30,000-50,000	146	141
	(1.84)	(1.82)
50,000-100,000	90	85
	(1.13)	(1.10)
100,000-250,000	33	27
	(0.41)	(0.35)
250,000-500,000	6	7
	(0.08)	(0.09)
Above 500,000	6	6
	(0.08)	(0.08)
Total	7,956	7,741

Notes. *Population* is the number of resident inhabitants as measured by the last available Census. Percentage values in parentheses.

Table 4: Individual characteristics of elected mayors and all candidates

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
				Mayors		
Below 1,000	0.07	47.86	12.51	0.18	0.32	0.29
1,000-3,000	0.07	47.14	13.79	0.13	0.45	0.19
3,000-5,000	0.06	47.26	14.28	0.12	0.50	0.16
5,000-10,000	0.09	46.95	14.53	0.11	0.53	0.14
10,000-15,000	0.07	47.54	15.02	0.08	0.58	0.13
15,000-30,000	0.07	47.86	15.21	0.08	0.59	0.13
30,000-50,000	0.05	48.09	15.42	0.05	0.62	0.10
50,000-100,000	0.05	48.95	15.94	0.05	0.68	0.09
Above 100,000	0.08	51.96	15.77	0.04	0.74	0.04
Total	0.07	47.42	13.87	0.13	0.46	0.19
Obs.	23,946	23,946	23,946	23,946	23,946	23,946
				All candidates		
Below 1,000	0.08	47.38	12.39	0.19	0.30	0.31
1,000-3,000	0.08	46.94	13.66	0.15	0.43	0.21
3,000-5,000	0.07	46.97	14.13	0.14	0.49	0.18
5,000-10,000	0.09	46.98	14.40	0.13	0.52	0.16
10,000-15,000	0.07	47.50	14.82	0.10	0.55	0.15
15,000-30,000	0.07	47.77	15.12	0.09	0.60	0.14
30,000-50,000	0.06	48.04	15.42	0.07	0.63	0.10
50,000-100,000	0.05	49.54	15.84	0.06	0.69	0.09
Above 100,000	0.08	51.98	15.80	0.07	0.73	0.05
Total	0.08	47.21	13.82	0.14	0.45	0.20
Obs.	43,406	43,406	43,406	43,406	43,406	43,406

Notes. *Population* is the number of resident inhabitants, as measured by the last Census. The other columns report average values. All variables are dummies, except *Age* and *Years of schooling* (both measured in years). *Years of schooling* 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. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. Terms from 1993 to 2007.

Table 5: Budget components per capita

Population	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and Interests	Goods and Services	Total	Transfers	Taxes	Tariffs
Below 1,000	27.01	2,071.85	1,013.92	594.30	465.67	2,044.85	1,558.96	280.04	205.84
1,000-3,000	17.32	1,328.13	548.41	380.25	400.00	1,310.81	926.29	242.62	141.91
3,000-5,000	20.00	1,135.76	416.89	323.62	395.24	1,115.75	734.10	246.56	135.09
5,000-10,000	13.51	1,026.63	309.86	304.60	412.17	1,013.11	603.47	274.74	134.90
10,000-15,000	18.42	1,078.62	303.81	317.90	456.90	1,060.20	621.87	291.04	147.29
15,000-30,000	17.85	1,069.86	278.74	317.57	473.54	1,052.01	599.31	300.20	152.51
30,000-50,000	17.38	1,090.03	264.67	337.50	487.86	1,072.65	617.60	316.67	138.38
50,000-100,000	22.53	1,281.60	328.52	390.07	563.01	1,259.06	768.98	329.99	160.09
Above 100,000	25.65	1,587.29	419.17	465.25	702.87	1,561.64	980.50	407.52	173.62
Total	19.66	1,402.61	570.24	404.89	428.13	1,382.95	961.79	265.16	156.01
Obs.	14,212	14,212	14,212	14,212	14,212	14,212	14,212	14,212	14,212

Notes. *Population* is the number of resident inhabitants, as measured by the last Census. The other columns report average values. All variables are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). *Transfers* refers to external transfers from the central government, the regional government, or the European Union. Terms from 1993 to 2001.

Table 6: Balance tests at the 1,000 threshold

	Area	North/South	Center-right party
		Bandwidth: 125	
Discontinuity	4.816	-0.091	-0.010
	(4.443)	(0.094)	(0.039)
		Bandwidth: 150	
Discontinuity	4.386	-0.085	-0.013
	(3.521)	(0.086)	(0.043)
		Bandwidth: 175	
Discontinuity	4.101	-0.056	-0.017
	(3.477)	(0.084)	(0.035)
Obs.	3,635	3,635	3,635

Notes. Discontinuity of invariant town characteristics at the 1,000 threshold (terms from 1999 to 2007). Weighted kernel estimation on both sides of the threshold, with binsize equal to 10 and bandwidth as specified. *Area* is measured in km^2; *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. Bootstrapped standard errors (200 replications) in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7: Balance tests at the 5,000 threshold

	Area	North/South	Center-right party
		Bandwidth: 125	
Discontinuity	5.866	-0.194	-0.010
	(18.074)	(0.203)	(0.034)
		Bandwidth: 150	
Discontinuity	6.983	-0.193	0.005
	(15.727)	(0.199)	(0.048)
		Bandwidth: 175	
Discontinuity	9.905	-0.199	0.013
	(15.132)	(0.177)	(0.052)
Obs.	851	851	851

Notes. Discontinuity of invariant town characteristics at the 5,000 threshold (terms from 1993 to 2001). Weighted kernel estimation on both sides of the threshold, with binsize equal to 10 and bandwidth as specified. *Area* is measured in km^2; *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. Bootstrapped standard errors (200 replications) in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 8: Political selection, LLR estimates at the 1,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			Maximum bandwidth			
Effect	0.005	0.079	-0.211	0.022	0.004	-0.039*
	(0.014)	(0.528)	(0.179)	(0.020)	(0.026)	(0.023)
Obs.	6,390	6,390	6,390	6,390	6,390	6,390
			Optimal bandwidth			
Effect	-0.001	0.079	-0.121	0.024	0.048	-0.069**
	(0.015)	(0.528)	(0.228)	(0.020)	(0.034)	(0.032)
h	800	1,000	550	900	500	400
Obs.	5,644	6,390	3,972	6,121	3,656	2,936
			All candidates			
			Maximum bandwidth			
Effect	-0.003	0.006	0.014	-0.003	0.015	-0.025
	(0.012)	(0.435)	(0.145)	(0.016)	(0.020)	(0.018)
Obs.	10,415	10,415	10,415	10,415	10,415	10,415
			Optimal bandwidth			
Effect	0.002	0.065	0.171	-0.002	0.003	-0.054**
	(0.013)	(0.476)	(0.183)	(0.019)	(0.022)	(0.026)
h	700	750	550	600	800	400
Obs.	8,212	8,744	6,509	7,133	9,190	4,813

Notes. Effect of the 12% wage increase at the 1,000 threshold on the characteristics of mayors and candidates (terms from 1999 to 2007). Estimates computed with Local Linear Regression (LLR) as in equation (6). The maximum symmetric bandwidth is 1,000; the optimal bandwidth h is chosen with cross-validation methods. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 9: Political selection, LLR estimates at the 5,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			Maximum bandwidth			
Effect	-0.024	-1.238	0.581*	0.009	0.054	-0.041
	(0.023)	(0.877)	(0.299)	(0.028)	(0.048)	(0.033)
Obs.	2,586	2,586	2,586	2,586	2,586	2,586
			Optimal bandwidth			
Effect	-0.012	-1.194	0.879***	0.006	0.098*	-0.046
	(0.034)	(1.003)	(0.346)	(0.030)	(0.054)	(0.038)
h	800	1,200	1,100	1,300	1,200	1,200
Obs.	1,339	2,052	1,904	2,222	2,052	2,052
			All candidates			
			Maximum bandwidth			
Effect	0.002	-1.222*	0.592***	-0.009	0.048	-0.017
	(0.016)	(0.629)	(0.213)	(0.020)	(0.034)	(0.024)
Obs.	5,580	5,580	5,580	5,580	5,580	5,580
			Optimal bandwidth			
Effect	0.005	-1.222*	0.906***	-0.014	0.111**	-0.042
	(0.018)	(0.629)	(0.279)	(0.022)	(0.045)	(0.034)
h	1,300	1,500	900	1,300	900	800
Observations	4,804	5,580	3,294	4,804	3,294	2,892

Notes. Effect of the 33% wage increase at the 5,000 threshold on the characteristics of mayors and candidates (terms from 1993 to 2001). Estimates computed with Local Linear Regression (LLR) as in equation (6). The maximum symmetric bandwidth is 1,500; the optimal bandwidth h is chosen with cross-validation methods. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 10: Political selection, robustness exercises at the 1,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			Split polynomial approximation with maximum bandwidth			
Effect	0.042	0.438	-0.312	0.059	0.010	-0.062
	(0.029)	(0.986)	(0.328)	(0.038)	(0.048)	(0.042)
Obs.	6,390	6,390	6,390	6,390	6,390	6,390
			LLR with optimal bandwidth and covariates			
Effect	-0.001	0.051	-0.111	0.023	0.047	-0.066**
	(0.015)	(0.525)	(0.218)	(0.020)	(0.034)	(0.032)
h	800	1,000	550	900	500	400
Obs.	5,644	6,390	3,972	6,121	3,656	2,936
			All candidates			
			Split polynomial approximation with maximum bandwidth			
Effect	0.020	0.014	0.037	0.033	0.018	-0.068**
	(0.024)	(0.807)	(0.265)	(0.030)	(0.038)	(0.034)
Obs.	10,415	10,415	10,415	10,415	10,415	10,415
			LLR with optimal bandwidth and covariates			
Effect	0.002	0.069	0.158	-0.001	0.004	-0.051*
	(0.013)	(0.473)	(0.173)	(0.019)	(0.022)	(0.026)
h	700	750	550	600	800	400
Obs.	8,212	8,744	6,509	7,133	9,190	4,813

Notes. Effect of the 12% wage increase at the 1,000 threshold on the characteristics of mayors and candidates (terms from 1999 to 2007). First robustness exercise: 3^{rd} order polynomial approximation on either side of the threshold as in equation (8); the maximum symmetric bandwidth is 1,000. Second robustness exercise: Local Linear Regression (LLR) with optimal bandwidth h and invariant town characteristics (*Area* in km^2 and *North/South* dummy) as additional covariates. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 11: Political selection, robustness exercises at the 5,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			Split polynomial approximation with maximum bandwidth			
Effect	0.042	-0.152	1.670***	-0.063	0.204**	-0.092
	(0.045)	(1.761)	(0.600)	(0.054)	(0.097)	(0.069)
Obs.	2,586	2,586	2,586	2,586	2,586	2,586
			LLR with optimal bandwidth and covariates			
Effect	-0.009	-1.083	0.797**	0.008	0.091*	-0.036
	(0.033)	(0.998)	(0.342)	(0.030)	(0.054)	(0.038)
h	800	1,200	1,100	1,300	1,200	1,200
Obs.	1,339	2,052	1,904	2,222	2,052	2,052
			All candidates			
			Split polynomial approximation with maximum bandwidth			
Effect	0.009	-0.298	1.256***	-0.047	0.146**	-0.078
	(0.031)	(1.259)	(0.448)	(0.038)	(0.073)	(0.048)
Obs.	5,580	5,580	5,580	5,580	5,580	5,580
			LLR with optimal bandwidth and covariates			
Effect	0.010	-1.095*	0.786***	-0.007	0.105**	-0.037
	(0.017)	(0.617)	(0.270)	(0.021)	(0.044)	(0.033)
h	1,300	1,500	900	1,300	900	800
Obs.	4,934	5,730	3,379	4,934	3,294	2,892

Notes. Effect of the 33% wage increase at the 5,000 threshold on the characteristics of mayors and candidates (terms from 1993 to 2001). First robustness exercise: 3^{rd} order polynomial approximation on either side of the threshold as in equation (8); the maximum symmetric bandwidth is 1,500. Second robustness exercise: Local Linear Regression (LLR) with optimal bandwidth h and invariant town characteristics (*Area* in km^2 and *North/South* dummy) as additional covariates. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 12: Political selection, placebo tests for the 1,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			LLR with maximum bandwidth, median above (1,589)			
Effect	-0.018	0.523	-0.023	-0.011	0.064	0.003
	(0.023)	(0.847)	(0.281)	(0.031)	(0.046)	(0.037)
Obs.	2,084	2,084	2,084	2,084	2,084	2,084
			LLR with maximum bandwidth, median below (444)			
Effect	0.027	-0.812	0.228	-0.014	0.045	-0.012
	(0.024)	(0.909)	(0.278)	(0.033)	(0.039)	(0.039)
Obs.	2,454	2,454	2,454	2,454	2,454	2,454
			All candidates			
			LLR with maximum bandwidth, median above (1,591)			
Effect	0.007	-0.106	0.177	-0.011	0.065*	-0.018
	(0.018)	(0.686)	(0.231)	(0.026)	(0.034)	(0.028)
Obs.	3,608	3,608	3,608	3,608	3,608	3,608
			LLR with maximum bandwidth, median below (453)			
Effect	0.020	-0.534	0.212	0.004	-0.020	0.024
	(0.019)	(0.719)	(0.224)	(0.027)	(0.030)	(0.030)
Obs.	3,781	3,781	3,781	3,781	3,781	3,781

Notes. Estimated discontinuities in the characteristics of mayors and other candidates at fake thresholds (i.e, median above and below the true 1,000 threshold, excluding cities within 250 inhabitants from the original threshold). Terms from 1999 to 2007. The maximum symmetric bandwidth is 1,000. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 13: Political selection, placebo tests for the 5,000 threshold

Population	Female	Age	Years of schooling	Not employed	High-skilled occupation	Low-skilled occupation
			Mayors			
			LLR with maximum bandwidth, median above (5,950)			
Effect	-0.068	1.581	0.623	0.007	0.026	-0.030
	(0.047)	(1.346)	(0.479)	(0.041)	(0.079)	(0.051)
Obs.	935	935	935	935	935	935
			LLR with maximum bandwidth, median below (3,975)			
Effect	0.013	1.061	0.236	-0.066*	0.059	-0.022
	(0.025)	(1.126)	(0.347)	(0.035)	(0.058)	(0.043)
Obs.	1,678	1,678	1,678	1,678	1,678	1,678
			All candidates			
			LLR with maximum bandwidth, median above (5,956)			
Effect	-0.021	1.536	0.206	0.018	-0.030	-0.010
	(0.026)	(0.983)	(0.340)	(0.030)	(0.055)	(0.038)
Obs.	2,071	2,071	2,071	2,071	2,071	2,071
			LLR with maximum bandwidth, median below (3,985)			
Effect	-0.006	0.175	0.178	-0.043*	0.069*	-0.010
	(0.017)	(0.805)	(0.266)	(0.025)	(0.040)	(0.033)
Obs.	3,555	3,555	3,555	3,555	3,555	3,555

Notes. Estimated discontinuities in the characteristics of mayors and other candidates at fake thresholds (i.e, median above and below the true 5,000 threshold, excluding cities within 250 inhabitants from the original threshold). Terms from 1993 to 2001. The maximum symmetric bandwidth is 1,500. *Age* and *Years of schooling* are measured in years; the other variables are dummies. *Not employed* includes unemployed, retired, and any other individual out of the labor force. *High-skilled occupation* includes lawyers, professors, physicians, self-employed, and entrepreneurs. *Low-skilled occupation* includes blue collars, clerks, and technicians. Other types of occupation in the residual category. 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 14: Budget components per capita, LLR estimates at the 5,000 threshold

	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and Interests	Goods and Services	Total	Transfers	Taxes	Tariffs
					Maximum bandwidth				
Overall effect	-3.632	-167.252**	-60.110	-19.319	-87.823***	-163.619**	-31.089	-30.990*	-101.540***
	(7.411)	(65.981)	(50.990)	(14.847)	(25.622)	(65.311)	(53.622)	(16.652)	(35.730)
Selection effect	-0.520	-200.915***	-78.869*	-28.179	-93.867***	-200.396***	-69.533	-34.996**	-95.867***
	(4.150)	(65.798)	(46.465)	(19.546)	(22.670)	(66.862)	(53.027)	(17.126)	(35.438)
Incentive effect	-3.113	33.664	18.759	8.860	6.045	36.777	38.443	4.006	-5.673
	(7.733)	(54.730)	(50.084)	(16.869)	(15.131)	(53.546)	(51.664)	(7.764)	(17.048)
Obs.	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016
					Optimal bandwidth				
Overall effect	-1.881	-224.246***	-69.825	-19.319	-91.290***	-188.134*	-17.442	-28.224	-120.611***
	(8.467)	(68.153)	(44.367)	(14.847)	(28.603)	(102.562)	(65.781)	(17.474)	(42.255)
Selection effect	1.901	-244.020***	-88.863*	-28.179	-97.606***	-247.029***	-46.265	-39.603**	-118.076***
	(4.334)	(79.770)	(53.056)	(19.546)	(26.545)	(81.435)	(53.803)	(17.947)	(44.942)
Incentive effect	-3.782	19.775	19.037	8.860	6.315	27.762	28.823	11.379	-2.535
	(8.508)	(51.646)	(43.118)	(16.869)	(17.706)	(50.253)	(63.145)	(8.256)	(19.412)
h	1,200	1,000	1,100	1,500	1,200	1,000	1,400	1,300	1,100
Obs.	816	696	758	1,016	816	696	950	880	758

Notes. Effect of the 33% wage increase at the 5,000 threshold on budget variables (terms from 1993 to 2001). Mayors observed for two terms, with binding term limit in the second. Estimates computed with Local Linear Regression (LLR) as in equation (7). The maximum symmetric bandwidth is 1,500; the optimal bandwidth h is chosen with cross-validation methods. 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 15: Budget components per capita, robustness exercises (1) at the 5,000 threshold

	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and Interests	Goods and Services	Total	Transfers	Taxes	Tariffs
LLR with maximum bandwidth for non-safe cities									
Overall effect	-11.029	-174.421*	-63.497	-16.720	-94.204***	-163.392	12.646	-32.283	-143.755**
	(8.562)	(100.048)	(78.399)	(22.526)	(35.017)	(99.588)	(80.650)	(21.299)	(56.815)
Selection effect	1.832	-207.388**	-99.619	-12.065	-95.704***	-209.220**	-40.013	-36.696	-132.510**
	(5.239)	(96.024)	(68.973)	(26.841)	(30.497)	(98.131)	(74.707)	(22.548)	(55.030)
Incentive effect	-12.862	32.966	36.122	-4.655	1.499	45.828	52.659	4.413	-11.244
	(8.699)	(83.183)	(76.973)	(24.246)	(20.806)	(81.515)	(78.731)	(10.106)	(26.766)
Obs.	613	613	613	613	613	613	613	613	613
LLR with maximum bandwidth for freshmen after 1993									
Overall effect	-4.658	-194.691**	-100.642	-30.539*	-63.510**	-190.033**	-95.169	-12.365	-82.499**
	(8.661)	(78.982)	(62.910)	(17.889)	(28.892)	(77.755)	(65.327)	(18.731)	(38.513)
Selection effect	-2.444	-211.854***	-95.740*	-35.216	-80.898***	-209.410***	-128.437**	-14.769	-66.204**
	(4.573)	(70.767)	(49.843)	(23.166)	(27.108)	(71.291)	(62.666)	(19.488)	(29.408)
Incentive effect	-2.214	17.163	-4.902	4.677	17.388	19.377	33.268	2.404	-16.295
	(9.281)	(66.743)	(60.263)	(20.123)	(17.172)	(65.209)	(64.130)	(8.954)	(20.279)
Obs.	796	796	796	796	796	796	796	796	796

Notes. Effect of the 33% wage increase at the 5,000 threshold on budget variables (terms from 1993 to 2001). Mayors observed for two terms, with binding term limit in the second. First robustness exercise: sample restricted to mayors elected in non-safe cities (i.e., with less than 55% of votes). Second robustness exercise: sample restricted to mayors elected for the first time after the 1993 reform. The maximum symmetric bandwidth is 1,500. 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 16: Budget components per capita, robustness exercises (2) at the 5,000 threshold

	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and Interests	Goods and Services	Total	Transfers	Taxes	Tariffs
Split polynomial approximation with maximum bandwidth									
Overall effect	-14.009	-240.616*	-90.582	-45.635*	-104.399*	-226.607*	-80.694	-21.732	-124.182
	(11.679)	(136.033)	(102.740)	(24.630)	(54.841)	(136.882)	(112.324)	(33.766)	(83.355)
Selection effect	16.887*	-311.892**	-188.987*	-34.079	-88.826**	-328.779**	-152.804*	-14.598	-161.377
	(8.706)	(141.418)	(100.306)	(37.208)	(41.045)	(146.071)	(91.564)	(33.435)	(105.032)
Incentive effect	-30.896***	71.276	98.405	-11.557	-15.573	102.172	72.111	-7.134	37.195
	(11.620)	(115.244)	(113.603)	(31.078)	(32.660)	(113.531)	(105.902)	(14.042)	(31.190)
Obs.	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016
LLR with optimal bandwidth and covariates									
Overall effect	0.489	-195.643***	-57.650	-10.261	-79.851***	-192.091***	15.587	-26.477*	-117.691***
	(8.586)	(66.569)	(42.147)	(15.733)	(29.126)	(66.638)	(57.861)	(14.907)	(44.621)
Selection effect	3.820	-212.966***	-76.309	-19.877	-82.905***	-217.042***	-12.647	-37.602**	-113.129**
	(4.173)	(79.923)	(53.669)	(19.660)	(26.128)	(81.583)	(49.643)	(15.712)	(47.534)
Incentive effect	-3.782	19.775	19.037	8.860	6.315	27.762	28.823	11.379	-2.535
	(8.518)	(51.721)	(43.176)	(16.886)	(17.728)	(50.326)	(63.213)	(8.266)	(19.438)
h	1,200	1,000	1,100	1,500	1,200	1,000	1,400	1,300	1,100
Obs.	816	696	758	1016	816	696	950	880	758

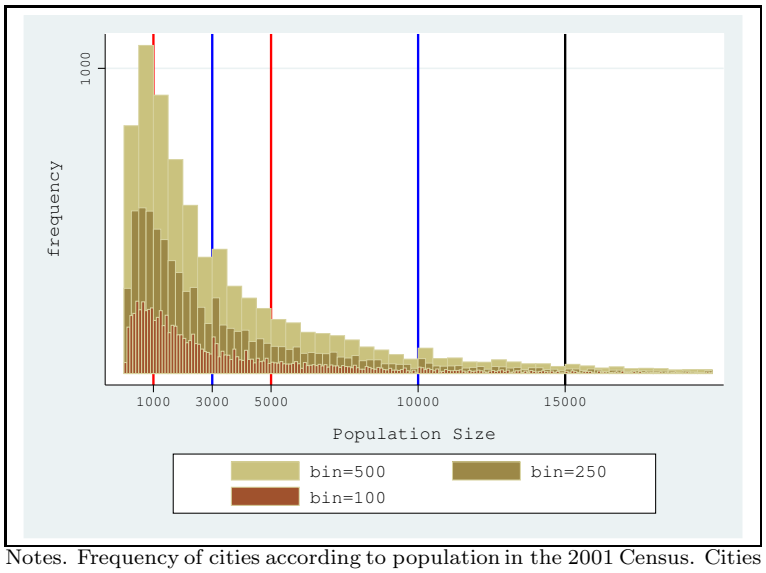
Notes. Effect of the 33% wage increase at the 5,000 threshold on budget variables (terms from 1993 to 2001). Mayors observed for two terms, with binding term limit in the second. First robustness exercise: 3^{rd} order polynomial approximation on either side of the threshold as in equation (8). The maximum symmetric bandwidth is 1,500. Second robustness exercise: Local Linear Regression (LLR) with optimal bandwidth h and invariant town characteristics (*Area* in km^2 and *North/South* dummy) as additional covariates. 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 17: Budget components per capita, placebo tests for the 5,000 threshold

	Deficit	Expenditure				Revenues			
		Total	Investments	Personnel and Interests	Goods and Services	Total	Transfers	Taxes	Tariffs
LLR with maximum bandwidth, median above (5,930)									
Overall effect	7.905	12.800	114.239	-68.196	-33.243	4.895	35.280	-1.264	-29.121
	(10.651)	(181.345)	(155.667)	(63.484)	(50.382)	(180.695)	(178.352)	(37.598)	(37.287)
Selection effect	-2.317	-41.196	-23.919	-13.002	-4.275	-38.878	-45.124	-2.352	8.597
	(7.250)	(101.094)	(58.821)	(34.484)	(48.306)	(102.191)	(84.220)	(39.468)	(38.272)
Incentive effect	10.222	53.996	138.158	-55.194	-28.968	43.773	80.403	1.088	-37.717
	(10.876)	(163.308)	(157.567)	(61.596)	(31.321)	(162.894)	(160.286)	(11.815)	(27.702)
Obs.	330	330	330	330	330	330	330	330	330
LLR with maximum bandwidth, median below (3,980)									
Overall effect	-3.973	-82.090	-115.983	31.049	2.844	-78.117	-90.837	11.272	1.449
	(9.176)	(185.576)	(163.807)	(22.911)	(30.245)	(181.394)	(166.851)	(22.842)	(26.220)
Selection effect	-8.732	-64.575	-42.358	-9.257	-12.960	-55.843	-65.916	16.620	-6.547
	(7.648)	(82.704)	(57.962)	(26.782)	(29.397)	(82.771)	(75.855)	(24.352)	(22.469)
Incentive effect	4.759	-17.515	-73.625	40.307*	15.804	-22.274	-24.921	-5.348	7.995
	(10.280)	(169.669)	(166.389)	(24.064)	(18.294)	(167.010)	(167.279)	(10.364)	(13.611)
Obs.	666	666	666	666	666	666	666	666	666

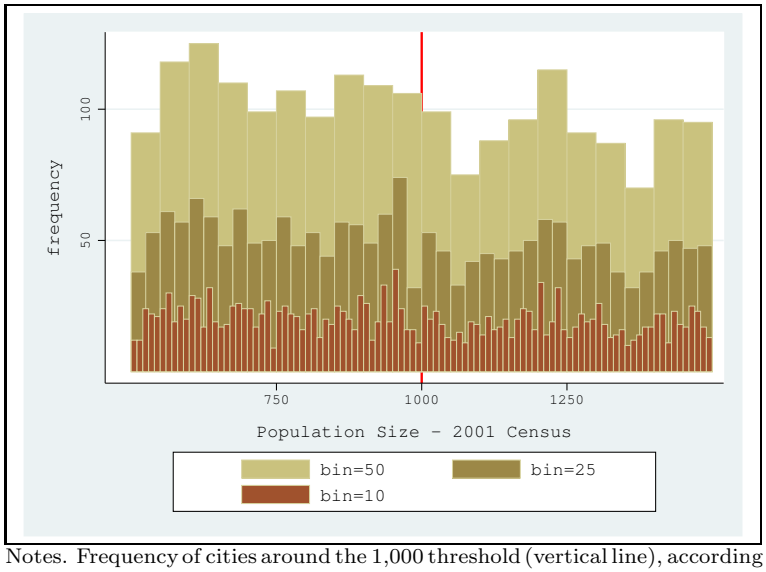
Notes. Estimated discontinuities in budget indicators at fake thresholds (i.e, median above and below the true 5,000 threshold, excluding cities within 250 inhabitants from the original threshold). Terms from 1993 to 2001. All variables are in per-capita terms, expressed in euros at 2000 prices, and averaged over the mayoral term (election years excluded). Mayors observed for two terms, with binding term limit in the second. The maximum symmetric bandwidth is 1,500. 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 ***.

Figure 1: Population distribution (<20,000)



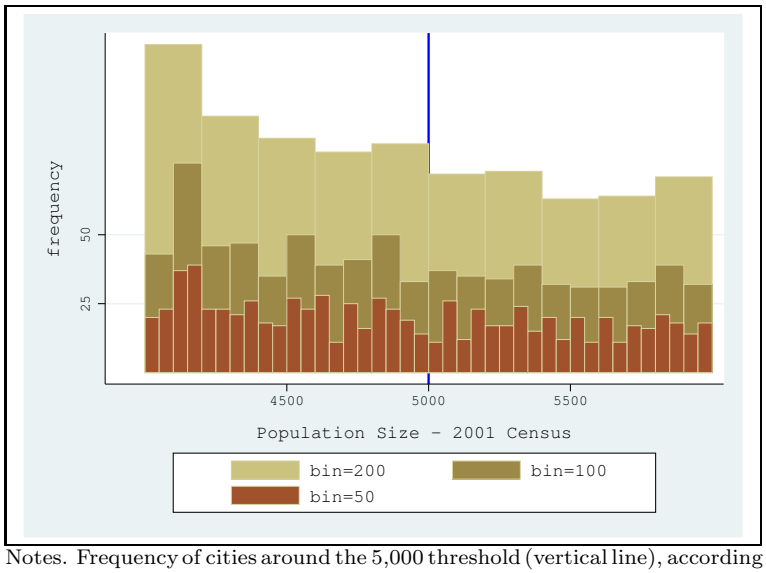
Notes. Frequency of cities according to population in the 2001 Census. Cities below 20,000 inhabitants only. Vertical lines identify policy thresholds.

Figure 2: Population density around the 1,000 threshold



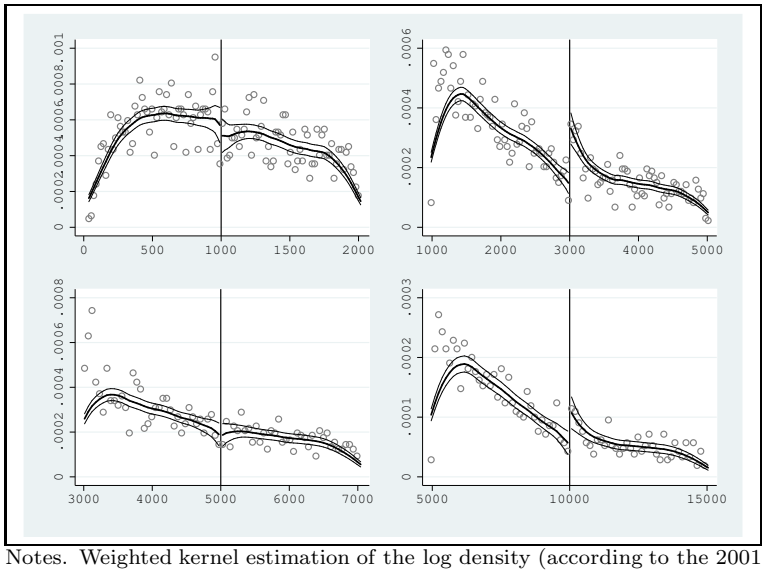
Notes. Frequency of cities around the 1,000 threshold (vertical line), according to population size in the 2001 Census.

Figure 3: Population density around the 5,000 threshold



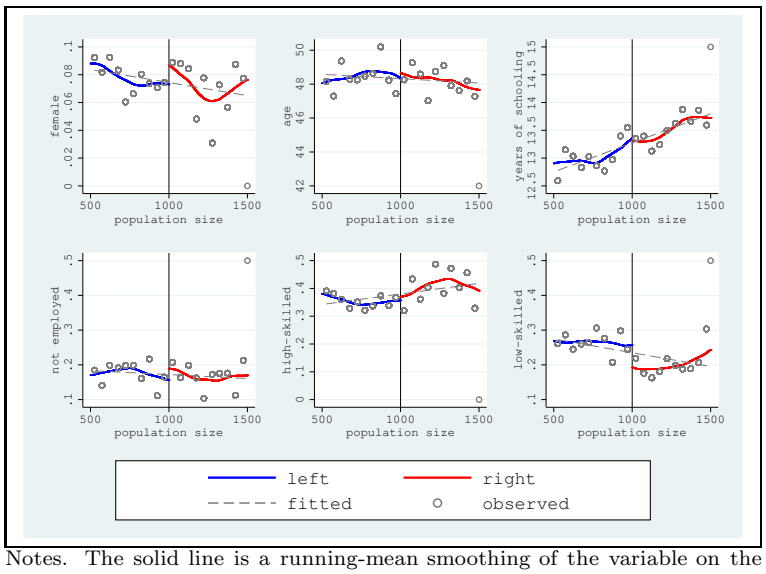
Notes. Frequency of cities around the 5,000 threshold (vertical line), according to population size in the 2001 Census.

Figure 4: McCrary tests



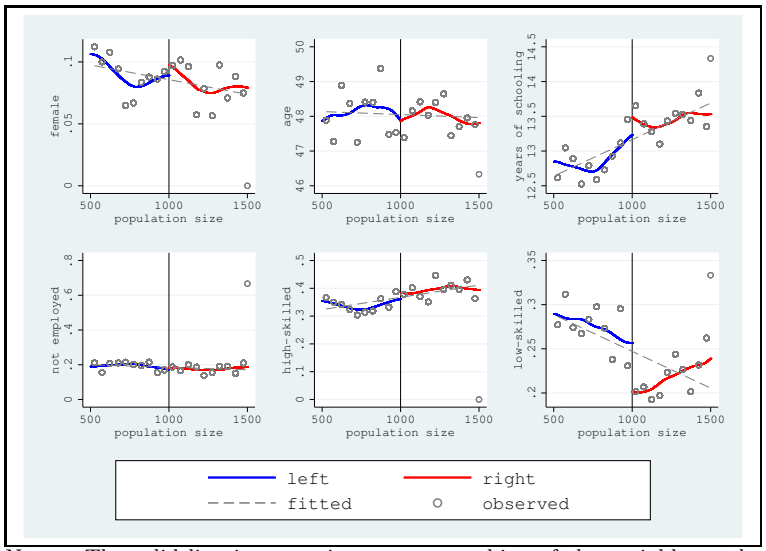
Notes. Weighted kernel estimation of the log density (according to the 2001 Census), performed separately on either side of the threshold. Optimal bin-width and binsize as in McCrary (2008). Thresholds: 1,000; 3,000; 5,000; and 10,000 (see Table 2 for the associated policies).

Figure 5: Mayors' characteristics around the 1,000 threshold



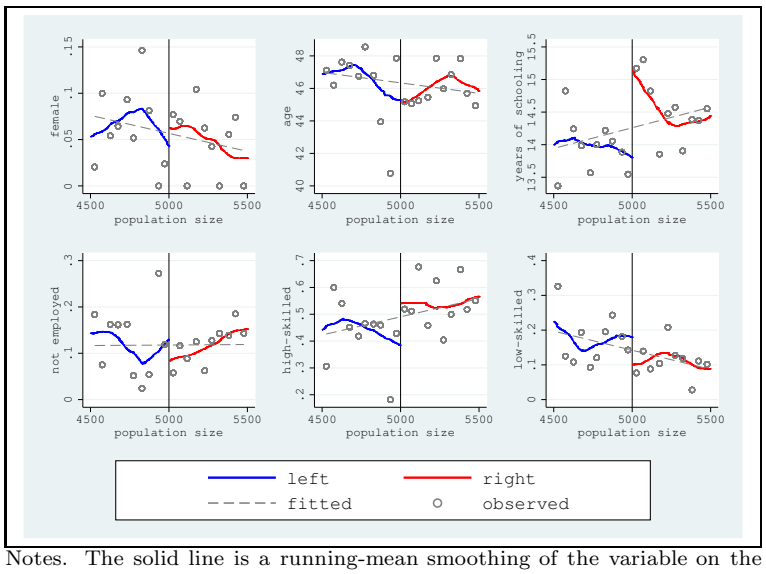
Notes. The solid line is a running-mean smoothing of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 1,000 threshold. The dash line is a fitted regression over the whole sample. The dots are the observed values averaged in intervals of 25 inhabitants.

Figure 6: Candidates' characteristics around the 1,000 threshold



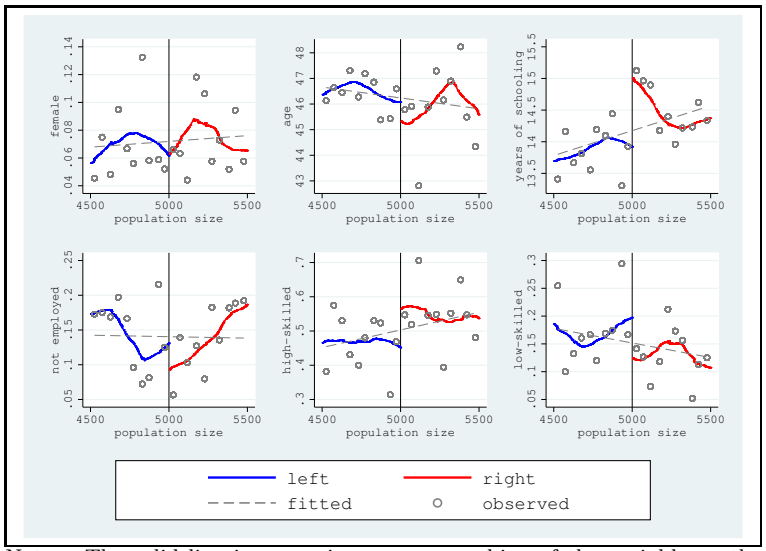
Notes. The solid line is a running-mean smoothing of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 1,000 threshold. The dash line is a fitted regression over the whole sample. The dots are the observed values averaged in intervals of 50 inhabitants.

Figure 7: Mayors' characteristics around the 5,000 threshold



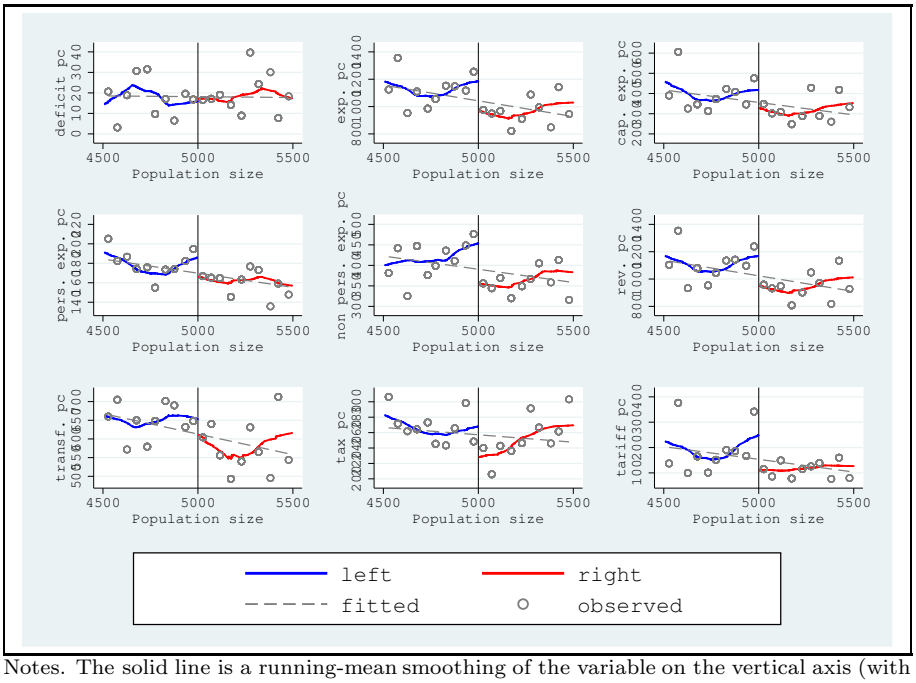
Notes. The solid line is a running-mean smoothing of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The dash line is a fitted regression over the whole sample. The dots are the observed values averaged in intervals of 50 inhabitants.

Figure 8: Candidates' characteristics around the 5,000 threshold



Notes. The solid line is a running-mean smoothing of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The dash line is a fitted regression over the whole sample. The dots are the observed values averaged in intervals of 50 inhabitants.

Figure 9: Budget components per capita around the 5,000 threshold



Notes. The solid line is a running-mean smoothing of the variable on the vertical axis (with a bandwidth of 1), performed separately on either side of the 5,000 threshold. The dash line is a fitted regression over the whole sample. The dots are the observed values averaged in intervals of 50 inhabitants.