Fiscal Rules and the selection of politicians: theory and evidence from Italy*

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Abstract

Fiscal rules, or constraints on the policymaking discretion of elected officials, are widely used to regulate fiscal policies. Using data on Italian municipalities, we employ a difference-in-discontinuity design to show a negative effect of fiscal rules on mayoral candidates' education. The effect is driven by municipalities where fiscal rules meaningfully restrict the action space of politicians. These results are consistent with a formal model of fiscal rules and political selection and highlight a new "general equilibrium" effect of the former. Namely, reducing discretion may alleviate pork-barrel spending, but may also lower the quality of politicians.

Keywords: fiscal rules, selection of politicians, deficit, difference-in-discontinuity.

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1 Introduction

This paper shows that reducing policymakers' discretion with fiscal rules has the important side effect of negatively impacting the quality of candidates willing to run for political office.

Fiscal rules are constraints on policies, and are widely adopted around the world[1] to reduce the incentives of national and local governments to accumulate debt and run deficits. The literature on fiscal rules includes theoretical (Battaglini and Coate, 2008; Halac and Yared, 2014; Azzimonti, Battaglini, and Coate, 2016; Halac and Yared, 2018; Halac and Yared, 2019) and empirical (Grembi et al., 2016; Daniele and Giommoni, 2020) contributions, both of which describe fiscal rules as a reduction in policymaking discretion that involves a trade-off between commitment and flexibility. On the one hand, fiscal rules provide commitment, limiting the incentives toward excessive spending. On the other hand, they impose a cost in terms of reduced flexibility and discretion, confirmed by anecdotal evidence,[2] since they limit governments' ability to respond to shocks.[3] Existing work has mainly addressed the effect of such rules on fiscal stability and economic outcomes (Alesina and Perotti, 1996; Wyplosz, 2012; Grembi et al., 2016). Here, we provide evidence of a "general equilibrium" effect of fiscal rules. Specifically, rather than focusing on the consequences for fiscal stability, we show that the application of fiscal rules, with the associated reduction in policymaking discretion and, potentially, in the value of holding office, can lower the quality of the political class.

Our main contribution is empirical. We use data on Italian municipalities from 1993 to 2012 to estimate the effect of fiscal rules on the quality of the political class, measured as politicians' education level.[4] Italy provides an interesting context for study in that in 1999,

[1] As Grembi et al. (2016) report, many countries have adopted rules to constrain local governments' fiscal policies in recent years, among them Argentina, Austria, Brazil, Canada, China, Colombia, Czech Republic, Denmark, Italy, Mexico, Poland, Spain, Sweden, and Turkey. One of the most famous examples is the European Stability Pact, which was introduced in 1997 by the European Union and applied to member countries.

[2] For two examples of Italian mayors complaining about the reduction in discretion implied by fiscal rules see `https://www.anci.it/patto-stabilita-sindaco-ravenna-si-ribella-abbiamo-14-milioni-da-spendere-venga-rivisto-subito/` and `https://www.anci.it/patto-stabilita-avanzo-di-500-mila-euro-bloccato-sindaco-del-padovano-non-ci-sta-non-alzo-le-tasse-mi-dimetto/`.

[3] It is important to point out that this reduction in flexibility can happen, with fiscal rules, even when they are optimal from an ex-ante point of view. As they introduce a constraint, the reduced flexibility may be costly from an ex-post perspective in some states of the world.

[4] As described in Section 2, education is an indicator often used in the political selection literature

the government introduced fiscal rules to limit incentives for accumulating debt and running deficits. These rules initially applied to all municipalities and were introduced under the so-called "Domestic Stability Pact" (DSP). In 2001, the central government removed the rules for all towns with less than 5,000 inhabitants. This relaxation remained in place until 2013, when the cutoff changed from 5,000 to 1,000 inhabitants.

This institutional framework would be ideal for a Regression Discontinuity Design if fiscal rules were the only policy change at the 5,000-inhabitant threshold. However, at the same cutoff, there is also a sharp increase in the wages paid to the mayor and the municipal ministers, based on a policy introduced by the Italian government in the 1960s (Gagliarducci and Nannicini, 2013; Grembi et al., 2016; Boffa et al. 2022). This policy thus represents a confounding factor. Indeed, Gagliarducci and Nannicini (2013), using data on Italian municipalities between 1993 and 2001, demonstrate that higher wages attract more educated individuals into politics. Hence, we exploit the 2001 removal of fiscal rules for municipalities with below 5,000 inhabitants to estimate a Difference-in-Discontinuity (*Diff-in-Disc*) model, making it possible to measure the effect of fiscal rules on political selection separately from that of the wage increase (Grembi et al., 2016). We find that fiscal rules negatively affect politicians' education level.[5] Specifically, fiscal rules induce a 10 percentage point reduction in the share of mayoral candidates with a university degree. Furthermore, we find that fiscal rules bring about a similar reduction in the probability of electing a mayor with a post-secondary education. Besides, in an additional exercise, we extend the data to 2015 and exploit the 2013 variation in the application of fiscal rules. The results of this exercise indicate similar trends, providing further evidence for the negative effect of fiscal rules on politician quality.

To rationalize this empirical evidence and shed light on underlying mechanisms, we propose a simple model of fiscal rules and political selection. Building on the existing theoretical literature on political selection (see Dal Bo and Finan, 2018, for a recent review), our model incorporates explicitly fiscal rules (FRs) and their well known flexibility-commitment trade-off (e.g., Halac and Yared, 2014) in a model where politician types are multidimensional

(Galasso and Nannicini, 2011; Gagliarducci and Nannicini, 2013; Boffa et al. 2022).

[5]The analysis also shows how fiscal rules impact other characteristics of politicians.

(differing in their observable education and unobservable pro-deficit bias). FRs avoid the improper use of public money by biased politicians, but they also make it more difficult to choose the correct fiscal policy. This impacts the value of holding office for different types of prospective mayors. Since higher educated politicians are better equipped to correctly understand the state of the economy, FRs have a greater negative impact on the value they derive from holding office compared to that of less educated candidates. In other words, tied hands are costlier for higher educated politicians since they are more likely to choose the correct policy, without FRs. This first, direct effect, which discourages higher educated politicians from running for office, has the additional effect of making the run more profitable for less educated politicians, increasing their chances of winning. As a result, fiscal rules are predicted to have an overall negative effect on the education level of the pool of candidates.

The model also suggests that an important channel through which FRs affect political selection is heterogeneity in ability, mirrored by education, combined with some degree of policy motivation by politicians and the fact that FRs constitute a restriction to what can be achieved. While we cannot test the latter aspect directly, we can incorporate two important elements of this mechanism into our analysis. First, the mechanism requires FRs to be a meaningful restriction to what can be done once in office. Hence, we should observe no effect if the action space is already *de facto* restricted, as in the case of financially constrained municipalities. For example, a municipality may be highly financially constrained if its balance sheet is burdened by a high share of rigid expenditures, such as personnel costs and debt repayment, which cannot be adjusted in the short run. Second, the mechanism requires higher educated politicians to be better, on average, at understanding the state of the economy and, hence, at choosing the correct policy. Consequently, there must be a difference between these politicians and less educated ones in terms of their probability of choosing the correct policy when fiscal rules are not in place. This difference should, however, disappear when fiscal rules are in place.

The empirical analysis confirms both of these elements. Specifically, we build a measure for the degree of pre-existing financial constraints facing a municipality. This measure shows that, consistent with the model, municipalities with lower pre-treatment financial constraints drive the negative effect of fiscal rules on politicians' education. Meanwhile, the negative

effect disappears in the sample of municipalities that are subject to greater financial constraints. Furthermore, we show in Appendix B that, without fiscal rules, higher educated politicians are more likely to choose the correct policy, given the state of the economy, *vis-à-vis* less educated politicians, but the difference disappears when FRs are introduced. We test this implication using a regression discontinuity design based on mixed electoral competitions between mayors with and without a university degree. Finally, Appendix C shows that alternative interpretations do not explain our results. First, we document that the results are not due to a reduction in real terms of the difference in wages paid to mayors across the threshold. Second, we show that different out-of-politics options for individuals with different levels of education are not sufficient to explain our empirical results. Third, we provide evidence that the effect of fiscal rules on the education of politicians is not an indirect consequence of the potential effect on other variables such as political experience, political orientation, and the probability of re-election. Indeed, we find that fiscal rules have no effect on these variables. Fourth, we show that the empirical results are unlikely to be due to the effect of fiscal rules on corruption estimated in the literature (Daniele and Giommoni, 2020).

2 Related literature

Our study relates to three strands of literature. The first consists of political economy studies on the selection of politicians (Besley, 2005; Braendle, 2016; Dal Bo and Finan, 2018), to which we contribute from both a theoretical and empirical point of view. On the theory side,[6] the timing of the model and our assumptions about the information structure are similar to Dal Bo and Finan (2018); the model of candidates' outside option echoes Besley (2004); we assume that high ability politicians have an advantage in performing office-related duties as in Caselli and Morelli (2004); and that there can be a scarcity of high quality politicians, as in Galasso and Nannicini (2011). However, none of these models consider the theoretical implications of fiscal rules on political selection, modeling them as a flexibility-commitment

[6]See, among others, Le Borgne and Lockwood, 2002; Besley, 2004; Caselli and Morelli, 2004; Messner and Polborn, 2004; Mattozzi and Merlo, 2008; Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015; Galasso and Nannicini, 2017; Dal Bo and Finan, 2018; Izzo, 2020.

trade-off consistent with Halac and Yared (2014). Like us, Le Borgne and Lockwood (2002) and Izzo (2020) explore the effect of economic dimensions on political selection, though their focus is on political budget cycles and economic crises, respectively.

The empirical literature on political selection has analyzed many different institutions that potentially affect the selection of politicians.[7] However, to the best of our knowledge, the role of fiscal rules has not been addressed. We contribute to this body of work by showing how a reduction in policymaking discretion and the value of holding office due to the application of fiscal rules can negatively affect the quality of the political class. Our results parallel those of Gagliarducci and Nannicini (2013) who, using data from Italian municipalities around the 5,000-inhabitant threshold from 1993 to 2001, show how higher wages paid to politicians can attract more competent individuals into politics. Our empirical analysis suggests that a reduction in policymaking discretion due to fiscal rules can offset the positive effect of higher pay for local politicians. These findings imply that, while higher pay for politicians may help to attract skilled individuals, their decision to enter politics depends on several different factors.

Our paper also intersects with work analyzing the effect of fiscal rules on fiscal stability and more general economic and political outcomes. The results of these studies are mixed, with some (Alesina and Perotti, 1996, and Wyplosz, 2012) noting that fiscal rules may not work for reasons of commitment. Grembi et al. (2016) offer a recent contribution along these lines. They use data from Italian municipalities to show that fiscal rules can effectively reduce the deficit run by local governments. Meanwhile, Daniele and Giommoni (2020) document that fiscal rules reduce corruption.[8] Carreri and Martinez (2021) also look at the

[7]The institutions and determinants studied include the wage paid to politicians (Besley, 2004; Ferraz and Finan, 2011; Gagliarducci and Nannicini, 2013; Kotakorpi and Poutvaara, 2011; Dal Bo et al., 2013; Fisman et al., 2015; Braendle, 2015), the role of outside earnings (Gagliarducci et al., 2010; Fedele and Naticchioni, 2013; Grossman and Hanlon, 2013), the role of monitoring institutions (Grossman and Hanlon, 2013; Artiles et al., 2020), the level of fiscal autonomy (Brollo et al., 2013; Peralta and Pereira dos Santos, 2018; Bordignon, Gamalerio, and Turati, 2020), political parties (Cervellati, Gulino, and Roberti, 2021), electoral rules (Beath et al., 2015; De Benedetto, 2018; Gulino, 2020), gender quotas (Baltrunaite et al., 2014), voter turnout (Lo Prete and Revelli, 2021), disclosure laws (Fisman, Schulz, and Vig, 2019), and criminal organizations (Daniele and Geys, 2015).

[8]Other papers in the literature have exploited the Italian context to study the consequences of fiscal rules on outcomes such as firms' dynamics (Coviello et al., 2021), the political budget cycle (Bonfatti and Forni, 2019), and distributional policies (Alpino et al., 2020). A study by Revelli (2016) examines the impact of tax limits on electoral turnout and local election outcomes. Finally, Vannutelli (2021) shows that fiscal rules are even more effective when enforcement involves independent auditors.

political outcomes of fiscal rules, though they focus on a different type of fiscal rule (the so-called "golden rule") in a different context (Colombia) and with different outcomes (support for the party of the incumbent mayor and protests against the municipal government). We contribute to these studies by investigating an unexplored consequence of fiscal rules on the quality of the political class.

Third, and more broadly, our study speaks to the literature on incentives and selection into the public sector[9] and beyond,[10] with a focus on intrinsic incentives. Deserrano (2018) observes that higher wages may deter prosocial candidates from applying for a government job; Ashraf et al. (2019) find similar results in terms of career perspectives. Bartling et al. (2012) experimentally document the complementarity between the possibility of screening employees' past performance and the amount of on-the-job discretion allowed by employers. The quasi-experimental setting of our paper means that we can explore the direct effect of a reduction in agents' discretion on the quality of "applicants" for public office, holding financial returns fixed. We show that more discretionary power attracts better candidates, at least for this type of "executive" job. In this respect, the relevance of intrinsic incentives highlighted herein is consistent with the findings of Gulzar and Khan (2021), who observe that increasing the salience of prosocial incentives motivates prosocial people to run for office and behave in ways more aligned with citizens' preferences.

Finally, we use the level of education of mayoral candidates and elected mayors to empirically measure the quality and competence of politicians. Various studies justify this focus on education, showing that the latter positively affects socio-economic outcomes such as wages (Card, 1997) and measures of citizenship (Dee, 2004). Of particular relevance to our paper, previous work documents that electing more educated political leaders can positively affect economic growth (Besley et al., 2011), the production of public goods (Martinez-Bravo, 2017), and fiscal sustainability (Meriläinen, 2021). Furthermore, education is extensively used as an indicator in the political selection literature (e.g., Gagliarducci and Nannicini, 2013; Galasso and Nannicini, 2011): it has been shown to positively correlate with measures of administrative competence (Carreri, 2020). Furthermore, local politicians with a

[9]See Finan et al. (2015) for a review focused on field experiments.

[10]See Oyer and Schaefer (2010) for a review.

higher level of education decrease support for anti-establishment populist parties (Boffa et al., 2022).

We look specifically at elected mayors and mayoral candidates for two reasons. First, as we describe in Section 4.1, Italian mayors are powerful at the municipal level. Second, the seminal paper by Besley (2005) suggests that the scope of authority enjoyed by elected politicians affects the selection of directly elected chief executives such as presidents, governors, and mayors to a greater extent than politicians in positions with less direct power. Thus, we can expect a reduction in policymaking discretion to significantly affect politicians in powerful positions, like mayors, rather than politicians in less prominent positions, like municipal councilors.

3 Theoretical framework

3.1 Model set up

There is a large number n of municipalities. Each has a representative voter V and two potential candidates affiliated with political parties. As in Dal Bo and Finan (2018), we use the general term "politicians" to indicate party members who may be chosen to run for office.

3.1.1 Politician types and payoffs

Politicians' education level is denoted by $\Gamma \in \{H, L\}$ and is observable. Higher education implies a better understanding of the state of the world: both high and low education politicians receive an informative signal about the state of the economy, but one is more precise than the other.[11]

When in office, politicians derive an office rent $E > 0$ capturing the direct motivation of the office (e.g., salary) and a policy-related utility, weighted by $k > 0$. Specifically, some politicians are biased in favor of spending, meaning that they receive a payoff of 1 when they choose deficit spending. We denote their bias $b \in \{0, 1\}$, with the common

[11]The results are qualitatively unchanged if we assume instead that signal precision depends on an underlying unknown level of true ability, positively correlated with the education level.

prior $Pr(b = 1) = \tau \in (0, 1)$. We assume that the bias is not correlated with education. Moreover, politicians learn their bias once in office. This assumption simplifies the game, since the entry decision might otherwise become a signalling game. Several political attitudes are contained in this idea of bias toward deficit spending: politicians may be present-biased, as in Halac and Yared (2014) or in Piguillem and Riboni (2015); they may enjoy public spending more when they are in office, and take into account the fact that they may not be in office tomorrow; they may be dishonest and use deficit-financed funds for private purposes; they may be willing (and able) to manipulate voters' views about their competence (as in Murtinu et al. 2021). The model is sufficiently general that it can capture all of these different motivations.[12] The remaining politicians are unbiased, meaning that they want to choose the correct policy with no pre-existing preference for or against deficit spending. This may be due to intrinsic motivations, re-election incentives, or part of an effort to enhance their career prospects. Once again, we are agnostic on the precise motivation. Finally, note that τ can also be interpreted as the probability that, in any given municipality, the incentive structure is such that running a budget deficit is rewarded, *vis-à-vis* choosing the correct policy (for example, because political budget cycles are very effective).

3.1.2 State of the economy and policies

The economy is summarized by a binary state of the world $\theta \in \{0, 1\}$ where $\theta = 0$ implies that the budget should be balanced and $\theta = 1$ implies that there should be deficit spending.[13] Assume $Pr(\theta = 1) = p \in (0, 1)$.

There are 2 possible actions: $x \in \{0, 1\}$, where $x = 0$ denotes a balanced budget and $x = 1$ is deficit spending. The voter's payoff is $u^V = \begin{cases} 1, & \text{if } x = \theta \\ 0, & \text{otherwise} \end{cases}$.

Politicians receive a signal s with realizations $\{0, 1\}$ such that $Pr(s = \theta|\theta, \Gamma) = \phi^\Gamma$, where $1 > \phi^H > \phi^L > max[p, 1 - p]$. This implies that the higher educated politician has a better understanding of the state of the world than the less educated one, and offers a way of capturing the advantage of high ability politicians once in office, consistent with Caselli

[12] See Yared (2019) for a recent taxonomy of political economic reasons behind pro-deficit biases in democracies.

[13] For example, in response to a negative economic shock.

and Morelli (2004).

3.1.3 Running decision

In each municipality, there are two political parties. One member from each party is selected to become a candidate. For simplicity, we assume that party 1 selects a high education member and party 2 selects a low education one,[14] and this is known to the players.[15] Simultaneously, the selected candidates choose whether to accept and run, as in Brollo et al. (2014) and Dal Bo and Finan (2018). If neither of the two party members run, a default mayor is in place.[16]

If the chosen politician decides not to run, she keeps her salary w^i, private information of potential candidate i. We assume that w^i is drawn from a uniform distribution on $[0, W^\Gamma]$, where $W^H > W^L$ are assumed to be sufficiently large to ensure an interior solution. This means that high education politicians have, on average, higher salaries in the private sector. Finally, we set the payoff of running and losing to zero.

Formally, we define $d^i \in \{r, nr\}$ as the decision to run or not by politician i, whose party has selected her to run. γ^i_Γ is her probability of winning the election (in equilibrium, this is endogenous as it depends on the decision of the opponent and the latter's education level). As a consequence, if i is highly educated and runs for office, we have the following objective function:

$$u^i(H, d^i = r) = \gamma^i_H \left(E + k\mathbb{E}_{b,\theta,s} u^P_H\right) \tag{1}$$

The term in parenthesis is the payoff in case of victory: politician i receives the ego rent E and the expectation of the policy-related utility u^P_H with respect to her bias, the state of the world and the signal realization. The subscript H captures the fact that high education politicians have a more precise signal of the true state of the economy. u^P is equal to u^V when $x = \theta$ if $b = 0$ and to 1 when $x = 1$ if $b = 1$, and 0 otherwise. $k > 0$ measures the

[14] Equivalently, we can assume that party 2 does not have high education candidates in the municipality. This assumption is similar to Izzo (2020) and simplifies the exposition. It is not crucial for our results.

[15] It is known that parties may select their candidates for reasons other than their education level or ability (Mattozzi and Merlo, 2015), or they may have a limited supply of high education candidates (Galasso and Nannicini, 2011).

[16] This particular timing is identical to Dal Bo and Finan (2018). However, the results are qualitatively unchanged if we assume that first potential politicians self-select into the pool of potential candidates and then parties pick one of those potential candidates, who is sure to run.

relative importance of the policy motivation. On the other hand, if politician i chooses not to run once selected, she receives

$$u^i(H, d^i = nr) = w^i \tag{2}$$

In the case of a potential candidate with low education we have

$$u^i(L, d^i = r) = \gamma_L^i \left(E + k\mathbb{E}_{b,\theta,s} u_L^P\right) \tag{3}$$

$$u^i(L, d^i = nr) = w^i \tag{4}$$

The difference between (1) and (3) lies in how informative the signal is in each case. In equilibrium, all unbiased politicians follow it, but H politicians are more likely to choose the correct policy.

3.1.4 Modelling fiscal rules

We model fiscal rules as a restriction on the action space of the incumbent in order to capture in the simplest possible way the flexibility-commitment trade off proposed by Halac and Yared (2014), among others. In particular, we assume that when fiscal rules are introduced, the action space is reduced to $x = 0$, i.e. politicians cannot run budget deficits. This implies that some flexibility is lost (i.e., an incumbent cannot choose $x = 1$ when the state of the economy requires it), but also that a biased politician cannot choose $x = 1$. As such, voters are indifferent in their electoral preferences for high and low education politicians when fiscal rules are in place. The way we break this indifference does not matter for our result. However, in order to capture the fact that education can be correlated with in-office performance on other issues separate from the budget policy, we assume that voters, if otherwise indifferent, choose the high education candidate.

3.1.5 Timing and solution concept

The game is one shot. The timing is as follows:

1. One politician per party is selected to run for office in each municipality. They simul-

taneously decide whether to run or not.

2. If there are two candidates, voters vote sincerely. If there is only one candidate, she wins directly. If there are no candidates, a default mayor is in place.

3. The winning politician privately learns b. Then she observes s and chooses x.

4. u^V is realized, payoffs are paid and the game ends.

Our solution concept is a Perfect Bayesian Nash Equilibrium (PBNE). As a tie-breaking rule, we assume that indifferent politicians choose to run.

3.1.6 "Outcome variable"

We are interested in the effects of fiscal rules on ex ante selection into politics. One way to measure this is to look at the probability of a candidate being highly educated, i.e.,

$$\hat{\lambda} := Pr(\Gamma^i = H | d^i = r) = \frac{Pr(d^i = r|\Gamma^i = H)0.5}{Pr(d^i = r)} = \frac{1}{1 + \frac{p_L}{p_H}} \tag{5}$$

where we define p_H (p_L) as the probability that a randomly picked higher (less) educated member of a party chooses to run.

Obviously, both p_H and p_L are determined in equilibrium and are influenced by the presence or absence of fiscal rules.

3.2 Analysis

In this section, we present a shorter and informal examination of the game, leaving the formal analysis and all the relevant proofs for Appendix D.1.

First, note that without fiscal rules the voter anticipates that each type of politician will choose her own individually optimal strategy, as described by Lemma D1, and she thus prefers to elect the high education candidate, if available. Candidates anticipate this and compare their payoffs from running or not running. In other words, they compare the respective expected utilities as described by equations (1) and (2) for the H education type, and equations (3) and (4) for the L type, taking into account the equilibrium behavior

described in lemma D1. This implies that, in the unique PBNE without FRs, the expected policy related payoffs for politicians choosing to run is:

$$\mathbb{E}u_{\Gamma}^{P} = ((1-\tau)\phi^{\Gamma} + \tau) \tag{6}$$

In either case, the running decision is captured by a threshold in w^i, such that only individuals with a private sector salary below the threshold decide to enter politics. As derived in lemma D3, those thresholds vary by education level and can be defined in a closed form as follows:

$$\bar{w}_H = \left(E + ((1-\tau)\phi^H + \tau)k\right)$$

$$\bar{w}_L = \left(1 - \frac{\bar{w}_H}{W^H}\right)\left(E + ((1-\tau)\phi^L + \tau)k\right)$$

The interpretation of the thresholds is straightforward. H politicians know that, if they run, they are sure to win. On the opposite side, L politicians know that they will win only if the H candidate does not run. The rest is their expected payoff from being in office. As a consequence, in equilibrium, $p_H = \frac{\bar{w}_H}{W^H}$ and $p_L = \frac{\bar{w}_L}{W^L}$.

When fiscal rules are in place, all politicians in office are constrained to choose $x = 0$. As a consequence, the relevant thresholds are those shown in lemma D4:

$$\bar{w}_H^{FR} = [E + (1-\tau)(1-p)k]$$

$$\bar{w}_L^{FR} = \left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)[E + (1-\tau)(1-p)k]$$

We are now in a position to compare the share of high education candidates with and without fiscal rules. Proposition 1 summarizes our findings:

Proposition 1. *The share of high education politicians among those willing to run for office is higher without fiscal rules.*

Proposition 1 (and its proof) provides several insights. First, note that we can focus on the comparison of the ratios between $\bar{w}_H$ and $\bar{w}_L$, with or without fiscal rules.

Second, fiscal rules have two distinct effects, both of which lead to a decrease in the share of H candidates. The first effect is a reduction in the expected policy payoff from office for

unbiased politicians, which is largest for H candidates. Note that without FRs, candidates can match the state of the world with probability $\phi^H > \phi^L$. With FRs, this happens with probability $1 - p$ irrespective of the education level. Hence, the reduction is relatively larger for H politicians. The second, related, effect is that H politicians are less likely to run with FRs in place. This, in turn, increases the incentive of L politicians to run, as they can win by running unopposed.

Third, the model highlights the importance of the first effect, which results from the different ability and policy-related incentives. As shown in Appendix D.4, if we shut these considerations down (i.e., if we assume purely office-motivated politicians), the effect of fiscal rules disappears. Conversely, the effect remains if we shut down the change in the probability of re-election.

3.2.1 Additional testable implications

Along with its main result, the model suggests an important mechanism[17] for the observed effect. This relies on FRs meaningfully restricting the set of available policies, combined with a differential in ability between higher and less educated politicians once they take office. Both of these implications can be brought to the data.

Financially constrained municipalities Consider the case of a municipality that is "financially constrained" such that the high share of rigid, pre-existing expenditures–i.e., personnel and debt repayment expenditures, which cannot be adjusted in the short run–make it more difficult for the incumbent mayor to react to a negative shock with greater deficit spending.[18] In such cases, the action space is effectively constrained to $x = 0$ irrespective of fiscal rules, meaning that FRs do not meaningfully restrict the action space of politicians in office. Hence, if the channel implied by the model does drive politicians' behavior, we should observe no effect of FRs on the composition of the candidate pool in financially constrained municipalities. Appendix D.3 discusses this case formally.

[17] In the sense of Ashworth et al. (2021).

[18] See section 5.3.2 for more details on our measure of spending rigidity.

Education level and policy choice The model's conclusions are primarily driven by higher educated politicians being more able to match the state of the world, which they are (sometimes) willing to do, though FRs may restrict them from doing so. This implies that, without fiscal rules, we should observe better policy choices from higher educated politicians, on average. However, this difference should disappear once fiscal rules are in place.

3.3 Summary

In a nutshell, the model makes one main prediction: fiscal rules decrease the share of higher educated candidates willing to run for office. Furthermore, there are two testable implications of the suggested mechanism:

1. The effect of fiscal rules on the quality of candidates should be visible where such rules meaningfully restrict the action space of politicians in office, hence the effect should disappear in financially constrained municipalities;

2. Higher educated politicians are more likely to choose the correct policy than less educated ones in the absence of fiscal rules. This difference disappears when fiscal rules are introduced.

4 Institutional Setting

4.1 Italian municipalities

There are 8,047 municipalities in Italy, 70.5% of which have less than 5,000 inhabitants. Municipalities are responsible for municipal police, infrastructure, transport, welfare, housing, garbage collection, and water supply. They manage 10% of total public expenditures, with around 20% of their revenues coming from local taxes. The rest of the revenues are discretionary transfers from higher levels of government, like provinces, regions, and the central state. The most important local taxes include property tax, introduced in 1993 by Legislative Decree 504/1992, and a surcharge on residents' income tax, introduced in 1999. Since 1993 (see Law 81 of 1993), mayors of Italian municipalities are directly elected by voters.

In municipalities below 15,000 inhabitants, mayors are elected using a single round plurality rule, while a run-off system is employed above this threshold. Mayors serve for a period of five years and, since 1993, for a maximum of two consecutive terms. They play a powerful role in municipal governments, as they can choose and dismiss the ministers that form part of the municipal government. Furthermore, if the municipal council decides to dismiss the mayor, new elections must be held.

4.2 The "Domestic Stability Pact" (DSP)

Fiscal rules for Italian municipal governments were introduced in 1999, following the European Stability and Growth Pact (SGP), signed in 1997 by various European countries. In Italy, these rules were called the "Domestic Stability Pact" (DSP). [19] The goal of the DSP was to reduce the incentives for local governments to accumulate debt and run deficits. Table A1 describes how the target imposed by the DSP changed between 1999 and 2015. We see that the target has not been constant over time, though, with the exception of 2005-2006, local governments did have to balance their budget each year.[20] The initial penalties introduced by the central government for not complying with the rules consisted of a 5% cut in grants transferred by the national government, a cut in reimbursement and non-absenteeism bonuses for municipal employees, and a ban on new municipal hires. Municipalities complying with the rules were rewarded with decreased interest expenses on loans received from the central government. In 2008, as described by Coviello et al. (2021), harsher penalties for not complying with the rules were instituted, including an increased cut to central government grants and an automatic 30% cut to the salaries of mayors and municipal councilors.

As can be seen in Table A1, fiscal rules applied to all municipalities for the first two years (1999-2000). In 2001, the central government then removed the fiscal rules for all the municipalities below 5,000 inhabitants in order to lift onerous constraints on places

[19] In Italian, *Patto Interno di Stabilita'*. The DSP was introduced through Law 448 of 23 December 1998, Article 28.

[20] The definition of balanced budget used in the target for most years has been based on the so-called fiscal gap (Grembi et al., 2016), or the municipal deficit net of transfers and debt service. The limits on the target have varied: in some years, municipalities were asked to apply a cap to the growth of the target; in other years, they were asked to cut the target. These limits have always been calculated with reference to past values of the target in specific reference years.

that were disadvantaged by economies of scale. Specifically, the law (Legislative Decree no. 267, article 156) identifies the municipalities subject to fiscal rules based on the number of residents as measured by the Italian Statistical Office (Istat) at the end of the second-most recent year (e.g., for the year 2002, the population figure from December 31, 2000 was used as a reference). In 2002, the Regions with Special Statute (i.e., Sardinia, Sicily, Valle d'Aosta, Trentino-Alto Adige, Friuli-Venezia Giulia) were allowed to establish their own fiscal rules; we accordingly exclude municipalities in these regions from the analysis. The 5,000-inhabitant threshold remained in place until 2013.[21] In that year, the threshold was reduced from 5,000 to 1,000 inhabitants for 2013-2015 (Daniele and Giommoni, 2020). Finally, the DSP was abolished in 2016 and replaced by a new set of balanced budget rules for all municipalities.

Table A2 reports the legislative population thresholds applicable to municipalities with less than 15,000 inhabitants. We observe that the wages paid to the mayor and ministers in the municipal government change at the 5,000-inhabitant threshold (Gagliarducci and Nannicini, 2013). This wage increase is a policy that dates back to the 1960s (Gagliarducci and Nannicini, 2013), which has remained constant in real terms until today. The mayor's pay is based on the population recorded in the last available population Census.

5 Empirical Evidence

5.1 Empirical Strategy: Difference-in-discontinuity

We test the main prediction of the theoretical model, together with the first implication, exploiting the variation over time in the application of fiscal rules around the 5,000-inhabitant threshold. In the absence of other policies changing across the threshold, this institutional setup would be appropriate for a regression discontinuity design (RDD) approach applied to the electoral terms between 2001 and 2012. However, the difference in mayoral pay in

[21] At the end of 2004, Budget Law 311/2004 extended the application of fiscal rules to municipalities between 3,000 and 5,000 inhabitants for the year 2005. However, Decree-Law 44/2005 and Law 88/2005 canceled this provision, such that the threshold remained at 5,000 inhabitants. Budget Law 266/2005 confirmed the 5,000-inhabitant threshold for 2006, and Budget Law 296/2006 confirmed it for 2007. The threshold then remained unchanged until Budget Law 183/2011 extended the application of fiscal rules to all municipalities with more than 1,000 inhabitants starting from the year 2013.

municipalities above the threshold represents a confounding policy that would invalidate the RDD approach, as it would not be possible to disentangle the effects of mayoral pay and fiscal rules. While a standard RDD is thus not appropriate in this context, the removal of fiscal rules in 2001 for municipalities below 5,000 inhabitants can be exploited to implement a Difference-in-Discontinuity (*Diff-in-Disc*) approach (see also Grembi et al., 2016), making it possible to estimate the effect of fiscal rules separately from that of the wage increase.[22]

The *Diff-in-Disc* approach is a strategy (Lalive, 2008; Campa, 2011; Leonardi and Pica, 2013; Casas-Arce and Saiz, 2015; Grembi et al., 2016) that combines the *pre/post treatment* variation typical of a Difference-in-Differences design with the variation *around a threshold* that characterizes an RDD approach. In the Italian context, adopting this strategy allows to measure the effect of fiscal rules on the selection of politicians while controlling for differences in pay, which is constant over time in real terms. In addition, we can also extend this approach to the electoral years after 2012, exploiting the 2013 reduction of the application threshold for fiscal rules to 1,000 inhabitants. This extension means we can evaluate whether the effect of fiscal rules disappears across the 5,000-inhabitant threshold once fiscal rules are applied again.

Specifically, we estimate the following empirical model, using data at the municipality and electoral year level:

$$\begin{aligned} Y_{it} = \rho_0 + \rho_1 R_{it} + (>5000_{it}) * (\beta_0 + \beta_1 R_{it}) + \\ + (Post_t) * [\pi_0 + \pi_1 R_{it} + (>5000_{it}) * (\phi_0 + \phi_1 R_{it})] + \eta_{it} \end{aligned} \tag{7}$$

where Y_{it} is the level of education of politicians. The variable $R_{it} = P_{it-1} - P_{5000}$ is the normalized population, which measures the distance of municipality i from the 5,000-inhabitant threshold P_{5000} at time t. As described in Section 4.2, the population P_{it-1} is the resident population measured at the end of the second-most recent year (e.g., the population on December 31, 2000 for the year 2002). In practice, P_{it-1} is the resident population measured

[22] As described in Section 4.2, the population of reference used for the application of fiscal rules is not the same as the population used for mayoral pay. However, the dummy variables for fiscal rules treatment and higher wages overlap for most municipalities, with the two coinciding in all but very small number of cases. Specifically, the correlation between the two treatments is 0.97. This further demonstrates that an RDD approach would be unable to disentangle the effects of fiscal rules and higher wages.

at the beginning of the previous year, which explains the subscript (t-1).[23] The dummy variable ($> 5000_{it}$) is equal to 1 if municipality i is above the 5,000-inhabitant threshold, while the dummy variable ($Post_t$) is equal to 1 for elections starting from 2001.[24] The temporal dummy variable ($Post_t$) is built this way as the selection of (new) politicians can only happen during election years and not during the mandate period (i.e., far from elections). Indeed, this is why we estimate the model using data at the election year level, unlike Grembi et al. (2016), who use yearly data. The treatment variable is the interaction term between ($> 5000_{it}$) and ($Post_t$). The coefficient of interest is ϕ_0, which captures the effect of fiscal rules on the selection of politicians, comparing municipalities that continue to apply fiscal rules and those that became exempt starting in 2001. The electoral years from 2001 on comprise the treatment period as this is the year when fiscal rules started to apply differently across the 5,000-inhabitant threshold. Therefore, we can expect differential behavior in terms of political selection due to fiscal rules to emerge across the cutoff only from 2001.

We estimate model 7 with a local linear regression (Gelman and Imbens, 2018), using the subsample of observations that lie within the interval $R_{it} \in [-h, +h]$ around the threshold, where the optimal bandwidth h is calculated using the MSE-optimal bandwidth following Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). We cluster standard errors at the local labor market area level.[25] This identification strategy requires three main assumptions, which we test in the analysis below. First, there must not be manipulative sorting of the running variable R_{it} around the 5,000-inhabitant threshold before and after 2001, such that municipalities must not be able to self-select themselves and decide which side of the cutoff they wish to stay on. We test this assumption in Figure A1, which shows that there is not any discontinuity in R_{it} at the 5,000-inhabitant threshold.[26]

[23] In previous versions of this paper, we used the population reported in the most recent Istat Census, either in 1991 or 2001, as the population of reference. The results we obtain using the Census population are similar and available upon request.

[24] For example, for a municipality that voted in 1995, 1999, 2004, and 2009, ($Post_t$) is equal to 0 for the electoral terms 1995 and 1999 and equal to 1 for 2004 and 2009.

[25] Local labor market areas are geographical units composed of neighboring municipalities that share the same local labor market and exhibit common socio-economic and population characteristics. In our dataset, we were able to identify 517 local labor areas using information from the 2001 Census. The results are robust to clustering the standard errors at different levels (e.g., municipal or provincial levels).

[26] In Figure A1, we present scatters and 4th-order polynomial estimates to test the null hypothesis of the continuity of population density around the 5,000-inhabitant threshold. We apply this test to R_{it} measured in the years before and after 2001. We also test the continuity of the difference between the density of the

Second, other potential outcomes and municipal characteristics must be balanced around the threshold before and after 2001. We test this assumption by running model 7 using municipal characteristics as dependent variables. Table A4 shows that municipal and geographical characteristics are balanced around the threshold before and after 2001. Finally, municipalities just below and just above the 5,000-inhabitant threshold must be on parallel trends before the 2001 relaxation, as is typical in a difference-in-differences analysis. We test this assumption in Section 5.3.1.

5.2 Data

We use data from Italian municipalities with less than 15,000 inhabitants for the period of 1993-2015. There are various reasons for this choice of sample population. First, municipalities with less than 15,000 inhabitants use a single-ballot majoritarian electoral system, while those above this threshold employ a run-off system (Gamalerio, Morelli, and Negri, 2021). To keep electoral institutions constant, we thus exclude the latter group. Second, in 1993, following a corruption scandal called *Mani Pulite* (Clean Hands), new electoral municipal laws and a municipal property tax were introduced (Bordignon, Gamalerio, and Turati, 2020). The Domestic Stability Pact (DSP), meanwhile, remained in place until 2015 after being introduced in 1999. Our data collection on municipal politicians and municipality characteristics therefore encompasses the 1993-2015 period. Finally, we exclude municipalities in the Special Statute Regions (i.e., Sardinia, Sicily, Valle d'Aosta, Trentino-Alto Adige, Friuli-Venezia Giulia) since they have distinct political and fiscal institutions, and a different set of fiscal rules in place since 2002.

The data set contains information on the characteristics of elected municipal politicians and mayoral candidates for the years 1993-2015. The main observable characteristics, available from the Italian Home Office, are gender, age, years of past political experience at all levels of politics, political orientation (i.e., left, right or independent), past professional background, and education. Information on municipality characteristics comes from Istat and includes the share of the population with a university degree, the share of the active

average population before and after 2001. As can be seen in the bottom graph, there is no evidence of sorting.

population (i.e., the population between 15 and 64 years old), the population of seniors (i.e., above 65 years old), income per capita, the number of firms and non-profit associations per capita, the area of the municipality in square kilometers, and population density. We use all of these variables as controls. They were measured in 2001, with the exception of firms and non-profit associations per capita, measured in 2005. We collected the data on municipal budget outcomes from the Aida PA database, an online archive managed by the Bureau Van Dijk. The data contains information on the fiscal items of the budgets of all Italian municipalities, covering the years 2000-2015. Finally, data on average income and income growth rate at the municipal level is provided by the Italian Ministry of Economics and Finance and covers the years 2000-2016.

The final sample consists of 26,005 electoral terms and 6,170 municipalities. Table A3 reports the summary statistics of this sample, distinguishing between municipalities below and above the cutoff.

5.3 Results

We divide the description of the main results into four parts. First, subsection 5.3.1 presents empirical evidence related to the main prediction of the theoretical model. Second, we provide evidence on implication 1 in subsection 5.3.2. Third, Appendix B describes the empirical results that refer to implication 2. Finally, we investigate alternative stories in Appendix C.

5.3.1 The effect of fiscal rules on the selection of politicians

This subsection tests the main prediction of the theoretical model. We use data from the 1993-2012 period and exploit the 2001 relaxation of fiscal rules. Figure 1 provides a preliminary look at how politicians' education evolved between the years before 2001 - i.e., when the only difference across the 5,000-inhabitant threshold was the wage paid to politicians - and the years afterwards - i.e., when the application of fiscal rules also differed across the cutoff. This descriptive introductory evidence is the result of running a regression discontinuity design (RDD) based on a second-order split polynomial of the education level of

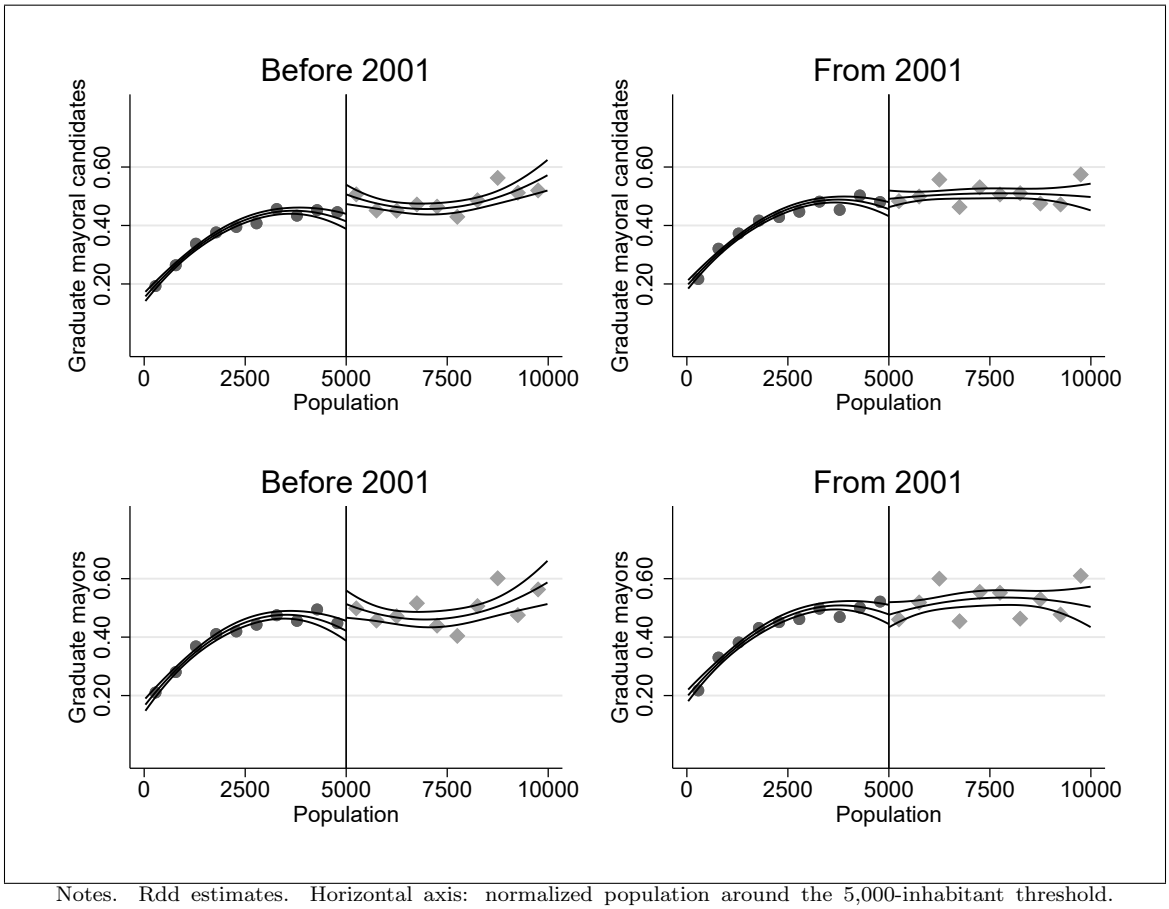
politicians from the sample of municipalities[27] for the electoral years before and after 2001.[28] Two facts emerge from Figure 1. First, there is a discontinuity across the 5000 inhabitants threshold for the electoral years before 2001. In those years, during which fiscal rules did not apply differently across the threshold, the share of mayoral candidates and elected mayors with a university degree was higher in municipalities just above the threshold. This result is consistent with the fact that mayoral pay is higher in municipalities with more than 5,000 inhabitants, enabling them to attract more skilled politicians (Gagliarducci and Nannicini, 2013). Second, the discontinuity around the 5,000-inhabitant threshold disappears in the years from 2001 to 2012. The effect of the wage disappears from 2001, which is when fiscal rules apply differently across the cutoff.

To confirm these results, we implement the *Diff-in-Disc* analysis, running model 7 on the sample of municipalities falling within the Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018) MSE-optimal bandwidth selector. The baseline results from the *Diff-in-Disc* analysis appear in Table 1, as do the estimates obtained running model 7 using the optimal bandwidth with a linear function in the running variable. We do not control for covariates in column 1, while the estimates in column 2 control for election year fixed effects. In column 3, we add municipal covariates and region fixed effects. Two main findings emerge. First, the positive coefficients in front of the dummy variable ($> 5000_{it}$) indicate that in the years prior to 2001 (i.e., when fiscal rules applied equally across the cutoff), mayoral candidates in municipalities just above 5,000 inhabitants more frequently had a university education. This evidence is consistent with Figure 1 and with the analysis of Gagliarducci and Nannicini (2013). Second, the negative coefficient in front of the interaction term between ($> 5000_{it}$) and ($Post_t$) suggests that the application of fiscal rules from 2001 onward in municipalities above 5,000 inhabitants offsets the positive selection effect induced

[27] One of the main purposes of this figure is to provide evidence of the discontinuity in politicians' education around the wage discontinuity in the years before 2001 (Gagliarducci and Nannicini, 2013). We therefore use the running variable based on the Census population, which is the population of reference for identifying mayoral pay. A similar picture emerges if we use the running variable based on the number of residents at the end of the second-most recent year, which determines the application of fiscal rules (see Section 4.2 for more detail). The results of the latter analysis are available upon request.

[28] The same evidence emerges if we use different polynomial orders or a more local approach, e.g., using the Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018) MSE-optimal bandwidth estimator and a linear polynomial. Results are available upon request.

Figure 1: Rdd graphical evidence



Notes. Rdd estimates. Horizontal axis: normalized population around the 5,000-inhabitant threshold. Vertical axis: share of mayoral candidates (top graphs) and mayors (bottom graphs) with a university degree. Scatter points are averaged over bins of 500 inhabitants. The central black line represents a split second-order polynomial of the outcome variable in the normalized population, fitted separately on each side of the threshold. The other two lines represent 90 percent confidence intervals.

by the higher wage paid. The results indicate that fiscal rules induced a reduction of around 10 percentage points in the share of mayoral candidates with a university degree. They also indicate that fiscal rules led to a similar reduction in the probability of electing a higher educated mayor.[29]

Table 1: The effect of fiscal rules on the education of politicians

	(1)	(2)	(3)
Control Function	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT
Election Year FE	No	Yes	Yes
Region FE	No	No	Yes
Controls	No	No	Yes
Panel A: mayoral candidates with university degree			
(> 5000)	0.091***	0.088**	0.071**
	(0.034)	(0.034)	(0.033)
(Post)	0.059**		
	(0.030)		
(Post)*(> 5000)	-0.102**	-0.107**	-0.107**
	(0.044)	(0.044)	(0.042)
Observations	3,576	3,576	3,576
Bandwidth (h)	1166	1166	1166
Mean outcome	0.472	0.472	0.472
Panel B: mayors with university degree			
(> 5000)	0.055	0.054	0.039
	(0.048)	(0.048)	(0.048)
(Post)	0.057		
	(0.047)		
(Post)*(> 5000)	-0.107*	-0.114*	-0.113*
	(0.060)	(0.060)	(0.059)
Observations	4,383	4,383	4,383
Bandwidth	1425	1425	1425
Mean outcome	0.486	0.486	0.486

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education level of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Variables in the table: 1) (> 5000) = 1 for municipalities with more than 5000 inhabitants; 2) (Post) = 1 for electoral terms starting after 2001; 3) (Post)*(> 5000) = interaction term between (> 5000) and Post. The outcome variable is the share of mayoral candidates with a university degree in Panel A, which is equal to 1 for mayors with a university degree in Panel B. The bandwidth is calculated using the MSE-optimal bandwidth h selector following Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Year of election fixed effects are considered in columns 2 and 3, and region fixed effects in column 3. Control variables in column 3 (measured in 2001 except for numbers 5 and 6, which were measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

After describing the baseline effect of fiscal rules on political selection, we test one of

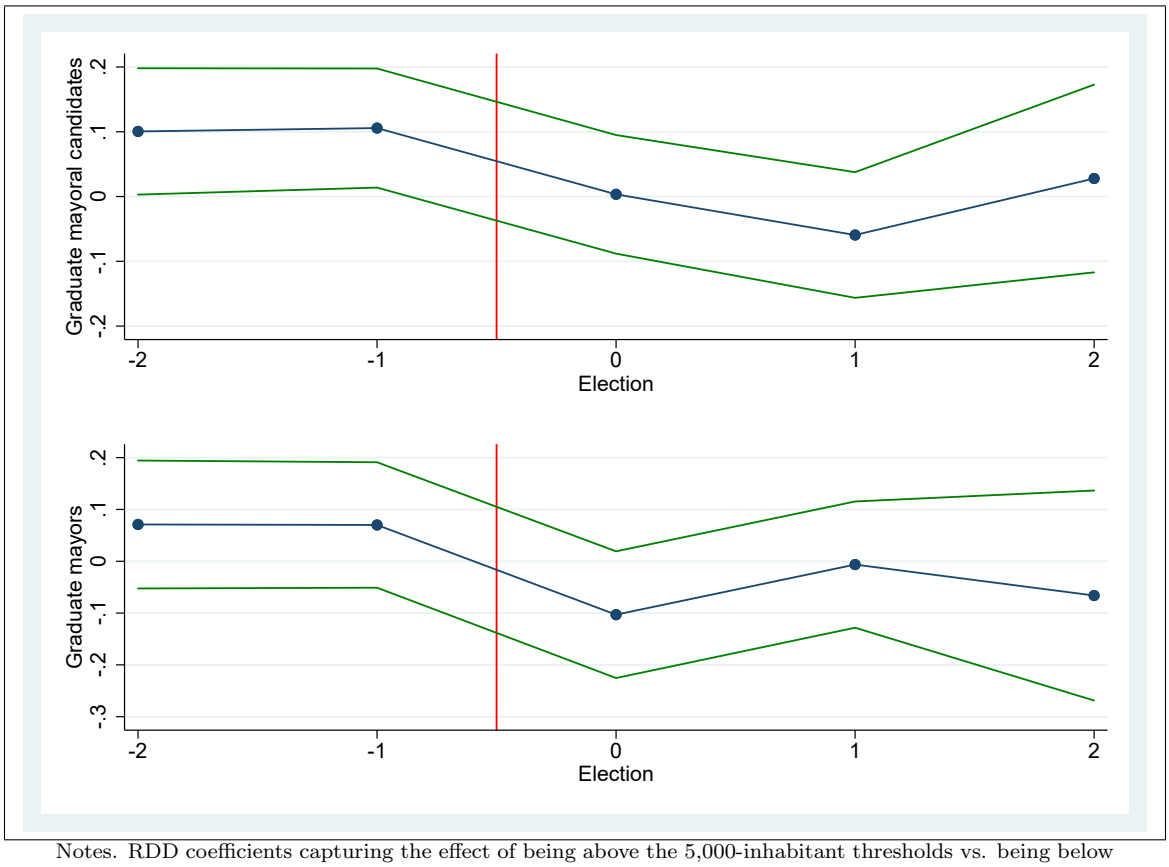
[29]Figure A2 shows how the estimated coefficients change with the bandwidth used. As is typical in an RDD setup, moving toward smaller bandwidths produces larger coefficients (i.e., lower bias) and larger confidence intervals (i.e., more inefficiency). Furthermore, Figure A3 shows that the results are not due to random chance. Specifically, we run a series of *Diff-in-Disc* local linear regressions at 1,000 fictional thresholds, half below and half above the original 5,000-inhabitant threshold (i.e., thresholds from 4,900 to 4,400, and from 5,100 to 5,600). The c.d.f. of the t-statistics from these regressions in Figure A3 shows that most of the t-statistics lie in the interval (-2,2), suggesting that it is not possible to find statistically significant results at the fictional thresholds.

the three assumptions of the *Diff-in-Disc* methodology, which is that the municipalities just below and just above the 5,000-inhabitant threshold must have been on parallel trends before the 2001 reform (see Section 5.1 for results on the other two assumptions). We provide evidence on the parallel trends assumption in two ways. First, we run a series of cross-sectional RDD regressions comparing the level of education of politicians across municipalities just below and above the 5,000-inhabitant threshold, where we group observations from different municipalities in a single regression depending on the distance of the electoral year from the 2001 relaxation of fiscal rules. For example, as reported on the x-axis of the two graphs in Figure 2, a value of 0 indicates that the first elections run immediately after the 2001 relaxation. The value of -1 refers to the elections run immediately before the 2001 reform, while -2 indicates elections run before the elections at time -1. These cross-sectional RDD coefficients make it possible to study the changing effect on politicians' education of a municipality being above the 5,000-inhabitant threshold. As we can observe, in the two elections before the 2001 removal of fiscal rules (i.e., -2 and -1, during which time fiscal rules applied consistently across the threshold), municipalities above the threshold attracted more educated individuals into politics. Crucially for the parallel trends assumption, the RDD coefficients are stable between elections -2 and -1, signaling that municipalities below and above the threshold followed the same trends during the pre-2001 period. Consistent with the evidence of a negative effect of fiscal rules on political selection, this effect disappears in the elections from 2001 (i.e., elections 0, 1, and 2, for which fiscal rules applied differently across the threshold).

Second, we run model 7 on the pre-2001 period only (i.e., elections -1 and -2 in Figure 2), introducing a placebo treatment for the elections run immediately before the 2001 relaxation of fiscal rules. This placebo treatment is equal to the interaction term between a dummy variable that we call (Pre_t), which is equal to 1 for the electoral terms immediately before the 2001 removal of fiscal rules,[30] and the dummy variable $(> 5000_{it})$. The parameter in front of the interaction term (Pre_t)*$(> 5000_{it})$ makes it possible to test for parallel trends between the treatment and control groups during the pre-treatment period. The results

[30]For example, for a municipality that voted in 1995, 1999, 2004, and 2009, (Pre_t) is equal to 1 for the 1999 electoral term and 0 otherwise. For a municipality that voted in the years 1994, 1998, 2003, and 2008, (Pre_t) is equal to 1 for the 1998 electoral term and 0 otherwise.

Figure 2: Cross-sectional RDD coefficients over time



Notes. RDD coefficients capturing the effect of being above the 5,000-inhabitant thresholds vs. being below it. On the x-axis, which goes from -2 to 2, we report the elections before and after the 2001 removal of fiscal rules, where 0 indicates the elections immediately after the relaxation of fiscal rules. We run the cross-section RDD regressions using the optimal CCT bandwidths reported in Table 1.

of this regression are reported in Table 2: we do not find any evidence of differential pre-trends across municipalities below and above the 5,000-inhabitant threshold,[31] indicating that municipalities just below and above the 5000-inhabitant threshold followed the same trend during the pre-treatment period between 1993 and just before the 2001 relaxation of fiscal rules.[32]

Table 2: Parallel trends test

	(1)	(2)
Dependent Variables	Share mayoral candidates with university degree	= 1 for Mayors with university degree
Control Function	Linear	Linear
Bandwidth	CCT	CCT
Election Year FE	No	No
Region FE	No	No
Controls	No	No
(Pre)*(> 5000)	0.026	0.042
	(0.054)	(0.067)
Observations	1,966	2,210
Bandwidth	1364	1534
Mean outcome	0.450	0.464

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2000. Variables in the Table: (Pre)*(> 5000)= interaction between dummy = 1 for election immediately before the 2001 removal of fiscal rules and dummy = 1 for municipalities with more than 5000 inhabitants. The outcome variable is the share of mayoral candidates with a university degree in column 1, while it is equal to 1 for mayors with a university degree in column 2. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

5.3.2 Evidence on financially constrained municipalities

This section provides evidence on implication 1 of the theoretical model. Specifically, we show that the effect of fiscal rules disappears when we consider municipalities that were already financially constrained before the 2001 relaxation of fiscal rules. Intuitively, a pre-existing financial constraint reduces politicians' discretion and policy space, making political

[31] In addition, Table A5 provides further evidence on the evolution of the effect of the fiscal rules during the pre-treatment period. Specifically, we observe no interaction between fiscal rules and the differential wage paid across the cutoff. This evidence is necessary to demonstrate that municipalities on either side of the threshold did not react differently to the introduction of fiscal rules (Grembi et al., 2016). To test that this is indeed the case, we interact the dummy variable $(> 5000_{it})$ with $(>= 1999)$, which is equal to 1 for the electoral years 1999-2000. We run model 7 using this interaction as the main variable of interest. We see in Table A5 that the interaction $(>= 1999) * (> 5000_{it})$ is never statistically different from zero.

[32] Conversely, in Table A6, we report the estimates obtained running a simple difference-in-differences model on the entire original sample. While the results go in the same direction as the *Diff-in-Disc* model, the parallel trends assumption does not appear to be met. The violation of this assumption justifies the use of the *Diff-in-Disc* model (Grembi et al., 2016), which allows us to compare more similar municipalities.

office less attractive for more highly educated individuals in a manner similar to fiscal rules. To show this, we repeat the *Diff-in-Disc* analysis, distinguishing between municipalities with a high pre-treatment level of rigidity in their spending and others with a low level of rigidity. To this end, we use the balance sheets of municipalities, classifying some expenditures as rigid; namely those that leave little room for adjustments. Classic examples of rigid expenditures are personnel costs and debt repayment, which take a long time to adjust and can be considered mandatory in the short run. For instance, it is very difficult for municipal governments to fire municipal employees or change their wages. At the same time, a mayor must repay the debt inherited from the previous governments. The literature documents that the debt produced by prior governments can represent a constraint on what the current government is able to do, thus reducing the level of policymaking discretion (Persson and Svensson, 1989; Alesina and Tabellini, 1990; Pettersson Lidbom, 2001; Alt and Lassen, 2006; Eslava, 2010). Hence, a high share of rigid expenditures can constrain that which a municipal government is able to do.

As described by Grembi et al. (2016), an average of two-thirds of municipal expenditures can be classified as rigid in Italian municipalities. However, we observe heterogeneity in terms of rigidity across municipalities with different histories and backgrounds.[33] Specifically, using data from Italian municipalities' balance sheets measured in the year 2000 (i.e., the year before the 2001 fiscal rules relaxation), we estimate the level of rigidity of a municipality's balance sheet as the ratio between personnel and debt repayment expenditures divided by total current revenues. The higher the level of this ratio, the greater the share of rigid expenditures on a municipality's balance sheet. In our data, this ratio takes an average value of 0.38 with a standard deviation of 0.14. If implication 1 from the model is correct, we should find that municipalities that are not constrained by fiscal rules but that do have a high level of rigidity behave similarly to municipalities limited by fiscal rules. Furthermore, we would expect the baseline effect to be driven by the group with a low level of rigidity. To test whether this is the case, in columns 1 and 3 of Table 3, we run the *Diff-in-Disc* using only the subsample of municipalities with a level of rigidity below the median. In columns

[33]For an example of heterogeneity in rigidity expenditures across the main Italian cities, see the article (in Italian) "Rigidità della spesa, quando il sindaco ha le mani legate" in Openpolis.

2 and 4, we run the regressions keeping only the municipalities with a level of rigidity above the median. We find, indeed, that the negative effect of fiscal rules on the education of politicians is driven by municipalities with a low level of rigidity. Furthermore, there are no statistically significant differences between municipalities just above and below the threshold in the subsample of municipalities with a high level of rigidity. This evidence further confirms that higher educated politicians are less likely to enter politics if they cannot enjoy a high level of discretion in setting fiscal policies.

Table 3: Financially constrained municipalities

	(1)	(2)	(3)	(4)
Dependent variable	*Mayoral candidates with university degree*		*Mayors with university degree*	
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Election Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	*Rigidity<median*	*Rigidity>median*	*Rigidity<median*	*Rigidity>median*
(Post)*(> 5000)	-0.182***	-0.002	-0.183**	-0.005
	(0.048)	(0.074)	(0.080)	(0.090)
Observations	2,436	1,495	2,334	2,361
Bandwidth	1409	1121	1342	1729
Mean outcome	0.427	0.528	0.452	0.529

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Sub-samples: 1) *(Rigidity < median)* = municipalities with a below-median level of personnel and debt expenditures as a fraction of total current revenues; 2) *(Rigidity > median)* = municipalities with an above-median level of personnel and debt expenditures as a fraction of total current revenues. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between > 5000 and Post. The outcome variable is the share of mayoral candidates with a university degree in column 1-2 and is equal to 1 for mayors with a university degree in column 3-4. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Year of election and region fixed effects added in all columns. Control variables in all columns (measured in 2001 except for numbers 5 and 6, which were measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

6 Concluding remarks

This paper investigates the effect of a reduction in policymaking discretion on the selection of politicians. We theoretically and empirically document that such a reduction, induced by the application of fiscal rules, negatively affects politicians' level of education. Beyond the political economy literature, this finding aligns with recent studies on discretion and public procurement, showing that, while political discretion can create opportunities for misuse of public money, it can also enable greater efficiency (Decarolis et al., 2021) and better

procurement outcomes (Coviello et al., 2022). Obviously, our results do not imply that FRs are generally inefficient or welfare-decreasing. This paper only documents an additional and previously unseen effect on political selection.

Our results highlight three considerations for future research. First, they underline the possibility that electing fewer educated politicians may lead to worse policies. The existing literature (Besley et al., 2011; Martinez-Bravo, 2017; Meriläinen, 2021) on the significance of educated leaders suggests that this may be the case. In the Italian context, Daniele and Giommoni (2020) observe that fiscal rules reduce corruption to a greater extent in municipalities with more educated mayors. Mitra (2020) meanwhile shows that educated mayors increase public investment in education without worsening the municipality's financial situation. Carreri (2020) uses a measure of the administrative competence of political executives that positively correlates with education to document that competent mayors are associated with better policies. The evidence in all these papers calls for further research on the policy implications of reducing the share of well-educated elected politicians.

Second, though we analyze the ex-ante quality of the political class, we do not address the latter's representativeness relative to the electorate. Indeed, that the election of fewer educated and skilled politicians represents a decrease in quality is but one facet of the story (Dal Bo et al., 2020; Carreri, 2020). The election of fewer competent individuals may also translate into a better representation of marginalized groups in terms of labor market performance and socioeconomic background. The existing literature says little, however, as to whether better political representation of marginalized groups may lead to more targeted policies towards them, an important issue that might be further explored. Third, this paper provides evidence using data from one country and strategies such as *Diff-in-Disc* and RDD. Such an approach allows to avoid the limitations of cross-country analyses. Moreover, the methodologies employed have strong internal validity and provide casual estimates. However, there is a potential cost in terms of external validity, calling for further research using data from other countries so as to understand whether our findings also apply in other contexts.

Finally, there are two policy implications related to our results. First, our findings suggest that the negative selection effect of fiscal rules should be taken into account in the design of such rules. Although local, the effect is sizeable and significant, indicating that the quality

of the candidate pool is endogenous to the rules themselves. Second, we show that reduced discretion brought about by fiscal rules compensates for the positive selection implied by higher wages paid just above the 5,000-inhabitant threshold. This suggests that one possible way to compensate for the negative selection effect of fiscal rules would be to combine them with higher pay for politicians.

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A Appendix: Additional tables and figures

Table A1: Fiscal rules in Italy: the Domestic Stability Pact (DSP)

Year	Target	Reference Year	Covered municipalities
1999	Budget Balance	1997	All
2000	Budget Balance	1998	All
2001	Budget Balance	1999	> 5000
2002	Budget Balance	2000	> 5000
	Current Expenditures	2000	
2003	Budget Balance	2001	> 5000
2004	Budget Balance	2003	> 5000
2005	Total Expenditures	2002-2004	> 5000
2006	Current Expenditures	2004	> 5000
	Capital Expenditures	2004	
2007	Budget Balance	2003-2005	> 5000
2008	Budget Balance	2003-2005	> 5000
2009	Budget Balance	2007	> 5000
2010	Budget Balance	2007	> 5000
2011	Budget Balance	2006-2008	> 5000
2012	Budget Balance	2006-2008	> 5000
2013	Budget Balance	2007-2009	> 1000
2014	Budget Balance	2009-2011	> 1000
2015	Budget Balance	2010-2012	> 1000

Notes. Domestic Stability Pact: fiscal rules enacted by the Italian central government, which apply to the covered municipalities on an annual basis. Columns: "Year" = year in which the rules were applied; "Target" = target decided by the central government for a specific year. The limits on the target decided by the central government are imposed with respect to specific past reference years, which are reported in the column "Reference years"; "Covered municipalities" = this indicates the municipalities that must apply the fiscal rules based on their resident population measured at the end of the second-most recent year. Legislative sources: annual national budget law (Legge Finanziaria) from 1999 to 2015. Other sources: Grembi et al. (2016); Chiades and Mengotto (2013). As described by Grembi et al. (2016), the main definition of budget balance used during the years has been the so-called fiscal gap, which is defined as municipal deficit net of transfers and debt service.

Table A2: Legislative population thresholds in Italy: Municipalities below 15,000

Population	Wage Mayor	Wage Ministers	Size Government	Size Council
< 1000	1,291	15 %	4	12
1000-3000	1,446	20 %	4	12
3000-5000	2,169	20 %	4	16
5000-10,000	2,789	50 %	4	16
10,000-15000	3,099	55 %	6	20

Notes. Legislative population thresholds that apply to Italian municipalities with less than 15,000 inhabitants. Columns: Population = municipal population as measured by the last Census; Wage Mayor = the wage paid to the mayor, expressed in Euros at 2000 prices; Wage Ministers = wage paid to the ministers as a percentage of the wage of the mayor; Size Government = maximum number of ministers that can be appointed in the municipal government; Size Council = number of seats in the municipal council. All wage thresholds date back to 1960, except the 1,000 and 10,000 thresholds, which were introduced in 2000. Sources: Gagliarducci and Nannicini (2013); Grembi et al. (2016).

Table A3: Descriptive statistics:
Municipalities below 5000 vs. Municipalities above 5000

	(1) Below 5000	(2) obs	(3) Above 5000	(4) obs	(5) p-value
Politicians characteristics					
Female mayors	0.088	4836	0.095	1334	0.229
Age mayors	48.23	4836	47.78	1334	0.023
High skills job mayors	0.227	4836	0.310	1334	0.000
Graduate mayors	0.373	4836	0.516	1334	0.000
Political experience mayors	8.25	4836	8.16	1334	0.490
Female mayoral candidates	0.104	4836	0.110	1334	0.213
Age mayoral candidates	48.10	4836	48.09	1334	0.936
High skills job mayoral candidates	0.213	4836	0.310	1334	0.000
Graduate mayoral candidates	0.355	4836	0.504	1334	0.000
Municipal characteristics					
South	0.252	4836	0.288	1334	0.008
Centre	0.136	4836	0.165	1334	0.006
North-West	0.504	4836	0.306	1334	0.000
North-East	0.107	4836	0.239	1334	0.000
Population density	145.64	4836	496.30	1334	0.000
Area	25.315	4836	43.145	1334	0.000
No profit associations	9.830	4836	34.327	1334	0.000
Firms per capita	0.075	4836	0.081	1334	0.000
Income per capita	9084	4836	10335	1334	0.000
% elderly	0.228	4836	0.176	1334	0.000
% 15-64 years old	0.643	4836	0.677	1334	0.000
% graduate	0.043	4836	0.051	1334	0.000

Notes. Municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. *Below 5000* = 1 for municipalities below 5,000 inhabitants. *Above 5000* = 1 for municipalities above 5,000 inhabitants. Columns (1) and (3) report the mean values for the two samples; *obs* is the number of observations; *p-value* is the p-value of the difference between the means of the two samples.

Table A4: Balance test on municipal covariates
Diff-in-Disc

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Characteristics municipal population								
Dependent variables	% university degree	% 15-64	% 65+	(log) income per capita	# firms	no-profit ass	area	population density
(Post)*(> 5000)	-0.001	-0.007	-0.002	-0.028	-0.001	-1.466	0.297	-11.640
	(0.002)	(0.004)	(0.006)	(0.049)	(0.003)	(1.358)	(5.241)	(51.774)
Observations	2,919	2,644	2,509	3,490	2,334	2,679	3,504	4,012
Bandwidth	944.6	859.8	818.2	1139	768.2	872.5	1142	1296
Panel B: Geographical characteristics municipalities, deficit and re-election/re-run status								
Dependent variables	NE	NW	CEN	SOU	Second term mayor	Re-elected		
(Post)*(> 5000)	-0.065	0.014	0.057	0.032	0.089	0.001		
	(0.050)	(0.061)	(0.040)	(0.064)	(0.068)	(0.062)		
Observations	3,865	3,772	5,105	3,182	2,706	3,412		
Bandwidth	1246	1220	1644	1034	879.8	1689		

Notes. Diff-in-disc estimates of the impact of fiscal rules on municipal covariates. Municipalities between 0-15,000. Electoral years between 1993 and 2012. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5,000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between > 5000 and Post. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Robust standard errors clustered 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 A5: Introduction of fiscal rules

	(1)	(2)
Dependent Variables	Share mayoral candidates with university degree	= 1 for Mayors with university degree
Control Function	Linear	Linear
Bandwidth	CCT	CCT
Election Year FE	No	No
Region FE	No	No
Controls	No	No
(>= 1999)*(> 5000)	-0.040	-0.043
	(0.060)	(0.077)
Observations	1,966	2,210
Bandwidth	1364	1534
Mean outcome	0.450	0.464

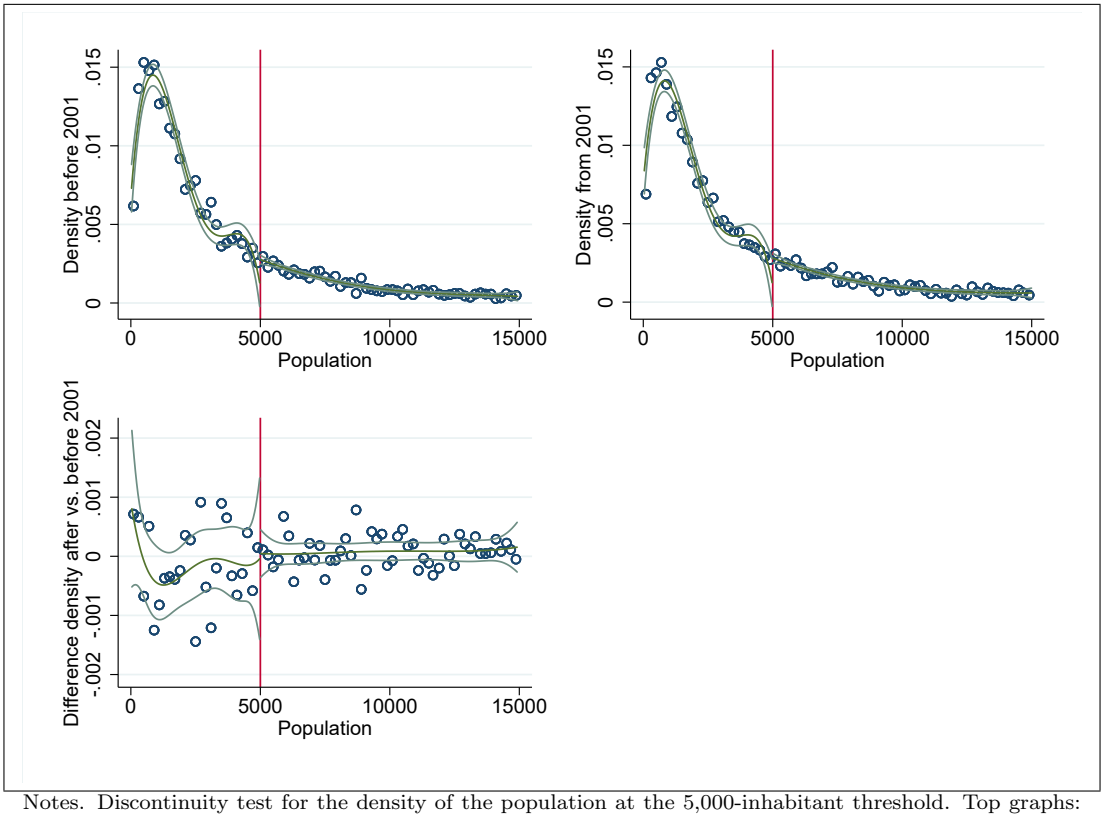
Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2000. Variables in the Table: (>= 1999)*(> 5000)= interaction between dummy = 1 for electoral years 1999-2000 and dummy = 1 for municipalities with more than 5,000 inhabitants. The outcome variable is the share of mayoral candidates with a university degree in column 1, while it is equal to 1 for mayors with a university degree in column 2. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table A6: The effect of fiscal rules on the education of politicians
Difference-in-differences estimates

	(1)	(2)	(3)	(4)
Election Year FE	No	No	No	No
Region FE	No	No	No	No
Covariates	No	No	No	No
Dependent Variables	Share mayoral candidates with university degree		= 1 for Mayors with university degree	
(> 5000)	0.154***	0.171***	0.135***	0.140***
	(0.013)	(0.014)	(0.016)	(0.017)
(Post)	0.036***	0.034***	0.023***	0.020**
	(0.005)	(0.006)	(0.007)	(0.008)
(Post)*(> 5000)	-0.019*	-0.037***	0.007	0.001
	(0.010)	(0.012)	(0.016)	(0.019)
Pre		-0.005		-0.006
		(0.005)		(0.006)
(Pre)*(> 5000)		-0.036***		-0.011
		(0.011)		(0.015)
Observations	26,005	26,005	26,005	26,005

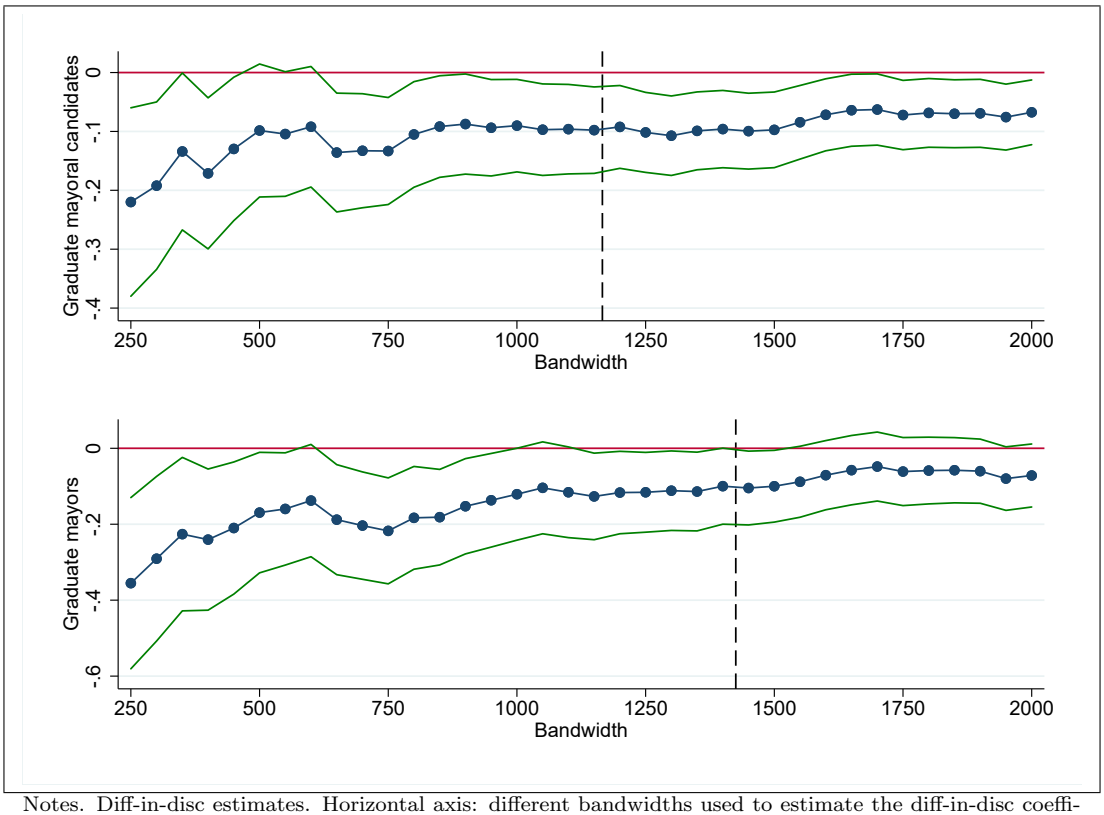
Notes. Difference-in-differences estimates of the impact of fiscal rules on the education of politicians. Municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5,000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between (> 5000) and Post; 4) (Pre) = 1 for election immediately before 2001 fiscal rules removal; 5) (Pre)*(> 5000) = interaction term between (> 5000) and (Pre). The outcome variable is the share of mayoral candidates with a university degree in columns 1 and 2, while it is equal to 1 for mayors with a university degree in columns 3 and 4. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure A1: Density test on the running variable



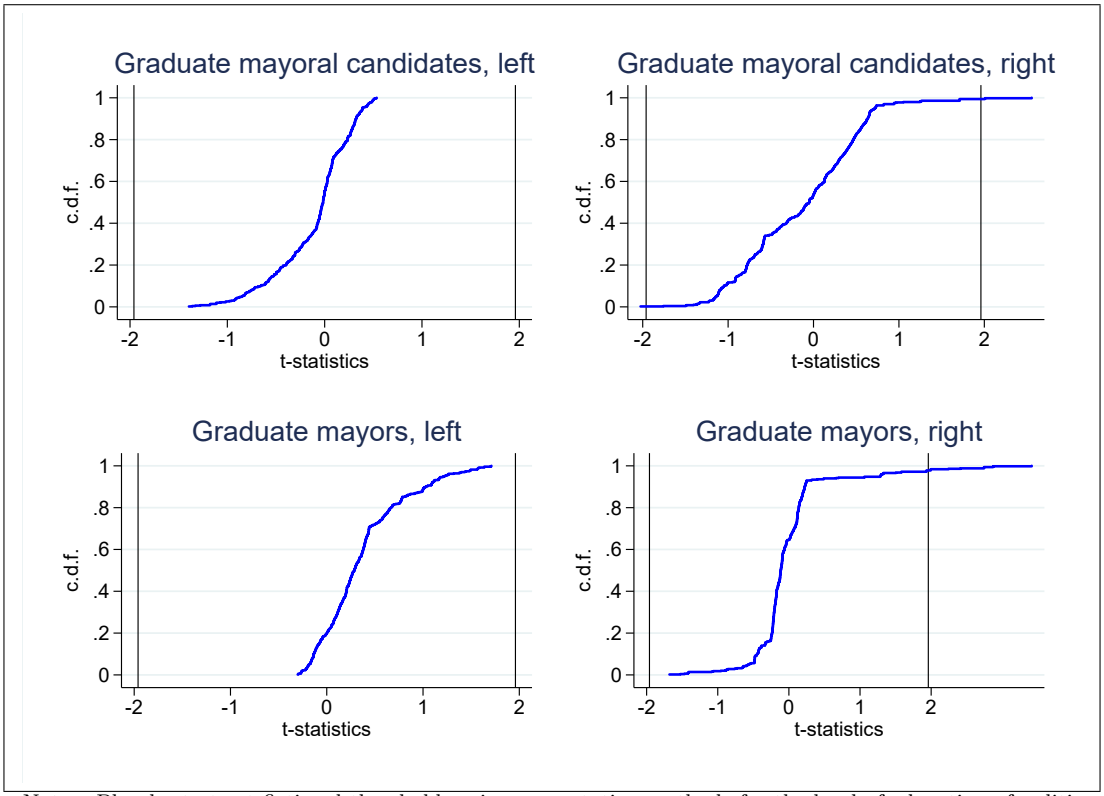
Notes. Discontinuity test for the density of the population at the 5,000-inhabitant threshold. Top graphs: (1) density test for R_{it} before 2001; (2) density test for R_{it} from 2001. Bottom graph: (1) discontinuity test for the difference between the density of average R_{it} from 2001 and the density of average R_{it} before 2001. The central green line represents a split fourth-order polynomial of the outcome variable in the normalized population, fitted separately on each side of the threshold. The grey lines represent the 95 percent confidence interval.

Figure A2: Diff-in-disc estimates: different bandwidths



Notes. Diff-in-disc estimates. Horizontal axis: different bandwidths used to estimate the diff-in-disc coefficients. Vertical axis: diff-in-disc coefficients. Dashed vertical line: optimal bandwidth calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018).

Figure A3: Diff-in-Disc
Placebo thresholds



Notes. Placebo tests at fictional thresholds using permutation methods for the level of education of politicians. The figure reports the c.d.f. of the t-statistics of a set of diff-in-disc regressions at 1,000 fictional thresholds, half below half above the 5,000 threshold (i.e., thresholds from 4,900 to 4,400, and from 5,100 to 5,600). The diff-in-disc model is run using a local linear regression. The vertical lines indicate t-statistics of -2 and 2. The top graphs report the c.d.f. of the t-statistics for the share of mayoral candidates with a university degree (respectively to the left and to the right of the 5,000 threshold). The bottom graphs report the c.d.f. of the t-statistics for the share of mayors with a university degree (respectively to the left and to the right of the 5,000 threshold).

B Appendix: Matching the state of the world

B.1 Regression discontinuity design

To test implication 2 of the model, we use a Regression Discontinuity Design (RDD) strategy based on close mixed electoral competitions, in which graduate mayors compete against non-graduate ones. We exploit the fact that, in mixed races decided by a narrow margin, election outcomes are determined by random factors and not by systematic municipal characteristics that could also affect policy outcomes. Hence, under certain assumptions, municipalities where mayors with a university degree barely lost can be used as a counterfactual for municipalities where they barely won. Following the recent developments introduced by Calonico, Cattaneo, and Titiunik (2014), Calonico, Cattaneo, and Farrell (2018), and Gelman and Imbens (2018), we estimate the following RDD strategy by local linear regression (LLR) using data at the municipality and electoral year level:

$$Y_{it} = \rho_0 + \rho_1 MV_{it} + \beta_0 Graduate_{it} + \beta_1 Graduate_{it} \cdot MV_{it} + \eta_{it} \tag{B.1}$$

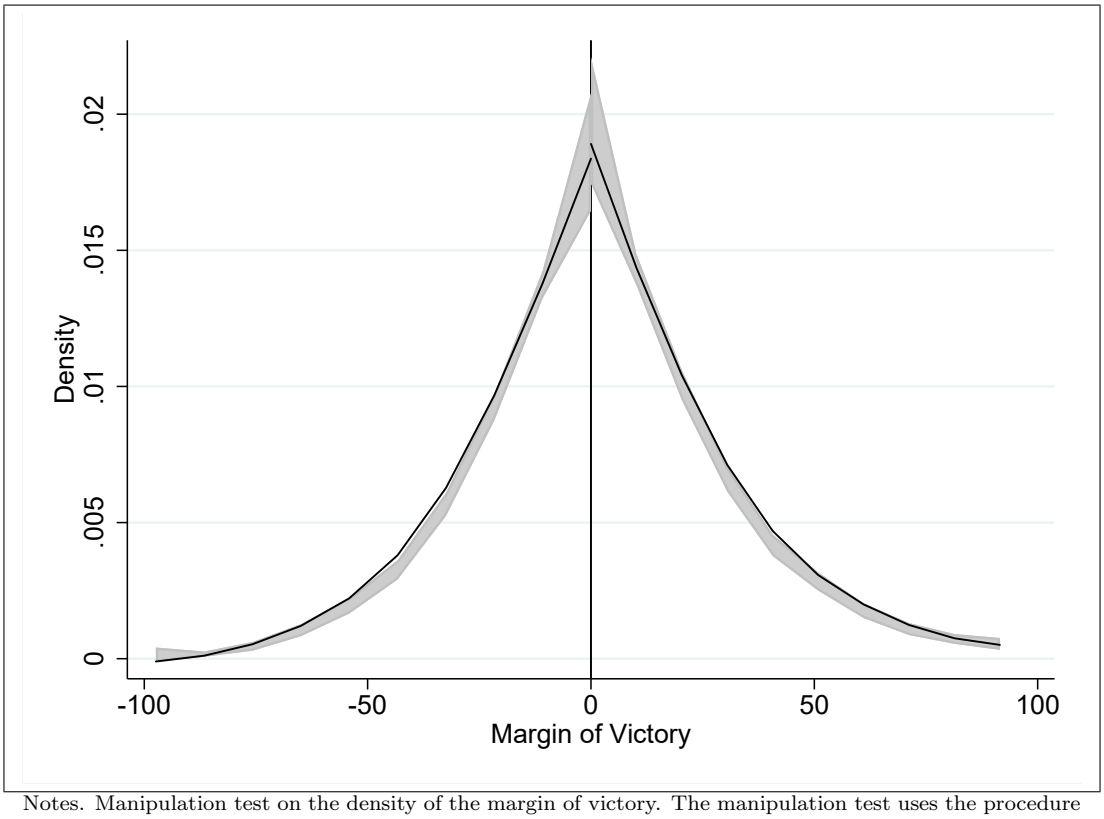
where the dependent variable Y_{it} is a dummy variable equal to 1 if a mayor chooses the correct policy to match the state of the economy.[1] The treatment is the dummy variable $Graduate_{it}$, which is equal to 1 for mayors with a university degree and 0 otherwise. The assignment to treatment is uniquely determined by the margin of victory MV_{it}, which is the difference between the vote share of the graduate candidate minus the votes share of the non-graduate one. At the threshold $MV_{it} = 0$ the level of education of the mayor sharply changes from 0 to 1.

We run model B.1 on the sub-sample of municipalities in the interval $MV_{it} \in [-h, +h]$, where the optimal bandwidth h is calculated following the MSE-optimal bandwidth of Calonico, Cattaneo, and Titiunik (2014), and Calonico, Cattaneo, and Farrell (2018). The coefficient of interest is β_0, which identifies the average treatment effect (ATE) of mayors with a university degree at the threshold $MV_{it} = 0$. In the analysis below, we report conventional RDD estimates with a conventional variance estimator (Conventional), bias-corrected

[1] As described in more detail in section B.2, Y_{it} is 1 if the mayor runs a deficit above the median when the economic growth is low or if the mayor runs a deficit below the median when economic growth is high.

RDD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RDD estimates with a robust variance estimator (Robust). We cluster standard errors at the local labor market area level. There are two main assumptions required for this identification to work correctly. First, there must be no sorting around the threshold $MV_{it} = 0$, such that voters in municipalities with narrow mixed electoral competitions are not able to manipulate the running variable MV_{it}. We test this assumption in Figures B1 and B2, using the test on the continuity of the density of the running variable proposed by Cattaneo, Jansson, and Ma (2018). The evidence in Figures B1-B2 excludes that sorting is happening.

Figure B1: Manipulation test on the margin of victory
Municipalities below 5,000



Notes. Manipulation test on the density of the margin of victory. The manipulation test uses the procedure developed by Cattaneo, Jansson, and Ma (2018). T-statistics: the conventional test statistics is 0.500, while the robust one is 0.679.

Second, observable municipal characteristics should vary smoothly at the threshold $MV_{it} = 0$. This assumption is required to guarantee that municipalities on one side of the threshold

Figure B2: Manipulation test on the margin of victory
Municipalities above 5,000

Notes. Manipulation test on the density of the margin of victory. The manipulation test uses the procedure developed by Cattaneo, Jansson, and Ma (2018). T-statistics: the conventional test statistics is 0.584, while the robust one is 1.244.

are a proper counterfactual for municipalities on the other side of the cutoff. We test this assumption in Tables B1 and B2, which confirm that municipal covariates are balanced.

Table B1: Balance test on municipal covariates
RDD, below 5000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Characteristics municipal population								
Dependent variables	% university degree	% 15-64	% 65+	(log) income per capita	# firms	no-profit ass	area	population density
Conventional	-0.000	0.003	-0.004	-0.015	0.001	0.712	1.547	-6.117
	(0.002)	(0.005)	(0.007)	(0.036)	(0.003)	(0.611)	(2.742)	(22.834)
Bias-corrected	0.001	0.004	-0.005	-0.013	0.001	0.889	2.210	-5.096
	(0.002)	(0.005)	(0.007)	(0.036)	(0.003)	(0.611)	(2.742)	(22.834)
Robust	0.001	0.004	-0.005	-0.013	0.001	0.889	2.210	-5.096
	(0.002)	(0.006)	(0.008)	(0.041)	(0.003)	(0.691)	(3.124)	(24.582)
Observations	2281	2583	2643	2624	2723	2821	2873	3134
Bandwidth	15.24	17.91	18.53	18.32	19.26	20.28	20.79	23.48
Panel B: Geographical characteristics municipalities								
Dependent variables	NE	NW	CEN	SOU	Past deficit			
Conventional	0.023	-0.032	-0.048	0.050	0.002			
	(0.036)	(0.061)	(0.043)	(0.060)	(0.005)			
Bias-corrected	0.021	-0.026	-0.065	0.068	0.003			
	(0.036)	(0.061)	(0.043)	(0.060)	(0.005)			
Robust	0.021	-0.026	-0.065	0.068	0.003			
	(0.041)	(0.068)	(0.047)	(0.066)	(0.006)			
Effective Observations	2897	2766	2336	2525	1850			
Bandwidth	21	19.62	15.63	17.34	17.44			

Notes. RDD estimates of the impact of graduate mayors on municipal covariates. Municipalities below 5,000 inhabitants. Electoral years between 2001 and 2012. Treatment variable: Graduate is a dummy variable equal to 1 when the mayor has a university degree, 0 otherwise. Estimation by RDD-LLR using the Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018) MSE-optimal bandwidth h selector. Estimates reported: conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator are reported (Robust). Municipal dependent variables in Panel A (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of elderly (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. In Panel B, the dependent variables are geographical dummy variables for different areas of Italy (i.e. North-West, North-East, Centre, South) and the deficit as a fraction of total revenues from the previous term. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table B2: Balance test on municipal covariates
RDD, above 5000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Characteristics municipal population								
Dependent variables	% university degree	% 15-64	% 65+	(log) income per capita	# firms	no-profit ass	area	population density
Conventional	-0.002	-0.001	0.002	-0.010	-0.002	3.050	3.715	-122.749
	(0.003)	(0.005)	(0.006)	(0.058)	(0.004)	(2.185)	(6.417)	(106.289)
Bias-corrected	-0.002	-0.001	0.004	-0.003	-0.003	3.597*	4.960	-141.923
	(0.003)	(0.005)	(0.006)	(0.058)	(0.004)	(2.185)	(6.417)	(106.289)
Robust	-0.002	-0.001	0.004	-0.003	-0.003	3.597	4.960	-141.923
	(0.004)	(0.005)	(0.007)	(0.064)	(0.004)	(2.486)	(7.348)	(114.794)
Observations	1101	1102	996	1054	808	1142	1152	1052
Bandwidth	20.73	20.75	17.71	19.24	13.50	21.92	22.33	19.02
Panel B: Geographical characteristics municipalities								
Dependent variables	NE	NW	CEN	SOU	Past deficit			
Conventional	0.111	-0.140	0.014	0.004	0.000			
	(0.079)	(0.098)	(0.060)	(0.074)	(0.004)			
Bias-corrected	0.105	-0.155	0.012	0.000	-0.000			
	(0.079)	(0.098)	(0.060)	(0.074)	(0.004)			
Robust	0.105	-0.155	0.012	0.000	-0.000			
	(0.087)	(0.107)	(0.068)	(0.081)	(0.005)			
Observations	887	907	1104	1236	704			
Bandwidth	15.23	15.64	20.96	24.75	15.75			

Notes. RDD estimates of the impact of graduate mayors on municipal covariates. Municipalities between 5000 and 15,000 inhabitants. Electoral years between 2001 and 2012. Treatment variable: Graduate is a dummy variable equal to 1 when the mayor has a university degree, 0 otherwise. Estimation by RDD-LLR using the MSE-optimal bandwidth h selector per Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018). Estimates reported: conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator are reported (Robust). Municipal dependent variables in Panel A (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. In Panel B, the dependent variables are geographical dummy variables for different areas of Italy (i.e. North-West, North-East, Centre, South) and the deficit as a fraction of total revenues from the previous term. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

B.2 Results on education level and policy choice

To test implication 2, we run the RDD model described by equation B.1 to investigate whether higher educated politicians have a higher probability of matching the state of the economy compared to less educated ones. Furthermore, we study whether the differences in behavior between more and less educated politicians disappear in municipalities where fiscal rules apply. We implement this analysis using the subsample of mixed electoral competitions between university graduate and non-graduate mayoral candidates, considering only electoral terms between 2001 and 2012. We employ this sample of mixed electoral competition and then we calculate the MSE-optimal bandwidth around the $MV_{it} = 0$ threshold per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). We run the RDD regression keeping the observations within this optimal bandwidth. We report the results of this analysis in Table B3. Following the procedure developed by Calonico, Cattaneo, and Farrell (2018), we report conventional RDD estimates with a conventional variance estimator (Conventional), bias-corrected RDD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RDD estimates with a robust variance estimator (Robust).

To run the analysis reported in Table B3, we build a dependent variable which is equal to 1 if a mayor chooses the correct policy to match the state of the economy in a specific year and 0 otherwise.[2] After building this variable at the year level, we collapse the data at the municipality and electoral term level. Hence, the dependent variable obtained is equal to the probability of matching the state of the economy over the electoral mandate. Panel A reports the results for municipalities not constrained by fiscal rules in the period 2001-2012, and Panel B the results for municipalities affected by fiscal rules. In column 1, we run model B.1 without control variables. In column 2, we check whether the estimated coefficients remain unchanged if we control for other personal characteristics of the mayors such as age,

[2]Specifically, to build this dependent variable, we create two dummy variables. The first is equal to 1 for mayors that run a deficit above the median in a specific year and 0 otherwise. The second dummy variable is equal to 1 for years characterized by a growth in municipal income below the median and 0 otherwise. Hence, the dependent variable for a mayor correctly matching the state of the economy in a specific year is equal to 1 when these two dummy variables are equal and 0 otherwise. Furthermore, to deal with the potential endogeneity of municipal income growth during a mayor's term, we create an alternative version of the dependent variable using the income growth predicted regressing income growth on pre-determined municipal characteristics and regional and year fixed effects. We get similar results if we use this alternative dependent variable for mayors correctly matching the state of the economy. Results can be made available upon request.

Table B3: Graduate mayors and matching the state of the economy

	(1)	(2)	(3)
Control Function	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT
Year of election FE	No	No	Yes
Region FE	No	No	Yes
Mayoral covariates	No	Yes	Yes
Municipal covariates	No	No	Yes
Panel A: municipalities below 5000			
Conventional	0.062**	0.076**	0.066**
	(0.030)	(0.030)	(0.029)
Bias-corrected	0.074**	0.087***	0.077***
	(0.030)	(0.030)	(0.029)
Robust	0.074**	0.087***	0.077**
	(0.033)	(0.034)	(0.032)
Observations	2094	2018	2005
Bandwidth	13.69	13.08	12.94
Panel B: municipalities above 5000			
Conventional	-0.021	-0.011	-0.022
	(0.038)	(0.038)	(0.036)
Bias-corrected	-0.016	-0.006	-0.018
	(0.038)	(0.038)	(0.036)
Robust	-0.016	-0.006	-0.018
	(0.046)	(0.046)	(0.042)
Observations	1165	1147	1111
Bandwidth	21.37	21.05	19.72

Notes. Municipalities below 15,000 inhabitants. Electoral terms between 2001 and 2012. Dependent variable: probability of matching the state of the economy over the electoral mandate. Treatment variable: Graduate is equal to 1 when mayor has a university degree, 0 otherwise. Estimation by RDD-LLR using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Estimates reported: conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator are reported (Robust). Mayoral covariates in columns 2-3: 1) age of the mayor; 2) political experience: years of past political experience of the mayor at any level of politics; 3) high skills job = 1 if mayor worked in a high skills occupation in the past; 4) female = 1 if mayor is a woman; 5) left = 1 for a center-left mayor. Municipal covariates in column 3 (measured in 2001 except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old) ; 3) share of population above 65 years old; 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality (square km); 8) population density; 9) past deficit from previous term. Year of election and Regional FE included in column 3. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

gender, political experience, professional background, and political orientation. In column 3, we control for municipal characteristics beyond regional and election year fixed effects. The results of Table B3 show that, where fiscal rules do not apply, higher educated mayors have a higher probability of matching the state of the economy compared to less educated ones. Conversely, we do not find statistically significant differences between graduate and non-graduate mayors in municipalities affected by fiscal rules.

Finally, the different results for municipalities below and above the 5,000-inhabitant

threshold may be due to the different wages paid to the mayors (Gagliarducci and Nannicini, 2013). To rule out this possibility and to further reinforce the results of Table B3, we repeat the RDD exercise using only fiscal outcomes measured during the 2013-2015 period, during which fiscal rules applied equally across the threshold. Conversely, during these years, the wage increase across the threshold was in place. Table B4 shows that the differences in matching the state of the economy disappear when fiscal rules apply in the same way across the threshold, as none of the estimated coefficients in the Table is statistically different from zero.

Table B4: Graduate mayors and matching the state of the economy
Years 2013-2015

	(1)	(2)	(3)
Control Function	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT
Year of election FE	No	No	Yes
Region FE	No	No	Yes
Mayoral covariates	No	Yes	Yes
Municipal covariates	No	No	Yes
Panel A: municipalities below 5000			
Conventional	-0.060	-0.038	-0.051
	(0.048)	(0.049)	(0.047)
Bias-corrected	-0.077	-0.053	-0.064
	(0.048)	(0.049)	(0.047)
Robust	-0.077	-0.053	-0.064
	(0.055)	(0.055)	(0.055)
Effective Observations	1119	1101	1059
Bandwidth	15.34	15.76	14.90
Panel B: municipalities above 5000			
Conventional	-0.087	-0.060	-0.048
	(0.074)	(0.071)	(0.074)
Bias-corrected	-0.083	-0.067	-0.048
	(0.074)	(0.071)	(0.074)
Robust	-0.083	-0.067	-0.048
	(0.089)	(0.084)	(0.088)
Observations	473	465	392
Bandwidth	15.17	16.22	12.64

Notes. Municipalities below 15,000 inhabitants. Years 2013-2015. Dependent variable = 1 in the event of above-median deficit coupled with below-median income growth or below-median deficit with above-median income growth. Treatment variable: Graduate is 1 when mayor has a university degree, 0 otherwise. Estimation by RDD-LLR using the MSE-optimal bandwidth h selector per Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018). Estimates reported: conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator are reported (Robust). Mayoral covariates in columns 2-3: 1) age of the mayor; 2) political experience = years of past political experience of the mayor at any level of politics; 3) high skills job = 1 if mayor worked in a high-skill occupation in the past; 4) female = 1 if mayor is a woman; 5) left = 1 for a center-left mayor. Municipal covariates in column 3 (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with university degree; 2) share of population between 15 and 64 years old in 2001; 3) share of population above 65 years old; 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density; 9) past deficit from previous term. Year of election and Regional FE included in column 3. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

C Appendix: Alternative stories

We deal with several potential alternative explanations to our main story. First, one potential alternative story is that the effect is due to a reduction in real terms of the difference in wages paid to mayors across the threshold. To rule out this possibility, in Table C1, we repeat the analysis using all the electoral years between 1993 and 2015, exploiting the fact that the application threshold for fiscal rules was reduced from 5,000 to 1,000 inhabitants between 2013 and 2015 (Daniele and Giommoni, 2020). Specifically, we rerun model 7 adding an interaction term between a dummy variable equal to 1 for elections from 2013 ($>= 2013_t$) and the dummy variable ($> 5000_{it}$). If the effect was actually due to a reduction in real terms of the difference in wages, we would expect the negative effect across the threshold to persist even after fiscal rules were reintroduced for municipalities below 5,000 inhabitants. The results in Table C1 show that this is not the case, given that, consistent with our story, the coefficients in front of $(>= 2013_t) * (> 5000_i)$ are small and not statistically different from zero.

Table C1: Wage depreciation

	(1)	(2)
Dependent Variables	Share mayoral candidates with university degree	= 1 for Mayors with university degree
Control Function	Linear	Linear
Bandwidth	CCT	CCT
Election Year FE	No	No
Region FE	No	No
Controls	No	No
(>= 2001)*(> 5000)	-0.102**	-0.107*
	(0.044)	(0.060)
(>= 2013)*(> 5000)	-0.033	0.009
	(0.075)	(0.090)
Observations	4,133	5,062
Bandwidth	1166	1425
Mean outcome	0.475	0.489

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2015. Variables in the Table: (>= 2001)*(> 5000)= interaction between dummy = 1 for electoral years from 2001 to 2012 and dummy = 1 for municipalities with more than 5000 inhabitants; (>= 2013)*(> 5000)= interaction between dummy = 1 for electoral years from 2013 to 2015 and dummy = 1 for municipalities with more than 5000 inhabitants. The outcome variable is the share of mayoral candidates with a university degree in column 1, while it is equal to 1 for mayors with a university degree in column 2. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Second, we show that the presence of different non-political outside options for individuals with different levels of education is not sufficient to explain our results. In principle, it may be possible for fiscal rules to affect the value of public office for individuals with a different level of education in the same way. This homogeneous effect could then affect the entry into politics of individuals with different levels of education heterogeneously given their different outside options. If higher educated individuals have a better outside option in the labor market compared to less educated ones, the overall effect could be a reduction in the quality of candidates. Table C2 seems to rule out this alternative story. Specifically, in Table C2, we use data on the municipal shares of employed individuals divided by income brackets to measure the probability that higher educated and less educated individuals have different opportunities in the labor market. To do so, we calculate a Herfindahl index of these income brackets share to measure whether employed individuals are concentrated in one or a few specific income brackets. Higher values of this index suggest a greater concentration in one specific bracket and thus more homogeneous opportunities for individuals with different levels of education. The results in Table C2 indicate that the findings do not change across municipalities with low vs. high values of the Herfindahl index, suggesting that different options outside of politics do not seem to play a particularly important role in this context.

Third, the application of fiscal rules may require the selection of more politically experienced politicians, who may be less educated. To rule out the latter explanation, in Table C3, we run the *Diff-in-Disc* model on other personal characteristics of local politicians, such as past professional background, age, gender, and past political experience. For characteristics potentially correlated with education, the estimated coefficient goes in the expected direction (i.e., a decline in the share of politicians from skilled occupations). On the other hand, gender and years of political experience do not seem to be affected by fiscal rules. The lack of an effect for political experience rules out the possibility that the application of fiscal rules may require the selection of more politically experienced politicians.[3]

Fourth, fiscal rules may affect politicians' political orientation, which in turn is correlated with their level of education. Table C4 excludes any effect of fiscal rules on politicians'

[3]Due to data limitations, it was only possible to reconstruct the past political experience for elected mayors, and not for mayoral candidates.

Table C2: Effect of fiscal rules and outside option in the private sector

	(1)	(2)	(3)	(4)
Dependent Variables	Share mayoral candidates with university degree		= 1 for Mayors with university degree	
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Election Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	Herfindal index > median	Herfindal index < median	Herfindal index > median	Herfindal index < median
(Post)*(> 5000)	-0.125***	-0.124*	-0.129*	-0.107
	(0.046)	(0.068)	(0.070)	(0.103)
Observations	2,287	1,599	2,713	1,526
Bandwidth	1233	1308	1510	1263
Mean outcome	0.411	0.569	0.422	0.574

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of mayoral candidates. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5,000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between (> 5000) and Post. The outcome variable is the share of mayoral candidates with a university degree in columns 1-2, while it is equal to 1 for mayors with a university degree in columns 3-4. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018). Election year and region fixed effects added in all columns. Control variables in all columns (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered at the municipality level are in parentheses. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

political orientation. Fifth, fiscal rules may change the profitability of being re-elected for mayors. The results in Panel B of Table A4 show that this is not the case. In addition, in Table C5, we use data at the mayoral candidate level and OLS to show that graduate mayoral candidates have better electoral performances than non-graduate ones independently of whether fiscal rules apply or not. Specifically, graduate candidates receive more votes, reach a better final ranking position, and are more likely to be elected. The results go in the same direction irrespective of whether we consider municipalities and electoral years with fiscal rules or without them. The evidence in Tables A4 and C5 suggests that voters do not change their electoral behavior due to the introduction of fiscal rules and that they maintained a preference towards more educated politicians.

Finally, educated mayors may be more corrupt than non-graduate ones. Daniele and Giommoni (2020) show that the introduction of fiscal rules should make it more challenging to extract rents, reducing the office value for individuals attracted by them. If graduates

Table C3: The effect of fiscal rules on other characteristics

	(1)	(2)	(3)	(4)
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Election Year FE	No	No	No	No
Region FE	No	No	No	No
Covariates	No	No	No	No
Dependent Variables	High skill	Age	Female	Pol Experience
Panel A: mayoral candidates				
(Post)*(> 5000)	-0.104**	1.042	0.002	
	(0.047)	(0.823)	(0.025)	
Observations	2,820	4,319	3,997	
Bandwidth	913.3	1400	1290	
Mean outcome	0.290	47.93	0.113	
Panel B: mayors				
(Post)*(> 5000)	-0.089	1.277	0.011	-0.541
	(0.062)	(1.445)	(0.034)	(0.769)
Observations	3,510	3,554	3,596	3,994
Bandwidth	1158	1168	1172	1290
Mean outcome	0.309	47.89	0.086	8.135

Notes. Diff-in-disc estimates of the impact of fiscal rules on politicians' characteristics. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between (> 5000) and Post. The outcome variables are 1) high skill: politicians from high-skill occupations; 2) Age: age of the politicians; 3) Female = 1 for female politicians; 4) Pol Experiences = years of political experience at any level of politics (for mayors only). The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018). Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

are more corrupt than non-graduates, the introduction of fiscal rules may make them less interested in entering politics. However, this does not seem to be the case. Using the Mafia index built by Calderoni (2011), which quantifies the presence of Mafia-style criminal organizations in Italian provinces, we run model (7) splitting the sample between municipalities in provinces below vs. above the median of mafia presence. As shown in table C6, the negative effect of FRs on the education of mayoral candidates is driven by municipalities in provinces with low mafia presence. These are the municipalities where corruption is less of an issue. Furthermore, as we can see from Table C7, graduate mayors do not appear to be more corrupt than non-graduate ones.[4]

[4]To measure this, we use the web archive of one of the leading Italian newspapers (La Repubblica) to find episodes of corruption linked to the mayors in the analysis. Using an algorithm based on the mayor's

Table C4: The effect of fiscal rules on ideology

	(1)	(2)	(3)	(4)
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Election Year FE	No	No	No	No
Region FE	No	No	No	No
Covariates	No	No	No	No
Dependent Variables	Left	Right	Center	Civic List
Panel A: mayoral candidates				
(Post)*(> 5000)	0.038	-0.016	0.013	-0.067
	(0.038)	(0.033)	(0.012)	(0.053)
Observations	3,653	4,309	4,701	3,394
Bandwdith	1185	1396	1529	1101
Mean outcome	0.203	0.215	0.0143	0.568
Panel B: mayors				
(Post)*(> 5000)	0.033	-0.037	0.010	-0.099
	(0.058)	(0.052)	(0.012)	(0.070)
Observations	4,023	3,841	5,194	3,261
Bandwdith	1305	1245	1680	1060
Mean outcome	0.256	0.175	0.0122	0.550

Notes. Diff-in-disc estimates of the impact of fiscal rules on the ideology of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between (> 5000) and Post. The outcome variables are: 1) Left = share of center-left candidates in Panel A, =1 for center-left mayors in Panel B; 2) Right = share of center-right candidates in Panel A, =1 for center-right mayors in Panel B; 3) Center = share of center candidates in Panel A, =1 for center mayors in Panel B; 4) Civic lists = share of independent candidates in Panel A, =1 for independent mayors in Panel B. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018). Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

first and last names, the name of the city, the years of the legislature, and a series of keywords related to episodes of corruption, we create a database of newspaper articles reporting episodes of corruption linked to the mayors in the dataset. We use this database to create a dummy variable equal to 1 for mayors found to be corrupt, and 0 otherwise. The coefficients reported in Table C7 are estimated using this dummy variable as the dependent variable.

Table C5: Candidate level regressions: graduate vs. non-graduate candidates

	(1)	(2)	(3)
Dependent Variables	Vote Shares	Ranking Position	=1 if elected Mayor
Panel A: all elections			
Graduate	1.982***	-0.087***	0.035***
	(0.215)	(0.009)	(0.005)
Observations	52,436	58,568	58,568
Panel B: fiscal rules applied			
Graduate	2.395***	-0.120***	0.042***
	(0.344)	(0.017)	(0.009)
Observations	19,301	19,318	19,318
Panel B: fiscal rules did not applied			
Graduate	1.739***	-0.070***	0.031***
	(0.263)	(0.010)	(0.006)
Observations	33,135	39,250	39,250

Notes. OLS estimates. Municipalities below 15,000 inhabitants. Electoral terms between 1993 and 2012. Dependent variables: 1) vote shares = vote share taken by mayoral candidate; 2) ranking position = position of the candidate in the final ranking of mayoral candidates; 3) =1 if elected mayor = 1 if candidate elected mayor. Independent variable reported in the Table is = 1 for mayoral candidates with a university degree, 0 otherwise. Election year and region fixed effects included in all columns. Mayoral candidate covariates included in all columns: 1) high skills job = 1 if candidate worked in a high-skill occupation in the past; 2) female = 1 if candidate is a woman; 3) age = age of the mayoral candidate; 4) independent = 1 if candidate is not affiliated to a national political party; 5) unemployed = 1 if candidate is unemployed. Municipal covariates in all columns (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita measured in 2001; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered 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 C6: The role of criminal organizations

	(1)	(2)	(3)	(4)
Dependent variable	*Mayoral candidates with university degree*		*Mayors with university degree*	
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Election Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample	*Mafia index<median*	*Mafia index>median*	*Mafia index<median*	*Mafia index>median*
(Post)*(> 5000)	-0.129**	-0.008	-0.250***	0.033
	(0.063)	(0.054)	(0.095)	(0.084)
Observations	1,464	2,341	1,722	1,994
Bandwidth	921.7	1532	1078	1304
Mean outcome	0.413	0.515	0.432	0.545

Notes. Diff-in-disc estimates of the impact of fiscal rules on the education of politicians. Original sample: municipalities between 0 and 15,000 inhabitants. Electoral terms between 1993 and 2012. Sub-samples: Mafia index<median if municipality located in a province with a low presence of Mafia-style criminal organizations; Mafia index>median if municipality located in a province with a high presence of Mafia-style criminal organizations. The mafia index comes from Calderoni (2011). Variables in the Table: 1) (> 5000) = 1 for municipalities with more than 5000 inhabitants; 2) (Post) = 1 for electoral terms starting from 2001; 3) (Post)*(> 5000) = interaction term between > 5000 and Post. The outcome variable is the share of mayoral candidates with a university degree in column 1-2 and is equal to 1 for mayors with a university degree in column 3-4. The bandwidth is calculated using the MSE-optimal bandwidth h selector per Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, and Farrell (2018). Year of election and region fixed effects added in all columns. Control variables in all columns (measured in 2001 except for numbers 5 and 6, which were measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table C7: The effect of graduate mayors on corruption

	(1)	(2)	(3)	(4)
Control Function	Linear	Linear	Linear	Linear
Bandwidth	CCT	CCT	CCT	CCT
Year of election FE	No	Yes	No	Yes
Region FE	No	Yes	No	Yes
Covariates	No	Yes	No	Yes
Municipalities	Below 5000		Above 5000	
Dependent variable = 1 if mayor corrupt				
Conventional	-0.008	0.002	-0.005	-0.029
	(0.015)	(0.013)	(0.040)	(0.038)
Bias-corrected	-0.004	0.005	-0.011	-0.029
	(0.015)	(0.013)	(0.040)	(0.038)
Robust	-0.004	0.005	-0.011	-0.029
	(0.018)	(0.015)	(0.049)	(0.046)
Effective Observations	2641	2459	1025	900
Bandwidth	18.48	16.57	17.64	14.77

Notes. Municipalities below 15,000 inhabitants. Electoral terms between 2001 and 2012. Treatment variable: Graduate is a dummy variable =1 when the mayor has a university degree, 0 otherwise. Estimation by RDD-LLR using the Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2018) MSE-optimal bandwidth h selector. Estimates reported: conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator are reported (Robust). Year of election fixed effects included in even columns. Region fixed effects included in even columns. Mayoral covariates included in columns 2 and 4: 1) female = 1 if mayor is a woman; 2) age = age of the mayor at the beginning of the term; 3) political experience = years of past political experience of the mayor at any level of politics; 4) left = 1 for center-left mayor; 5) high skills job = 1 if mayor worked in a high-skill occupation in the past. Municipal covariates in columns 2 and 4 (measured in 2001, except for numbers 5 and 6, which are measured in 2005): 1) share of population with a university degree; 2) share of active population (i.e. population between 15 and 64 years old); 3) share of seniors (i.e. population above 65 years old); 4) log of income per capita; 5) number of firms per capita; 6) number of non-profit associations per capita; 7) area of municipality in square km; 8) population density. Robust standard errors clustered at the local labor area level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

D Appendix to the theoretical framework

D.1 Formal analysis of the model

D.1.1 No fiscal rules

We solve the game by backward induction. When there are no fiscal rules, any elected politician is free to choose the policy once in office. As a consequence, at the policy stage biased politicians choose $x = 1$, unbiased politicians choose $x = s$ and as a consequence they pick the correct policy with probability ϕ^{Γ}.

Lemma D1. *Without fiscal rules, there is a unique PBNE whose policy choice is as follows*

- *Biased politicians always choose $x = 1$;*
- *Unbiased politicians choose $x = s$.*

Proof of Lemma D1. Once in office, politicians learn their bias and there is no trade off with respect to their favourite policy. Hence, biased politicians choose $x = 1$ irrespective of the state. Unbiased politicians always choose $x = s$, because $\phi^L > max[p, 1-p]$, hence the signal realization always indicates the most likely state of the world. ■

The voter anticipates the equilibrium choices described above. Since higher educated unbiased politicians behave in a better way, in expectation, V prefers to elect the candidate with $\Gamma = H$ when the election is contested.

Lemma D2. *If both candidates run, $\gamma_H^i = 1$.*

Proof of Lemma D2. At the voting stage, V anticipates the policy choices outlined in Lemma D1. Suppose both candidates run: from V's point of view, the expected utility of choosing the H candidate is $\mathbb{E}u^V(\Gamma = H) = \tau p + (1-\tau)\phi^H$, because the biased politician matches the state with probability p and the unbiased one with probability ϕ^{Γ}. It is easy to see that $\mathbb{E}u^V(\Gamma = H) > \mathbb{E}u^V(\Gamma = L) = \tau p + (1-\tau)\phi^L$ because $\phi^H > \phi^L$. ■

Combining these results, we can derive the relevant thresholds in w.

Lemma D3. *In the unique PBNE equilibrium without fiscal rules the entry threshold of politicians is defined by*

$$\bar{w}_H = \left(E + ((1-\tau)\phi^H + \tau)k\right)$$

$$\bar{w}_L = \left(1 - \frac{\bar{w}_H}{W^H}\right)\left(E + ((1-\tau)\phi^L + \tau)k\right)$$

Proof of Lemma D3. Start from the H politician. He compares (1) and (2), choosing to enter iff $w^i \leq \gamma^i(E + k\mathbb{E}_{b,\theta,s}u_H^P)$. Given Lemma D1, it is clear that $\mathbb{E}_{b,\theta,s}u_H^P = ((1-\tau)\phi^H + \tau)$. Given Lemma D2, it is clear that $\gamma^i = 1$ irrespective of the choice of the other candidate. Moving to the L politician, the logic on $\mathbb{E}_{b,\theta,s}u_L^P$ is the same. However, he knows he can win office only if H does not run, hence with probability $\left(1 - \frac{\bar{w}_H}{W^H}\right)$. ■

D.1.2 Fiscal rules

If fiscal rules are present, all politicians in office are constrained to choose $x = 0$. As a consequence,

Lemma D4. *When fiscal rules are in place, equilibrium entry thresholds are as follows:*

$$\bar{w}_H^{FR} = [E + (1-\tau)(1-p)k]$$

$$\bar{w}_L^{FR} = \left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)[E + (1-\tau)(1-p)k]$$

Proof of Lemma D4. Given our assumption that, even in case of FRs, the H candidate is chosen whenever he runs, the proof for this Lemma follows the same logic as the proof of Lemma D3. The sole difference is that now $\mathbb{E}_{b,\theta,s}u_H^P = \mathbb{E}_{b,\theta,s}u_L^P = (1-\tau)(1-p)$. The reason is that now both types of politicians, being constrained to play $x = 0$, derive utility only if $\theta = 0$ and they are unbiased. ■

D.2 Proof of the main proposition

Proof of Proposition 1. The proposition implies a comparison between $\hat{\lambda}$ and $\hat{\lambda}^{FR}$, defined using equation (5) and replacing the relevant p_H and p_L. We have:

$$
\begin{aligned}
&\hat{\lambda} > \hat{\lambda}^{FR} \qquad \text{(D.1)}\\
&\frac{p_L^{FR}}{p_H^{FR}} > \frac{p_L}{p_H}\\
&\frac{\bar{w}_H}{\bar{w}_L} > \frac{\bar{w}_H^{FR}}{\bar{w}_L^{FR}}\\
&\frac{\left(E + ((1-\tau)\phi^H + \tau)k\right)}{\left(1 - \frac{\bar{w}_H}{W^H}\right)\left(E + ((1-\tau)\phi^L + \tau)k\right)} > \frac{(E + (1-\tau)(1-p)k)}{\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)\left[E + (1-\tau)(1-p)k\right]}\\
&\frac{\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)\left(E + ((1-\tau)\phi^H + \tau)k\right)}{\left(1 - \frac{\bar{w}_H}{W^H}\right)\left(E + ((1-\tau)\phi^L + \tau)k\right)} > 1
\end{aligned}
$$

where the second line follows directly from (5), the third from the definition of p_Γ and the fact that we can cancel out the denominators because of the uniform distribution, and the last one from the equilibrium strategies already defined. Note that the inequality is always verified: first, $E + ((1-\tau)\phi^H + \tau)k > E + ((1-\tau)\phi^L + \tau)k$. Second, $\bar{w}_H > \bar{w}_H^{FR}$ and as a consequence $\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right) > \left(1 - \frac{\bar{w}_H}{W^H}\right)$ ■

D.3 Financially constrained municipalities

With respect to the baseline model, we add a second group of municipalities, those that we define as "financially constrained." The latter are characterized by a high share of rigid expenditures, such as personnel and debt repayment expenditures, which cannot be adjusted in the short run. For simplicity, we assume that those municipalities are constrained to the policy $x = 0$ irrespective of the true state of the world, even in the absence of fiscal rules, as they cannot adapt their expenditures quickly when they should respond to negative shocks. Given the above, it is easy to see that, in financially constrained municipalities, $\bar{w}_H = (E + (1-\tau)(1-p)k) = \bar{w}_H^{FR}$, irrespective of whether FR are in place or not.

Proposition D1. *The share of high education politicians willing to run in financially constrained municipalities is the same with or without fiscal rules.*

Proof of Proposition D1. Given the exogenous constraint to $x = 0$ irrespective of fiscal rules,

we have $\bar{w}_H = \bar{w}_L = (E + (1-\tau)(1-p)k) = \bar{w}_H^{FR} = \bar{w}_L^{FR}$, hence if we substitute in equation (5) we obtain $\hat{\lambda} = \hat{\lambda}^{FR}$ ■

D.4 Discussion on the theoretical framework

In this appendix we further discuss some of the assumptions and implications of the model.

D.4.1 The necessity of policy-motivated politicians

This section shows that some degree of policy motivation (irrespective of its direction) is necessary for our result.

Corollary D1. *If $k = 0$, the share of high education politicians is the same with and without fiscal rules.*

Proof of Corollary D1. Substituting $k = 0$ in the LHS of equation (D.1) and on the relevant equations of Lemma D3 and D4, we obtain

$$\frac{\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)\left(E + ((1-\tau)\phi^H + \tau)k\right)}{\left(1 - \frac{\bar{w}_H}{W^H}\right)\left(E + ((1-\tau)\phi^L + \tau)k\right)} = \frac{\left(1 - \frac{E}{W^H}\right)E}{\left(1 - \frac{E}{W^H}\right)E} = 1$$

Hence, $\hat{\lambda} = \hat{\lambda}^{FR}$. ■

Intuitively, when $k = 0$, FRs have no effect on the incentives of H politicians: they get E for being in office irrespective of the policy they choose. Hence, their probability of running is the same, and nothing changes for L politicians as well.

On the other hand, the observed effect of FR holds if politicians are purely policy motivated and if the winning probability does not enter in their decision. In particular:

Corollary D2. *Assume that $E = 0$ and $k > 0$. In this case, $\hat{\lambda} > \hat{\lambda}^{FR}$.*

Proof of Corollary D2. Substituting $E = 0$ in the LHS of equation (D.1) and on the relevant equations of Lemma D3 and D4, we obtain

$$\frac{\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)\left((1-\tau)\phi^H + \tau\right)}{\left(1 - \frac{\bar{w}_H}{W^H}\right)\left((1-\tau)\phi^L + \tau\right)}$$

with $\bar{w}_H = (1-\tau)\phi^L + \tau)k > (1-\tau)(1-p)k = \bar{w}_H^{FR}$. Hence, $\hat{\lambda} > \hat{\lambda}^{FR}$. ■

Corollary D3. *Assume that politicians receive w^i also when he runs and loses. In this case, $\hat{\lambda} > \hat{\lambda}^{FR}$.*

Proof of Corollary D3. With this assumption, equations (1) and (3) become $\gamma^i \left(E + k\mathbb{E}_{b,\theta,s} u_H^P\right) + (1-\gamma^i)w^i$ and $\gamma^i \left(E + k\mathbb{E}_{b,\theta,s} u_L^P\right) + (1-\gamma^i)w^i$ respectively. This means that $\bar{w}_\Gamma$ does not depend on γ anymore. However, replacing in the LHS of equation (D.1), we obtain

$$\frac{\left(E + ((1-\tau)\phi^H + \tau)k\right)}{(E + ((1-\tau)\phi^L + \tau)k)} > 1$$

Hence the result holds. ■

D.4.2 Education and bias

Suppose bias is correlated with education, i.e. we have τ_H and τ_L. We can derive a sufficient condition for our result to hold even in the most extreme case (i.e. $\tau_H = 0$ and $\tau_L = 1$), keeping the condition that H politicians are preferred by V.[5]

Proposition D1. *Assume $\tau_H \neq \tau_L$. If $E \geq \frac{W^H - \phi^H k}{\phi^H + p}$ then $\hat{\lambda} > \hat{\lambda}^{FR}$ for every τ_H, τ_L.*

Proof of Proposition D1. Using (D.1), but noticing that we cannot simplify the RHS as before, we have that $\hat{\lambda} > \hat{\lambda}^{FR}$ iff

$$\frac{\left(E + ((1-\tau_H)\phi^H + \tau_H)k\right)}{\left(1 - \frac{\bar{w}_H}{W^H}\right)(E + ((1-\tau_L)\phi^L + \tau_L)k)} > \frac{(E + (1-\tau_H)(1-p)k)}{\left(1 - \frac{\bar{w}_H^{FR}}{W^H}\right)[E + (1-\tau_L)(1-p)k]} \tag{D.1}$$

Note that the LHS of (D.1) is decreasing in τ_L and the RHS is increasing in τ_L. Hence, the condition is least likely to hold if $\tau_L = 1$. Furthermore, the LHS of (D.1) is increasing in τ_H and the RHS is decreasing in τ_H. Hence, the condition is least likely to hold if $\tau_H = 0$. Replacing $\tau_L = 1, \tau_H = 0$ in (D.1), we obtain

$$\frac{\left(E + \phi^H k\right)}{\left(1 - \frac{E + \phi^H k}{W^H}\right)(E + k)} > \frac{(E + (1-p)k)}{\left(1 - \frac{E + (1-p)k}{W^H}\right)E}$$

[5] This translates into the assumption that $(1-\tau_H)\phi^H + \tau_H p > (1-\tau_L)\phi^L + \tau_L p$, i.e. $\tau_L > \tau_H \frac{\phi^H - p}{\phi^L - p} - \frac{\phi^H - \phi^L}{\phi^L - p}$.

It is clear that the numerator of the LHS is bigger than the numerator of the RHS. As a consequence, the condition is surely true if $\left(1 - \frac{E+\phi^H k}{W^H}\right)(E+k) \leq \left(1 - \frac{E+(1-p)k}{W^H}\right)E$. Rearranging, this simplifies to $E \geq \frac{W^H - \phi^H k}{\phi^H + p}$. ■