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Austerity and Distributional Policy

Abstract

What are the effects of austerity on distributional policy? We exploit the autonomy of Italian municipalities in setting non-linear income taxes and the exogenous introduction of a fiscal rule to show that austerity increases tax progressivity. Consistent with this evidence, we find that in a panel of countries austerity correlates with higher marginal tax rates on top, but not on average earners. The increase in progressivity in Italy is driven by mayors having college-degree or working in high-skill occupations, while less-educated or lower-skilled mayors raise taxes uniformly. In the first post-reform election, mayors of former type have higher reelection odds.

JEL-Codes: D780, H240, H700.

Keywords: austerity, fiscal rules, non-linear income taxation, difference-in-discontinuity.

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1 Introduction

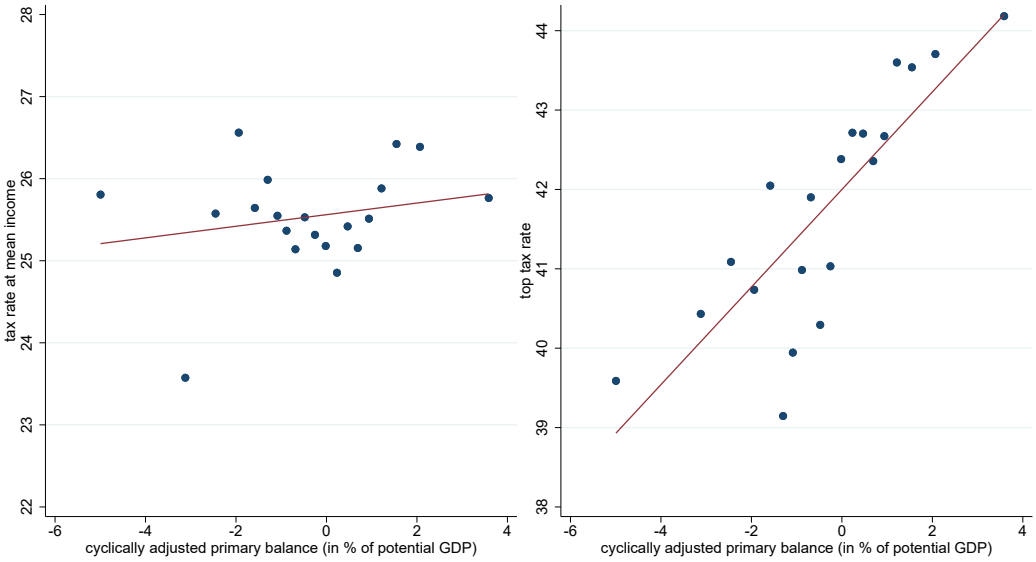
Large fiscal stimulus packages, such as those enacted during the global financial crisis of 2007-09 or those in response to the Covid-19 pandemic of 2020, led many governments to engage in some form of austerity in years following the crises. A large academic and policy literature debates the efficiency aspects of fiscal adjustment reforms (see, among others, Alesina et al., 2019). The social and distributional effects of austerity have recently also come under scrutiny by economists, and perhaps even more so by the general public. The popular belief is that austerity hurts the poor disproportionately (Blyth, 2013; Mendoza, 2014; Varoufakis, 2016), and that it has far reaching consequences on social outcomes such as on health (Stuckler and Basu, 2013), violence (Cooper and Whyte, 2017), gender equality (Karamessini and Rubery, 2013), aspects of local governance (Phillips-Fein, 2013), and election outcomes (Dal Bo et al., 2018; Fetzer, 2019).

In this paper we study the effects of austerity on distributional policy. We start by investigating the relationship between cyclically adjusted primary balance (as a measure of austerity) and statutory personal income tax rates (as a measure of distributional policy) in a panel of countries. The estimated relationship, plotted in Figure 1 and shown in Table A.1, shows that conditional on country and year fixed effects, a 1% increase in the cyclically adjusted primary balance to GDP ratio is associated with a 0.6 percentage point increase ($p < 0.05$) in the marginal income tax rate at the top (right panel), while we observe no correlation between austerity and marginal income tax rates at mean levels of income (left panel). This preliminary evidence suggests that austerity is associated with higher not lower tax progressivity.

Of course, it is hard to draw definitive conclusions from this exercise, since these regressions do not have a causal interpretation. Therefore, our aim in this paper is to provide the first quasi-experimental evidence on the effect of fiscal austerity on distributional policy.[1] To do so, we study a large exogenous reduction of

[1]Previous quantitative work has mostly appeared in response to the global financial crisis, and it usually finds that periods of fiscal austerity are associated with an increase in income inequality

Figure 1: Fiscal austerity and tax rates at mean (left) and top (right) incomes



Notes: This graph shows estimates from the following regression $y_{it} = \gamma_i + \lambda_t + \beta capb_{it} + \delta X_{it} + \epsilon_{it}$, where y_{it} is either the tax rate at mean incomes (left panel) or at top incomes (right panel), $capb_{it}$ is the cyclically adjusted primary balance in percent of potential GDP, X_{it} includes log GDP per capita and log population as control variables, γ_i are country fixed effects and λ_t are year fixed effects. The sample and data are described in Table A.1.

the fiscal space of Italian municipalities caused by the imposition of a fiscal rule by the national government. More specifically, our quasi-experiment relies on a reform in 2013 that extended the budget surplus requirement of the Domestic Stability Pact (DSP) to previously exempted municipalities based on a population cutoff (1,000 inhabitants) giving rise to a difference-in-discontinuity design. Italy is well-suited to study our research question due to the substantial autonomy that Italian municipalities have over setting local non-linear income taxes.

We find that local governments respond to the introduction of the fiscal rule by increasing income tax rates.[2] Crucially, the increase in tax rates is monotonic along the income distribution, and only becomes significantly larger than zero for taxpayers located above the median taxable income. This finding is consistent with

(Ball et al., 2013; Heimberger, 2018; Woo et al., 2013). The microsimulations of Avram et al. (2013) and Paulus et al. (2016) on several European countries present a more nuanced picture on the distributive effects of austerity that depend on country contexts and measures of austerity.

[2]This is consistent with official bills legislating the tax increases, which often report the need to comply with the DSP as the main reason. See for example the bills by Cassano Spinola (1,600 inhabitants) in December 2013, by Castel di Lucio (1,250 inhabitants) in November 2013, by Cerva (1,100 inhabitants) in December 2013, and by Mairago (1,400 inhabitants) in September 2014.

the predictions of the median voter theorems of Meltzer and Richard (1981) and Bierbrauer et al. (2020). The relative effects are quite sizeable, with the tax rates on earners in the top decile of the municipal income distribution increasing by 13% compared to the sample mean, and by about 3.5 times compared to the lowest decile. Part of this effect is driven by municipalities switching to a progressive schedule in tax rates, and part of it is due to increases in the level of the exemption threshold.

Since local income tax rates are small in absolute magnitude in Italy, these reform-induced tax rate changes imply only small increases in realized tax revenues.[3] Whereas annual income tax revenues increase on average by about 5€ per capita, revenues from the top bracket increase by an order of magnitude more, amounting to about 73€ per capita on average. Contextual evidence, as we discuss in more detail in Section 2.4, supports the conjecture that, unlike its small absolute size, the local income tax is politically a very salient tax tool of redistribution at the local level.[4] The size of the reforms which we study here are also in line with the sizes of the reforms that much of the contemporaneous literature on welfare analysis considers. The reason is the common approximation that marginal individuals who behaviorally respond to a reform experience no net benefits. While this assumption will likely hold for small reforms, it may fail for larger ones (for an early application see, Harberger (1964), for applications in taxation, see, Bierbrauer et al. (2020) and Saez (2001), and for general discussion and possible approaches for analyzing large reforms, see, Finkelstein and Hendren, 2020; Kleven, 2020). Considering other policy instruments that can potentially be employed locally for redistributive purposes, we do not find evidence that the reform affects other local taxes or non-tax revenues raised by municipalities, including the property tax.[5] We also do not find evidence

[3]Note that we do not have to make assumptions on the elasticity of taxable income since these revenue effect estimates are based on realized revenues following the tax reforms.

[4]For example, the deputy mayor of Corciano (21,000 inhabitants) said in 2019: "For those who like me earn 1,250€ net per month, the increase is equal to 19.32€ per year" and concluded that "by giving up one pizza a year we help 5,349 citizens who earn less than us".

[5]Potentially the property tax has distributive implications, but since 2013 there was limited scope to increase revenues using this instrument. See Appendix A.4 for more details.

for adjustments in total or redistributive spending, suggesting that a reduction of public goods provision is unlikely to offset the progressive effects of the local income tax.

We interpret our findings as the impact of austerity on distributional policy. Considering the introduction of the DSP as a case of austerity is natural because it necessarily required a fiscal adjustment in municipalities where the rule bound. Consistent with this interpretation, previous evidence shows that the DSP induces substantial fiscal consolidation (Chiades and Mengotto, 2015; Coviello et al., 2019; Grembi et al., 2016). Contextual details of the Italian economic situation of the time further reinforce our interpretation: the reform took place in the midst of a severe recession caused by the sovereign crisis, with Italian real GDP shrinking by 3% in 2012 and by 1.8% in 2013, while the central government cut transfer to municipalities in several occasions between 2009 and 2015 (see Figure B.1 and Marattin et al., 2019 for details). The DSP, vertically imposed by the national administration upon municipalities, became a symbol of austerity in the eyes of local administrators[6] and was grossly unpopular among mayors across the political spectrum.[7]

Our paper contributes to a large and important strand of literature studying the political economy of taxation (for reviews, see Acemoglu et al., 2015; Persson and Tabellini, 2002) by providing an empirical counterpart to results that by and large remain theoretically grounded. Past research is based on models of voting over tax schedules with competition between parties (Downs, 1957) and candidates (Besley and Coate, 1997; Osborne and Slivinski, 1996; Panunzi et al., 2020). While

[6]For example, the national representative of the Italian mayors saluted the abolition of the DSP in 2016 with these words: "We have recently ended a period of cuts, austerity measures, progressive reduction of resources [...]. A difficult period that was interrupted, fortunately, with the 2016 Budget Law which [...] resulted in [...] the end of indiscriminate transfer cuts, in the abolition of the DSP [...].

[7]For example, at a rally against the DSP in November 2012 hundreds of mayors from all major parties rallied behind a banner saying "Let us set our municipalities free from the stupidity pact." According to news accounts, the extension of the DSP to municipalities below 5,000 inhabitants in 2013 sparked similar outrage among mayors of these towns.

most of this literature, such as Meltzer and Richard (1981), analyze the political economy of linear income taxes, our contribution is to study non-linear taxes, which are much more prevalent in practice. In particular, our baseline result that, upon an exogenous shock governments implement tax reforms using the instruments of marginal income tax rates and exemption thresholds such that tax rates increase monotonically along income with positive values for taxpayers located above the median earner is consistent with Bierbrauer et al. (2020) who characterize the conditions of politically feasible non-linear tax reforms. In addition to this theoretical work, our evidence is in line with historical explanations for the occurrence and rise of progressive taxation. This literature emphasizes the role of compensatory arguments as the main mechanism behind the popular support and ultimately the implementation of progressive taxes (Scheve and Stasavage, 2010, 2012, 2016). The idea is that high taxes on the rich allow politicians to compensate the majority of relatively poor voters for some fundamental unfairness induced by the state. Given that in our sample period Italy endured a double dip recession and that the DSP was very unpopular, this line of argument is also consistent with our results.

Next, we study whether mayors, the crucial decision makers at the local level, respond to austerity in a homogeneous manner. Our analysis is motivated by the theoretical work of Bierbrauer and Boyer (2013), who introduce vote-share maximizing politicians with ex-ante valence differences in a Mirrleesian model of income taxation. They show that in equilibrium the high-valence candidate is able to capitalize on her advantage and target the majority consisting of relatively poorer voters by proposing a progressive tax schedule, whereas the low-valence politician is left to lobby for the votes of the rich. In our empirical application, we find that the reform-induced tax progressivity is driven by mayors with a college degree or working in a high-skill occupation, while other observable characteristics, such as age, gender, party affiliation, among others, do not play a meaningful role. On the contrary, mayors without a college degree or those working in low-skilled occupations rely on flat increases in the local income tax to comply with the reform. To address the

issue of selection of mayors, we compare the outcomes of the two types of politicians elected in close races. This empirical finding is consistent with Bierbrauer and Boyer (2013) assuming that the education or skill-level of the mayor is a good proxy for the theoretical and rather abstract concept of valence.

The latter result is also related to the literature on how the quality of politicians matters for policy outcomes. High-skilled politicians have been shown to increase economic growth (Besley et al., 2011), to improve fiscal capacity by investing in tax collection (Ferraz et al., 2018) to provide a higher quality of public goods (Martinez-Bravo, 2017), among other findings. We extend this literature by showing that the competence of politicians also matters for distributional policy. This is true as long as the education or skill-level of the mayor is a reliable measure of competence. Section 6.1 contains a more detailed discussion of how the existing literature tries to empirically measure quality as well as valence of politicians. While our evidence on the differential response of mayors to austerity is consistent with the valence argument of Bierbrauer and Boyer (2013), it may also be driven by other mechanisms. For example, it is plausible that more educated mayors are more able to understand non-linear tax systems as well as the electoral implications that these reforms potentially generate. Although we do not find this hypothesis very likely, we ultimately cannot disentangle the specific mechanism or mechanisms that are at work.[8]

Finally, we test whether the introduction of the DSP had electoral consequences for the incumbents. While we do not find such evidence for the average mayor, we show that differences in adjustment strategies between high- and low-skilled mayors made for large differences in electoral outcomes. In the first election following the imposition of the fiscal rule, low-skilled incumbents were on average 30 to 37 percentage points less likely to be reelected conditional on running for office again, whereas high-skilled mayors did not experience a significant decline in their

[8]One reason is the availability of a simple tax calculator, as described in Section 2.3, which mayors can easily use to simulate the effects of reforms in local marginal tax rates and exemptions.

reelection prospects. Crucially, these differences in reelection odds only manifest after the reform, and not before. These findings suggest that politicians implement progressive tax reforms in order to stay in office, and that high-skilled mayors are more able or more willing to use such a strategy than low-skilled mayors.

This finding relates to a rather polarized literature interested in understanding the political costs of fiscal austerity. One strand of this literature finds that incumbent politicians do not face electoral costs when implementing fiscal consolidations at the national level (Alesina et al., 2012; Arias and Stasavage, 2019; Brender and Drazen, 2008).[9] On the other hand, a number of papers show that fiscal austerity has negative effects on voter support for the incumbent (Hübscher et al., 2018; Talving, 2017) as well as on broader socio-political outcomes such as increasing support for right-wing populism (Dal Bo et al., 2018; Fetzer, 2019), or increasing social unrest (Ponticelli and Voth, 2019). We contribute by showing that austerity can indeed carry significant electoral costs, but that these costs depend on the consolidation strategy. In particular, we show that electoral costs can be mitigated by mainly increasing taxes on high-income earners.

2 Institutional Setup

2.1 Municipal Fiscal Rule

Since 1999, Italian municipalities have been subject to a fiscal rule, the Domestic Stability Pact (*Patto di stabilita' dei comuni*), introduced by the national government.[10] Originally, all municipalities were subject to the fiscal rule, but in 2001

[9]Possible explanations are that voters are fiscally conservative (Peltzman, 1992), that leaders implement fiscal austerity in times and as part of policy packages that allow them to electorally survive these reforms (Bansak et al., 2020), or that the divergent framing of the same issue provided by partisan media mitigates voter responses (Barnes and Hicks, 2018).

[10]The main goal of fiscal rules is to achieve fiscal sustainability. Currently over ninety countries have such rules (Eyraud et al., 2018). Asatryan et al. (2018) find constraining effects of balanced budget rules on debt, but only for a class of rules that are enshrined in national constitutions, while Eliason and Lutz (2018) show that a comprehensive state-level rule in Colorado does not affect public finances, which is partly due to non-compliance with the rule. A meta-study by

those below 5,000 inhabitants were excluded. In 2013, the threshold was lowered to 1,000, which is the reform that we exploit. Finally, in 2016, the Domestic Stability Pact was abolished and a balanced budget rule for all municipalities was introduced.

In our period of analysis, the Domestic Stability Pact's target object has always been the *Saldo Finanziario*, which is defined as the difference between expenditures and revenues, net of repayment of outstanding debt and of lending. Some budget items were always or occasionally excluded from the *Saldo Finanziario* (e.g. spending for natural disaster relief, EU structural funds). The formula to calculate the numerical target varied over the years, but it was usually defined as a function of budget items in previous years (see Table B.1).

Monitoring of compliance by the central government was tightened in 2008 with the introduction of a compulsory reporting system, and of severe punishment for non-compliers by the central government (Coviello et al., 2019). For instance, punishments include bans on hiring, cuts of transfers from the central government (proportional to the deviation from the rule), salary cuts to mayors and city councilors, a growth cap on current spending at zero percent as well as a ban on new municipal debt. Qualitative evidence from the Ministry of the Interior suggests that the central government implemented the reform quite thoroughly.[11] Dovis and Kirpalani (2020) show that the fiscal behavior of local governments will crucially depend on central government's reputation, and the strict regulations and enforcement practices of the Italian context suggest that it is very unlikely that Italian local governments tried not to comply with the DSP.

Heinemann et al. (2018) finds that numerical fiscal rules constrain fiscal policy, but results become much less assuring once one accounts for endogeneity. Fiscal rules have also been shown to curb corruption (Daniele et al., 2019), to alleviate political budget cycles (Repetto, 2018) and to worsen the selection of politicians (Gamalerio, 2019).

[11] More than one hundred municipalities faced legal procedures according to ministerial decrees available on the website of the Ministry of the Interior.

2.2 Municipal Governance

Municipal governments are composed of a city council, an executive committee, and the mayor. In municipalities with less than 15,000 inhabitants, each candidate for the mayoral office has to be supported by a list of candidates for the city council. Voters cast a single vote for a mayoral candidate, and can express one preference vote for one council candidate within the same list. The mayoral candidate who gets the most votes is elected as mayor. The seats in the city council are split as follows: 2/3 to the list of the mayor, and 1/3 split across the other lists in proportion to their votes shares. The mayor appoints the members of the executive committee, and can also remove them from office at any time. The mayoral term is five year long, and the mayor cannot serve for more than two consecutive terms.[12] These institutional details make the mayor the most important player in municipal politics, while the city council's influence is more limited. The list supporting a mayoral candidate is sometimes backed by national-level parties or coalitions, but is often independent (so-called civic lists), especially in small municipalities. Also, since being a politician in a small town is not a full-time job, most mayors work in their normal job while being in office.

2.3 Municipal Fiscal Policy

The municipal budget is financed with transfers from higher levels of government and international institutions, and by municipal resources such as local taxes and fees connected to the use of public services. Local taxation plays an important role in municipal revenues, averaging about 21% of total revenues in our sample period (see Figure B.1). The three largest tax instruments in terms of revenues are the property tax, the local income tax and the waste tax, accounting for 8.7%, 4.4% and 7.9% of total revenues in 2015 respectively. In this paper, we focus on the local income tax surcharge, as it allows different degrees of progressivity and its distributional

[12]This was extended to three terms in 2014 for municipalities below 3,000 inhabitants.

impact is straightforward. The property tax and the waste tax potentially also have distributional consequences, but those are more complicated to detect and to analyze.[13] Furthermore, the upper bound on the main local property tax rate was significantly decreased by the national government in 2013 and 2014, leaving limited scope to increase revenues from this instrument in response to the introduction of the DSP.[14]

In 1999, the local income tax was introduced as a municipal surcharge on the national income tax to grant municipalities more tax autonomy. In our sample period, the income brackets of the national income tax were split at 15,000€, 28,000€, 55,000€, and 75,000€, with their respective marginal tax rates being 21%, 27%, 38%, 41% and 43%.[15] In general, the tax base is composed of wage income, pension income, self-employed income, capital income, rents, and other sources of income. However, income from several sources can be subject to alternative and more favorable taxation (e.g. rents from real estate, investment in government bonds, self-employed income below a certain threshold), so the bulk of the taxable income consists of wage and pension income.[16]

The revenues from the municipal surcharge are based on the residency principle and flow completely to the municipal budget. Starting in 1999, the law allowed municipalities to apply uniform tax rates of up to 0.5% of taxable income on top of the national tax rates. In the period from 2007 to 2011, the cap was raised to 0.8% and municipalities were given the autonomy to set an exemption threshold: tax payers with income below the threshold were fully exempted from the tax, while those above would pay a tax calculated on their total income. Since 2012, municipalities can also set differentiated tax rates in every bracket of the national

[13] For more information on the distributional consequences of these fiscal instruments see Messina and Savegnago (2014) and Messina et al. (2018).

[14] We test the effects on the property tax and report the results in Appendix A.4 together with additional institutional details.

[15] The final tax bill is the gross tax bill net of deductions (*detrazioni*). The gross tax bill is calculated applying the tax rates on taxable income. The taxable income is calculated as total income net of exemptions (*deduzioni*).

[16] Approximately 80% both in terms of taxpayers and of taxable income in 2011.

income tax schedule. In other words, since 2007 municipalities can levy non-linear income taxes. The increase in flexibility of this tax instrument was coupled with technical assistance from the Ministry of Finance: at least since 2011, municipal officials have access to an online calculator that uses individual level data from the tax administration allowing to simulate how revenues and tax base respond to changes in tax rates and in the exemption threshold. In particular, users can vary the following parameters: tax revenues, tax rates, the exemption threshold, and the number of exempted tax payers (broken down by employees, retirees and self-employed). This setting allows us to study the progressivity of income taxation at the local level.

The adoption of differentiated tax rates by municipalities has evolved quickly over time and increased even further with the 2012 reform (Giommoni, 2019). Restricting attention to small municipalities (below 2,500 inhabitants), no municipality operated under a regime with an exemption threshold and a flat tax, 67% implemented a flat tax without exemption, and 33% did not introduce any surcharge in 2007. In 2015, 8% operated a system with five tax rates, with or without exemption, 12% implemented a flat tax with exemption, 56% implemented a flat tax without exemption, and 24% did not have any surcharge. Conditional on having a exemption threshold, the average threshold is about 10,000€ with considerable variation around the mean (see Figure B.2).

Municipalities account for about 10% of total public expenditures (Grembi et al., 2016). They are responsible for providing a variety of public services, such as administrative services (30% of municipal expenditures in our sample period 2007-2015), waste and water management (24%), public transport and maintenance of municipal roads (15%), social services (8%), education services (7%), culture and recreation (5%), economic development and tourism (3%), and local police and judiciary (2%).

Spending on social and educational programs is of special importance to us, given their potential redistributive nature and Italian municipalities' relatively large

discretion over these items. Social spending includes, among others, assistance to poor people, child care, or care for elderly. Education expenditures on the municipal level comprise of spending for pre-school and primary school services, such as refectories and school buses. In our sample period, Italian municipalities are only allowed to take up loans to finance new investment expenditures if the total amount of interest paid was lower than a certain fraction of revenues from taxes, fees and transfers.[17] The main source of borrowing for small municipalities are loans from the Italian Public Investment Bank (*Cassa Depositi e Prestiti*) accounting for almost 80% of debt holdings.

2.4 The Local Income Surcharge in Municipal Politics

Anecdotal evidence from newspapers, social media, electoral platforms and council's minutes suggest that incumbent mayors often refer to the local income surcharge tax in public statements. When raising the exemption threshold as well as the tax rates for high incomes, mayors underline that these reforms increase progressivity, are fair, and help disadvantaged people with little cost for others. For example, the mayor or Brandico wrote in his 2014 electoral platform: "To help disadvantage people, we need to raise the exemption threshold [...] and to introduce progressivity (by raising tax rates more for higher brackets)". The mayor of Milano wrote on Facebook in 2019: "[...] The exemption threshold raises from 21,000 to 23,000 euros, extending the no tax area to 50,000 more citizens. [...] It is the right thing to do to support households and workers." There are also instances when opposition politicians blame incumbents for not exploiting the tax flexibility and implementing a flat tax instead.[18] These anecdotes suggest that the local income surcharge is an important topic in municipal politics, and are consistent with recent empirical evidence that the introduction of differentiated rates generated an election cycle:

[17] The fraction varied over time, from 15% in 2007 to 10% in 2014.

[18] For example, the opposition group *Borgo in Comune* in Borgo San Lorenzo (18,000 inhabitants).

the surcharge tends to decrease before elections and increase afterwards (Giommoni, 2019). As documented in the same paper with data on google searches and surveys, the municipal income surcharge is a salient fiscal instrument for taxpayers. This is consistent with the fact that the amount paid due to this tax is usually clearly visible on the monthly payslips received by employees and retirees.

3 Data

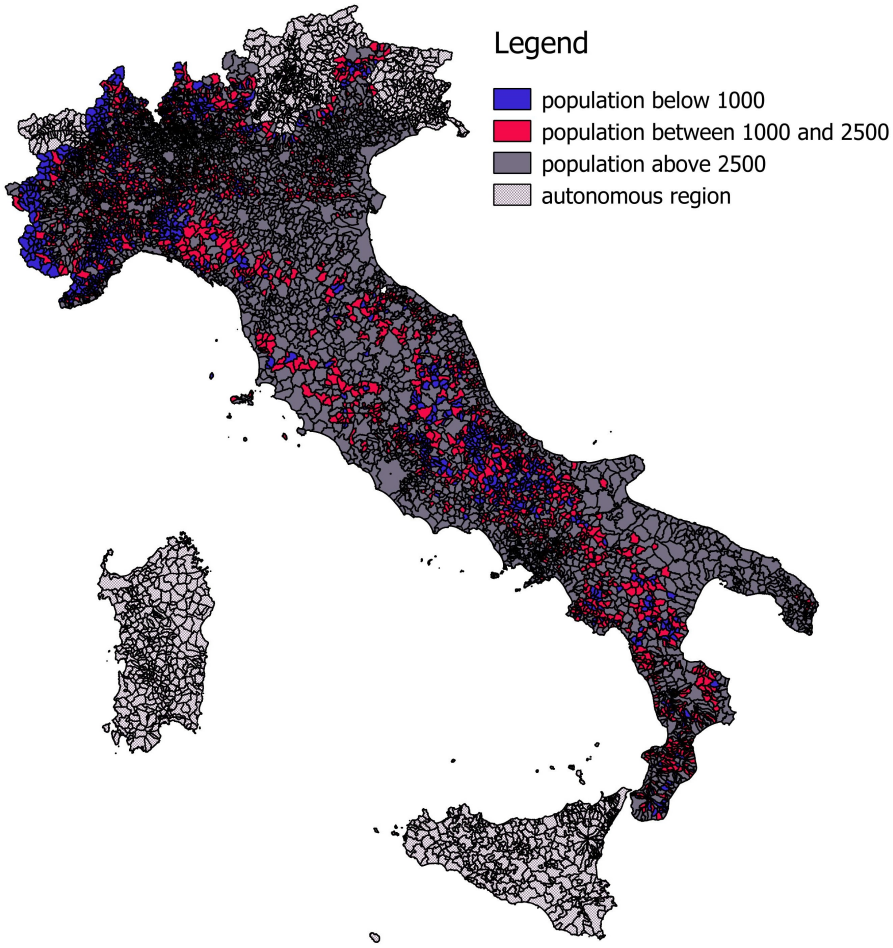
3.1 Sample

Our sample starts in 2007, the first year municipalities were allowed to levy non-linear income taxes, and ends in 2015, since all municipalities were subject to a new rule in 2016. We apply a number of restrictions on our sample. First, we drop all observations that are part of a union for inter-municipal cooperation (*Unione dei Comuni*) and at the same time have less than 1,000 inhabitants, since these municipalities are subject to the fiscal rule irrespective of their population (931 municipalities). Second, we drop all municipalities located in one of the five autonomous regions (*Friuli-Venezia Giulia*, *Sardegna*, *Sicilia*, *Trentino-Alto Adige*, and *Valle d'Aosta*), since they are granted a special status by the Italian constitution allowing them to set their own rules (1,392 municipalities). Third, we drop all municipalities that merged in the sample period (79 municipalities). Altogether, our final sample consists of 6,638 municipalities, which represent about 82% of all Italian municipalities.

Our empirical strategy relies on comparing municipalities that are above and below the 1,000 population threshold (see next section). Our estimating sample will always exclude municipalities with more than 2,500 inhabitants, as they are too close to the next policy-relevant threshold at 3,000 inhabitants.[19] Figure 2 shows a map

[19]Note that our estimates actually leverage on variation from an even smaller sample of municipalities located around the 1,000 population threshold, and selected using the optimal bandwidth by Grembi et al. (2016). See section 4 for more details.

Figure 2: Map of Italian municipalities



of municipalities in our sample, distinguishing between municipalities below 1,000 inhabitants (blue) and those between 1,000 and 2,500 (red). Due to our population restrictions, our sample is composed mainly by municipalities located on the Alps or on the Apennines, the two main Italian mountain ranges. The map suggests also that blue and red municipalities are distributed rather uniformly along these two mountain ranges. Table B.3 in the Appendix shows summary statistics of all variables for the whole sample as well as for municipalities below and above 2,500 inhabitants.

3.2 Municipal Tax Rates

We collect annual information on the local income tax from the Italian Ministry of Finance. This includes marginal tax rates for all income brackets and exemption

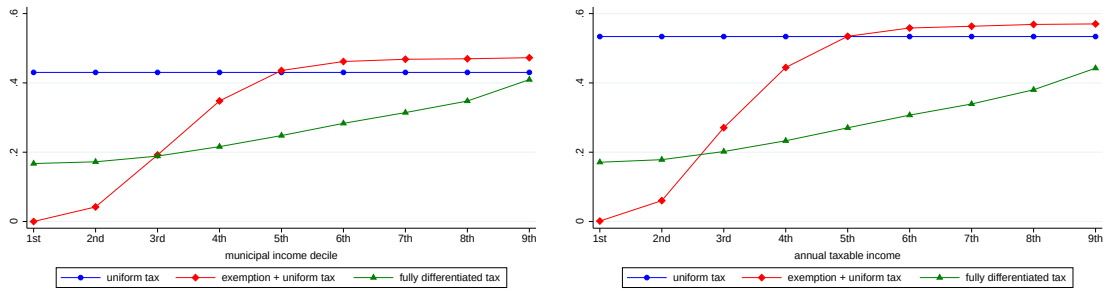
levels at the municipal level. We also obtain the (approximate) municipal-level income tax base distribution from the Italian Fiscal Agency (*Agenzia delle Entrate*). In particular, for every municipality we observe both the number of taxpayers and the tax base in a number of income brackets.[20] We make the simplifying assumption that taxpayers are uniformly distributed within the brackets in order to construct income deciles on the municipality level. This allows us to know the tax rates that apply to each income decile of the respective municipality, e.g., the statutory tax rate that a household earning as much as the 90th percentile of the municipal income distribution has to pay. Using these tax rates as outcome variables allows us to gauge which part of the distribution is affected by changes in tax policy.[21] As discussed above, one can distinguish between three different tax regimes: a uniform tax, an exemption level and a uniform tax, or a fully differentiated tax schedule. We plot the sample mean of the average tax rates for municipalities in the three tax regimes before (Figure 3a) and after the fiscal rule reform (Figure 3b). As the blue line indicates, the average uniform tax rate is about 0.48%. For both municipalities with an exemption threshold and those with a fully differentiated tax schedule, the mean tax rate monotonically increases along the municipal income distribution.

To test the distributional effect of the fiscal rule we employ several outcome measures. First, we directly look at the tax rates at the nine income deciles of the municipal income distribution. Second, we study the level of the exemption threshold. Third, we use a binary indicator of whether a given municipality has a progressive tax system or not. Fourth, to obtain a comprehensive measure of progressivity, we use two indicators from the literature: the average and marginal rate progression (Peter et al., 2010). We construct these variables by running the

[20] See Figure B.3 for a histogram of the number of taxpayers in each bracket.

[21] We also use the average tax rate paid on annual incomes from 5,000 to 75,000€as alternative outcomes.

Figure 3: Municipal income tax: average tax rates



(a) before reform (b) after reform

Notes: The figure presents the mean average tax rates in the deciles of the municipal income distribution for three groups of municipalities: those with a uniform tax (blue line), those that have an exemption level and a uniform tax rate that applies to income exceeding the exemption level (red line), and those with a fully differentiated tax schedule (green line). The sample includes only municipalities with less than 2,500 residents. Panel (a) presents data for the period 2007-2012; panel (b) for the period 2013-2015.

following regression for each municipality-year pair (i, t) separately:

$$TaxRate_{yit} = \beta_0 + \beta_1 log(y) + \epsilon_{ity} \;\; \forall\, y \in \{1000, 2000, ..., 99000, 100000\} \qquad (1)$$

where $TaxRate_{yit}$ is the average (marginal) tax rate at income y in municipality i in year t, and β_1 is an estimate of the average (marginal) rate progression. We normalize the progressivity measures with their sample standard deviations to ease interpretation. The resulting coefficient is by construction negative for regressive, zero for flat, and positive for progressive tax schedules.

Furthermore, we calculate income tax revenues by income brackets. Specifically, we take the product of the total tax base and the average tax rate for each individual bracket. To derive the average tax revenues per taxpayer, we then divide by the number of tax payers in the specific bracket.

3.3 Municipal Budget

We complement the data above with municipal budget data from the Italian Ministry of the Interior (*Certificati Consuntivi*). These include detailed accounts of revenues and expenditures. Budgets report figures according to two accounting criteria: cash

and accrual bases. We use the latter, since policy changes are reflected in accrual accounts more quickly. We convert all monetary values into 2015 euro and per capita figures using the CPI series and annual population counts from the Italian National Institute of Statistics (ISTAT). Furthermore, we winsorize all budget variables at the first and 99th percentile to account for outliers.

Expenditure figures are split between capital and current spending, and are further disaggregated in broad categories (e.g. education, social). Revenues are available by their source (e.g. local income tax surcharge, transfers from central government). We rely on the officially defined deficit (*disavanzo*) in the accounts, which is the difference between revenues and expenditures plus the difference between revenue carry-overs and expenditure carry-overs from preceding years. Carry-overs are the difference between the figures calculated according to the cash and accrual bases (e.g. credit vis-a-vis taxpayers, or debt vis-a-vis suppliers). Therefore, the official deficit accounts for obligations originated in previous years, which still weight on the public finances. According to this official measure of deficits, 54% of all municipalities in our sample are in surplus.

3.4 Politician and Election Data

We collect information on local elections from the historical electoral archive, and information on politicians from the registry of local public office holders. Both datasets are maintained by the Italian Ministry of the Interior. The first database includes the names of all the candidates and of the lists supporting them, and reports information on election results. This allows us to construct both a rerun and reelection dummy for incumbents. The former is equal to one if the incumbent is not term-limited and runs again, and equals zero in case the incumbent is not term-limited but does not run again. The latter variable equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected.

The second database has demographic information on all individuals who ever held municipal public office, that is mayors, members of the executive committee, and councilors. Usually runners-up are elected to the city council, so that we also have information on them unless they give up their seat immediately after the election. Therefore, we have information on birthplace, party, (potential) term limit, gender, (former) occupation and education level for both the mayor and the runner-up. Using these variables, we construct our two measures of politician's quality, a dummy for having a college degree and a dummy for being employed in a high-skill profession before becoming a politician.[22] We merge the two databases by matching on name, surname, year and municipality code in order to obtain background information on mayors and runners-up. The matching is successful in 70% of the cases.[23]

3.5 Municipal Characteristics

We collect several further (time-invariant) variables on municipal characteristics from the 2011 census: the share of female, college-educated, and inhabitants older than 60 years as well as geographic variables such as altitude, geographic area and a dummy for coastal location. The annual population numbers are retrieved from ISTAT. We calculate the yearly share of income held by the top income earners (more than 55,000€) from the tax base data by the Italian Fiscal Agency.

[22]For the latter, we rely on the ISTAT classification of occupations (ISTAT, 2013). We classify occupations in category 1 (legislators, entrepreneurs and managers) and 2 (intellectual, scientific and highly specialized occupations) as high-skill occupations. Among mayors from high-skill occupations, 76% hold a college degree, whereas among those from other occupations only 27% have a college degree.

[23]Non-matches are likely due to second-placed candidates not joining the city council. Table B.4 of the Appendix compares the covariates of matched and non-matched mayors.

4 Empirical Strategy

4.1 Difference-in-Discontinuity Design

Our empirical strategy relies on a natural experiment resulting from the extension of the fiscal rule in the year 2013 to municipalities that were previously exempted. In our sample period of 2007-2015, the Domestic Stability Pact applied to municipalities with 5,000 or more inhabitants until 2013, and to municipalities with 1,000 or more inhabitants from 2013 to 2015. One possible strategy could be a comparison of municipalities around the 1,000 threshold using only data for the period 2013-2015 in a classic regression discontinuity design. However, other policies change discontinuously at the 1,000 cutoff (see Table B.2 for details) and thus the standard continuity assumption is violated.

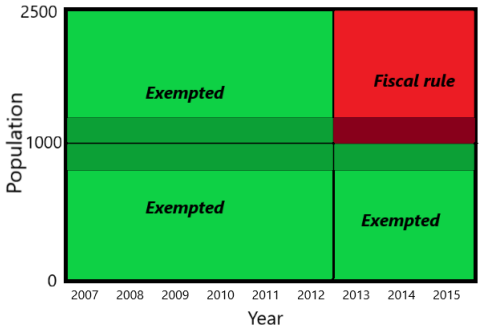
In order to isolate the effects of the fiscal rule, we employ a difference-in-discontinuity design (Asatryan et al., 2017; Grembi et al., 2016). The intuition behind this empirical strategy is that a confounding policy jump can be netted out if the policy is *time-constant*. This assumption holds in our setup, as all of the confounding policy discontinuities are constant over the whole sample period. This implies that one can estimate the confounding effect at the 1,000 threshold in the years before 2013 and subtract it from the compounded fiscal rule and confounding effect estimated at the 1,000 threshold between 2013 and 2015. In other words, this strategy amounts to a difference-in-differences design evaluated at the 1,000 threshold.[24] Figure 4 provides a visual representation of our empirical strategy.

More formally, let Y_{it} be an outcome variable in municipality i at time t (e.g. tax progressivity) and $\tilde{p}_{it} = p_{it} - 1,000$ its normalized population in the previous year. According to the law, the treatment status of a municipality is based on the population of the preceding year.[25] We therefore use $\tilde{p}_{it-1}$ as our forcing variable,

[24] We do not evaluate the change of the 5,000 inhabitants threshold, since there is a *simultaneous* policy change of gender quotas in local elections in 2013 (see Table B.2).

[25] Consistent with the institutional framework, we are using the yearly population numbers from ISTAT.

Figure 4: Visualization of the empirical strategy



Notes: This diagram illustrates our empirical strategy in the two-dimensional space year-population within our sample restrictions. Municipalities in the green quadrants are exempted from the fiscal rule (controls), and those in the red quadrant are subject to the fiscal rule (treated). The shaded area exemplifies the bandwidth used for the estimation.

where at the cutoff the treatment status jumps sharply from 0 to 1. The difference-in-discontinuity estimator can be written as follows:

$$\hat{\tau}_{diff-in-disc} = \left(\lim_{p \to 0^+} E[Y_{it} | \tilde{p}_{it-1} = p, t \geq 2013] - \lim_{p \to 0^-} E[Y_{it} | \tilde{p}_{it-1} = p, t \geq 2013] \right) - \left(\lim_{p \to 0^+} E[Y_{it} | \tilde{p}_{it-1} = p, t < 2013] - \lim_{p \to 0^-} E[Y_{it} | \tilde{p}_{it-1} = p, t < 2013] \right)$$

where the first row describes the jump in the outcome variable at the threshold between 2013 and 2015 (i.e. the compounded fiscal rule *and* confounding effect), and the second row subtracts the jump in the outcome variable before the reform (i.e. *only* the confounding effect).

We implement this estimator using a local linear regressions as in Grembi et al. (2016) and estimate the following equation:[26]

$$Y_{it} = \beta_0 + \beta_1 \tilde{p}_{it-1} + T_{it}(\beta_2 + \beta_3 \tilde{p}_{it-1}) + Reform_t[(\beta_4 + \beta_5 \tilde{p}_{it-1}) + T_{it}(\beta_6 + \beta_7 \tilde{p}_{it-1})] + \epsilon_{it} \quad \forall\ (i,t) \ \ s.t.\ |\tilde{p}_{it-1}| < h^\star \tag{2}$$

where T_{it} takes the value of one if municipality i is subject to the fiscal rule in year t, $Reform_t$ is a dummy equaling one from 2013 to 2015, and $h^\star$ is the optimal bandwidth determined by the algorithm suggested by Grembi et al. (2016).[27] Since

[26] We also estimate global polynomial regressions with varying polynomial degrees.

[27] We conduct a standard RD before and after the reform using the STATA command *rdrobust*

the results of local linear regressions may be sensitive to the choice of the bandwidth, we also estimate results obtained with different bandwidths. Standard errors are clustered at the municipal level to account for arbitrary serial correlation in the error term. The local average treatment effect (LATE) of the fiscal rule is then identified by the coefficient β_6.

The difference-in-discontinuity estimator identifies the effect of interest if the following identifying assumptions are met. First, as discussed above, other confounding variables can change discontinuously at the threshold, but we must assume that the change is *time-constant*. We test this assumption of local parallel trends by means of placebo reforms. That is, we pretend that the reform was implemented in some earlier year instead of 2013, and then re-do the baseline analysis on the pre-reform sample. Second, in contrast to a classical regression discontinuity design, where there cannot be any manipulation of the running variables, the difference-in-discontinuity estimator allows for time-constant sorting unrelated to the reform. If municipalities were to react to the reform by manipulating their population numbers in order to avoid the fiscal rule, we would have selection bias in the treatment and control assignment. We test this assumption with McCrary density tests both before and after the reform, as well as with a density test of the change in density because of the reform. One important caveat is that, even when our identifying assumptions hold, we are estimating the *local* average treatment effect of the fiscal rule. That means our results only apply to small municipalities and are not representative for all Italian local governments.

4.2 Heterogeneous Effects

To examine the mechanisms driving our results, we also test for heterogeneous effects. We put special focus on the mayor's quality measured by having a college education or coming from a high-skill occupation. Following the literature on het-

(see Calonico et al., 2014) and then take the average of the two optimal bandwidths.

erogeneous effects in an RD setup (see Becker et al., 2013), we interact every term in equation 2 with a dummy for being a high-skilled mayor D_{it}:

$$\begin{aligned} Y_{it} =& \beta_0 + \beta_1 \tilde{p}_{it-1} + T_{it}(\beta_2 + \beta_3 \tilde{p}_{it-1}) + Reform_t[\beta_4 + \beta_5 \tilde{p}_{it-1} + T_{it}(\beta_6 + \beta_7 \tilde{p}_{it-1})] + \\ & D_{it}[\beta_0^{int} + \beta_1^{int} \tilde{p}_{it-1} + T_{it}(\beta_2^{int} + \beta_3^{int} \tilde{p}_{it-1}) + Reform_t[\beta_4^{int} + \beta_5^{int} \tilde{p}_{it-1} + T_{it}(\beta_6^{int} + \\ & \beta_7^{int} \tilde{p}_{it-1})]] + \gamma_i + X_{it} + \epsilon_{it} \quad \forall\ (i,t) \ \ s.t.\ |\tilde{p}_{it-1}| < h^{\star} \end{aligned} \tag{3}$$

The heterogeneous treatment effect is then measured by β_6^{int}. X_{it} includes dummies indicating whether the mayor is female, has a college degree, is backed by a left-wing, right-wing or centrist party, is term-limited, her age and her win margin in the last election, the number of years to the next election, as well as the top income share and pre-reform deficits of the municipality.[28]

We also include municipality fixed effects γ_i to absorb any time-invariant heterogeneity. Nevertheless, we cannot fully exclude the possibility of unobserved time-varying confounding variables determining both the mayor's quality and our outcome of interest. For example, if municipalities whose population has a higher preference for redistribution tend to elect more skilled mayors, then we would erroneously attribute the estimated increase in progressivity to mayoral quality rather than to the population's preferences.

For this reason, we turn to a more exogenous source of variation in the mayor's quality. We exploit close mixed elections, i.e. races in which the winning candidate and runner-up have a different educational level.[29] First, we restrict our sample to municipalities whose mayors have been elected in a mixed election. Next, we subtract the vote share of the non-college candidate from that of the college-educated candidate to get the vote margin vm_{it}, which acts as our running variable. For

[28] In some specifications, we add additional interaction terms from X_{it} other than D_{it} to test their relative importance in a "horse race".

[29] This strategy has been extensively used in the literature on the effect of female mayors (see, for example, Baskaran and Hessami, 2018). We focus on mixed races between mayors of different education levels since the number of races between mayors from low- and high-skill occupations is considerably smaller.

positive vm_{it}, the college-educated candidate wins the election, whereas if vm_{it} is negative, the non-college candidate wins. Our identifying variation then stems from close elections, comparing municipalities, in which the college-educated candidate barely won, to those in which she barely lost. More formally, let D_{it} be an indicator that takes the value one if the mayor of municipality i in year t is college-educated. We then estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 vm_{it} + D_{it}(\beta_2 + \beta_3 vm_{it}) + X_{it} + \epsilon_{it} \quad \forall\ (i,t)\ \ s.t.\ |vm_{it}| < h \tag{4}$$

where X_{it} includes all control variables described above, as well as additional characteristics of the second-placed candidate (gender, age and party), and h is the chosen bandwidth. The effect of having a college-educated mayor is then identified by β_2. The most important identifying assumption is that the education level is the *only* characteristic that changes at the threshold. We test this by using other observable characteristics from X_{it} as outcome variables to see whether they also jump at the threshold.

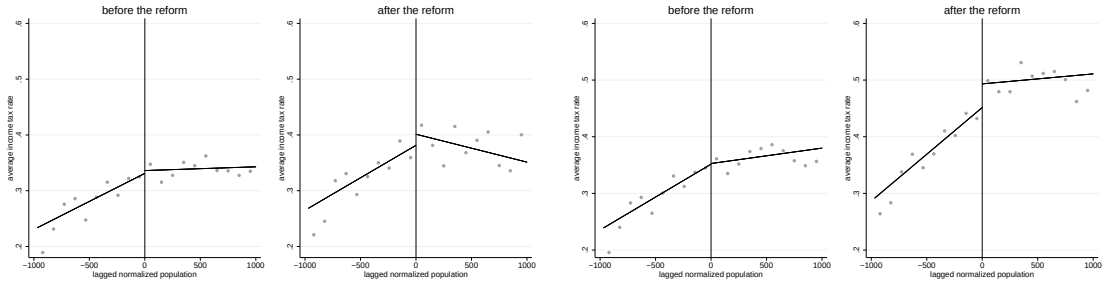
In a last step, we combine equations 3 and 4 to identify our heterogeneous effects model using only the variation in the quality of the mayor induced by close elections. That is, we interact every term in equation 3 with the vote margin between college-educated and non-college-educated candidates and estimate it on the sample of mixed elections. By comparing college-educated and non-college mayors that barely won in a mixed election, we effectively control for unobserved confounders that could possibly drive both the mayor's educational level and tax policy.

5 Results

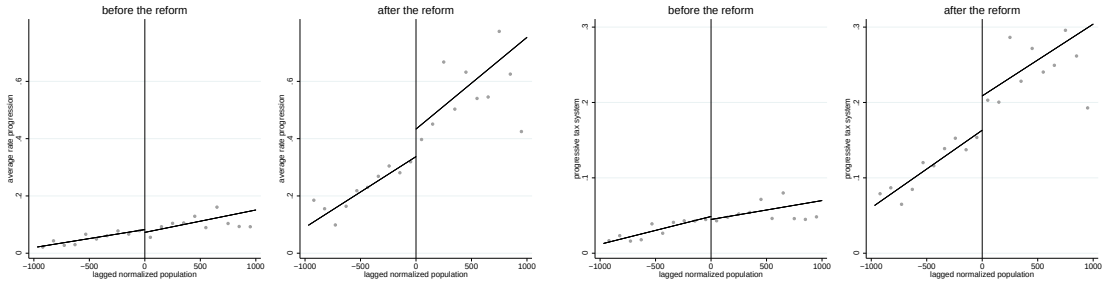
5.1 Baseline Results

We start by presenting some graphical evidence of our results. Figure 5 shows standard RD graphs estimated separately on the pre-reform (2007-12, on the left)

Figure 5: Regression discontinuity plots: tax progressivity before and after the reform



(a) income tax rate at the 1st decile (b) income tax rate at the 9th decile

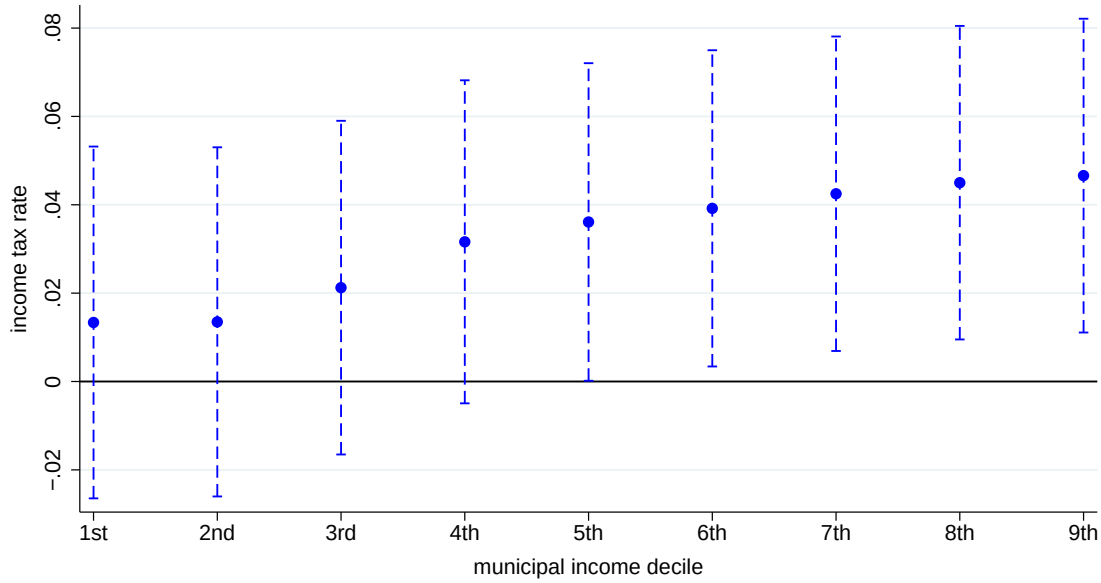


(c) average rate progression (d) progressive tax

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each graph is a regression discontinuity plot for pre-reform years (2007-12, on the left) and post-reform years (2013-15, on the right). The outcome variable is reported underneath each graph. The running variable is lagged normalized population. Plots are obtained with the STATA command *rdplot* (Calonico et al., 2015) organizing the data in 10 bins on each side of the threshold. The lines are linear fits estimated separately on each side of the threshold.

and post-reform (2013-15, on the right) samples for four outcome variables in subfigures: a) income tax rate at the first decile, b) income tax rate at the ninth decile, c) average rate progression, and d) a dummy for a progressive tax. Each graph plots local means of the outcome variable in ten normalized population bins on each side of the threshold, and a linear fit of the data estimated separately on each of them. Before the reform, the figure does not show a visible jump at the threshold for any of the outcome variables. After the reform, we observe a positive jump in the average tax rate at the first decile, and a more sizable one for the tax rate at the ninth decile. This preliminary graphical evidence suggests that the reform induced a disproportionate increase in the tax for higher incomes. The finding is confirmed by the fact that both measures of progressivity display a positive jump at the threshold in the post-reform years, but not in pre-reform years (panels c and d).

Figure 6: Effect of the reform on the income tax rate at different income deciles



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots the local average treatment effects also reported in Table 1 and their 95% confidence bands. The LATEs are from difference-in-discontinuities models estimated with a separate local linear regression for each tax rate and correspond to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The deciles refer to the income distribution in each municipality.

Next, we turn to the estimates obtained from the difference-in-discontinuity estimation (equation 2). Figure 6 plots the local average treatment effect (LATE) of the reform on the average tax rates at all deciles of the municipal income distribution (estimates are shown in Table 1). We find that, first, all point estimates are positive. This is consistent with the interpretation that municipalities raise local income taxes to comply with the fiscal rule. Second, the size of the point estimates is monotonically increasing along the municipal income distribution. Third, the estimated effect on the top tax rate translate to about 13% of the sample mean, and is about 3.5 times as large as the estimated tax rate effect on the lowest earners.

To test whether the estimated effects on high-earners are statistically larger than the effects on low-earners, we re-estimate equation 2 for all nine tax rates jointly, with seemingly unrelated regressions (SUR).[30] We then implement several one-sided Wald tests with a null hypothesis that the effect on higher incomes is not

[30] We use SUR because the tax rates along the income distribution are jointly determined by the municipal government, and thus can not be considered as independent outcome variables. As such, the confidence intervals plotted in Figure 6 are not useful for testing whether effects on different tax rates are significantly different from each other.

Table 1: Effect of the reform on the income tax rate at different income deciles

	(1) 1st decile	(2) 2nd decile	(3) 3rd decile	(4) 4th decile	(5) 5th decile	(6) 6th decile	(7) 7th decile	(8) 8th decile	(9) 9th decile
LATE	0.013	0.013	0.021	0.032*	0.036**	0.039**	0.043**	0.045**	0.047**
	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
> 1st decile	-	0.488	0.216	0.061	0.035	0.022	0.012	0.008	0.007
> 2nd decile	-	-	0.194	0.049	0.027	0.016	0.009	0.006	0.005
> 3rd decile	-	-	-	0.063	0.034	0.018	0.008	0.005	0.004
> 4th decile	-	-	-	-	0.192	0.091	0.034	0.019	0.018
> 5th decile	-	-	-	-	-	0.049	0.005	0.003	0.008
> 6th decile	-	-	-	-	-	-	0.014	0.007	0.015
> 7th decile	-	-	-	-	-	-	-	0.028	0.050
> 8th decile	-	-	-	-	-	-	-	-	0.157
mean	0.331	0.335	0.347	0.358	0.364	0.366	0.367	0.368	0.370
bandwidth	661	661	661	661	661	661	661	661	661
N	17,609	17,609	17,609	17,609	17,609	17,609	17,609	17,609	17,609

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The deciles refer to the income distribution in each municipality. The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at the first to eighth municipal income decile, respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

larger than the effect on lower incomes. We present the p-values of all these tests in Table 1. Overall, we can reject the null hypothesis at the 5% or 10% level for almost all comparisons.

Alternatively, we also use the average tax rates at annual incomes from 5,000€ to 75,000€ as outcomes. The effects are again positive for all tax rates and monotonically increasing in income, but only significant at the 95% level for incomes above the national median income (see Figure C.2). As before, one-sided Wald tests reject the hypothesis that there are no differences between the effects on high- and low-earners (see Table C.1).

As a final test for the effect of introducing the fiscal rule on tax progressivity, we estimate the difference-in-discontinuity design (equation 2) using our four measures of progressivity: the average rate progression, the marginal rate progression, the exemption level (in €), and a dummy equal to one if the overall income tax schedule is progressive. Table 2 shows that the reform induces an increase in the average (marginal) rate progression of 0.14 (0.16) standard deviations, corresponding to 80% (86%) of the sample mean. The reform also increases the probability of adopting a progressive tax system by six percentage points (Table 2, column 2). This large

Table 2: Effect of the reform on progressivity measures

	(1) average rate progression	(2) progressive tax	(3) marginal rate progression	(4) exemption level
LATE	0.140**	0.056**	0.155**	600*
	(0.062)	(0.027)	(0.066)	(316)
mean	0.175	0.087	0.181	892
bandwidth	668	650	668	635
N	17,775	17,319	17,775	16,955

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The average and marginal rate progressions are estimates of the slope of the average and marginal income tax schedules. Progressive tax is a dummy for whether the municipality has a tax rate which is not uniform. Exemption level is the amount of income (in €) exempted from the income tax. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

increase in progressivity is partly driven by the effect on the exemption level, which increases by 600€, that is approximately by 67% of the sample mean (Table 2, column 4).

Exploiting information on the municipal income distribution, we also estimate the effects on tax revenues levied from taxpayers assigned to different brackets both in aggregate and in per taxpayer terms (see Section 3). In line with the progressive nature of the income tax rate adjustment, our findings suggest that individuals from upper tax brackets contribute more to the extra revenues generated by the reform. The average tax increase for a taxpayer in the top income bracket (above 120,000€) amounts to 73€ (47% relative to the sample mean) which is about an order of magnitude larger than the effect on a taxpayer in the 15,000€ to 26,000€ bracket. In general, the additional tax revenues per taxpayer induced by the reform are strictly increasing in taxable income (see Table 3)

However, since only few taxpayers have large taxable incomes (on average 15 individuals have taxable incomes above 55,000€), more than half of the extra revenue is levied from tax payers with taxable income between 15,000€ and 55,000€. Our findings also suggest that individuals with taxable income below 10,000€ (on average 38% of the total taxpayers) are the only ones to almost entirely escape the tax rate increase. This result is consistent with our previous findings of an increase in exemption levels.

Table 3: Effect of the reform on income tax revenues by bracket

	(1) <10k€	(2) 10k€-15k€	(3) 15k€-28k€	(4) 28k€-55k€	(5) 55k€-75k€	(6) 75k€-120k€	(7) >120k€
tax revenues per taxpayer							
LATE	0.78	4.92**	7.80**	15.88**	22.49*	52.88**	73.05**
	(1.02)	(2.36)	(3.69)	(6.25)	(13.53)	(21.81)	(30.60)
mean	18.14	47.11	76.11	129.62	177.36	195.38	154.85
mean # of taxpayer	308	136	241	109	8	5	2
bandwidth	664	654	665	660	618	479	726
N	17,684	17,444	17,709	17,587	16,544	13,163	19,180
total tax revenues							
LATE	6.24	596.17*	1561.08*	1938.44**	406.45***	486.49***	627.70***
	(307.52)	(317.41)	(919.09)	(760.57)	(149.98)	(182.34)	(239.48)
mean	4,857.04	5,796.83	16,824.49	12,589.17	1,747.59	1,566.40	1,020.69
bandwidth	700	657	628	647	653	608	688
N	18,550	17,508	16,776	17,247	17,408	16,287	18,279

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable. The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The outcome variables are per capita (upper panel) and total (bottom panel) tax revenues in 2015 Euros generated by tax payers with taxable income included in the bracket reported on top of each column. The table reports also the sample mean of the outcome variable, the average number of taxpayers in each bracket, the used bandwidth and the number of observations. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Furthermore, we also test the effects of the reform on the tax base, and we do not find evidence that taxable income changed (see Table C.2). Taken together with the evidence of a positive effect on revenues, this finding suggest that mayors were able to raise additional income tax revenues without substantially hurting their tax base. Finally, we also compute the upper pareto bounds proposed by Bierbrauer et al. (2020) to test whether the pre-reform tax systems were on the left or on the right of the peak of the Laffer curve. We find that, even assuming a large elasticity of taxable income, more than 95% of the municipalities were on the left of the peak, making thus possible for mayors to raise revenues by increasing tax rates (see Appendix A.3 for details.).

5.2 Sensitivity Checks

In this section, we discuss the validity of the two major identifying assumptions as described in Section 4. We also perform a number of additional robustness tests with respect to the choice of bandwidth size and polynomial degrees, and a permutation test using placebo thresholds.

First, the local parallel trends assumption states that any difference at the

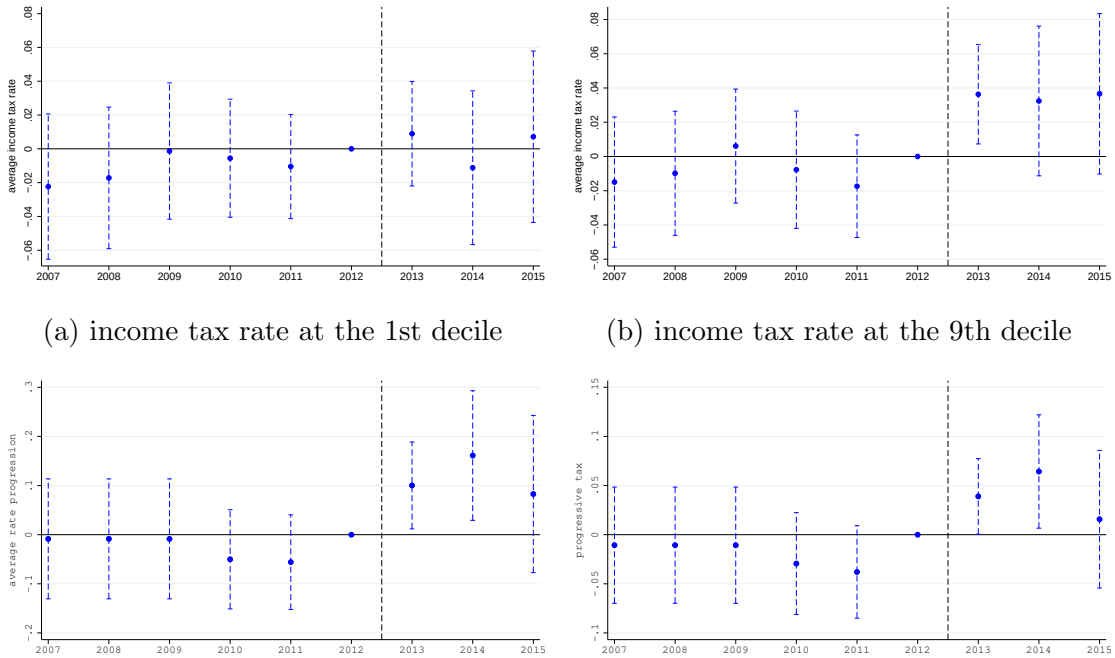
threshold other than the fiscal rule has to be time-constant. To formally test whether the local common trends assumption holds, we use a dynamic version of equation 2, where we replace the $Reform_t$ dummy with year dummies. Normalizing our effects to the pre-reform year of 2012, this allows us to track the local trends before the reform and the dynamic effects after the reform. As Figure 7 shows, there is no significant pre-treatment trend in the bottom tax rate (Panel a), top tax rate (Panel b), the average rate progression (Panel c), or the probability of a progressive tax system (Panel d).[31] After the reform, there is an immediate significant increase in all variables, but the bottom tax rate. As a further robustness check, we conduct placebo reforms in every pre-reform year of our sample. Specifically, we restrict our sample to the pre-reform period and re-estimate equation 2 with the $Reform_t$ dummy taking the value 1 from year t onward with $t \in \{2008, 2009, 2010, 2011, 2012\}$. If any confounding effect was not time-constant, one would expect to pick up a significant effect by at least one of these placebo reforms. Figure D.2 plots the results of the five placebo estimations as well as that of the baseline results. The results show zero effects for every placebo reform and every tax rate. As expected, the placebo estimates exhibit a constant rather than a monotonically increasing relationship between the estimated tax rate effect and the level of income. Next, we test the continuity assumption by using pre-determined variables as outcomes. Table D.1 shows that none of the 16 variables are significantly influenced by the reform at conventional levels.

Our second identifying assumption is that there is no manipulation of the population numbers in reaction to the reform. In order to test this claim, we present standard McCrary graphs (McCrary, 2008) displaying the density of municipalities around the threshold before and after the reform, as well as a "dynamic" McCrary graph, which shows the difference between the density around the threshold before and after the reform (see Asatryan et al., 2017; Grembi et al., 2016).[32] We do

[31] This also holds for our other outcomes variables (see Figure D.1).

[32] For the "dynamic" McCrary, we first divide normalized log population size in bins of width

Figure 7: Dynamic effects of the reform



(a) income tax rate at the 1st decile

(b) income tax rate at the 9th decile

(c) average rate progression

(d) progressive tax

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each panel plots estimates from the dynamic model on a different outcome variable, reported underneath each plot. The dynamic model is an extension of the baseline difference-in-discontinuities model that includes year dummies instead of the reform dummy. The bandwidth is selected following Grembi et al. (2016). Each dot is the estimate of the deviation of the outcome variable in the year reported on the horizontal axis relative to the pre-reform year 2012. Dotted bars are 95% confidence bands.

not find evidence of a significant jump in the density of observations at the 1,000 population threshold either before (Figure D.3a) or after (Figure D.3b) the reform. This evidence of *no* manipulation of population numbers in response to the reform is confirmed by the results of the "dynamic" McCrary test presented in Figure D.4.

Our results are also robust to the selection of different bandwidths. Figure D.5 plots the effect on low- and high-earners for bandwidths ranging from 400 to 1,000. As expected, the standard errors somewhat decrease with larger bandwidth, but the point estimates remain stable. Furthermore, Figure D.6 and Table D.2 show that global polynomial regressions yield very similar results to local linear regressions. Additionally, we conduct permutation tests by re-estimating equation 2 at placebo thresholds and show that our baseline effect on high incomes is larger than any of

0.01. Then we calculate the change in the total number of observations within each bin from the pre- to the post-reform period. Finally, we fit local polynomial plots using a quadratic degree and a triangular kernel.

the placebo estimates (see Figure D.7). Finally, we show in Appendix A.2 that a difference-in-difference approach yields estimates very similar to our main estimates.

6 Mechanisms and Electoral Implications

We have thus far established that local governments increase tax progressivity in response to exogenous consolidation requirements induced by the fiscal rule. This section first explores heterogeneity in the treatment effects estimated in the previous section. In particular, we study whether the type of tax adjustment is different depending on mayor characteristics, with a special emphasis on her skill level. We then study whether introducing the fiscal rule affects reelection chances of mayors.

6.1 The Role of High-Skilled Mayors

Following the literature on competence of politicians and its effects on policy outcomes (see Section 1), we proxy skill with the politician's education level, specifically if she holds a college degree.[33] As a robustness check, we also use a dummy for being employed in a high-skill profession. About 45% of the mayors in our sample have a college degree and 38% work in a high-skill occupation (see Table B.3).

We first test whether highly-educated mayors are driving our progressivity results as measured by both of our progression measures, the exemption level, and a dummy for progressive rather than flat tax systems. Table 4 presents estimates of equation 3, where the interaction variable D_{it} is a dummy equal to one if the mayor holds a college degree. It turns out that college-educated mayors drive almost all of the increase in progressivity estimated in our baseline model. Columns 1 shows that mayors with a college education increase the average rate progression by 0.30 standard deviations in response to the fiscal rule, whereas non-college-educated mayors

[33] In this measurement choice we follow the literature that most often approximates the skill of politicians by their level of education (see, for example, Besley and Reynal-Querol, 2011; Gagliarducci and Nannicini, 2013). Other papers measure the skill of politicians by utilizing data on politicians' experience, pre-office market income, quality (rather than only level) of education or the skill level of their occupation (Bertrand et al., 2020; Besley et al., 2017; Fisman et al., 2015).

Table 4: Differential effect of the reform by mayor's skill

	(1) average rate progression	(2) average rate progression	(3) average rate progression	(4) average rate progression	(5) average rate progression	(6) average rate progression	(7) average rate progression	(8) average rate progression
LATE	0.012	0.013	-0.004	-0.016	0.010	0.015	-0.009	-0.010
	(0.082)	(0.082)	(0.081)	(0.167)	(0.076)	(0.077)	(0.076)	(0.162)
LATE x college degree	0.298**	0.293**	0.231**	0.229**				
	(0.120)	(0.119)	(0.106)	(0.108)				
LATE x high-skill job					0.359***	0.339**	0.294**	0.281**
					(0.133)	(0.133)	(0.120)	(0.119)
LATE x female mayor				0.069				0.076
				(0.163)				(0.168)
LATE x left-wing mayor				0.025				0.020
				(0.154)				(0.155)
LATE x right-wing mayor				-0.360				-0.344
				(0.297)				(0.300)
LATE x centrist mayor				-0.489				-0.605
				(0.342)				(0.388)
LATE x low win margin				0.097				0.088
				(0.114)				(0.117)
LATE x term limit				-0.041				-0.060
				(0.100)				(0.101)
LATE x high pre-reform deficit				0.133				0.129
				(0.132)				(0.132)
LATE x low top income share				-0.177				-0.174
				(0.132)				(0.132)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.176	0.178	0.178	0.178	0.173	0.177	0.177	0.177
bandwidth	668	668	668	668	668	668	668	668
N	17,378	17,092	17,092	17,092	17,292	16,741	16,741	16,741

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

do not change the progressivity of the income tax at all. This result holds when including municipality fixed effects and several other interactions with potential confounders, such as gender, a proxy for electoral competition, political orientation, binding term limits, pre-reform fiscal position, and income structure (see columns 2 to 4 of Table 4). Furthermore, results look very similar when using the skill level of the mayor's occupation as an alternative measure (see columns 5 to 8 of Table 4). Tables C.3, C.4 and C.5 show that this heterogeneous effect also holds for the introduction of progressive tax systems, exemption levels and the marginal rate progression. These results do not mean that low-skilled mayors did not raise local income taxes in response to the reform, but rather that they increased tax rates uniformly (see Figure C.3).

The heterogenous effects estimated so far in this section using equation 3 do not have a causal interpretation because mayors' education is not assigned at random to different municipalities. As such, unobserved factors at the mayor or municipal-level might induce omitted variable bias and thus drive the estimated heterogeneity. To tackle this issue, we focus on mixed elections, in which the winner and runner-up

Table 5: Differential effect of the reform by mayor's skill: mixed election RD

	(1) average rate progression	(2) average rate progression	(3) average rate progression	(4) average rate progression	(5) average rate progression	(6) average rate progression
LATE	-0.025	-0.035	-0.223	-0.391		
	(0.101)	(0.100)	(0.332)	(0.302)		
college degree	-0.045	-0.048	-0.002	0.167	-0.019	0.145
	(0.030)	(0.032)	(0.094)	(0.264)	(0.042)	(0.100)
LATE x college degree	0.296**	0.310**	1.033**	0.954**		
	(0.140)	(0.146)	(0.466)	(0.400)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	0.179	0.179	0.168	0.168	0.081	0.081
population bandwidth	668	668	668	668	668	668
close election bandwidth			0.20	0.20	0.20	0.20
N	13,384	12,355	2,621	2,621	1,861	1,861

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi et al. (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 4. Details on all covariates are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

have different education levels. Using this sample, we estimate the heterogeneous effect at the election threshold by interacting all variables with the vote margin between the two candidates. In other words, we combine equations 3 and 4.

This empirical strategy accounts for any municipal level unobserved differences between municipalities with or without a college-educated mayor, such as unobserved preference for redistribution or differences in the income distribution. However, it does not account for mayoral characteristics correlated with education. We thus start by testing whether any characteristic besides the education level of the mayor changes discontinuously at the election threshold. Table D.3 in the Appendix shows that out of 16 variables only the mayor's gender varies significantly between college-educated and non-college mayors. Educated mayors are more likely to be female. Since there was no effect of gender in Table 4 and we control for gender in all of our previous specifications, we do not regard this imbalance as a serious threat to our empirical strategy. We include all these mayoral characteristics as control variables in the regression models that combine our baseline difference-in-discontinuity with close elections. Furthermore, Figure D.8 shows that there is no discontinuity in the density of the margin of victory.

Estimates from these models are shown in Table 5 for the average rate progression. Columns 1 and 2 show that the heterogeneous effect estimated on the sample of municipalities, in which the mayor was elected in a mixed election, is very similar to the estimates obtained on the full sample in Table 4. Next, we present the results of equation 3 interacted with the vote margin between the college-educated and non-college-educated candidate in columns 3 and 4. The result confirms our previous findings. The reform-induced increase in progressivity is driven entirely by municipalities ruled by college-educated mayors.This result also holds when using other measures of progressivity (see Tables C.6, C.7 and C.8) or varying the bandwidth of the close election RD (see Table D.9). We also test whether the interaction effect is driven by any specific job category by dropping one job category at the time. As Figure D.11 shows, the effect does not change notably for any job category.

The findings in this section have established that college-educated mayors react to the introduction of the fiscal rule by increasing income taxes progressively, while other mayors increase taxes uniformly. We can rule out that college-educated mayors favor more tax progressivity in general. Using a simple regression discontinuity design based on close elections (equation 4) and restricting our attention to years before the introduction of the fiscal rule, we do not find any evidence that municipalities ruled by college-educated mayors have more progressive tax systems (see columns 5 and 6 of Table 5).

6.2 Political Costs of Austerity

We now test whether the introduction of the fiscal rule was associated with a political cost for the incumbent mayor. In particular, we estimate the baseline differences-in-discontinuities model (equation 2) with the reelection and rerun dummies as outcome variables (see Section 3 for a detailed description of these variables). Note that the mayor's skill level is a predetermined characteristic with respect to the reform in 2013, since we only consider the first election after the reform.

Table 6: Effects of the reform on mayors' reelection odds

	(1) reelection	(2) reelection	(3) reelection	(4) reelection	(5) re-run	(6) re-run	(7) re-run	(8) re-run
LATE	-0.004	-0.297**	-0.370***		-0.090	-0.085	-0.138	
	(0.059)	(0.142)	(0.132)		(0.060)	(0.111)	(0.111)	
college degree		-0.073	-0.036	-0.025		0.209*	0.180	-0.019
		(0.226)	(0.218)	(0.021)		(0.119)	(0.118)	(0.022)
LATE x college degree		0.472**	0.471**			0.105	0.102	
		(0.235)	(0.230)			(0.190)	(0.193)	
municipality FE		yes	yes			yes	yes	
controls			yes	yes			yes	yes
pre-reform sample				yes				yes
mean	0.832	0.832	0.834	0.833	0.607	0.607	0.607	0.594
bandwidth	1059	1059	1059	1059	1088	1088	1088	1088
N	2,833	2,833	2,745	1,410	4,271	4,271	4,135	2,357

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Columns (1) and (5) report estimates of the local average treatment effect (LATE) in the baseline difference-in-discontinuities model. Columns (2), (3), (6) and (7) report estimates of the LATE for mayors without a college degree (LATE) and for mayors with a college degree (LATE x college degree), estimated using the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. Columns (4) and (8) report estimates of the college effect from a regression of the outcome on a dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform. Bandwidths are selected following Grembi et al. (2016). The reelection outcome variable in columns (1) to (4) equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected. The rerun outcome variable in columns (5) to (8) equals one for incumbents that are not term-limited and choose to run again, and is zero for those who do not and are not term-limited. Control variables are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Based on a standard median-voter model, progressive taxation should be less costly than uniform taxation, since only a minority of rich households are taxed at a higher rate (Bierbrauer et al., 2020). Additionally, in our context of austerity, compensatory arguments behind progressive taxes (Scheve and Stasavage, 2016) would suggest to shift the tax increase away from the poorest households if the fiscal austerity imposed by the national government is perceived as unfair towards the poor.

The near zero point estimate in column 1 of Table 6 suggests that there is no evidence of political costs for the average incumbent. However, this average effect hides interesting heterogeneity. When allowing for heterogeneity in mayoral education in columns 2 and 3, we find that mayors without a college degree experienced a severe drop of 30 to 37 percentage points in reelection probability, while educated mayors do not undergo these costs at all. Both point estimates are significant at conventional levels.[34] The probability of running for office again drops on average, driven by mayors without a college degree, but these effects are not significant at conventional levels (see columns 5 to 7 of Table 6). This is consistent with non-college mayors also self-selecting out of office, but our results seem to be mainly driven by voter selection. Again, results point in the same direction when using the mayor's occupation as an alternative measure of skill (see Table C.9).

[34]The effect is very stable when varying the bandwidths (see Figure D.10).

Taken together, our findings suggest that more skilled politicians have avoided the political cost of austerity by designing a fiscal adjustment based on progressive taxation. Although we can not provide a direct causal link from increased progressivity to higher reelection odds, we can rule out that skilled politicians have higher re-election odds in general. Using a simple regression discontinuity design based on close elections (equation 4) and restricting our attention to years before the introduction of the fiscal rule, we do not find any evidence that college-educated mayors are more likely to be reelected or to run again (see columns 4 and 8 of Table 6). Any alternative explanation for our findings must thus explain why the introduction of the DSP differentially increased the skilled mayors' re-election odds during our sample period. Existing evidence on the lifting of the DSP in 2001, when implementing local tax progressivity was not yet feasible, actually shows that the fiscal rule decreased the mayors' education level in that occasion (Gamalerio, 2019).[35]

6.3 Alternative Channels of Adjustment

As discussed in Section 2, the local income tax is not the only policy instrument that Italian municipalities can use to comply with the fiscal rule. To shed more light on the full adjustment behavior of affected municipalities, we estimate the effects of the fiscal rule on all revenue and spending categories using our baseline model (equation 2) and municipal account data.

In line with our findings on the local income tax rates, revenues from the local income tax increase significantly (see column 1 of Table 7). We do not find significant increases in any of the other revenue categories: property tax, waste tax, other taxes or fees, sales, loans, and other revenues (see columns 2 to 7 of Table 7). We go into more detail regarding property taxation in Appendix A.4, where we

[35] According to the author's interpretation, skilled politicians avoid entering the political arena when their discretion over fiscal policy is constrained. The difference with our results can thus be explained by the additional policy instrument of differentiated local tax rates, which was not available to mayors in 2001. Furthermore, our results seem to be driven mainly by voters' demand rather than purely by the supply of politicians.

discuss the institutional setting and show that property tax rates did not change. We also do not find any significant effects of the reform on capital nor current expenditures (see columns 8 to 10 of Table 7). Placebo tests for both expenditure and revenue categories show that treatment and control municipalities were on parallel trends before the reform (see Table D.4). To test whether the average expenditure effect is masking heterogeneous effects across different categories of expenditures, we estimate the impact of the fiscal rule on each one separately. Looking at various expenditure items rather than just at social transfers only allows us to take into account potential in-kind transfers which have been shown to matter for inequality (Aaberge et al., 2018). Out of the twelve subcategories of municipal expenditures, only tourism spending is reduced significantly with the other point estimates fluctuating around zero (see Figure C.4). Importantly, the two categories perhaps most associated with redistribution, social and education spending, are hardly affected, with the point estimate of social spending even being positive. Still, this null result might hide heterogeneity between high- and low-skilled mayors that could also explain their differential political outcomes. To test this hypothesis, we estimate the heterogeneous treatment effect of high-skilled mayors for all spending categories. Table C.10 shows that there is no significant difference in any of the spending items. We conclude that the redistributive effect of more progressive income taxes is unlikely to be offset by adjustments on the expenditure side of local budgets.

Finally, we investigate the effects of the introduction of the fiscal rule on municipal deficits. As one can see in column 11 of Table 7, we find that the official deficit is reduced by 36€ per capita (significant at the 1% level). Hence, it appears that the fiscal rule was effective in terms of reducing municipal deficits.

Table 7: Effect of the reform on municipal budget accounts

	(1) income tax revenues	(2) property tax revenues	(3) trash tax revenues	(4) non-tax revenues	(5) transfer revenues	(6) loan revenues
LATE	5.10***	-3.89	6.05	-6.22	-82.26	5.44
	(1.89)	(14.51)	(6.70)	(27.70)	(78.20)	(25.47)
mean	36.19	167.93	109.76	405.36	870.54	161.87
bandwidth	682	574	566	495	562	581
N	17,856	15,243	15,055	13,408	14,960	15,430
	(7) other revenues	(8) total expenditures	(9) capital expenditures	(10) current expenditures	(11) deficit	
LATE	-0.33	-98.21	-25.19	-52.02	-35.73***	
	(20.20)	(84.17)	(29.68)	(65.13)	(8.17)	
mean	114.93	1360.23	824.49	513.61	5.87	
bandwidth	616	515	473	563	666	
N	16,255	13,923	12,929	15,111	17,642	

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. All revenue, expenditure, and deficit variables are expressed in per capita terms and 2015 Euros and are winsorized. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusion

This paper provides the first quasi-experimental evidence that governments try to ease the potential distributional implications of austerity by favoring the relatively poor through more progressive income tax policies. Additional evidence suggests that this strategy is used by more competent mayors and is subsequently rewarded in the polls.

These results are consistent with the view that progressive taxation is preferable to uniform taxation for the median voter. We believe that our evidence is particularly relevant for austerity episodes induced by external factors (e.g., resulting from the imposition of budget constraints from a higher layer of government, or being due to inter-regional spillovers in economic crisis), which can be seen as unfair from the perspective of the local population. Our study suggests that governments can tune their fiscal reform packages to mitigate the distributional consequences of austerity, as recommended by the IMF (2014), and that this adjustment strategy allows them to improve their reelection odds.

Our findings are relevant for policy makers in countries subject to fiscal constraints, but whose public opinion is growing critical of austerity policies. Although our evidence from small Italian towns cannot be immediately extended to other settings, our cross-country evidence on the positive relation between austerity and top income tax rates adds to the external validity of our results.

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Online Appendix (not for publication)

A Additional Analysis

A.1 Country-level Analysis

Table A.1: Cyclically adjusted primary balance and tax rates

	(1) tax rate at mean incomes	(2) top tax rate
cyclically adjusted primary balance	0.071	0.615**
(in % of potential GDP)	(0.089)	(0.291)
country FE	yes	yes
year FE	yes	yes
controls	yes	yes
mean	25.533	41.749
N	806	806

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Robust standard errors are clustered at the country level. This table shows estimates from the regression $y_{it} = \gamma_i + \lambda_t + \beta capb_{it} + \delta X_{it} + \epsilon_{it}$, where y_{it} is either the tax rate at mean incomes (column 1) or at top incomes (column 2), $capb_{it}$ is the cyclically adjusted primary balance in percent of potential GDP, X_{it} includes log GDP per capita and log population as control variables, γ_i are country fixed effects and λ_t are year fixed effects. Top tax rates are drawn from Rubolino and Waldenström (2019), tax rates at mean incomes are from Peter et al. (2010), supplemented by tax data from the OECD. The budget data comes from IMF DataMapper. Our sample consists of 40 countries (Argentina, Australia, Austria, Belgium, Canada, Chile, China, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, India, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Luxembourg, Malaysia, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, South Korea, Spain, Sweden, Switzerland, Turkey, the United Kingdom and the United States) over the period 1990-2017.

A.2 Difference-in-Difference Analysis

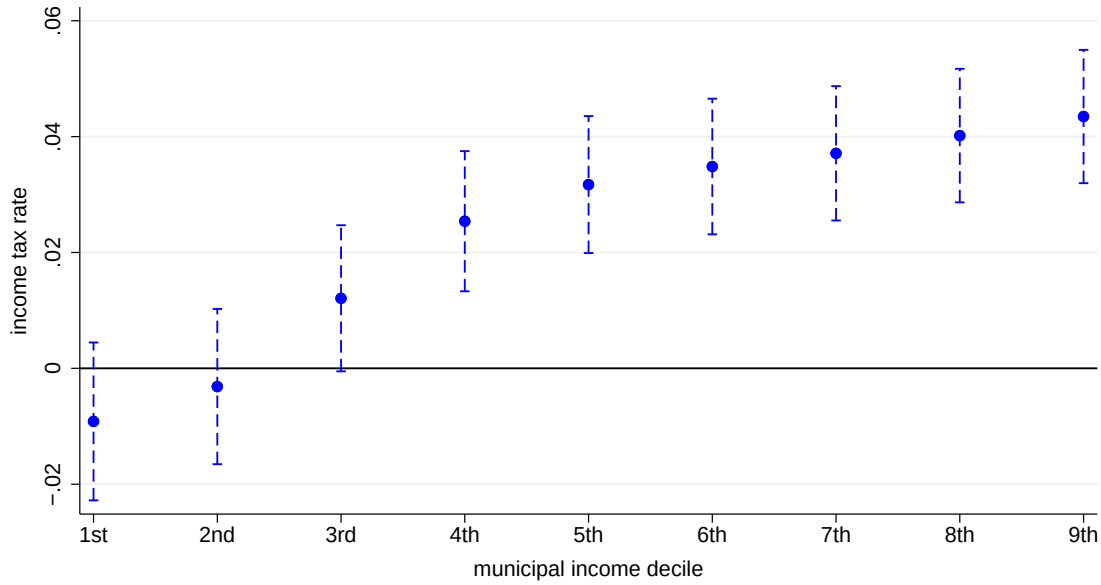
In addition to our main empirical specification, we also run a classical difference-in-difference regression. This allows us to investigate whether our effects can be generalized to broader set of municipalities than just those closely below or above the threshold. To implement this strategy, we define municipalities between 1,000 and 2,000 inhabitants as our treatment group ($T_{it} = 1$) and municipalities with 999 or less inhabitants as our control group ($T_{it} = 0$). The regression equation reads as follows:

$$Y_{it} = \gamma_i + \omega_t + \beta T_{it} Reform_t + \epsilon_{it} \tag{5}$$

where $Reform_t$ is a dummy taking the value 1 for the year 2013, 2014 and 2015. γ_i represent municipality fixed effects, whereas ω_t are year fixed effects. Standard error are clustered at the municipality level. The difference-in-difference estimate is then represented by the coefficient β.

Figure A.1 plots the β coefficients for all nine income deciles. The pattern of the estimates is very similar to the pattern of our main difference-in-discontinuity estimates. The tax increase is monotonically increasing in income. The effect size is also close to our main estimates, but standard errors are significantly smaller. In Table A.2 we also present difference-in-difference estimates for our four progressivity measures. The estimates are all positive and statistically significant at the 1% level. In terms of size, the effects are slightly larger than our main estimates. Taken together, these results suggest that our main estimates are not specific to municipalities at the threshold.

Figure A.1: Difference-in-difference: income tax rate at different income deciles



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots β from equation 5 and its 95% confidence bands. The deciles refer to the income distribution in each municipality.

Table A.2: Difference-in-difference: progressivity measures

	(1) average rate progression	(2) progressive tax	(3) marginal rate progression	(4) exemption level
LATE	0.257***	0.096***	0.259***	975***
	(0.028)	(0.011)	(0.029)	(132)
mean	0.173	0.084	0.178	870
N	24,081	24,081	24,081	24,081

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports β from equation 5 estimated for each outcome variable (reported at the top of each column). The average and marginal rate progressions are estimates of the slope of the average and marginal income tax schedules. Progressive tax is a dummy for whether the municipality has a tax rate which is not uniform. Exemption level is the amount of income (in €) exempted from the income tax. In the bottom panel, the sample mean of the outcome variable and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3 Pareto Bounds by Bierbrauer et al. (2020)

We construct upper pareto bounds as suggested by Bierbrauer et al. (2020), that indicate a level of taxation that is inefficiently high. If tax rates are higher than

these bounds, cutting taxes (even for the rich) would lead to a Pareto improvement because revenues would increase. Intuitively, if the upper Pareto bound is violated, the marginal tax rate is on the right side of the Laffer curve. The upper bound is constructed as follows: $D^{upper}(y_0) = \frac{1-F_y(y_0)}{f_y(y_0)y_0}\frac{1}{\epsilon}$, where $F_y(y_0)$ is the cumulative distribution function of taxable income y evaluated at y_0, $f_y(y_0)$ is the density function of taxable income y evaluated at y_0 and ϵ is the elasticity of taxable income (ETI). The bound is violated if $\frac{T'(y_0)}{1-T'(y_0)} > D^{upper}(y_0)$ holds, where $T'(y_0)$ is the marginal tax rate on income y_0. We construct $T'(y_0)$ by adding up the federal, regional and municipality tax rates. Since we only have information on the distribution of taxable income in brackets, we make the simplifying assumption that income uniformly distributed within brackets. For the elasticity of taxable income, we assume values from 0.25 (as reported in a survey of the literature by Saez et al. (2012) and in a meta-analysis by Neisser (2017)) to very high values like 1.25 as found by Rubolino (2020).

As Table A.3 shows, the bounds are never violated for $\epsilon \leq 1$ and only 3% of our sample municipalities violate them if we assume an ETI of 1.25. We additionally split our sample into municipalities with a college-educated mayor and those without one. The two groups of municipalities do not show any difference with respect to the share of violators. Take together, these findings suggest that before 2013 taxation was not inefficiently high, in the sense that after the introduction of the DSP, it was feasible to increase revenues by raising tax rates. Furthermore, the scope for increasing revenues was not different between municipalities with mayors with different skill levels, thus ruling our the possibility that our heterogeneous results are driven by differences in the income distributions or in the pre-reform tax systems. Additionally, we show that the reform itself did not force municipalities to violate their pareto bounds. As Table A.4 shows, we do not find any effect when we use a dummy for the pareto bounds being violated as our outcomes variable. This holds both for municipalities with a college-educated mayor and for municipalities without one.

Table A.3: Share of municipalities violating the upper pareto bounds

	all municipalities					college-educated mayor					non-college-educated mayor				
ETI	0.25	0.50	0.75	1.00	1.25	0.25	0.50	0.75	1.00	1.25	0.25	0.50	0.75	1.00	1.25
tax on 1st decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 2nd decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 3rd decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 4th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 5th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 6th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 7th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
tax on 8th decile	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.00	0.03
tax on 9th decile	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: The sample is the same as in our main analysis. We additionally restrict the sample to the year 2012 as it was the last year before the reform.

Table A.4: Effect of the reform on violating the pareto bounds

	(1)	(2)	(3)	(4)	(5)
	tax inefficiently high	tax inefficiently high	tax inefficiently high	tax inefficiently high	tax inefficiently high
LATE	-0.009	-0.024	-0.023	0.003	-0.035
	(0.019)	(0.028)	(0.028)	(0.028)	(0.048)
LATE x college degree		0.040	0.038	-0.013	0.006
		(0.035)	(0.033)	(0.033)	(0.033)
LATE x female mayor					0.069
					(0.163)
LATE x left-wing mayor					0.025
					(0.154)
LATE x right-wing mayor					-0.360
					(0.297)
LATE x centrist mayor					-0.489
					(0.342)
LATE x low win margin					0.097
					(0.114)
LATE x term limit					-0.041
					(0.100)
LATE x high pre-reform deficit					0.133
					(0.132)
LATE x low top income share					-0.177
					(0.132)
controls			yes	yes	yes
municipality FE				yes	yes
mean	0.035	0.035	0.035	0.035	0.035
bandwidth	663	663	663	663	663
N	17,433	17,048	17,048	17,048	17,048

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities model and the model extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. We measure mayors' skills using a dummy for college degree, which is equal to one in case the mayor holds one. Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.4 Property Taxation

Property taxation was reformed several times during our sample period 2007-2015 (see Messina and Savegnago (2014) for a detailed review in Italian). The main property tax at the start of our sample period was named ICI and its tax base was based on the cadastral value, the cadastral zone and on the size as well as the type of the dwelling. Municipalities had some flexibility in setting tax rates and they could set a different tax rate for main dwellings (where the taxpayer has his

regular registered residence) and other dwellings. Municipalities also could set a flat reduction on the tax bill, which was applied only on main dwellings. All revenues would accrue to municipalities. The ICI on main dwellings was abolished in 2008, while it remained in operation on other dwellings. In 2012, a new tax named IMU replaced ICI. In the first year IMU was levied on both main and other dwellings, but already in 2013 a discount on main dwellings was introduced. Since 2014 the IMU on main dwellings was abolished, while it remained in operation for other dwellings. Next, a new tax, TASI, was introduced in 2014 in addition to IMU. The tax base for TASI was the same as for ICI and IMU, but TASI was also (partially) levied on renters. Municipalities could set different TASI tax rates for main and other dwellings, as well as a flat reduction for main dwellings. The range of feasible TASI tax rates and reductions was lower than for IMU. Due to these reforms, there was limited scope for mayors to increase revenues by increasing property taxation in 2013 and later years, due to the abolition of IMU on main dwellings and the introduction of the less remunerative TASI. However, we are going to test whether the introduction of the DSP had any effect on property taxation using data on IMU (tax rates and reductions on main dwellings, and tax rates on other dwellings), and on TASI (only the two tax rates, as the data on reductions is not available). Recall that, in our setting the pre-reform period is 2007-12, while the post-reform period is 2013-15. To test for effects of property taxation on main and other dwellings, we restrict our sample to the year between 2012 and 2015 and add up the IMU and TASI tax rates since they share the same tax base.

Table A.5 contains the difference-in-discontinuity estimates on the property tax rates for both main dwellings and other dwellings as well as the flat reduction on the tax bill. We do not find a significant effect on any of the tax instruments. This is consistent with our result from Table 7 that property tax revenues did not change because of the introduction of the fiscal rule. Furthermore, we also estimate our interaction model with respect to the mayor's skill. As Table A.6 shows, high-skilled mayors choose (weakly) higher property tax rates on other dwellings, whereas

Table A.5: Effect of the reform on property tax rates

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount
LATE	0.127	0.103	1.026
	(0.105)	(0.083)	(1.142)
mean	3.032	8.914	198.770
bandwidth	429	512	578
N	4,898	5,824	6,488

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

there are no no differential effects with respect to the property tax on main dwellings. While these effects are very small and only weakly significant, they are consistent with our main finding. In fact, people subject to the rate on other dwellings are either residents elsewhere, and therefore not eligible voters in the municipality, and/or owners of more than one dwelling, and so likely wealthier than the median voter. We also do not find any differential effect by the share of non-resident dwellings taken from the census (see Table A.7).

The waste tax can be considered as a property tax in disguise, because in most instances the tax bill is a function of the surface of the dwelling and the number of households components (Messina et al., 2018). Unfortunately, there exists no digitized data on the waste tax. Therefore, we can not investigate effects on waste tax rates. However, in Table 7 we show that revenues from the waste tax did not change due to the reform. In total, we conclude that the reform had no impact on property taxes.

Table A.6: Effect of the reform on property tax rates by mayor's skill

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount	(4) property tax on main dwellings	(5) property tax on other dwellings	(6) deduction amount
LATE	0.122	-0.034	1.697	0.084	-0.005	1.847
	(0.145)	(0.114)	(1.472)	(0.135)	(0.102)	(1.654)
LATE x college degree	-0.082	0.376*	-1.766			
	(0.233)	(0.196)	(2.535)			
LATE x high-skill job				-0.054	0.329	-2.222
				(0.258)	(0.209)	(2.121)
mean	3.030	8.913	198.777	3.021	8.911	198.715
bandwidth	429	512	578	429	512	578
N	4,797	5,709	6,362	4,689	5,586	6,214

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Effect of the reform on property tax rates by share of non-resident dwellings

	(1) property tax on main dwellings	(2) property tax on other dwellings	(3) deduction amount
LATE	0.141	0.153	0.257
	(0.121)	(0.094)	(1.131)
LATE x above-median non-resident dwellings	-0.076	-0.264	4.224
	(0.236)	(0.205)	(3.833)
mean	3.032	8.914	198.770
bandwidth	429	512	578
N	4.898	5.824	6.488

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. The sample includes the years 2012 to 2015. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B Institutions and Summary Statistics

Table B.1: Fiscal rule details

year	target	target function of	reference period	pop. threshold
2007	Saldo Finanziario	expenditures	2003-05	3,000
2008	Saldo Finanziario	expenditures	2003-05	3,000
2009	Saldo Finanziario	Saldo Finanziario	2007	5,000
2010	Saldo Finanziario	Saldo Finanziario	2007	5,000
2011	Saldo Finanziario	current expenditures	2006-08	5,000
2012	Saldo Finanziario	current expenditures	2006-08	5,000
2013	Saldo Finanziario	current expenditures	2007-09	1,000
2014	Saldo Finanziario	current expenditures	2009-11	1,000
2015	Saldo Finanziario	current expenditures	2010-12	1,000

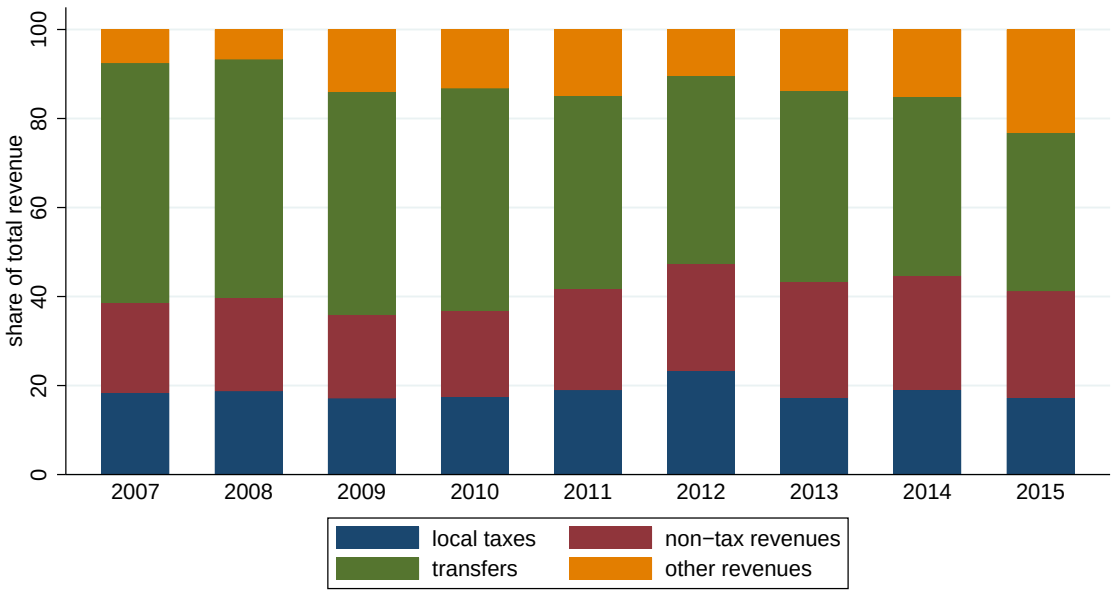
Notes: The table reports details on the target of the fiscal rule for different years. *Saldo Finanziario* is defined as the difference between expenditures and revenues, net of repayment of outstanding debt and of lending. The target *Saldo Finanziario* must be below a target defined as a function of some budget account items measured in a reference period.

Table B.2: Population cutoffs in Italian municipalities before and after 2013

population	mayor's wage		wage of ex. committee		size of city council		signature requirement		gender quota		fiscal rule	
	before	after	before	after	before	after	before	after	before	after	before	after
below 1,000	1,291	1,291	15%	15%	12	12	0	0	no	no	no	no
1,000 - 2,000	1,446	1,446	20%	20%	12	12	30	30	no	no	no	yes
2,000 - 3,000	1,446	1,446	20%	20%	12	12	40	40	no	no	no	yes
3,000 - 5,000	2,169	2,169	20%	20%	16	16	40	40	no	no	no	yes
5,000 - 10,000	2,789	2,789	50%	50%	16	16	80	80	no	yes	yes	yes

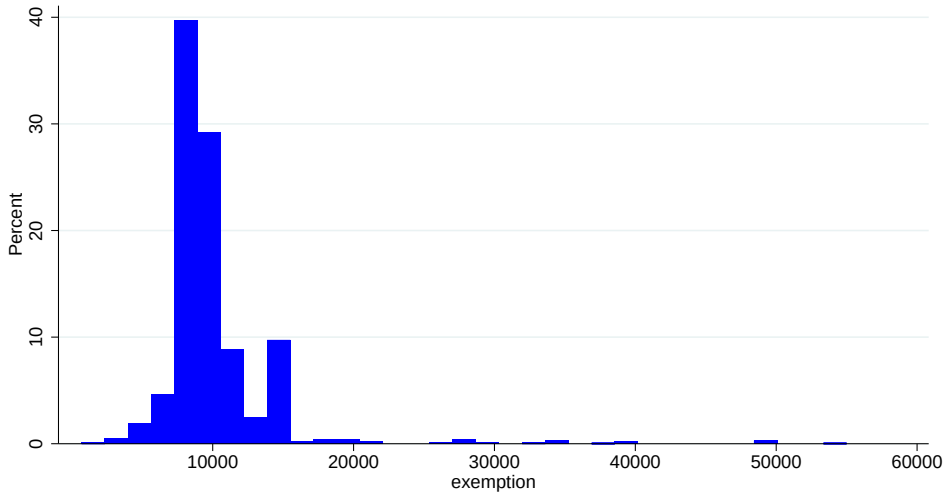
Source: Grembi et al. 2016, Vincent 2017, Baltrunaite et al. 2018. Notes: Policies varying at different legislative thresholds in the period 2007 - 2015. The *before* columns indicate the situation from 2007 to 2012, while the *after* columns refer to period from 2013 to 2015. Discontinuities at thresholds over 5,000 inhabitants are omitted. Population is the number of resident inhabitants. The wage of both the mayor and the executive committee refer to monthly gross wages and the latter is expressed as a percentage of the former. Size of city council is the number of seats in the city council. The signature requirement refers to number of signatures a candidate for mayor requires to be allowed to run, while the gender quota refers to candidate lists and new a system of double preference voting conditional on gender.

Figure B.1: Municipal revenues over time



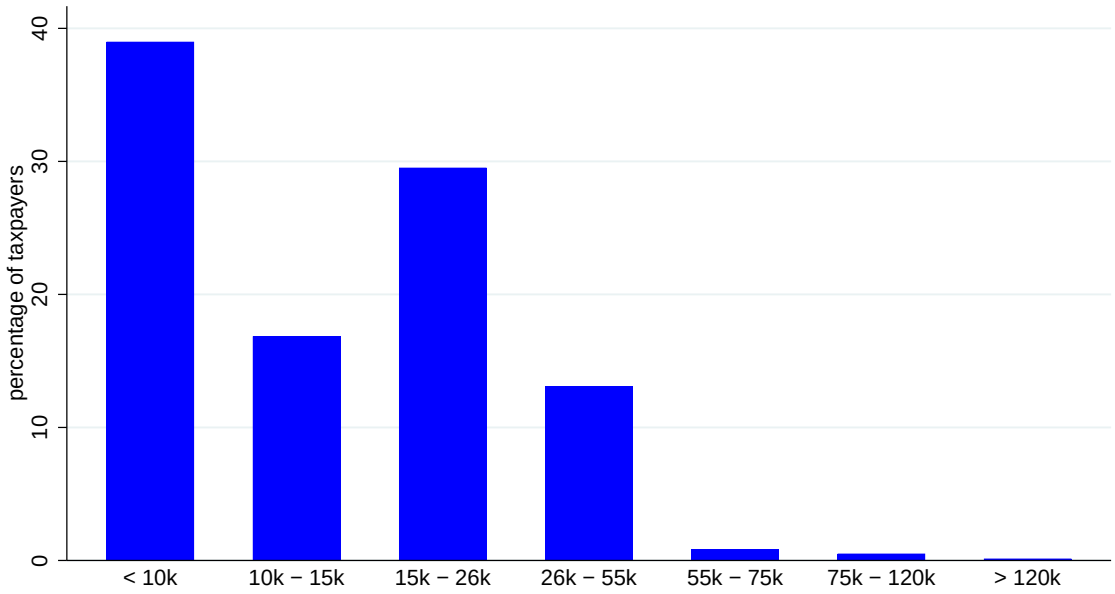
Notes: This figure plots the share of total revenue for different revenue categories of Italian municipalities below 2,500 inhabitants. Transfers also include revenues from the solidarity fund, financed by the property tax. Source: Municipal budget accounts (*Conti consuntivi*, accrual basis, Ministry of the Interior.)

Figure B.2: Distribution of exemption levels



Notes: This figure plots the density of exemption levels for the local personal income tax in Italian municipalities. The sample is restricted to municipalities that have a positive exemption level and less than 2,500 inhabitants.

Figure B.3: Distribution of taxpayers



Notes: This figure plots the percentage of tax payers in each taxable income bracket for municipalities with less than 2,500 inhabitants.

Table B.3: Descriptive statistics

	full sample			population $\leq$ 2500			population $>$ 2500			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	mean	sd	N	mean	sd	N	mean	sd	N	difference
area (in km^2)	34.384	47.298	58,323	23.364	23.063	28,335	44.795	60.209	29,988	-21.431***
coast dummy	0.068	0.252	58,323	0.022	0.147	28,335	0.112	0.315	29,988	-0.090***
altitude (in m)	334.405	275.099	58,323	448.871	292.956	28,335	226.249	204.985	29,988	222.622***
mayor: age	51.912	10.138	57,278	52.018	10.550	28,068	51.811	9.725	29,210	0.207
mayor: female	0.118	0.322	57,278	0.117	0.322	28,068	0.118	0.322	29,210	-0.001
mayor: college degree	0.454	0.498	56,581	0.364	0.481	27,642	0.540	0.498	28,939	-0.176***
mayor: high-skill occupation	0.378	0.485	55,517	0.306	0.461	27,335	0.448	0.497	28,182	-0.142***
mayor: political	0.295	0.456	56,481	0.154	0.361	27,646	0.430	0.495	28,835	-0.275***
mayor: last win margin	0.259	0.256	57,346	0.318	0.306	28,098	0.201	0.179	29,248	0.117***
mayor: term limit	0.302	0.459	57,181	0.287	0.452	28,007	0.317	0.465	29,174	-0.030***
years to next election	1.994	1.410	57,095	1.980	1.404	27,911	2.008	1.415	29,184	-0.028***
share: age $\geq$ 60	0.292	0.064	58,323	0.321	0.068	28,335	0.265	0.045	29,988	0.056***
share: female	0.508	0.015	58,323	0.505	0.019	28,335	0.511	0.010	29,988	-0.005***
share: college degree	0.074	0.027	58,323	0.067	0.024	28,335	0.081	0.028	29,988	-0.014***
top income share	0.106	0.078	58,323	0.075	0.075	28,335	0.135	0.068	29,988	-0.060***
taxable income per capita	12,605.327	3,309.273	57,569	11,962.550	3,047.158	28,148	13,220.292	3,431.061	29,421	-1,257.742***
tax rate at the 1st decile	0.348	0.279	58,062	0.329	0.264	28,148	0.366	0.292	29,914	-0.037***
tax rate at the 2nd decile	0.355	0.278	58,062	0.333	0.264	28,148	0.377	0.290	29,914	-0.044***
tax rate at the 3rd decile	0.388	0.272	58,062	0.346	0.262	28,148	0.426	0.276	29,914	-0.080***
tax rate at the 4th decile	0.412	0.265	58,062	0.358	0.259	28,148	0.464	0.260	29,914	-0.105***
tax rate at the 5th decile	0.425	0.259	58,062	0.365	0.257	28,148	0.482	0.248	29,914	-0.117***
tax rate at the 6th decile	0.430	0.256	58,062	0.367	0.256	28,148	0.490	0.241	29,914	-0.123***
tax rate at the 7th decile	0.433	0.255	58,062	0.369	0.255	28,148	0.494	0.239	29,914	-0.125***
tax rate at the 8th decile	0.436	0.254	58,062	0.370	0.255	28,148	0.498	0.236	29,914	-0.128***
tax rate at the 9th decile	0.440	0.253	58,062	0.372	0.255	28,148	0.504	0.234	29,914	-0.132***
average rate progression	0.428	1.000	58,323	0.198	0.672	28,335	0.646	1.192	29,988	-0.448***
marginal rate progression	0.433	1.000	58,323	0.204	0.690	28,335	0.649	1.183	29,988	-0.444***
exemption level	2,019.226	4,718.198	58,035	997.086	3,348.429	28,135	2,981.029	5,545.364	29,900	-1983.943***
progressive tax	0.179	0.383	58,062	0.094	0.292	28,148	0.259	0.438	29,914	-0.166***
deficit	0.136	202.621	57,400	5.711	268.320	28,061	-5.197	106.800	29,339	10.908***
income tax revenues	45.811	32.760	57,104	36.972	32.142	27,822	54.209	31.091	29,282	-17.237***
property tax revenues	173.672	476.889	57,104	182.533	249.620	27,822	165.252	619.811	29,282	17.281***
trash tax revenues	106.744	84.675	57,104	114.252	88.938	27,822	99.610	79.764	29,282	14.642***
non-tax revenues	370.977	467.983	57,104	443.152	627.985	27,822	302.401	206.758	29,282	140.751***
transfer revenues	682.851	1,296.016	57,104	993.877	1,752.916	27,822	387.333	420.556	29,282	606.544***
loan revenues	143.526	383.864	57,104	173.298	489.218	27,822	115.238	241.489	29,282	58.060***
other revenues	104.349	700.645	57,104	137.844	760.640	27,822	72.525	636.822	29,282	65.319***
total expenditures	1,371.050	1,676.247	57,656	1,784.216	2,254.043	28,237	974.485	555.983	29,419	809.730***
current expenditures	859.789	595.954	57,656	1,003.021	772.813	28,237	722.312	290.224	29,419	280.709***
capital expenditures	511.262	1,360.819	57,656	781.195	1,858.662	28,237	252.174	420.030	29,419	529.022***
exp: administrative	358.101	415.556	57,656	471.887	530.687	28,237	248.887	209.219	29,419	223.000***
exp: culture	25.129	98.424	57,656	27.745	133.727	28,237	22.619	42.526	29,419	5.126***
exp: development	15.072	123.668	57,656	19.856	167.832	28,237	10.481	53.803	29,419	9.374***
exp: education	97.632	123.998	57,656	102.513	155.077	28,237	92.947	83.705	29,419	9.566***
exp: environment	320.293	949.229	57,656	445.122	1,323.818	28,237	200.479	233.465	29,419	244.643***
exp: judiciary	1.132	12.033	57,656	0.591	14.434	28,237	1.651	9.124	29,419	-1.060***
exp: police	32.768	44.306	57,656	33.043	57.428	28,237	32.505	26.107	29,419	0.537
exp: social	97.483	152.768	57,656	93.587	183.620	28,237	101.223	115.537	29,419	-7.635***
exp: sport	32.031	341.246	57,656	43.866	485.028	28,237	20.671	46.479	29,419	23.195***
exp: resources	25.341	461.792	57,656	39.354	649.422	28,237	11.890	113.004	29,419	27.464***
exp: transport	183.282	376.503	57,656	264.642	511.934	28,237	105.191	117.561	29,419	159.451***
exp: tourism	26.458	278.320	57,656	43.964	390.665	28,237	9.655	68.931	29,419	34.309***
re-run	0.594	0.491	13,149	0.599	0.490	6,563	0.400	0.492	6,586	0.010
reelection	0.798	0.401	8,271	0.827	0.378	4,266	0.768	0.422	4,005	0.059***

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Column 10 displays the difference between columns 4 and 7. All expenditure, revenue, and deficit variables are expressed in per capita terms and 2015 Euros.

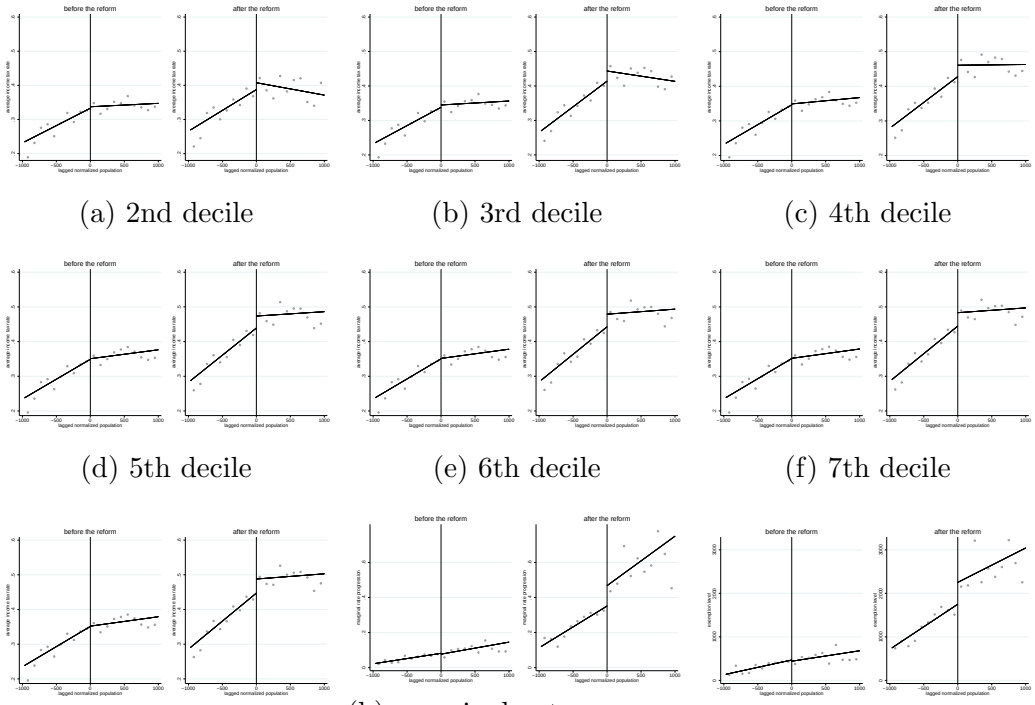
Table B.4: Descriptive statistics: matched sample

	matched sample			non-matched sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	mean	sd	N	mean	sd	N	difference
area (in km^2)	34.855	47.672	44,781	33.000	45.448	12,357	1.855**
coast dummy	0.067	0.250	44,781	0.071	0.257	12,357	-0.004
altitude (in m)	332.851	275.745	44,781	342.933	277.283	12,357	-10.082*
mayor: female	0.115	0.319	44,768	0.127	0.333	12,249	-0.012*
mayor: college degree	0.456	0.498	44,240	0.451	0.498	12,082	0.004
mayor: age	51.807	10.072	44,768	52.261	10.312	12,249	-0.454**
mayor: political	0.296	0.457	44,129	0.293	0.455	12,093	0.003
mayor: term limit	0.295	0.456	44,725	0.330	0.470	12,196	-0.036***
years to next election	2.007	1.433	44,779	1.949	1.319	12,316	0.058***
share: college degree	0.074	0.027	44,781	0.074	0.028	12,357	0.000
share: female	0.508	0.015	44,781	0.507	0.016	12,357	0.001**
share: age $\geq$ 60	0.292	0.064	44,781	0.296	0.064	12,357	-0.004***
top income share	0.105	0.077	44,781	0.104	0.079	12,357	0.001
taxable income per capita	12,557.276	3,307.428	44,549	12,696.957	3,273.648	12,272	-139.681**
average rate progression	0.427	0.999	44,781	0.398	0.966	12,357	0.029*
marginal rate progression	0.431	0.998	44,781	0.403	0.966	12,357	0.028*
exemption level	2,006.282	4,701.750	44,764	1,930.638	4,669.887	12,353	75.644
progressive tax	0.178	0.383	44,781	0.170	0.375	12,357	0.009
runner-up: female	0.147	0.354	44,781				
runner-up: age	51.830	10.951	44,781				
runner-up: college degree	0.443	0.497	43,232				
runner-up: political	0.277	0.447	41,809				
vote margin	-0.033	0.291	44,240				
mixed race	0.439	0.496	42,765				

Notes: The matched sample includes observations for which we were able to match the main dataset with the election data. The non-matched sample includes the remaining observations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Column 7 displays the difference between columns 1 and 4.

C Additional Findings

Figure C.1: Regression discontinuity plots: other outcomes



(a) 2nd decile (b) 3rd decile (c) 4th decile

(d) 5th decile (e) 6th decile (f) 7th decile

(g) 8th decile (h) marginal rate progression (i) exemption level

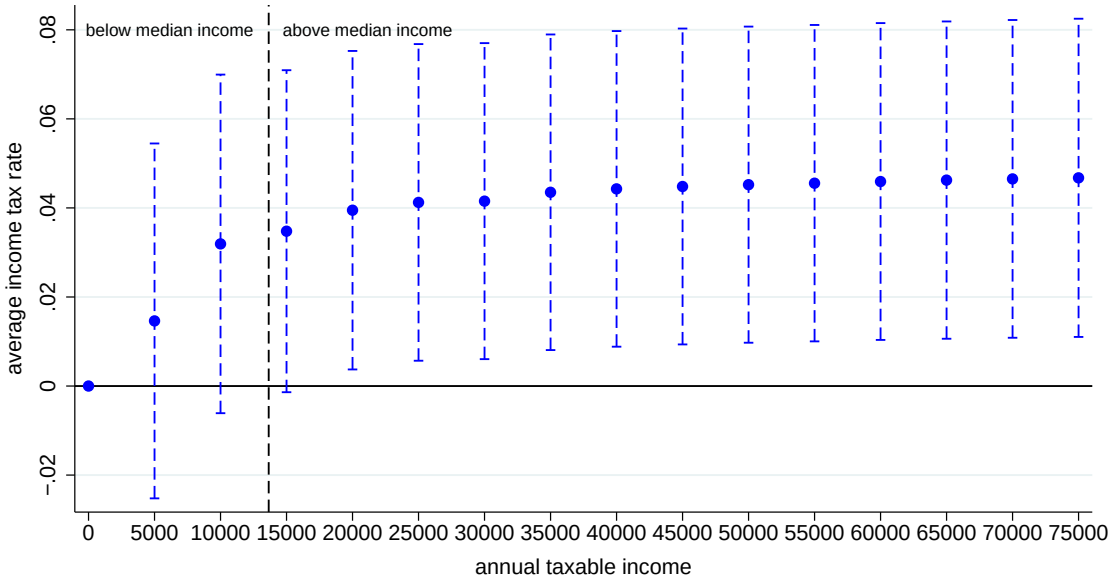
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each graph is a regression discontinuity plot for pre-reform years (2007-12, on the left) and post-reform years (2013-15, on the right). The outcome variable is reported underneath each graph. The running variable is lagged normalized population. Plots are obtained with the STATA command *rdplot* (Calonico et al., 2015) organizing the data in 10 bins on each side of the threshold. The lines are linear fits estimated separately on each side of the threshold.

Table C.1: Effect of the reform on the average income tax rate at different income levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	5k€	10k€	15k€	20k€	25k€	30k€	35k€	40k€	45k€	50k€	55k€	60k€	65k€	70k€	75k€
LATE	0.014	0.032	0.034*	0.038**	0.040**	0.040**	0.042**	0.043**	0.044**	0.044**	0.044**	0.045**	0.045**	0.045**	0.046**
	(0.020)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
> 5k€	-	0.034	0.049	0.026	0.020	0.020	0.015	0.013	0.013	0.012	0.012	0.012	0.012	0.012	0.012
> 10k€	-	-	0.358	0.195	0.152	0.152	0.116	0.106	0.100	0.096	0.093	0.091	0.089	0.088	0.087
> 15k€	-	-	-	0.109	0.067	0.076	0.050	0.046	0.044	0.043	0.043	0.044	0.044	0.045	0.046
mean	0.331	0.351	0.363	0.367	0.368	0.368	0.369	0.370	0.371	0.371	0.371	0.372	0.372	0.372	0.373
bandwidth	663	663	663	663	663	663	663	663	663	663	663	663	663	663	663
N	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660	17,660

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at yearly incomes of 5,000€, 10,000€, and 15,000€ respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure C.2: Effect of the reform on income tax rates at different income levels



Notes: This figure plots the local average treatment effects reported in Table C.1 and their 95% confidence bands.

Table C.2: Effect of the reform on the income tax base by bracket

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<10k€	10k€-15k€	15k€-28k€	28k€-55k€	55k€-75k€	75k€-120k€	>120k€
log taxbase							
LATE	-0.03	0.01	0.03	0.06	0.07*	0.00	-0.06
	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.07)
mean	14.07	14.17	15.14	14.79	13.24	13.47	14.07
bandwidth	406	394	462	479	935	1058	1515
N	10,974	10,632	12,688	13,163	14,678	12,150	6,436

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable. The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). The outcome variables are per capita (upper panel) and total (bottom panel) tax revenues in 2015 Euros generated by tax payers with taxable income included in the bracket reported on top of each column. The table reports also the sample mean of the outcome variable, the average number of taxpayers in each bracket, the used bandwidth and the number of observations. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Differential effect of the reform by mayor's skill

	(1) progressive tax	(2) progressive tax	(3) progressive tax	(4) progressive tax	(5) progressive tax	(6) progressive tax	(7) progressive tax	(8) progressive tax
LATE	-0.002	-0.002	-0.020	-0.008	-0.000	0.004	-0.009	0.012
	(0.034)	(0.034)	(0.032)	(0.070)	(0.032)	(0.032)	(0.031)	(0.069)
LATE x college degree	0.138***	0.136***	0.121***	0.116**				
	(0.053)	(0.052)	(0.046)	(0.046)				
LATE x high-skill job					0.153***	0.141**	0.113**	0.106**
					(0.059)	(0.059)	(0.051)	(0.050)
LATE x female mayor				0.045				0.045
				(0.070)				(0.072)
LATE x left-wing mayor				0.006				0.006
				(0.064)				(0.065)
LATE x right-wing mayor				-0.190				-0.187
				(0.134)				(0.136)
LATE x centrist mayor				-0.292				-0.327
				(0.232)				(0.253)
LATE x low win margin				0.027				0.024
				(0.047)				(0.048)
LATE x term limit				-0.007				-0.021
				(0.041)				(0.042)
LATE x high pre-reform deficit				0.037				0.029
				(0.054)				(0.054)
LATE x low top income share				-0.076				-0.077
				(0.054)				(0.055)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.087	0.088	0.088	0.088	0.086	0.088	0.088	0.088
bandwidth	650	650	650	650	650	650	650	650
N	16,932	16,663	16,663	16,663	16,848	16,321	16,321	16,321

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Differential effect of the reform by mayor's skill

	(1) exemption level	(2) exemption level	(3) exemption level	(4) exemption level	(5) exemption level	(6) exemption level	(7) exemption level	(8) exemption level
LATE	-45	2	-211	167	-77	10	-158	289
	(406)	(403)	(385)	(861)	(357)	(354)	(355)	(830)
LATE x college degree	1494**	1457**	1363**	1361**				
	(604)	(603)	(553)	(558)				
LATE x high-skill job					1918***	1741**	1629**	1552**
					(700)	(705)	(656)	(647)
LATE x female mayor				466				339
				(819)				(847)
LATE x left-wing mayor				-94				-91
				(743)				(750)
LATE x right-wing mayor				-1988				-1950
				(1612)				(1646)
LATE x centrist mayor				-4601				-4856
				(3388)				(3518)
LATE x low win margin				124				138
				(571)				(583)
LATE x term limit				-168				-260
				(471)				(476)
LATE x high pre-reform deficit				470				366
				(657)				(656)
LATE x low top income share				-1115*				-1068
				(668)				(670)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	896	906	906	906	886	904	904	904
bandwidth	635	635	635	635	635	635	635	635
N	16,577	16,319	16,319	16,319	16,493	15,985	15,985	15,985

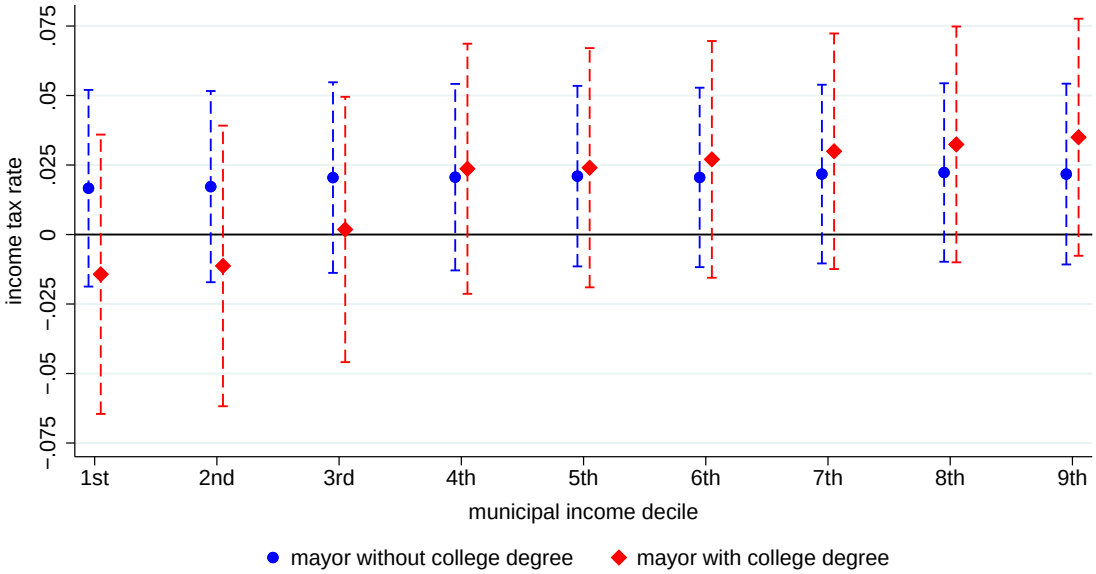
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Differential effect of the reform by mayor's skill

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	marginal rate progression	marginal rate progression	marginal rate progression	marginal rate progression	marginal rate progression	marginal rate progression	marginal rate progression	marginal rate progression
LATE	0.005	0.006	-0.009	0.055	0.016	0.024	-0.002	0.080
	(0.086)	(0.086)	(0.084)	(0.180)	(0.081)	(0.082)	(0.082)	(0.176)
LATE x college degree	0.353***	0.346***	0.281**	0.281**				
	(0.127)	(0.126)	(0.115)	(0.117)				
LATE x high-skill job					0.393***	0.364***	0.329***	0.317**
					(0.139)	(0.139)	(0.127)	(0.125)
LATE x female mayor				0.039				0.039
				(0.168)				(0.172)
LATE x left-wing mayor				-0.009				-0.008
				(0.160)				(0.161)
LATE x right-wing mayor				-0.459				-0.444
				(0.330)				(0.334)
LATE x centrist mayor				-0.523				-0.630*
				(0.331)				(0.375)
LATE x low win margin				0.082				0.076
				(0.121)				(0.125)
LATE x term limit				-0.079				-0.105
				(0.105)				(0.107)
LATE x high pre-reform deficit				0.126				0.115
				(0.140)				(0.140)
LATE x low top income share				-0.244*				-0.245*
				(0.143)				(0.144)
controls		yes	yes	yes		yes	yes	yes
municipality FE			yes	yes			yes	yes
mean	0.182	0.184	0.184	0.184	0.179	0.183	0.183	0.183
bandwidth	668	668	668	668	668	668	668	668
N	17,378	17,092	17,092	17,092	17,292	16,741	16,741	16,741

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. We measure mayors' skills using two dummies: college degree, which is equal to one in case the mayor holds one; and high-skill job, which is equal to one in case the mayor was employed in a managing position or in an intellectual profession (e.g. lawyer, medical doctor). Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure C.3: Effect of the reform on income tax rates by mayor's skill level



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The blue estimates refer to the local average treatment effect for mayors without a college degree (β_6), while the red estimates plot the sum of β_6 and β_6^{int} referring to the effect for mayors with a college degree in equation 3.

Table C.6: Differential effect of the reform by mayor's skill: mixed election RD

	(1) progressive tax	(2) progressive tax	(3) progressive tax	(4) progressive tax	(5) progressive tax	(6) progressive tax
LATE	-0.019	-0.024	-0.066	-0.143		
	(0.042)	(0.043)	(0.183)	(0.189)		
college degree	-0.006	-0.007	-0.009	0.235	0.024	0.074
	(0.018)	(0.019)	(0.048)	(0.210)	(0.022)	(0.064)
LATE x college degree	0.140**	0.148**	0.510**	0.339		
	(0.062)	(0.064)	(0.257)	(0.224)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	0.088	0.089	0.088	0.088	0.041	0.041
population bandwidth	650	650	650	650	650	650
close election bandwidth			0.15	0.15	0.15	0.15
N	13,043	12,028	1,949	1,949	1,377	1,377

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi et al. (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 4. Details on all covariates are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Differential effect of the reform by mayor's skill: mixed election RD

	(1) exemption level	(2) exemption level	(3) exemption level	(4) exemption level	(5) exemption level	(6) exemption level
LATE	-72	-87	-1550	-932		
	(498)	(502)	(2280)	(2368)		
college degree	-118	-116	-182	941	116	386
	(163)	(177)	(539)	(2092)	(209)	(526)
LATE x college degree	1440*	1458*	5694*	3007		
	(735)	(754)	(3130)	(2848)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	910.04	909.19	862.72	862.72	371.17	371.17
population bandwidth	635	635	635	635	635	635
close election bandwidth			0.16	0.16	0.16	0.16
N	12,764	11,770	2,119	2,119	1,509	1,509

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi et al. (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 4. Details on all covariates are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Differential effect of the reform by mayor's skill: mixed election RD

	(1) marginal rate progression	(2) marginal rate progression	(3) marginal rate progression	(4) marginal rate progression	(5) marginal rate progression	(6) marginal rate progression
LATE	-0.022	-0.034	-0.157	-0.317		
	(0.105)	(0.105)	(0.387)	(0.392)		
college degree	-0.045	-0.049	0.017	0.303	0.022	0.072
	(0.029)	(0.031)	(0.103)	(0.390)	(0.041)	(0.078)
LATE x college degree	0.320**	0.335**	1.077**	0.828*		
	(0.146)	(0.153)	(0.524)	(0.478)		
controls		yes	yes	yes	yes	yes
mixed election RD			yes	yes	yes	yes
municipality FE				yes		yes
pre-reform sample					yes	yes
mean	0.184	0.184	0.175	0.175	0.081	0.081
population bandwidth	668	668	668	668	668	668
close election bandwidth			0.18	0.18	0.18	0.18
N	13,384	12,355	2,418	2,418	1,725	1,725

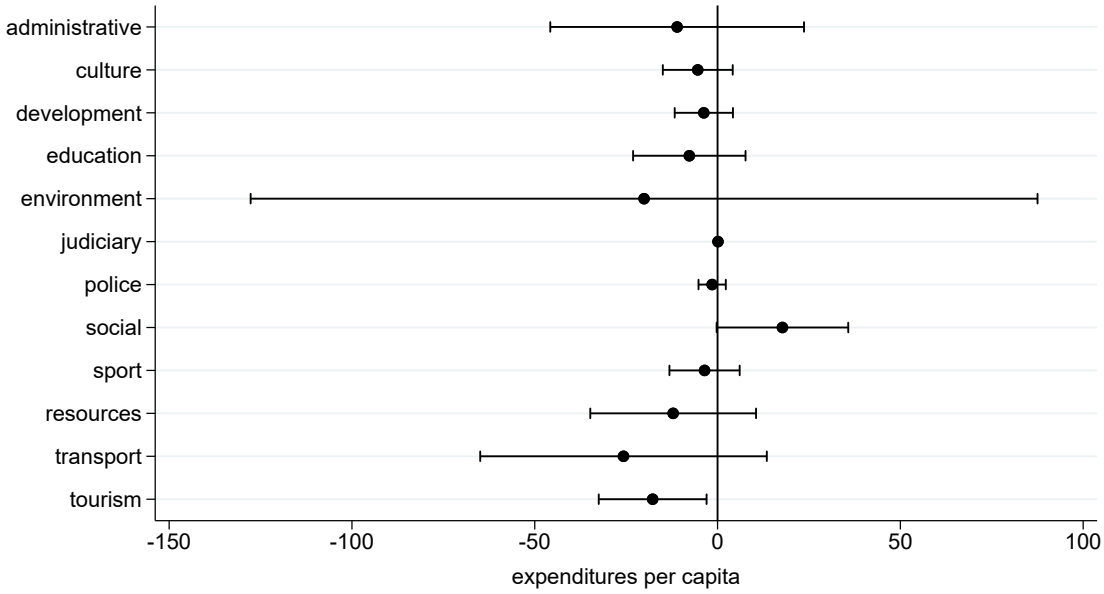
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The sample is restricted to observations for which we were able to match the main dataset with the election data. Columns (1), (2), (3) and (4) report estimates of the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. In rows with "mixed election RD" switched on, the model is augmented with the margin of victory and its interaction with all other terms, and the sample is further restricted to municipality-year observations, in which the incumbent mayor was elected in a race against a runner up with a different education level (college vs. non-college). Population bandwidths are selected following Grembi et al. (2016). Election bandwidths are selected using the using the STATA command *rdrobust*. Columns (5) and (6) report estimates of the college effect from regression discontinuity models where the running variable is the margin of victory, the treatment dummy is equal to one if the mayors holds a college degree, and the sample is restricted to years before the reform (β_2) in equation 4. Details on all covariates are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidths and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Effects of the reform on mayors' reelection odds

	(1) reelection	(2) reelection	(3) reelection	(4) reelection	(5) re-run	(6) re-run	(7) re-run	(8) re-run
LATE	-0.037	-0.323**	-0.383***		-0.065	-0.069	-0.041	
	(0.060)	(0.144)	(0.129)		(0.061)	(0.112)	(0.116)	
high-skill job		0.400	0.235	-0.019		0.190	0.099	0.010
		(0.295)	(0.278)	(0.025)		(0.134)	(0.137)	(0.027)
LATE x high-skill job		0.644**	0.570**			0.001	-0.183	
		(0.269)	(0.251)			(0.227)	(0.235)	
municipality FE		yes	yes			yes	yes	
controls			yes	yes			yes	yes
pre-reform sample				yes				yes
mean	0.837	0.837	0.839	0.839	0.624	0.624	0.629	0.618
bandwidth	1059	1059	1059	1059	1088	1088	1088	1088
N	2,675	2,675	2,548	1,276	3,935	3,935	3,720	2,059

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Columns (1) and (5) report estimates of the local average treatment effect (LATE) in the baseline difference-in-discontinuities model. Columns (2), (3), (6) and (7) report estimates of the LATE for mayors without a high-skill job (LATE) and for mayors with a high-skill job (LATE x high-skill job), estimated using the difference-in-discontinuities model extended to estimate heterogeneous treatment effects. Columns (4) and (8) report estimates of the high-skill job effect from a regression of the outcome on a dummy is equal to one if the mayors has a high-skill job, and the sample is restricted to years before the reform. Bandwidths are selected following Grembi et al. (2016). The reelection outcome variable in columns (1) to (4) equals one for incumbents that run again and are reelected, and is zero for those who rerun and fail to be reelected. The rerun outcome variable in columns (5) to (8) equals one for incumbents that are not term-limited and choose to run again, and is zero for those who do not and are not term-limited.Control variables are described in Section 4. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure C.4: Effect of the reform on municipal expenditures by categories



Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The figure plots the LATE corresponding to β_6 in equation 2 and its 95% confidence bands. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. All variables are expressed in per capita terms and 2015 Euros and are winsorized.

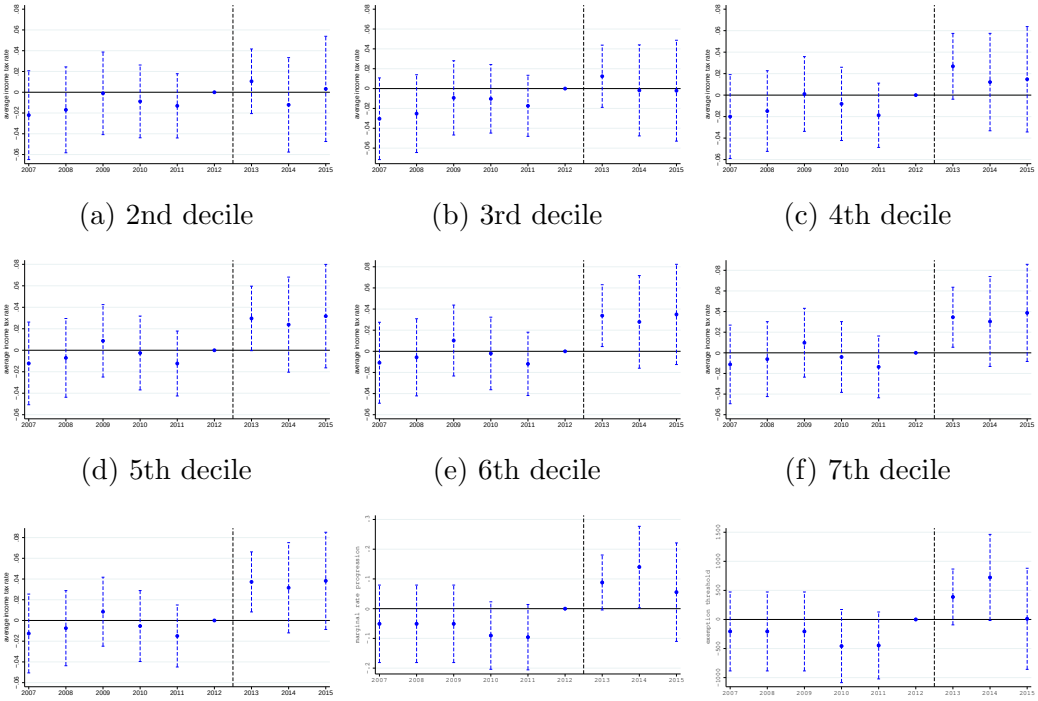
Table C.10: Differential effect of the reform by mayor's skill

	(1) exp: administrative	(2) exp: culture	(3) exp: development	(4) exp: education	(5) exp: environment	(6) exp: judiciary
LATE	-19.07	1.50	-1.95	-6.96	-54.82	-0.03
	(17.02)	(5.11)	(3.30)	(10.22)	(60.10)	(0.08)
LATE x college degree	47.95	-3.91	-5.48	-3.57	93.65	0.15
	(34.15)	(9.03)	(6.96)	(16.26)	(105.79)	(0.12)
municipality FE	yes	yes	yes	yes	yes	yes
controls	yes	yes	yes	yes	yes	yes
mean	418.10	21.28	11.73	104.06	379.13	0.19
bandwidth	514	664	694	583	509	530
N	13,397	16,918	17,618	15,036	13,306	13,738
	(7) exp: police	(8) exp: social	(9) exp: sport	(10) exp: resources	(11) exp: transport	(12) exp: tourism
LATE	0.59	4.31	-6.73	-9.68	-28.19	-12.81*
	(1.77)	(10.44)	(5.41)	(9.63)	(20.84)	(6.85)
LATE x college degree	1.03	1.62	9.58	28.80*	44.90	4.98
	(3.27)	(16.52)	(8.62)	(16.12)	(37.95)	(12.92)
municipality FE	yes	yes	yes	yes	yes	yes
controls	yes	yes	yes	yes	yes	yes
mean	32.38	84.83	29.58	23.14	225.53	25.72
bandwidth	640	549	777	564	591	688
N	16,359	14,225	19,469	14,577	15,216	17,486

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The table reports estimates of difference-in-discontinuities models extended to estimate heterogeneous treatment effects. The extended models include one or more binary covariates and their interactions with all the terms included in the baseline model. The row LATE reports the local average treatment effect in case the additional interaction variables are equal to zero (β_6), while the interaction rows report the differential effects when the interaction variables are switched on (β_6^{int}) in equation 3. Details on all covariates are described in Section 4. The estimation method is local linear regression. The bandwidth is selected following Grembi et al. (2016). In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Robustness Tests

Figure D.1: Dynamic model: other outcomes



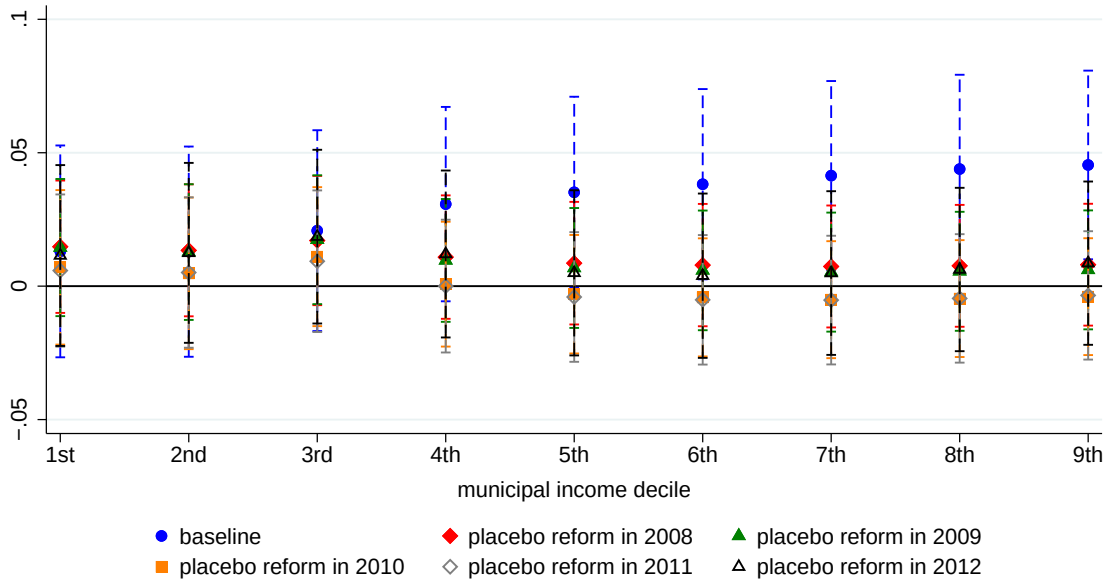
(a) 2nd decile (b) 3rd decile (c) 4th decile

(d) 5th decile (e) 6th decile (f) 7th decile

(g) 8th decile (h) marginal rate progression (i) exemption level

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. Each panel plots estimates from the dynamic model on a different outcome variable, reported underneath each plot. The dynamic model is an extension of the baseline difference-in-discontinuities model that includes year dummies instead of the reform dummy. The bandwidth is selected following Grembi et al. (2016). Each dot is the estimate of the deviation of the outcome variable in the year reported on the horizontal axis relative to the pre-reform year 2012. Dotted bars are 95% confidence bands.

Figure D.2: Income tax rates by municipal income deciles: placebo reforms



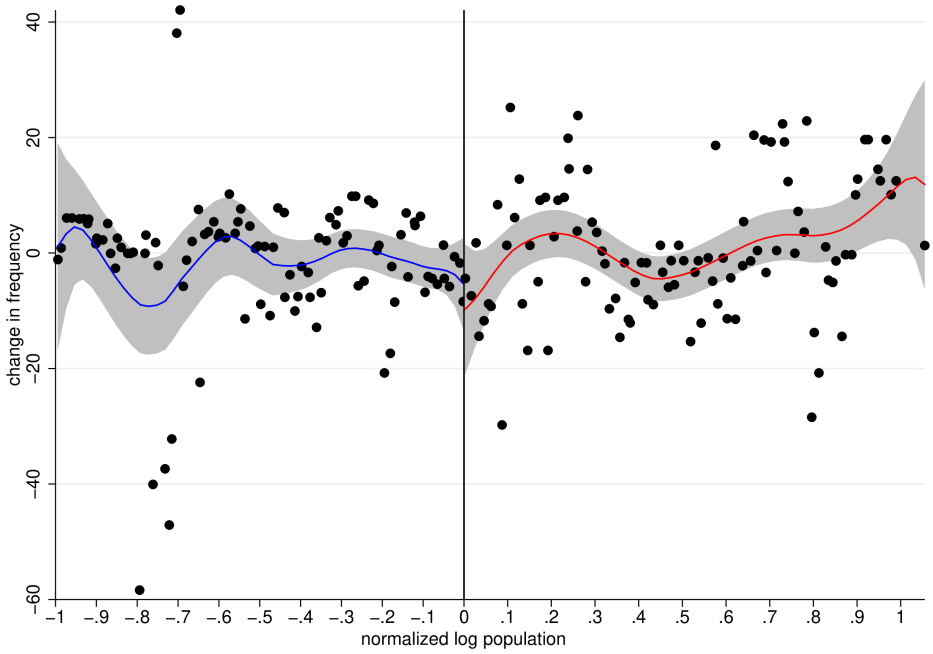
Notes: The blue line plots the local average treatment effect (β_6) and its 95% confidence bands from Table 1. All other lines plot placebo estimates. These are obtained by restricting the sample to pre-reform years, assigning the reform to a different year from 2008 to 2012 and finally re-estimating equation 2.

Table D.1: Continuity tests

	(1) mayor: female	(2) mayor: age	(3) mayor: college degree	(4) mayor: high-skill occupation	(5) mayor: right-wing	(6) mayor: left-wing	(7) mayor: center	(8) mayor: term limit
LATE	-0.012	0.492	-0.007	-0.008	-0.003	0.001	0.000	-0.021
	(0.026)	(0.876)	(0.037)	(0.043)	(0.011)	(0.028)	(0.007)	(0.029)
mean	0.118	52.029	0.361	0.312	0.023	0.108	0.011	0.287
bandwidth	658	625	662	530	597	619	668	563
N	17,404	16,565	17,238	13,946	15,917	16,464	17,670	15,046
	(9) share: age $\geq$ 60	(10) share: female	(11) log area	(12) coast dummy	(13) altitude (in m)	(14) years to next election	(15) top income share	(16) log taxable income per capita
LATE	0.005	-0.002*	0.039	0.012*	-23.308	-0.066	0.006	0.020
	(0.005)	(0.001)	(0.052)	(0.007)	(21.546)	(0.077)	(0.005)	(0.014)
mean	0.321	0.506	2.766	0.021	445.792	1.971	0.075	9.998
bandwidth	586	621	587	581	526	511	614	658
N	15,771	16,604	15,796	15,650	14,244	13,835	16,437	17,537

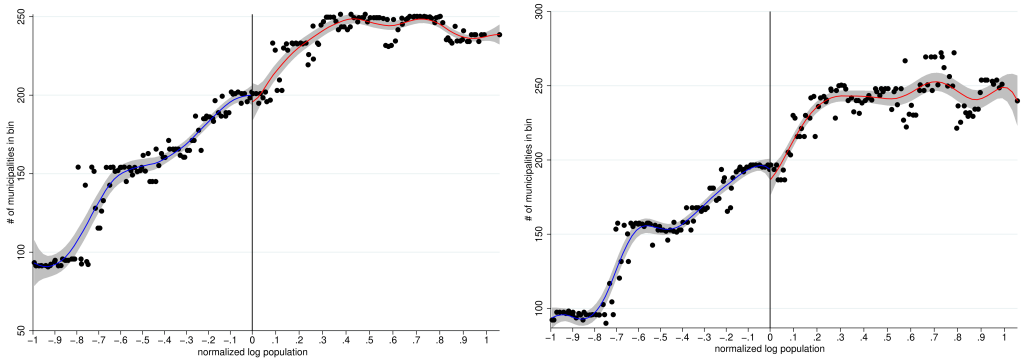
Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate local linear regression for each outcome variable (reported at the top of each column). The LATE corresponds to β_6 in equation 2. The bandwidth is selected following Grembi et al. (2016). Outcome variables are reported on top of each column. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure D.4: Dynamic McCrary test



Notes: The figure presents the density plot for the difference-in-discontinuities design in the spirit of (McCrary, 2008). Each dot is the local average of the change in the total number of observations between the the pre- and post-reform periods within each bin of normalized log population. Each bin has width equal to 0.01. The lines are fit of local polynomial using a quadratic degree and a triangular kernel. Grey bans are the corresponding 95% confidence bands.

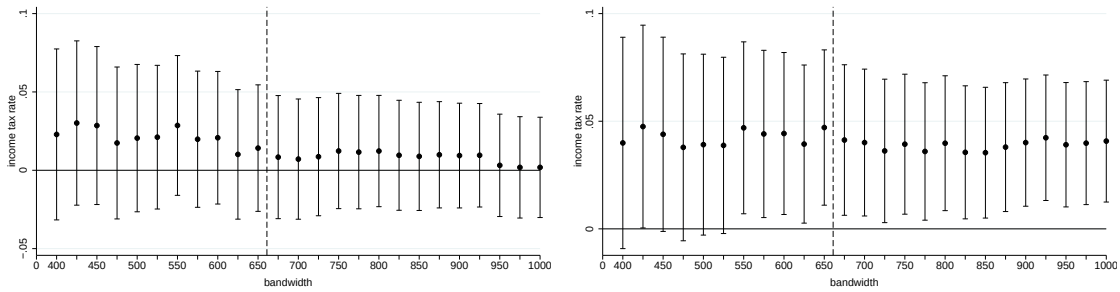
Figure D.3: McCrary test before and after the reform



(a) pre-reform (b) post-reform

Notes: This figure presents McCrary density plots (McCrary, 2008). The left panel shows a pooled graph for all pre-reform years, while the right panel shows pool graph for all post-reform years.

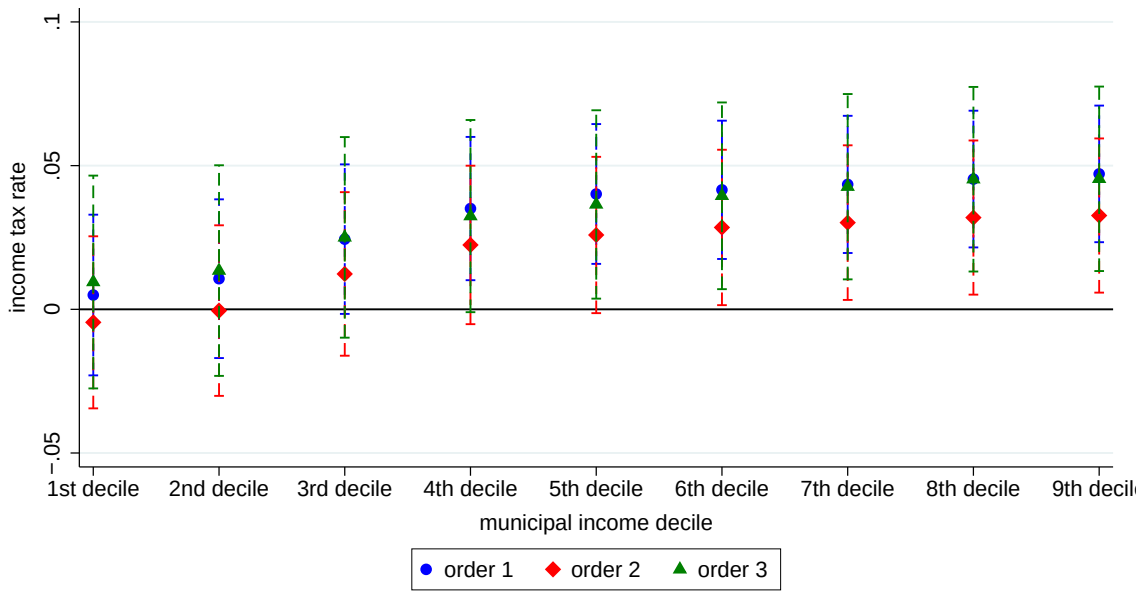
Figure D.5: Estimates by bandwidth



(a) Income tax rate at the first decile (b) Income tax rate at the 9th decile

Notes: This figure plots the local average treatment effect (β_6) and its 95% confidence bands reported in Table 1 for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure D.6: Income tax rates: global polynomial regressions



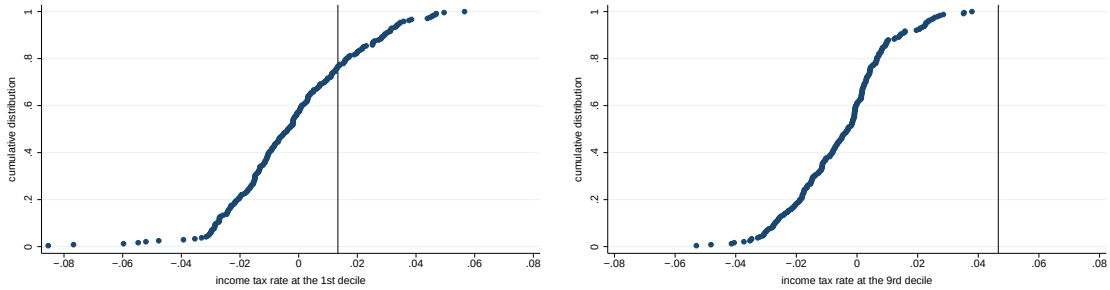
Notes: This figure plots the local average treatment effects reported in Table D.2 and their 95% confidence bands.

Table D.2: Income tax rates: global polynomial regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
tax rate at	1st decile	2nd decile	3rd decile	4th decile	5th decile	6th decile	7th decile	8th decile	9th decile
polynomial of order 1									
LATE	0.005	0.011	0.024*	0.035***	0.040***	0.042***	0.043***	0.045***	0.047***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.055	0.006	0.001	0.000	0.000	0.000	0.000	0.000
> 2nd decile	-	-	0.024	0.002	0.001	0.001	0.000	0.000	0.000
> 3rd decile	-	-	-	0.029	0.012	0.010	0.006	0.004	0.003
> 4th decile	-	-	-	-	0.125	0.097	0.056	0.036	0.026
> 5th decile	-	-	-	-	-	0.252	0.100	0.056	0.040
> 6th decile	-	-	-	-	-	-	0.066	0.042	0.030
> 7th decile	-	-	-	-	-	-	-	0.102	0.051
> 8th decile	-	-	-	-	-	-	-	-	0.075
polynomial of order 2									
LATE	-0.005	-0.000	0.012	0.022	0.026*	0.028**	0.030**	0.032**	0.033**
	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.101	0.015	0.002	0.001	0.001	0.000	0.000	0.000
> 2nd decile	-	-	0.035	0.004	0.003	0.001	0.001	0.001	0.001
> 3rd decile	-	-	-	0.03	0.022	0.011	0.007	0.004	0.005
> 4th decile	-	-	-	-	0.214	0.103	0.060	0.038	0.042
> 5th decile	-	-	-	-	-	0.055	0.016	0.011	0.026
> 6th decile	-	-	-	-	-	-	0.082	0.034	0.063
> 7th decile	-	-	-	-	-	-	-	0.053	0.107
> 8th decile	-	-	-	-	-	-	-	-	0.285
polynomial of order 3									
LATE	0.010	0.013	0.025	0.032*	0.036**	0.039**	0.043***	0.045***	0.045***
	(0.019)	(0.019)	(0.018)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
mean	0.329	0.332	0.346	0.358	0.364	0.367	0.368	0.369	0.371
N	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128	28,128
> 1st decile	-	0.161	0.055	0.023	0.014	0.009	0.005	0.003	0.004
> 2nd decile	-	-	0.095	0.040	0.024	0.015	0.008	0.005	0.006
> 3rd decile	-	-	-	0.139	0.082	0.050	0.025	0.016	0.021
> 4th decile	-	-	-	-	0.215	0.120	0.053	0.030	0.042
> 5th decile	-	-	-	-	-	0.140	0.030	0.014	0.036
> 6th decile	-	-	-	-	-	-	0.024	0.011	0.050
> 7th decile	-	-	-	-	-	-	-	0.039	0.147
> 8th decile	-	-	-	-	-	-	-	-	0.462

Notes: The reform is the introduction of the fiscal rule for municipalities above 1,000 inhabitants in 2013. The top panel reports the local average treatment effect (LATE) of the difference-in-discontinuities model estimated with a separate global regression for each outcome variable (reported at the top of each column) on the sample of all municipalities below 2,500 inhabitants with different polynomial orders n. The deciles refer to the income distribution in each municipality. The middle panel displays p-values for pairwise one-sided tests (estimated by seemingly unrelated regression) whether the effect is higher than the effect on the tax rate at the first to eighth municipal income decile, respectively. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure D.7: Placebo thresholds



(a) tax rate at the 1st decile (b) tax rate at the 9th decile

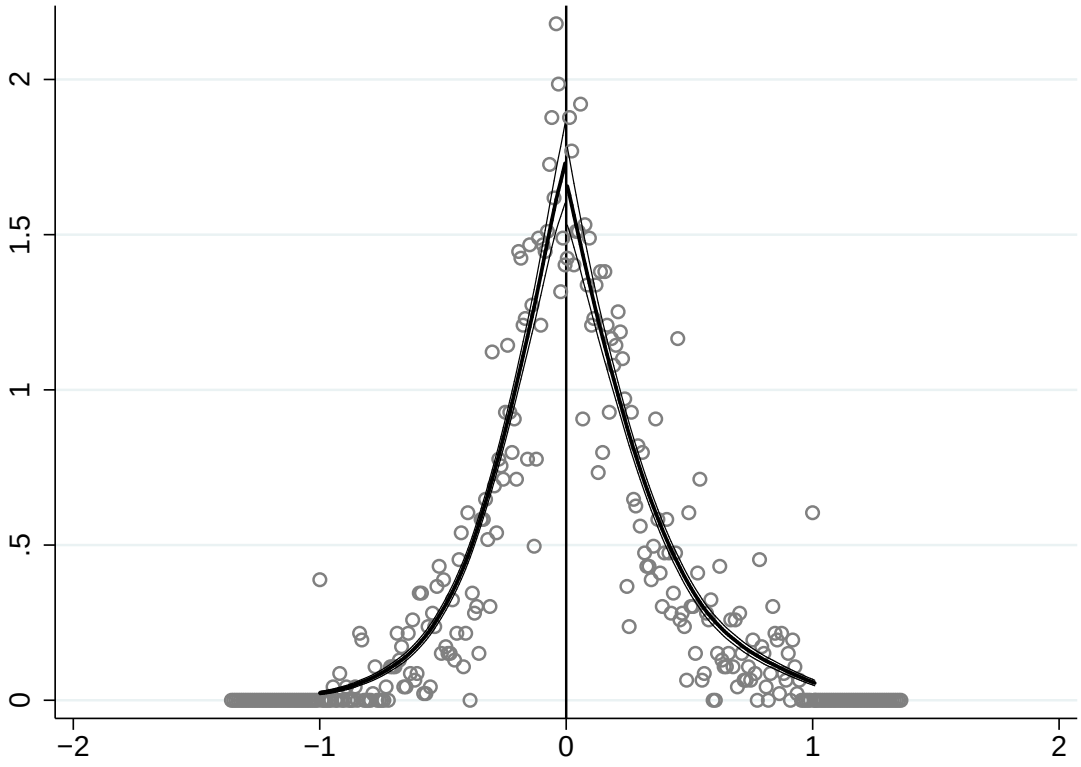
Notes: This figure plots the cumulative distribution of placebo estimates for the income tax rate at the first (panel a) and nineth decile (panel b). The placebo estimates are obtained by estimating equation 2 at false thresholds between 400 and 900 as well as 1,100 and 1,600. The effect at the true threshold is indicated by the vertical line.

Table D.3: Mixed election disconuinity: covariate balancing

	(1) mayor: female	(2) mayor: age	(3) mayor: political	(4) mayor: term limit	(5) runner-up: female	(6) runner-up: age	(7) runner-up: political	(8) years to election
RD estimate	0.187***	-2.602	-0.001	-0.071	-0.024	3.120	0.036	0.020
	(0.066)	(1.872)	(0.058)	(0.060)	(0.075)	(2.669)	(0.070)	(0.053)
mean	0.131	51.227	0.130	0.155	0.128	52.016	0.182	1.977
bandwidth	0.15	0.13	0.14	0.13	0.10	0.11	0.15	0.15
N	2,223	2,001	2,010	2,001	1,668	1,740	2,063	2,229
	(9) share: college	(10) share: female	(11) share: age $\geq$ 60	(12) log area	(13) coast dummy	(14) altitude (in m)	(15) top income share	(16) log taxable income per capita
RD estimate	0.003	-0.002	-0.007	0.043	0.011	-15.828	0.013	-0.008
	(0.004)	(0.003)	(0.013)	(0.158)	(0.037)	(49.379)	(0.014)	(0.047)
mean	0.069	0.507	0.321	2.886	0.018	478.622	0.069	9.306
bandwidth	0.12	0.11	0.13	0.12	0.12	0.16	0.14	0.14
N	1,892	1,750	2,001	1,834	1,852	2,375	2,027	2,108

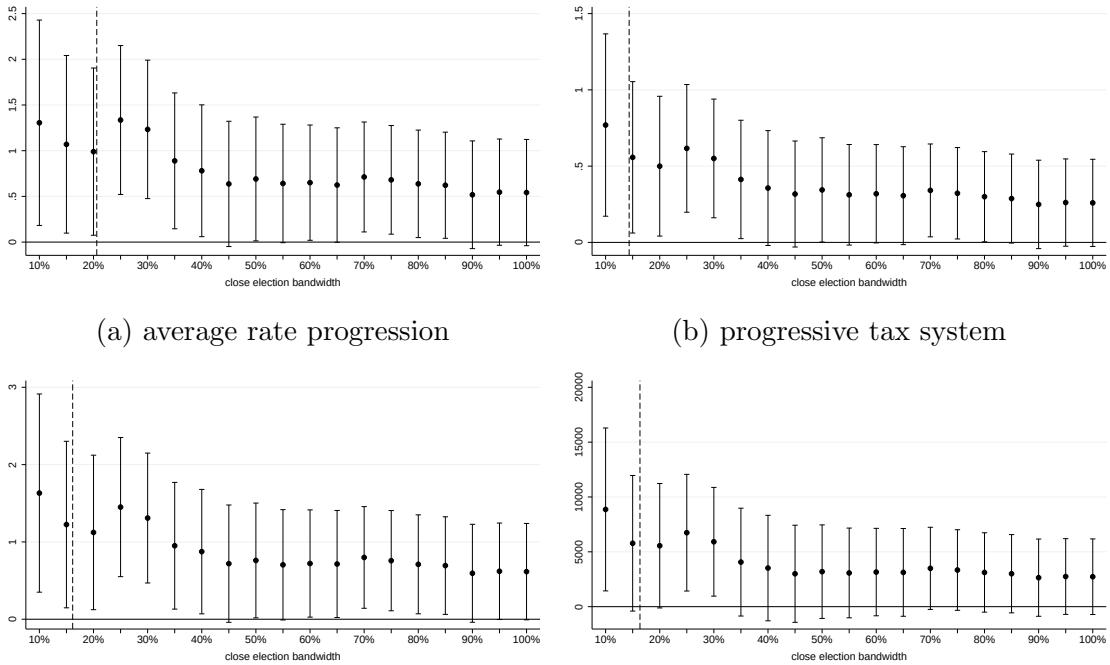
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; This table displays regression discontinuity estimates using the STATA command *rdrobust* in a mixed election regression discontinuity design for the whole sample period (Calonico et al., 2017).

Figure D.8: McCrary test for mixed elections between college- and non-college-educated candidates



Notes: This figure presents the McCrary density plot for close elections between college- and non-college-educated candidates.

Figure D.9: Close election RD: estimates by bandwidth



(a) average rate progression

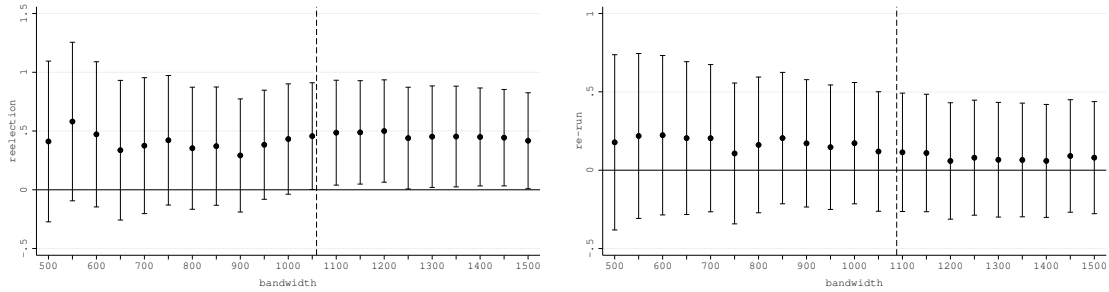
(b) progressive tax system

(c) marginal rate progression

(d) exemption level

Notes: This figure plots the local average treatment interaction effect for the mayor having a college degree (LATE x college degree) and its 95% confidence bands reported in Tables 5 (Panel a), C.6 (Panel b), C.8 (Panel c) and C.7 (Panel d) for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure D.10: Estimates by bandwidth: political outcomes

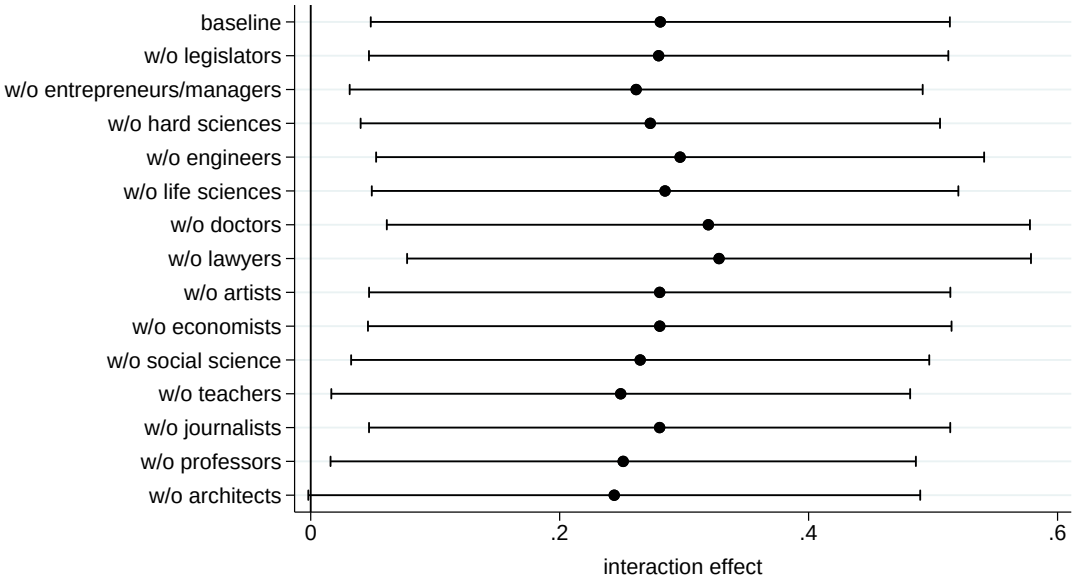


(a) reelection

(b) re-run

Notes: This figure plots the local average treatment interaction effect for the mayor having a college degree (LATE x college degree) and its 95% confidence bands reported in Table 6 for different bandwidths. The dashed vertical line refers to the optimal bandwidth.

Figure D.11: Average progression rate interaction effect: dropping job categories



Notes: This figure plots the heterogeneous average treatment effects for college-educated mayors dropping one job category at a time.

Table D.4: Municipal budget accounts: placebo regressions

	(1) income tax revenues	(2) property tax revenues	(3) trash tax revenues	(4) non-tax revenues	(5) transfer revenues	(6) loan revenues
placebo in 2008	0.49	5.42	-3.01	-20.09	-4.23	7.35
	(1.57)	(9.45)	(3.93)	(19.04)	(80.50)	(26.08)
placebo in 2009	1.20	-1.54	-6.04	-24.38	37.52	6.43
	(1.36)	(8.26)	(3.79)	(20.60)	(66.85)	(25.17)
placebo in 2010	-0.34	-0.56	-7.93*	-28.42	45.86	9.97
	(1.25)	(7.77)	(4.05)	(20.92)	(67.41)	(23.42)
placebo in 2011	0.53	3.37	-8.56**	-19.42	-39.05	-10.07
	(1.28)	(8.09)	(4.31)	(21.88)	(66.91)	(23.06)
placebo in 2012	2.51	12.62	-3.93	-14.18	-32.32	-17.42
	(1.57)	(9.54)	(4.09)	(24.05)	(76.90)	(26.41)
mean	32.64	182.74	112.06	383.67	887.94	159.74
bandwidth	682	574	566	495	562	581
N	12,440	10,633	10,503	9,319	10,440	10,757

	(7) other revenues	(8) total expenditures	(9) capital expenditures	(10) current expenditures	(11) deficit
placebo in 2008	-5.00	68.86	15.99	50.35	-13.81
	(14.26)	(68.45)	(46.74)	(29.51)	(10.67)
placebo in 2009	0.80	4.05	-11.83	15.61	-8.07
	(13.18)	(66.38)	(49.27)	(25.06)	(7.50)
placebo in 2010	7.22	53.77	17.43	23.39	-4.04
	(11.87)	(70.00)	(51.36)	(25.62)	(7.02)
placebo in 2011	24.51*	-32.43	-57.67	19.81	-2.69
	(13.95)	(73.29)	(56.43)	(23.72)	(7.66)
placebo in 2012	4.18	-22.16	-63.45	34.29	3.16
	(13.59)	(78.65)	(62.00)	(23.45)	(10.11)
mean	75.21	1307.39	501.62	784.42	19.49
bandwidth	616	515	563	473	666
N	11,349	12,258	12,258	12,258	12,339

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; This table displays placebo effects using equation 2. These are obtained by restricting the sample to pre-reform years, assigning the reform to a different year and finally re-estimating equation 2. In the bottom panel, the sample mean of the outcome variable, the used bandwidth and the number of observations are shown. All revenue, expenditure, and deficit variables are expressed in per capita terms and 2015 Euros.