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Austerity and distributional policy☆

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ABSTRACT

How does fiscal austerity affect redistributive policies? We document that during austerity episodes, countries tend to increase marginal income tax rates on top earners, but not on average earners. We then show that, in response to an exogenously imposed fiscal rule, Italian municipalities increase local non-linear income taxes progressively. They do not adjust other fiscal policies. College-educated mayors are more likely than less educated mayors to implement progressive reforms, and they perform better in the upcoming election. Survey evidence suggests that the differential policy response can be explained by college-educated mayors being more informed about the available policy options.

1. Introduction

In response to crises, such as the global financial crisis of 2007-09 or the Covid-19 pandemic, governments typically enact large fiscal stimulus packages, and this often leads to the need for austerity measures in subsequent years. A large academic and policy literature has debated the effects of fiscal adjustment reforms on output (Alesina et al., 2019a; House et al., 2020). The distributional consequences of austerity have also been recently discussed, but the evidence is mostly anecdotal in nature (see, e.g., Varoufakis, 2016).

In this paper, we study the effects of austerity on distributional policy, and strive to understand how the political process shapes distributive policies in equilibrium. We start by investigating for a panel of countries the relationship between the cyclically adjusted primary balance, which we use as a measure of austerity, and statutory personal income tax rates. Figure 1 shows that, conditional on country and year fixed effects, a 1% increase in the cyclically adjusted primary balance is

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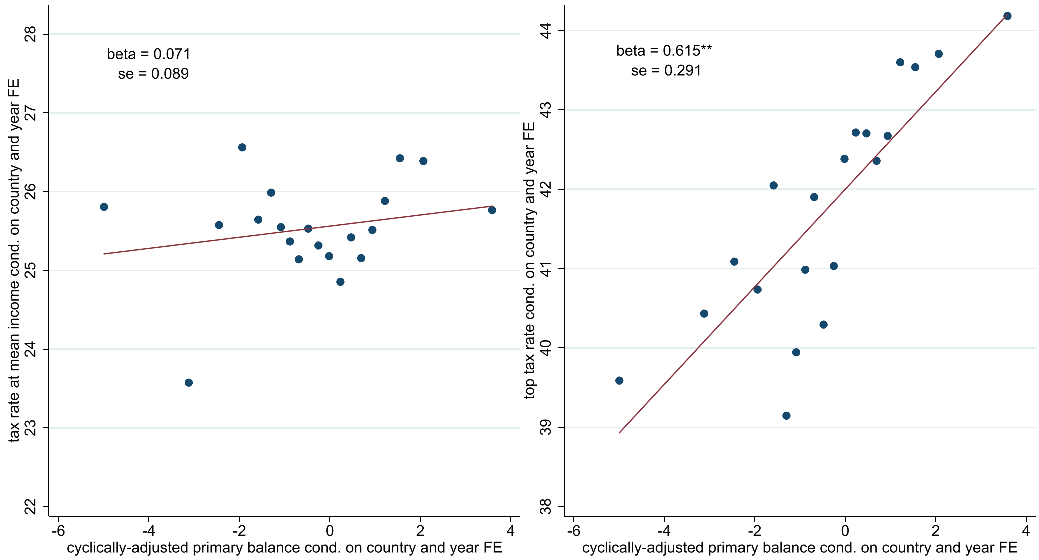


Fig. 1. Fiscal austerity and tax rates at mean and top incomes. Notes: This graph shows estimates from equation (A.1) as described in Appendix A using the tax rate at mean incomes and the top tax rate as outcomes. The sample and data are described in Appendix A.

associated with a 0.6 percentage point increase in the marginal income tax rate at the top, while we observe no correlation between austerity and marginal income tax rates at mean levels of income. This suggests that austerity is associated with stronger, not weaker distributive tax policies.[1]

Of course, it is hard to draw definitive conclusions from these correlations. Therefore, we exploit data from local governments to provide the first quasi-experimental evidence on the effect of fiscal austerity on distributional policy, as well as to explore how the local democratic process leads to the adoption of distributive policies. We study a large exogenous reduction in the fiscal space of Italian municipalities due to the imposition of a fiscal rule by the national government. 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, which allows us to implement a difference-in-discontinuity design. Italy is well-suited for the study of our research question due to the substantial autonomy that Italian municipalities have in setting local non-linear income taxes.

We find that local governments responded to the introduction of the fiscal rule by increasing tax rates progressively. The increase in tax rates is larger for higher incomes and only becomes significant for taxpayers located above the median taxable income. The relative effects are quite sizeable, with 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.[2] This effect is partially attributable to municipalities switching to a progressive schedule in tax rates, and partially to increases in the exemption threshold for low earners. We also consider other local policy instruments that can have distributive effects and which could potentially be employed as austerity measures in addition to the income tax. However, we find no evidence that the reform affects other tax or non-tax revenues, including property taxes, nor does it affect redistributive or total spending.

We interpret our findings as the impact of austerity on distributional policy. Considering the introduction of the DSP as austerity is natural because it required a fiscal adjustment in municipalities where the rule was binding. Consistent with this interpretation, previous evidence shows that the DSP induced substantial fiscal consolidation (Coviello et al., 2021; Grembi et al., 2016). Contextual details concerning Italy's economic situation at the time further bolster our interpretation. The reform took place in the midst of a severe recession caused by the sovereign debt crisis, with Italian GDP shrinking by 3% in 2012 and by 1.8% in 2013, while the central government cut transfers to municipalities on several occasions between 2009 and 2015 (Marattin et al., 2021). The DSP, vertically imposed by the national government upon municipalities, became a symbol of austerity in the eyes of local administrators and was grossly unpopular among mayors across the political spectrum (see Appendix B.2 for anecdotal evidence).

To shed light on the mechanisms underlying our findings, we study whether mayors, the crucial decision-makers at the local level, responded to austerity in a heterogeneous manner. We find that the increase in tax progressivity is driven by

[1] We show in Appendix A that a similar relationship holds when using the narrative measures of austerity provided by Alesina et al. (2019b) on a smaller set of countries.

[2] In Appendix E, we show that municipalities are not constrained by the efficiency cost of raising income tax rates as they were clearly below the pareto bounds of Bierbrauer et al. (2021) across the income distribution. Therefore, municipalities could have potentially increased revenues by either a flat or a progressive tax reform.

mayors with a college degree, while other observable characteristics, such as age, gender, or party affiliation of mayors, do not play a meaningful role. By contrast, mayors without a college degree rely on flat increases in the local income tax to comply with the reform. To address the potential issue of selection of mayors by education, we compare the outcomes for the two types of mayors elected in close races and find similar results.

According to the median voter models of Meltzer and Richard (1981) and Bierbrauer et al. (2021) for linear and non-linear taxes, respectively, the different adjustment strategies between the two groups of mayors should lead to different electoral outcomes. Given that college-educated politicians increased the income tax for a smaller share of the electorate, they should be electorally more successful than mayors without a college degree who raised taxes on everybody. We test this hypothesis by estimating the effect of the fiscal rule on the reelection probability of mayors. We show that mayors with and without a college degree exhibit significant differences in their electoral outcomes. In the first election following the imposition of the fiscal rule, incumbents without a college degree were substantially less likely to be reelected, whereas college-educated mayors did not experience a significant decline in their reelection odds. One caveat is that we do not have an experiment to causally identify the effect of flat versus progressive tax reforms on the electoral success of the mayors who enacted them. There could be reasons other than the differential use of progressive taxation that could explain the different political outcome. However, we show that the difference in the reelection odds of mayors with and without a college degree only manifests itself following the reform. We also do not find differences between mayors with and without a college degree in other important policy instruments, such as other taxes or expenditures, that could potentially be relevant for reelection odds. There might be differences in other variables that we cannot observe, so we cannot fully exclude all alternative explanations. Still, our preferred interpretation is that our results are consistent with the hypothesis that austerity enacted through flat tax reforms is politically more costly than austerity enacted through progressive tax reforms.

If this interpretation is correct, it leads to a puzzle. Why do less educated mayors implement electorally costly flat tax reforms in response to the need for austerity if they could, in principle, follow college-educated mayors and implement electorally more favorable progressive tax reforms? To study the reasons behind the differential behavior of college-educated and less educated mayors, we fielded a brief online survey of Italian mayors.[3] The survey data do not support the notion that the two types of mayors have different equity-efficiency preferences, different beliefs about the political effect of local taxation, nor do they indicate different administrative abilities with respect to income tax reforms. We do find evidence for knowledge gaps between mayors with and without college degrees. In particular, our evidence suggests that less educated mayors are more likely to be unaware that the local income tax can be designed to have a progressive schedule than college-educated mayors. This lack of information among less educated mayors is consistent with their tax reform choices being politically sub-optimal.

Our paper contributes to three main strands of literature. First, our paper is related to the literature on the economic effects of fiscal adjustments. This literature has mainly focused on the efficiency costs of fiscal policies implemented by national (Ilzetzki et al., 2013; Ramey, 2011) and sub-national governments (Chodorow-Reich, 2019; Nakamura and Steinsson, 2014). It distinguishes between tax and expenditure adjustments (Alesina et al., 2019b) and between episodes of austerity and stimulus (Barnichon et al., 2022).

By contrast, we focus on the distributional effects of fiscal adjustments and provide the first quasi-experimental evaluation of this effect. Previous quantitative work on this question uses cross-country data and mostly focuses on policy responses to the global financial crisis. This work finds that periods of fiscal austerity are associated with an increase in income inequality (Heimberger, 2018). Microsimulations for several European countries present a more nuanced picture on the distributive effects of austerity that depend on country contexts and measures of austerity (Paulus et al., 2016).

Second, we contribute to the literature studying the political economy of taxation by providing an empirical counterpart to results that by and large remain theoretically grounded. While 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.[4] In particular, our baseline result – namely that, upon an exogenous shock governments increase the income tax disproportionally on taxpayers above the median earner – is consistent with Bierbrauer et al. (2021), who characterize the conditions of politically feasible non-linear tax reforms.

Third, our findings relate 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 consolidation at the national (Alesina et al., 2012; Brender and Drazen, 2008) or local level (Carreri and Martinez, 2021).[5] On the other hand, a number of papers show that fiscal austerity has negative effects on voter support for the incumbent (e.g. Hübscher et al., 2021) as well as on broader socio-political outcomes, such as increasing support for right-wing populism

[3] Note that we survey current mayors who are most likely not the same mayors as in our main analysis as it is conducted nine years after the reform. Furthermore, the survey has a response rate of 6% and this sub sample, despite being similar to the overall population along several observable characteristics, could still be selected on characteristics unobservable to us.

[4] While we focus on the political economy drivers of non-linear taxes, of course economic factors also drive them. Heathcote et al. (2017) studies what shapes the optimal degree of progressivity. Wu (2021) shows that economic changes can explain about 60% of the reduction in progressivity in the US, while the rest can be explained by shifts in the government's social welfare function.

[5] Possible explanations include 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., 2021), or that the divergent framing of the same issue provided by partisan media mitigates voter responses (Barnes and Hicks, 2018).

(Dal Bó et al., 2018; Fetzer, 2019). Our results are consistent with recent cross-country evidence from Furceri et al. (2021), who show that tax-based austerity can induce significant electoral costs. We contribute to this literature by showing that austerity can carry significant electoral costs, but that these costs depend on the distributional nature of the consolidation strategy. In particular, we show that electoral costs can be potentially mitigated by increasing taxes on high-income earners.

2. Institutional setup

This section describes our institutional setup. We discuss the fiscal rule that we exploit as an exogenous austerity shock, the municipal fiscal policies with special emphasis on potentially redistributive policies, and the institutions governing local politics.

2.1. Municipal fiscal rule

In 1999, the Italian central government introduced a fiscal rule, the Domestic Stability Pact (DSP), which initially applied to all municipalities.[6] In 2001, municipalities with less than 5000 inhabitants were exempted from this rule (Grembi et al., 2016). In 2013, another reform lowered the population threshold to 1000, such that municipalities between 1000 and 5000 inhabitants were also subject to the rule. This is the reform that we exploit. Finally, in 2016, the DSP was abolished and a balanced budget rule for all municipalities was introduced.

In our period of analysis, the target of the DSP was the difference between expenditures and revenues, net of repayment of outstanding debt and lending. The exact formula varied over the years, but it was always defined as a function of budget items in previous years (see Appendix Table C.1). Regulations by the central government that monitor the compliance to the fiscal rule are generally quite strict. These were tightened in 2008 with the launch of a compulsory reporting system and the introduction of severe penalties for non-compliers (Coviello et al., 2021). Potential punitive measures included bans on hiring, reductions of transfers from the central government, and salary cuts to mayors and city councilors. The strict enforcement rules suggest that it is very likely that Italian local governments complied with the DSP.[7]

2.2. Municipal fiscal policy

The municipal budget is financed with transfers from higher levels of government and international institutions, and by municipalities' own resources such as local taxes and fees from the use of public services. Local taxation accounts for 21% of total revenues in our sample period (see Appendix Figure C.1). The three largest tax instruments are property taxes, waste taxes and local income taxes. These accounted for 8.7%, 7.9% and 4.4% of total revenues in 2015, respectively. Our main focus is the local income tax. Since the tax explicitly allows for different levels of marginal tax rates on different levels of income, its distributional implications are straightforward. The property tax and waste tax could potentially also have distributional effects, but these are more complicated to measure and to analyze. Furthermore, the property tax rate was significantly reformed 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.[8]

The local income tax was introduced in 1999 as a municipal surcharge on the national income tax. The tax base includes individual income from several sources, and wages and pensions make about 80% of the tax base. Individuals are taxes according to the residency principle. Tax revenues directly flow to the municipal budget. The income brackets are set by the central government, and in our sample period, these were set at € 15000, € 28000, € 55000, and € 75000. The national marginal tax rates within these brackets were, respectively, 21%, 27%, 38%, 41% and 43%.

Initially, municipalities were allowed to only apply uniform tax rates of up to 0.5% of taxable income on top of the national tax rates. In 2007, the cap was raised to 0.8% and municipalities were given the autonomy to set their own exemption threshold. Taxpayers with incomes below the threshold were fully exempted, while those above would pay the tax on their total taxable income. In 2011, municipalities were also allowed to set differentiated tax rates within the brackets of the national income tax schedule. Therefore, since 2007 municipalities have been able to levy non-linear income taxes. The tax rates and the exemption level can be adjusted every year and about 27% of municipalities change their tax code in a given year. The gradual increase in flexibility of this tax instrument was coupled with technical assistance. Since 2011, municipal officials have had access to a tax simulator, which uses data from the tax administration to simulate how revenues would respond to changes in tax rates and in the exemption threshold. Users can set parameters on tax revenues, tax rates and/or the exemption threshold. This allows mayors to target specific income groups while fulfilling a revenue target.

The adoption of differentiated tax rates by municipalities has increased over time. In 2007, 33% of municipalities with less than 2500 inhabitants did not have any local income taxes and 67% had flat taxes without any exemptions, thus none

[6] The main goal of fiscal rules is to achieve fiscal sustainability. 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. A meta-study by Heinemann et al. (2018) finds that numerical fiscal rules constrain fiscal policy, but results become less clear when accounting for endogeneity.

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

[8] We test the effects of the DSP on property and waste taxes in Appendix F and find null results. That section also presents additional institutional details on these taxes, while Messina and Savegnago (2014) and Messina et al. (2018) present further information on their potential distributional consequences.

of these small municipalities operated a non-linear tax schedule. In 2015, 24% of these municipalities did not have any local income taxes, 56% had a flat tax without exemptions, 12% had a flat tax with exemptions, and 8% had a schedule with five different tax rates either with or without exemptions. Conditional on having an exemption threshold, the average threshold is about € 10000, but there is considerable variation around the mean (see Appendix Figure C.2).

On the spending side, municipalities account for about 8% of total public expenditures. They are responsible for providing a variety of public services, including administrative services, public transport and road maintenance, utility services, social services, education, and policing. We focus on social and educational spending, given their potential redistributive nature. Social spending includes, among other things, assistance to poor people, child care, and care for the elderly. Education expenditures at the municipal level include spending for pre-school and primary school services, such as cafeterias and school buses. In our sample period, municipalities were allowed to take out loans only to finance new investments, with some limits to the overall interest payments.

2.3. Municipal politics

Municipal governments are composed of the mayor, who proposes the annual budget, the city council, which must approve it, and the executive committee. Municipalities with less than 15,000 inhabitants hold single round direct elections for the mayor. Voters cast one vote for a mayoral candidate, and express one preference vote for one council candidate within the list that is affiliated with the same mayoral candidate. The mayoral candidate who gets the most votes is elected. Two thirds of the seats in the council are then allocated to the list of the elected mayor, who can appoint and remove members of the executive committee. The remaining seats are split across the other lists proportional to their vote shares. Each term is five years long, and the mayor cannot serve for more than two consecutive terms.[9] 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 in most cases it is a so-called civic list. Also, since being a politician in a small town is not a full-time job, mayors often keep their former job while being in office.

There is ample descriptive evidence for the salience of the local income tax in municipal politics. For example, Giommoni (2019) shows that web searches from Google Trends for the term “municipal surcharge” exhibit strong seasonality, with peaks in June, when the tax is due, and in January, right after the tax rate is set or updated. This is consistent with the fact that the amount paid due to the local income tax is clearly visible on the monthly payslips received by employees and retirees. Also, municipal tax rates can easily be looked up at the website of the Ministry of Finance and are featured in local media. Additionally, we gathered anecdotal evidence from newspapers, social media, electoral platforms and councils' minutes suggesting that incumbent mayors often refer to the local income surcharge and to its degree of progressivity when campaigning for office (see Appendix B.1 for the exact quotes). In these statements, mayors justify raising the exemption threshold as well as the tax rates on high incomes by emphasizing that these reforms increase progressivity, are fair, and help disadvantaged people with little cost for others. There are also instances of opposition politicians criticize incumbents for implementing a flat tax instead of progressive schedule.

3. Data

We assemble a rich data set at the municipality–year level from several official sources. This section describes the most important variables. Appendix C.2 presents the sources, construction and further details about our data.

To capture the distributional effects of the local income tax, we employ several outcome variables. First, we use the tax rates applying to the income deciles of the municipal income distribution. Second, we use a binary indicator of whether a given municipality has a progressive tax system, either through an exemption threshold or through increasing tax rates. Third, we study the level of the exemption threshold. Fourth, we construct two comprehensive measure of progressivity, the average and marginal rate progression, following Peter et al. (2010). We construct these variables by running the following regression for each municipality–year pair (i, t) separately:

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

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 a regressive, zero for a flat, and positive for a progressive tax schedule.

We use detailed accounts of municipal revenues and expenditures from the Italian Ministry of the Interior. Expenditures are split between capital and current spending, and are further disaggregated in broad categories. Revenues are also disaggregated by their source. Additionally, we have data on taxable income, the number of taxpayers, and income tax revenues within income brackets. Last, we use the officially defined deficit, which is the difference between revenues and expenditures plus the difference between revenue and expenditure carry-overs from preceding years. We collect data on local elections and elected politicians as well as runner-up candidates. This allows us to construct both a rerun and reelection

[9] This was extended to three terms in 2014 for municipalities below 3000 inhabitants.

dummy for incumbents. We have information on the vote share, party affiliation, term limit, age, gender, and education level for both the mayor and the runner-up.

We start our sample in 2007, the first year municipalities were allowed to levy non-linear income taxes, and end it in 2015, since after that all municipalities were subject to the fiscal rule. We apply a number of restrictions to our sample. First, we drop all municipalities with less than 1000 inhabitants that are part of an inter-municipal cooperation union, as these municipalities are subject to the fiscal rule irrespective of their population. Second, following Grembi et al. (2016), we exclude the five autonomous regions (Val d'Aosta, Friuli-Venezia Giulia, Trentino-Alto Adige, Sardinia and Sicily), as they were allowed to change both the set of municipalities covered by the rule and the numerical details of the fiscal rule.[10] Third, we drop all municipalities that merged within the sample period. Our empirical strategy relies on comparing municipalities that are above and below the 1000 inhabitant threshold. Our baseline sample excludes municipalities with more than 2500 inhabitants. Appendix Figure C.3 shows a map of municipalities in our sample, distinguishing between municipalities below 1000 inhabitants in blue and those between 1000 and 2500 in red. Appendix Table C.3 shows summary statistics for the whole sample as well as for municipalities below and above 2500 inhabitants.

4. Empirical strategy

Our empirical strategy exploits the 2013 extension of the fiscal rule to municipalities that were previously exempted. Until 2012 the rule applied to municipalities with at least 5000 inhabitants, and it was extended to those with at least 1000 inhabitants afterward. We cannot compare municipalities around the 1000 threshold in a standard regression discontinuity design due to the presence of other policies, such as for example the mayor's salary, which changes discontinuously at this same cutoff (see Appendix Table C.2).

Instead, we employ a difference-in-discontinuity design, which requires that the confounding policies at the threshold are time-constant over the sample period (Grembi et al., 2016). This requirement holds in our setting. The intuition behind this empirical strategy is that any effects from a change in the confounding policy can be netted out by taking the difference between regression discontinuity estimates from before and after the reform. In practice, this strategy amounts to a difference-in-differences design evaluated at the 1000 inhabitants threshold.[11]

Let Y_{it} be an outcome in municipality i at time t and $\tilde{p}_{it} = p_{it} - 1000$ its normalized population. Since treatment status is based on the population of the preceding year, $\tilde{p}_{it-1}$ is our forcing variable. We implement the difference-in-discontinuity design using local linear regression and estimate the following equation:

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

where $Treat_{it}$ takes the value of one if the lagged population of municipality i is larger or equal to 1000 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).[12] Standard errors are clustered at the level of municipalities to account for serial correlation. The effect of the fiscal rule is identified by β_6.

The difference-in-discontinuity estimator identifies the effect of the 2013 reform on municipalities around the threshold if the following two identifying assumptions are met. First, other confounding variables can change discontinuously at the 1000 threshold, but we must assume that the change is time-constant. We test this assumption of local parallel trends by estimating yearly effects by replacing $Reform_t$ with yearly dummies. Second, we must assume that municipalities do not manipulate their population counts to escape the fiscal rule. We test this assumption using McCrary density tests conducted separately before and after the reform (McCrary, 2008), as well as with a density test of the change in density around the reform year (Asatryan et al., 2017; Grembi et al., 2016).

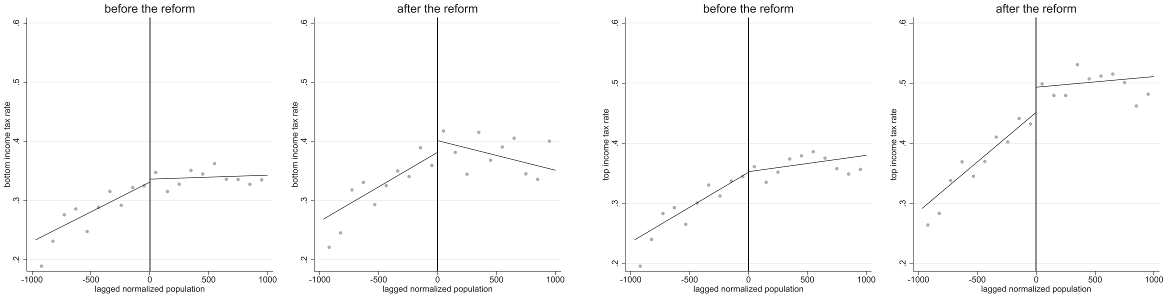
4.1. Heterogeneous effects

We also study the heterogeneous effects of the fiscal rule with respect to mayoral characteristics, in particular their education. Appendix C.3 presents a detailed description of our empirical strategy for estimating these heterogeneous effects. The intuition is to interact every term in Eq. (2) with a dummy of whether the mayor has a college degree or not. In this specification, we also include dummies indicating whether the mayor has a college degree, is female, is backed by a left-wing, right-wing or centrist party, is allowed to run again as well as her age and win margin in the last election, the number of years to the next election, the top income share and the pre-reform deficit of the municipality as control variables. Additionally, we add municipality fixed effects as well as interactions with the above-mentioned control variables.

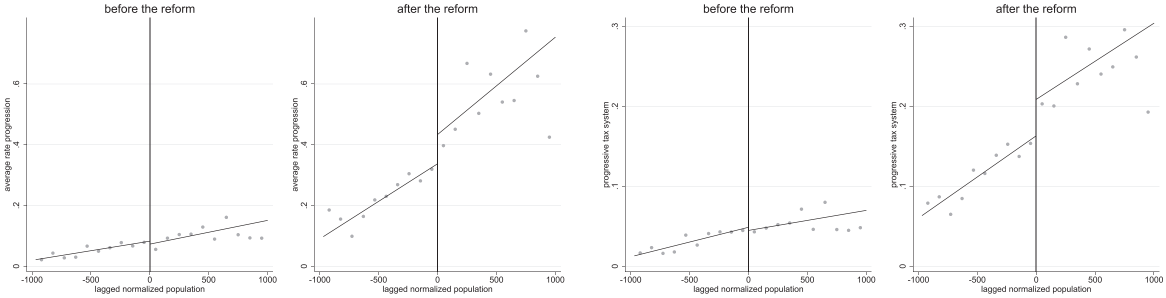
[10] We include the autonomous regions in a robustness check.

[11] We do not evaluate the change of the 5000 inhabitants threshold, since there is a *simultaneous* policy change in the same year (see Appendix Table C.2). More specifically, a gender quota in local elections was introduced for municipalities with 5000 or more inhabitants in 2013. Furthermore, the two groups of municipalities are not as comparable, since those with 5000 or more inhabitants were subject to the rule before the reform, whereas those below were not.

[12] We conduct a standard regression discontinuity design before and after the reform and take the average of the two optimal bandwidths. Since the results of local linear regressions may be sensitive to the choice of the bandwidth, we also estimate results obtained with different bandwidths.



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



(c) average rate progression (d) progressive tax

Fig. 2. Regression discontinuity plots: tax progressivity before and after the reform. *Notes:* The reform is the introduction of the fiscal rule for municipalities above 1000 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* 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.

A causal interpretation of the differential effect of the reform by mayoral education is not straightforward, because we cannot exclude the possibility of unobserved time-varying confounding variables determining both the mayor's education and our outcomes. For example, if municipalities whose population has a higher preference for redistribution tend to elect more educated mayors, then we would erroneously attribute any differential effect to mayoral quality rather than to the underlying population's preferences.

For this reason, we exploit close elections to leverage an exogenous source of variation in the mayor's education. To do so, we focus on races in where the winning candidate and the runner-up have different levels of education. In this sub-sample, having a college-educated mayor is a discontinuous function of the difference between the vote share for the college-educated candidate and the vote share of the less educated candidate. The college-educated candidate won (lost) the election to the less educated candidate if the vote share variable is positive (negative). Therefore, we use the margin of victory as our second running variable, and interact it with every term in the specification aimed at estimating the heterogeneous effects. Therefore, our identifying variation stems from comparing municipalities in which the college-educated candidate barely won, to those in which she barely lost. In this specification, we control for the same variables mentioned above as well as municipality fixed effects. This augmented specification allows to effectively control for unobserved confounders that could possibly drive both the mayor's educational level and tax policies.

5. Results

In this section, we first present the baseline results of the difference-in-discontinuity analysis on our main outcomes. Next, we verify that our identifying assumptions are met and conduct further robustness checks. Last, we extend the external validity of our results to municipalities far away from the population threshold under study.

5.1. Baseline results

We start by presenting graphical evidence of our results. Figure 2 shows standard regression discontinuity graphs estimated separately on the pre- and post-reform samples for several measures of the progressivity of the local income tax. As outcome variables, we take the average tax rate at the first and at the ninth income deciles, the average rate progression, and the dummy for having a progressive tax or not. Each graph plots local means of the outcome variable in ten normalized

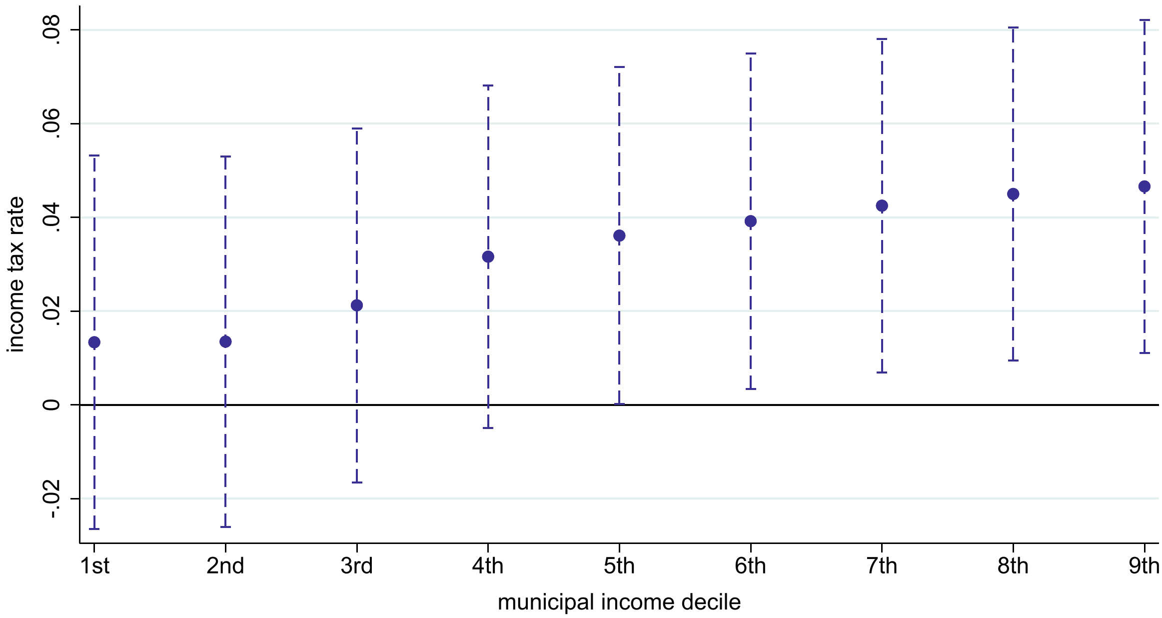


Fig. 3. Effect of the reform on the income tax rate at different income deciles. *Notes:* The reform is the introduction of the fiscal rule for municipalities above 1000 inhabitants in 2013. The figure plots the local average treatment effects also reported in Table D.1 and their 95% confidence bands. The estimates correspond to β_6 in Eq. (2). The bandwidth is selected following Grembi et al. (2016). The deciles refer to the income distribution in each municipality.

population bins on each side of the threshold, and a linear fit of the data estimated separately on each side of the threshold. Before the reform, there is no visible jump at the threshold for any of the four outcome variables. After the reform, we observe a small jump in the average tax rate at the first decile, and a more sizable one for the tax rate at the ninth decile. The average rate progression and probability of having a progressive tax system also increase discontinuously at the threshold after the reform, but show no difference in the pre-reform years. This preliminary graphical evidence suggests that the reform induced a disproportionate increase in the tax for higher incomes.

Next, we turn to the estimates obtained from the difference-in-discontinuity specification. First, we estimate the difference-in-discontinuity design using our four measures of progressivity. Table 1 shows that the reform induces an increase in the average (marginal) rate progression of 0.14 (0.16) standard deviations, corresponding to an increase of 80% (86%) compared to the sample mean. The reform also increases the probability of adopting a progressive tax system by six percentage points. This large increase in progressivity is partly driven by the effect on the exemption level, which increases by € 600 or approximately by 67% of the sample mean.

Furthermore, Fig. 3 plots the estimated effect of the reform on the average tax rates at all deciles of the municipal income distribution. We find that all point estimates are positive. This is consistent with the interpretation that municipalities raise local income taxes to comply with the fiscal rule. The size of the point estimates is monotonically increasing along the municipal income distribution: The estimated effect on the top tax rate translates 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 Eq. (2) for all nine tax rates jointly, with seemingly unrelated regressions (SUR).[13] We then implement several one-sided Wald tests with a null hypothesis that the effect on higher incomes is not larger than the effect on lower incomes. We present the p-values of all these tests in Appendix Table D.1. Overall, we can reject the null hypothesis at the 5% or 10% level for all comparisons between top and bottom tax rates.

We also estimate the effects of the fiscal rule on tax revenues collected from each income brackets in terms of per-taxpayer values as well as in aggregate terms. In line with the main result that income tax rate adjustments were progressive, these results suggest that individuals from upper tax brackets contribute more to the extra revenues generated by the reform (see Table 2). The average tax increase for a taxpayer in the top income bracket (above € 120000) 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 € 15000 to € 26000 bracket. In general, the additional tax revenues per taxpayer are strictly increasing in taxable income. Since few taxpayers have large taxable incomes, more than half of the extra revenue is collected from taxpayers with taxable income between € 15000 and € 55000. Our findings suggest that individuals with taxable incomes below € 10000, about 38% of all

[13] We use SUR because the tax rates along the income distribution are jointly determined by the municipal government, and thus cannot be considered as independent outcome variables.

Table 1
Effect of the reform on progressivity measures.

	(1) average rate progression	(2) progressive tax	(3) marginal rate progression	(4) exemption level
Reform x Treat	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	17775	17319	17775	16955

Notes: The reform is the introduction of the fiscal rule for municipalities above 1000 inhabitants in 2013. The table reports β_6 from Eq. (2) estimated with a separate local linear regression for each outcome variable. 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2
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							
Reform x Treat	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	17684	17444	17709	17587	16544	13163	19180
total tax revenues							
Reform x Treat	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	18550	17508	16776	17247	17408	16287	18279

Notes: The reform is the introduction of the fiscal rule for municipalities above 1000 inhabitants in 2013. The table reports β_6 from Eq. (2) estimated with a separate local linear regression for each outcome variable. 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 taxpayers 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

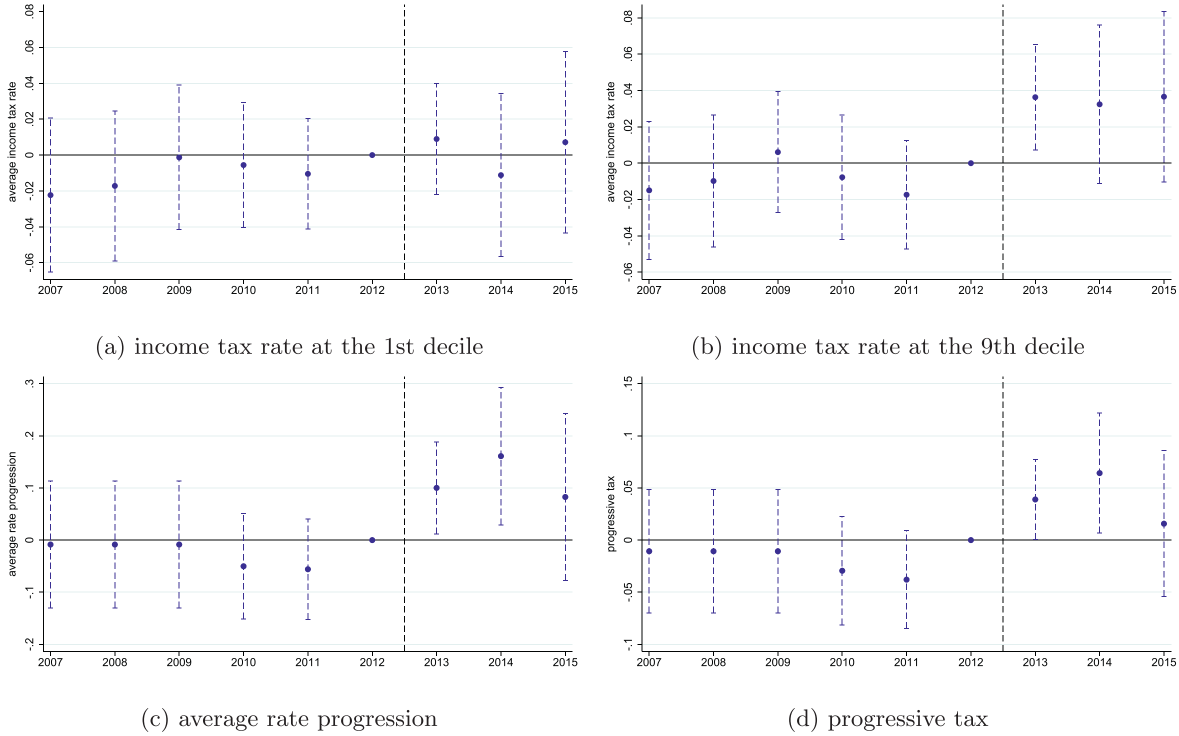
taxpayers, almost entirely escape the tax rate increase. This result is consistent with our previous finding of an increase in exemption levels.

Furthermore, we test the effects of the reform on the tax base, and we do not find evidence that total taxable income or the number of taxpayers changed (see Appendix Table D.2). Consistent with this, we also do not find any evidence for the reform inducing tax competition. Appendix Table D.3 shows that there was no effect on the average tax rates of neighboring municipalities. We also compute the upper pareto bounds proposed by Bierbrauer et al. (2021), showing that, even when assuming a large elasticity of taxable income of 1.25, the pre-reform tax systems were clearly to the left of the peak of the Laffer curve (see Appendix E for details). This suggests that municipalities could have raised additional tax revenues from low earners if they wanted to. Taken together, these findings suggest that mayors were able to raise additional income tax revenues without substantially hurting their tax base. Finally, we also test whether the introduction of the fiscal rule had an effect on any other local policy instrument. In Appendix F, we show that it did not significantly affect other taxes, including the property tax, overall spending, or individual redistributive spending categories. Last, we also show that the fiscal rule did significantly reduce municipal deficits.

5.2. Sensitivity checks

In this section, we discuss the validity of the two major identifying assumptions as described in Section 4, and we also perform a number of additional robustness tests.

First, the local parallel trends assumption states that any difference at the threshold other than the fiscal rule has to be time-constant. To test whether the local parallel trends assumption holds, we use a dynamic version of Eq. (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



(a) income tax rate at the 1st decile

(b) income tax rate at the 9th decile

(c) average rate progression

(d) progressive tax

Fig. 4. Dynamic effects of the reform. *Notes:* The reform is the introduction of the fiscal rule for municipalities above 1000 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.

trace the local trends before the reform and the dynamic effects after the reform. As Fig. 4 shows, there is no significant pre-treatment trend in the bottom tax rate, top tax rate, the average rate progression, or in the probability of a progressive tax system.[14] After the reform, there is an immediate significant increase in all variables aside from the bottom tax rate. As a further robustness check, we conduct placebo reforms in every pre-reform year of our sample.[15] Appendix Figure D.2 plots the results of the five placebo estimations as well as that of the baseline results. The results show null 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. Appendix Table D.4 shows that none of the 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 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 Grembi et al., 2016). We find evidence neither for of a significant jump in the density of observations at the 1000 population threshold before (see Appendix Figure D.3a) or after (see Appendix Figure D.3b) the reform, nor in the difference over time (see Appendix Figure D.4).

We conduct a number of additional robustness checks. First, our results are also robust to the selection of different bandwidths (see Appendix Figure D.5). Second, Appendix Table D.5 shows that global polynomial regressions yield very similar results to local linear regressions. Third, our results are robust to the inclusion or exclusion of all municipalities that are part of a municipal union as well as to the inclusion of region-time fixed effects or dropping one region at a time to account for regional clustering (see Appendix Table D.6 and Appendix Figure D.6). Fourth, our results are robust to the inclusion of the five autonomous regions, where the institutional setup is potentially different (see Appendix Table D.6). Fifth, we run permutation tests at placebo thresholds, between 400 and 900 inhabitants as well as between 1100 and 1600 inhabitants, as an alternative method to conduct inference. This exercise confirms our baseline findings (see Appendix Figure D.7). Last, we

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

[15] Specifically, we restrict our sample to the pre-reform period and re-estimate Eq. (2) with the $Reform_t$ dummy taking the value 1 from year t onward with $t \in \{2008, 2009, 2010, 2011, 2012\}$.

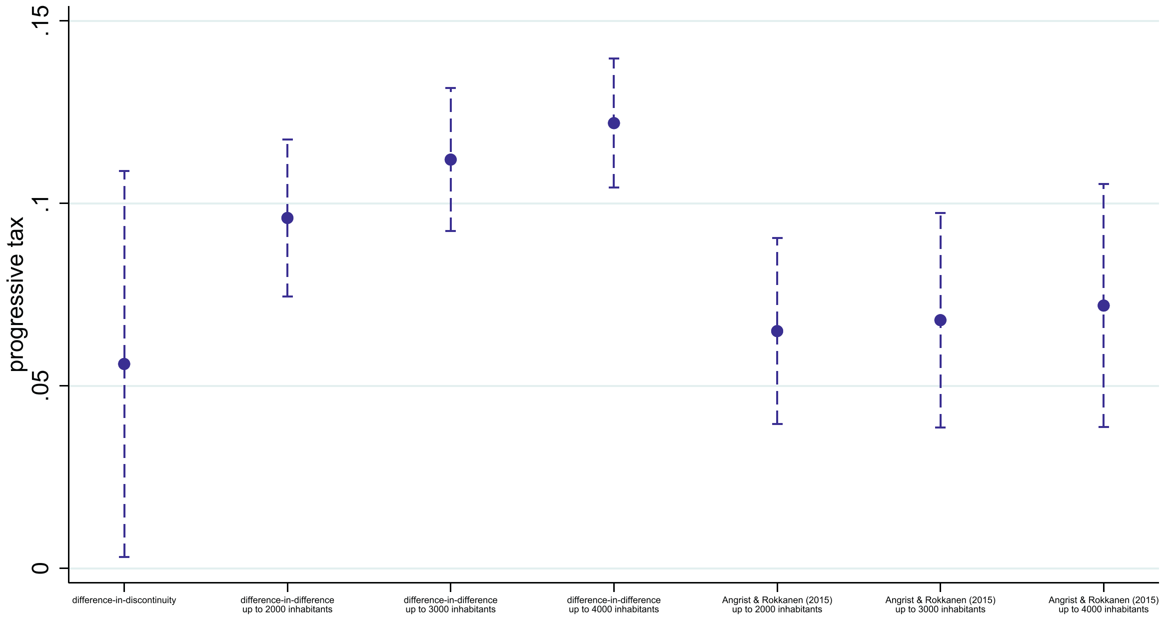


Fig. 5. External Validity. *Notes:* This figure plots our baseline difference-in-discontinuity estimate from Table 1, the difference-in-difference estimates from Appendix Table G.1 as well as the propensity score estimates based on Angrist and Rokkanen (2015) from Appendix Table G.3 together with their respective 95% confidence bands. See Appendix G for more details on the difference-in-difference design and the matching procedures.

show that standard errors are also robust to clustering at the higher province level and to accounting for spatial correlation (see Appendix Table D.7).

5.3. External validity

Our difference-in-discontinuity results only apply to municipalities at the population threshold we are exploiting. In this section, we use a difference-in-difference design and the regression discontinuity extrapolation method by Angrist and Rokkanen (2015) to show that our results can be generalized to larger municipalities. Appendix G describes the estimation methods in more detail.

For the difference-in-difference design, we use municipalities with fewer than 1000 inhabitants as our control group, and municipalities with between 1000 and 4000 inhabitants serve as our treatment group.[16] Since difference-in-difference designs estimate the treatment effect on the treated, this allows us to estimate the effect for municipalities of up to 4000 inhabitants.

Angrist and Rokkanen (2015) show how to extend results in a sharp regression discontinuity design to observations away from the threshold using matching estimators if a set of control variables are able to break the correlation between the outcome and the running variable. We exploit the fact that the difference-in-discontinuities estimator can be rewritten as a regression discontinuity design estimated on the long-difference of the outcome variables. Next, we show that a set of control variables selected by a double lasso procedure can break the correlation between progressivity and lagged population, our running variable (see Appendix Table G.2). Last, we estimate the effects away from the threshold for municipalities of up to 4000 inhabitants using both propensity score and nearest neighbour matching (see Appendix Table G.3).

Figure 5 shows our baseline difference-in-discontinuity estimate, using the dummy for having a progressive tax system as an outcome, as well as the difference-in-difference and Angrist and Rokkanen (2015) estimates for samples of up to 4000 inhabitants. Generally, the difference-in-difference estimates are larger than our baseline estimate and grow larger when extending them to larger municipalities. The extrapolation results from the Angrist and Rokkanen (2015) method are also somewhat larger than the baseline and all estimates are statistically significant at conventional levels. These results suggest that our results can be generalized to larger municipalities.

6. Mechanisms and electoral implications

We have thus far established that local governments increase tax progressivity in response to exogenous austerity requirements induced by the fiscal rule. This section explores heterogeneities in the baseline treatment effects. In particular,

[16] We stop at 4000 since the simultaneous policy changes at the 5000 threshold would confound our results (see Appendix Table C.2 for details).

Table 3
Differential effect of the reform by mayor's education: progressive tax.

	(1) progressive tax	(2) progressive tax	(3) progressive tax	(4) progressive tax
Reform x Treat	-0.002	-0.002	-0.020	-0.008
	(0.034)	(0.034)	(0.032)	(0.070)
Reform x Treat x college degree	0.138***	0.136***	0.121***	0.116**
	(0.053)	(0.052)	(0.046)	(0.046)
Reform x Treat x female mayor				0.045
				(0.070)
Reform x Treat x left-wing mayor				0.006
				(0.064)
Reform x Treat x right-wing mayor				-0.190
				(0.134)
Reform x Treat x centrist mayor				-0.292
				(0.232)
Reform x Treat x low win margin				0.027
				(0.047)
Reform x Treat x term limit				-0.007
				(0.041)
Reform x Treat x high pre-reform deficit				0.037
				(0.054)
Reform x Treat x low top income share				-0.076
				(0.054)
controls		yes	yes	yes
municipality FE			yes	yes
mean	0.087	0.088	0.088	0.088
bandwidth	650	650	650	650
N	16932	16663	16663	16663

Notes: The reform is the introduction of the fiscal rule for municipalities above 1000 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 table reports β_6 and β_6^{int} from equation (C.3) as described in Appendix C.3. Details on all controls 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

we study whether the type of the tax adjustment is different depending on the characteristics of the mayor, with an special emphasis on her education level, which has been shown to matter for economic outcomes in the literature (Besley et al., 2011; Martinez-Bravo, 2017). We then study whether introducing the fiscal rule affects the reelection chances of mayors. Finally, we present evidence from a survey of Italian mayors with the aim to better understand the reasons behind the tax policy choices of local policymakers.

6.1. The role of college-educated mayors

To understand the mechanisms underlying our results, we study whether mayors, the crucial decision-makers at the local level, respond to austerity in a heterogeneous manner. We put an emphasis on the mayor's education, but also consider other mayoral characteristics, such as, for example, party affiliation, and a battery of other municipal variables.

Table 3 presents heterogeneous treatment effects, where the interaction variable D_{it} is a dummy equal to one if the mayor holds a college degree or not (see equation (C.3) in Appendix C.3). About 45% of the mayors in our sample have a college degree (see Appendix Table C.3). It turns out that college-educated mayors drive almost all of the increase in progressivity estimated in our baseline model. Column (1) shows that mayors with a college education increased the probability of having a progressive tax system by 13.8 percentage points in response to the fiscal rule, whereas less educated mayors did not change the progressivity of the income tax at all. This result holds when including control variables, municipality fixed effects and several other interactions with potential confounders, such as gender, a proxy for electoral competition, political orientation, term limits, pre-reform fiscal position, and income structure (see columns (2) to (4) of Table 3). Furthermore, the results look very similar when using the exemption level as well as the average and marginal rate progression as alternative outcomes (see Appendix Tables D.8, D.9 and D.10). These results do not mean that mayors without a college degree did not raise local income taxes in response to the reform, but rather that they increased tax rates uniformly (see Appendix Figure D.8).

The heterogeneous effects estimated so far in this section do not have a causal interpretation because mayors' education is not assigned at random to different municipalities. To tackle this issue, we focus on close elections in which the winner and runner-up have different education levels (see Appendix C.3 for a detailed description of the empirical strategy). This empirical strategy accounts for any unobserved differences between municipalities with or without a college-educated mayors, such as unobservable preference for redistribution.

Table 4
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
Reform x Treat	-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)
Reform x Treat 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 1000 inhabitants in 2013. Columns (1) and (5) report β_6 from Eq. (2) estimated with a separate local linear regression for each outcome variable. Columns (2) to (4) and (6) to (8) report β_6 and β_6^{int} from equation (C.3) as described in Appendix C.3. Bandwidths are selected following Grembi et al. (2016). 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We present the results of the heterogeneous effects model using variation from close elections in Appendix Tables D.11, D.12, D.13 and D.14. First, in columns (1) and (2), we show that the heterogeneous effect estimated on the sample of mixed municipalities is very similar to the estimate obtained on the full sample in Table 3. Next, we present the results using only variation from close elections between the college-educated and less educated candidate in columns (3) and (4). The result confirms our previous findings. The reform-induced increase in progressivity is driven entirely by municipalities governed by college-educated mayors. We can also rule out that college-educated mayors favor more tax progressivity in general. Using a simple regression discontinuity design based on close elections and restricting to the pre-reform sample, we do not find any evidence that college-educated mayors implement more progressive tax systems (see columns (5) and (6)). These results also holds when varying the bandwidth of the close-election regression discontinuity design (see Appendix Figure D.9).

6.2. Political costs of austerity

Based on a standard median-voter model, progressive taxation should be less costly than uniform taxation, since only a minority of households earning high incomes are taxed at a higher rate (Bierbrauer et al., 2021). Therefore, we test whether the introduction of the fiscal rule was associated with a political cost for the incumbent mayor. We expect college-educated mayors to perform better at the polls since they tax a smaller share of the electorate. Note that the mayor's education is a predetermined characteristic with respect to the reform in 2013, since we only consider the first election after the reform.

The near-zero point estimate in column (1) of Table 4 suggests that there is no evidence of political costs for the average incumbent. However, this average effect hides interesting heterogeneity. When allowing for heterogeneous effects by mayoral education, we find that mayors without a college degree experienced a significant drop of 30 percentage points in their reelection probability, while college-educated mayors did not suffer these costs. These results hold when controlling for municipality fixed effects and other mayoral and municipality characteristics, both in levels and when interacted with the treatment effect.[17] The probability of running for office again does not change significantly. This holds both for the overall sample and for the interacted model (see columns (5) to (8) of Table 4). Thus, our results seem to be mainly driven by voter support and not by the choice of politicians not to run again.

Taken together, our findings suggest that more educated politicians avoided the political cost of austerity by designing a fiscal adjustment based on progressive taxation. The results are broadly consistent with recent work by Furceri et al. (2021), who show that tax-based austerity is electorally costly in a large panel of countries, but that spending-based austerity entails political costs only for left-wing governments. In our case, we focus on different types of tax-based measures and find suggestive evidence that their distributional nature might be an important factor for electoral outcomes.

One limitation of our result is that they do not establish a direct causal link from increased progressivity to higher reelection odds. To establish such a link, we need to assume that the only effect of the fiscal rule that explains the better electoral performance of college-educated mayors is increased tax progressivity. One can think of several reasons why this assumption could be violated. For example, college-educated politicians could have higher reelection odds in general. However, when we restrict our sample to the years before the introduction of the fiscal rule, we do not find evidence that this is the case (see Appendix Table D.15). Another possibility is that mayors with different education levels have a differential response to the fiscal rule that is not limited to the progressivity of the income tax. However, we show in Appendix F

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

Table 5
Mayor survey responses by mayor's education.

	(1) income tax can be progressive	(2) income tax should be progressive	(3) income tax influences reelection	(4) income tax leads to relocation	(5) tax simulator important	(6) progressive tax more difficult
Panel A: Without controls						
College degree	0.131***	0.051	0.064*	-0.012	0.002	-0.008
	(0.044)	(0.047)	(0.035)	(0.025)	(0.036)	(0.048)
controls	no	no	no	no	no	no
mean	0.682	0.606	0.149	0.076	0.831	0.490
N	437	437	437	437	437	437
Panel B: With controls						
College degree	0.106**	0.036	0.050	-0.008	-0.018	-0.013
	(0.047)	(0.050)	(0.036)	(0.026)	(0.038)	(0.051)
controls	yes	yes	yes	yes	yes	yes
mean	0.682	0.606	0.149	0.076	0.831	0.490
N	437	437	437	437	437	437

Notes: This table displays results from equation (H.7) as described in Appendix H. The outcomes are a dummy taking the value one if the mayor knows that the local income tax can be progressive in column (1), a dummy taking the value one if the mayor (strongly) agrees that the local income tax should be progressive in column (2), a dummy taking the value one if the mayor (strongly) agrees that the local income tax influences reelection chances in column (3), a dummy taking the value one if the mayor (strongly) agrees that the local income tax induced the relocation of taxpayers in column (4), a dummy taking the value one if the mayor (strongly) agrees that using the tax simulator is important for making decisions in column (5) and a dummy taking the value one if the mayor (strongly) agrees that creating a progressive tax reform is more difficult than a flat tax in column (6). The controls are described in Appendix H. Robust standard errors in parentheses. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

that none of the other major municipal policy instruments were differently affected by the mayors with different education levels.[18] Thus, any remaining difference would need to originate from other differential behaviour of the two types of politicians. While we cannot rule out this possibility, our preferred interpretation of the results is that the differential reelection rates are driven by college-educated mayors relying on progressive taxation, while mayors without a college degree raised taxes uniformly.

6.3. Survey of mayors

Why do mayors without a college degree respond to austerity by implementing electorally costly flat tax increases if, similar to educated mayors, they could have implemented electorally more favorable progressive tax increases? We consider four main sets of possible and mutually non-exclusive explanations. First, college-educated and less educated mayors may have different preferences concerning the equity–efficiency trade-off of non-linear income taxes. Mayors can be heterogeneous in both their preferences over the social welfare weights they assign to winners and losers of tax reforms, and their beliefs about the efficiency costs of income taxes. Second, the two types of mayors may have different administrative abilities when designing and implementing tax reforms. For example, mayors without a college degree may have a higher propensity to choose the easier solution of a flat tax, since it may be administratively more complicated to implement progressive tax reforms. Third, they may have different beliefs about the potential political costs of different types of income tax reforms if mayors without a college degree are, for example, more myopic. Fourth, college-educated mayors may be more informed and be more more likely to know that the local income tax can be modulated as a progressive tax.

We fielded a brief online survey of current Italian mayors in 2022 to better understand whether equity–efficiency preferences, administrative abilities, political beliefs or knowledge gaps can explain the differential responses of college-educated and less educated mayors. Appendix H presents the survey design, the exact questions and a detailed description of the results. 461 mayors fully completed our survey which corresponds to a response rate of about 6%. We link these responses to administrative data on the characteristics of mayors. Despite the relatively low response rate, there is no statistically significant difference between participants and non-participants with respect to their party membership and education level as well as the population size of their municipality (see Appendix Table H.1). However, participants are slightly more likely to be female and below 40 years of age. Therefore, we also show results using sample weights to account for these observable differences. Nevertheless, one should interpret the results with some caution since there might be other unobserved factors influencing selection into the survey. A further limitation is that the surveyed mayors are most likely not the same mayors from our main analysis since the survey was conducted nine years after the reform.

Table 5 studies our four main explanations by testing whether mayors with and without a college degree responded differently to these respective questions in the survey. We do not find evidence that college-educated and less educated mayors have significantly different redistributive preferences or beliefs about efficiency costs with respect to the local in-

[18] We go even one step further and show that controlling for all other policy variables as well as the income tax base does not change our results (see Appendix Table D.16).

come tax (see columns (2) and (4) of Table 5), and nor do we find evidence that they have different administrative abilities (see columns (5) and (6) of Table 5). The result that clearly stands out is that less educated mayors seem to be less informed about the options for customizing the local income tax. In particular, they are 13 percentage points less likely to know that the local income tax can be progressive than college-educated mayors. This difference is statistically significant at conventional levels and holds when conditioning on other mayoral characteristics, when restricting the sample to small municipalities, or when using weights to account for the observable selection into survey participation (see Appendix Tables H.2 and H.3). Consistent with our results on differential election outcomes, the survey suggests that college-educated mayors are more likely to believe that the income tax affects their reelection chances, while mayors without a college degree seem to be more myopic regarding the political costs of tax reforms. However, this difference is not precisely estimated and becomes indistinguishable from zero when including control variables, limiting the sample to small municipalities or using weights.

Overall, the survey presents suggestive evidence that knowledge gaps play a role in explaining the question as to why some mayors seem to go against their electoral incentives when implementing tax reforms. While the existing literature is almost exclusively concerned with uninformed voters (see, for example, Stantcheva, 2021), we show that the lack of correct knowledge among politicians with respect to the precise tools of fiscal policy under their control can be important as well. This finding is consistent with recent work that extends information provision experiments to the domain of policymakers (Hjort et al., 2021).[19]

7. Conclusion

Periods of fiscal austerity can be of major historical importance for the countries implementing them. In principle, every country could find itself in a position where it is forced to make large fiscal adjustments. Yet, austerity measures do not only impact aggregate economic trends, but may also have far reaching distributional consequences. For example, recent evidence has shown that austerity can affect a range of outcomes of societal relevance, such as health (Stuckler and Basu, 2013), human capital formation (Pavese and Rubolino, 2021), violence (Cooper and Whyte, 2017), gender equality (Karamessini and Rubery, 2013), and support for populism (Dal Bó et al., 2018; Fetzer, 2019), among others.

In this paper, we provide the first quasi-experimental evidence showing that governments try to ease the potential distributional implications of austerity by sparing the relatively poor through progressive income tax policies. These take the form of higher marginal tax rates on the rich, and of tax exemptions for the poor. This result is in accordance with recommendations of the IMF, which stresses that governments could make use of progressive income taxation to ease the distributional cost of austerity (IMF, 2014). More generally, this evidence is in line with compensatory arguments behind the historically observed rise of progressive taxation. 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 (Scheve and Stasavage, 2012; 2016) – in our case an unpopular fiscal rule enacted during a double dip recession. Although our evidence from Italian towns cannot be easily 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. We believe that our evidence is particularly informative on austerity episodes induced by external factors, such as resulting from the imposition of fiscal constraints from a higher layer of government, which can be seen as unfair from the perspective of the local population.

Our results are also relevant for the debate on the political costs of austerity. We find that mayors without a college degree that are more likely to implement flat tax reforms in response to the need for austerity bear significant electoral costs, while college-educated mayors that more often opt for progressive tax reforms perform better in the upcoming elections. This finding on the political importance of the distributive nature of reform packages can be relevant for policymakers in jurisdictions subject to strong fiscal constraints, but whose public opinion is growing critical of austerity policies. Whereas our work focuses on income taxation, future work may think about the role of other fiscal policies that have a clear distributive component, such as wealth and other capital taxes or government transfers.

Finally, our paper speaks to the question of why policymakers decide to opt for flat versus progressive tax reforms. Our findings highlights the role of information deficits at the local level, which can prevent politicians from implementing electorally more favorable tax reforms. This evidence can have implications for the literature on the political economy of taxation, where equilibrium policies are typically determined in a game of political competition by well-informed and vote-share-maximizing politicians. It remains for future work to study the question of why policymakers decide to acquire or avoid information. Another question that remains to be studied is whether information deficits, perhaps of more sophisticated forms, are also relevant in settings other than small municipalities or in policy areas other than non-linear taxes.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jmoneco.2022.07.006.

[19] See Cotton and Li (2018) for a model of information acquisition among politicians.

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