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**A Fistful of Dollars: Rent Seeking Behaviour and Local Tax Manipulation**


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# A Fistful of Dollars: Rent Seeking Behaviour and Local Tax Manipulation*

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**Abstract**

The aim of this paper is to study whether politicians manipulate fiscal policy to extract private rents. We focus on the local personal income tax (PIT), in the setting of Italian cities, which is a progressive instrument that allows mayors to set different rates to distinct wage groups. We exploit discontinuities in mayors' salaries, that are based on population thresholds, to study whether mayors systematically apply lower rates to their own tax bracket. The main results document large rent-seeking activity in fiscal policy. First, we show that when mayors's salary is exogenously located in the following tax bracket this receives a significantly lower tax rate than the previous bracket, compared to the control group. Second, we show that this rent-seeking activity is highly detrimental for the public treasury, with a considerable reduction in fiscal revenues. And finally, we document that the monetary gains for *rent-seeker* politicians are rather limited. These results suggest that when fiscal policy is prone to be manipulated politicians do not hesitate to engage in rent-seeking activities even in case of little profits.

**JEL codes:** D72, E62, H71, P16.
**Keywords:** rent-seeking, fiscal policy, personal income tax, efficiency wage, regression discontinuity design.

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# 1 Introduction

The strategic manipulation of policies committed by the decision-maker is often the cause of policy failure and the emergence of sub-optimal outcomes (Persson, Tabellini 2002). But what explain policy manipulation? On the one hand, these behaviours are generally motivated by electoral incentives, in order to maximize the re-election probability. This can lead to the phenomenon of the "political budget cycle", the reduction of taxes or the increase in public expenditures before elections, in order to gain political support (Alesina et al. 1997, Drazen, Eslava 2010). On the other hand, these behaviours may be generated by the pursuit of private interests. Politicians often distort public policies for private gains in order to favour their own relatives (Folke et al. 2017), facilitate the business of close firms (Ferguson, Voth 2008), or extract rents from the office (Eggers, Hainmueller 2009). Nevertheless, despite the convincing evidence that politicians do distort policies for private gains, there are no studies that analyse manipulation of fiscal policy. There are, indeed, many anecdotal evidence[1] suggesting that also fiscal policy is the object of considerable rent-seeking activity and this is an important aspect to study in terms of policy implications.

This paper aims to fill this gap in the literature as it is among the first to show that politicians are willing to manipulate fiscal policy to extract private rents, when they have the possibility. We are able to show that local administrators distort fiscal choices to introduce favourable tax rates in order to pay less taxes on their own incomes, and we document such large rent-seeking activities even in a setting in which monetary gains are limited. Moreover, we also provide evidence that this activity is highly distorting for the public budget and we quantify the loss of revenues inflicted to the public treasury. These results suggest that when policies are prone to be manipulated politicians do not hesitate to distort them even in case of little profits.

To address this question we focus on Italian municipalities and we study the local component of the income tax, the municipal surcharge (*Addizionale comunale IRPEF*). This is a direct and personal tax that represents an important source of revenues for Italian municipalities: it is the third most important local tax after the real estate and the waste tax, and its importance is growing over time. The municipal surcharge can be introduced with a multi-rate structure allowing to tax differently the distinct income brackets. This allows the mayors, the heads of the municipal government, to decide the tax rate of all the income brackets, including the one at which their own revenues are taxed.

Furthermore, the way Italian mayors are remunerated allows us to conduct a causal analysis. The salary of mayors is, indeed, determined according to the

[1] There are many anecdotes in recent Italian history about strategic decision in fiscal policy. One case is due to 2004 and 2005 budget laws (Law n. 350/2003, art. 4, comma 153 and n. 311/2004, art. 1, comma 246) that introduced an incentive for cable-box purchases in Italy. The law was introduced by centre-right government held by Silvio Berlusconi and the main beneficiary was a company whose majority stakeholder was Paolo Berlusconi, brother of the prime minister. Another anecdote is about inheritance tax: this was reformed in 2006 by the centre-left government held by Romano Prodi and it leads to an increase in inheritance fees. After the reform, word spreads that the prime minister made a consistent donations to his sons before the application of the law.

population of the city, as expressed by *Law Bassanini* n. 265 (03/08/1999), and this sharply modifies in correspondence of specific population thresholds, based on the most recent national census. City population, therefore, jointly determines the salary and the income bracket of the mayor. In this paper we focus on a specific threshold of population, 5,000 inhabitants, where the income of the mayors sharply raises by the 28.6% as it shifts from €26,040 to €33,480 (yearly, gross) and the corresponding income bracket exogenously raises from the second to the third one. The specificity of the setting allows us to test whether mayors manipulate fiscal policy to extract rents by comparing the rates introduced by similar mayors, that pay taxes in different income brackets.

To causally test our hypothesis, we conduct the analysis as a sharp Regression Discontinuity Design. We compare mayors of similar cities whose salary is exogenously placed in different income brackets and we focus on their choices in terms of fiscal policy. In particular, we consider mayors around the 5,000 population threshold whose income bracket exogenously shifts from the second to the third one and we compare the tax rates introduced to the corresponding brackets. With this strategy it is possible to study whether mayors that are placed in the third bracket tax less that bracket compared to the second one, using as control group mayors, of similar cities, that pay taxes in the second bracket.

One possible concern can be that mayors' salary can include other earnings such as rents from financial assets or real estates, as well as wages from their "civic" occupation[2]. These additional earnings may raise mayor's taxable income leading to a shift upward in the subsequent bracket. This generates measurement error and it is likely to attenuate our estimates but it is not a concern for the identification strategy. A similar concern can happen whether mayors are eligible for deductions: in case of large deductions, mayor's taxable income may shift downward in the precedent bracket. This can generate additional noise in our estimates, further attenuating the effect. In these regards, what we are capturing in this analysis is an intention to treat rather than a local average treatment effect.

The main results of this analysis document large rent-seeking activities in this setting. In particular, mayors of cities above the threshold, that pay taxes in the third bracket, tend to introduce lower tax rates on the that bracket compared to mayors of cities below it and the effect is sizeable and statistically significant as it amounts to between 39% and 61% of the dependent variable average value, depending on the specification. Moreover, there are no further discontinuities in other positions of the tax structure around the threshold, suggesting that this effect is local in correspondence of the third bracket. These results are robust to the use of different dependent variables, the delta between tax rates and the rates in absolute value, and to many possible bandwidth specifications. Moreover, large heterogeneous effects emerge as rent-seeking behaviour is stronger when mayors are young (under-40), male, self-employed workers or highly educated (holding at least a university degree). These outputs seem to suggest that rent-seeking is stronger when

[2]Gagliarducci et al. (2010) focus on Italian members of Parliament and document consistent levels of moonlighting; they also underline a trade-off between the quality of elected officials and the effort exerted in political life, as good politicians tend to keep their outside jobs and are more likely to shrink once elected.

mayors are less likely to have additional salaries (young), can hide or cover part of their revenues (self-employed workers) or are more skilled, informed and aware about mechanisms of the fiscal system (educated). Moreover, rent-seeking is larger in cities that are located in the south of Italy, that are less financially constrained (with large fiscal revenues and public spending) and where electoral incentives are weaker (in the years far from the elections).

As a second result, we find that this rent-seeking activity is highly detrimental for public finance outcomes. Indeed, tax revenues generated by the surcharge significantly decrease in those municipalities where the manipulation takes place. To provide evidence on this, we simulate bracket-specific revenues for each city and we observe large reductions in the revenues generated by the third bracket as a result of this manipulation: the average effect is sizeable and it amounts to the 8.1% of the dependent variable average variable. Moreover, through a back to the envelope calculation, we estimate that revenue loss generated by this rent-seeking amounts to 30 millions Euros per year.

Furthermore, another outcome that emerges from these analysis is that monetary gains from this rent-seeking activity are rather limited. The amounts saved by *rent-seeker* mayors, indeed, ranges between 29 and 264 Euros per year, depending on the specification. These figures represent the amount that mayors in treated cities would pay additionally on their income tax if the tax scheme was similar to the one in the control group. This result suggests that little profit is enough to motivate local administrators to generate even large distortions in fiscal policy. Therefore, these behaviours would be even more likely in case of larger returns.

Nevertheless, there may be additional explanations to interpret this result. First, the baseline effect may be artificially low because of the large measurement error, due to omitted revenues. The fact that we find large heterogeneous effects, indeed, suggests that the average effect is driven by a small group of compliers. Second there may be behavioural reasons as mayors may simply want to keep low their tax rate, without computing the actual savings, i.e., they may simply focus on the direction and not on the magnitude of the tax cut. Third, and finally, it is possible that mayors are not only favouring themselves with a reduction in the same tax rate; rather, it is plausible that they also want to reward people in their inner circle, such as friends, relatives or colleagues, who are likely to be part of the same economic group and to pay taxes into the third bracket. With this interpretation the overall return from rent-seeking is much larger.

This paper combines many strands of literatures. First, we relate to the literature studying returns from office of politicians. Many papers show that members of parliaments may collect consistent post-congressional earnings (Diermeier, et al. 2005, Eggers, Hainmueller 2009, Parker, Parker 2009) and that these extra-revenues raise with political relevance (Parker 1992). Moreover, political returns often involve family members of politicians, that have better outcomes in the labor market in terms of earnings (Gagliarducci, Manacorda 2017, Fafchamps, Labonne 2017) and quality of the occupation (Folke et al. 2017).

Second, another set of papers focuses on connections with firms. There is evidence that firms connected with politicians face improvements in their operational and stock-market performances (Fisman 2001, Ferguson, Voth 2008, Goldman et al.

2009a, Niessen, Ruenzi 2010) and these results are confirmed when business owners themselves take a political office (Faccio 2006, Bunkanwanicha, Wiwattanakantang 2009). Moreover, firms which are connected with politicians happen to have better access to credit (Khywaya, Mian 2005), higher probability to be bailed out by the government (Faccio et al. 2006), larger offering prices in the IPO market (Francis et al. 2009) and better procurement contracts (Goldman et al. 2009b). Our paper contributes to these literatures as we are among the first to document large rent-seeking activities, aimed at extracting private rents, in fiscal policy. Secondly, we manage to provide an estimate of the detrimental effect of this manipulation for local public finance and to compute the monetary return from this activity. Our results show that little profits are enough to motivate politicians to introduce large distortions in fiscal policy.

Furthermore, this paper is related to the literature studying how remuneration of politicians affects their performances in office and the process of political selections. The main results suggest that higher wages improve politicians quality, measured with personal characteristics, such as education, previous profession or political experience and lead to better political performances (Ferraz, Finan 2009, Gagliarducci, Nannicini 2013, Fisman et al. 2015). Another group of studies, then, focuses on moonlighting, the practice of carrying out further jobs in addition to the political mandate. These papers mainly study effects on performances and selection (Eggers, Hainmueller 2009, Gagliarducci et al. 2010, Geys, Mause 2013, Mocan, Altindag 2013, Campbell, Cowley 2015) and they find that this practice reduces performances of members of parliaments in office. Our project provides a contribution in this literature as we show that, beside politicians' remuneration, also the corresponding taxation represents an important determinant of performances in office. Fiscal treatment of public salary, indeed, affects politicians' incentives and this should be carefully designed by the policy-maker in order to avoid manipulation.

Finally, this paper is linked to the literature exploring the manipulation of public balance sheets for electoral purposes. These studies focus on the practice of "political budget cycle" that leads to cyclical manipulation of taxes and public expenditures in order to please voters as elections approach. On the one hand, the literature documents the phenomenon from a cross-country perspective (Alesina et al. 1997) and describes how this practice is affected by electoral and political systems (Persson, Tabellini 2002). On the other hand, there is a large literature studying the issue at the local level. Many papers uncover consistent balance sheets manipulation in several countries involving public finance variables such as taxes (Kneebone, McCKenzie 2001, Alesina, Paradisi 2015), public expenditure (Drazen, Eslava 2010) and public transfers (Akhmedov, Zhuravskaya 2004). In addition, few papers inspect which institutional frameworks promote or weaken the practice (Rose 2006, Benito et al. 2013, Repetto 2016, Giommoni 2019). This large literature underlines that politicians tend to use fiscal instruments to increase their probability of re-election or to improve their future political career but it does not take into consideration other types of incentives. Our paper shows that politicians may commit strategic choices in fiscal policy not only for electoral incentives, but also to extract private rents.

The paper is structured as follows: section 2 discusses the institutional setting

and the data. Section 3 presents the identification strategy. Section 4 shows the main results. Section 5 discusses the heterogeneous effects and the impact on fiscal revenues. Section 6 discusses the robustness checks and section 7 concludes.

# 2 Institutional setting and data

## 2.1 Local and political background

Municipalities are the smallest Italian administrative units: local government is composed of an elected mayor (*Sindaco*) that appoints an executive committee (*Giunta*), and an elected city council (*Consiglio Comunale*). The mayor and the committee are in charge of the administration of the local government and they propose annually the budget that should be voted by the city council. In Italy there were 8,046 municipalities in 2015, divided into 110 provinces and in 20 regions. The total number of municipalities slightly changes every years due to merges, incorporations or separations of administrative units.

Mayors and city councils are replaced with municipal elections every five years and there is a two-terms limit for mayors, but only in case of consecutive terms. A large majority premium is granted to the winning mayor in order to ensure local government stability. Moreover, the electoral system implies a single-round for cities under 15,000 inhabitants and a runoff, between two most voted candidates, for those above this population threshold. Municipal elections are staggered as every year a different group of cities host municipal elections, and this divides Italian municipalities into five groups according to their election year.

Municipalities are important centres for public spending. They administer about 10% of total Italian public expenditure (Grembi et al. 2016); they manage several local public services, such as local welfare, waste management, municipal police, infrastructure and water supply. In terms of revenues, these come from taxes, fees (e.g. public services, advertisement), capital transfers, sales of public assets, borrowing and transfers (from the central or regional government or from the European Union). Taxes are the most important source of revenues as they account for around 50% of total revenues (in 2012). Nonetheless, municipalities are still highly dependent on transfers, mostly from regional or national government. Moreover, municipalities are subject to the domestic fiscal rules (*Patto di stabilitá interno*) aimed at reducing local debt accumulation and deficit growth. These budget rules are structured according to several population thresholds and have been found to be effective to contain public deficit and to maintain adequate taxation levels (Grembi et al. 2016): until 2012 fiscal rules apply to cities with population higher than 5,000 inhabitants and this threshold has been reduced to 1,000 inhabitants in 2013 (Daniele et al. 2019).

In this paper, we focus on the Italian income tax, the IRPEF (*Imposta sul reddito delle persone fisiche*) and in particular on the municipal surcharge to the income tax, the *Addizionale comunale IRPEF*[3]. This tax is direct and personal, its taxable

[3]The Italian income tax is divided into three distinct rates: a national one, a regional one (*Addizionale regionale IRPEF*) and a municipal one (*Addizionale regionale IRPEF*), that we evaluate in this paper.

income is the sum of all gross incomes of an individual and it is approved yearly by the municipal council, upon mayor's proposal. Moreover, the revenues generated by this tax pertain the municipal treasury. In 2012 the surcharge was reformed and it was introduced the possibility to set it progressively: the mayor could set different tax rates, in the range 0-0.8%, to the different national IRPEF wage brackets, as well as introducing an exemption threshold, below which the rate is zero[4]. The main objective of the reform was twofold: increasing municipal revenues and strengthening fiscal equity at the local level, introducing a highly flexible fiscal instrument (Giommoni 2019)[5].

The surcharge is an important local tax: it is the third source of local revenues, after the real estate and the waste tax, and its importance grew quickly from its introduction in 1999. Figure 1 shows the evolution of the three most important municipal taxes as percentage of total fiscal revenues, on average for all Italian municipalities: what emerges is that the surcharge represents an important source of revenues (it generated about 4.3 billions Euro in 2015) and its relative importance is raising over time, compared to the other taxes that are losing weight. Moreover, the tax is a salient fiscal instrument for taxpayers and it is considered a particularly unpleasant local tax: Giommoni (2019) shows that the attention toward this tax is comparable to the one devoted to the other local taxes using *Google trend* traffic and a set of survey evidence. Moreover, it emerges that the salience of this tax is heterogeneous across different groups of people, indeed, it raises with education and wealth of respondents. Table 1 shows average rates for the five income brackets: column 2 considers the whole sample of cities, while columns 3 and 4 focus, respectively, on cities that have a surcharge higher than zero and on cities that introduced flexible rates, *i.e.* a multiple rates structure or an exemption threshold. We can note that the tax structure is always progressive as the marginal rate raises with income and that progressivity is higher for the sub-samples of cities in columns 3

[4]In case a multi-rate structure is introduced with many different tax rates, the application method of the tax can be freely decided by the city administration. In particular, the different tax rates can be applied in a progressive way, *i.e.* the corresponding tax rate is applied only to the revenue within each specific bracket, or with the method of the overall income, *i.e.* the tax rate of the highest bracket applies to the overall income declared by the taxpayer. It is important to note the application of the latter method generates several kinks, or notches, in the tax schedule. This issue has been highly debated by different Italian Institutions. Many cities, indeed, raise the issue after the reform of 2012 and both the Italian Ministry of Economics and Finance and the National Association of Italian Municipalities (*ANCI - Associazione Nazionale Comuni Italiani*) suggested to apply the multi-rate structure progressively. Nevertheless, in the original formulation, the municipal surcharge had to be applied with the method of the overall income (`https://st.ilsole24ore.com/art/norme-e-tributi/2011-11-04/addizionali-irpef-scaglioni-211925.shtml?uuid=AaSWDrIE`). Moreover, the same issue emerges for the application of the regional surcharge (the region of Emilia-Romagna, for instance, opted for the method of the overall income). Ultimately, the decision on which method to apply is, *de facto*, left to each municipal administration. There are, indeed, many cases of cities that decided to apply the method of the overall income as evinced by their municipal acts (the authors conducted a manual check on a set of municipalities). The details on the method of applications are, unfortunately, not available for the entire sample of cities and, therefore, we cannot control for this in our analysis.

[5]Giommoni (2019) shows that after this reform, that introduced income progressivity at the municipal level, mayors start manipulating the municipal surcharge in order to obtain electoral consensus and this leads to large political budget cycles.

and 4.

## 2.2 Mayors remuneration

The focus of this paper is on public remuneration of mayors. The salary of Italian mayors is determined according to the population of the city, as expressed by *Law Bassanini* n. 265 (03/08/1999), and this sharply modifies in correspondence of specific population thresholds, based on the most recent national census.

Table 2 shows the population groups that determine mayors' salary and the corresponding number of cities: the largest increases take place around 5,000 inhabitants, where the wage raises by the 28.6% (it shifts from €26,040 to €33,480, gross per year) and around 500,000 with an increase of the 34.9% (it shifts from €69,360 to €93,600, gross per year). Moreover, Table 2 also shows the corresponding income bracket where the mayor pays his own income taxes. For cities with less than 5,000 inhabitants, indeed, mayors' salary is located in the second bracket, while, in cities above it moves from the second to the third one. Another shift takes place after 100,001 inhabitants, where mayor's wage is located in the fourth bracket and, lastly, after 500,001 inhabitants, the last bracket is reached. In this paper we focus on the 5,000 population threshold where mayors' income bracket exogenously changes, according to the size of the city. This threshold, indeed, is the only one where the salary increase corresponds to a shift in the income bracket and the sample size is large enough to conduct a statistical analysis.

Furthermore, a set of additional policies vary exogenously with population of municipalities. Table 3 shows these policy changes. Few of these vary around the 5,000 population threshold, overlapping with the policy change that we study in this paper. We will discuss in the empirical session why these simultaneous policy changes are not a problem for the identification strategy. In particular three policies vary at the 5,000 threshold. First, the wage of executive committee members sharply raises from 20% to 50% of the mayor's wage . Second the domestic stability pact, applies to cities above that population cutoff, but the threshold have been reduced to 1,000 in 2013: this policy is aimed at containing local deficit imposing a gradual reduction in municipal spending (Daniele et al. 2019). Third, and finally, starting from 2013, gender quotas have been introduced to cities with population higher than 5,000 inhabitants: this policy has the objective to raise female representation in municipal councils and it introduces a double preference voting, conditioned on gender.

Finally, city population is not the unique criterion for the computation of the salary of Italian mayors: this also depends on other factors such as budget performances of the municipality and occupational status of the mayor, whether she is an employee or a self-employed worker or whether she is on leave of absence. In particular, if the mayor is a self-employed worker she can cumulate the earnings, differently, if she is a dependent employee, she has to ask for a leave of absence, otherwise the public salary is cut by half. We will discuss later why this is not an issue for the identification strategy as this is only a source of noise for the estimates[6]. Moreover,

[6]However, there is evidence that the fraction of cities where mayors ask for a leave of absence is not different around the population threshold. In particular Gagliarducci, Nannicini (2013)

under specific and documented circumstances, the executive committee of the city can raise mayor's remuneration up to 15%, upon the approval of the Ministry of Internal Affairs. If applied, this policy would simply generate additional noise in our estimate as mayors of cities below 5,000 inhabitants would move to the third income bracket. Nonetheless, Gagliarducci, Nannicini (2013) provide survey evidence that this policy applies in very few municipalities around the 5,000 inhabitants threshold.

## 2.3 Data

The dataset used in this study combined different sources and covers the period 2012-2017. First, we make use of the data on balance sheets of Italian municipalities: the source is the Ministry of Internal Affairs (*Ministero dell'Interno*). These data contain detailed information on revenues, expenditures, deficit, debt and transfers. Table 4, Panel A, includes the descriptive statistics for public finance indicators. Revenues from the municipal surcharge amount to 47.8 Euros per-capita, around one fourth of the real estate tax, the most important Italian local tax. Moreover, we have rich data on the municipal surcharge, the local tax evaluated in this paper, drawn from the Italian Fiscal Agency (*Agenzie delle Entrate*): the rate set for each wage group, the exemption threshold, the day when city council sets the annual surcharge level and, eventually, additional requirements to obtain the exemption. Table 4, Panel B, shows the descriptive statistics of the municipal surcharge: most municipalities, around two thirds, introduce the flat regime, while the other third introduces an exemption or a multi-rate structure.

Second, we use data on local politics from the Italian Ministry of Internal Affairs (*Ministero degli Interni*) for all Municipalities, in the time span in analysis. These dataset includes information on election dates, elected politicians (mayors, aldermen, councillors), their characteristics (education, age, gender and occupation), the votes each candidate gets and the municipal turnout. Table 4, Panel C, shows descriptive statistics on the political background.

Third, we make use of data on the income distribution of Italian taxpayers over time: the source of this data is the Italian Ministry of Economics and Finance (*Ministero dell'Economia e delle Finanze*). This information, aggregated at the city level, come from yearly declarations of tax-payers to the fiscal authority, and they cover the time span 2012-2017. These data contain information on the amount declared and the frequency of tax-payers for each income bracket: Italian cities have on average 3,742 tax-payers declaring an aggregate amount of 91.2 millions of Euros. Table 4, Panel D, shows these statistics. And finally, we include data on population from the Italian Statistical Office (ISTAT), Panel D of Table 4.

The sample in analysis consists of all Italian municipalities, both from ordinary and special regions, and the time span covered is 2012-2017. We keep in the analysis all electoral terms which ended prematurely due to a governmental crisis in order to avoid sample manipulation.

conducted a phone interview survey of mayors in towns of between 4,900 and 5,100 inhabitants (in office on 1 May 2009). They obtained reply from 36 out of 57 mayors and it emerged that the fraction of part-time mayors was 53%, with the others working full-time as mayor. Importantly, this fraction was almost identical for towns below and above the threshold (54% and 53%, respectively).

# 3 Empirical analysis

## 3.1 Identification strategy

In this project we want to explore whether local politicians tend to tax less wage brackets where their own salaries are located. We could not simply study whether mayors with higher salaries tend to tax less rich people than mayors with lower wages, comparing the two groups, because this could generate biased estimates, due to endogeneity issues: it could be, for instance, that mayor's salary is correlated with local income distribution and this is likely to affect incentives to tax middle and high incomes. Furthermore, remuneration of mayors can be related with city size which could also affect the use of the surcharge and the tax structure.

In order to avoid these issues of endogeneity, we exploit the unique Italian setting and we make use of population thresholds that determine the public salary of mayors and that induce exogenous variations in the tax bracket where the mayors pay their own taxes. In particular, we rely for this analysis on the 5,000 inhabitants threshold where mayors' wage jumps from € 26,040 (yearly gross) to € 33,480 (yearly gross) and this induces an exogenous shift of mayor's wage from the second to the third income bracket, as already discussed in the institutional setting section[7]. This setting allows us to test our hypothesis with a sharp regression discontinuity design technique where municipal population represents the forcing variable of the analysis. Therefore, the treatment of the analysis is mayor's income (and the associated tax bracket) and the dependent variable is the difference in tax rates between the second and the third bracket.

The main identifying assumption of the model is that covariates are continuous around the population threshold and that there are no sharp changes in factors able to affect the tax structure in a non linear way, *i.e.* with modifications in specific areas of the tax scheme. In particular, we have to exclude the presence of discontinuities that may affect the tax scheme in correspondence of the shift between the second and the third tax bracket.

Many possible threats to this identification strategy can arise. First, as discussed by Eggers et al. (2015), one pitfall in the use of population-threshold RDD can be that the same threshold is used to determine multiple policies, and this case is not an exception. Indeed, as already discussed in the institutional setting sections, other three policies move sharply around the 5,000 inhabitants threshold. First, wage of executive officers sharply changes from an amount equal to 20% of mayor's salary to 50%. This may affect local government of the city but there are no reasons why this may modify fiscal policy non-linearly. Moreover, the sharp increase in executive officers' salary does not provide them with any incentives to manipulate the income tax, and in particular the third bracket tax rate: their salary, indeed, moves from 5,208 to 16,740 Euros, gross per year. Second, balanced-budget rule, the domestic stability pact, changes at this threshold. In particular, Italian stability pact applies

[7]As already discussed in the footnote 4 there are two possible methods to apply the municipal surcharge in case of a multi-rate structure, the progressive and the overall income method. The fiscal treatment of mayors' salary around the population threshold, therefore, changes depending on the applied method, but in both cases mayors of cities above the threshold have an incentive to keep low the third income bracket tax rate.

only to municipalities above 5,000 inhabitants. Moreover, this restriction only holds for 2012, as the policy changed in 2013 and the threshold has been decreased to 1,000 inhabitants (Daniele et al. 2019). In order to avoid a confounding effect due to the incidence of the stability pact that may affect fiscal capacity differently around the threshold, we exclude the year 2012 from the sample in the main empirical analysis. Third, in 2013 gender quotas have been introduced in Italian cities with population higher than 5,000 inhabitants: this policy implies the adoption of a double preference voting conditioned on gender (*Law 215/2012*). Baltraunite et al. (2016) show that this intervention leads to a consistent increase in female representation in local councils. Nonetheless, in 2014, a reinforced version of this gender quota policy has been extended to municipalities above 3,000 inhabitants (*Law 56/2014*), which states that elected politicians of each gender cannot have less than 40% of municipal seats. In order to clean for the impact of this policy we always control for gender of mayors and councillors.

A second threat can be the risk of manipulation around the threshold since cities can self-select in order to get better policies, as pointed out in the European context by Eggers et al. (2015). This should not be a problem in our case as mayors' wage depends on population data from the latest census, taking place in 2011, before the surcharge became progressive. It is, then, implausible that mayors tried to manipulate figures to get better treatment before the policy took place. Nevertheless, we show that there is no evidence of manipulation around the cutoff conducting a McCrary test. Furthermore, we run standard tests for continuity of covariates around the threshold of 5,000 inhabitants to provide evidence that all relevant factors, but mayors' wage bracket, vary smoothly around the cutoff. Finally, mayors may have additional sources of earning, such as their "civic" occupation or financial and real estate rents, and these may raise their taxable income locating them in a tax bracket that is higher than the one imputed in this analysis. This will probably attenuate our estimates but it won't be a concern for the identification strategy. A similar concern can be whether mayors are eligible for deductions, lowering their taxable incomes. In case of large deductions, it may be possible that treated mayors, those in cities above the 5,000 inhabitants threshold, reach the precedent income bracket. This can generate additional noise in our estimates, further attenuating the effect.

## 3.2 Specification

The regression discontinuity design strategy requires to estimate, by local linear regression (LLR), the following model (according to Calonico et al. 2014 and Gelman, Imbens 2014):

$$y_{it} = \alpha + \beta_1 \hat{Pop}_i + \beta_2 Wage_i + \beta_3 \hat{Pop}_i * Wage_i + \delta_t + \lambda_r + \zeta X_{it} + \epsilon_{it} \qquad (1)$$

The dependent variable, $y_{it}$, captures the difference of the municipal surcharge rate between the second to the third income bracket, $\tau_3 - \tau_2$ (henceforth the *second tax gap*), in municipality $i$ in year $t$. This allows us to study modifications in local fiscal policy in that specific part of the income distribution. However, we also study the impact of the policy on the second and third tax rates expressed in levels.

$\hat{Pop}_i$ is the forcing variable, $\hat{Pop}_i = Pop_i - \bar{Pop}$, and it captures the distance between the population in municipality $i$, according to the last census (2011), $Pop_i$, and the population threshold, $\bar{Pop} = 5,000$. $Wage_i$ is the treatment dummy, equal to 1 if the income bracket of the mayor in city $i$ is the third one, *i.e.* $Pop_i \geq 5,000$, and zero otherwise. $\delta_t$ are year fixed effects and $\lambda_r$ are macro-region fixed effects. Finally, $X_{it}$ includes mayors and councillors characteristics (age, gender and education), which are important factors to control for in this setting as these are important determinants of local policies. Moreover, Gagliarducci, Nannicini (2013) show that, above 5,000 population threshold, mayors and candidates are more educated and more likely to be white collar. To deal with this result, we directly test this hypothesis in our time sample and we do not find any discontinuity in politicians' characteristics around this population threshold: this may be due to the fact that profiles of local politicians were different in the early 2000, the period studied by Gagliarducci, Nannicini (2013). Moreover, $X_{it}$ includes controls on local politics (last elections turnout and vote share of most voted candidate), to control for relative power of the mayor in the city council. Finally, standard errors are always robust and clustered at the municipal level. The coefficient we are interested in is $\hat{\beta}_2$ that captures how fiscal policy changes as the mayor pays taxes in the third bracket and identifies the local average treatment effect (LATE). The implementation of the RDD-LLR model is limited to the sub-sample of municipalities in the interval $Pop_i \in [\bar{Pop} - h; \bar{Pop} + h]$, with symmetric optimal bandwidths, according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b).

As a second analysis, we apply the method of the spline polynomial approximation to study the same relation (Bordignon et al. 2016): for the sake of brevity, the outputs of this analysis are in the appendix. This method uses the whole sample of municipalities between 1,000 and 10,000 inhabitants and chooses a flexible functional form to fit the relationship between $y_{it}$ and $Wage_i$ on either side of the cutoff of 5,000 inhabitants. Specifically, we estimate the model:

$$y_{it} = \sum_{k=0}^{p} (\beta_k \hat{Pop}_i^k) + Wage_i \sum_{k=0}^{p} (\gamma_k \hat{Pop}_i^k) + \delta_t + \lambda_r + \zeta X_{it} + \epsilon_{it} \tag{2}$$

$\hat{Pop}_i^k$, $Wage_i$, $\delta_t$, $\lambda_r$ and $X_{it}$ are defined as in the previous model and $\gamma_0$ captures the jump between the two regression functions in correspondence of the cutoff. Thus, the local average treatment effect is consistently estimated by $\hat{\gamma_0}$ and we show these results to multiple orders of the polynomial (namely, $p = 1$, $p = 2$ and $p = 3$). Standard errors are, then, robust and clustered at the municipal level.

Finally, we want to provide a placebo showing that the discovered effect takes place only locally, between the second and the third income brackets. To provide this evidence, we study the impact of the reform also in correspondence of the shifts among the other tax brackets. Therefore, we run again the model (1) with a different set of dependent variables capturing the change in the tax rate between the second and the first bracket ($\tau_2 - \tau_1$), the fourth and the third bracket ($\tau_4 - \tau_3$) the fifth and the fourth bracket ($\tau_5 - \tau_4$). These tests guarantee that the effect is local in the tax structure and that we are not capturing a modification in the overall tax scheme taking place for cities with population larger than 5,000 inhabitants.

# 4 Results

## 4.1 Effect on tax rates difference

Table 5 contains the main results from the analysis of the model (1). As already discussed, the dependent variable of the analysis is the rate difference between the second and the third income bracket, "Second rate gap" ($\tau_3 - \tau_2$), shown in column 2. Moreover, we include all other tax rate differences as placebos: the "First rate gap" captures rate difference between first and second brackets ($\tau_2 - \tau_1$), the "Third rate gap" between third and fourth brackets ($\tau_4 - \tau_3$) and the "Fourth rate gap" between fourth and fifth brackets ($\tau_5 - \tau_4$), respectively columns (1), (3) and (4). Presented estimates include the set of controls and fixed effects discussed in the previous section and optimal bandwidths are symmetric and computed according to Calonico et al. (2014a, 2018a, 2018b). The table is organized in two panels, focusing on different sub-samples: on the one hand, Panel A considers municipalities that use the municipal surcharge as a source of local revenues, *i.e.* we exclude from the sample cities that never introduce the surcharge in this time span. On the other hand, Panel B only focuses on cities that set a differentiated rate structures, *i.e.* which introduce a multiple rates or exemption thresholds. These cities clearly constitute the set of compliers as they tax differently the different tax brackets.

Column (2) shows the main result and it suggests that the rate difference between second and third brackets is significantly lower in cities where the mayor is subject to the third bracket tax rate, compared to similar cities where the mayor is located in the second one. In other words, third bracket tax rate raises less, compare to the second bracket one, in cities that are above the cutoff, where mayors' income ends up in the third bracket. The average effect on rate difference is not negligible as it varies between 39% and 61% of the dependent variable average value. As a second result, it does not emerge any discontinuity in the other tax rate differences around the cutoff: focusing on columns (1), (3) and (4) we can never reject the null hypothesis that average rate gaps are equal around the threshold. This evidence suggests that the increase in mayors' salary impacts only locally in the tax structure, between the second and the third bracket, which is where the salary shift takes place. Moreover, as all other tax rates do not show discontinuities around the threshold, it is implausible that the result is due to some other factors affecting uniformly local taxation. Similar results emerge in Panel B of Table 5 and the marginal effect on second rate gap is more than three times larger than in Panel A. This suggests that municipalities that use differentiated rate structures are those where this rent-seeking activity actually takes place[8]. Moreover, despite the sizeable reduction in the sample, these estimates are more precisely estimated than those of Panel A[9]

These results are shown graphically in Figure 2 that contains the outcomes for cities with flexible surcharge rates, given the optimal bandwidth. The negative

[8]We also checked whether the probability of introducing a multi-rate structure changes endogenously around the 5,000 threshold. We did not find evidence that cities above the cutoff tend to use more or less often multi-rates. These results are omitted for the sake of brevity and are available upon request.

[9]Table 12, in the Appendix 1, shows the same outcomes computed without including controls and fixed effects: similar results emerge from the table, with slightly lower coefficients.

discontinuity for the second rate gap emerges clearly around the threshold in the top-right plot. Moreover, all other plots show the other rate gaps and there are no evidence of discontinuities. Furthermore, in order to show that this results is robust to many bandwidth specifications, we redo the main RDD analysis on the second rate gap using many possible bandwidths in the range 1,500-5,000. Figure 3 plots the coefficients of these analysis as function of the adopted bandwidth: what emerges from the table is that the coefficient is almost always negative and statistically significant confirming that these results are robust to many possible specifications.

These results support the hypothesis that mayors engage in rent-seeking behaviour manipulating fiscal policy in order to obtain monetary payoffs: what emerges is that local politicians tend to tax less their own income bracket when they have the possibility. In particular, mayors paying their own taxes into the third bracket keep the associated tax rate lower than those subject to the second bracket rate. These results provide new causal evidence on rent-seeking behaviour in terms of fiscal policy at the local level. Moreover, this type of manipulation induces mayors to reduce the degree of progressivity of local taxation in their own city as the tax structure becomes flatter and this represents an additional negative effect of these behaviours.

## 4.2 Effect on single tax rates

An important issue, related to the results of Section 4.1, is whether the drop in the second rate difference, in treated cities, is due to an increase in the tax rate of the second bracket or to a decrease in the third bracket tax rate: only the second case is coherent with the story of manipulation. In order to shed light on this point, we provide a test in Table 13, in the Appendix 1. We repeat the analysis of model 1, using as dependent variables the single rates: $\tau_1$, $\tau_2$, $\tau_3$ and $\tau_4$. The main goal of this test is to check whether the results obtained in Table 5 imply a reduction in $\tau_3$ or an increase in $\tau_2$ for cities above the 5,000 cutoff. The set of controls and fixed effects is the same as the main specification plus the inclusion of a control capturing the average surcharge rate in the previous year, to control for the overall taxation level of the city. A caveat for this additional exercise is that the identifying assumption is less conservative: differently from the main specification, indeed, where we use as dependent variable the delta between tax rates, we are now comparing tax rates in levels. And this makes easier violations of the identifying assumption.

Table 13 contains the results of this test: what emerges is that the tax rate of the third income bracket is significantly lower for cities above the cutoff, while there is no difference in the tax rate of the second income bracket. These results provide support for the idea that mayors located above the threshold manipulate taxes for personal benefit assigning a lower rate to their own income bracket.

# 5 Additional results

## 5.1 Heterogeneous effects: mayors' characteristics

One important caveat in reading the main results is that mayors may actually pay taxes in a bracket which is different than the one imputed in the analysis because of the incidence of additional incomes as well as deductions. As already discussed in section 3, this is not a threat for the identification strategy but it may downward-bias these estimates. Therefore, it is possible that the group of complier cities is a subsample where mayors below the 5,000 cutoff actually pay taxes in the second bracket and those above are located in the third one. This makes particularly important the study of heterogeneous effects in order to clarify which is the group of mayors reacting more to these incentives.

We performed two types of heterogeneity analysis, with respect to mayors (Panel A) and cities characteristics (Panel B), contained in Table 6. Let's start from Panel A. As a first test we focus on demographic characteristics of mayors: columns (1) and (2) of Table 6 divide between mayors with high and low education, respectively with and without a university degree: it emerges that mayors with high education manipulate more the surcharge, compared to the other category, and the effect is larger than the one found in the main specification and statistically significant. Mayors with higher education may have a better knowledge on the functioning of tax system and they may be more capable to exploit fiscal tools to extract personal rents.

As a second analysis we study whether the effect changes with the age of mayors and we divide the sample between young and old administrators, respectively under-40 and over-40 years old. This characteristic may be correlated with the possibility of having additional earnings and young mayors are more likely to get their revenues primarily from the institutional salary. Thus, it is more likely that these estimates are less affected by measurement error. Results of this test are shown in columns (3) and (4) of Table 6 and confirm this conjectures as the effect is larger for young mayors as well as stronger than those found in the main analysis. Thirdly, we focus on gender of the mayor. There is a large literature suggesting that public misbehaviours such as corruption, bribery and embezzlement are less likely to be committed by women, compared to men. These studies, mostly relying on experimental evidence, acknowledge several possible channels such as risk aversion and preference for reciprocation (see Serra, Wantchekon 2012 for a survey of the literature). We want to study whether differences across genders appear also in this setting and we divide between female and male mayors: columns (5) and (6) of Table 6 contain results of this analysis and confirm the classic view as the effect only emerges for male mayors.

Moreover, we study whether the type of mayors' occupation is correlated with rent-seeking behaviour. On the one hand, self-employed workers who directly fill-in their own tax return have a deeper knowledge of the the tax system and they are probably more capable to manipulate fiscal policy. On the other hand, employees, whose employers are in charge of paying taxes, are less likely to be strategic. Moreover, self-employed workers have more chances to hide or cover part of their revenues, and to declare lower incomes. In columns (7) and (8) of Table 6 we split

the sample between employees and self-employed mayors and main results confirm this idea as the effect is absent for the former group and it is very large and precise for latter one.

Finally, we graphically test the robustness of these heterogeneous results to multiple bandwidth specifications. The first four sub-figures of Figure 4 plot the regression coefficients of the following heterogeneous samples: mayors with high education, young mayors, male mayors and self-employed mayors. The coefficients are almost always statistically different from zero and rather robust as the bandwidth widens.

## 5.2 Heterogeneous effects: city characteristics

The second block of heterogeneous analysis focuses on whether the manipulation varies with characteristics of the city. First, we focus on different effects across geography and we divide the sample between Centre-north and South of Italy. This is an important geographical division under many aspects such as economic development, local public finance, civic spirit and social capital (Banfield 1958, Putnam 1993, Harari, Tabellini 2009). Results of this test, shown in columns (1) and (2) of Table 6 (Panel B), suggest that the effect is entirely driven by the southern regions of the country as the coefficient is very large and precisely estimated[10]. Second, we focus on local budget variables. Columns (3-8) of Table 6 (Panel B) split the sample of cities in terms of total fiscal revenues, current expenditures and capital expenditures, all in per-capita terms, and perform the main analysis for cities that are below and above median values. What emerges is that manipulation is larger for cities that have fiscal revenues, current and capital expenditures above the median level. This suggests that mayors of richer cities are more likely to extract rents from the office and the main reason may be that these cities are less financially constrained. The last four sub-figures of Figure 4 provide a graphical analysis of the robustness of these results to many bandwidth specifications plotting the regression coefficient for cities in the south of Italy, with fiscal revenues, current and capital expenditures above the median value. The coefficients are almost always robust to many bandwidths and significant.

As a third analysis we raise the issue whether political accountability concerns affect mayors' rent-seeking behaviour. Many studies show that politicians tend to modify several components of the budget in order to raise their chances of re-election[11]. Moreover, there is compelling evidence that electoral concerns limits dishonest behaviours such as corruption and malfeasance, as shown by Bobonis et al. (2016) for municipalities in Puerto Rico. According to this idea, we want to show whether mayors engage in less rent-seeking behaviour due to political accountability concerns. We perform these tests in Table 7 where we compare rent-seeking

[10] In Table 14, in the Appendix 1, we study the heterogeneous effects of cities of southern regions depending on characteristics of the mayor and the city: unsurprisingly, results for this sub-sample of cities are even larger.

[11] Bordignon et al. (2015) shows that mayors with re-election incentives reduce more visible taxes, Alesina, Paradisi (2015) document large political budget cycles with respect to the most important local tax, the real-estate tax, and Giommoni (2019) shows that also the municipal surcharge is subject to large political budget cycle dynamics, especially after 2012 when the tax became progressive.

behaviour in years which are close to the next elections (last two years of the term), where the incentive to behave honestly is particularly high, with the years far from next polls (first three years of the term). Main results support the political accountability hypothesis as manipulation only emerges in years far from the elections, where electoral incentives are weaker. This evidence seems to suggest that mayors strategically reduce rent-seeking when it is more convenient behaving honestly for electoral incentives.

## 5.3 Monetary returns from tax manipulation

In this section we focus on monetary returns of the mayor from this form of rent-seeking. In particular, we want to explore how much do mayors save with this manipulation. In order to answer this question we simulate the amount of Euros saved given that they are paying taxes in the third income bracket and that they declare a maximum amount of 55,000 Euros. With estimated coefficients emerges that the average amount saved ranges between 29 and 264 Euros per year[12]. These figures represent the amount that mayors in treated cities would paid additionally on their income tax if the tax scheme was similar to the one in the control group. There are clearly large differences depending on the sub-sample considered and this is reasonable as different groups of mayors have different incentives and possibilities to extract these types of rents.

There may be several possible explanations to justify why local politicians engage in this form of rent-seeking even with limited economic returns. First, it is possible that the baseline effect obtained in the main analysis is artificially low because of the large measurement error, due to omitted revenues. The fact that we find large heterogeneous effects, indeed, suggests that the average effect is driven by a small group of compliers. Second there may be behavioural reasons under this strategic behaviour: it is possible that mayors simply want to keep low their tax rate, but they do not compute the fiscal savings, i.e., they may simply focus on the direction and not on the magnitude of the tax cut. Moreover, they may feel unconformable to raise their own tax rate, and they may be tempted to postpone, in the tax structure, the tax increase to avoid this self-punishment. Third, and finally, it is possible that the mayor is not only favouring himself with a reduction in the same tax rate; rather, it is plausible that he also wants to reward people in his inner circle, such as friends, relatives or colleagues, who are likely to be part of the same economic group and to pay taxes in the third bracket. With this interpretation the overall return from manipulation is much larger. Nevertheless, if mayors are capable to generate sizeable distortions in fiscal policy for these limited amounts this suggests that these behaviours are even more likely in case payoffs are larger.

[12]The upper bound emerges in the specification where we study southern cities with young mayors. Moreover, to compute these figures we use the method of the overall income, if the other method was applied these numbers would be lower.

## 5.4 Impact on fiscal revenues

In the last sections we showed that mayors manipulate tax rates of the municipal surcharge in order to pay a favourable tax rate and this results in a reduction of the third income bracket tax rate for treated cities. We want now to study whether this rent-seeking activity damages local public finance outcomes. In particular, we analyse whether and how this manipulation has an effect on fiscal revenues generated by the surcharge and, possibly, on other components of the budget.

First, we simulate the amount of fiscal revenues generated by each income bracket in each cities making use of data on the gross income declared in each bracket and the corresponding rate. The average amount of revenues per group is reported in Table 4, Panel B, in per-capita terms. The bracket-specific revenues raise as we move to higher brackets, meaning that the average amount declared is larger for richer taxpayers. This simulation does not take into account the presence of deductions, as we do not have information on deducted amounts, and this leads to an upward bias of these numbers. Anyway, this would be a problem only if the accuracy was different around the threshold. To check for this, we sum up the overall simulated revenues for each city and we compare it with the realized revenues from the surcharge, that we obtain from the official balance sheets: it emerges that the simulated revenues were 26.5% higher than the true ones for cities below the threshold and they were 22.9% higher for cities above the threshold[13]. This evidence shows that the inaccuracy is not different around the 5,000 inhabitants threshold, suggesting that we can use simulated revenues with limited risk of distortion. Finally, in order to express these simulated revenues in per-capita terms, we use as denominator the number of taxpayers in each income bracket.

Then, we conduct the standard analysis of the model (1) using as the main dependent variable the difference in fiscal revenues generated by the third and the second income bracket: the *Second revenues gap*, $R_{\tau_3} - R_{\tau_2}$. We always include in these analysis the complete set of fixed effects and controls. Moreover, we also study the impact on all the others revenue gaps as a set of placebo (first revenues gap, $R_{\tau_2} - R_{\tau_1}$, third revenues gap, $R_{\tau_4} - R_{\tau_3}$ and fourth revenues gap, $R_{\tau_5} - R_{\tau_4}$). Table 8 shows main results. First, a clear discontinuity emerges for the second revenues gap, which drops above the population threshold. The coefficient is only significant for cities adopting flexible rates (Panel B) but the average effect is sizeable as it amounts to a reduction of the 8.1% of the variable average value. Second, the other revenues gaps do not vary around the population threshold as all coefficients are non significant. This result shows that the manipulation of the tax rates, conducted by the mayors, damages the balance sheet and it leads to a reduction of fiscal revenues generated by the municipal surcharge[14]. This specific form of rent-seeking behaviour results, then, to be very costly for the municipality. Moreover, we conducted a back to the envelope calculation in order to quantify the aggregate damage of this practice and it turns out that the fiscal revenues lost because of this manipulation amounts

[13] To conduct this test we use an arbitrary bandwidth of 1,500 inhabitants.

[14] Moreover, we also test for the presence of complementary effects in the budget due to this manipulation. In particular, we run the main specification looking at the impact on total fiscal revenues, fees, taxes, total spending, current and capital spending and we do not find significant evidence of complementary effects.

to 30 millions of Euros per year. Further, Figure 5, shows the main outcomes graphically and the significant drop in the second revenues gap is visible in the top-right plot and Figure 6 shows that this result is robust to many population bandwidth specifications.

Finally, we conducted the same analysis on the amount of revenues generated by each income bracket, in levels, to study whether the drop in the second revenues gap is driven by an increase in revenues in the second bracket or by a drop in the third bracket. Only the second hypothesis, indeed, is in line with our story of rent-seeking. Main outputs, shown in Table 16 in the Appendix 1, confirm our conjecture as revenues generated by the third bracket significantly drop above the population threshold, while there are no effects for the other variables.

# 6 Robustness checks

## 6.1 Continuity of covariates

First, we test the standard assumption in the regression discontinuity design methodology which requires that covariates vary smoothly around the cutoff. In this case, we need to check whether observables characteristics are continuous around the 5,000 threshold and between the second and the third income bracket of the tax scheme. We perform two types of tests. On the one hand, for the set of bracket-specific variables, we study their continuity around the threshold and the tax brackets. These outputs are reported in Table 9 where we focus on four main variables: gross income declared (Panel A), gross income declared per-capita (Panel B), number of tax-payers (Panel C) and number of tax-payers per-capita (Panel D). These characteristics vary smoothly across income brackets as coefficients are never significantly different between municipalities around the population threshold.

On the other hand, for the city-specific variables, constant across brackets, we only study their continuity around the population threshold. These outputs are reported in Table 10 where we focus on political variables such as mayor's characteristics (education, age, gender and whether she is a white collar worker), political background characteristics (vote share of the winner, political turnout), both reported in Panel A, and public finance indicators (total fiscal revenues, revenues from the real estate tax, current and capital expenditures), reported in Panel B. Also these characteristics vary smoothly around the population threshold supporting the continuity of covariates assumption. These results confirm that the effect is not driven by any other city-specific factors that may have affected local management and they support our results on rent-seeking. Furthermore, the absence of a sharp change in mayors'characteristics around the threshold suggests that in this setting we do not have any selection of politicians due to a better remuneration, differently from what Gagliarducci, Nannicini (2013) show, for the period 1993-2001. With this test, then, we exclude the presence of a possible confounding factor due to mayors' characteristics. The incoherence of our results with those of Gagliarducci, Nannicini (2013) may be due to the fact that profiles of local politicians modified consistently over time.

Finally, a possible violation of the continuity assumption may be due to the

fact that, starting from 2012, cities with population larger than 5,000 inhabitants experienced a consistent reduction in governmental transfers. Marattin et al. (2019) show that this policy leads to an increase in revenues from local taxes. Nevertheless, this result does not represent a problem for our identification strategy as the reform did not affect the revenues collected from the municipal surcharge which is equal on either side of the 5,000 threshold.

## 6.2 Manipulation around the threshold

Second, we test for the presence of manipulation around the 5,000 population threshold due to an endogenous shift of cities. This is ex-ante implausible as the reference population is recorded in the last census of 2011, which is antecedent to the introduction of progressivity of the municipal surcharge that has been introduced in 2012. Furthermore, we test this assumption conducting a McCrary test on the number of cities in the first year of our analysis, 2013. Figure 7 shows the output of the test suggesting that there is no difference in the density of cities around the 5,000 inhabitants threshold[15].

## 6.3 Fake thresholds

As a third test, in order to show that the drop in the tax rate between second and third rate is actually due to the change in mayors' salary bracket, we repeat the same exercise using different, fake, population thresholds, where salary of mayors raises but the income bracket does not modify. This placebo allows us to figure out whether the effect depends solely on the fact that mayors' income raises or, instead, whether it is due to the shift to an higher income bracket. In particular, we run the main analysis using two different population thresholds: 3,000 and 10,000 inhabitants. Around these thresholds the income of the mayors raises but it does not move to a higher income bracket: around 3,000 threshold the yearly income raises from €17,400 to €26,040 and around 10,000 threshold it raises from €33,480 to €37,200. Table 11 shows results of these placebos: columns (1) and (2) study the evolution of the second rate gap around 3,000 cutoff and columns (3) and (4) focus on 10,000 cutoff, we include both specifications with and without controls. From the table it does not emerge any discontinuity in the second rate gap, in correspondence of both population thresholds. This result seems to suggest that what matters is the modification in the income bracket of mayors instead of the mere increase in their salary.

## 6.4 Spline polynomial approach

Fourth, we validate main results conducting the main analysis using the methodology of the spline polynomial approach (Bordignon et al. 2016). Table 15 in the

[15]This test has been conducted using a bandwidth of 3,000 inhabitants, but the results are similar using bandwidths of 2,000 and 1,000 inhabitants. Results of these additional tests are not shown and are available upon request.

Appendix 1 contains the analysis according to the model 2 using as dependent variable the second rate gap, $\tau_3 - \tau_2$. Panel A and B show the analysis considering, respectively, the sample of cities that use the surcharge as a source of revenue and those that introduce a differentiated rate structure, for both versions outputs with and without controls are presented. From the table the main result is confirmed, although sometimes the coefficient is not statistically significant.

# 7 Concluding remarks

This is one of the first papers to provide causal evidence that politicians are willing to manipulate fiscal policy for personal gains, if they have the possibility. We find that local administrators tend to introduce a favourable tax rate to their own income bracket, in order to pay less taxes, and that this generates large distortions in fiscal policy. Furthermore, sizeable heterogeneous effects emerge as the manipulation is larger when mayors are young, male, self-employed or highly educated and they administer cities in the South of Italy and with high fiscal revenues and public expenditures.

Secondly, we show that this type of rent-seeking leads to little personal profits: the average fiscal savings, indeed, ranges between 29 and 264 Euros per year. This result shows that a limited incentive is enough to induce the decision-maker to introduce sub-optimal policies, suggesting that local politicians, at least in our setting, are highly vulnerable to these types of behaviours. Finally, we manage to quantify the revenue loss inflicted by this strategic behaviour. We simulate the revenues generated by each income brackets and we estimate a loss of 30 millions Euros per year due to this rent-seeking activity.

Our results suggest that fiscal policy is prone to be manipulated if there may be profits for the decision-makers. Moreover, these findings are relevant as they suggest the importance to design policies that are difficult to be manipulated and that do not offer ways of extracting personal profits.

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# Figures

Figure 1: Proportion of fiscal revenues by year of property tax, waste tax and municipal surcharge, as percentage of total fiscal revenues.

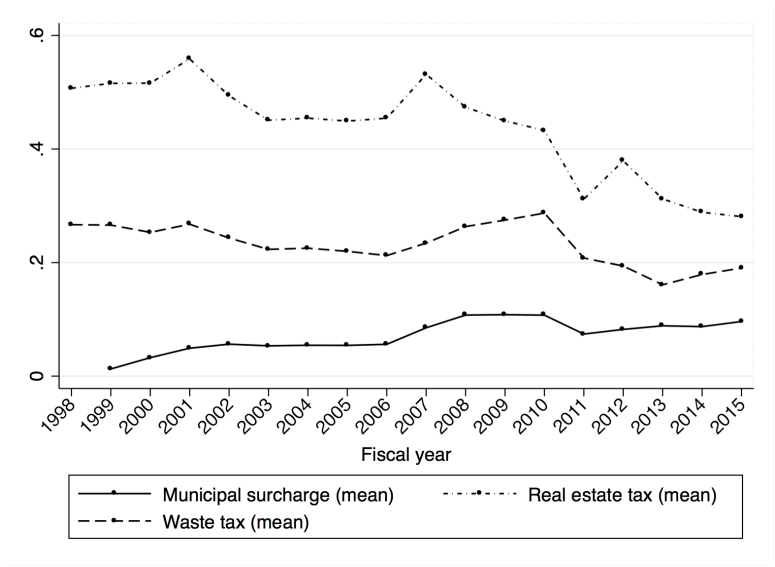
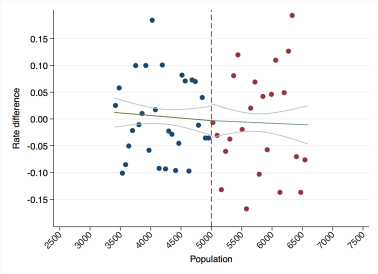
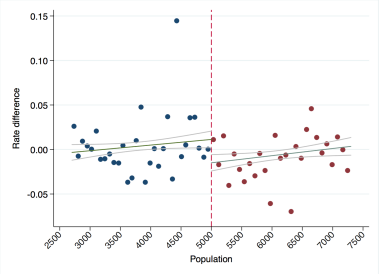


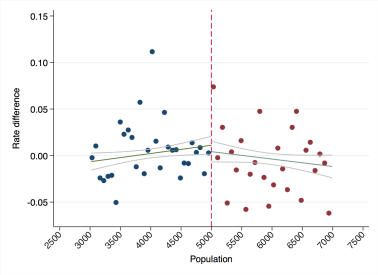

Figure 2: Rate differences around 5,000 inhabitants threshold



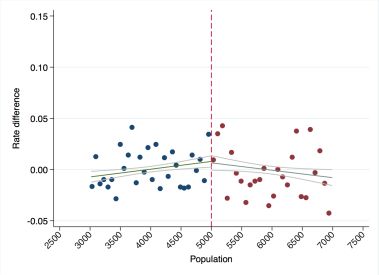

(a) Between first and second bracket




(b) Between second and third bracket



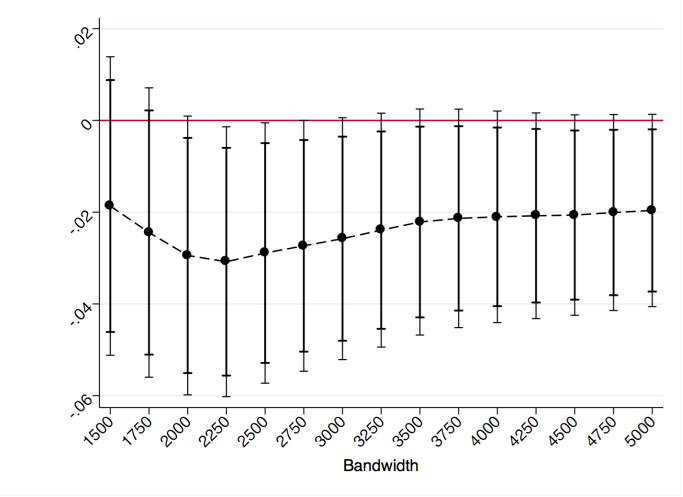

(c) Between third and fourth bracket




(d) Between fourth and fifth bracket

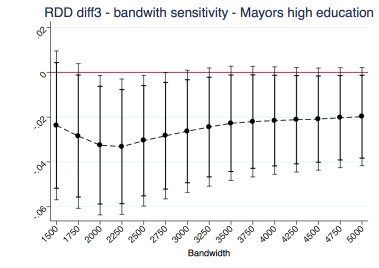
*Notes*: The plot focuses on Italian municipalities with flexible surcharge rate for the period 2013-2017. The picture includes lines of best fit with 95% confidence intervals, performed separately on either side of 5,000 inhabitants threshold. The complete set of controls is always included as well as year and macro-region fixed effects (sa presented in Section 4).

Figure 3: RDD coefficient on the analysis on second rate gap-different bandwidths

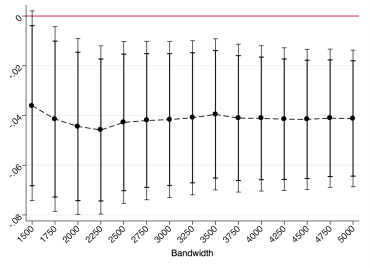



*Notes*: The plot focuses on Italian municipalities with flexible surcharge rate for the period 2013-2017. The picture includes the RDD estimator for each bandwidth specified as well as 5% and 10% confidence intervals. The complete set of controls is always included as well as year and macro-region fixed effects (as presented in Section 4).

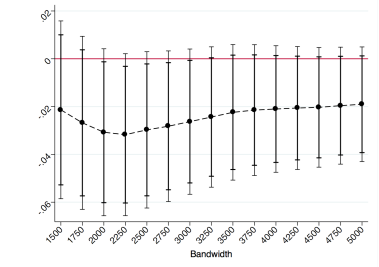
Figure 4: RDD coefficient - analysis on second rate gap: heterogeneous specifications-different bandwidths



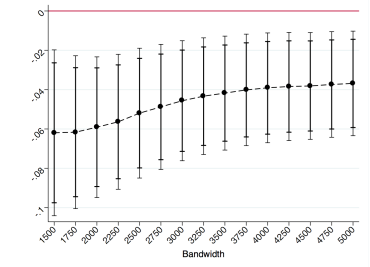

(a) Mayors high education



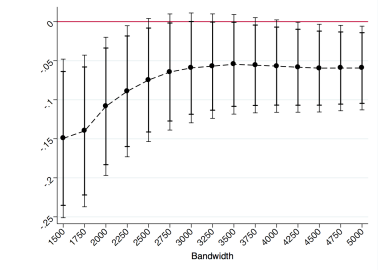

(b) Young mayors



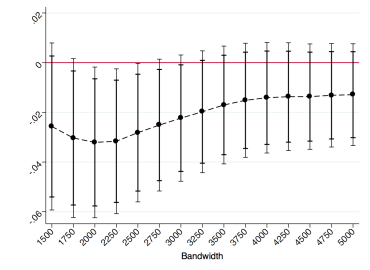

(c) Male mayors



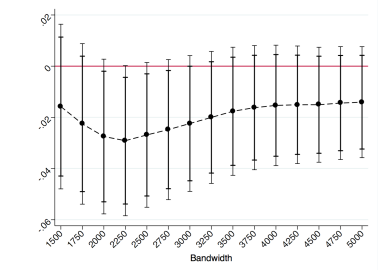

(d) Mayors self-employed



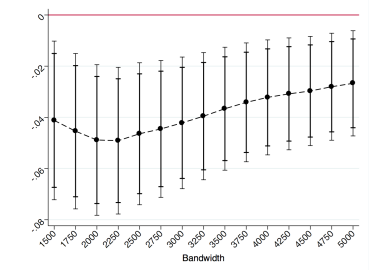

(e) South of Italy




(f) Fiscal revenues above the median



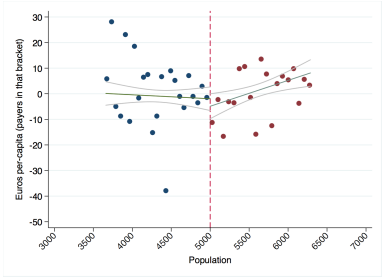

(g) High current expenditures



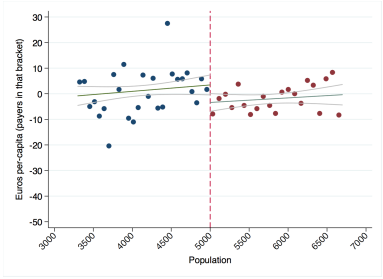

(h) High capital expenditures

*Notes*: The plot focuses on Italian municipalities with flexible surcharge rate for the period 2013-2017. The picture includes the RDD estimator for each bandwidth specified as well as 5% and 10% confidence intervals. The complete set of controls is always included as well as year and macro-region fixed effects (sa presented in Section 4).

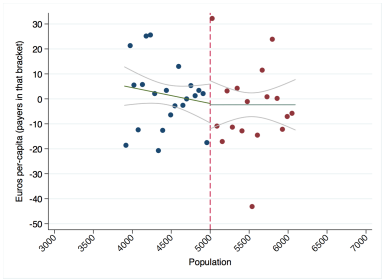
Figure 5: Revenues differences around the 5,000 inhabitants threshold



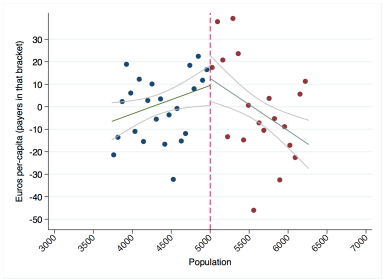

(a) Between first and second bracket




(b) Between second and third bracket



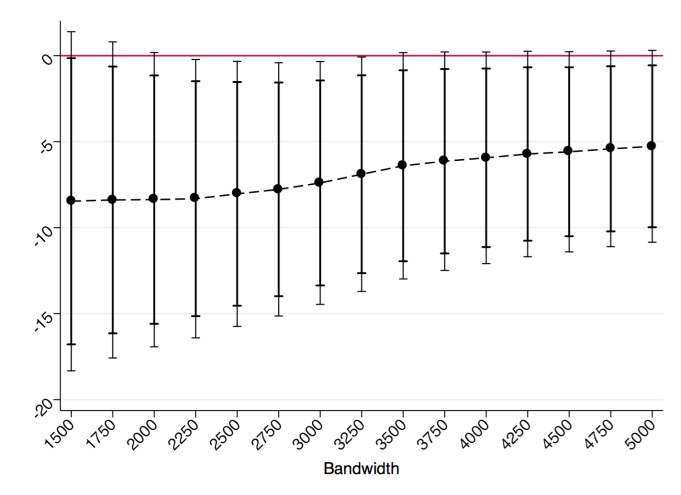

(c) Between third and fourth bracket




(d) Between fourth and fifth bracket

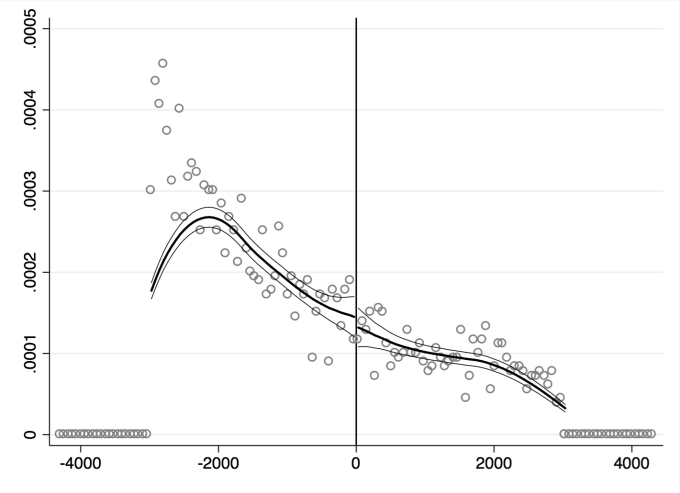
*Notes*: The plot focuses on Italian municipalities with flexible surcharge rate for the period 2013-2016. The picture includes lines of best fit with 95% confidence intervals, performed separately on either side of 5,000 inhabitants threshold. The complete set of controls is always included as well as year and macro-region fixed effects (sa presented in Section 4).

Figure 6: RDD coefficient on the analysis on revenues differences-different bandwidths




*Notes*: The plot focuses on Italian municipalities with flexible surcharge rate for the period 2013-2017. The picture includes the RDD estimator for each bandwidth specified as well as 5% and 10% confidence intervals. The complete set of controls is always included as well as year and macro-region fixed effects (as presented in Section 4).

Figure 7: McCrary test




*Notes*: The plot shows the McCrary test conducted using the sample in the year 2013, including a population bandwidth of 3,000 inhabitants.

# Tables

Table 1: Surcharge rate per bracket

| Wage bracket | Surcharge rate (all cities) | Surcharge rate (citie with positive surcharge) | Surcharge rate (cities with flexible rates) |
|---|---|---|---|
| (1) | (2) | (3) | (4) |
| I bracket [0-15,000] | .366 % | .445 % | .254 % |
| II bracket [15,001-28,000] | .472 % | .573 % | .538 % |
| III bracket [28,001-55,000] | .484 % | .588 % | .633 % |
| IV bracket [55,001-75,000] | .494 % | .600 % | .714 % |
| V bracket [over 75,001] | .500 % | .607 % | .769 % |
| N | 32,287 | 26,593 | 11,390 |

*Notes*: The column "all cities" contain all the Italian Municipalities, the column "cities with positive surcharge" contain all the Italian cities that introduce the municipal surcharge at least one year in the time sample and the column "cities with flexible rates" contain all the cities where a multiple rate structure or an exemption threshold has been introduced at least once.

Table 2: Mayors' salary and tax brackets

| Population | Number of cities | **Monthly** salary of mayor (gross) | **Yearly** salary of mayor (gross) | **Tax bracket** of mayor's salary |
|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) |
| Up to 1,000 | 2,152 | € 1,290 | € 15,480 | II bracket [15,001-28,000] |
| Between 1,001-3,000 | 2,525 | € 1,450 | € 17,400 | II bracket [15,001-28,000] |
| Between 3,001-5,000 | 1,111 | € 2,170 | € 26,040 | II bracket [15,001-28,000] |
| Between 5,001-10,000 | 1,152 | € 2,790 | € 33,480 | III bracket [28,001-55,000] |
| Between 10,001-30,000 | 858 | € 3,100 | € 37,200 | III bracket [28,001-55,000] |
| Between 30,001-50,000 | 161 | € 3,460 | € 41,520 | III bracket [28,001-55,000] |
| Between 50,001-100,000 | 89 | € 4,130 | € 49,560 | III bracket [28,001-55,000] |
| Between 100,001-250,000 | 32 | € 5,010 | € 60,120 | IV bracket [55,001-75,000] |
| Between 250,001-500,000 | 6 | € 5,780 | € 69,360 | IV bracket [55,001-75,000] |
| Over 500,001 | 6 | € 7,800 | € 93,600 | V bracket [over 75,000] |

*Notes*: Population is the number of resident inhabitants as measured by the last available Census. Number of cities refers to values in 2011. Amounts are expressed in Euros according to *Law Bassanini, n. 265 03/08/1999* and are gross of taxes.

## Table 3: Policies varying around population thresholds

| Population | Wage executive committee | Fee municipal council | Executive committee size | Municipal council size | Electoral rule | Neighbourhood councils | Hospital/ health | Domestic stability pact - until 2012 | Domestic stability pact - from 2013 | Gender quotas |
|---|---|---|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Up to 1,000 | 15% | 18 | 4 | 12 | single | no | no/no | no | no | no |
| Between 1,001-3,000 | 20% | 18 | 4 | 12 | single | no | no/no | no | yes | no |
| Between 3,001-5,000 | 20% | 18 | 4 | 16 | single | no | no/no | no | yes | no |
| Between 5,001-10,000 | 50% | 18 | 4 | 16 | single | no | no/no | yes | yes | yes |
| Between 10,001-15,000 | 55% | 22 | 6 | 20 | single | no | no/no | yes | yes | yes |
| Between 15,001-20,000 | 55% | 22 | 6 | 20 | runoff | no | no/no | yes | yes | yes |
| Between 20,001-30,000 | 55% | 22 | 6 | 20 | runoff | no | yes/no | yes | yes | yes |
| Between 30,001-50,000 | 55% | 36 | 6 | 30 | runoff | allowed | yes/no | yes | yes | yes |
| Between 50,001-60,000 | 75% | 36 | 6 | 30 | runoff | allowed | yes/no | yes | yes | yes |
| Between 60,001-100,000 | 75% | 36 | 6 | 30 | runoff | allowed | yes/yes | yes | yes | yes |
| Between 100,001-250,000 | 75% | 36 | 10 | 40 | runoff | yes | yes/yes | yes | yes | yes |
| Between 250,001-500,000 | 75% | 36 | 12 | 46 | runoff | yes | yes/yes | yes | yes | yes |
| Over 500,001 | 75% | 36 | 14-16 | 50-60 | runoff | yes | yes/yes | yes | yes | yes |

*Notes*: Notes: Population is the number of resident inhabitants as measured by the last available Census. Wage Executive committee represents the monthly gross wage of the members of the executive committee, expressed in percentage terms respect to the gross monthly wage of the mayor (column 3 of Table 2). Fee Council is the reimbursement per session paid to councillors, measured in Euros. The wage thresholds at 1,000 and 10,000 were introduced in 2000; all of the others date back to 1960. Executive committee size is the maximum allowed number of executives appointed by the mayor. Municipal council size is the number of seats in the City Council. All of the size thresholds were set in 1960. Since 1993, Electoral Rule can be either single round (with 60% premium) or runoff (with 66% premium) plurality voting. Neighbourhood Councils are bodies that represent different neighbourhoods within the city and are provided with independent budgets. Hospital/Health captures whether the municipality is allowed to have a hospital or a health-care district. The domestic stability pact was introduced in 1999 and it was reformed in 2013 with the extension of the policy to cities with population between 3,000 and 5,000. Gender quotas have been introduced in 2013.

Table 4: Descriptive statistics

| | Average value | Standard deviation |
|---|---|---|
| **Panel A: Public finance** | | |
| Fiscal revenues PC | 610.5 | 357.0 |
| Real estate revenues PC | 198.8 | 230.9 |
| Municipal surcharge revenues PC | 47.8 | 35.3 |
| Current expenditures PC | 941.5 | 653.0 |
| Capital expenditures PC | 487.8 | 1415.4 |
| **Panel B: municipal surcharge** | | |
| Flat rate dummy | .686 | .464 |
| Exemption dummy | .191 | .393 |
| Multi-rate dummy | .123 | .329 |
| $\tau_1$: tax rate 0-15,000 (%) | .371 | .284 |
| $\tau_2$: tax rate 15,000-28,000 (%) | .484 | .282 |
| $\tau_3$: tax rate 28,000-55,000 (%) | .497 | .285 |
| $\tau_4$: tax rate 55,000-75,000 (%) | .507 | .289 |
| $\tau_5$: tax rate 75,000-120,000 (%) | .513 | .294 |
| First tax gap: $\tau_2 - \tau_1$ (%) | .114 | .199 |
| Second tax gap: $\tau_3 - \tau_2$ (%) | .013 | .044 |
| Third tax gap: $\tau_4 - \tau_3$ (%) | .009 | .037 |
| Fourth tax gap: $\tau_5 - \tau_4$ (%) | .007 | .025 |
| Surcharge revenues $\tau_1$ PC | 27.5 | 21.0 |
| Surcharge revenues $\tau_2$ PC | 96.3 | 56.4 |
| Surcharge revenues $\tau_3$ PC | 166.2 | 95.6 |
| Surcharge revenues $\tau_4$ PC | 283.1 | 204.4 |
| **Panel C: political background** | | |
| Municipal turnout (%) | 71.2 | 9.9 |
| Mayor's education | 4.4 | .693 |
| Mayor's age | 50.1 | 10.4 |
| Mayor's gender (female) | .130 | .337 |
| Mayor white collar dummy | .321 | .467 |
| Mayor employee dummy | .608 | .488 |
| Mayor self-employed dummy | .326 | .469 |
| Av. elected education | 4.1 | .347 |
| Av. elected age | 44.7 | 4.4 |
| Proportion female elected | .256 | .132 |
| Vote share most voted candidate (%) | 59.3 | 17.8 |
| **Panel D: general information** | | |
| Population | 7,431.1 | 41,805.9 |
| Number of taxpayers | 3,742.1 | 21,751.4 |
| Aggregate declared amount (mln) | 91.2 | 680.0 |
| $N$ | 48,379 | |

*Notes*: The variable *Aggregate declared amount* is expressed in millions of Euros, the variables $\tau_1$, $\tau_2$, $\tau_3$ and $\tau_4$ are expressed in percentage terms. The variables *Surcharge revenues* $\tau_1/\tau_2/\tau_3/\tau_4$ are in Euros per-capita. Public finance variables in Panel C are expressed in Euros and in per-capita terms.

## Table 5: RDD analysis on rate differences

| **Panel A**: Cities with **positive surcharge** | First rate gap $\tau_2 - \tau_1$ | **Second rate gap** $\tau_{\mathbf{3}} - \tau_{\mathbf{2}}$ | Third rate gap $\tau_4 - \tau_3$ | Fourth rate gap $\tau_5 - \tau_4$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| RD_Estimate | -0.0383 | -0.00978 | -0.0000416 | 0.000469 |
| | (0.0249) | (0.00514)* | (0.00560) | (0.00433) |
| Outcome variable mean | .144 | .016 | .012 | .008 |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,600.4 | 2,293.5 | 1,892.8 | 1,825.9 |
| N | 5,619 | 8,486 | 6,791 | 6,516 |
| **Panel B**: Cities with **flexible rates** | | | | |
| RD_Estimate | -0.0326 | -0.0307 | 0.00914 | 0.00860 |
| | (0.0465) | (0.0149)** | (0.0183) | (0.0140) |
| Outcome variable mean | .298 | .077 | .062 | .043 |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,600.4 | 2,293.5 | 1,892.8 | 1,825.9 |
| N | 1,279 | 1,900 | 1,571 | 1,494 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 6: RDD analysis on second rate gap - Heterogeneity analysis (only cities with flexible rate)

| | Dep. variable: Second rate gap ($\tau_3 - \tau_2$) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Panel A**: Mayor's characteristics | **Low** education | **High** education | **Under-40** | **Over-40** | **Male** | **Female** | **Employees** workers | **Self-employed** workers |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| RD_Estimate | 0.0125 | -0.0325 | -0.0431 | -0.0248 | -0.0303 | -0.0278 | -0.0275 | -0.0538 |
| | (0.0240) | (0.0153)** | (0.0197)** | (0.0192) | (0.0168)* | (0.0209) | (0.0189) | (0.0174)*** |
| Outcome variable mean | .077 | .077 | .077 | .077 | .077 | .077 | .077 | .077 |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,981.4 | 2,332.6 | 1,480.7 | 1,941.8 | 2,447.3 | 1,984.3 | 2,309.2 | 2,347.5 |
| N | 130 | 1,791 | 272 | 1,269 | 1,753 | 261 | 1,225 | 647 |
| **Panel B**: City characteristics | **North** of Italy | **South** of Italy | Fiscal rev. (below median) | Fiscal rev. (above median) | Current Expend. (below median) | Current Expend. (above median) | Capital Expend. (below median) | Capital Expend. (above median) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| RD_Estimate | -0.0155 | -0.149 | -0.0295 | -0.0284 | -0.0304 | -0.0264 | -0.0150 | -0.0471 |
| | (0.0163) | (0.0520)*** | (0.0191) | (0.0143)** | (0.0206) | (0.0157)* | (0.0173) | (0.0143)*** |
| Outcome variable mean | .077 | .077 | .077 | .077 | .077 | .077 | .077 | .077 |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,857.8 | 1,599.2 | 2,254.9 | 2,492.7 | 1,914.1 | 1,894.8 | 2,203.3 | 2,471.2 |
| N | 1,280 | 198 | 1,037 | 965 | 954 | 637 | 1,029 | 919 |

*Notes*: Estimation by RDD-LLR. Cities included in the sample are those with flexible surcharge rates. The analysis covers the period 2013-2017 and it is limited to the set of cities which introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: RDD analysis - Differences over the electoral cycle

| **Panel A**: Cities with **positive surcharge** | Second rate gap ($\tau_3 - \tau_2$) | | |
|---|---|---|---|
| | All years | Far from elections | Close to elections |
| | (1) | (2) | (3) |
| RD_Estimate | -0.00978 | -0.0130 | -0.00541 |
| | (0.00514)* | (0.00543)** | (0.00518) |
| Controls | Yes | Yes | Yes |
| Bandwidth (h) | 2,293.5 | 2,289.6 | 2,372.3 |
| N | 8,486 | 5,095 | 3,502 |
| **Panel B**: Cities with **flexible rates** | | | |
| RD_Estimate | -0.0307 | -0.0421 | -0.0130 |
| | (0.0149)** | (0.0158)*** | (0.0151) |
| Controls | Yes | Yes | Yes |
| Bandwidth (h) | 2,293.5 | 2,289.6 | 2,372.3 |
| N | 1,900 | 1,148 | 769 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Columns "Far from elections" includes the first three years of the electoral terms, while those "Close to elections" include the last two years of the electoral term. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: RDD analysis on revenues differences

| **Panel A**: Cities with **positive surcharge** | Surcharge revenues Difference - PC (over tax-payers in each bracket) | | | |
|---|---|---|---|---|
| | First revenues gap ($R_{\tau_2} - R_{\tau_1}$) | **Second revenues gap** ($R_{\tau_3} - R_{\tau_2}$) | Third revenues gap ($R_{\tau_4} - R_{\tau_3}$) | Fourth revenues gap ($R_{\tau_5} - R_{\tau_4}$) |
| | (1) | (2) | (3) | (4) |
| RD_Estimate | -3.669 | -1.913 | -1.536 | 4.248 |
| | (2.826) | (1.869) | (5.244) | (8.473) |
| Outcome variable mean | 85.4 | 85.7 | 146.6 | 124.1 |
| Controls | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,351.2 | 1,716.8 | 1,114.7 | 1,272.4 |
| N | 3,762 | 4,890 | 3,109 | 3,576 |
| **Panel A**: Cities with **flexible rates** | | | | |
| RD_Estimate | -7.980 | -8.397 | 1.003 | 13.50 |
| | (6.119) | (4.730)* | (11.82) | (15.95) |
| Outcome variable mean | 93.2 | 103.7 | 217.2 | 208.5 |
| Controls | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,351.2 | 1,716.8 | 1,114.7 | 1,272.4 |
| N | 867 | 1,102 | 732 | 854 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2016. The dependent variable of the analysis is the simulated revenues generated by the municipal surcharge from each income bracket. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 9: Continuity of covariates - Brackets specific characteristics

| **Panel A**: Gross income declared | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | First rate gap | **Second rate gap** | Third rate gap | Fourth rate gap |
| | $\tau_2 - \tau_1$ | $\tau_3 - \tau_2$ | $\tau_4 - \tau_3$ | $\tau_5 - \tau_4$ |
| RD_Estimate | -14623.6 | 632372.5 | -292421.0 | -105496.0 |
| | (442780.1) | (533356.3) | (535996.3) | (73533.0) |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 2,224.8 | 1,850.1 | 1,801.0 | 2,179.6 |
| N | 6,634 | 5,343 | 5,171 | 6,470 |
| **Panel B**: Gross income declared (per-capita) | | | | |
| RD_Estimate | 5.087 | 114.9 | -77.81 | -20.21 |
| | (84.40) | (98.90) | (97.26) | (14.03) |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,365.7 | 1,897.5 | 2,031.9 | 1,528.7 |
| N | 4,747 | 6,826 | 7,367 | 5,317 |
| **Panel C**: Frequency of tax-payers | | | | |
| RD_Estimate | -24.94 | 23.50 | -13.35 | -1.947 |
| | (44.85) | (18.84) | (15.10) | (1.373) |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 2,276.9 | 2,095.1 | 1,990.3 | 2,333.2 |
| N | 2,825 | 6,161 | 5,786 | 7,008 |
| **Panel D**: Frequency of tax-payers (per-capita) | | | | |
| RD_Estimate | -0.00266 | 0.00450 | -0.00262 | -0.000392 |
| | (0.00779) | (0.00434) | (0.00388) | (0.000238)* |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,720.1 | 1,499.3 | 1,238.2 | 1,917.1 |
| N | 4,920 | 4,206 | 3,503 | 5,555 |

*Notes:* Estimation by RDD-LLR. The analysis covers the period 2013-2017 and includes only cities using the municipal surcharge. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 10: Continuity of covariates - Cities specific characteristics

| **Panel A:** Political background | Mayor's education | Mayor's age | Mayor's gender | Mayor white collar | Vote share of winner | Turnout |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RD_Estimate | 0.0244 | -0.460 | -0.0480 | -0.0564 | -0.00630 | 0.00169 |
| | (0.0881) | (1.453) | (0.0402) | (0.0671) | (0.0198) | (0.00988) |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 1,381.2 | 1,696.4 | 2,148.2 | 1,521.7 | 1,904.6 | 1,718.9 |
| N | 1,143 | 1,493 | 1,921 | 1,336 | 1,709 | 1,589 |
| **Panel B:** Local public finance | Fiscal revenues PC | Revenues from real real estate PC | Current expenditures PC | Capital expenditures PC | | |
| | (1) | (2) | (3) | (4) | | |
| RD_Estimate | 3.875 | 14.03 | 44.60 | 13.32 | | |
| | (27.51) | (19.31) | (37.50) | (37.06) | | |
| Covariates | Yes | Yes | Yes | Yes | | |
| Bandwidth (h) | 1,267.3 | 1,342.6 | 1,309.5 | 954.9 | | |
| N | 2,657 | 2,793 | 2,734 | 1,963 | | |

*Notes:* Estimation by RDD-LLR. The analysis covers the period 2013-2017 and includes only cities using the municipal surcharge. The set of controls includes political variables (turnout in last elections and vote share of the most voted candidate) for columns 5 and 6 of panel A and for panel B as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Placebo - Fake thresholds

| **Panel A**: Cities with **positive surcharge** | Threshold 3,000 Second rate gap: $\tau_3 - \tau_2$ | | Threshold 10,000 Second rate gap: $\tau_3 - \tau_2$ | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| RD_Estimate | -0.00478 | -0.00589 | -0.00853 | -0.00562 |
| | (0.00430) | (0.00444) | (0.00818) | (0.00781) |
| Outcome variable mean | .016 | .016 | .016 | .016 |
| Covariates | No | Yes | No | Yes |
| Bandwidth (h) | 1,428.8 | 1,428.8 | 4,083.9 | 4,083.9 |
| N | 10,318 | 9,895 | 6,099 | 5,854 |
| **Panel B**: Cities with **flexible rates** | | | | |
| RD_Estimate | -0.00790 | -0.00920 | -0.0115 | -0.0111 |
| | (0.0150) | (0.0152) | (0.0147) | (0.0146) |
| Outcome variable mean | .077 | .077 | .077 | .077 |
| Covariates | No | Yes | No | Yes |
| Bandwidth (h) | 1,428.8 | 1,428.8 | 4,083.9 | 4,083.9 |
| N | 1,952 | 1,811 | 1,833 | 1,750 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

# Appendix 1: Additional tables

Table 12: RDD analysis on rate differences - Without controls

| **Panel A**: Cities with **positive surcharge** | (1) First rate gap $\tau_2 - \tau_1$ | (2) **Second rate gap** $\tau_{\mathbf{3}} - \tau_{\mathbf{2}}$ | (3) Third rate gap $\tau_4 - \tau_3$ | (4) Fourth rate gap $\tau_5 - \tau_4$ |
|---|---|---|---|---|
| RD_Estimate | -0.0367 | -0.00850 | -0.000404 | 0.000876 |
| | (0.0255) | (0.00501)* | (0.00549) | (0.00413) |
| Outcome variable mean | .144 | .016 | .012 | .008 |
| Covariates | No | No | No | No |
| Bandwidth (h) | 1,600.4 | 2,293.5 | 1,892.8 | 1,825.9 |
| N | 6,130 | 9,306 | 7,401 | 7,093 |
| **Panel B**: Cities with **flexible rates** | | | | |
| RD_Estimate | -0.0426 | -0.0259 | 0.00601 | 0.00843 |
| | (0.0459) | (0.0142)* | (0.0174) | (0.0130) |
| Outcome variable mean | .298 | .077 | .062 | .043 |
| Covariates | No | No | No | No |
| Bandwidth (h) | 1,600.4 | 2,293.5 | 1,892.8 | 1,825.9 |
| N | 1,400 | 2,079 | 1,707 | 1,628 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The set of fixed effects includes year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: RDD analysis on single rate

| **Panel A**: Cities with **positive surcharge** | (1) First rate $\tau_1$ | (2) Second rate $\tau_2$ | (3) **Third rate** $\tau_{\mathbf{3}}$ | (4) Fourth rate $\tau_4$ |
|---|---|---|---|---|
| RD_Estimate | 0.0243 | 0.0000683 | -0.00672 | -0.00284 |
| | (0.0194) | (0.00854) | (0.00788) | (0.00879) |
| Outcome variable mean | .53 | .58 | .59 | .60 |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 2,097.4 | 1,663.4 | 1,665.3 | 1,703.8 |
| N | 7,657 | 5,866 | 5,871 | 6,016 |
| **Panel B**: Cities with **flexible rates** | | | | |
| RD_Estimate | 0.00992 | -0.0209 | -0.0392 | -0.0163 |
| | (0.0346) | (0.0229) | (0.0211)* | (0.0157) |
| Outcome variable mean | .41 | .55 | .62 | .67 |
| Covariates | Yes | Yes | Yes | Yes |
| Bandwidth (h) | 2,097.4 | 1,663.4 | 1,665.3 | 1,703.8 |
| N | 1,732 | 1,340 | 1,340 | 1,373 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender, education), political controls (turnout in last elections and vote share of the most voted candidate) and the average surcharge rate in the previous year as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 14: RDD analysis on second rate gap - Heterogeneity for cities of the South

| **Panel A**: Cities with **flexible rates** | Dep. var.: second rate gap - only **Southern** regions | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | South of Italy | High education | Under-40 | Male | Self-employed worker | Fiscal rev. (above median) | Current Expend. (above median) | Capital Expend. (above median) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| RD_Estimate | -0.147 | -0.165 | -0.479 | -0.162 | -0.166 | -0.175 | -0.105 | -0.174 |
| | (0.0514)*** | (0.0443)*** | (0.00283)*** | (0.0466)*** | (0.0345)*** | (0.0616)*** | (0.0440)** | (0.0403)*** |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth | 1,599.1 | 1,599.1 | 1,599.1 | 1,599.1 | 1,599.1 | 1,599.1 | 1,599.1 | 1,599.1 |
| N | 198 | 75 | 184 | 35 | 171 | 81 | 107 | 113 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A only includes cities that introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender-omitted in column 4-, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 15: Spline polynomial analysis on second rate gap

| **Panel A**: Cities with **positive surcharge** | Dep. variable: Second rate gap | | | | | |
|---|---|---|---|---|---|---|
| | Spline $1^{st}$ | | Spline $2^{nd}$ | | Spline $3^{rd}$ | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Third bracket | -0.00992 | -0.00874 | -0.00787 | -0.00698 | -0.00869 | -0.0107 |
| | (0.00325)*** | (0.00337)*** | (0.00503) | (0.00524) | (0.00643) | (0.00668) |
| Outcome variable mean | .016 | .016 | .016 | .016 | .016 | .016 |
| FE and covariates | No | Yes | No | Yes | No | Yes |
| N | 21,086 | 21,086 | 21,086 | 21,086 | 21,086 | 21,086 |
| **Panel B**: Cities with **flexible rates** | | | | | | |
| Third bracket | -0.0185 | -0.0179 | -0.0250 | -0.0263 | -0.0251 | -0.0283 |
| | (0.00989)* | (0.0104)* | (0.0145)* | (0.0155)* | (0.0177) | (0.0189) |
| Outcome variable mean | .077 | .077 | .077 | .077 | .077 | .077 |
| FE and covariates | No | Yes | No | Yes | No | Yes |
| N | 4,334 | 4,334 | 4,334 | 4,334 | 4,334 | 4,334 |

*Notes*: The analysis covers the period 2013-2017 and focuses on cities with population between 1,000 and 10,000 inhabitants. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The treatment of the analysis is a dummy capturing cities whose mayor pays taxes in the third income bracket. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and region fixed effects. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 16: RDD analysis on revenue generated by the municipal surcharge

| **Panel A**: Cities with **positive surcharge** | Surcharge revenues - PC (over tax-payers in each bracket) | | | |
|---|---|---|---|---|
| | First rate ($R_{\tau_1}$) | Second rate ($R_{\tau_2}$) | Third rate ($R_{\tau_3}$) | Fourth rate ($R_{\tau_4}$) |
| | (1) | (2) | (3) | (4) |
| RD_Estimate | 1.507 | 0.181 | -1.829 | -6.338 |
| | (1.477) | (1.821) | (2.959) | (7.361) |
| Controls | Yes | Yes | Yes | Yes |
| Bandwidth | 2,098.0 | 1,642.6 | 1,645.0 | 11,88.4 |
| N | 6,166 | 4,652 | 4,672 | 3,344 |
| **Panel A**: Cities with **flexible rates** | | | | |
| RD_Estimate | 0.131 | -4.560 | -13.06 | -23.96 |
| | (2.727) | (4.855) | (7.532)* | (15.35) |
| Controls | Yes | Yes | Yes | Yes |
| Bandwidth | 2,098.0 | 1,642.6 | 1,645.0 | 11,88.4 |
| N | 1,385 | 1,064 | 1,064 | 803 |

*Notes*: Estimation by RDD-LLR. The analysis covers the period 2013-2016. The dependent variable of the analysis is the simulated revenues generated by the municipal surcharge from each income bracket. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.