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**Working Paper**

# Does progressivity always lead to progress? The impact of local redistribution on tax manipulation

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## Abstract

The goal of this paper is to study the effects of introducing income redistribution at the municipal level, with the adoption of local tax progressivity. In particular, we analyse whether this complex fiscal tool modifies the incentives of local politicians to be strategic leading to higher tax manipulation, in the form of political budget cycle. We exploit an Italian reform of the local personal income tax (PIT), which was flat before the intervention, that allows mayors to introduce progressive schemes. First, we make use of the staggered timing of local elections to estimate a Difference-in-Differences model and we find that the reform consistently amplifies political budget cycle of local PIT. In terms of mechanism, progressivity allows mayors to target diverse income groups and to play different strategies: high income rates, indeed, are subject to larger manipulation than the moderate ones. Second, we exploit the fact that income concentration level is a valid predictor for the introduction of progressivity. The main results are confirmed in a Triple-Differences analysis. And finally, we show that manipulation is rewarding from an electoral point of view. These results reveal a negative side of decentralizing income redistribution as it may lead to consistent tax manipulation and large distortions in fiscal policy.

JEL-Codes: D720, E620, H710, P160.

Keywords: tax progressivity, personal income tax, political budget cycle, tax manipulation, fiscal federalism.

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# 1 Introduction

There is an old debate in Public Finance literature on whether income redistribution should be an exclusive prerogative of the central government or whether it may be efficient to allow some forms of local redistribution. The classic view argues that the center is more efficient than the local administration in carrying out redistributive policies (Musgrave 1959): a central approach, indeed, may avoid the strategic mobility of tax-payers among different fiscal jurisdictions, á lá Tiebout (Tiebout 1956), and the phenomenon of fiscal competition between cities. Nevertheless, there are also valid arguments in favor of decentralized redistribution, according to the decentralization theorem (Oates 1972): a local approach would allow decision makers to exploit strategic information on the local background and this may be welfare improving (Pauly 1973)[1]. It may be possible, for instance, to enact policies that take into account different tastes and preferences across geography, such as the heterogeneity of preferences for redistribution (Alesina Giuliano 2009).

Many countries adopt some forms of local income redistribution[2] and there is evidence that these fiscal tools lead to large sub-national migration (Moretti, Wilson 2017, Roller, Schmidheiny 2017, Agrawal, Foremny 2019) to strategic relocation of high-income individuals (Kleven et al., 2013, 2014) and to mixed effects on income inequality (Agrawal Foremny 2019; Milligan, Smart 2019). Nonetheless, despite the attention devoted to the phenomenon, very little is still known on the political economy of local redistribution, i.e. on how local administrators make use of these tools and whether political incentives play a role.

This is the aim of our paper, which is among the first to provide an empirical evaluation of the political effects of adopting income redistribution locally, with the introduction of a local progressive tax. In particular, we analyze whether politicians make a strategic use of this new tax, exploiting its flexibility and sophistication, in order to get electoral consensus. And whether this leads to fiscal manipulation that we measure in terms of political budget cycle, the strategic distortion of the budget in pre-electoral years (Alesina et al. 1997, Persson, Tabellini 2002).

From a theoretical point of view, the impact of adopting local redistribution on tax manipulation is not clear *ex-ante*. The main models of political budget cycle emphasize the role of information asymmetries on incumbent's competence: the incumbent tries to signal his ability to voters in order to maximize consensus, through budget manipulation (Rogoff, Silbert 1988, Persson, Tabellini 2002). On the one hand, the introduction of a progressive tax, allows the decision-maker to tax differently distinct groups and this could attenuate incentives for fiscal manipulation: signals may be sent with tax reductions targeted to specific groups. The amount of manipulated resources may reduce in equilibrium. On the other hand, this could encourage and ease the practice. First, tax manipulation is now less costly, as it is possible to divert smaller amounts of public resources than with a flat tax. Second, the decision-maker can manipulate the balance sheet less visibly, as the reduction of a single rate is less evident than decreasing the overall tax, with a reduction

[1] This hypothesis is coherent with the subsidiarity principle, which recommends to assign powers, to the extent possible, to governments that are closest to the people (Breton et al. 1998 for a review).

[2] Differentiated local income tax is present in the United States, Swedish municipalities and Swiss cantons and localities (Roller, Schmidheiny, 2017). Moreover, Canadian provinces have the authority to set their own tax bracket and rates (Milligan and Smart 2016).

in the "blame cost" of manipulation (Pelzman 1992). This could result in an exacerbation of the political budget cycle.

In this paper we want to empirically test these hypotheses, exploiting a natural experiment taking place in Italian municipalities. We concentrate on local personal income tax, the municipal surcharge, which is an important source of revenues and a salient fiscal instrument for voters. The tax is determined annually by the mayor and approved by the municipal council at the end of each year. The reform we want to exploit introduced tax progressivity locally: the tax was initially flat, i.e. the marginal tax rate was the same for the entire population, and the reform introduced the possibility to set a progressive rate structure, with different tax rates for distinct income groups and exemption thresholds. This intervention was sudden and unanticipated as it was part of a larger reform approved to face 2011 sovereign debt crisis, with the aim of increasing local revenues and promote fiscal equity.

In order to study the causal effect of the reform on the political budget cycle, we rely on the staggered timing of local elections in Italy: the over 8,000 municipalities can be divided into five groups according to their electoral schedule. The position of a municipality into a group, which determines his political cycle, is due to historical reasons, such as the substitution of war cabinets after the Second World War in 1946, and it is unlikely to be correlated with current trends in fiscal variables. Moreover, in order to validate this hypothesis we evaluate whether public finance variables evolve differently in the electoral cycle, before the reform, for cities in different groups. The main results show parallel fluctuations, disproving the hypothesis of endogenous formation of city groups.

We estimate a difference-in-differences model, comparing the degree of fiscal manipulation before and after the reform, and using fluctuation in the political cycle of the surcharge as the dependent variable. To perform this exercise we make use of several datasets: data on municipal balance sheets and on surcharge rates, information on local income distribution, data on local political background and information on elections. Two main results emerge from the empirical analysis. On the one hand, there is no evidence of surcharge manipulation in the pre-reform period: the fluctuation of the tax over the political cycle is very limited before the reform. On the other hand, after the reform, a significant manipulation emerges as average tax rate fluctuates consistently in the political cycle. The rate gradually decreases as elections approach, reaching the minimum in the pre-electoral year with a reduction between 8.6% and 9.4% compared to the average tax rate[3]. Then, it raises again, reaching its maximum in the post-electoral year. This result suggests that the introduction of progressivity consistently exacerbates tax manipulation in this context: introducing these new tools results in strategic behavior of administrators and in large distortions in fiscal policy. Nevertheless, we estimate a second model in order to complement these results: we make use of the fact that local income distribution works as a mediator for the reform, as progressive rates are more likely to be introduced in cities with more uniform income distribution. We estimate a triple-differences model and estimates confirm previous results: the reform amplifies political budget cycle and income distribution works as a mediator, since tax manipulation increases as incomes are more uniformly distributed.

As a second inspection, to explore the main channels, we perform the same diff-in-diff

[3]This corresponds to an average amount of diverted money between 0.8% and 1% of total fiscal revenues of Italian municipalities.

analysis considering the set of rates imposed to the five income brackets as dependent variables. The main goal is to check whether mayors use tax progressivity to treat differently diverse income groups. The results confirm this idea. On the one hand, tax rates associated to poor income groups are always maintained low in the political cycle, in order to secure their electoral consensus. On the other hand, tax rates of middle-high income groups fluctuate consistently in the cycle, as these are high in the first years of the term and they decrease gradually as elections approach. This suggests that mayors do not want to entirely renounce the fiscal flows paid by these groups and they tax them properly far from the elections; but they also want to favour them with convenient tax rates as elections approach. These results complement the main findings and they suggest that mayors use progressivity to target specific groups and to perform *ad hoc* manipulation, with the aim of maximizing the electoral consensus without reducing too much fiscal revenues.

Finally, we focus on the electoral reward of manipulating the municipal surcharge: in particular, we consider whether the surcharge rate affects the probability that the incumbent is confirmed. The results support this conjecture: first, it emerges that the incumbents that introduce higher surcharge rates are confirmed less frequently; second, it turns out that tax rates in the last years of the term (election and pre-electoral years) are those that affect more re-election probability. Furthermore, we study the political economy of tax flexibility in order to explore whether partisanship affects the extent to which mayors use progressive tax rates. The analysis is set as a close election Regression Discontinuity Design, where we focus on two aspects: first, we find that political mayors tend to use more flexible rates and to introduce more progressive tax schedules compared to civic mayors, which are not officially supported by any party. Second, the same result emerges for the dichotomy left/right: left-wing mayors use more flexible rates and are more keen on introducing progressive tax schemes than right-wing administrators. These outcomes suggest that partisanship affects the use of progressive rates mediating the effect of the reform.

This study is related to several strands of literature in Political Economy and Public Economics. First, it is linked with the large literature which focuses on the phenomenon of political budget cycle. Seminal papers document the presence of consistent balance sheets manipulation along the political cycle with cross-sectional approach. Alesina et al. (1997) focus on a large set of OECD countries, there is convincing evidence for European countries (Mink, De Haan 2006, Efthyvoulou 2012) and Block (2002) finds consistent evidence of deficit manipulation with a sample of Sub-Saharan African countries. In addition, many studies explore the effect of political institutions such as electoral rules (Persson, Tabellini 2002, Chang 2008), governmental form (Brender, Drazen 2005) and the presence of check and balances in the political system (Streb et al. 2009). More recently, the issue has been inspected at the local level. These papers show that manipulation involves all aspects of public finance: Akhmedov, Zhuravskaya (2004) focus on transfers to voters and document an increase before elections; Kneebone, McCKenzie (2001) find that the most visible taxes are reduced before elections and public expenditures are raised; finally, Drazen, Eslava (2008) show that local incumbents modify expenditure composition for electoral purposes, raising targeted expenditure. Moreover, Alesina, Paradisi (2015) explore political budget cycle in the Italian context: they focus on the introduction of a new real estate tax and they document large levels of local manipulation. Nevertheless,

few contributions focus on the causal impact of institutional and administrative features on balance sheet manipulation. Rose (2006) shows that balance budget rules limit manipulation while Benito et al. (2013) find that local politicians often manage to bypass those fiscal rules. Finally, a related paper is Repetto (2016), who studies the impact of a reform that increased voters information, as it forced Italian municipalities to disclose their balance sheets before elections. Findings show that, when balance sheets are published before elections, the strategic fluctuation of investment spending reduces significantly. Our paper contributes to this literature as it is the first analysis to evaluate the causal impact of the design of taxes on the political budget cycle: in particular, we show that the introduction of fiscal progressivity at the local level consistently raises tax manipulation. Given that local progressivity is an issue in several other countries, these results are relevant from the policy-maker perspective and, to a certain extent, could be generalized to other locations.

Furthermore, the paper is linked with the large literature studying the effects of local redistributive tools on mobility and inequality. On the one hand, many papers document tax-induced mobility across jurisdictions for fiscal concerns, with particular attention to high income tax-payers (see Esteller et al. 2017 for a review): there is evidence for Europe (Martinez 2017 and Roller, Schmidheiny 2017 for Switzerland, Agrawal et al. 2019 for Spain, Rubolino 2018 for Italy) as well as for the US and Canada (Piketty et al. 2014, Milligan, Smart 2016, Young et al. 2016). Nonetheless, a group of papers study specific group of top-taxpayers such as star scientists (Moretti, Wilson 2017; Akcigit et al. 2016), athletes (Kleven et al. 2013), or foreigners subject to preferential taxation (Kleven et al. 2014; Schmidheiny, Slotwinski 2015). On the other hand, a consistent group of papers analyze the impact of local redistribution on income inequality with mixed results (Feldstein, Wrobel, 1998;Leigh, 2008; Bruce et al. 2010; Yang, Heim, 2017; Spreen, 2018; Agrawal, Foremny 2019; Milligan, Smart, 2019). Our paper contributes to this literature as it is among the first to document that local redistribution may generate distortions also from the decision-maker perspective: in particular, these tools are very convenient to be manipulated by local politicians leading to fiscal distortions such as the creation of political budget cycle dynamics.

Finally, this paper is connected to the literature that studies the effects of fiscal complexity and visibility. Many papers argue that complexity makes taxes more difficult to evaluate from people perspective, and they study this issue in relation to goods consumption: Chetty et al. (2009) show, in two field experiments, that consumers under-react to taxes that are not salient and Cabral, Hoxby (2012) focus on salience of the property tax. Moreover, other works link tax complexity to strategic behaviour of politicians and unawareness of citizens[4]. Bracco et al. (2013) show, in the Italian context, that as electoral competition raises, local administrators tend to substitute more visible taxes with less salient ones and Bordignon et al. (2015) show that non-term limited politicians tend to manipulate more visible taxes vis á vis the term limited ones. Furthermore, other studies evaluate the effects of the overall complexity of fiscal systems: Awasthi, Bayraktar (2014) show, in a cross-country analysis, that the degree of fiscal complexity is correlated

[4]Oates (1988) studies the concept of fiscal illusion defined as "the notion that systematic misperception of key fiscal parameters may significantly distort fiscal choices by the electorate". Moreover, Dollery, Worthington (1996) formulate the so called output expansion hypothesis where taxes are underestimated by the voters, in fragmented tax systems, and this leads to an expansion of the public budget.

with higher levels of corruption in tax administration; Gratton et al. (2017) focus on legal complexity, induced by political instability, and they show that this is associated to bureaucratic inefficiency. Finally, some studies focus on the impact of tax complexity on compliance and tax evasion (Forest, Sheffrin 2002). Our study contributes to this literature as it evaluates the impact of raising fiscal complexity on politicians incentive to manipulate taxes and to generate political budget cycle dynamics.

The paper is organized as follows. Section 2 presents background information and introduces the reform. Section 3 describes the dataset. Section 4 discusses the effects of the reform. Section 5 presents the Diff-in-Diff analysis and section 6 shows the main results. Section 7 contains Triple-Diff analysis and it presents the main results. Section 8 discusses several robustness checks. Section 9 studies the effects of manipulation on re-election probability. Section 10 focuses on the impact of partisanship on tax flexibility and Section 11 concludes.

# 2 Institutional background

## 2.1 Local income tax

We focus on the Italian income tax, the *IRPEF* (*Imposta sul reddito delle persone fisiche*) which has been introduced in 1974 and has been reformed several times. This tax is a direct and personal tax and its taxable income is the sum of all gross incomes of an individual[5]; the final rate paid by the tax-payer $t$ in municipality $m$, belonging to region $r$, in year $y$ is the sum of three sub-rates:

$$IRPEF^{t}_{mry} = [f_1(Nat_y) + f_2(Reg_{ry}) + f_3(Mun_{mry})] * Taxable\,Income^{t}$$

- The national rate ($Nat_y$): it follows the scheme in force which was modified the last time in 2007[6]. National income rates are progressive and range from 23% to 43%. The amount generated by this portion pertains to the national Treasury;
- The regional surcharge ($Reg_{ry}$), *Addizionale Regionale Irpef*, is approved by the regional parliament every year with a regional law, the rate structure can be progressive or flat with the rate varying in the range 0.9-1.4%. The amount belongs to the region Treasury;
- The municipal surcharge ($Mun_{mry}$), *Addizionale Comunale Irpef*, is set by the municipal council (*Consiglio comunale*) every year and the rate structure can be progressive or flat with the rate varying in the range 0-0.8%. The generated amount pertains to the municipality.

[5]Taxable income is the sum of following incomes: self-employed, employee, capital, land, enterprise and other incomes (including capital gains).

[6]In 2007 left-wing government in charge modified the system of progressive rates with an increase in each bracket rate and in the progressivity of the overall system, moreover the brackets have been slightly modified (*Disposizioni per la formazione del bilancio annuale e pluriennale dello stato-legge finanziaria 2007*).

In this project we will focus on the municipal surcharge which is the municipal component of the *IRPEF*[7]. The tax is decided by the municipal council by December 20th every year, differently the past rate automatically applies for the following year. Moreover, as all income taxes, the rate decided in the year $y$ will be paid in year $y+1$ on taxable income of year $y$.

### 2.1.1 Importance of the surcharge as a source of revenues

The municipal surcharge plays an important role in municipal balance sheet. First, it is an important source of revenues. Figure 1 shows the evolution of three most important municipal taxes as percentage of total fiscal revenues, on average for all Italian municipalities. Three most important local taxes are the property tax (*ICI* before 2011, *IMU* after 2011), the waste tax and the municipal surcharge that account on average, respectively, for 43%, 23% and 7% of total fiscal municipal revenues (for instance fiscal revenue generated by municipal surcharge in 2015 was around 4.3 billions Euro). Second, as emerges from Figure 1, relative importance of municipal surcharge is raising over time, relatively to other taxes that are losing weight: in 2001, two years after its introduction, municipal surcharge accounts, respectively, for 1/10 and 1/6 of property and waste tax, in 2015 it accounts for 1/3 and 1/2 of them.

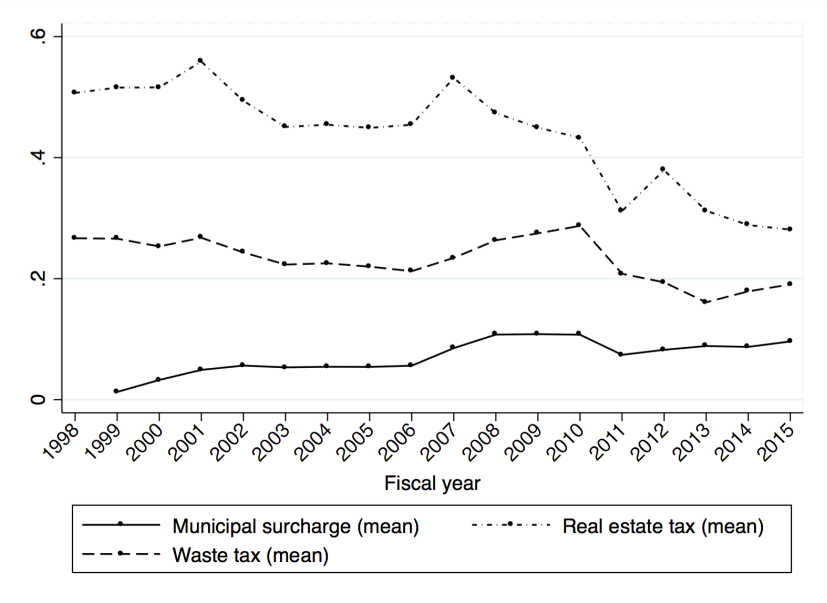



Figure 1: Proportion of fiscal revenues by years of property tax, waste tax and municipal surcharge, as percentage of aggregate fiscal revenues.

Finally few municipalities decide not to introduce the municipal surcharge (setting a rate equal to zero) and the fraction is quickly decreasing over time: it goes from 62.7% in 2000 to 16.1% in 2015.

[7]Income tax surcharge is common in many federations and regional government in Europe such as Spain, Switzerland, Scandinavian countries and United Kingdom (IBFD 2003). Moreover, provinces of Canada apply a surcharge on the yield of the central income tax (Monteduro, Zanardi 2005).

### 2.1.2 Salience of the surcharge for tax-payers

The second important feature of the tax is that it is a salient fiscal instrument for tax-payers. Salience and visibility of taxes are, indeed, important aspects which deeply affect the incentives of administrators to use them (Bracco et al. 2013, Bordignon et al. 2015). In order to capture the attention to municipal surcharge and how it is distributed over time we study Google trend traffic[8] for the keyword *Municipal surcharge* (*Addizionale comunale*) with monthly aggregation, for the time span 2004-2016 for all Italian regions; Figure 2 contains traffic for this Google query; we can note that the attention is very volatile and that a strong seasonality emerges. On the one hand, the first and the most important peak in attention is localized in the month of June of each year and this is due to the fact that the deadline for payment of the main portion of the municipal surcharge (the *saldo*) is in this month[9]. Left panel of Figure 2 underlines this attention peak. We may think that tax payers are very active to seek information on the municipal surcharge in this month because of the incoming deadline. On the other hand, there is a second peak in Google traffic in the month of January and this is probably due to the fact that the municipal surcharge is approved by the council by December 20, and this stimulates attention in following days on this tax. Right panel of Figure 2 shows this second attention peak. A final remark is that traffic levels in 2007 and 2012 are higher than in other years, this is probably due to the fact that surcharge was reformed in those two years.

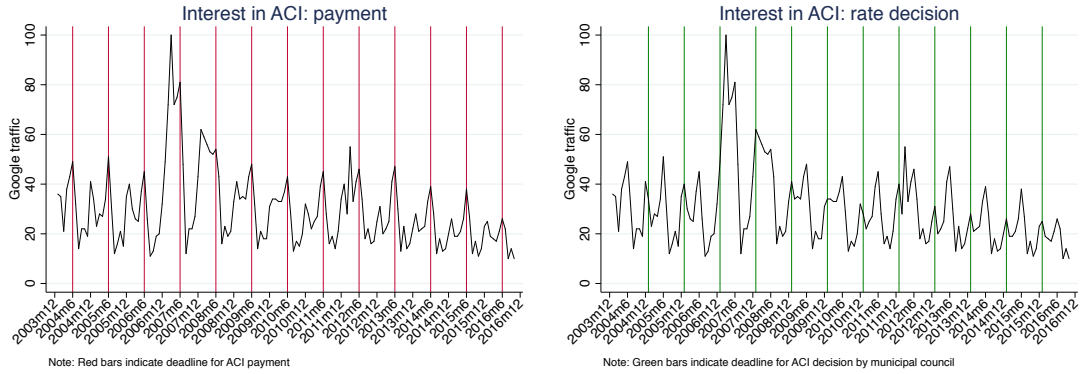



Figure 2: Attention to municipal surcharge in Italy.

As a second inspection, we study whether the attention devoted to this tax is comparable with respect to the interest in other, traditionally more salient, taxes (Bordignon et al. 2015). In particular, we focus on other Italian income taxes (national and regional income taxes) and real estate tax. Left panel of Figure 3 shows Google traffic for keywords *Municipal surcharge*, *Regional surcharge* and *IRPEF tax rates* (respectively *Addizionale comunale*, *Addizionale regionale* and *Scaglioni IRPEF*); the picture seems to suggest that municipal surcharge is the most relevant rate, among other income taxes. Right panel of Figure 3 contains Google traffic for keywords *Municipal surcharge*, *ICI rate* property

[8]Google trend shows a measure of search activity, capturing the fraction of queries that include keywords in the selected geographical unit/window of time, relative to the total number of queries (Stephens-Davidowitz, Varian 2015).

[9]There is another smaller payment, the *acconto*, which is due within the first days of December.

tax until 2011 and *IMU rate* property tax from 2011 (respectively *Addizionale comunale*, *Aliquota ICI* and *Aliquota IMU*). As expected, we can note that real estate taxes are more salient than the surcharge, but the difference in attention is not too wide; the unique exception is the interest in the *IMU* during 2012, when the tax was introduced as an emergency measure by the technical cabinet, and during 2013, when the tax rate on first residency has been abolished by the ruling centre-left government.

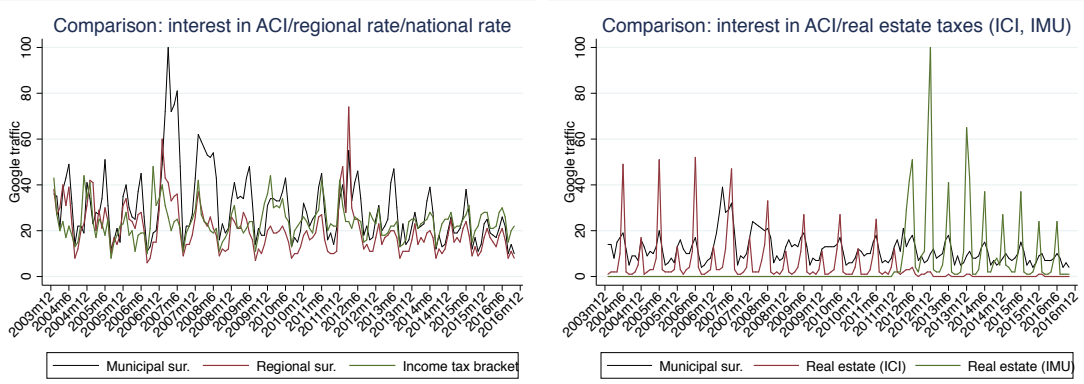



Figure 3: Comparison of attention to municipal surcharge, other income taxes and real estate tax.

Finally, we exploit the Bank of Italy survey "*Balance sheets of Italian families*", wave 2014, in order to shed light on which sub-populations are more interested and aware in the municipal surcharge. We make use of one question asking whether *Municipal surcharge is important for municipal revenues*. Table 1 shows percentage of individuals answering "*yes*" by income brackets: it seems that perceived importance of the surcharge raises with income levels. This is a reasonable evidence since employees and retired people, traditionally the category of tax-payers with middle-low income, receive their wage already net of income taxes, differently from entrepreneurs or self-employed workers, and this could lead to an under-valuation of the importance of the surcharge for this group of people[10]. Second, Table 2 suggests that perceived importance of the surcharge raises with education as well: education is highly correlated with income but also with a better knowledge of the fiscal system as well as of economic and political issues. These pieces of evidence suggest that municipal surcharge is a salient tax and that the relevance of this fiscal instrument varies much across groups of people.

[10]In particular, employers or pension funds are in charge of paying income taxes, directly returning net incomes. Amounts paid remain anyway visible on the wage bill. Moreover, employees or retired people can customize their wage bill, for instance by adding items to be deducted, such as medical expenditures.

Table 1: Percentage of respondents thinking municipal surcharge is important, by income bracket.

| **Income bracket** | mean |
|---|---|
| 0-10,000 € | .327 |
| 10,000-15,000 € | .336 |
| 15,000-26,000 € | .426 |
| 26,000-55,000 € | .580 |
| 55,000-75,000 € | .639 |
| 75,000-120,000 € | .733 |
| More than 120,000 € | .710 |
| | |
| Total | .415 |
| *N* | 13,533 |

Table 2: Percentage of respondents thinking municipal surcharge is important, by education level

| **Education level** | mean |
|---|---|
| No education | .323 |
| Elementary | .302 |
| Lower middle-school | .382 |
| Higher middle-school | .399 |
| High school | .500 |
| Bachelor degree | .525 |
| Master degree | .608 |
| PhD | .540 |
| | |
| Total | .413 |
| *N* | 19,366 |

## 2.2 The reform

### 2.2.1 Evolution of the tax

Municipal surcharge has been modified several times since its introduction and it evolved from a flat tax, where every tax-payers face the same marginal tax rate (proportional system) to a flexible tax, where different groups of tax-payers face different rates (progressive system). We can identify three different fiscal regimes:

- **Regime a** [1999-2006] flat tax: the surcharge has been introduced in 1998 (*Art.1 D.Lgs. n.360*) with the goal to provide municipalities with a more adaptable source of revenues, compared to the local property tax (Bordignon et al. 2015). Municipalities could set a unique tax rate within the threshold of 0.5%, with maximal yearly increase of 0.2%[11].

- **Regime b** [2007-2011] flat tax+exemption: the first reform of the surcharge has taken place in 2007 (*Disposizioni per la formazione del bilancio annuale e pluriennale dello stato-legge finanziaria 2007*) with two main novelties, (1) the possibility for the municipal council to set an exemption threshold, *i.e.* an income threshold below which the surcharge rate is zero and (2) the increase of the maximum allowed rate to 0.8%.

- **Regime c** [2012-2015] differentiated rates: finally, the second reform has taken place in 2012 (*D.L. n. 138/2011*, with modification of the *D.L. n. 201/2011*) and it has introduced the possibility for the municipal council to set differentiated rates

[11] As already anticipated, the payment of the municipal surcharge takes place in two moments of the year: the *saldo*, pertains taxes on previous year income with as deadline first days of July; the *acconto* an anticipation of the following year rate (accounting 30% of total amount), computed with the previous year surcharge: this has to be paid within first days of December (small wages) or in two rates one within first days of December, the other within first days of July (large wages).

following wage brackets of the national income tax. This reform deeply modifies the municipal surcharge from a flat to a completely progressive tax.

In this paper we are interested in studying effects of 2012 reform that introduced tax progressivity at the local level making the municipal surcharge a highly flexible and sophisticated fiscal tool. The reform was a sudden intervention included in the package proposed by the ruling technical government and it had two goals: the increase of municipal revenues and the strengthen of fiscal equity at the local level, providing local administrators with a highly flexible instrument. In these regards, the timing of intervention can be easily considered unrelated to the political cycle of each city as it was mainly motivated to face the Italian sovereign debt crisis in 2011 and 2012. It is unlikely, for instance, that the timing of the reform has been strategically decided by the legislators for political reasons. Nevertheless, the reform was simultaneous to the introduction of the *IMU*, the property tax on the main residency and on commercial properties, I will discuss later why this contemporaneous policy changes are not a problem for my identification strategy.

Finally, two minor interventions introduced a cap to rate increase in two different moments of time (2003-2006 and 2009-2010)[12]: these measures, by the way, were temporary in both cases, and this was publicly known, and their application was limited[13]. I deal with this issue running, as a robustness check, the main specification excluding cap years from the sample.

## 2.3 Local political and economic background

Municipalities are the smallest Italian administrative units; municipal 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 annual budget to be voted by the city council, which include decision on the municipal surcharge[14]. 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.

Municipality revenues come from taxes, fees (*e.g.* public services, advertisement), capital transfers, sales of public assets, borrowing and transfers (from central or regional government or from the European Union). Taxes are the most important source of revenues, for instance they accounted for around 50% of total revenues in 2012. Nonetheless, municipalities are still highly dependent on transfers, mostly from regional or national government (Carozzi, Repetto 2016). In terms of public spending, municipalities 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,

[12]Another minor reform was the increase of the maximum rate to 0.9% for the city of Rome in 2011.

[13]On the one hand, the measures were temporary as in both cases the cap was set until the attainment of an agreement on fiscal federalism application at the local level (*Legge n. 289/2002*, *D.L. n. 112/2008*). On the other hand, the application of the cap was only partial as it involves only two thirds of Italian municipalities

[14]Main responsibilities of the municipal council include overseeing activity of the mayor and the executive committee and approving policies that are proposed.

infrastructure and water supply. Moreover, municipalities are subject to the domestic fiscal rules (*Patto di stabilità interno*) aimed at reducing local debt accumulation and deficit growth; these 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).

Mayors and city councils are replaced with municipal elections every five years (the term lasted four years until 2000). Mayors face a two-terms limit (starting from 2000), but only in case of consecutive terms. A large majority premium is granted to winning mayor in order to ensure local government stability. 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 they take place every year, and this divides Italian municipalities into five groups according to their election year (Coviello, Gagliarducci 2010, Giommoni 2017). Repetto (2016) discusses what determines municipality location in each group: these are mainly historical and pertain the substitution of war councils after Second World War in 1946[15]. On top of this, the peculiar recent history of each municipalities contributed to locate it in a certain group: in particular, a municipality could change group due to early elections caused by early termination, governmental crisis or modification in the law (as it happened in 1993 and 2000). So, it is plausible to consider the position of a municipality in a certain election year as not being correlated with local public finance. I will discus more extensively this issue in section 5.

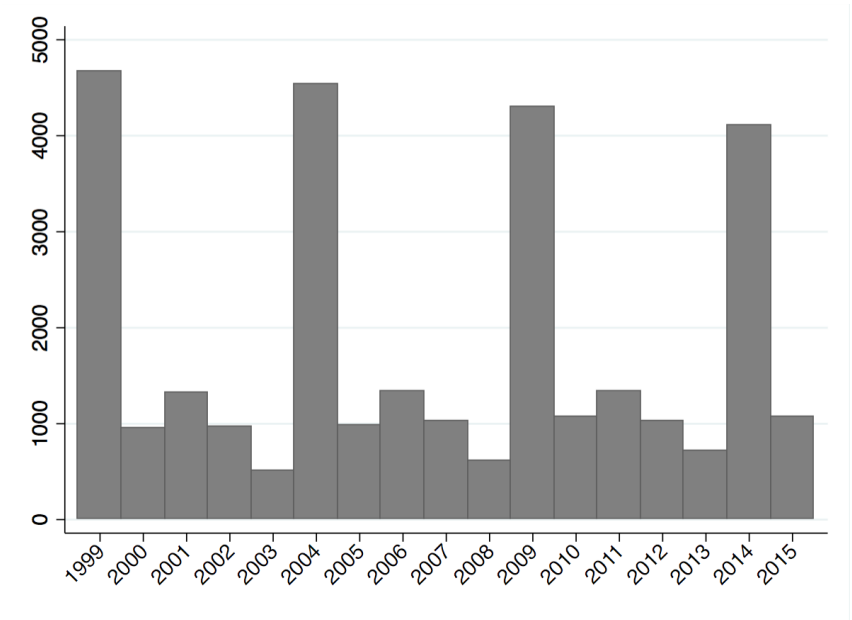



Figure 4: Number of municipalities having elections each year.

[15]In 1946 the first local democratic elections took place in Italy, after the fall of fascist regime: the 71.6% of municipalities voted from March to November 1946. In 1947, 1948 and 1949 elections took place in municipalities with governmental crisis and in cities, such as Bolzano or Gorizia that joined Italy officially after 1946; in 1950 elections to renew local government elected in 1946 were postponed in order to approve a new electoral law (only municipalities with governmental crisis voted in that year) and they took place in 1951.

Figure 4 shows the distribution of all municipalities according to their election year: we can see that the group of municipalities having elections in 1999 is the largest one as it includes more than half of them, among which all the ones that never experienced an early termination; then there are three comparable groups containing around 1,000 municipalities each and finally the smallest group with around 500 municipalities.

# 3 Data

## 3.1 Dataset

The dataset used in this study combined different sources. 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 and expenditures of municipalities: including data on revenues from the main local taxes such as property tax, waste tax and municipal surcharge. Table 3, Panel A, shows descriptive statistics of municipal fiscal revenues: fees (the category including property tax and municipal surcharge) accounts, on average, for 65.7% while taxes (including waste tax) only for 24.3%. Moreover, average per-capita revenue of municipal surcharge is €27.6.

Table 3: Descriptive statistics

| | mean | sd | max |
|---|---|---|---|
| **Panel A: fiscal revenues** | | | |
| Total fiscal revenues | 403.8 | 291.9 | 10,889.3 |
| Total fees | 253.8 | 203.8 | 9,577.4 |
| Total taxes | 97.9 | 78.3 | 2,123.9 |
| Municipal surcharge | 27.6 | 29.4 | 1,420.3 |
| Real estate tax | 172.0 | 177.0 | 9,577.4 |
| Waste tax | 84.5 | 71.0 | 1,518.5 |
| **Panel B: expenditures** | | | |
| Total expenditures | 1,695.2 | 1,884.2 | 104,358.6 |
| Current expenditures | 836.1 | 555.8 | 28,934.0 |
| Capital expenditures | 604.4 | 1,359.3 | 75,800.9 |
| Debt expenditures | 114.5 | 238.1 | 14,239.1 |
| **Panel C: surcharge rate** | | | |
| Municipal surcharge rate (median) | .295% | .261 | .90 |
| **Panel D: local political background** | | | |
| Municipal turnout | 77% | 10 | 1 |
| Margin of victory | .27 | .27 | 1 |
| Number of candidates | 2.74 | 1.36 | 74 |
| Education of the mayor | 4.29 | .73 | 6 |
| Age of the mayor | 48.86 | 9.79 | 94 |
| Gender of the mayor | .10 | .30 | 1 |
| **Panel E: income concentration** | | | |
| Herfindal Index-income concentration | 24.98 | 10.57 | 100 |
| *N* | 137,560 | | |

*Note*: All amounts in Panel A and B are in per-capita terms and expressed in Euros.

Second, we make use of data on municipal surcharge, from the Italian Fiscal Agency (*Agenzie delle entrate*). These data contain all information on the municipal surcharge: the rate set for each wage group, the (eventual) exemption threshold, the day when city council set the annual surcharge level and eventual additional requirements to get the

exemption[16]. Table 11 (in the Appendix 2) shows average surcharge rates, for the entire time span, depending on the type of surcharge chosen by the municipality: when the tax is flat the average rate is around .42% for the entire population, when it is flat plus an exemption the average rate is higher, .6%, and the average exemption amount is around €10,000. Finally, when the municipality introduces a differentiated rate, the marginal rate raises with the taxable income: the table reports average rate for six wage levels, the rate is small for low incomes, for instance it is .173% for incomes of €1,000, and it is large (close to the maximum) for high incomes, .766% for income of €75,000.

Thirdly, we make use of data on distribution of wage in Italian municipalities over time: the source of the data is the Italian Ministry of Economics and Finance (*Ministero dell'Economia e delle Finanze*). We have data on wage distribution, from yearly declaration of tax-payers to fiscal authority, for every municipality, in the time span 2000-2015. Wage distribution varies considerably across municipalities and across years. Figure 5 shows the aggregate frequency of the median tax-payers over time, for wage groups: the figure suggests that municipalities are heterogeneous in terms of their median tax-payers and that wage distribution changes significantly over time: in 2000 almost all municipalities have their median payer in the wage ranges €0-10,000 or €10,000-15,000, after fifteen years the situation is completely different as the majority of municipalities have their median payer in the group €15,000-26,000 , finally in every year very few municipalities have their median payer in the group €26,000-55,000, while other wage groups are not included in the picture as they never host the median payer. In terms of distribution of population among income groups, the fraction of population in the first four income brackets is very heterogeneous across cities, while the last two groups always represent a very small portion of city population. These evidence are shown in Figure 17 (in the Appendix 1) which shows the distribution of fraction of population in each bracket, averaged over time. Nevertheless, the distribution of the average income raises as we move to higher income brackets and it shifts gradually to the right. Figure 18 (in the Appendix 1) shows the distribution of the average income.

Fourth, we utilize 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 and gender), the votes each candidate gets and voter turnout. And finally, we include data from the Italian Statistical Office (*ISTAT*) on population of municipalities over time.

The sample of the analysis consists of all Italian municipalities, both from ordinary and special regions, and the time span covered is 1999-2015. I keep in the analysis all terms ended prematurely due to a governmental crisis in order to avoid sample manipulation.

## 3.2 Measures of municipal surcharge

The dependent variable of the analysis is the tax rate of the municipal surcharge. On the one hand, we consider an overall rate, to capture the aggregate tax level of a municipality in a certain year. We measure this in two different ways: first, we exploit the revenue side

[16]Sometimes, there are almost 500 cases, the surcharge exemption does not only depend on declared wage, but also on additional requirements, such as the number of components of the family or the age of the tax-payer.

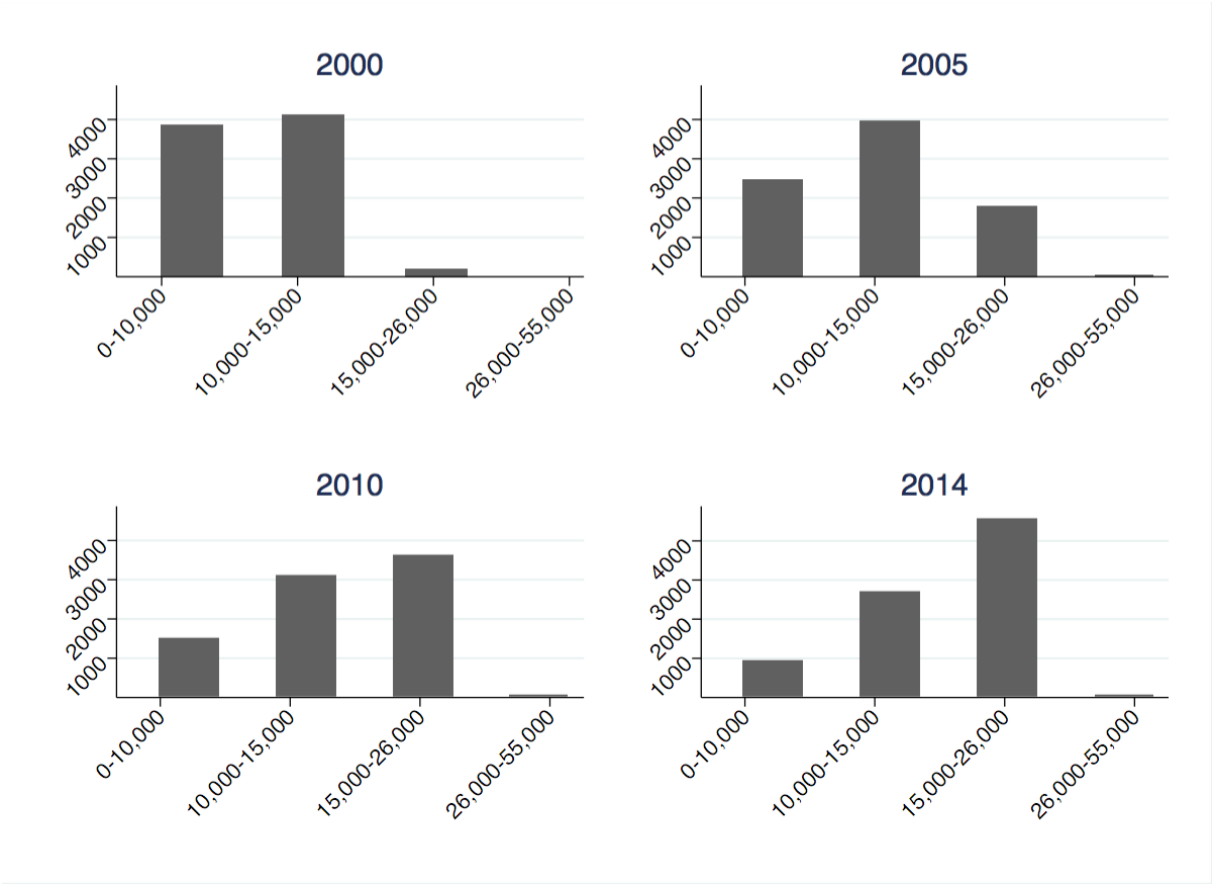



Figure 5: Aggregate frequency of the median tax-payers over time, for wage-groups.

and we adopt the surcharge per-capita revenue; this measure also incorporates economic conditions of the municipality and its evolution over time. Second, we rely on the surcharge rate and we consider the tax rate paid by the median tax-payer[17]. Table 3, Panel A, includes the descriptive statistics of the surcharge per-capita revenue, that has been already commented; Table 3, Panel C, contains the descriptive statistics of the municipal surcharge: the median tax rate has an average value equal to .295% and a similar standard deviation.

On the other hand, the second set of dependent variables includes the rates imposed on different income groups that coincide when the surcharge is flat. Table 12 (in the Appendix 2) shows average tax rates of each bracket, for the sample of cities that introduced a

[17]We also build a third measure which is the weighted average of the surcharge rate, *i.e.* the average rate weighted by the numerosity of each income group. To compute this weighted average we need to make some assumptions on the distribution of wages within each wage group: in order to do that we exploit all available information which is 1) how many people are contained in each wage group and 2) the total wage declared in each group. We generate a synthetic distribution within each wage group that has to be coherent with these two pieces of information. This approach is more precise than simply assuming that tax-payers are uniformly distributed within each group as it does not discard any piece of information. This measure is more precise as it takes into account both wage distribution and rate structure of a municipality. Main results, not shown but available upon request, are confirmed using this additional measure.

flexible rate (exemption of multi-rate scheme) at least once, for the post-reform period. It emerges that, limited to the group in consideration, the tax rate significantly raises with income, as the average rate imposed on the last group (more than €120,000) is almost four times larger than the one of the first bracket (€0-10,000).

# 4 Effects of the reform

## 4.1 Introduction of local progressivity

One important issue to cover is whether municipalities use flexibility that was introduced with 2012 reform: from that year, indeed, each municipalities could choose among setting a flat rate, a flat rate plus an exemption threshold or a differentiated rate. Figure 6 represents the percentage of municipalities adopting some sort of flexibility over time, making distinction between the portion introducing an exemption threshold alone or a differentiated rate. The figure suggests that the adoption of flexibility raises quickly over time: until 2012 few municipalities adopt the exemption threshold; from 2012, instead, the portion raises and more than 30% of Italian municipalities have flexible rate in 2014 and 2015.

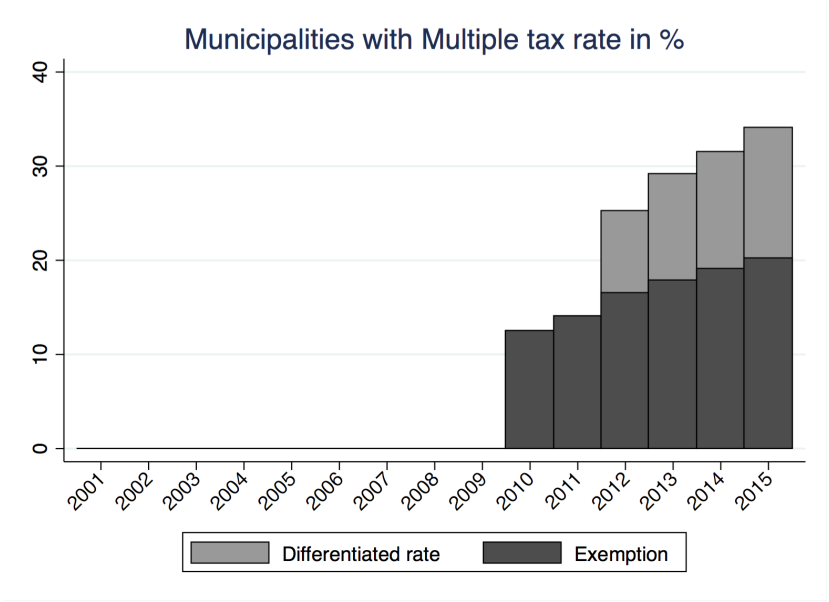



Figure 6: Percentage of municipalities adopting flexible rates (*i.e.* either *Exemption* or *Differentiated rates*), among all cities.

On the other hand, an important issue is how local administrators make use of the flexibility, *i.e.* whether flexible rates are used to decrease rate of certain population groups (for instance low income tax-payers), or to generate a progressive taxation scheme for the entire population. Figure 7 shows average surcharge rate per income level in three fiscal regimes: in regime a (1999-2006), the rate is unsurprisingly flat among income levels; in regime b (2007-2011) the rate is slightly lower for income below €15,000 and it becomes flat for higher incomes; finally, in regime c (2012-2015) the rate follows a

progressive scheme as it raises with income levels. This evidence suggests that flexibility is used by local administrators to structure taxation in a progressive way; on top of this, we can note that the average rate is different across regimes, this will be captured in the econometric analysis by time fixed effects. Nevertheless, we want to explore whether there are differences among municipalities adopting different fiscal methods after 2012 reform: *i.e.* whether marginal rate varies among municipalities that adopt exemption, multi-rate or flat scheme after 2012. Figure 8 focuses on regime c and it shows the average surcharge rate for each scheme introduced. Municipalities choosing the flat tax have the same rate for all tax payers; municipalities with exemption alone have a progressive structure for income below €20,000; finally municipalities with differentiated rates have a progressive scheme.

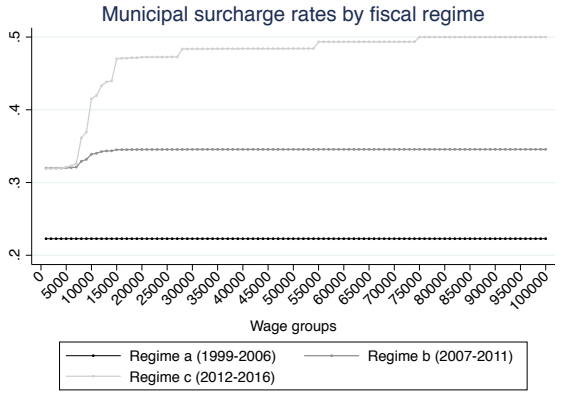



Figure 7: Municipal surcharge per income level, by fiscal regime

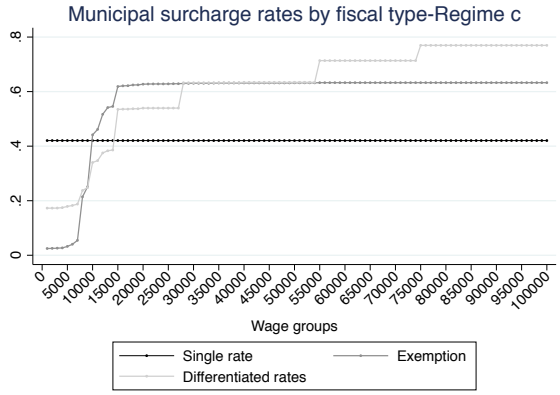



Figure 8: Municipal surcharge per income level in Regime c, by fiscal type

## 4.2 Characteristics of cities introducing progressivity

In this section we want to describe characteristics of cities introducing a progressive rate structure (multi-rate or exemption threshold) after the 2012 reform, in order to have an idea of the set of cities that adopt redistribution at the local level. Figure 19 (in the Appendix 1) includes graphical outcomes of this descriptive analysis: all variables inspected are considered in the last year before the reform, 2011. First, we focus on local GDP per-capita, defined as the aggregate income declared by city tax-payers. Sub-figure (a) of Figure 19 shows the proportion of cities introducing a progressive rate divided in deciles of local GDP and it emerges that the use of progressivity raises with income as richer cities are more likely to introduce exemption thresholds or multirate structures.

Second, we focus on income distribution. Sub-figure (b) divides cities in deciles of income concentration: we constructed an Herfindal Index capturing how concentrated to the left is the income distribution of cities[18], this index can be interpreted in terms of size of middle-high income groups and of income inequality. Large values of the index suggest that income distribution is highly skewed toward the left and indicate cities with

[18]Details on the construction of the index are contained in section 7 where we show that this index is a valid mediator for the introduction of progressivity at the local level.

many tax-payers in the first brackets while small values of the index indicate cities with similar proportion of population in the different brackets, with a more uniform income distribution. From the figure it emerges that the use of progressivity decreases as income concentration raises. Moreover, sub-figure (c) divides cities according to the difference between the average income of a tax-payer in the first and the last bracket: higher values indicate higher level of income inequality in a certain city, and it emerges that the adoption of progressivity raises when this difference widens. These two evidences seem to suggest that mayors of cities with higher income inequality tend to set higher levels of income redistribution. This evidence is not coherent with results of the literature that studies income inequality from a cross-country perspective, which shows that countries with higher levels of income inequality are not always marked by high income redistribution (Piketty, Saez 2003, Piketty, Saez 2007).

Third, we study whether the use of progressivity varies according to variables of local public finance: sub-figures (d)-(j) show these outputs (all variables are in per-capita terms). Cities that are more likely to introduce progressive rates are those with intermediate levels of fiscal revenues, with lower levels of public expenditures and governmental transfers and with intermediate amounts of deficit. Moreover, cities obtaining higher revenues from the municipal surcharge and those more fiscally and financially autonomous tend to introduce local progressivity more likely. Fourth, sub-figure (k)-(m) study political variables: from these outputs it emerges that local turnout and margin of victory in last elections do not seem to be correlated with the decision to introduce progressivity while it emerges that more educated mayors choose local redistribution more often than less educated ones. Finally, progressivity is more likely to be adopted in the centre-north of the country, sub-graph (n), and in large cities, sub-graph (o).

## 4.3 Complementary effects to the introduction of progressivity

Another important issue is whether the introduction of local progressivity may induce complementary modifications to other public finance variables over time. This analysis is important to describe the overall effects of having redistribution at the local level. In order to show these complementary effects we divide cities between those only using flat tax and the group that adopts progressivity: this is a purely descriptive analysis as this choice may be endogenous to local characteristics and these outcomes cannot be interpreted as causal.

Figure 20 (in the Appendix 1) shows the outcomes of this analysis. In terms of aggregate public revenues, there are no differences between the two groups of cities both in terms of fees, sub-figure (a), and taxes, sub-figure (b). But it emerges that there are differences in the composition of public revenues as cities with local progressivity experience an increase in revenues from the municipal surcharge, sub-figure (c), and a complementary decrease in real estate revenues, sub-figure (d), while trend are rather parallel in terms of waste tax, sub-graph (e). This suggests that the introduction of local progressivity is, in aggregate terms, revenue neutral, but it comes along with a modification in the composition of aggregate revenues within cities balance sheets. Finally, there is no evidence of differencial trends in governmental transfers, sub-figure (f), total expenditures, sub-figures (g), current expenditures, sub-figure (h) and capital expenditures, sub-figure (i).

# 5 Empirical strategy: Diff-in-Diff

## 5.1 Identification strategy

In this paper we want to study the causal effect of adopting local progressivity on the strategic use of the tax for electoral reasons, *i.e.* the willingness of decision-makers to adopt the practice of the political budget cycle, with regard to the municipal surcharge. For this analysis the ideal experiment would consist in allowing a group of municipalities, the treatment group, to introduce a flexible surcharge rate opposed to the remaining set of municipalities, the control group, where the rate could only be flat. In this case, the two groups would only differ in the potential progressivity of the surcharge rate and the comparison of levels of political budget cycle, across the two groups, would estimate the effect of interest.

To study this issue in our setting we rely on the 2012 reform as a natural experiment, as it allowed municipalities to introduce progressive tax schemes and that affected them in different positions of their cycle. We want to construct the analysis as a difference-in-differences analysis exploiting two sources of variation (similarly as Repetto 2016). On the one hand, the time variation induced by the reform. On the other hand, we exploit the staggered timing of local elections, which induces variation across cities in the distance from the next polls: this affects the incentive of local politicians to manipulate taxes, higher as elections approach. Therefore, the treatment consists in the possibility of introducing progressive rates and it affects differently municipalities in different position of their cycle, according to the distance to the next elections. The idea at the basis of this identification consists in exploiting both cross sectional variation, with the comparison of cities with different distances from the elections, and time variation, with the comparison of the same city over time. The difference-in-differences estimator is, then, obtained by comparing municipalities of a certain group with municipalities in other groups, and with themselves before the reform. With such design the treatment and the control groups change over time.

The key identifying assumption in this identification strategy is that in the absence of the reform, budget cycle between treated and control groups would be comparable, this guarantees that municipalities of different groups can be used as controls for each others. In order to motivate this, we rely on two arguments. On the one hand, the location of a municipality into a certain group is unlikely to be correlated with current local trends in fiscal variables, in particular in the use of municipal surcharge. This is reasonable because, as already discussed, the position of a municipality in a group is mainly due to historical reasons and to its political history. We will discuss some checks in the robustness section. On the other hand, the reform was part of a larger package that was suddenly approved as an emergency measure to react to the sovereign debt crisis of 2011 and 2012. Therefore, reform timing is unlikely to be strategically set or correlated with municipalities cycles.

To explore the validity of the parallel trend assumption, we perform two distinct tests. On the one hand, we study whether public finance variables of cities that belong to different electoral groups evolve differently along the political cycle, before the reform. On the other hand, we perform the standard test to check for the presence of pre-trends before the reform and we study the fluctuation of the tax rate over the years. These tests suggest that the groups of cities are rather comparable and they confirm that the manipulation emerges after the adoption of local progressivity, ruling out the possibility

of pre-trends in the main results. These analysis will be discussed in section 5.3.

Finally, one potential concern about the reform is the fact that it comes along with other interventions on local finance: in particular, a reduction in transfers from the central authority to local administrations as well as an increase in the portion of local taxes pertaining the central authority and the introduction of a new real estate tax, the *IMU* (Alesina, Paradisi 2015). This can be a concern if these additional interventions affect the dependent variable differently between treated and control groups. We take this into account in the robustness checks section where we study whether the reform affects differently municipalities in different position of their political cycles in terms of main local taxes. The analysis shows that none of these modifies significantly after 2012 between treatment and control.

## 5.2 Specification

The dependent variable varies at the municipality/year level and it captures the surcharge rate introduced by the local administration. On the one hand, we consider the overall surcharge level, using the tax rate of the median payer and per capita revenues, on the other hand, we focus on the set of tax rates assigned to each single income groups. The empirical model of the analysis is as follows:

$$y_{it} = \alpha + \beta_1 \mathbf{C}_{it} + \beta_2' \mathbf{C}_{it} \cdot Post_t + \gamma' X_{it} + \delta_i + \zeta_{mt} + \mu_r \cdot \lambda_t + \epsilon_{it}$$

Where $y_{it}$ is the dependent variable (for example per-capita surcharge revenue) for municipality $i$ in year $t$, $Post_t$ the dummy indicating post-reform years equal to one from 2012 (included) on[19], and $\mathbf{C}_{it}$ characterizes the position in the cycle of municipality $i$:

$$\mathbf{C} = \begin{cases} c_{t-3} = 1 & 3\ years\ before\ elections \\ c_{t-2} = 1 & 2\ years\ before\ elections \\ c_{t-1} = 1 & 1\ year\ before\ elections \\ c_t = 1 & election\ year \end{cases}$$

and zero otherwise, moreover $c_{t+1}$, the indicator for post-election year, is omitted to avoid multicollinearity and it acts as a reference year[20]. $\mathbf{C}_{it}$ characterizes the fluctuation of fiscal variables around the cycle and it varies across municipalities according to allocations in groups; moreover, early terminations leading to premature elections induce $\mathbf{C}_{it}$ to vary within municipalities as well. The Diff-in-Diff estimator is obtained by the interaction term $\mathbf{C}_{it} \cdot Post_t$ which captures the effect of the reform on fiscal behaviour in different position of the political cycle.

[19]The reform in analysis was approved in 2011 and first year of application was 2012.

[20]The choice of using post-electoral year as reference is not arbitrary: it is supported by two arguments. First, post-election year is usually the first rate decided by the newly elected mayor: there is evidence, indeed, that is the incumbent, not the new elected mayor, the one usually choosing election year tax rate, despite the new mayor could modify an already decided tax. In particular, old mayors choose surcharge rate in the election year 71% of times while new mayors only 29%. The second evidence is that the rate in electoral year is highly manipulated during the pre-reform period: it is indeed the lowest rate in the cycle compared to all other years. This piece of evidence is also documented by Bordignon et al. (2015) that show this pattern for municipal surcharge during period 1999-2005. Moreover, an additional evidence is that when the incumbent set the rate in election year, he sets on average a lower rate (0.39%) compared to the one set by the new mayor (0.51%); and this seems to suggest that the incumbents choose the surcharge strategically in elections year, compared to newly elected mayors.

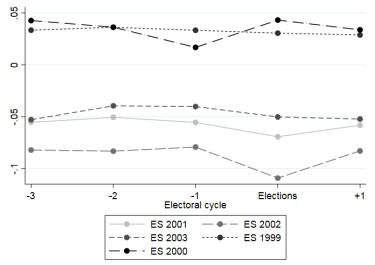
The vector $X_{it}$ contains a set of political, social and economic controls. On the one hand, we include a set of variables to capture local political background: characteristics of mayors such as age, education and gender as well as the dummy whether mandate was completed or ended prematurely and the control for term limit. Finally, we include local turnout in last elections to control for political participation and competition. On the other hand, we control for the economic and social characteristics of municipalities. First, we include data on municipality population over time, in logarithm, to capture for city size and its evolution over time. Second, income distribution at the city level affects consistently the impact of the reform and it is important to control for this aspect. Therefore, we include the following set of control to capture for the local income structure and its evolution: share of taxpayer in the eight wage brackets, total number of taxpayers and yearly aggregate income declared. Moreover, we include municipality fixed effects, $\delta_i$, to control for municipal-specific, time-invariant, unobservables, for macro-region year fixed effects, $\zeta_{mt}$, that account for common shocks in Italian macro-regions over time and for regional specific time trends $\mu_r \cdot \lambda_t$, to capture specific fiscal trends in different Italian regions. Furthermore, we include voting group fixed effects to account for group specific factors. Finally, robust standard errors are clustered at the municipality level and assumed to be uncorrelated with the set of explanatory variable of interest.

## 5.3 Parallel trend assumption

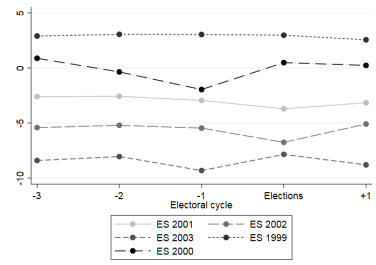
In this section I present evidence in support of the parallel trend assumption. First, we study whether public finance variables of cities in different groups evolve differently along the political cycle. For this exercise we focus on the pre-reform period 1999-2011. In case fluctuations of variables are not similar, this may indicate the presence of substantial differences between groups as they may be the result of a self-selection process. Figure 9 shows the evolution of a set of public finance indicators in the electoral cycle. The graphs refer to the pre-reform period (1999-2011) and variables are reported after cleaning for municipality and macro-region/year fixed effects and the complete set of economic and political controls (as reported in section 5.2). In terms of public revenues (sub-graphs from a to f) the five groups seem to have similar fluctuations with systematic decrease in correspondence of the electoral or the pre-electoral years. The only, partial, exception is the fifth group, having election in 2000. This group contains more than fifty percent of cities belonging to special regions[21], which benefit from particularly high levels of transfers from the national government. Moreover, similar results emerge in terms of public spending (sub-graph g and h), with a clear increase in spending in pre-electoral years (as shown by Repetto 2016). These tests seem to support the parallel trend assumption as cities of different groups are reasonably similar in terms of observable characteristics and their public finance variables have (fairly) parallel fluctuations in the electoral cycle.

[21]Italian special regions are Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sicily and Sardinia, these regions benefit from special forms of legislative, administrative and financial autonomy, laid down in the Italian Constitution.

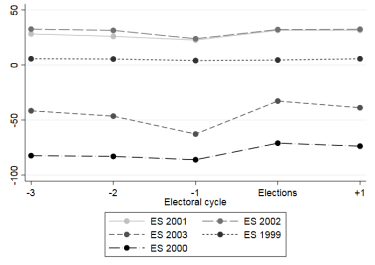
Figure 9: Testing parallel trend assumption: group-specific pre-reform political cycle.



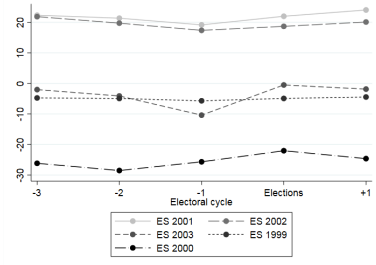

(a) PIT surcharge: median rate



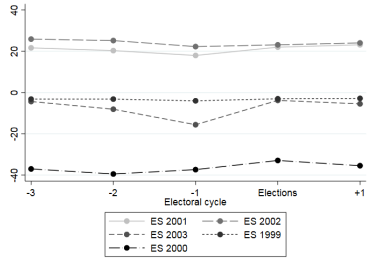

(b) PIT surcharge: revenues (per-capita)



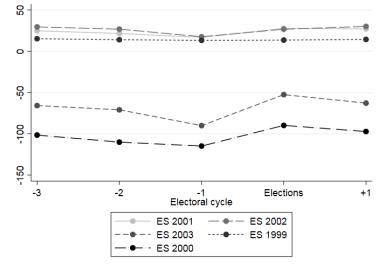

(c) Real estate tax: revenues (per-capita)



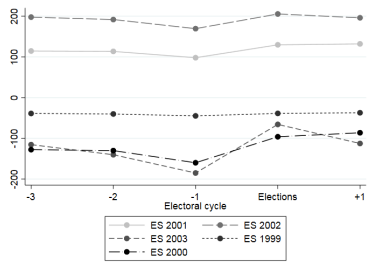

(d) Total revenues (per-capita)



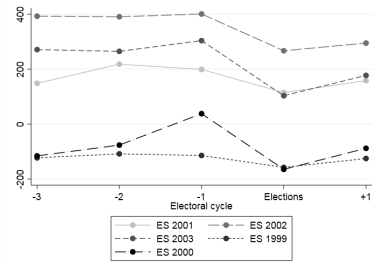

(e) Taxes (per-capita)




(f) Fees (per-capita)




(g) Current expenditures (per-capita)



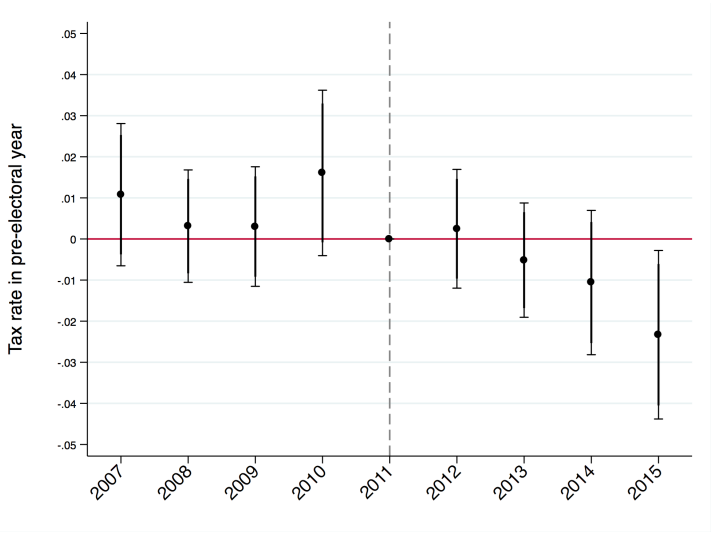

(h) Capital expenditures (per-capita)

*Notes*: pre-reform period 1999-2011. Outcomes are cleaned for municipality and macro-region/year FE, and the complete set of political and economic controls. Municipalities are grouped according to the election year.

Second, we present a test for the presence of pre-trends before the adoption of local progressivity and we evaluate whether the strategic behaviour of mayors takes place already before the reform. In order to do this, we focus on the tax rate in the pre-electoral year, as this is the rate which is more likely to be manipulated by local politicians. So, we test whether this tax rate is different from the ones introduced in other positions of the

political cycle over years[22]. Graphical results of this test are presented in the Figure 10. It emerges clearly that the tax rate in the pre-electoral year was not different from the other rates before the 2012, suggesting the absence of manipulation with the flat regime. Nevertheless, after the adoption of local progressivity the pre-electoral rate is consistently lower than the ones in other years of the electoral cycle and this indicates the emergence of tax manipulation. Moreover, it emerges a clear negative trend after the reform which may suggest that mayors learn how to make use of progressivity over time and they increase the manipulation of the surcharge gradually. This result indicates that there are no pre-trends, taking place before the introduction of local progressivity, in surcharge manipulation.

Figure 10: Pre-trends in terms of the median tax rate in the pre-election year.




*Notes*: This graph shows the evolution of the median tax rate in the pre-electoral year over time. The year before the reform, the 2011, is set as zero and works as a benchmark. The set of fixed effects in the regression include municipal, group-year, macro region-year and region specific-electoral cycle fixed effects. Moreover the sample is restricted to the set of cities that adopt the municipal surcharge at least once in the time span in analysis. The graph includes point estimates and the 95% and 90% confidence intervals.

[22]The empirical model estimated for this test is as follows:

$$y_{it} = \alpha + \beta t + \gamma t * c_{it} + \delta' X_{it} + \epsilon_{it}$$

With $y_{it}$ the tax rate of the median tax payer, $t$ the set of year fixed effects and $t * c_{it}$ the interaction between year fixed effects and the dummy capturing the pre-electoral year. Finally, $X_{it}$ contains the following set of fixed effects and controls: electoral group-year fixed effects and macro region-year fixed effects to capture, respectively, for the different evolution of fiscal policy across electoral groups and macro-regions, region specific-electoral cycle fixed effects to control for geographical effects differential along the electoral cycle and finally we include the fiscal revenue generated by the surcharge in the previous year and municipal fixed effects. I exclude from the analysis municipalities that never introduce the surcharge in the time span in analysis and and I conduct this check for the period 2007-2015 (however, similar results emerge with the standard time sample 1999-2015). Finally, I set the 2011, the last year before the reform, as the benchmark year of the analysis.

# 6 Results: Diff-in-Diff

## 6.1 The impact of the reform on political budget cycle

In this part we focus on the tax rate to which is subject the median tax-payer and the per capita revenues generated by the surcharge in a municipality. Table 4 shows results of the difference-in-differences analysis for these two dependent variables. Columns (1) and (2) contain estimates for the rate of the median tax-payer, respectively, with municipality and macro-region year fixed effects alone, and adding time trends and economic/political controls. Focusing on column (2), un-interacted dummies describe the fluctuation of the tax in the political cycle before the reform and they show that there is very limited strategic behaviour in this period, as the rate has very moderate fluctuation: tax rate is almost constant in the first three years of the term and positive in relation with the benchmark of the post-election year, then there is a slight decrease in the electoral year, corresponding to an average 0.8% reduction, compared to the tax rate average value. This evidence suggests that politicians don't manipulate much municipal surcharge in the pre-reform period and this is coherent with evidence found by Bordignon et al. (2015) that document very limited political budget cycle for the period 1999-2006.

On the other hand, cycle dummies interacted with post indicator describe the post-reform period. From these outcomes it is evident that the reform remarkably amplifies political budget cycle: the average tax rates gradually decreases as elections approach, it reaches the minimum in the pre-electoral year, it raises again in election years and it has a positive spike in post-election year, when the cycle starts again. Notably, average level of surcharge rate reduces by 0.015%, 0.02% and 0.025% respectively three, two and one years before elections, compared to pre-reform years and by 0.02% in election year, with post-elections year working as a benchmark. Considering the average surcharge rate, these reductions correspond to an average rate decrease of 5.4%, 7.1% and 8.6%, respectively, three, two and one years before elections and of 7.0% in the election year. These results indicate that the reform has a sizeable effect on the extent to which local incumbents manipulate the tax along the cycle. As emerges from the table, the impact of the reform weakens when we include economic/political controls and time trends moving form column (1) to column (2), suggesting that local variable such as political participation and competition or local wage distribution play an important role. Moreover, similar results hold when we use per-capita surcharge revenues as dependent variable (column (3) and (4)): in particular, focusing on column (4), revenues decreases by €2.18, €1.9 and €2.59 respectively three, two and one years before elections and by €2.55 in electoral year (corresponding to a decrease of 7.9%, 6.9%, 9.4% and 9.2% along the political cycle, compared to the per-capita revenue average level).

Figure 11 provides a graphical representation of results from Table 4. Left graphs of Figure 11 simulate the fluctuation of the surcharge over the political cycle in the pre/post-reform periods, plotting average values of the surcharge in differential terms to the post-election year, set as zero and working as a benchmark. Sub-graph (a) focuses on the median tax rate and sub-graph (c) on per-capita revenues. Both graphs show the consistent amplification in the political budget cycle, induced by the reform. Moreover, sub-figures (b) and (d) focus on the marginal effect of the reform, *i.e.* the interaction terms for the two dependent variables in analysis.

These outputs show that tax progressivity consistently exacerbates fiscal manipulation,

Table 4: Effect of the reform on surcharge manipulation

| Dependent variable: **Municipal surcharge** | **Median tax rate** | | **Per-capita revenue** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 3 years before elections | 0.00111 | 0.00207 | 0.160 | 0.264 |
| | (0.000681) | (0.000714)*** | (0.0994) | (0.104)** |
| 2 years before elections | 0.00251 | 0.00475 | 0.189 | 0.350 |
| | (0.000925)*** | (0.000960)*** | (0.113)* | (0.116)*** |
| 1 year before elections | 0.00607 | 0.00252 | 0.596 | 0.269 |
| | (0.000940)*** | (0.000960)*** | (0.114)*** | (0.118)** |
| Election year | -0.00584 | -0.00222 | -0.149 | 0.106 |
| | (0.000707)*** | (0.000790)*** | (0.0977) | (0.104) |
| 3 years before elections*Post-reform | -0.0183 | -0.0158 | -2.187 | -2.183 |
| | (0.00275)*** | (0.00276)*** | (0.358)*** | (0.362)*** |
| 2 years before elections*Post-reform | -0.0232 | -0.0208 | -2.178 | -1.905 |
| | (0.00242)*** | (0.00249)*** | (0.418)*** | (0.439)*** |
| 1 year before elections*Post-reform | -0.0358 | -0.0253 | -3.586 | -2.599 |
| | (0.00263)*** | (0.00271)*** | (0.346)*** | (0.356)*** |
| Election year*Post-reform | -0.0165 | -0.0206 | -2.077 | -2.557 |
| | (0.00215)*** | (0.00224)*** | (0.410)*** | (0.428)*** |
| Mean of the dep. variable | 0.295 | 0.295 | 27.6 | 27.6 |
| Municipality & Macro-region Year FE | Yes | Yes | Yes | Yes |
| Time trends & Political/Economic controls | No | Yes | No | Yes |
| Observations | 133,146 | 127,809 | 131,339 | 126,198 |
| Adjusted $R^2$ | 0.762 | 0.768 | 0.700 | 0.708 |

*Notes*: Dependent variables are the three measures of the surcharge. The specification includes municipality and macro-region/year fixed effect as well as region specific time trend, log of population, characteristics of mayors (*i.e.* education, age, proportion of women), turnout in last elections, dummy for a full mandate, dummy for term limit and variables to characterize income distribution (*i.e.* fraction of population in all wage brackets, total number of tax-payers and total amount of income declared). Moreover, electoral groups fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

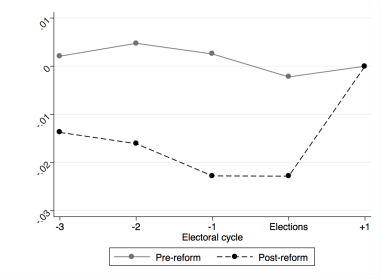
leading to higher levels of political budget cycles and it stimulates the strategic behaviour of local administrators. These results are likely to be due to the fact that manipulation is now easier, more affordable and harder to identify: on the one hand, allowing mayors to introduce progressive rates makes political budget cycle potentially cheaper in terms of public budget, as it is now possible to decrease taxes only to a portion of tax-payers. Moreover, manipulation became less visible as the reduction of a single rate is less evident than reducing the overall tax rate. And this is likely to reduce the "blame cost" associated with tax manipulation (Pelzman 1992). On the other hand, it is possible to tax differently different groups of tax-payers and this allows mayors to favour specific set of voters. Thus, the possibility of doing targeting may make this type of tax manipulation more effective in order to obtain political consensus. Testing whether different groups are manipulated differently is the aim of the next section.

## 6.2 The manipulation of single income groups

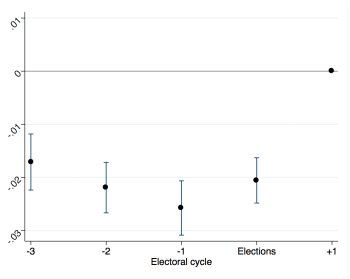
The main results from the previous section indicate that the reform amplifies the strategic use of the municipal surcharge and it exacerbates the practice of political budget cycle. We want now to study whether mayors treat differently tax rates of single income brackets and, in particular, whether they target distinct income groups with different forms of manipulation. With this aim, we perform the same analysis as in section 6.1, using as dependent variables the set of tax rates introduced to the five income groups.

Figure 12 shows the main results graphically: on the one hand, a single line represents the pre-reform period as the tax was flat and every income group has the same tax rate, the

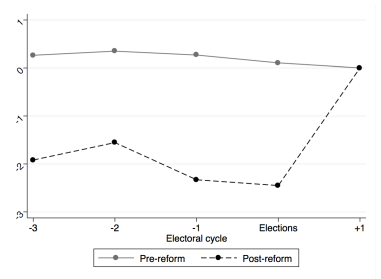
Figure 11: Fluctuations of the municipal surcharge along the political cycle



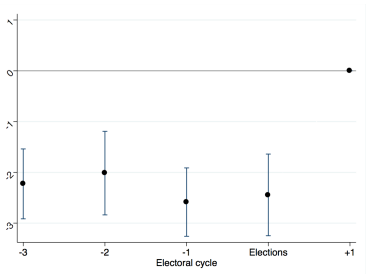

(a) Median rate: PBC




(b) Median rate: marginal effect




(c) Per-capita revenue: PBC




(d) Per-capita revenue: marginal effect

*Notes*: The figure plots coefficients of columns (2) and (4) of Table 4. Left graphs show simulated tax rates along the political cycle: "Pre-reform period" line describes the cycle before the reform, 1999-2011: coefficient $\hat{\beta}_1$ of regression model; "Post-reform period" focuses on period after the reform, 2012-2015: coefficient $\hat{\beta}_1 + \hat{\beta}_2$ of regression model. Budget cycles lines are in deviation from the average level in the post-electoral year. Right graphs show the marginal effect of the reform on the tax rates in each position of the political cycle, *i.e.* the coefficient $\hat{\beta}_2$ of the regression model.

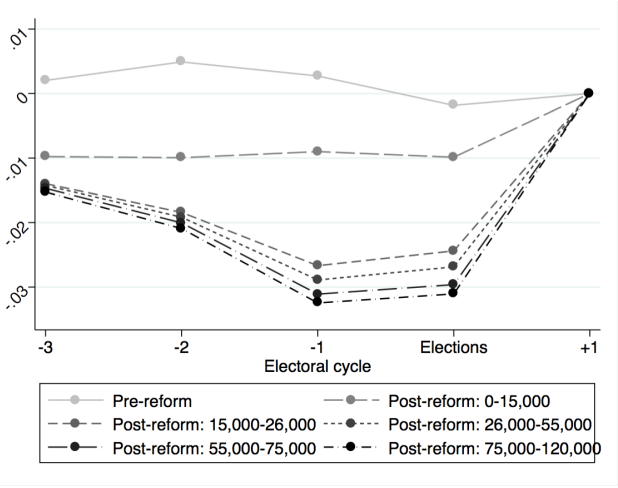
results are confirmed as manipulation is very limited in this period[23]. On the other hand, we have five lines in order to describe the post-reform period, one for each income bracket: first, we can note that the tax rate associated to the first income bracket, €0-15,000, does not fluctuate much, meaning that mayors do not manipulate tax rate associated to low income tax-payers, second it emerges that tax manipulation raises with income and that tax rates associated to higher income groups (€15,000-26,000, €26,000-55,000, €55,000-75,000) have gradually higher fluctuation along the electoral cycle. This suggests that mayors play different strategies with different income groups since they tend to be more strategic with groups associated to high income levels[24]. Moreover, Table 12 (in the Appendix 2), shows the average tax rate associated to each income group, focusing on the post-reform period, and it emerges that the surcharge rate raises quickly with income. This suggests that the average strategy played by the mayors implies that tax rates of low income groups are constantly low in every position of the electoral cycle. While rates of the middle-high income groups are high when election are far, and voters are not responsive to fiscal cuts, and they are strategically low as elections approach, and they

[23]In the pre-reform period there are only tiny differences in tax rates of different income groups, due to the few municipalities that introduced exemption threshold in the period 2007-2011. Anyway, these are very few cases that only affects marginally the fluctuation of each single rates.

[24]These outputs are contained in Table 13 in the Appendix 2: the differential impact of the reform on single income brackets rates is evident as, for instance, the marginal effects in the pre-electoral year on the last income bracket, more than €120,000, is more than three times larger than that on the first income group, €0-15,000.

are very responsive. These results complement those of Drazen, Eslava (2008) who find that local administrators change the composition of public spending before elections with an increase in targeted components to please voters and those of Kneebone, McCKenzie (2001) showing that more visible expenditures are more likely to be manipulated. Our results, instead, are among the first to document that there is targeting on the revenue side and we are able to detect which groups are targeted and to compute the magnitude of the electoral favour. Finally, these results are confirmed by Figure 21 (in the Appendix 1) that shows the marginal effects for the five income groups: first, the tax rate of the first income group varies little in the cycle, second the fluctuation consistently raises for larger income groups with the rate reaching the minimum in the pre-electoral year[25].

Figure 12: Fluctuations of the municipal surcharge along the political cycle: single rates




*Notes*: This figure shows the fluctuations of surcharge rates along the political cycle for single wage brackets in Euros (columns 1-6 of Table 13): pre-reform line shows the fluctuation of the (unique) rate before the reform, 1999-2011, with the plot of $\hat{\beta}_1$ coefficient of regression model. Post-reform lines show fluctuation of single brackets rates, after the reform, 2012-2015, with the plot of $\hat{\beta}_1 + \hat{\beta}_2$ coefficient of regression model. Budget cycles lines are in deviation from the average level in the post-electoral year.

These results confirm that tax progressivity stimulates fiscal manipulation leading to higher levels of political budget cycle, but they also suggest that the reform allows mayors to apply different manipulative schemes to single income brackets and to target specific groups of tax-payers. In particular, it emerges that local politicians apply two parallel strategies. On the one hand, tax rates associated to low incomes are maintained low in all positions of the electoral cycle, in order to secure electoral consensus of these groups. On the other hand, the strategy played on middle-high income groups is more sophisticated. Their tax rates, indeed, fluctuate consistently in the cycle, as these are high in the first years of the term and they decrease gradually as elections approach. This suggests that

[25]Figure 22, in the Appendix 1, shows the marginal effect considering the tax rate introduced to each single thousands of income, for four position in the electoral cycle: three, two, one year before elections and election year. First, we can note that the marginal effect of the reform gets larger as income raises, there are regular drops after certain income levels, in correspondence of national income brackets. Moreover coefficients decline quickly in the left part of the graph, due to the presence of exemption thresholds that can be set freely by the administration. Second, the effect is larger as elections get closer, it reaches the minimum in the pre-election year and it raises again in the election year.

mayors do not want to entirely give up to the fiscal flows paid by these groups and they tax them properly far from the elections; but they also want to favour them with convenient tax rates as elections approach. Moreover, as confirmed by Table 1, middle-high income groups are more likely to be aware about the surcharge and they may, therefore, be more responsive to tax cuts. This may explain why mayors do want to manipulate also their tax rates. In section 9 we will focus on this issue and we will show that middle-high income groups are more responsive to electoral favours: in particular, the re-election probability of the incumbent seems to benefit more from tax cuts to middle-high groups than to poor ones.

### 6.3 Heterogeneous effects

In this section we want to discuss whether there are heterogeneous effects in the impact of the reform on tax manipulation, with a particular focus on whether local income distribution plays a role in decisions of mayors. This means testing whether administrators take into account information on the local background when they make decisions on local taxes. On the one hand, we study whether the frequency of tax-payers in each income brackets affects the manipulation of that rate. Indeed, it is plausible to expect that mayors have higher incentives to please larger groups that ensure more electoral consensus. In particular, we divide cities in quartiles according to the proportion of population in each group, making use of pre-reform (2011) income distribution: Figure 23 (in the Appendix 1) shows the main results. First, there are not differential effects across quartiles for the first two income groups (€0-15,000, €15,000-26,000): this means that the extent to which low income brackets are manipulated is not affected by the numerosity. Second, clear heterogeneous effects emerge considering the last three income brackets (€26,000-55,000, €55,000-75,000 and €75,000-120,000): in particular, it seems that post-reform manipulation raises with the fraction of population. This suggests that mayors are more strategic when a group contains larger portions of tax-payers, and this is due to the fact that manipulating larger groups is more rewarding from an electoral point if view. These results shed further light on the optimal strategy of Italian mayors: it seems that they take into account information on the local income distribution to calibrate their strategies, but only for high income tax brackets, while no heterogeneous effects emerge considering low incomes. This result confirms the subsidiarity principle (Oates 1972, Pauly 1973), *i.e.* the capacity of local politicians to exploit their strategic knowledge of the local background to make decisions about policies, but it also suggests that mayors use their information to improve the manipulation of fiscal instruments. Therefore, in this setting, allowing mayors to exploit their better knowledge may lead to large distortions as this information is used for strategic goals.

As a second analysis, we focus on the distribution of income within each bracket, and whether this affects the level of manipulation, considering the bracket-specific average value of declared income. The effect is ex-ante ambiguous: on the one hand, people in a group with an higher level of declared income are happier of a tax cut and they may be more responsive in following elections. Nevertheless, decreasing the rate of a rich group is clearly more expensive for the public budget. Therefore, it is not clear how the incentive to manipulate the surcharge relates to this characteristic. Figure 24 (in the Appendix 1) shows the graphical outputs for this analysis: also in this case, we divide cities in quartiles,

according to the average income declared in each bracket, making use of the pre-reform (2011) income distribution. From the results does not emerge any clear pattern both for low and for high income groups. We can conclude that the average income declared in each bracket does not seem to affect the extent to which mayors manipulate the surcharge.

# 7 Alternative empirical strategy: Triple-Diff

## 7.1 Income distribution as reform mediator

In this section we propose an alternative, more robust, identification strategy with the aim of corroborating the results found in section 6. This new strategy relies on the idea that income distribution of a municipality mediates the effect of the reform. In particular, the reform introduces the possibility to set different rates and its potential impact is higher where income distribution is highly uniform along the income brackets, compared to places where it is highly concentrated. In the latter case, indeed, most of the population belongs to one single group and mayor's capability to set different rates to different portions of population is rather limited. Figure 13 presents an example: left panel shows the distribution of tax-payer population across the seven wage brackets in 2011 for the city of Segrate, an Italian municipality located in the north of the country. The income distribution is highly uniformly distributed (low income concentration), the reform has a sizeable potential impact on this municipality. On the other hand, right panel of Figure 13 shows income distribution of a similar municipality, Vittoria, from southern Italy, that is highly concentrated in the first bracket: the impact of the reform in this municipality is fairly limited as two thirds of the population belongs to few single groups and the mayor cannot differentiate much income tax rate across the population. An alternative explanation for the reason why income distribution may mediate this effect is that in cities similar to Segrate, middle-high income groups are larger and they represent a consistent electorate, differently from the case of Vittoria. Thus, the incumbent may be more incline to introduce multi-rate structure in order to manipulate middle-high income rates and to attract their votes in following elections. Nevertheless, the degree of income concentration is also correlated to the degree of income inequality which may be another way of reading this relation.

We want to exploit this variation, generated by income distribution at the local level, to better identify the effect of the reform on the strategic use of the surcharge. In these regards, we make use of data on income distribution in 2011, the most recent predetermined income distribution, to be sure this characteristic is not endogenous with the reform itself. In order to measure income concentration at the local level, we rely on an Herfindal index of concentration, that we calculate as follows:

$$H_{i,2011} = \sum_{j=1}^{7} s_{ji,2011}^2$$

with $s_{ji,2011}$ the share of population in wage bracket $j$ of municipality $i$ in year 2011, higher values of the index correspond to higher levels of concentration and so lower potential impact of the reform[26]. Furthermore, we rescale the index in the scale 0-100. Figure 13

[26]We compute alternative versions of the concentration index: first, one version including only 6/5/4

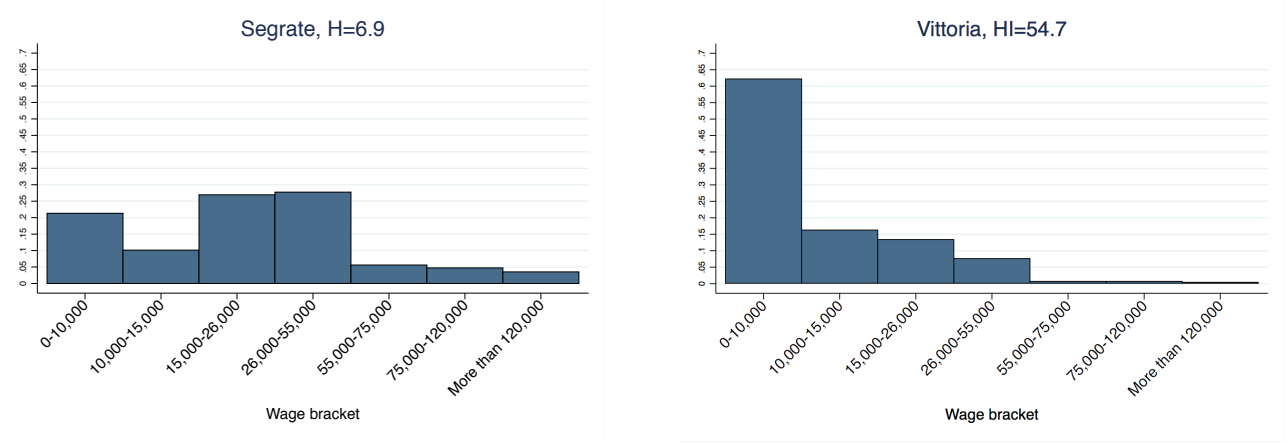



Figure 13: Left panels shows a municipality with uniform income distribution (**low concentration**), right panel shows a municipality with dispersed income distribution (**high concentration**).

shows the index at work: Herfindal index for the city of Segrate (6.9), a city with low level of income concentration, is almost one tenth compared to the one of Vittoria (54.7), which is instead highly concentrated. Panel E of Table 3 shows descriptive statistics of the concentration index computed for the year 2011: average value is 25 with an average variation around 10 points.

Nevertheless, in order to validate that income concentration degree affects decisions on the municipal surcharge, we present in Table 5 some correlations between degree of income concentration and fiscal variables at the local level. We group municipalities in deciles of income concentration: cities in group one have highly uniform distributions while cities in group ten have highly concentrated ones. Several clues emerge from this table: first, as concentration raises the number of municipalities that make use of flexibility decreases: in column 1 this is measured with the percentage of city/years when a flexible rate is introduced, and in column 2 with the portion of cities that set a flexible surcharge at least once. This seems to confirm the original conjecture that more concentrated income distributions mediate the effect of the reform leading to a moderate use of progressive rates in these cities. Second, the number of cities that never introduced municipal surcharge raises as income concentration increases, although the relation is U-shaped; this seems to confirms original conjecture as well (column 3). Finally, taking into account average surcharge rate, both median and weighted averages, it does not emerge any clear pattern: tax rate is high for intermediate values of concentration and it is low for extreme ones (columns 4, 5) and this is motivated by the U-shaped dynamics of the fraction of cities that never set the surcharge higher than zero. In general it seems that, as supposed, municipalities with more concentrated income distributions make a lower use of fiscal flexibility.

groups aggregating richest groups; second, a weighed version where we use as weighs the size of the wage group to capture relative dimension of each bracket. Moreover, we also compute a simpler set of indices which capture the fraction of middle-high income taxpayers in a certain municipality: we generate three version including, respectively, the fraction declaring more than 15,000/26,000/55,000 Euros. Results are similar as the one using the concentration index but magnitudes are weaker (these estimates are not shown and are available upon request).

Table 5: Characteristics of municipalities across income concentration (deciles)

| **Concentration index - deciles** | Flexibility adoption (1) | Flexibility mun. (2) | Never set surcharge (3) | Median surcharge rate (4) | Weighed average surcharge rate (5) |
|---|---|---|---|---|---|
| 1 | .139 | .552 | .174 | .267 | .287 |
| 2 | .129 | .511 | .134 | .291 | .308 |
| 3 | .121 | .491 | .123 | .292 | .308 |
| 4 | .108 | .449 | .107 | .306 | .320 |
| 5 | .091 | .379 | .115 | .315 | .327 |
| 6 | .076 | .323 | .114 | .313 | .322 |
| 7 | .068 | .284 | .135 | .302 | .311 |
| 8 | .053 | .227 | .175 | .284 | .292 |
| 9 | .041 | .183 | .206 | .257 | .256 |
| 10 | .034 | .163 | .243 | .229 | .222 |
| *N* | 137,375 | 137,375 | 137,375 | 137,288 | 137,290 |

*Notes*: *Flexibility adoption* indicates fraction of municipalities that set a non flat rate, *Flexibility mun.* indicates municipalities that set a non flat rate at least once, *Never set surcharge* indicates municipalities that never set the surcharge >0. All variables are in precentage points.

## 7.2 Empirical specification

In the previous section we show that income distribution prior the reform works as a mediator since the use of flexibility raises as distribution becomes less concentrated: we want to exploit this mediating factor in the empirical analysis. We structure the specification as a triple-differences study, where we exploit predetermined variations in the income distribution across municipalities. This new exercise consists in studying the effect of the reform across cities in different positions of the political cycle and marked by different degrees of concentration in their income distribution. The key identifying assumption is now more conservative: we need to rule out the presence of local events contemporaneous to the reform that affect differently cities placed in different positions of their political cycle and characterized by different income distribution: this would be an event taking place in 2012 affecting solely cities in a specific political cycle position and with different impact between cities with uniform income distribution or concentrated one. For the sake of clarity, in this empirical analysis we make use of the version of the index expressed in quartiles of income concentration.

The new empirical model to be estimated is as follows:

$$\begin{aligned} y_{it} = \alpha + \beta_1' \mathbf{C}_{it} + \beta_2 H_{i,2011} + \\ + \beta_3' \mathbf{C}_{it} \cdot Post_t + \beta_4 Post_t \cdot H_{i,2011} + \beta_5' \mathbf{C}_{it} \cdot H_{i,2011} + \\ + \beta_6' \mathbf{C}_{it} \cdot Post_t \cdot H_{i,2011} + \gamma' X_{it} + \delta_i + \zeta_{mt} + \mu_r \cdot \lambda_t + \epsilon_{it} \end{aligned}$$

With $y_{it}$, $Post_t$ and $\mathbf{C}_{it}$ defined as in section 6.2 and $H_{i,2011}$ being the value of income concentration index in municipality $i$ in 2011 (expressed in quartiles of concentration). The specification includes the complete set of double interaction terms between $Post_t$, $\mathbf{C}_{it}$ and $H_{i,2011}$ and the triple interaction $\mathbf{C}_{it} \cdot Post_t \cdot H_{i,2011}$ which is the Triple-Differences estimator that captures the effect of interest: in case the associated coefficient, $\beta_6'$, is positive, the interaction term means that the exacerbating impact of the reform on the

political budget cycle is more attenuated for cities that have more concentrated income distribution *vis à vis* those with more uniform distribution. Moreover, the set of controls contains political (mayors' characteristics and information on the term), social (population) and economic variables (information on income distribution). Finally, fixed effects and time trends are the same as in the main analysis of section 6, and robust standard errors are clustered at the municipal level.

## 7.3 Results: Triple-Diff

Table 6 contains results for the triple-diff model discussed in the previous section for all dependent variables: column (1) shows result for the median rate and column (2) for per-capita revenue. For the sake of brevity, we did not include in the table coefficients of un-interacted terms. Panel A contains the interaction between the dummy describing political cycle and the post-reform period, $\mathbf{C}_{it} \cdot Post_t$. Estimated coefficients confirm the result found in the analysis of Table 4: the reform amplifies political budget cycle as interaction terms are negative and significant, with coefficients larger than in the main model. Panel B includes the interaction term between post-reform dummy and the concentration index, $Post_t \cdot H_{i,2011}$ and it suggests that cities with higher pre-reforms levels of concentration set lower tax rates. Moreover, Panel C shows the interaction between political cycle dummies and concentration index, $\mathbf{C}_{it} \cdot H_{i,2011}$: this relation is quite ambiguous as it seems that as income concentration raises tax rate in pre-electoral year, two and three years from elections reduces. Nevertheless, the coefficients small and the statistical significance is weak.

Finally, Panel D contains estimates of our interest and it shows the triple interaction between the dummy characterizing political cycle, the post-reform dummy and the index of income concentration: this terms describes how pre-determined income concentration mediates the effect of the reform on the emergence of political budget cycle. Estimated coefficients for the two dependent variables are all positive and significant, meaning that as income distribution is more concentrated, the amplifying effect of the reform attenuates gradually: *i.e.* cities with more uniform income distribution face a larger amplification of their political budget cycle after 2012 reform, compared to cities with more concentrated distributions. Focusing on the median rate (column (1)), in order to study the magnitude of this mediation, we can note that, *ceteris paribus*, a difference in concentration index of one quartile implies, for more concentrated city, an average reduction in the effect of the reform by 2.7%, 2.7% and 2.0%, respectively, three, two and one years before elections, compared to the dependent variable average value, and by 3.4% in the election year. The same result emerges when we use per-capita revenue (column (3)) as dependent variable of the analysis. Furthermore, we obtain similar results when we run this specification and we use different versions of income concentration measure, instead of this index of quartiles of concentration[27].

Figure 14 provides a graphical evidence of the result reported in Table 6 with the focus on the median surcharge rate. In particular, we simulate average median rate along the political cycle for post-reform period varying the degree of concentration and we

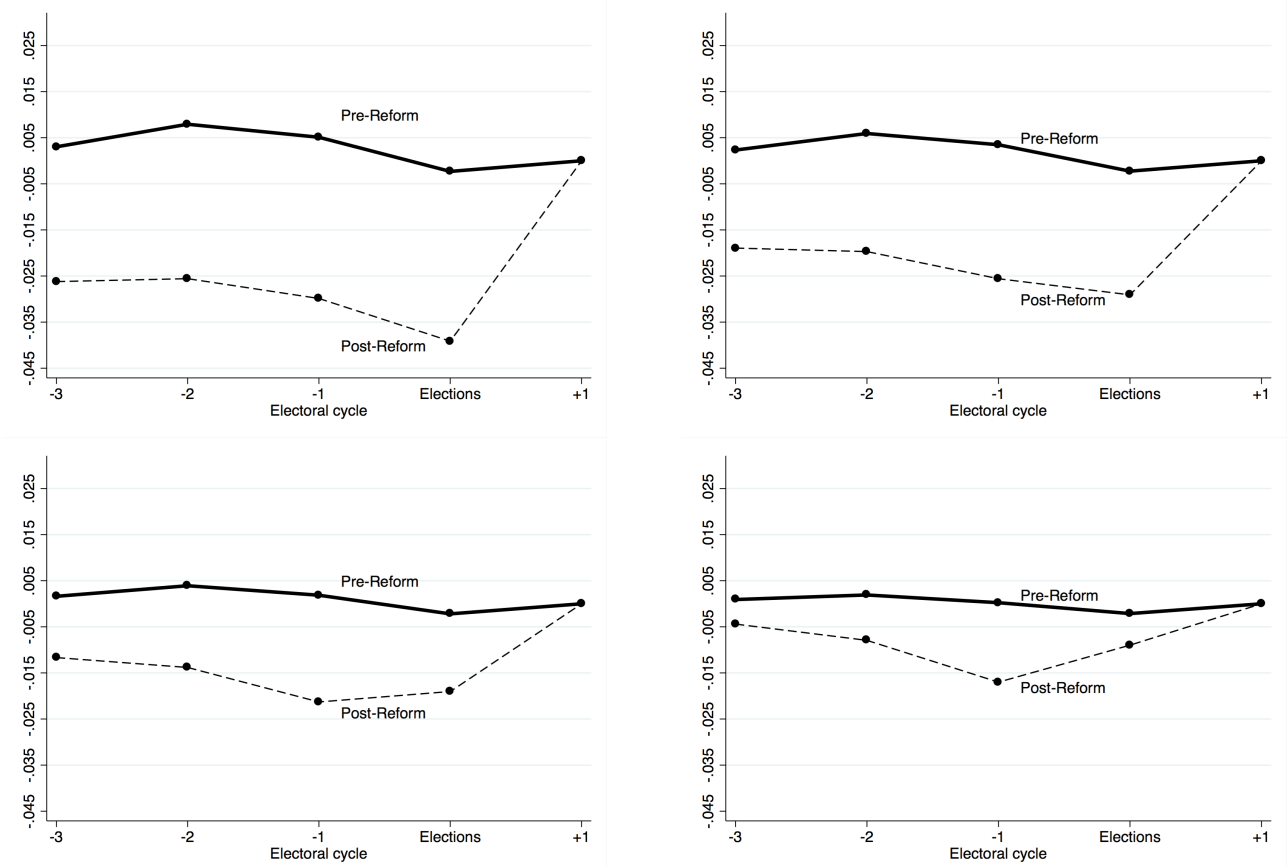
[27]In particular, we have other three measures of income concentration: a dummy whether a city is above the median, one dividing in deciles the index and another one with the index varying in the range 0-100.

Table 6: Triple-Diff analysis with income concentration as a mediator

| Dependent variable: **Municipal surcharge** | **median** rate (1) | **per-capita revenues** (2) |
|---|---|---|
| **Panel A**: $\mathbf{C}_{it} \cdot Post_t$ | | |
| 3 years before elections*Post-reform | -0.0372 | -6.518 |
| | (0.00710)*** | (1.054)*** |
| 2 years before elections*Post-reform | -0.0414 | -5.653 |
| | (0.00573)*** | (1.328)*** |
| 1 year before elections*Post-reform | -0.0409 | -6.627 |
| | (0.00596)*** | (0.947)*** |
| Election year*Post-reform | -0.0468 | -6.641 |
| | (0.00502)*** | (1.004)*** |
| **Panel B**: $Post_t \cdot H_{i,2011}$ | | |
| Post-reform*Concentration index | -0.0275 | -5.137 |
| | (0.00269)*** | (0.342)*** |
| **Panel C**: $\mathbf{C}_{it} \cdot H_{i,2011}$ | | |
| 3 years before elections*Concentration index | -0.000702 | 0.0403 |
| | (0.000563) | (0.0783) |
| 2 years before elections*Concentration index | -0.00200 | -0.218 |
| | (0.000771)*** | (0.0962)** |
| 1 year before elections*Concentration index | -0.00162 | -0.204 |
| | (0.000760)** | (0.0944)** |
| Election year*Concentration index | 0.0000605 | 0.0949 |
| | (0.000564) | (0.0882) |
| **Panel D**: $\mathbf{C}_{it} \cdot Post_t \cdot H_{i,2011}$ | | |
| 3 years before elections*Post-reform*Concentration index | 0.00797 | 1.613 |
| | (0.00241)*** | (0.315)*** |
| 2 years before elections*Post-reform*Concentration index | 0.00788 | 1.435 |
| | (0.00208)*** | (0.403)*** |
| 1 year before elections*Post-reform*Concentration index | 0.00591 | 1.544 |
| | (0.00217)*** | (0.293)*** |
| Election year*Post-reform*Concentration index | 0.00999 | 1.557 |
| | (0.00176)*** | (0.278)*** |
| Mean of the dep. variable | 0.295 | 27.6 |
| Municipality & Macro-region Year FE | Yes | Yes |
| Time trends & Political/Economic controls | Yes | Yes |
| Observations | 127792 | 126181 |
| Adjusted $R^2$ | 0.768 | 0.711 |

*Notes*: Dependent variables are the three measures of the surcharge. This output does not show un-interacted terms. The concentration index included in the analysis is te version in quartiles. The specification includes municipality and macro-region/year fixed effect as well as region specific time trend, log of population, characteristics of mayors (*i.e.* education, age, proportion of women), dummy whether mayor is term-limited and variables to characterize income distribution (*i.e.* fraction of population in all wage brackets, total number of tax-payers and total amount of income declared). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 14: Graphical evidence of Triple-Diff analysis - Median surcharge rate.




*Notes*: The graph plots coefficients of column (1) of Table 6. "Pre-Reform" line describes the cycle before the reform, 1999-2011, and it varies only with income concentration: coefficient $\hat{\beta'}_1 + \hat{\beta'}_5 \cdot H_{i,2011}$; "Post-Reform" line describes the cycle after the reform, 2012-2015, with different levels of pre-determined income concentration: $\hat{\beta'}_1 + \hat{\beta'}_3 + (\hat{\beta'}_5 + \hat{\beta'}_6) \cdot H_{i,2011}$. The four panels show the political budget cycle in correspondence of different quartiles of the income concentration index, $H_{i,2011}$. Budget lines are in deviation from the average level in the post-electoral year.

compare it with the average rate in pre-reform period[28]. We simulate four simple cases: perfectly uniform, highly uniform, weakly concentrated and highly concentrated income distribution (first, second, third and fourth quartile of income concentration). From the figure, the mediating effect of income distribution emerges clearly: for cities in the first quartile of concentration (upper-left panel of the figure), the political budget cycle after the reform is very pronounced; then, as income distribution becomes more concentrated the post-reform political cycle attenuates and gets closer to the one in the pre-reform period; finally, for cities in the fourth quarter of concentration (lower-right panel of the figure), the two cycles get very close, meaning that the reform has a very limited effect for these cities. The same result emerges in the analysis using per-capita revenue as dependent variables (graphical outputs are shown in the Appendix 1, Figure 25).

[28]In doing this simulation we are making the simplistic assumption that the positive effect of the mediator (income concentration) on the reform is linear. Of course, we cannot rule out the presence of non-linearities in this relation and we would have to conduct *ad hoc* analyses to shed further light on this issue.

# 8 Robustness analysis

There can be several threats to the identification that could undermine the empirical strategy presented in previous sections: here we discuss some of these threats providing evidence for the robustness of our results. First, there were other, minor, reforms affecting municipal surcharge before the 2012 reform evaluated in this paper. Second, there can be interventions, contemporaneous to the surcharge rate reform, that may have affected local public finance differently for municipalities in different years of their political term. Finally, we present a set of alternative models where we enrich the specification and we refine the sample of cities in analysis.

## 8.1 Other reforms of the surcharge rate

The reform evaluated in this contribution is the most important intervention to the municipal surcharge after its introduction in 1999. Nevertheless, there have been other minor modifications over time: we want to check whether and how these additional interventions contributed to the result we found here.

The first minor intervention was the introduction of a cap to surcharge rises that was applied twice[29]: these changes were temporary and they affected only a subset of Italian municipalities. The first cap was introduced in 2003, partially relaxed in 2004 and removed in 2007, and the second was introduced in 2009 and removed in 2011. These interventions prevented local administrators to raise surcharge rates and, despite the absence of limitations for rate reductions, overall volatility of the tax rate could have decreased due to the cap. One possible concern is that as caps took place in pre-reform period these could have mechanically decreased volatility of the surcharge rate compared to post-reform period and this could have amplified the effect of 2012 reform. In order to exclude this possible distortion, we run the main specification of the Diff-in-Diff model excluding cap years from the sample, *i.e.* 2003-2006 and 2009-2011. Columns (1-2) of Table 7 show the results for the median tax rate and the per-capita revenues: two remarks can be noted. First, estimates are very similar to the ones obtained in the full sample analysis. The political budget cycle before the reform is very weak: the rate is almost flat until election year when it slightly reduces. And after the reform the cycle amplifies with the same shape as in the analysis with the full sample. Second, the exacerbating effect of the reform is slightly milder.

The second minor reform took place in 2006 and it raises the maximum surcharge rate from a level of 0.5% to 0.8%[30]. This may have raised the fluctuation of the rate and the overall volatility after 2012. In order to control for this, we run the main specification for the reduced time span 2006-2015 excluding the set of years when the maximum rate was 0.5%. Columns (3-4) of Table 7 show the estimates: results are very similar to the previous check. On the one hand pre-reform cycle is very mild and almost flat; on the other hand, the reform is still effective in amplifying political budget cycle, despite coefficients being a bit weaker than estimates obtained with the complete sample. We can conclude that

[29]These modifications were introduced by following laws: *legge n. 289/2002, Art. 3* and *D.L. n. 112/2008, Art. 77 bis*.

[30]This modification was introduced by *legge n. 296/2006, Art. 1, c. 142*; beyond that the reform removes maximum yearly increase of the surcharge rate, originally set at 0.2%.

Table 7: Robustness checks: Effect of the reform on surcharge manipulation

| Dependent variable: **Municipal surcharge** | **Excluding cap years** | | **Excluding pre-2006** | | **Excluding early elections** | | **Controlling for cycle effects** | |
|---|---|---|---|---|---|---|---|---|
| | Median | Per-capita | Median | Per-capita | Median | Per-capita | Median | Per-capita |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (7) |
| 3 years before elections | 0.00522 | 0.523 | 0.00478 | 0.671 | 0.00306 | 0.418 | 0.00572 | 0.790 |
| | (0.00198)*** | (0.211)** | (0.00115)*** | (0.178)*** | (0.000800)*** | (0.128)*** | (0.00244)** | (0.382)** |
| 2 years before elections | 0.00193 | -0.0940 | 0.00543 | 0.838 | 0.00619 | 0.451 | 0.0164 | 1.953 |
| | (0.00238) | (0.243) | (0.00146)*** | (0.198)*** | (0.00108)*** | (0.140)*** | (0.00289)*** | (0.444)*** |
| 1 year before elections | 0.000754 | 0.0216 | 0.000122 | 0.582 | 0.00378 | 0.242 | 0.0135 | 1.943 |
| | (0.00284) | (0.274) | (0.00135) | (0.195)*** | (0.00104)*** | (0.136)* | (0.00271)*** | (0.463)*** |
| Election year | -0.00364 | -0.0919 | -0.00923 | -0.0926 | -0.00213 | 0.171 | -0.0125 | -0.234 |
| | (0.00202)* | (0.219) | (0.00146)*** | (0.194) | (0.000956)** | (0.125) | (0.00228)*** | (0.389) |
| 3 years before elections*Post-reform | -0.0175 | -2.293 | -0.00876 | -1.426 | -0.0228 | -3.231 | -0.0209 | -2.955 |
| | (0.00316)*** | (0.393)*** | (0.00277)*** | (0.373)*** | (0.00372)*** | (0.485)*** | (0.00292)*** | (0.380)*** |
| 2 years before elections*Post-reform | -0.0167 | -1.277 | -0.0145 | -1.671 | -0.0263 | -2.519 | -0.0242 | -2.315 |
| | (0.00306)*** | (0.450)*** | (0.00271)*** | (0.469)*** | (0.00300)*** | (0.542)*** | (0.00256)*** | (0.442)*** |
| 1 year before elections*Post-reform | -0.0221 | -2.195 | -0.0157 | -2.127 | -0.0314 | -3.190 | -0.0290 | -3.077 |
| | (0.00357)*** | (0.417)*** | (0.00275)*** | (0.388)*** | (0.00309)*** | (0.426)*** | (0.00278)*** | (0.361)*** |
| Election year*Post-reform | -0.0172 | -2.156 | -0.00477 | -1.431 | -0.0297 | -3.268 | -0.0252 | -3.102 |
| | (0.00296)*** | (0.489)*** | (0.00248)* | (0.451)*** | (0.00293)*** | (0.665)*** | (0.00236)*** | (0.435)*** |
| Mean of the dep. variable | 0.295 | 27.6 | 0.295 | 27.6 | 0.295 | 27.6 | 0.295 | 27.6 |
| Municipality & Macro-region Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Time trends & Political/Economic controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 75,031 | 73,853 | 67,221 | 66,424 | 99778 | 98601 | 127809 | 126198 |
| Adjusted $R^2$ | 0.750 | 0.708 | 0.846 | 0.780 | 0.773 | 0.705 | 0.769 | 0.710 |

*Notes*: Dependent variables are the two measures of the surcharge. Columns (1-2) contain the estimates excluding years when the cap to the surcharge increases was introduced. Columns (3-4) contain the estimates excluding the years when the maximum surcharge rate was different. Columns (5-6) contain the estimates with the sample without the electoral terms that end prematurely. Columns (6-7) contains the estimates with the inclusion of region fixed effects interacted with the electoral cycle. The specification includes municipality and macro-region/year fixed effect as well as region specific time trend, log of population, characteristics of mayors (*i.e.* education, age, proportion of women), turnout in last elections, dummy for a full mandate, dummy for term limit and variables to characterize income distribution (*i.e.* fraction of population in all wage brackets, total number of tax-payers and total amount of income declared). Moreover, electoral groups fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

these minor interventions did not play a consistent role in the emergence of the political budget cycle of the municipal surcharge.

## 8.2 Additional checks

Another potential issue may be the fact that sometimes municipalities endogenously change their electoral group: the most common case is when the mayor does not complete the full mandate of five years and the city faces an early election. This premature end of the term can be due to a governmental crisis of the local cabinet or to other, more rare, events such as resignation or death of the mayor. These events may confound the estimates, leading to some sort of selection of cities within electoral groups. In order to control for this potential source of selection, we run a robustness check to study whether cities experiencing early elections contribute to the main results of this analysis. Columns (5-6) of Table 7 show the outputs of the main analysis with the exclusion of the group of cities with early termination. The main results are confirmed, and the effects of the reform on political budget cycle are also larger than in the main specification. This result suggests that this form of self-selections of cities across groups does not constitute a threat for the internal validity of the analysis.

Finally, we include a more sophisticated set of fixed effects and we try to capture for regional effects that are differential along the political cycle. Columns (7-8) of Table 7 show the results for this specification where we include a set of region specific fixed effects interacted with the electoral cycle. This test allows us to clean for differential behaviour of cities in the electoral cycle that are specific to each regions. The outputs of this test confirm the main results and show a larger effect of the reform on the political budget cycles than in the main specification.

### 8.3 Simultaneous public finance intervention

Another potential threat to identification can be the presence of additional reforms, taking place simultaneously to the one we study here, and that affected differently municipalities in different position of their political cycle: this would be a violation of the identifying assumption of the model presented in section 6. Starting from 2011, Italy experienced a deep sovereign debt crisis that led to an early termination of Berlusconi IV government and to the technical cabinet held by Mario Monti. This was an intense reform season aimed at consolidating Italian public accounts with attention both to national and local level[31].

We want to make sure that the outputs are not, even partially, the result of these simultaneous reform. In order to check for this, we run the main specification of section 6, using as dependent variables a set of public finance indicators to see whether they vary significantly along their political cycle, after 2012. If this was the case this would suggest that there are interventions that affect municipalities differently along the cycle and this could be a threat for the Diff-in-Diff identifying assumption. The set of public finance variables considered is large: main local taxes such as real estate and waste tax, aggregate revenues from special fees and the amount of current expenditure and investments, all these variables are in per-capita terms. Columns (1), (4-7) of Table 8 contain the estimates using the dependent variables in per-capita terms: it seems that few variables fluctuate differently in the cycle after 2012: it emerges that the expenditures in investments and the revenue from the waste tax seem to decline after the reform, but the effect is only significant in the pre-electoral year. Finally, there is an increase, uniform in all positions of the cycle, in revenues generated by the real estate tax; and this is likely to be due to the (re-)introduction of the real estate tax in 2011, as discussed by Alesina, Paradisi (2015)[32]. As a second analysis, we perform in columns (2-3) of Table 8 the same exercise considering the tax rate of the real estate tax, which is decided by the mayors every year: we focus, in column (2), on the ordinary rate, the one applicable to commercial properties and to private houses different from the first one and, in column (3), on the tax on first properties, which is usually lower than the ordinary tax. Main results seem to suggest that there are not consistent effects to the real estate rates after 2012, differential in the cycle: unique modification is a reduction in the pre-election year of the ordinary real estate rate, the magnitude of the effect, by the way, is very limited as it represents a decrease of the 0.65%, compared to the tax rate average value. These results seem to suggest that there are no other significant interventions, simultaneous to the 2012 surcharge reform, that could confound our Diff-in-Diff estimates.

## 9 Fiscal manipulation and re-election probability

In the previous sections we find that progressivity stimulates manipulation of the municipal surcharge and it is likely that this behaviour is motivated by obtaining electoral

[31] Main interventions at the local level include the introduction, in 2011, of the property tax, *IMU* that has been studied by Alesina, Paradisi (2015), and the reform of the Italian cadastres with revaluation of property tax base; finally, another important intervention was the reduction in transfers from the central governments to municipalities.

[32] The real estate tax on the first residency was then abolished again in 2013, by centre-left Government.

Table 8: Placebo tests: Effect of the reform on public finance variables

| Dependent variable: | **Real Estate** tax (PC revenues) | **Real Estate** rate (ordinary) | **Real Estate** rate (first) | **Waste** tax (PC revenues) | **Special fees** (PC revenues) | **Current expend.** (PC revenues) | **Capital expend.** (PC revenues) |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| 3 years before elections | 1.166 | 0.000640 | 0.00149 | 0.347 | 4.477 | 1.132 | 26.37 |
| | (0.566)** | (0.000311)** | (0.000464)*** | (0.267) | (0.534)*** | (2.232) | (12.36)** |
| 2 years before elections | 0.856 | -0.000217 | 0.00111 | -0.118 | 2.187 | 5.021 | 57.87 |
| | (0.693) | (0.000397) | (0.000582)* | (0.271) | (0.587)*** | (2.137)** | (11.83)*** |
| 1 year before elections | -0.0533 | -0.00170 | -0.000149 | -0.927 | 1.732 | 3.506 | 72.15 |
| | (0.565) | (0.000390)*** | (0.000477) | (0.279)*** | (0.474)*** | (1.768)** | (12.65)*** |
| Election year | -0.857 | -0.00385 | -0.00150 | -0.877 | 0.926 | -1.204 | -66.25 |
| | (0.502)* | (0.000328)*** | (0.000461)*** | (0.260)*** | (0.464)** | (1.649) | (11.73)*** |
| 3 years before elections*Post-reform | 6.693 | 0.00650 | 0.00346 | -2.231 | -14.06 | 9.610 | -28.59 |
| | (2.351)*** | (0.00148)*** | (0.00129)*** | (1.598) | (1.960)*** | (6.489) | (33.24) |
| 2 years before elections*Post-reform | 9.094 | 0.000400 | -0.000215 | -0.926 | -0.606 | 4.044 | -7.429 |
| | (2.611)*** | (0.00140) | (0.00126) | (1.426) | (1.600) | (5.877) | (25.47) |
| 1 year before elections*Post-reform | 8.341 | -0.00426 | -0.0000820 | -2.960 | 1.057 | 9.134 | -64.39 |
| | (2.979)*** | (0.00145)*** | (0.00129) | (1.486)** | (1.818) | (6.257) | (23.22)*** |
| Election year*Post-reform | 9.102 | 0.000685 | -0.000868 | -1.588 | -1.996 | -0.402 | 27.42 |
| | (2.312)*** | (0.00118) | (0.00105) | (1.341) | (1.629) | (4.712) | (25.20) |
| Mean Dep. Var. | 171.98 | .656 | .493 | 84.45 | 50.75 | 836.07 | 604.40 |
| Municipality & Macro-region Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Time trends & Economic controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 126,199 | 127,604 | 131,971 | 126,200 | 126,200 | 126,241 | 126,241 |
| Adjusted $R^2$ | 0.806 | 0.850 | 0.593 | 0.623 | 0.629 | 0.863 | 0.318 |

*Notes*: The specification includes municipality and macro-region/year fixed effect as well as region specific time trend, log of population, characteristics of mayors (*i.e.* education, age, proportion of women), turnout in last elections, dummy for a full mandate, dummy for term limit and variables to characterize income distribution (*i.e.* fraction of population in all wage brackets, total number of tax-payers and total amount of income declared). Moreover, groups fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

consensus. In this section we want to provide (mostly correlation) evidence that being strategic is actually rewarding in subsequent elections and that voters choose the incumbent that favour them with lower tax rates. Differently, it would be difficult to justify the emergence of massive manipulation for the surcharge as elections approach. In particular, we focus on the probability that the incumbent is confirmed at the polls and whether the surcharge rate significantly modifies this probability: we focus on the group of elections where the incumbent runs again and we consider the impact of the surcharge introduced in the previous term. The final sample includes 11,680 elections for the time span 2000-2015[33]: the fraction of re-elected incumbent in our sample is quite high, 73.5%; this is a peculiarity of the Italian local political background, as already documented by other studies (*e.g.* Repetto 2016).

We study this question with a standard Probit regression model using as dependent variable a dummy equal to one whether the incumbent reruns and is re-elected and zero whether she reruns without being confirmed. Given the difficulty to get an instrument for the treatment, these results should be mainly interpreted as correlations. The specification always includes year and region fixed effects as well as a set of economic controls (total fiscal revenues, real estate revenues, waste tax revenues, all in per-capita terms in the election year) to account for the impact of other taxes on re-election probability of the incumbent; moreover, political controls are included (education, age and gender of the incumbent and voter turnout) referring to the past term, to capture the ability of the incumbent and the political background as well as total population and a dummy for province capitals to control for specific characteristics of capitals. Moreover robust

[33]We exclude elections taking place in 1999 as this is the year of introduction of the surcharge in Italy.

Table 9: Effect of the surcharge on incumbent confirmation - Probit analysis

| | **Elasticity** - Dep. Var: Incumbent is re-elected | | | | | | |
|---|---|---|---|---|---|---|---|
| **Panel A:** Median surcharge rates | Revenue (per capita) (1) | Average (all years) (2) | Election year (3) | Pre-election year (4) | Two years before elections (5) | Three years before elections (6) | Four years before elections (7) |
| Municipal surcharge rate | -0.00127 | -0.0929 | -0.199 | -0.123 | -0.0923 | -0.0100 | 0.0159 |
| | (0.000721)* | (0.0758) | (0.0618)*** | (0.0654)* | (0.0689) | (0.0743) | (0.0766) |
| Region & Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political/economic controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 8,392 | 8,392 | 9,897 | 9,436 | 9,176 | 8,753 | 8,422 |
| **Panel B:** Single rates (election year) | Bracket 0-15,000 (1) | Bracket 15,000-26,000 (2) | Bracket 26,000-55,000 (3) | Bracket 55,000-75,000 (4) | Bracket 75,000-120,000 (5) | | |
| Municipal surcharge rate | -0.148 | -0.202 | -0.210 | -0.216 | -0.216 | | |
| | (0.0622)** | (0.0625)*** | (0.0622)*** | (0.0618)*** | (0.0613)*** | | |
| Region & Year FE | Yes | Yes | Yes | Yes | Yes | | |
| Political/economic controls | Yes | Yes | Yes | Yes | Yes | | |
| Observations | 9,897 | 9,897 | 9,897 | 9,897 | 9897 | | |

*Notes*: Probit regressions with as dependent variable a dummy variable equal to one if the incumbent runs again and is re-elected and zero if it is not confirmed. The sample includes all elections where the incumbent runs again as candidate in the time span 2000-2015. The specification always includes year and region fixed effects, economic controls (total fiscal revenues, real estate revenues, waste tax revenues all in per-capita terms), political controls (education, ange and gender of the mayor and voter turnout), total population and a dummy for province capitals. Robust standard errors clustered at the municipality level are in parentheses: $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

standard errors are clustered at the municipal level[34]. Table 9, Panel A, shows the results of this analysis: we performed different tests. First, in column (1), we study the impact of the surcharge revenues (per-capita), averaged in the five years of the last term. It emerges that higher levels of surcharge rates penalise the incumbent reducing her probability of being confirmed: in particular, an increase in the surcharge revenues by one standard deviation (in this sample it amounts to €25.2) reduces the probability of re-electing the incumbent by 3.2%, a considerable amount. Column (2) focuses on the median surcharge rates, averaged in the five years of the last term: the negative effect of the surcharge is confirmed, but the relation is not statistically significant. Moreover, in columns (3-7) we study whether the surcharge rate affects re-election probability differently, depending on the moment in the electoral cycle: it emerges that the negative effect of the surcharge is strong close to the polls and it weakens for years further back in time. Column (3) focuses on the rate in the election year (the last rate decided by the incumbent) that turns out to be highly, negatively, correlated with re-election probability, one standard deviation increase in the rate reduces probability of re-election by 5.2%; moreover, column (4) focuses on the rate in the pre-election year and the effect is negative and significant but weaker. Finally columns (5-7) consider the tax rates, respectively, two, three and four years before elections: the magnitude of coefficients gradually decrease and these are not statistically different from zero. These outputs suggest that the municipal surcharge does affect re-elections chances of the incumbent as people seem to take it into account when deciding whom to vote. This confirms that municipal surcharge is a salient tax at the local level and it explains why mayors have interests in manipulating it.

As a second analysis, we study whether tax rates introduced to different income groups

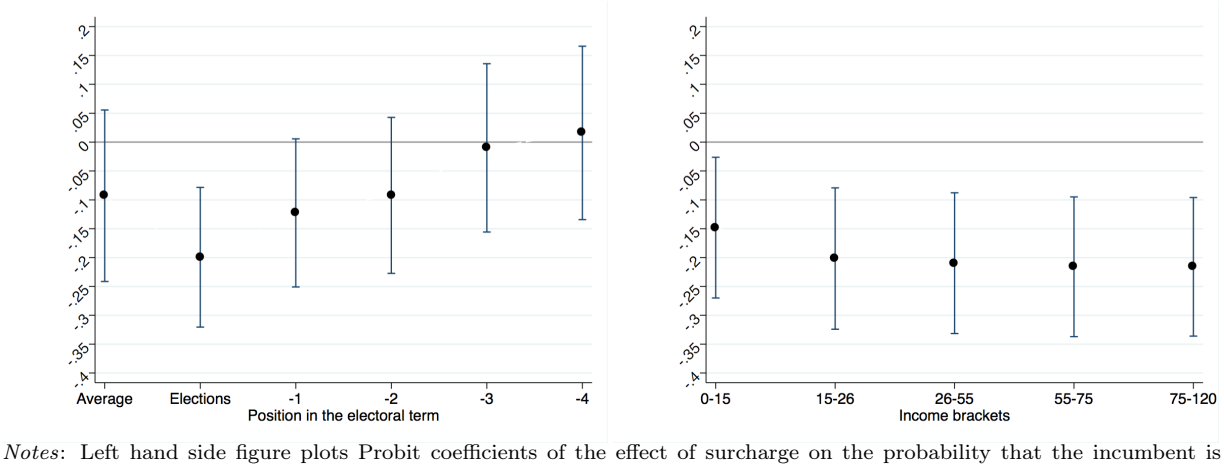
[34]The estimated regression model is as follows:

$$y_{it} = \alpha + \beta Sur_{it} + \gamma' X_{it} + \epsilon_{it}$$

With $y_{it}$, dummy variable capturing whether the incumbent runs again and is confirmed, $Sur_{it}$ the rate of the municipal surcharge (many versions are included) and $X_{it}$ the set of controls. We are interested in the coefficient $\beta$ that may be read as an elasticity. In particular it may be interpreted as the impact of a unitary increase in the explanatory variable $Sur_{it}$ on the probability that $y_{it}$ equals one, given the set of controls included.

affect differently re-election probability. Results are shown in Panel B of Table 9, where we only present coefficients for the rate in the election year. The emerging pattern is clear and it is coherent with previous results: the magnitude of the negative effect raises as we consider rates of higher income groups, despite these effects not being statistically different from each others. This result means that it is more costly, in electoral terms, for the incumbent, to keep high surcharge rates for middle-high income groups than that for low ones and this may be due to the fact that rich people are more aware and informed about the tax and punish more likely the incumbent. Moreover, these results complement the outputs found in section 6.2 and suggest that mayors prefer to do targeting and to manipulate rates of middle-high income groups because these are more responsive in following elections. Figure 15 show these results graphically: each dot represents the coefficient associated to the surcharge and it is a separate regression. Left sub-graph contains coefficients of Panel A and right sub-graph of Panel B.

Figure 15: Impact of the surcharge on incumbent re-election




*Notes*: Left hand side figure plots Probit coefficients of the effect of surcharge on the probability that the incumbent is confirmed. "Average" coefficient represents the average surcharge rate during the term, "elections/-1/-2/-3/-4" coefficients represents surcharge rates, respectively, in the election year, pre-election year, two years before elections, three years before elections and four years before elections. Right hand side figure plots Probit coefficients of the effect of the surcharge of single rates on the probability that the incumbent is confirmed. Only the coefficients for the election year are presented. The specifications always include year and region fixed effects, incumbent's characteristics (education, age, gender), indicator of local public finance (per capita total fiscal revenues, real estate tax and waste tax), turnout, total population and a dummy for province capital. Robust standard errors are clustered at the municipal level. Finally the sample is restricted to the group of cities where the incumbent runs again.

These results relate to the literature on political budget cycle as they confirm that manipulation is rewarding in electoral terms, as already shown for many components of the budget, in different settings (Akhmedov, Zhuravskaya 2004, Repetto 2016). Nonetheless, we are among the first to show that targeted fiscal manipulation is rewarding for the incumbent and that different groups have heterogeneous responses to electoral favours. This suggests the reason why providing local administrators with sophisticated instruments, that allow to target portions of the electorate, may foster manipulation. Moreover, these results point out that when targeting is possible voters that are informed about budget choices may be more responsive to electoral favours. And this may consequently stimulate manipulation. These results are, therefore, not consistent with theoretical findings

by Shi, Svensson 2006 that show that providing information on public finance to voters leads politicians to reduce their strategic behaviour, with associated improvements in the political budget cycle.

# 10 Impact of partisanship on tax flexibility

## 10.1 Usage and degree of flexibility

In this paragraph we want to explore whether partisanship of local administrators plays a role in determining whether and to what extent progressivity is introduced in a municipality. We explore two different angles of partisanship. First, we focus on the effect of having a "political mayor", officially part of a political party, rather than a "civic mayor", part of an independent movement, officially not supported by any party. This dichotomy is very important in Italian local politics; Gamalerio (2016) shows, in an RDD analysis, that party affiliation affects fiscal discipline. In particular, political mayors are more fiscally responsible with lower deficit, less debt and fewer expenditures, compared to civic mayors. We want to see whether this dichotomy matters in this context as well. Second, we inspect the classic left/right division in order to check which political force cares more about redistribution in this local context.

In terms of outcome variable, we focus on two aspects. On the one hand, we look whether partisanship affects the probability that a flexible rate, exemption or differentiated rate, is introduced, measured as the fraction of years in a legislature with a flexible rate. On the other hand, we focus on the degree of flexibility introduced, measured as the progressivity level of the tax schedule chosen by the mayor. After 2012 reform, local administrators have considerable freedom to modulate the tax schedule and they can choose among more or less redistributive systems. We want to measure how redistributive a fiscal system is, *i.e.* its degree of progressivity, and we want to see how this depends on mayor partisanship. To provide an instance, figure 16 shows the tax schedule of four Italian municipalities in 2015, ordered with increasing degree of progressivity. Starting from the upper-left figure, first two cities, Bellagio and Olbia, have a flat tax associated with no redistribution, degree of progressivity raises with the third city, Avellino, and raises again with the last city, Barletta. In order to create an index of progressivity, we rely on the literature of Public Finance dealing with measuring and comparing progressive tax systems (Govori 2015). The proposed index is as follows:

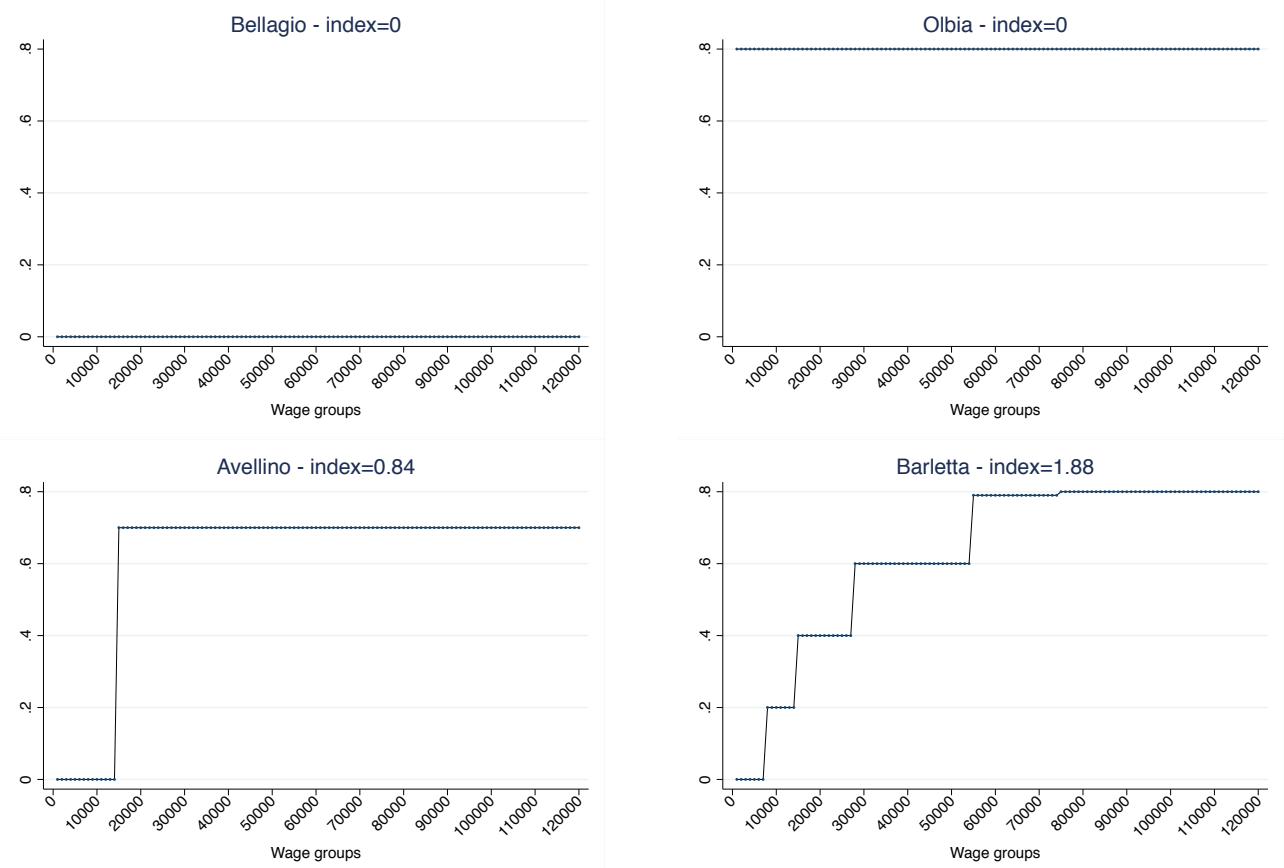
$$\sum_{i=2,000}^{120,000} (MRP_i)^{\frac{1}{2}}$$

With $MRP_i$ the "Marginal Rate Progression" (Pigou 1960) from income level $i-1,000$ to $i$, which is defined as the difference $t_i - t_{i-1,000}$, with $t_i$ tax rate applied to the income level $i$. The index results from the summation of the square root of marginal rate progressions in the income range €1,000 - €120,000. This measure can be considered as the inverse of an Herfindal index as it weighs dispersion and it measures the degree of progressivity in the tax schedule of a municipality[35]. Figure 16 shows corresponding values of the index,

[35]Mean of the index is .082 with a standard deviation of .292, the index goes from a minimum of 0 to a maximum level of 1.99.

as expected when progressivity raises the index is larger.

Figure 16: Index of progressivity - examples




*Notes*: These graphs plot surcharge rate for every wage groups in 2015 for four Italian municipalities with different degrees of "progressivity" index.

## 10.2 Close election RDD

The analysis is structured as a sharp regression discontinuity design (RDD), as in Gamalerio (2016). We could not simply compare, using OLS, outcomes of two groups of municipalities, respectively, civic/political and left/right, as these two groups are likely to differ in several observable and unobservable characteristics and this could generate biased estimates due to endogeneity issues. Thus, the analysis performed is limited to the set of close elections: we focus on mixed electoral competitions, where political mayors run against civic ones (or left-wing candidates run against right-wing ones), whose outcomes are decided by narrow margins. In these cases, it is plausible to assume that election results are determined by random factors rather than characteristics of municipalities that are likely to impact on local public finance variables. Following recent developments by Calonico, et al. (2014) and Gelmans, Imbens (2014) the RDD model, estimated by local linear regression (LLR), is as follows:

$$y_{it} = \alpha + \beta_1 VM_{it} + \beta_2 P_{it} + \beta_3 MV_{it} \cdot P_{it} + \gamma X_{it} + \delta_t + \zeta_m + \epsilon_{it}$$

where $y_{it}$ is the dependent variable of the analysis capturing either the usage of the flexibility (fraction of flexible years in the legislature) or the degree of progressivity introduced (value of the progressivity index) for municipality $i$ at time $t$; $\delta_t$ are year fixed effects, $\zeta_m$ macro-region fixed effects and $X_{it}$ is a vector controlling for political background (turnout, number of candidates, number of lists, share of civic lists, vote share, concentration index, vote share of the most voted candidate, dummy for term limit, education, age and gender of mayor and councillors) and economic control of election year (surcharge weighed average rate, lagged current expenditures, lagged real estate and lagged waste tax rate). The unit of analysis is the legislature, which starts in the election year and ends in the pre-electoral year, included. The treatment of the analysis is captured by the dummy $P_{it}$ which is equal to zero, in case of a civic (left-wing) mayor and one in case of a political (right-wing) mayor. The forcing variable of the RDD is the variable $VM_{it}$, victory margin of municipality $i$ at time $t$, which is calculated as the difference between vote shares of two most voted candidates. At the threshold $VM_{it} = 0$ the treatment changes sharply: in municipalities where $VM_{it} < 0$ a civic (left-wing) candidate barely wins, $P_{it} = 0$, while where $VM_{it} > 0$ a political (right-wing) candidate barely wins, $P_{it} = 1$. The RDD model is estimated in the sub-sample of municipalities where $VM_{it}$ belongs to the interval $[-h, +h]$, where $h$ is the optimal bandwidth, computed according to Calonico et al. (2014), Calonico et al. (2017), and Calonico et al. (2017). In the proposed model the coefficient of interest is $\hat{\beta}_2$ representing the average treatment effect (ATE) of the impact of partisanship. Finally, robust standard errors are clustered at the municipality level.

Main identifying assumptions of this RDD model require that there is no discontinuity of other covariates around the threshold and that there is not manipulation at the cutoff; we test these two assumptions later. For the analysis we consider all Italian municipalities for the time span 2010-2015, the period where mayors were allowed, and actually started, to introduce flexible rates. The sample of the analysis includes all mixed races civic/political and left/right where the mayor wins with a narrow margin: the final sample includes, respectively, 1,235 and 436 elections.

## 10.3 Results

As a first step, we test the main assumptions of the RDD model in our local framework. First identifying assumption requires that pre-determined covariates vary smoothly around the threshold $VM_{it} = 0$. We test for this running the main model using as dependent variables a set of characteristics of municipality and elected mayors from the last year of previous term. We run the same test for both samples, for the analysis civic/political and for the one left/right. Table 14 (in the Appendix 2) shows the outcomes of this test. The specification included time and entity fixed effects. From Panel A, focusing on the analysis civic/political, it is evident that there is no discontinuity in municipal characteristics such as population, share of immigrants, number of candidates, number of taxpayers and aggregate income declared, first row. Same results emerge if we focus on mayor's characteristics as education, age, gender and term limit, second row, as well as with local public finance variables, third row. The only exception seems to be that at the cutoff municipalities where political mayors barely win have lower current expenditure, but the estimate is almost non-significant and this problem does not emerge in other fiscal variables. From Panel B, focusing on left/right analysis, the same result emerges for all

municipalities characteristics, with the only exception of total fiscal revenues. Second the identifying assumption requires that there is no manipulation around the threshold, meaning that municipalities do not self-select around the cutoff. In case of evidence of manipulation, this would suggest that voters are capable to choose between a civic/political mayor (or a left/right in the second analysis) even when margins are narrow; and this would weaken the assumption that electoral outcome are as good as random around the cutoff. Figure 26 (in the Appendix 1) shows the McCrary test performed following Cattaneo et al. (2017): the figure confirms that there is no sizeable discontinuity in municipality density around the cutoff and this is evident for the civic/political analysis (left panel) as well as for the left/right one (right panel).

As a second step, we focus on the outcomes of the analysis. Panel A of Table 10 shows the causal impact of partisanship on the propensity of introducing a flexible rate, defined as the fraction of years in the term with exemption or differentiated rates. Columns (1)-(3) of Panel A deal with the effect of having a civic rather than a political mayor focusing on elections within, respectively, the optimal bandwidth $h$, half of the optimal bandwidth $h/2$ and two times the optimal bandwidth $2h$. All specifications show that political mayors tend to introduce more flexible rates compared to civic mayors. The effect is large and significant: focusing on column (1) political mayors average flexibility is 13.5% points higher than civic ones and this corresponds to an increase of 61% of the variable mean. This result can be interpreted in terms of electoral targeting and in terms of political experience: first political mayors consensus is generally eradicated in specific groups of the society, compared to civic mayors whose consensus is more general. This could motivate why political mayors are more willing to tax differently different groups of tax-payers, using more tax flexibility. As a second motivation, political mayors are more likely to have political experience or to have access to political experts and this makes more desirable for them to use flexibility for strategic purposes.

Columns (4)-(6) of Panel A study the causal effect of having a left-wing mayor rather than a right-wing one and suggest that left-wing administrators use more flexible rates, despite the relation not being significant in the specification with $h/2$. Focusing on column (4) the effect is sizeable as having a left-wing mayor raises average flexibility in the term by 25,0% corresponding to an increase of 111% of the variable mean. This result is coherent with the classical view about political forces where redistributive policies are usually prerogative of the left, and flexibility can be used as a redistributive tool in this context. Secondly, Panel B of Table 10 studies the causal impact of partisanship on the degree of progressivity introduced, measured with the progressivity index introduced in the previous section. Two main results emerge from this table: first, political mayors tend to introduce more progressive schemes rather than civic ones (columns 1-3 Panel B) and, focusing on political forces, there is weak evidence that left-wing mayors introduce more progressivity than right-wing ones (columns 4-6 of Panel B). These results are coherent with previous explanations and show that partisanship affects the degree of progressivity introduced in a municipality.

These outcomes suggest that partisanship affects both *whether* and *how* flexibility is used by local administrators. In particular, it emerges that political (left-wing) mayors are more willing to introduce flexible rates and to choose more progressive tax schemes compared to civic (right-wing) mayors. These outcomes represent a causal evidence that partisanship matters for the use of progressivity at the local level.

Table 10: Use/degree of flexibility - Close election RDD.

| Panel A: Dep. Var.: **Proportion of year with flexible rate** | Civic/Political mayor | | | Left/Right mayor | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RD_Estimate | 0.135 | 0.200 | 0.0964 | -0.247 | -0.138 | -0.250 |
| | (0.0722)* | (0.0982)** | (0.0545)* | (0.146)* | (0.162) | (0.109)** |
| Outcome variable mean | 0.221 | 0.221 | 0.221 | 0.221 | 0.221 | 0.221 |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth | 0.162 ($\hat{h}$) | 0.081 ($\hat{h}/2$) | 0.324 ($2\hat{h}$) | 0.12 ($\hat{h}$) | 0.06 ($\hat{h}/2$) | 0.24 ($2\hat{h}$) |
| Observations | 1235 | 1235 | 1235 | 436 | 436 | 436 |
| Panel B: Dep. Var.: **Degree of progressivity introduced** | | | | | | |
| RD_Estimate | 0.152 | 0.220 | 0.116 | -0.250 | -0.245 | -0.246 |
| | (0.0826)* | (0.117)* | (0.0602)* | (0.178) | (0.206) | (0.117)** |
| Outcome variable mean | 0.459 | 0.459 | 0.459 | 0.459 | 0.459 | 0.459 |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Bandwidth | 0.172 ($\hat{h}$) | 0.086 ($\hat{h}/2$) | 0.344 ($2\hat{h}$) | 0.147 ($\hat{h}$) | 0.0735 ($\hat{h}/2$) | 0.294($2\hat{h}$) |
| Observations | 1235 | 1235 | 1235 | 436 | 436 | 436 |

*Notes:* Estimation by RDD-LLR using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth h selector; a local polynomial of order one is used to construct point estimate, and a local polynomial of order two is used to construct the bias correction. The unit of observation of the analysis is the legislature over a time span 2010-2015. Dependent variable in the panel A is the fraction of years in a legislature where a flexible rate (*i.e. exemption or multiple rate*) is introduced and in panel B is the degree of progressivity introduced in a legislature, measure with the progressivity index. All specifications include political controls (number of candidates, number of lists, share of civic lists, vote share concentration index, vote share of the most voted candidate, dummy for term limit, education, age and gender of mayors and councillors) and economic controls of election year (municipal surcharge weighed average rate, lagged current expenditure, lagged real estate and lagged waste tax rate); finally year and macro-region fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# 11 Conclusions

In this work we explore whether introducing income redistribution at the local level strengthens the practice of political budget cycle modifying incentives of politicians to manipulate taxes. In particular, we focus on tax progressivity in the context of Italian municipalities. We exploit a reform that affects the local personal income tax, which was flat before the intervention, and that allows mayors to introduce differentiated tax rates or exemption thresholds. This unique natural experiment allows to test the political economy of local income redistribution. What emerges, in a Diff-in-Diff setting, is that progressivity consistently affects the extent to which mayors manipulate taxes, as average fluctuation of the tax rate over the political cycle raises significantly; and this suggests that political budget cycle amplifies after the reform. The reform affects all years of the cycle and its effect is not negligible, average tax rate in the pre-electoral year, for instance, declines between 8.6% and 9.4%, compared to pre-reform period. We complement results from the Diff-in-Diff model studying a different specification where we exploit the role of local income concentration, as a mediating factor for the reform: we implement a Triple-Diff model studying effects over the political cycle between cities with different degrees of (pre-determined) income concentration and the main results are confirmed.

In order to shed light on the main channels, we study whether mayors use progressivity to treat differently diverse income groups. Main results suggest that different strategies are performed as the tax rate of the poor income groups are always low while those of middle-high ones fluctuate consistently in the political cycle. These results suggest that mayors want to maximize electoral consensus, without decreasing too much fiscal revenues, and that progressivity allows them to achieve this goal realizing a targeted manipulation. Nevertheless, we document that manipulating the municipal surcharge is rewarding in electoral terms. Indeed, it emerges that the surcharge rate significantly affects re-election probability of the incumbent and that this effect is mostly due to changes in tax rates in the last years of the term, and for middle-high income groups. Finally, we raise the issue

whether partisanship of administrators affects the use of progressivity: we perform an RDD analysis focusing on close races and we find that political (left-wing) mayors tend to use more flexibility and to introduce more progressive tax schemes than civic (right-wing) mayors.

These results confirm the hypothesis that introducing local tax progressivity stimulates strategic behaviour of decision-makers, and the main channel seems to be the possibility of targeting different income groups. The result could also be due to the fact that higher flexibility makes tax manipulation easier and less expensive for local administrators. Indeed, it is now possible to divert smaller amounts of money while, with a flat tax, manipulation was more costly implying a decrease in the overall rate. In addition to this, progressivity makes manipulation less visible and this decreases the "shame cost" of being strategic. The results of this paper underline how complex and subtle can be the effect of reforming taxes: tax progressivity has many positive aspects being an important tool to fight income inequality and to enrich the set of fiscal tools at the disposal of the mayor. Nevertheless, the results of this paper underline how local progressivity can have some drawbacks, leading to large fiscal distortions and to high levels of tax manipulation, with the amplification of the political budget cycle, at the local level.

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# Appendix 1: Additional figures

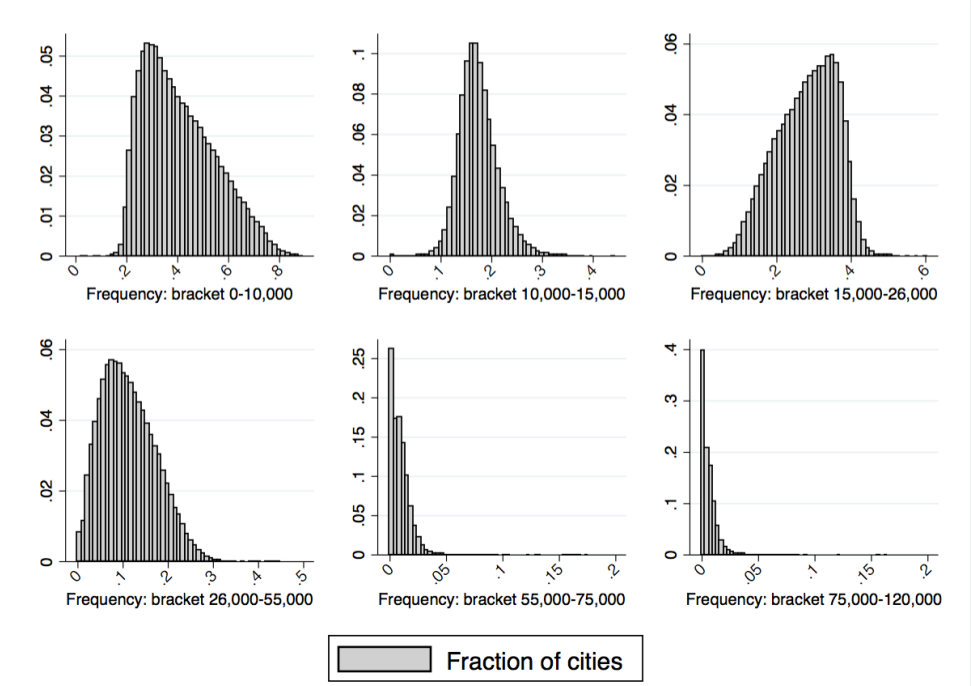



Figure 17: Distribution of fraction of population in each bracket.

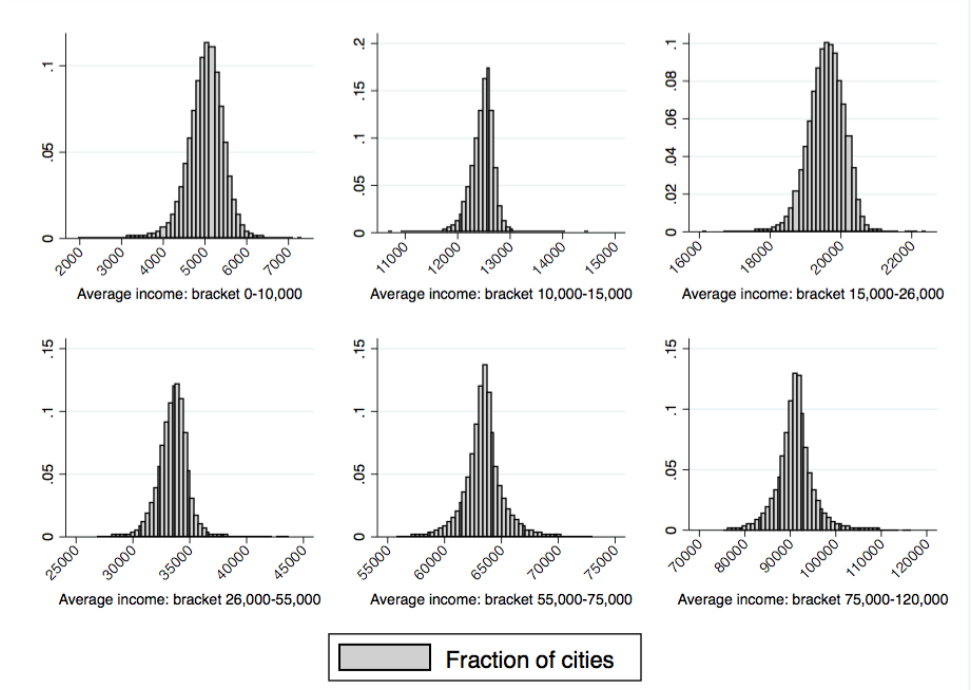
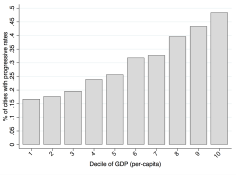
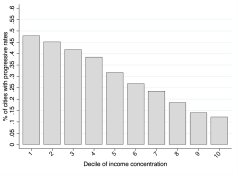


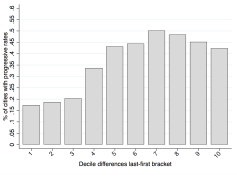

Figure 18: Distribution of average income in each bracket.



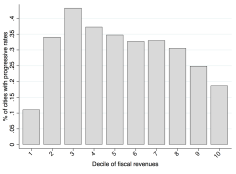

(a) Local GDP



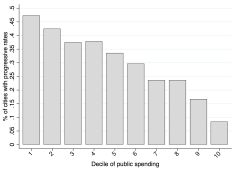

(b) Income concentration



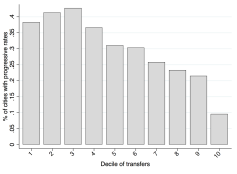

(c) Difference first-last bracket



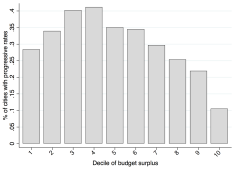

(d) Fiscal revenues



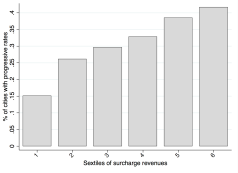

(e) Total expenditures



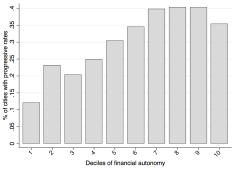

(f) Governmental transfers



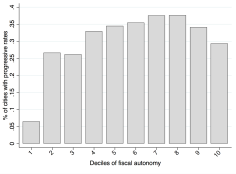

(g) Deficit



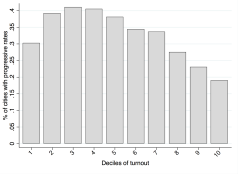

(h) Revenue from the surcharge



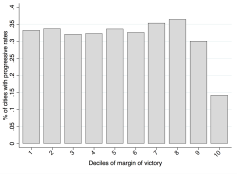

(i) Financial autonomy



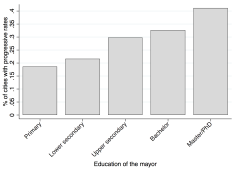

(j) Fiscal autonomy



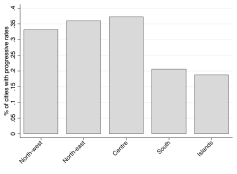

(k) Local turnout



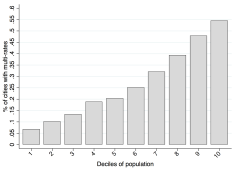

(l) Margin of victory




(m) Education of the mayor

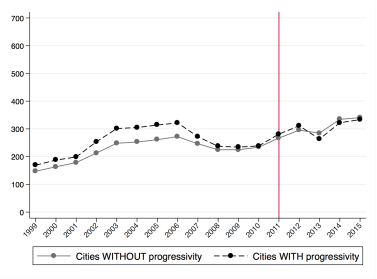



(n) Macro-region

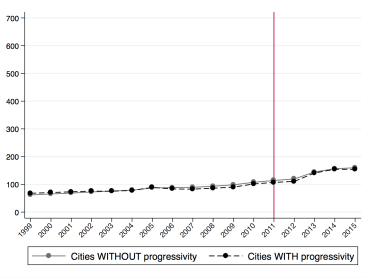



(o) Population

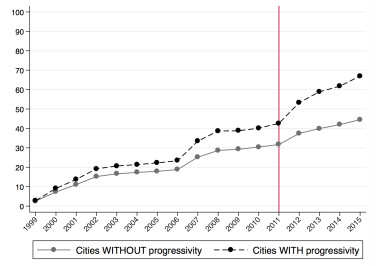
Figure 19: Characteristics of cities introducing progressivity




(a) Fees

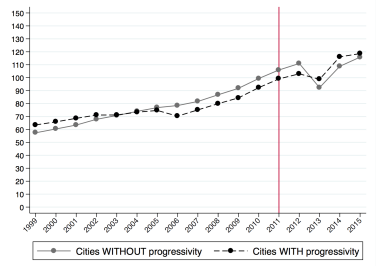



(b) Taxes

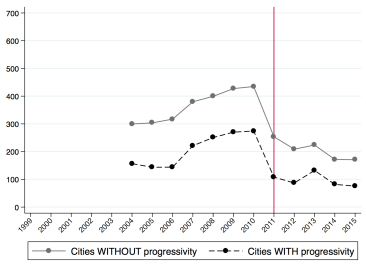



(c) Municipal surcharge


(d) Real estate tax




(e) Waste tax




(f) Governmental transfers

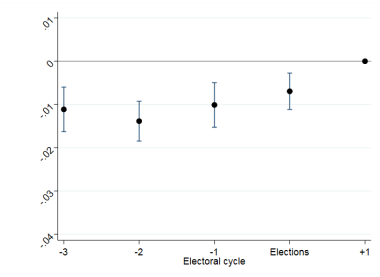

(g) Total expenditures


(h) Current expenditures

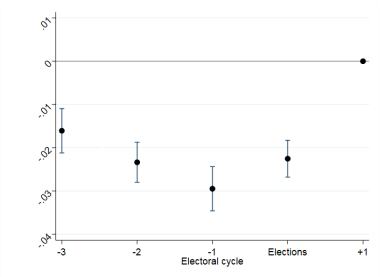

(i) Capital expenditures

Figure 20: Evolution of balance sheet components

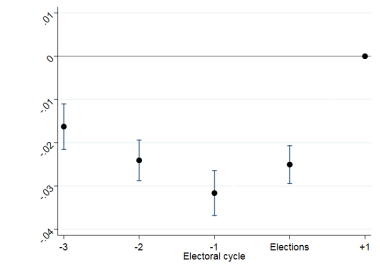
Figure 21: Marginal effects of the reform for each income bracket rates



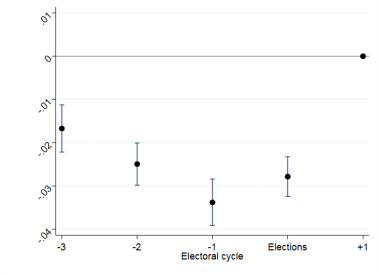

(a) Marginal effect: rate 0-15,000



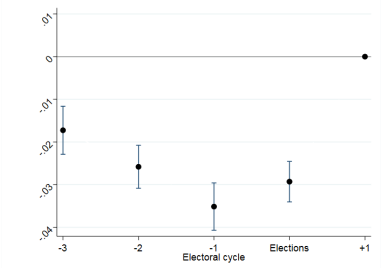

(b) Marginal effect: rate 15,000-26,000




(c) Marginal effect: rate 26,000-55,000



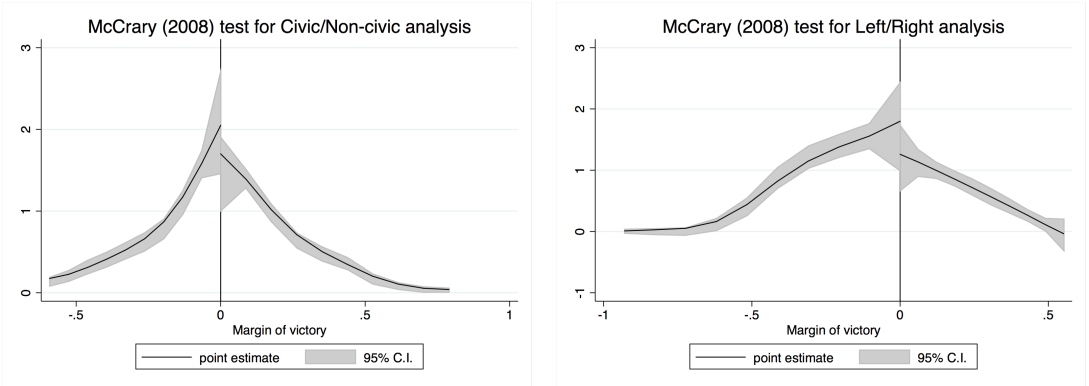

(d) Marginal effect: rate 55,000-75,000




(e) Marginal effect: rate 75,000-120,000

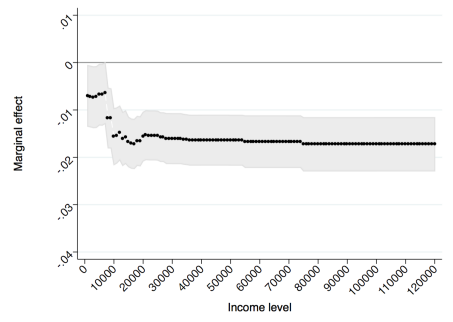
*Notes*: This figure shows the marginal effect of the reform for single wage brackets rates in Euros (columns 1-6 of Table 13).

Figure 26: Manipulation test

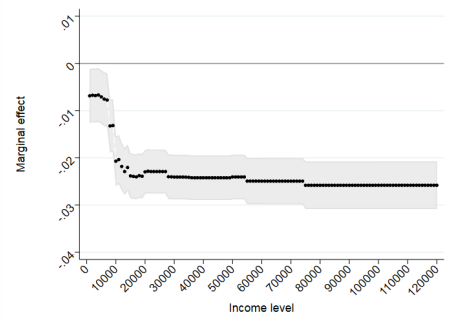



*Notes*: Frequency of municipal election in the time span 2010-2015. Manipulation testing has been performed using the local polynomial density estimators proposed in Cattaneo, Jansson and Ma (2017). Left panel shows the outcome for civic/non-civic analysis, point estimate: -1.555. Right panel shows the outcome for left/right analysis, point estimate: -1.056.

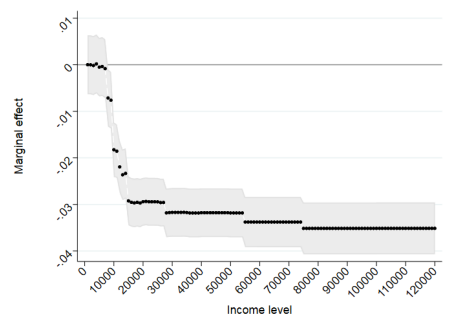
Figure 22: Marginal effects of the reform for each thousand of income



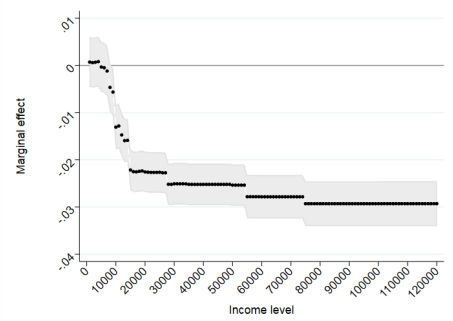

(a) Three years before elections




(b) Two years before elections



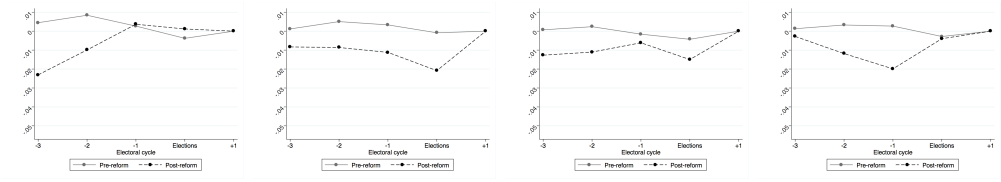

(c) Pre-electoral year



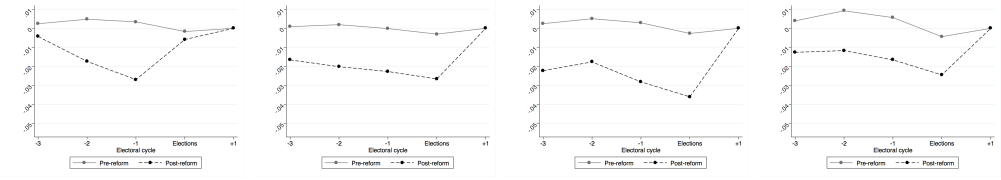

(d) Electoral year

*Notes*: This figure shows the marginal effect of the reform for each thousand of taxable income in Euros, showing separately the effect on each position of the political cycle: three years before elections, two years before elections, pre-electoral year and electoral year. These coefficients result from the main Diff-in-Diff regression (as defined in section 6.2), with the dependent variable being the tax rate associated to each thousand of income in the range 1,000-120,000 [regression tables for these outcomes are not shown and are available upon request].

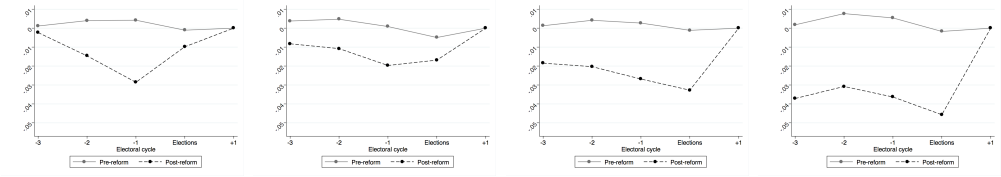
Figure 23: Heterogeneous effects of the reform according to the fraction of population in each brackets (divided in quartiles)



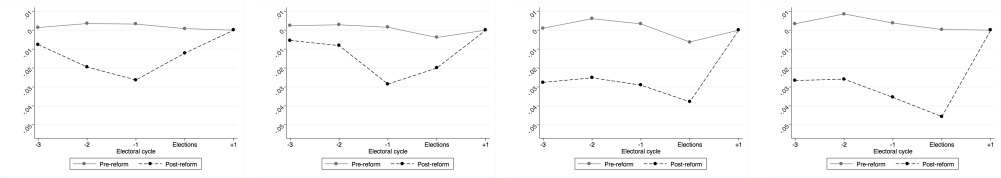

(a) Income bracket 0-15,000: first/second/third/fourth quartile



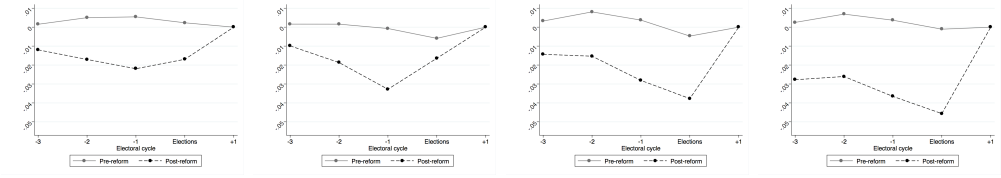

(b) Income bracket 15,000-26,000: first/second/third/fourth quartile




(c) Income bracket 26,000-55,000: first/second/third/fourth quartile



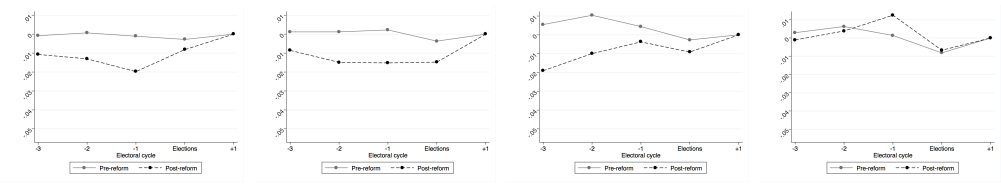

(d) Income bracket 55,000-75,000: first/second/third/fourth quartile



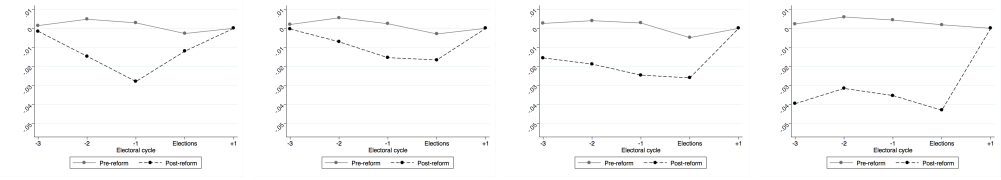

(e) Income bracket 75,000-120,000: first/second/third/fourth quartile

*Notes*: This figure shows the fluctuations of surcharge rates along the political cycle for single wage brackets in Euros, differential according to the fraction of taxpayers in each income brackets. The fraction of taxpayers is measured as the ratio between the number of taxpayers in a bracket and the total number of taxpayers and the sample is divided in quartiles according to the frequency in the reference year 2011. Pre-reform line shows the fluctuation of the tax rate before the reform, 1999-2011, with the plot of $\hat{\beta}_1$ coefficient of regression model. Post-reform lines show fluctuation of single brackets rates, after the reform, 2012-2015, with the plot of $\hat{\beta}_1 + \hat{\beta}_2$ coefficient of regression model. [Regression tables are omitted and are available upon request]

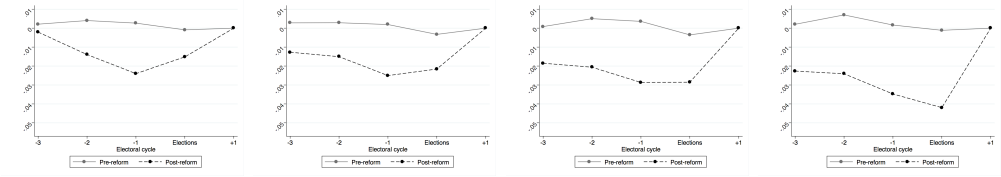
Figure 24: Heterogeneous effects of the reform according to the average income in each brackets (divided in quartiles)



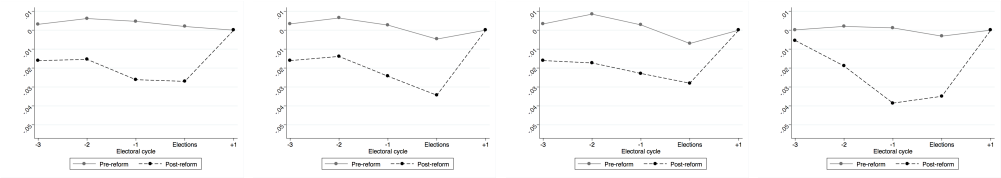

(a) Income bracket 0-15,000: first/second/third/fourth quartile



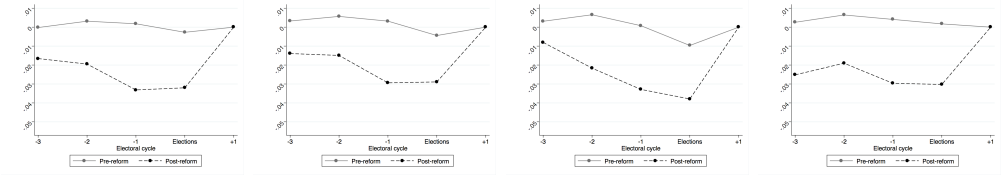

(b) Income bracket 15,000-26,000: first/second/third/fourth quartile




(c) Income bracket 26,000-55,000: first/second/third/fourth quartile



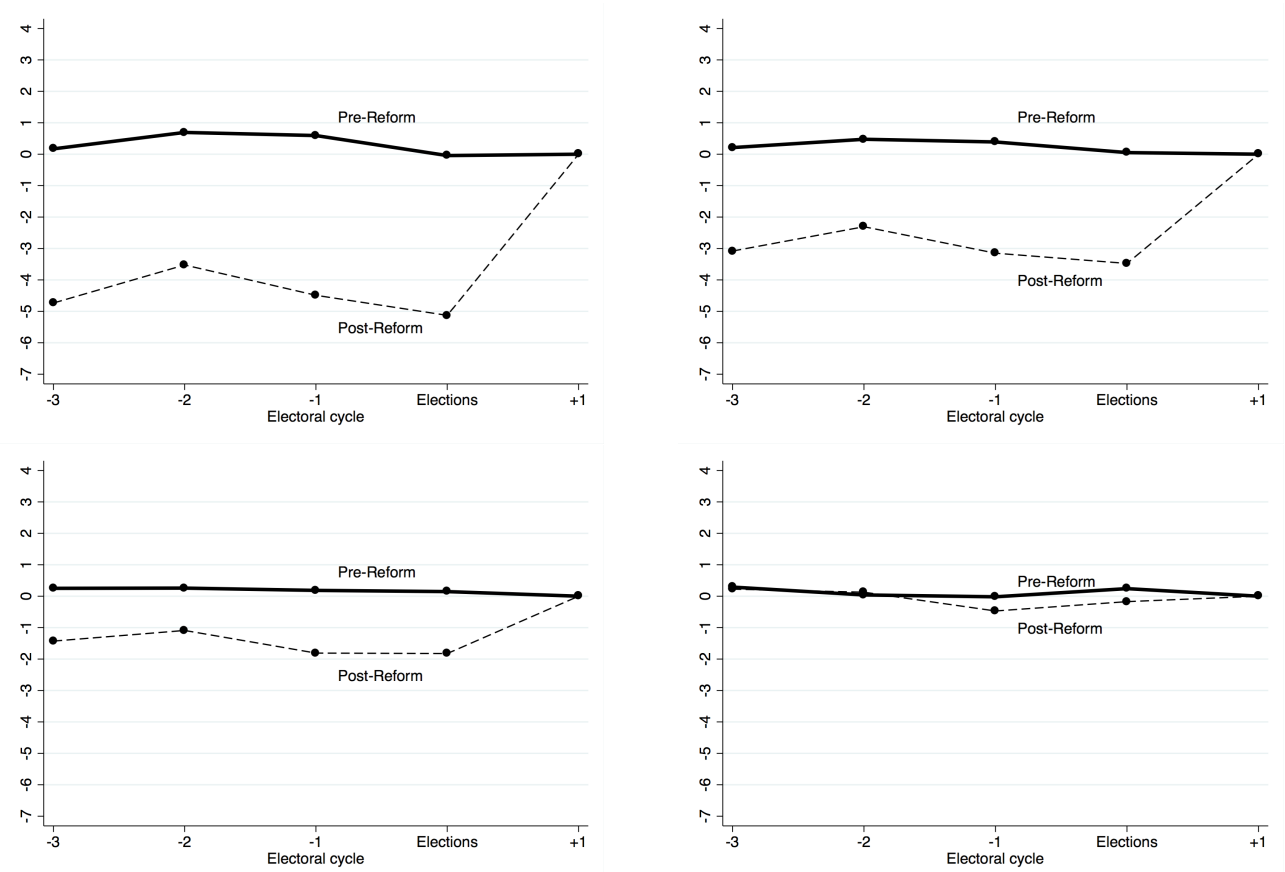

(d) Income bracket 55,000-75,000: first/second/third/fourth quartile




(e) Income bracket 75,000-120,000: first/second/third/fourth quartile

*Notes*: This figure shows the fluctuations of surcharge rates along the political cycle for single wage brackets in Euros, differential according to the average income in each brackets. The average income is measured as the ratio between aggregate income in a bracket and total number of taxpayers in that bracket and the sample is divided in quartiles according to the average income in the reference year 2011.. Pre-reform line shows the fluctuation of the tax rate before the reform, 1999-2011, with the plot of $\hat{\beta_1}$ coefficient of regression model. Post-reform lines show fluctuation of single brackets rates, after the reform, 2012-2015, with the plot of $\hat{\beta_1} + \hat{\beta_2}$ coefficient of regression model. [Regression tables are omitted and are available upon request]

Figure 25: Graphical evidence of Triple-Diff analysis - Per-capita surcharge revenues.




*Notes*: The graph plots coefficients of column (2) of Table 6. "Pre-Reform" line describes the cycle before the reform, 1999-2011, and it varies only with income concentration: coefficient $\hat{\beta'}_1 + \hat{\beta'}_5 \cdot H_{i,2011}$; "Post-Reform" line describes the cycle after the reform, 2012-2015, with different levels of pre-determined income concentration: $\hat{\beta'}_1 + \hat{\beta'}_3 + (\hat{\beta'}_5 + \hat{\beta'}_6) \cdot H_{i,2011}$.The four panels show the political budget cycle in correspondence of different quartiles of the income concentration index, $H_{i,2011}$. Budget lines are in deviation from the mean level in the post-electoral year.

# Appendix 2: Additional tables

Table 11: Municipal surcharge average rate for three fiscal types.

| Fiscal regime | Av.rate | St.dev. |
|---|---|---|
| **flat tax** | .419 % | .189 |
| **flat tax+exemption**<br>**av. threshold:** € 10,560 | .601 % | .189 |
| **differentiated tax:** | | |
| € 1,000 | .173 % | .234 |
| € 5,000 | .180 % | .236 |
| € 10,000 | .340 % | .235 |
| € 25,000 | .540 % | .150 |
| € 50,000 | .633 % | .127 |
| € 75,000 | .766 % | .083 |

*Note*: Average tax rates are computed for the time span 1999-2015, according to the fiscal type adopted by each municipalities. Municipalities that set the surcharge equal to zero are excluded from this sample.

Table 12: Average surcharge rates of single wage groups within cities/electoral term.

| Wage group | mean | sd | min | max |
|---|---|---|---|---|
| € 0-10,000 | .167 | .206 | 0 | .8 |
| € 10,000-15,000 | .443 | .247 | 0 | .81 |
| € 15,000-26,000 | .563 | .187 | 0 | .9 |
| € 26,000-55,000 | .595 | .176 | 0 | .9 |
| € 55000-75,000 | .624 | .174 | 0 | .9 |
| € 75000-120000 | .642 | .178 | 0 | .9 |
| more than € 120,000 | .642 | .178 | 0 | .9 |
| $N$ | 32,287 | | | |

*Notes*: The table shows average surcharge rate of each wage group, for the post-reform period (2012-2015), for municipalities that use at least once flexibility.

Table 13: Effect of the reform on surcharge manipulation - single tax rates

| Dependent variable: | rate 0-15,000 | rate 15,000-26,000 | rate 26,000-55,000 | rate 55,000-75,000 | rate 75,000-120,000 | rate more than 120,000 |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 3 years before elections | 0.00136 | 0.00208 | 0.00203 | 0.00200 | 0.00198 | 0.00198 |
| | (0.000717)* | (0.000679)*** | (0.000680)*** | (0.000682)*** | (0.000683)*** | (0.000683)*** |
| 2 years before elections | 0.00390 | 0.00494 | 0.00490 | 0.00489 | 0.00489 | 0.00488 |
| | (0.00101)*** | (0.000924)*** | (0.000923)*** | (0.000926)*** | (0.000928)*** | (0.000928)*** |
| 1 year before elections | 0.00110 | 0.00278 | 0.00272 | 0.00269 | 0.00268 | 0.00267 |
| | (0.00105) | (0.000903)*** | (0.000903)*** | (0.000907)*** | (0.000910)*** | (0.000910)*** |
| Election year | -0.00292 | -0.00187 | -0.00184 | -0.00180 | -0.00177 | -0.00177 |
| | (0.000866)*** | (0.000752)** | (0.000753)** | (0.000755)** | (0.000757)** | (0.000757)** |
| 3 years before elections*Post-reform | -0.0111 | -0.0161 | -0.0163 | -0.0167 | -0.0173 | -0.0173 |
| | (0.00262)*** | (0.00262)*** | (0.00268)*** | (0.00278)*** | (0.00287)*** | (0.00287)*** |
| 2 years before elections*Post-reform | -0.0138 | -0.0234 | -0.0241 | -0.0249 | -0.0258 | -0.0258 |
| | (0.00235)*** | (0.00236)*** | (0.00240)*** | (0.00249)*** | (0.00257)*** | (0.00257)*** |
| 1 year before elections*Post-reform | -0.0101 | -0.0295 | -0.0316 | -0.0338 | -0.0351 | -0.0351 |
| | (0.00263)*** | (0.00261)*** | (0.00265)*** | (0.00275)*** | (0.00283)*** | (0.00283)*** |
| Election year*Post-reform | -0.00695 | -0.0225 | -0.0250 | -0.0278 | -0.0293 | -0.0293 |
| | (0.00215)*** | (0.00217)*** | (0.00223)*** | (0.00233)*** | (0.00242)*** | (0.00242)*** |
| Mean of the dep. variable | .271 | .299 | .302 | .304 | .306 | .306 |
| Municipality & Macro-region Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time trends & Political/Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 127,809 | 127,809 | 127,809 | 127,809 | 127,809 | 127,809 |
| Adjusted $R^2$ | 0.735 | 0.776 | 0.775 | 0.770 | 0.766 | 0.766 |

*Notes*: Dependent variables are the set of surcharge rates for each income bracket. The specification includes municipality and macro-region/year fixed effect as well as region specific time trend, log of population, characteristics of mayors (*i.e.* education, age, proportion of women), turnout in last elections, dummy for a full mandate, dummy for term limit and variables to characterize income distribution (*i.e.* fraction of population in all wage brackets, total number of tax-payers and total amount of income declared). Moreover, electoral groups fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# Table 14: Test for continuity of municipalities and mayors characteristics

| **Panel A: Civic/Non-civic analysis** | | | | | |
|---|---|---|---|---|---|
| | Population | Share of immigrants | Number of candidates | Number of taxpayers | Aggregate income declared |
| | (1) | (2) | (3) | (4) | (5) |
| RD_Estimate | 2810.1 | 0.00345 | -0.123 | 1736.6 | 44290129.8 |
| | (2647.6) | (0.00536) | (0.258) | (1487.2) | (37129937.0) |
| Year/Macro-region FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 1278 | 1278 | 1316 | 1318 | 1318 |
| Bandwidth | 0.107 | 0.184 | 0.191 | 0.096 | 0.094 |
| | Education mayor | Age mayor | Gender mayor | Mayor term limited | |
| | (1) | (2) | (3) | (4) | |
| RD_Estimate | -0.0856 | 0.0454 | 0.00962 | -0.0906 | |
| | (0.0999) | (1.582) | (0.0549) | (0.0687) | |
| Year/Macro-region FE | Yes | Yes | Yes | Yes | |
| Observations | 1264 | 1307 | 1309 | 1318 | |
| Bandwidth | 0.185 | 0.178 | 0.216 | 0.205 | |
| | Current expenditure (PC) | Capital expenditure (PC) | Total fiscal revenues (PC) | | |
| | (1) | (2) | (3) | | |
| RD_Estimate | -81.13 | -7.276 | -4.586 | | |
| | (49.27)* | (92.17) | (30.61) | | |
| Year/Macro-region FE | Yes | Yes | Yes | | |
| Observations | 1255 | 1255 | 1273 | | |
| Bandwidth | 0.188 | 0.226 | 0.183 | | |
| **Panel B: Left/Right analysis** | | | | | |
| | Population | Share of immigrants | Number of candidates | Number of taxpayers | Aggregate income declared |
| | (1) | (2) | (3) | (4) | (5) |
| RD_Estimate | 19930.6 | -0.00366 | 0.0376 | 5034.3 | 52840792.6 |
| | (15546.7) | (0.00854) | (0.521) | (6749.4) | (169167672.1) |
| Year/Macro-region FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 446 | 446 | 479 | 479 | 479 |
| Bandwidth | 0.084 | 0.208 | 0.167 | 0.083 | 0.08 |
| | Education mayor | Age mayor | Gender mayor | Mayor term limited | |
| | (1) | (2) | (3) | (4) | |
| RD_Estimate | 0.0653 | 3.200 | 0.0755 | -0.0838 | |
| | (0.169) | (3.125) | (0.103) | (0.120) | |
| Year/Macro-region FE | Yes | Yes | Yes | Yes | |
| Observations | 459 | 468 | 474 | 479 | |
| Bandwidth | 0.123 | 0.124 | 0.128 | 0.187 | |
| | Current expenditure (PC) | Capital expenditure (PC) | Total fiscal revenues (PC) | | |
| | (1) | (2) | (3) | | |
| RD_Estimate | -17.24 | -26.01 | -93.20 | | |
| | (67.57) | (85.04) | (39.69)** | | |
| Year/Macro-region FE | Yes | Yes | Yes | | |
| Observations | 438 | 438 | 444 | | |
| Bandwidth | 0.133 | 0.107 | 0.178 | | |

*Notes*: Estimation by RDD-LLR using the Calonico, Cattaneo and Titiunik (2014) optimal bandwidth h selector. The unit of observation of the analysis is the legislature over a time span 2010-2015. Dependent variables refer to the last year of previous term. All specifications include year and macro-region fixed effects. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$