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Date 10/12/2018

Essays in Political Economy and Public Finance

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Abstract

This thesis consists of three chapters on political economy and public finance.

In the first chapter we study the effects of local corruption on political participation which are mediated by the press. Focusing on Italian municipalities for the period 1999-2014, we generate a daily and local measure of exposure to corruption, screening newspaper articles of the main Italian press agency. We concentrate on local elections and, in an event-study analysis, we find two main results. On the one hand, corruption exposure affects *citizens* participation: voter turnout decreases and there is no effect on politicians selection. On the other hand, corruption impacts on *politicians* participation: the number of candidates and electoral lists decrease after a scandal and candidates with political tenure are more likely to run, while freshmen leave the competition. These results suggest that exposure to corruption has general and negative effects on political participation, leading to a worsening of the local political background.

In the second chapter we study the effects of introducing income redistribution at the municipal level, with the adoption of local tax progressivity. In particular, we analyse whether this modifies the incentives of politicians to be strategic and leads to higher levels of 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 different tax rates for distinct groups. In a *Difference-in-Differences* analysis 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 to estimate a *Triple-Differences* analysis. The main results are confirmed. And finally, we show that manipulation, induced by the use of progressivity, 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.

The third chapter studies whether politicians manipulate fiscal instruments for personal interests. We focus on local personal income tax (PIT), annually decided by Italian mayors, a progressive instrument which allows administrators to tax differently distinct wage groups. We exploit discontinuities in mayors income levels, based on population thresholds, which determine their wage brackets and we study, with a *Regression Discontinuity Design* technique, whether mayors systematically apply lower rates to their own tax bracket. The empirical analysis focuses on 5,000 inhabitants threshold, in which mayors' income sharply moves from the second to the third wage bracket. The main results suggest that mayors whose income reaches the third fiscal bracket tend to tax less the third group, in comparison to mayors of similar cities that are subject to the second bracket rate. Moreover, large heterogeneous effects emerge in terms of mayors' characteristics. These results suggest that electoral incentives are not the unique source of strategic behaviour and that personal interests can play a significant role in fiscal manipulation.

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Chapter 1

Does Progressivity Always Lead to Progress? The Impact of Local Redistribution on Tax Manipulation

1.1 Introduction

The strategic manipulation of the budget in order to get electoral advantage, phenomenon known as political budget cycle, is a relevant issue for modern democracies. It generates sub-optimal resource allocations, which lead to sizeable waste of public money. This phenomenon has been deeply documented in the past twenty years: a large literature indicates its relevance both from a cross-country perspective (Alesina et al. 1997, Persson, Tabellini 2002) and at the local level (Kneebone, McKenzie 2001, Akhmedov, Zhuravskaya 2004). Nevertheless, very little is known about which fiscal institutions promote or discourage political budget cycle. In particular, there is almost no evidence about the interaction between the design of fiscal tools and the incentive to manipulate balance sheets, which is a vital issue for the policy-maker. In this paper, we focus on this issue: we concentrate on the impact of *fiscal progressivity*, defined as the ability of the decision-maker to differentiate tax rates between tax-payers, on the political budget cycle.

From a theoretical point of view, 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, a highly flexible fiscal instrument allows the decision-maker to tax differently distinct groups and this could attenuate incentives for fiscal manipulation: the incumbent could signal his type with tax reductions targeted to specific groups, rather than cutting taxes to the entire population. The amount of diverted resources for political purposes may reduce in equilibrium. On the other hand, an increase in fiscal flexibility could encourage and ease the practice. First, tax manipulation is now less expensive and more affordable, as it is possible to divert smaller amounts of public resources than with a flat tax; moreover, the possibility to target a specific electoral group makes the tax cut more effective. 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. This reduces the "blame cost" associated with tax manipulation (Pelzman 1992). This could result in an exacerbation of 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.* 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 political cycle, before the reform, for cities in different electoral 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 average tax rate¹. 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: giving more flexible fiscal instruments to local administrators results in more strategic behaviour and in larger 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 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 suggest that mayors use progressivity to target specific groups and perform *ad hoc* manipulation with the aim of maximizing the electoral consensus, without reducing too much fiscal revenues.

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

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 confirm 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 flexible tax rates. The analysis is set as a close election RDD, 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, Block (2002) finds consistent evidence of deficit manipulation with a sample of Sub-Saharan African countries and there is convincing evidence for countries of the European Union (Mink, De Haan 2006, Efthyvoulou 2012). 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, 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 citizens². Bracco et

²Oates (1988) studies the concept of *fiscal illusion* defined as “the notion that systematic misperception

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 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, many 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.

1.2 Institutional background

1.2.1 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 individual³; 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:

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

- The national rate (Nat_y) follows the scheme in force, last modification to the scheme has been made in 2007⁴. 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.

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 public budget.

³Taxable income is the sum of following incomes: self-employed, employee, capital, land, enterprise and other incomes (including capital gains).

⁴In 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*⁵. 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 .

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 1.1 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 1.1, 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 1.1 here]

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.

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 traffic⁶ for the keyword *Municipal surcharge (Addizionale comunale)* with monthly aggregation, for the time span 2004-2016 for all Italian regions; Figure 1.2 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 month⁷. Left panel of Figure 1.2 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 deadline for municipal surcharge decision is set by December 20, and this stimulates attention in following days on this tax. Right panel of Figure 1.2 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.

⁵Income 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).

⁶Google 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).

⁷There is another smaller payment, the *acconto*, which is due within the end of November.

[Figure 1.2 here]

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 1.3 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 1.3 contains Google traffic for keywords *Municipal surcharge*, *ICI rate* property 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 1.3 here]

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 1.1 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 taxpayers 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 people⁸. Second, Table 1.2 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.

1.2.2 The reform

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 fiscal instrument) to a flexible tax, where different groups of tax-payers face different rates (progressive fiscal instrument). 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 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%⁹.

⁸In 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.

⁹As 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*

- **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 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 raising fiscal flexibility of the municipal surcharge. The reform was a sudden intervention included in the package proposed by the 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 local political situation as it was mainly driven by the harsh sovereign debt crisis that hit Italy 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 change is 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)¹⁰. By the way, these measures were temporary in both cases, and this was publicly known, and their application was limited¹¹. I deal with this issue running, as a robustness check, the main specification excluding cap years from the sample.

1.2.3 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 surcharge¹². 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

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).

¹⁰Another minor reform was the increase of the maximum rate to 0.9% for the city of Rome in 2011.

¹¹On 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

¹²Main responsibilities of the municipal council include overseeing activity of the mayor and the executive committee and approving policies that are proposed.

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, 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)¹³. 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. 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 1946¹⁴. 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 discuss more extensively this issue in section 5.

[Figure 1.4 here]

Figure 1.4 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.

1.3 Data

1.3.1 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 main local taxes such as property tax, waste tax and municipal surcharge. Table 1.3, Panel A, shows descriptive statistics of municipal fiscal revenues: fees (the category including property tax and municipal surcharge) accounts, on average,

¹³The constraint only applies to consecutive terms.

¹⁴In 1946 first local 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.

for 65.7% while taxes (including waste tax) only for 24.3%. Moreover, average per-capita revenue of municipal surcharge is €27.6.

Second, we make use of data on municipal surcharge, from 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 exemption¹⁵. Table 1.4 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, marginal rate raises with the taxable income: the table reports average rate for six wage levels, the rate is very moderate for low incomes, .173% for €1,000, and it is large (close to the maximum) for high incomes, .766% for €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; in particular, this dataset contains information on the number of tax payers and on total wage declared for eight wage groups which are (in Euro): 1) less or equal to 0, 2) 0-10,000, 3) 10,000-15,000, 4) 15,000-26,000, 5) 26,000-55,000, 6) 55,000-75,000, 7) 75,000-120,000 and 8) more than 120,000. Wage distribution varies considerably across municipalities and across years. Figure 1.5 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 changed 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 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, Figure 1.6 shows the distribution of fraction of population in each bracket, averaged over time: from the figure cities of the sample result to be highly heterogeneous in terms of fraction of population in the first four income brackets, while the last two groups always represent a very small portion of city population. Nevertheless, Figure 1.7 shows the distribution of the average income: from the figure it is evident that this raises as we move to higher income brackets and the distribution shifts gradually to the right.

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 Italian Statistical Office (*ISTAT*) on population of municipalities over time.

The sample of the analysis consists in 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.

[Figure 1.5 here]

[Figure 1.6 here]

¹⁵Sometimes, 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 1.7 here]

1.3.2 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 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-payer¹⁶. Table 1.3, Panel A, includes the description of surcharge per-capita revenue, that has been already commented; Table 1.3, 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 1.5 shows average tax rates of each bracket, for the sample of cities that introduced a 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).

1.4 Effects of the reform

1.4.1 Introduction of local progressivity

One important issue is whether municipalities used 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 1.8 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 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 1.8 here]

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 1.9 shows average surcharge rate per income level in three fiscal regimes: in regime a

¹⁶We 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 are confirmed using this additional measure.

(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 may wonder 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 1.10 focuses on regime c and it shows the average surcharge rate as a function of the 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 1.9 here]

[Figure 1.10 here]

1.4.2 Characteristics of cities introducing progressivity

In this section we want to describe characteristics of cities introducing a progressive rate structure after the 2012 reform, in order to have an idea of the set of cities that adopt redistribution at the local level. Figure 1.11 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 1.11 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 cities¹⁷. Large values of the index suggest that income distribution is highly skewed toward the left and indicate cities with 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, that showed that redistribution is not always larger where income inequality is wider (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.

¹⁷Details 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.

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 affect 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).

1.4.3 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 1.12 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 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 differential trends in governmental transfers, sub-figure (f), total expenditures, sub-figures (g), current expenditures, sub-figure (h) and capital expenditures, sub-figure (i).

1.5 Empirical strategy: Diff-in-Diff

1.5.1 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 which is larger 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. Both these tests suggest that the different groups of cities are rather comparable and confirm that the manipulation emerges as local progressivity has been introduced locally, excluding the possibility of pre-trends in the main results. These tests 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.

1.5.2 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) on¹⁸, and \mathbf{C}_{it} characterizes the position in the cycle of municipality i :

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

¹⁸The reform in analysis was approved in 2011 and first year of application was 2012.

and zero otherwise, moreover c_{t+1} , the indicator for post-election year, is omitted to avoid multicollinearity and since it acts as a reference year¹⁹. 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 C_{it} to vary within municipalities as well. The Diff-in-Diff estimator is obtained by the interaction term $C_{it} \cdot Post_t$ which captures the effect of the reform on fiscal behaviour in different position of the political cycle.

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, a crucial aspect in this analysis is the income distribution which consistently affects the impact of the reform. With this aim, data on income distribution at the municipal level are included to control for the local income structure and its evolution: share of taxpayer population in the eight wage brackets in addition to the total number of taxpayers and the yearly aggregate income declared. Moreover, we include municipality fixed effects, δ_i , to control for municipal-specific, time-invariant, unobservables, for macro-region year fixed effects, ζ_{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.

1.5.3 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 1.16 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

¹⁹The choice of using post-electoral year as reference is not arbitrary: it follows from two pieces of evidence. 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.

than fifty percent of cities belonging to special regions²⁰, 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.

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 years²¹. Graphical results of this test are presented in the Figure 1.17. 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 over time. This result indicates that there are no pre-trends, taking place before the introduction of local progressivity, in surcharge manipulation.

[Figure 1.16 here]

[Figure 1.17 here]

1.6 Results: Diff-in-Diff

1.6.1 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 1.6 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 tax in the political cycle before the reform and they show that the strategic use of the tax is limited in this period, as the rate has very

²⁰Italian 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.

²¹The 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 I conduct this check for the period 2007-2015. Finally, I set the 2011, the last year before the reform, as the benchmark year of the analysis.

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 from 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 1.13 provides a graphical representation of results from Table 1.6. Left graphs of Figure 1.13 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, 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.

[Figure 1.13 here]

1.6.2 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 1.14 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 results are confirmed as manipulation is very limited in this period²². 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 levels²³. Moreover, Table 1.5, shows the average tax rate associated to each income group, focusing on the post-reform period, and it is evident that the surcharge rate quickly raises with income. This suggests that the optimal strategy of the mayors implies that tax rates of low income groups are constantly low in every position of the electoral cycle, while those of the middle-high income are high when election are far and they are strategically decreased as elections approach. 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, McKenzie (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 1.15 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 year²⁴.

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,

²²In 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.

²³These outputs are contained in Table 1.7: 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.

²⁴Figure 1.25, in the Appendix, 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.

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 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 1.1, 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 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.

[Figure 1.14 here]

[Figure 1.15 here]

1.6.3 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 1.18 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 of view. These results shed further light on the optimal strategy of Italian mayors: it seems that they take into account information on 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 information hypothesis, *i.e.* the fact that local politicians exploit their superior knowledge of the local background to make decisions about fiscal policy, 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 1.19 shows the graphical outputs for this analysis: also in this case, we divide cities in quartiles, according to the average income in each bracket, making use of 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.

[Figure 1.18 here]

[Figure 1.19 here]

1.7 Alternative empirical strategy: Triple-Diff

1.7.1 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 1.20 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 1.20 shows income distribution of a similar municipality, Rosarno, from southern Italy, that is highly concentrated in the first bracket: reform impact in this municipality is fairly limited as two thirds of the population belongs to one single group and the mayor cannot differentiate much income tax rate across the population. An alternative explanation for the reason why income distribution mediates this effect may be that in cities similar to Segrate, middle-high income groups are larger and they represent a consistent electorate, differently from the case of Rosarno. Thus, the incumbent may be more inclined 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.

[Figure 1.20 here]

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 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 reform²⁵. Furthermore, we rescale the index in the scale 0-100. Figure 1.20 shows the index at

²⁵We compute alternative versions of the concentration index: first, one version including only 6/5/4 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.

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 Rosarno (63.1), which is instead highly concentrated. Panel E of Table 1.3 shows descriptive statistics of the concentration index computed for year 2011: mean value is 25 with a non negligible level of average variation, around 10 points.

Nevertheless, in order to check that income concentration degree truly affects municipal surcharge rates, we present in Table 1.8 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 mediates the effect of the reform lowering effectiveness of flexible rates and this translates into moderate use of flexible 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.

1.7.2 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 divided in quartiles of income concentration.

The new empirical model to be estimated is as follows:

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

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, in the results presented here we make use of the index divided in quartiles of income 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 Difference-in-Difference-in-Differences estimator that captures the effect of

interest: in case the associated coefficient, β'_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.

1.7.3 Results: Triple-Diff

Table 1.9 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, $C_{it} \cdot Post_t$: estimated coefficients confirm the result found in the analysis of Table 1.6 that the reform amplifies political budget cycle as interaction terms are negative and significant with coefficients magnitude higher 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, $C_{it} \cdot H_{i,2011}$: 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 progressively: *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 concentration²⁶.

[Figure 1.21 here]

Figure 1.21 provides a graphical evidence of the result reported in Table 1.9 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 compare it with average

²⁶In 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.

rate in pre-reform period²⁷. 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, Figure 1.26).

1.8 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.

1.8.1 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 twice²⁸: 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 amplify 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 1.10 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.

²⁷In 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.

²⁸These modifications were introduced by following laws: *legge n. 289/2002, Art. 3* and *D.L. n. 112/2008, Art. 77 bis*.

The second minor reform took place in 2006 and it raises the maximum surcharge rate from a level of 0.5% to 0.8%²⁹. 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 1.10 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 these minor interventions did not play a consistent role in the emergence of the political budget cycle of the municipal surcharge.

1.8.2 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. Since these events are highly correlated with the contingent history of the town, they may confound the estimates as they may lead 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 1.10 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 1.10 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.

1.8.3 Simultaneous public finance intervention

Another potential threat to identification can be the presence of alternative 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 level³⁰.

²⁹This 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%.

³⁰Main 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 Italian cadastres with revaluation of property tax base; finally, another important intervention was the reduction in transfers from the central governments to municipalities.

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), (3-7) of Table 1.11 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)³¹. As a second analysis, we perform in columns (2-3) of Table 1.11 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.

1.9 Fiscal manipulation and re-election probability

In previous sections we find that progressivity stimulates manipulation of the municipal surcharge and it is likely that this behaviour is motivated by obtaining electoral consensus. In this part 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 2000-2015³²: 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

³¹The real estate tax on the first residency was then abolished again in 2013, by centre-left Government led by Matteo Renzi.

³²We exclude elections taking place in 1999 as this is the year of introduction of the surcharge in Italy.

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 municipal specific characteristics. Moreover robust standard errors are clustered at the municipal level³³. Table 1.12, 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-election 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 affect differently re-election probability. Results are shown in Panel B of Table 1.12, where we only present coefficients for the rate in election years. 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 1.22 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 1.22 here]

³³The 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 β 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.

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.

1.10 Impact of partisanship on tax flexibility

1.10.1 Usage and degree of flexibility

In this paragraph we want to shed light on the political economy of tax flexibility. In particular, we explore whether partisanship of local administrators plays a role in determining whether and to what extent flexibility 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 1.23 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" (Govori 2015 quoting 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 municipality³⁴. Figure 1.23 shows corresponding values of the index, as expected when progressivity raises the index is larger.

[Figure 1.23 here]

1.10.2 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:

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

with Y_{it} 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 ; δ_t are year fixed effects, ζ_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

³⁴Mean 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.

the mayor wins with a narrow margin: the final sample includes, respectively, 1,235 and 436 elections.

1.10.3 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 1.13 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 identifying assumption is 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 1.24 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).

[Figure 1.24 here]

As a second step, we focus on the outcomes of the analysis. Panel A of Table 1.14 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 24,7% 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 1.14 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 flexibility at the local level.

1.11 Conclusions

In this contribution we explore whether fiscal tools design affects the practice of political budget cycle modifying incentives of politicians to manipulate taxes. In particular, we focus on tax progressivity, defined as the capability of the politician to differentiate the tax rate among different tax-payers, in the context of Italian municipalities. We exploit a reform that affects the local personal income tax, flat before, and that allows mayors to introduce differentiated tax rates or exemption thresholds. This unique natural experiment allows to test the effect of raising fiscal flexibility on tax manipulation. 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 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 tax rates in the last years of the term, the electoral and the pre-electoral years. Finally, we raise the issue whether partisanship of administrators affects the use of fiscal flexibility: 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, flexibility 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 larger fiscal distortions and to higher levels of tax manipulation, with the amplification of the political budget cycle, at the local level.

1.12 Figures

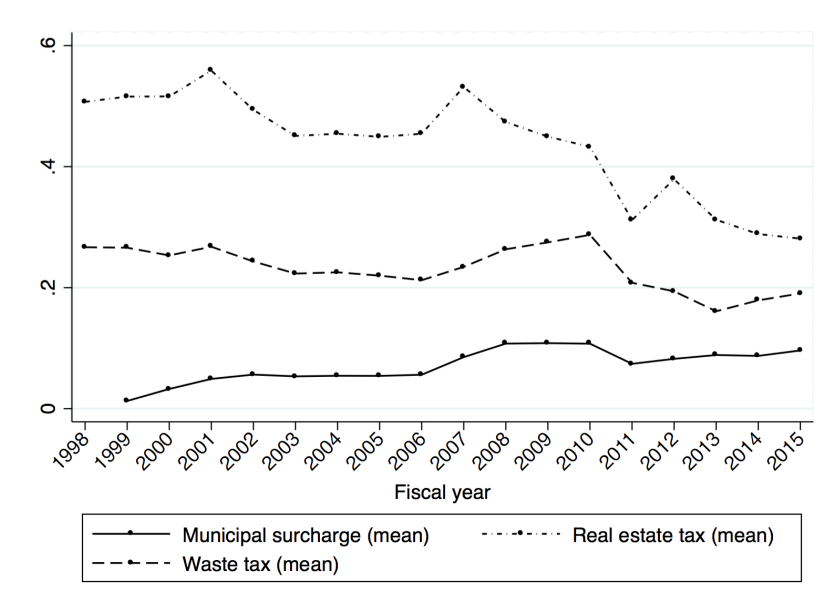


Figure 1.1: Proportion of fiscal revenues by years of property tax, waste tax and municipal surcharge, as percentage of aggregate fiscal revenues.

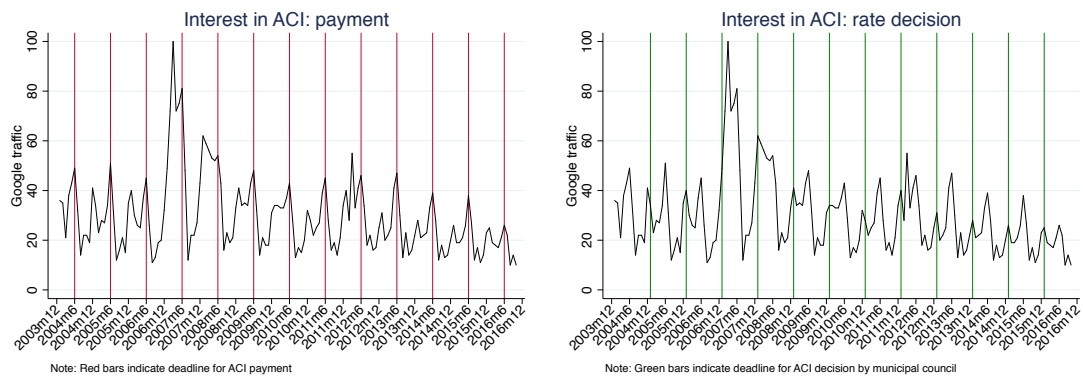


Figure 1.2: Attention to municipal surcharge in Italy.



Figure 1.3: Comparison of attention to municipal surcharge, other income taxes and real estate tax.

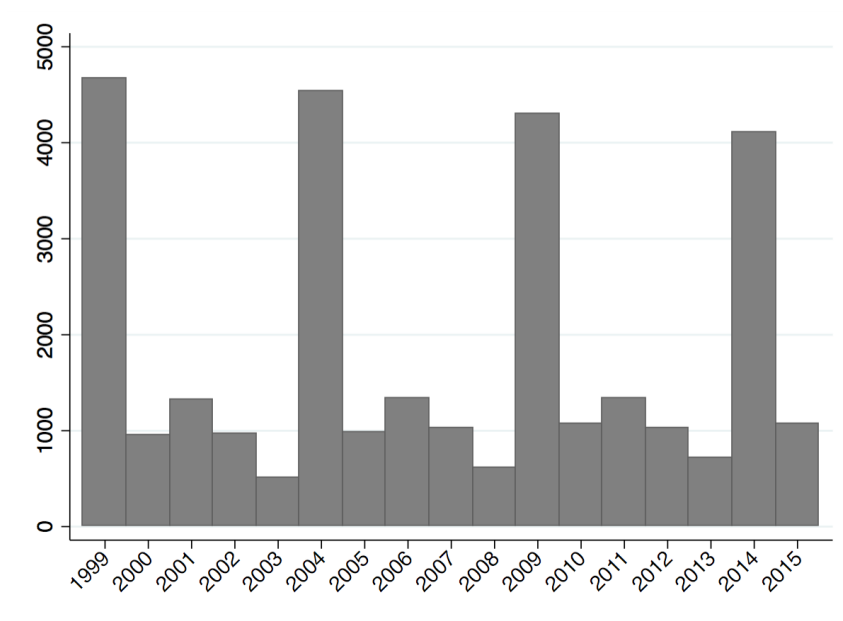


Figure 1.4: Number of municipalities having elections each year.

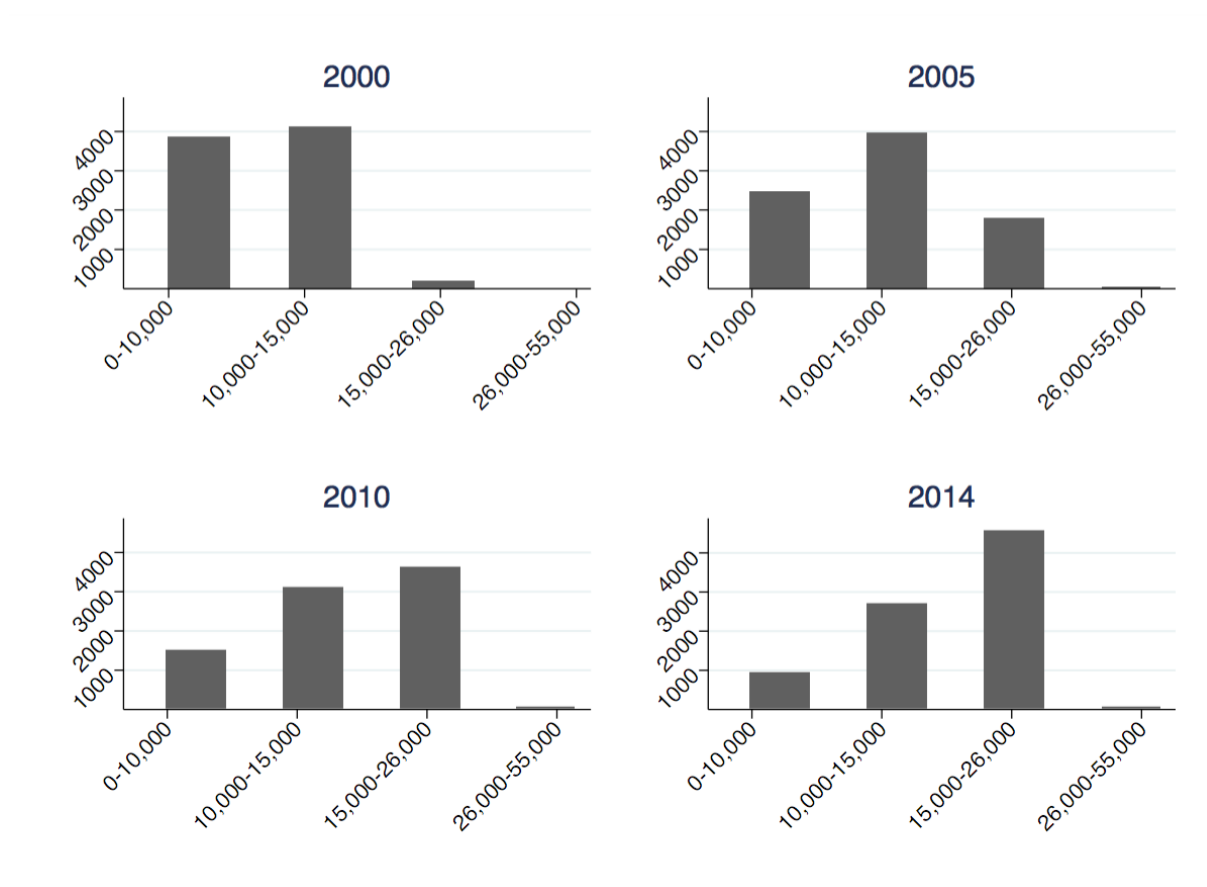


Figure 1.5: Aggregate frequency of the median tax-payers over time, for wage-groups.

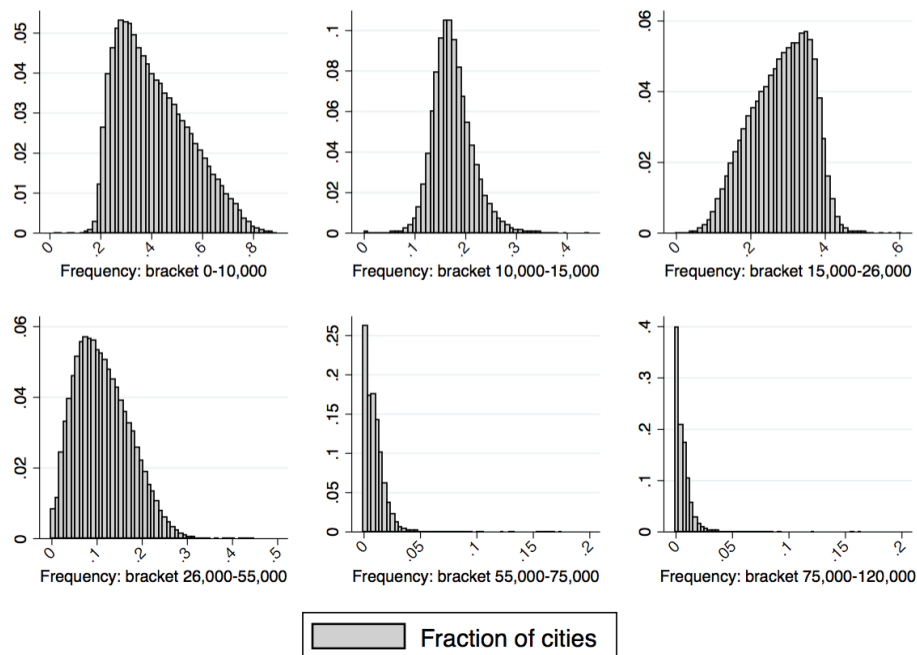


Figure 1.6: Distribution of fraction of population in each bracket.

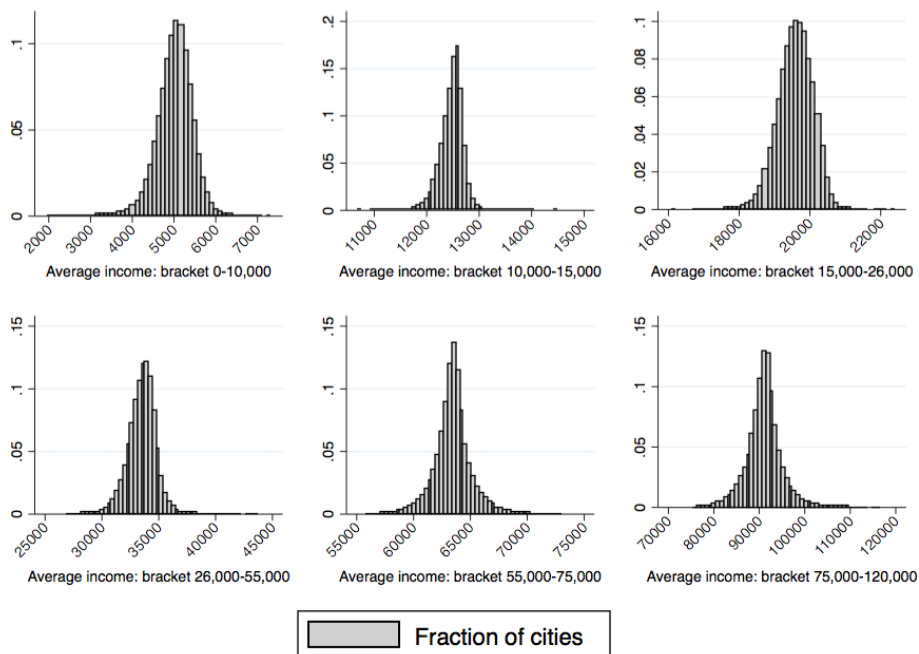


Figure 1.7: Distribution of average income in each bracket.

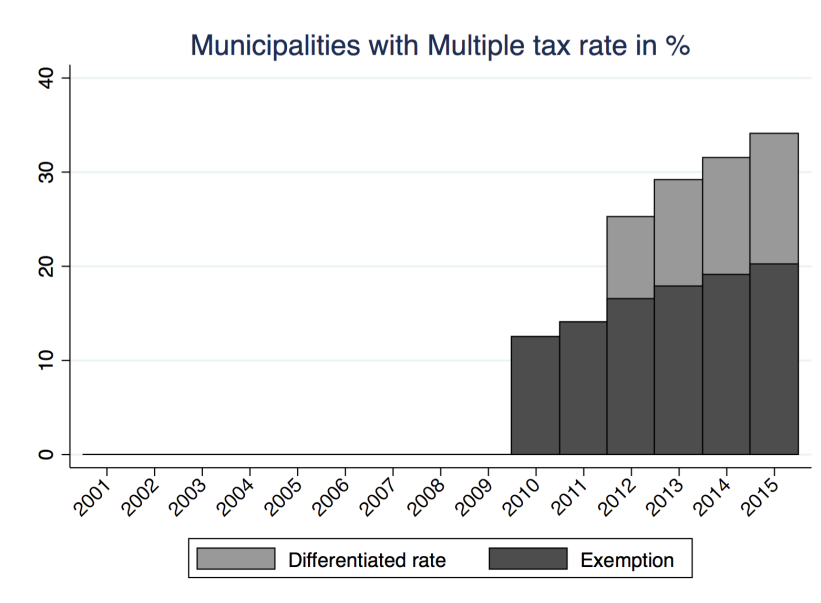


Figure 1.8: Percentage of municipalities adopting flexible rates (*i.e.* either *Exemption* or *Differentiated rates*), among all cities.

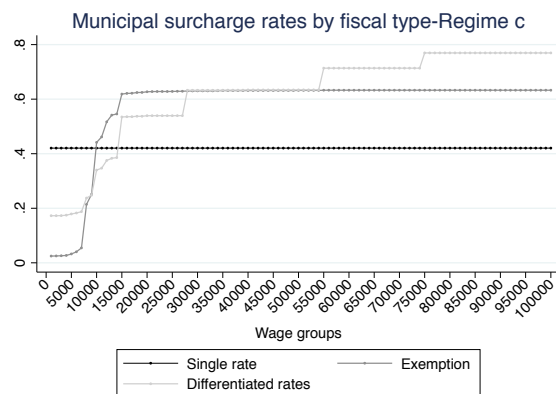
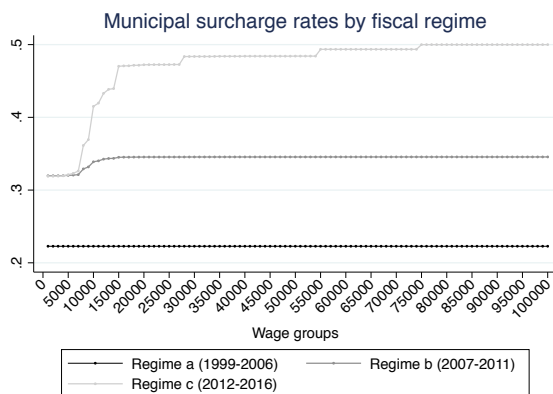


Figure 1.9: Municipal surcharge per income level, by fiscal regime

Figure 1.10: Municipal surcharge per income level in Regime c, by fiscal type

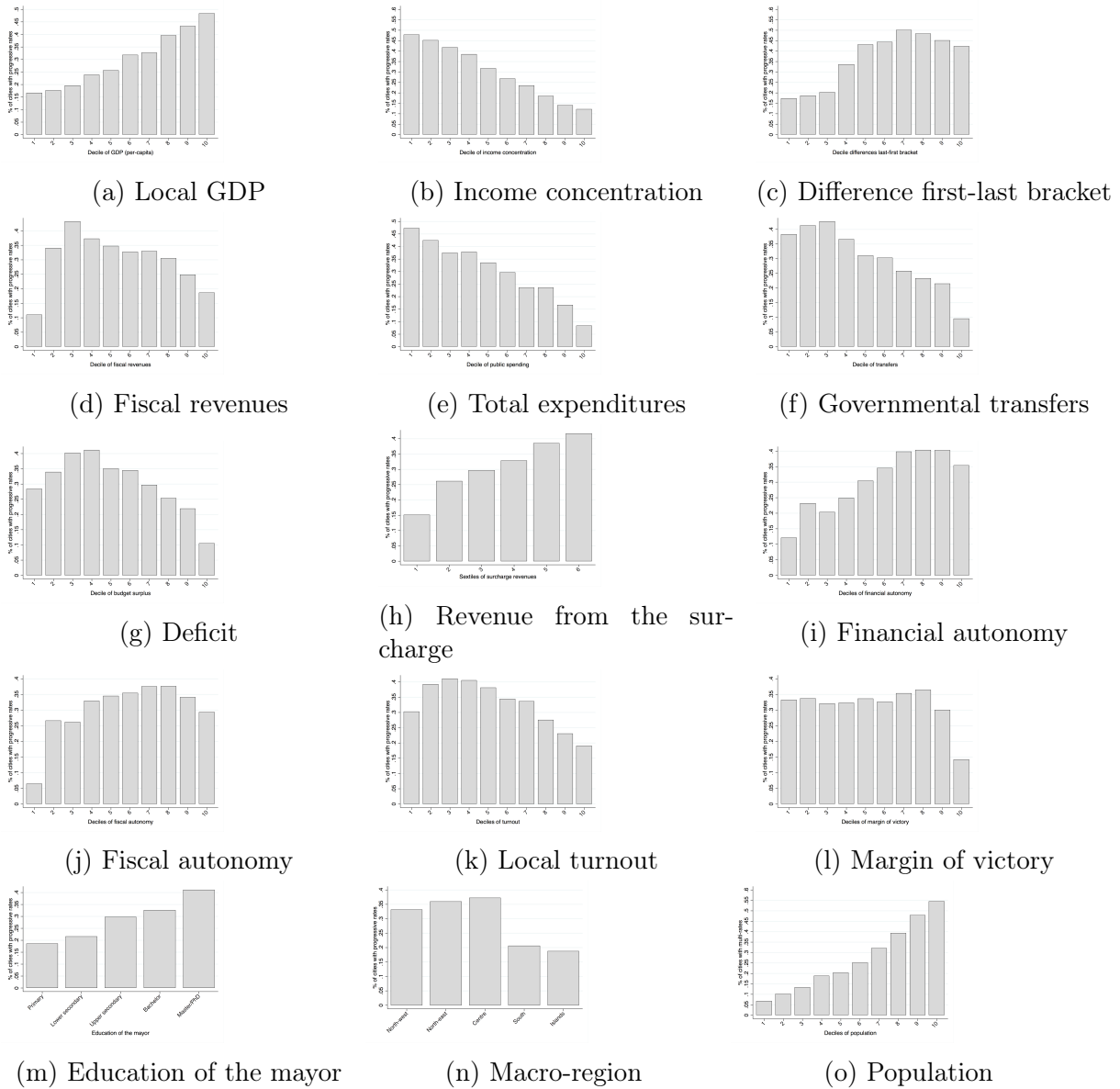
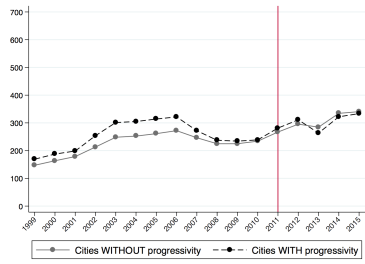
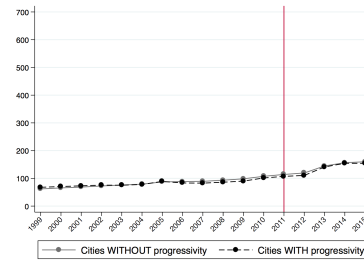


Figure 1.11: Characteristics of cities introducing progressivity



(a) Fees



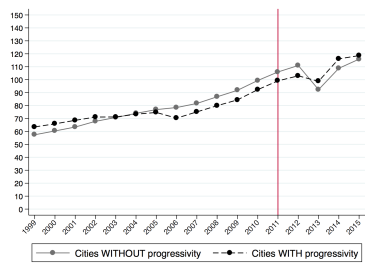
(b) Taxes



(c) Municipal surcharge



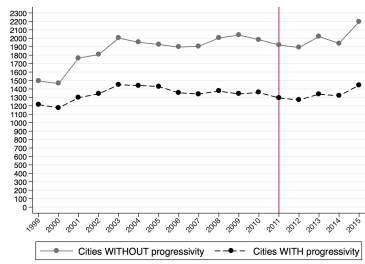
(d) Real estate tax



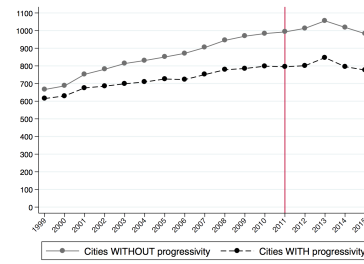
(e) Waste tax



(f) Governmental transfers



(g) Total expenditures



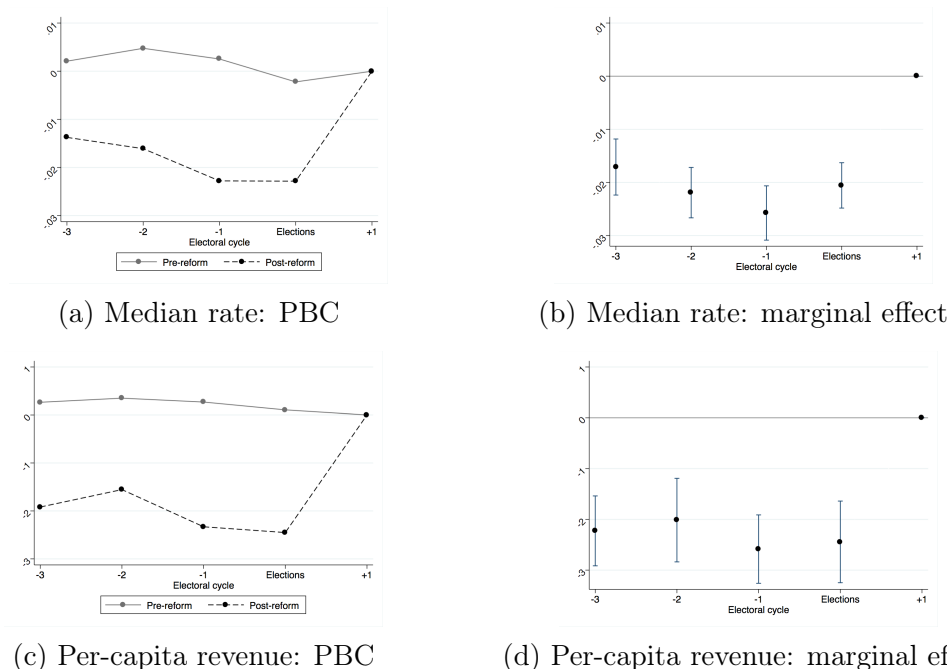
(h) Current expenditures



(i) Capital expenditures

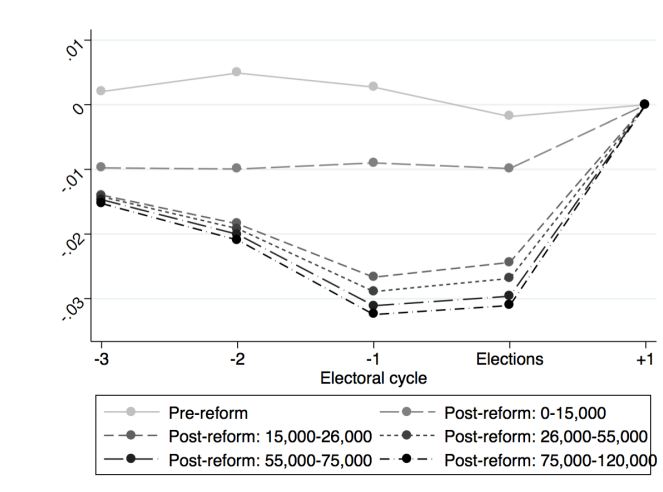
Figure 1.12: Evolution of balance sheet components

Figure 1.13: Fluctuations of the municipal surcharge along the political cycle



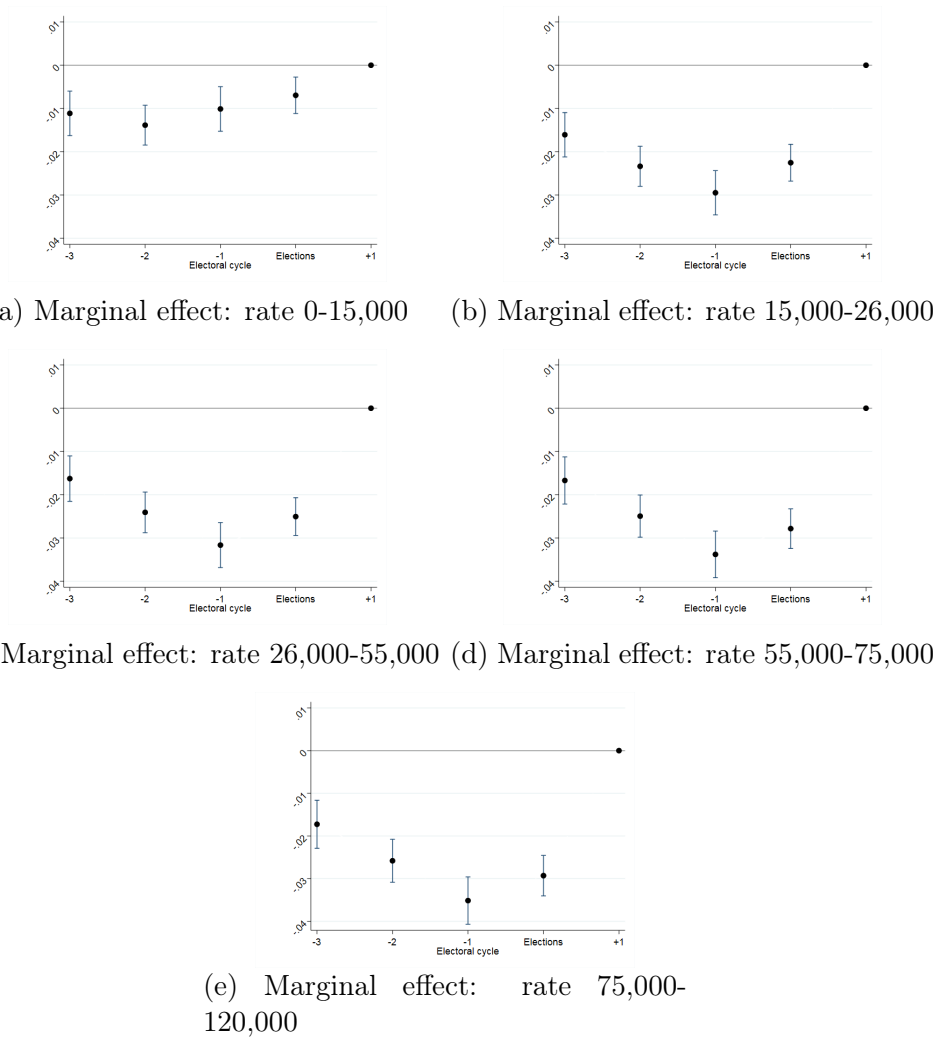
Notes: The figure plots coefficients of columns (2) and (4) of Table 1.6. Left graphs show simulated tax rates along the political cycle: "Pre-reform period" line describes the cycle before the reform, 1999-2011: coefficient $\hat{\beta}_1$ of regression model; "Post-reform period" focuses on period 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.

Figure 1.14: Fluctuations of the municipal surcharge along the political cycle: single rates



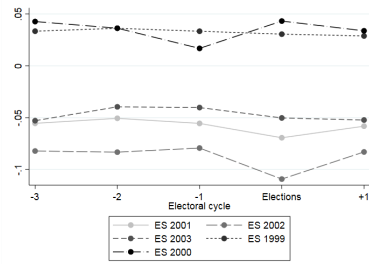
Notes: This figure shows the fluctuations of surcharge rates along the political cycle for single wage brackets in Euros (columns 1-6 of Table 1.7): pre-reform line shows the fluctuation of the (unique) rate before the reform, 1999-2011, with the plot of $\hat{\beta}_1$ coefficient of regression model. 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.

Figure 1.15: Marginal effects of the reform for each income bracket rates

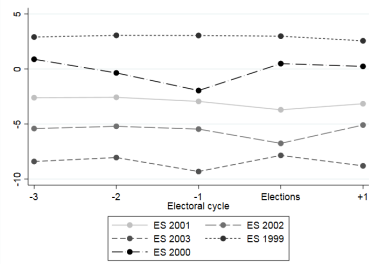


Notes: This figure shows the marginal effect of the reform for single wage brackets rates in Euros (columns 1-6 of Table 1.7).

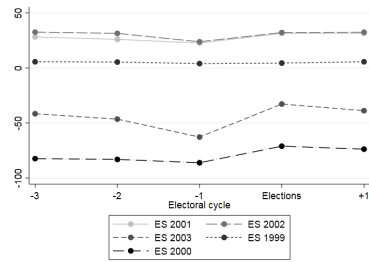
Figure 1.16: Testing parallel trend assumption: group-specific pre-reform political cycle.



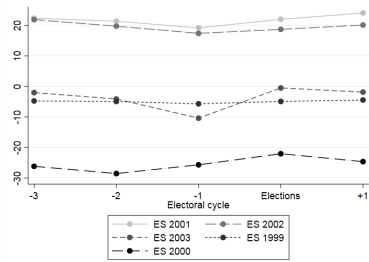
(a) PIT surcharge: median rate



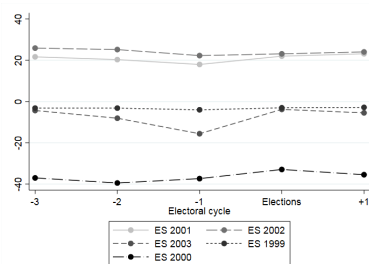
(b) PIT surcharge: revenues (per-capita)



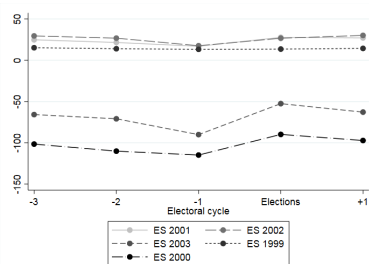
(c) Real estate tax: revenues (per-capita)



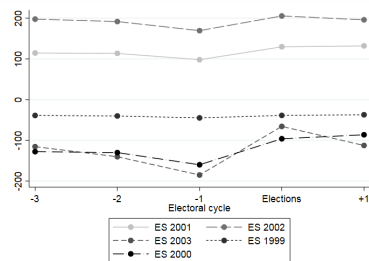
(d) Total revenues (per-capita)



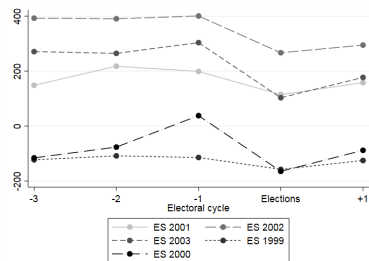
(e) Taxes (per-capita)



(f) Fees (per-capita)



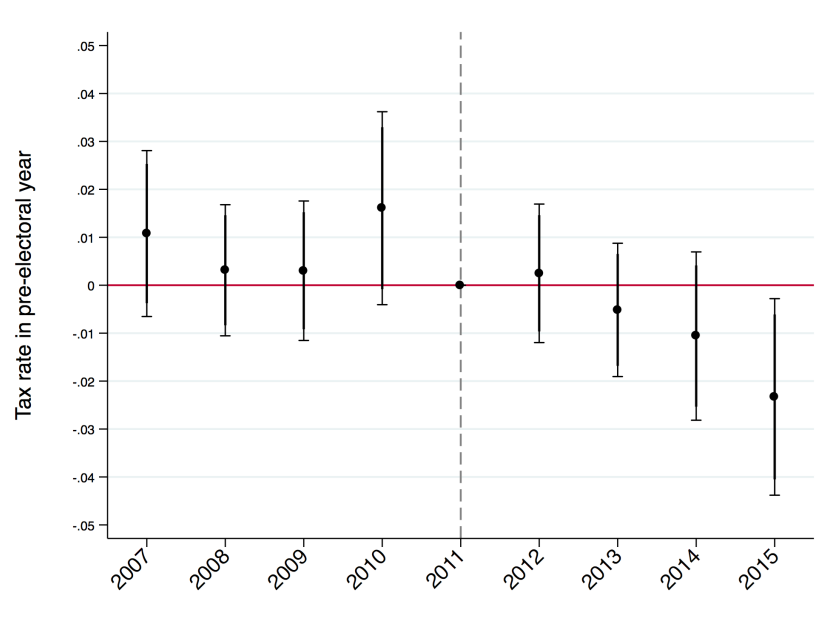
(g) Current expenditures (per-capita)



(h) Capital expenditures (per-capita)

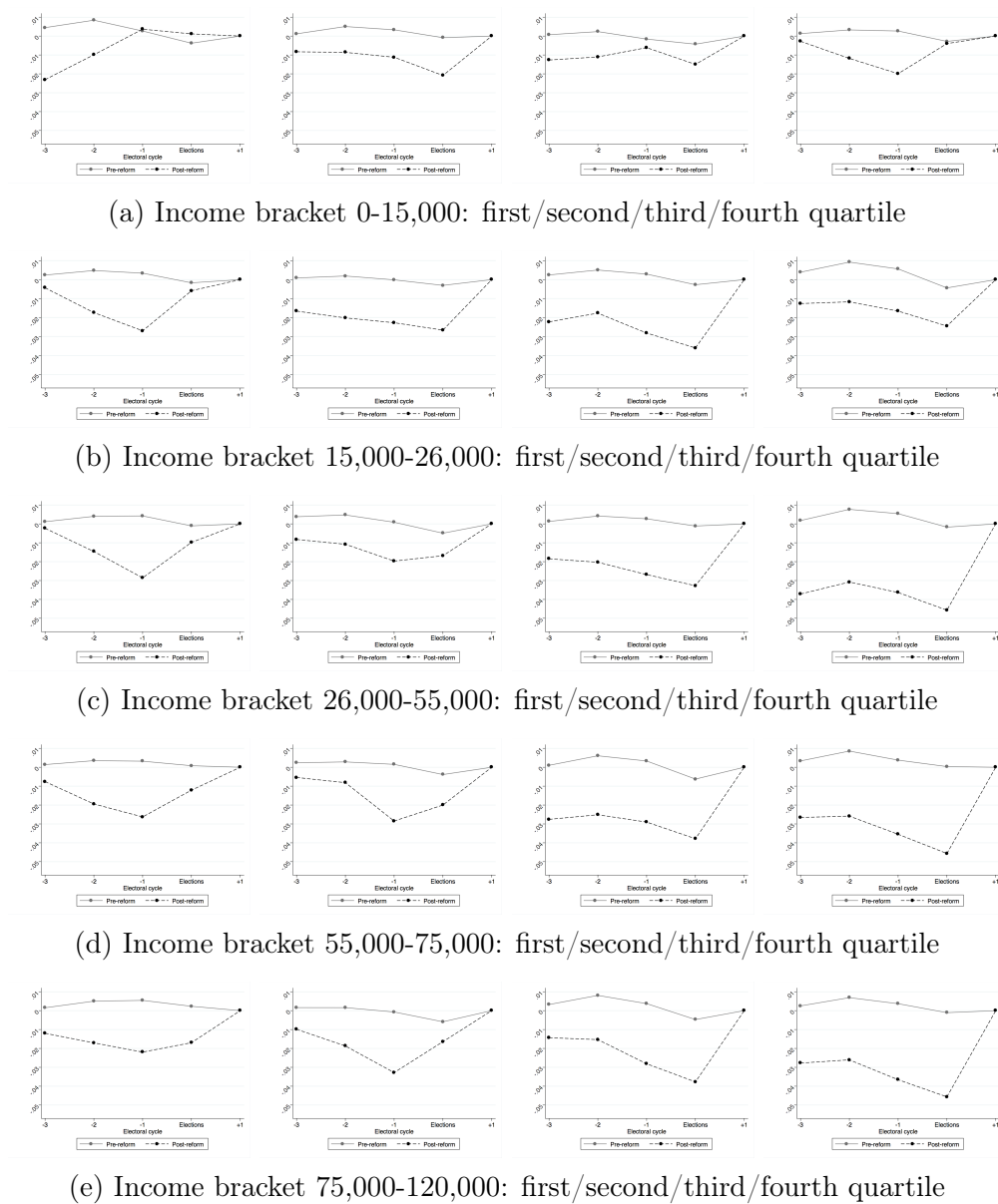
Notes: pre-reform period 1999-2011. Outcomes are cleaned for municipality and macro-region/year FE, and the complete set of political and economic controls. Municipalities are grouped according to the election year.

Figure 1.17: Pre-trends in terms of the median tax rate in the pre-election year.



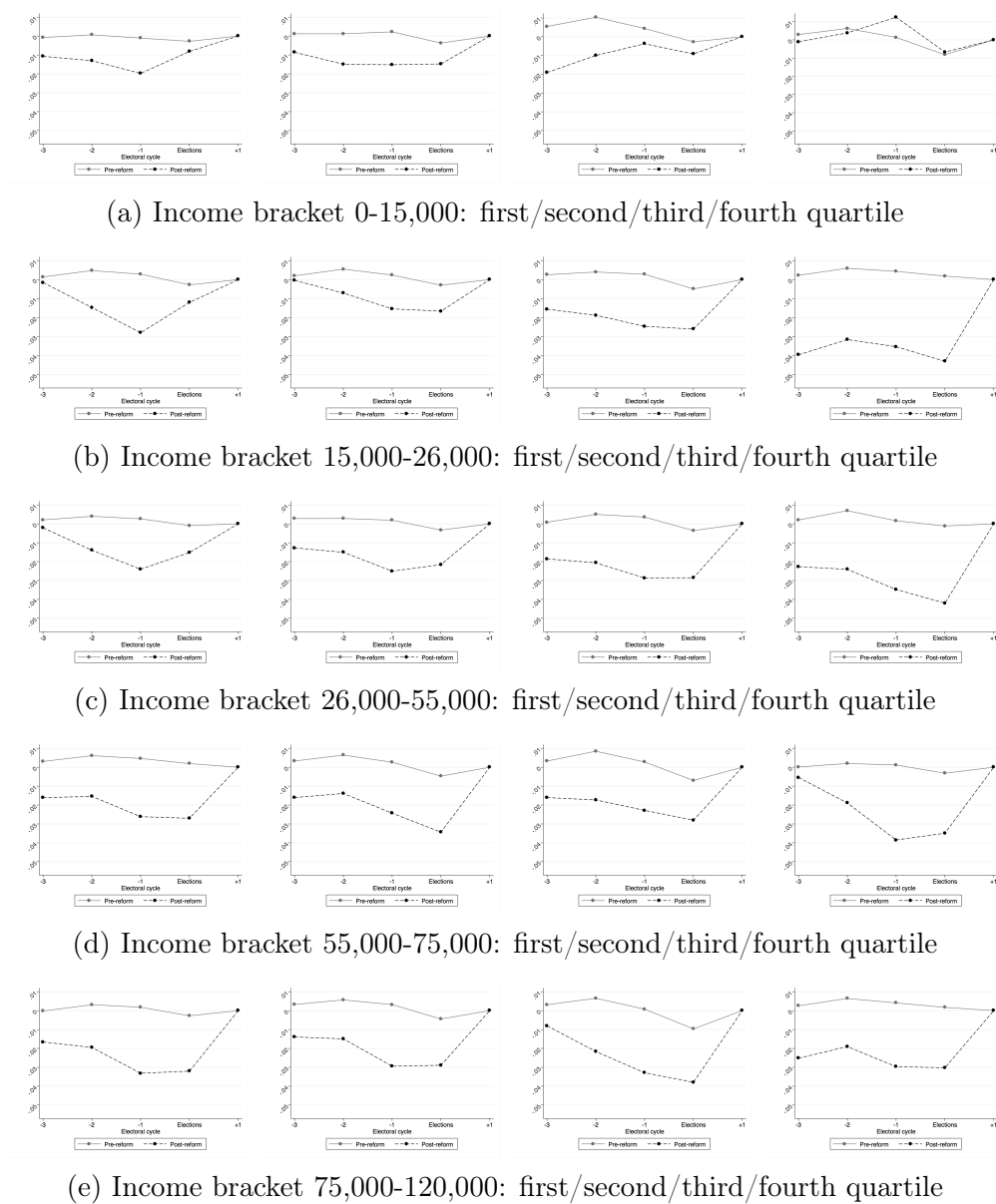
Notes: This graph shows the evolution of the median tax rate in the pre-electoral year over time. The year before the reform, the 2011, is set as zero and works as a benchmark. The set of fixed effects in the regression include municipal, group-year, macro region-year and region specific-electoral cycle 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.

Figure 1.18: Heterogeneous effects of the reform according to the fraction of population in each brackets (divided in quartiles)



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



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]

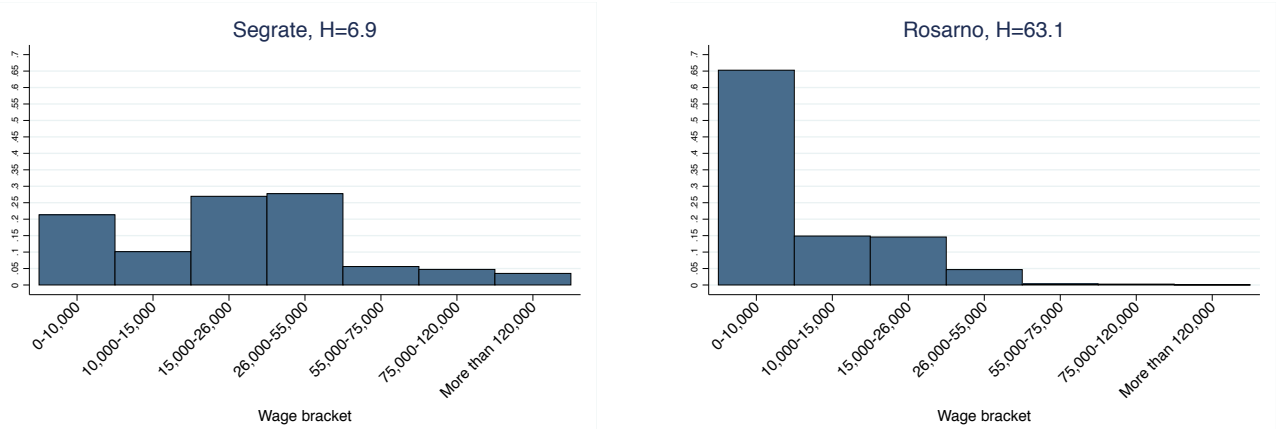
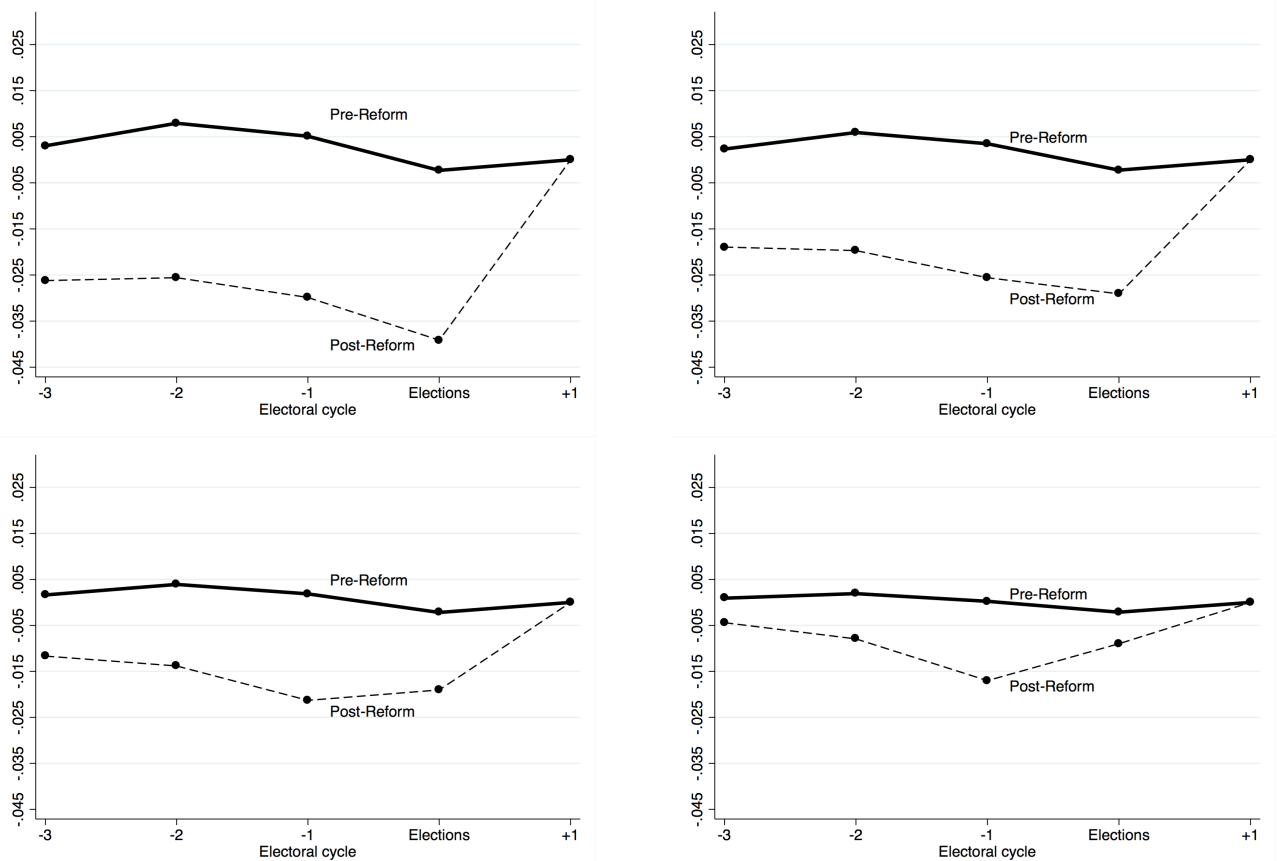


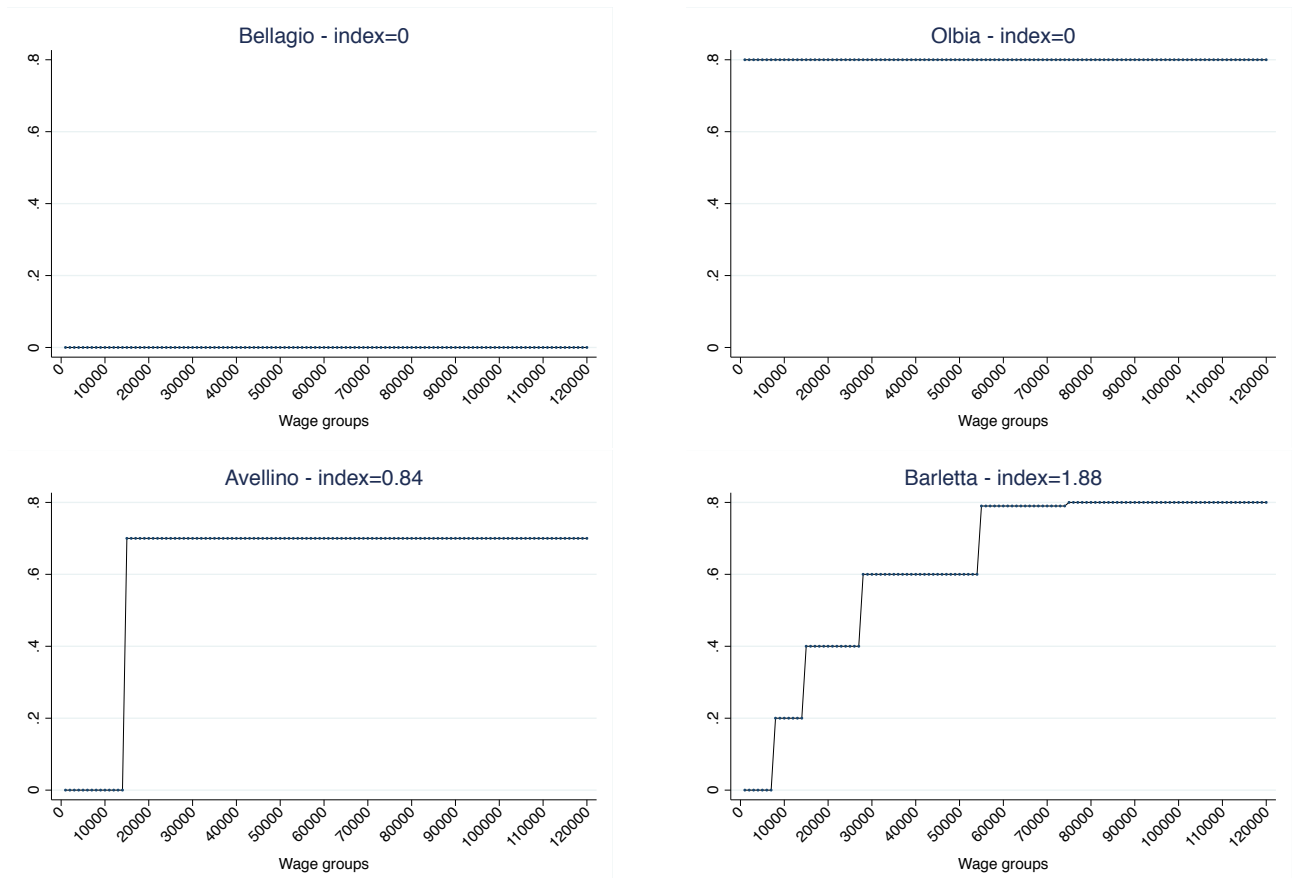
Figure 1.20: Left panels shows a municipality with uniform income distribution (**low concentration**), right panel shows a municipality with dispersed income distribution (**high concentration**).

Figure 1.21: Graphical evidence of Triple-Diff analysis - Median surcharge rate.



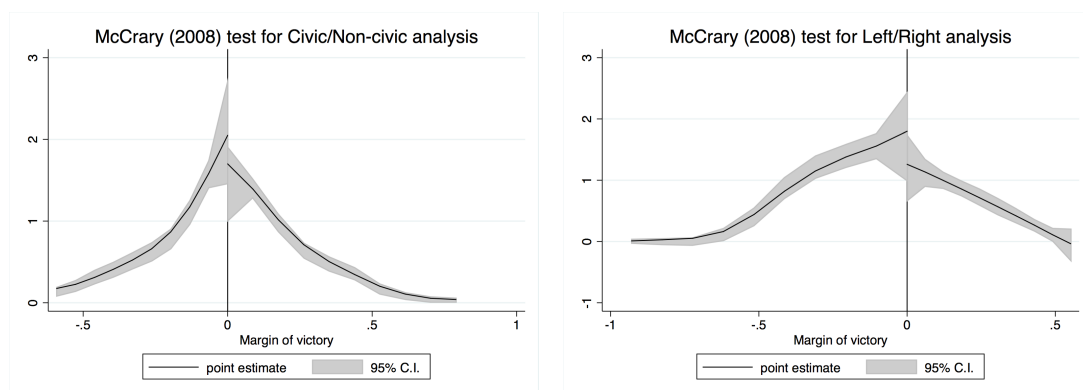
Notes: The graph plots coefficients of column (1) of Table 1.9. "Pre-Reform" line describes the cycle before the reform, 1999-2011, and it varies only with income concentration: coefficient $\hat{\beta}'_1 + \hat{\beta}'_5 \cdot H_{i,2011}$; "Post-Reform" line describes the cycle after the reform, 2012-2015, 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.

Figure 1.23: Index of progressivity - examples



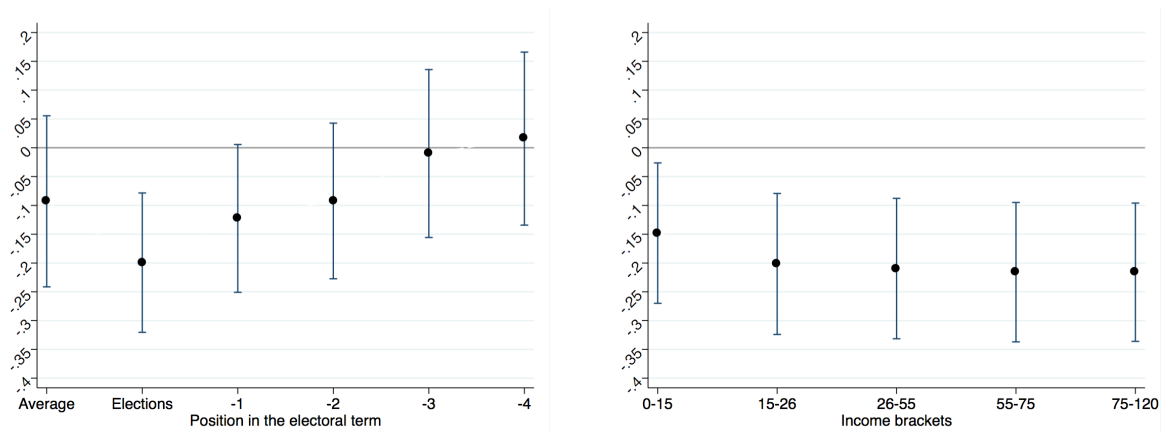
Notes: These graphs plot surcharge rate for every wage groups in 2015 for four Italian municipalities with different degrees of "progressivity" index.

Figure 1.24: 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 1.22: 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.

1.13 Tables

Table 1.1: 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
<i>N</i>	13,533

Table 1.2: 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
<i>N</i>	19,366

Table 1.3: 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
<i>N</i>	137,560		

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

Table 1.4: Municipal surcharge average rate for three fiscal types.

Fiscal regime	Av.rate	St.dev.
flat tax	.419 %	.189
flat tax+exemption 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 1.5: 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
<i>N</i>	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 1.6: 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 (0.000681)	0.00207 (0.000714)***	0.160 (0.0994)	0.264 (0.104)**
2 years before elections	0.00251 (0.000925)***	0.00475 (0.000960)***	0.189 (0.113)*	0.350 (0.116)***
1 year before elections	0.00607 (0.000940)***	0.00252 (0.000960)***	0.596 (0.114)***	0.269 (0.118)**
Election year	-0.00584 (0.000707)***	-0.00222 (0.000790)***	-0.149 (0.0977)	0.106 (0.104)
3 years before elections*Post-reform	-0.0183 (0.00275)***	-0.0158 (0.00276)***	-2.187 (0.358)***	-2.183 (0.362)***
2 years before elections*Post-reform	-0.0232 (0.00242)***	-0.0208 (0.00249)***	-2.178 (0.418)***	-1.905 (0.439)***
1 year before elections*Post-reform	-0.0358 (0.00263)***	-0.0253 (0.00271)***	-3.586 (0.346)***	-2.599 (0.356)***
Election year*Post-reform	-0.0165 (0.00215)***	-0.0206 (0.00224)***	-2.077 (0.410)***	-2.557 (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 taxpayers 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 1.7: Effect of the reform on surcharge manipulation - single tax rates

Dependent variable:	rate 0-15,000 (1)	rate 15,000-26,000 (2)	rate 26,000-55,000 (3)	rate 55,000-75,000 (4)	rate 75,000-120,000 (5)	rate more than 120,000 (6)
3 years before elections	0.00136 (0.000717)*	0.00208 (0.000679)***	0.00203 (0.000680)***	0.00200 (0.000682)***	0.00198 (0.000683)***	0.00198 (0.000683)***
2 years before elections	0.00390 (0.00101)***	0.00494 (0.000924)***	0.00490 (0.000923)***	0.00489 (0.000926)***	0.00489 (0.000928)***	0.00488 (0.000928)***
1 year before elections	0.00110 (0.00105)	0.00278 (0.000903)***	0.00272 (0.000903)***	0.00269 (0.000907)***	0.00268 (0.000910)***	0.00267 (0.000910)***
Election year	-0.00292 (0.000866)***	-0.00187 (0.000752)**	-0.00184 (0.000753)**	-0.00180 (0.000755)**	-0.00177 (0.000757)**	-0.00177 (0.000757)**
3 years before elections*Post-reform	-0.0111 (0.00262)***	-0.0161 (0.00262)***	-0.0163 (0.00268)***	-0.0167 (0.00278)***	-0.0173 (0.00287)***	-0.0173 (0.00287)***
2 years before elections*Post-reform	-0.0138 (0.00235)***	-0.0234 (0.00236)***	-0.0241 (0.00240)***	-0.0249 (0.00249)***	-0.0258 (0.00257)***	-0.0258 (0.00257)***
1 year before elections*Post-reform	-0.0101 (0.00263)***	-0.0295 (0.00261)***	-0.0316 (0.00265)***	-0.0338 (0.00275)***	-0.0351 (0.00283)***	-0.0351 (0.00283)***
Election year*Post-reform	-0.00695 (0.00215)***	-0.0225 (0.00217)***	-0.0250 (0.00223)***	-0.0278 (0.00233)***	-0.0293 (0.00242)***	-0.0293 (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 1.8: Characteristics of municipalities across income concentration (deciles)

Concentration index - deciles	Flexibility adoption	Flexibility mun.	Never set surcharge	Median surcharge rate	Weighed average surcharge rate
	(1)	(2)	(3)	(4)	(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
<i>N</i>	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 percentage points.

Table 1.9: Triple-Diff analysis with income concentration as a mediator

Dependent variable: Municipal surcharge	median rate	per-capita revenues
	(1)	(2)
Panel A: $C_{it} \cdot Post_t$		
3 years before elections*Post-reform	-0.0372 (0.00710)***	-6.518 (1.054)***
2 years before elections*Post-reform	-0.0414 (0.00573)***	-5.653 (1.328)***
1 year before elections*Post-reform	-0.0409 (0.00596)***	-6.627 (0.947)***
Election year*Post-reform	-0.0468 (0.00502)***	-6.641 (1.004)***
Panel B: $Post_t \cdot H_{i,2011}$		
Post-reform*Concentration index	-0.0275 (0.00269)***	-5.137 (0.342)***
Panel C: $C_{it} \cdot H_{i,2011}$		
3 years before elections*Concentration index	-0.000702 (0.000563)	0.0403 (0.0783)
2 years before elections*Concentration index	-0.00200 (0.000771)***	-0.218 (0.0962)**
1 year before elections*Concentration index	-0.00162 (0.000760)**	-0.204 (0.0944)**
Election year*Concentration index	0.0000605 (0.000564)	0.0949 (0.0882)
Panel D: $C_{it} \cdot Post_t \cdot H_{i,2011}$		
3 years before elections*Post-reform*Concentration index	0.00797 (0.00241)***	1.613 (0.315)***
2 years before elections*Post-reform*Concentration index	0.00788 (0.00208)***	1.435 (0.403)***
1 year before elections*Post-reform*Concentration index	0.00591 (0.00217)***	1.544 (0.293)***
Election year*Post-reform*Concentration index	0.00999 (0.00176)***	1.557 (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 the 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.10: 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 (1)	Per-capita (2)	Median (3)	Per-capita (4)	Median (5)	Per-capita (6)	Median (7)	Per-capita (7)
3 years before elections	0.00522 (0.00198)***	0.523 (0.211)**	0.00478 (0.00115)***	0.671 (0.178)***	0.00306 (0.000800)***	0.418 (0.128)***	0.00572 (0.00244)**	0.790 (0.382)**
2 years before elections	0.00193 (0.00238)	-0.0940 (0.243)	0.00543 (0.00146)***	0.838 (0.198)***	0.00619 (0.00108)***	0.451 (0.140)***	0.0164 (0.00289)***	1.953 (0.444)***
1 year before elections	0.000754 (0.00284)	0.0216 (0.274)	0.000122 (0.00135)	0.582 (0.195)***	0.00378 (0.00104)***	0.242 (0.136)*	0.0135 (0.00271)***	1.943 (0.463)***
Election year	-0.00364 (0.00202)*	-0.0919 (0.219)	-0.00923 (0.00146)***	-0.0926 (0.194)	-0.00213 (0.000956)**	0.171 (0.125)	-0.0125 (0.00228)***	-0.234 (0.389)
3 years before elections*Post-reform	-0.0175 (0.00316)***	-2.293 (0.393)***	-0.00876 (0.00277)***	-1.426 (0.373)***	-0.0228 (0.00372)***	-3.231 (0.485)***	-0.0209 (0.00292)***	-2.955 (0.380)***
2 years before elections*Post-reform	-0.0167 (0.00306)***	-1.277 (0.450)***	-0.0145 (0.00271)***	-1.671 (0.469)***	-0.0263 (0.00300)***	-2.519 (0.542)***	-0.0242 (0.00256)***	-2.315 (0.442)***
1 year before elections*Post-reform	-0.0221 (0.00357)***	-2.195 (0.417)***	-0.0157 (0.00275)***	-2.127 (0.388)***	-0.0314 (0.00309)***	-3.190 (0.426)***	-0.0290 (0.00271)***	-3.077 (0.361)***
Election year*Post-reform	-0.0172 (0.00296)***	-2.156 (0.489)***	-0.00477 (0.00248)*	-1.431 (0.451)***	-0.0297 (0.00293)***	-3.268 (0.665)***	-0.0252 (0.00236)***	-3.102 (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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.11: 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 (0.566)**	0.000640 (0.000311)**	0.00149 (0.000464)***	0.347 (0.267)	4.477 (0.534)***	1.132 (2.232)	26.37 (12.36)**
2 years before elections	0.856 (0.693)	-0.000217 (0.000397)	0.00111 (0.000582)*	-0.118 (0.271)	2.187 (0.587)***	5.021 (2.137)**	57.87 (11.83)***
1 year before elections	-0.0533 (0.565)	-0.00170 (0.000390)***	-0.000149 (0.000477)	-0.927 (0.279)***	1.732 (0.474)***	3.506 (1.768)**	72.15 (12.65)***
Election year	-0.857 (0.502)*	-0.00385 (0.000328)***	-0.00150 (0.000461)***	-0.877 (0.260)***	0.926 (0.464)**	-1.204 (1.649)	-66.25 (11.73)***
3 years before elections*Post-reform	6.693 (2.351)***	0.00650 (0.00148)***	0.00346 (0.00129)***	-2.231 (1.598)	-14.06 (1.960)***	9.610 (6.489)	-28.59 (33.24)
2 years before elections*Post-reform	9.094 (2.611)***	0.000400 (0.00140)	-0.000215 (0.00126)	-0.926 (1.426)	-0.606 (1.600)	4.044 (5.877)	-7.429 (25.47)
1 year before elections*Post-reform	8.341 (2.979)***	-0.00426 (0.00145)***	-0.0000820 (0.00129)	-2.960 (1.486)**	1.057 (1.818)	9.134 (6.257)	-64.39 (23.22)***
Election year*Post-reform	9.102 (2.312)***	0.000685 (0.00118)	-0.000868 (0.00105)	-1.588 (1.341)	-1.996 (1.629)	-0.402 (4.712)	27.42 (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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.12: 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 (0.000721)*	-0.0929 (0.0758)	-0.199 (0.0618)***	-0.123 (0.0654)*	-0.0923 (0.0689)	-0.0100 (0.0743)	0.0159 (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 (0.0622)**	-0.202 (0.0625)***	-0.210 (0.0622)***	-0.216 (0.0618)***	-0.216 (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, age 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.13: Test for continuity of municipalities and mayors characteristics

Panel A: Civic/Non-civic analysis					
	Population (1)	Share of immigrants (2)	Number of candidates (3)	Number of taxpayers (4)	Aggregate income declared (5)
RD_Estimate	2810.1 (2647.6)	0.00345 (0.00536)	-0.123 (0.258)	1736.6 (1487.2)	44290129.8 (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 (1)	Age mayor (2)	Gender mayor (3)	Mayor term limited (4)	
RD_Estimate	-0.0856 (0.0999)	0.0454 (1.582)	0.00962 (0.0549)	-0.0906 (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) (1)	Capital expenditure (PC) (2)	Total fiscal revenues (PC) (3)		
RD_Estimate	-81.13 (49.27)*	-7.276 (92.17)	-4.586 (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 (1)	Share of immigrants (2)	Number of candidates (3)	Number of taxpayers (4)	Aggregate income declared (5)
RD_Estimate	19930.6 (15546.7)	-0.00366 (0.00854)	0.0376 (0.521)	5034.3 (6749.4)	52840792.6 (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 (1)	Age mayor (2)	Gender mayor (3)	Mayor term limited (4)	
RD_Estimate	0.0653 (0.169)	3.200 (3.125)	0.0755 (0.103)	-0.0838 (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) (1)	Capital expenditure (PC) (2)	Total fiscal revenues (PC) (3)		
RD_Estimate	-17.24 (67.57)	-26.01 (85.04)	-93.20 (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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

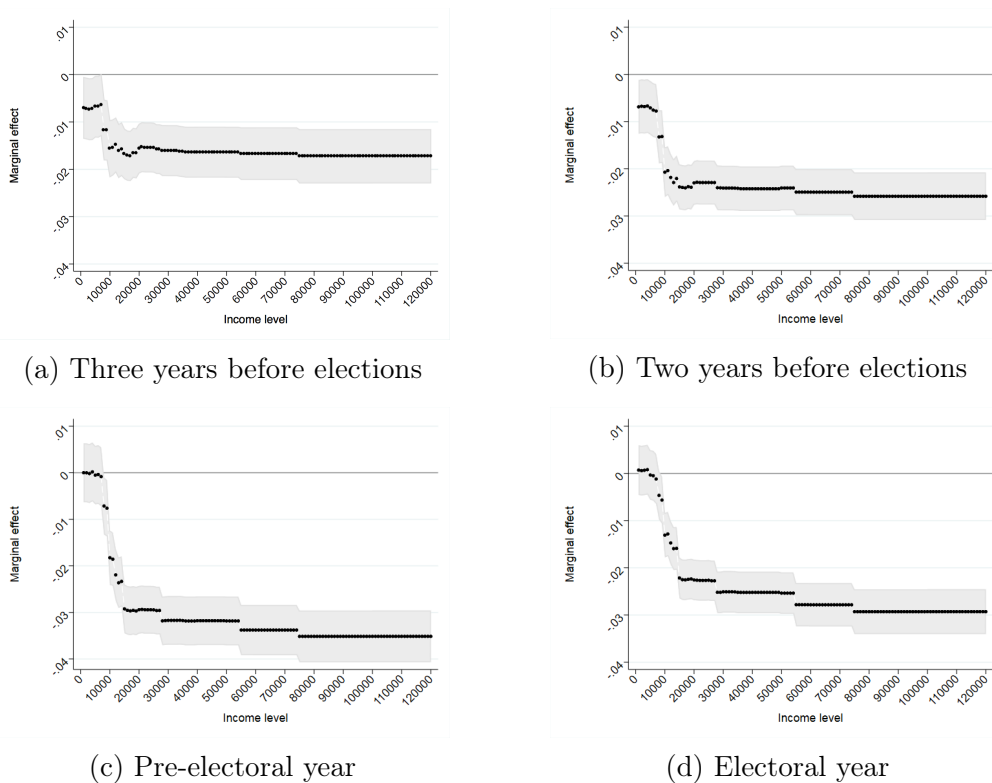
Table 1.14: Use/degree of flexibility - Close election RDD.

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 (0.0722)*	0.200 (0.0982)**	0.0964 (0.0545)*	-0.247 (0.146)*	-0.138 (0.162)	-0.250 (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	Civic/Political mayor			Left/Right mayor		
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.152 (0.0826)*	0.220 (0.117)*	0.116 (0.0602)*	-0.250 (0.178)	-0.245 (0.206)	-0.246 (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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

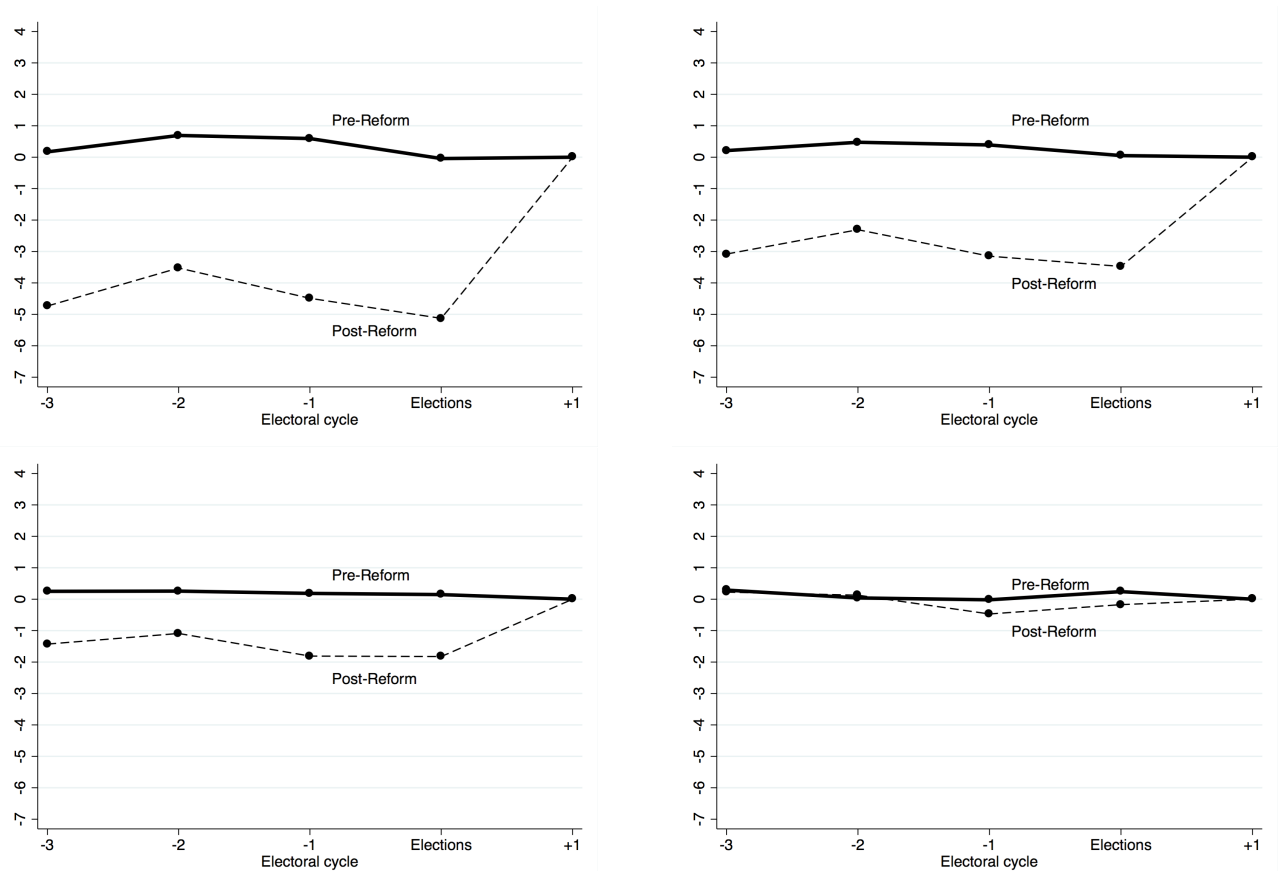
1.14 Appendix: additional outputs

Figure 1.25: Marginal effects of the reform for each thousand of income



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



Notes: The graph plots coefficients of column (2) of Table 1.9. "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.

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Chapter 2

Exposure to Corruption and Political Participation: Evidence from Italian Municipalities

2.1 Introduction

Corruption is a broad phenomenon with sizeable and complex effects on the political sphere. On the one hand, corruption has relevant impacts on the context in which the scandal occurs: in corrupted localities incumbents face lower re-election probability (Larreguy et al. 2014, Costas-Pérez, et al. 2012, Ferraz, Finan 2008), subsequent administrators tend to behave more honestly (Avis et al. 2017) and local audits play a crucial sanctioning effect (Bobonis et al. 2016). On the other hand, there is evidence that corruption has broader effects on society and it negatively affects voters' confidence and trust in public institutions (Clausen, et al. 2011), in governments' capability (Caillier 2010), and in political systems and politicians (Morris, Klesner 2010). Finally, these effects are crucially mediated by the media (Muço 2017, Rizzica, Tonello 2015, Fiorino et al. 2012) and this mediation is often characterized by reporting bias (Puglisi, Snyder 2010, Di Tella, Franceschelli 2009). Nevertheless, despite the broad debate on political consequences of corruption, the impact of this phenomenon on political participation, and how this relation is mediated by the press, received limited attention. This is a vital aspect to explore in order to better understand the impacts of this phenomenon.

This paper focuses on this specific issue and studies the effects of local corruption on political participation of citizens and politicians. We concentrate on an effect which is mediated by the press, generating an original daily and local measure of media coverage of corruption, through the screening of newspaper articles of the main national press agency. Moreover, we study all aspects of political participation. On the one hand, we consider the effect of corruption on participation of citizens: on *whether* people vote -voter turnout- and on *how* people vote -selection of elected politicians (characteristics of mayors and councillors) and political background of elected mayors. On the other hand, we look at the impact on participation of local politicians (*who* runs), focusing on endogenous political supply: the number of candidates running for mayor, the number of lists of candidates and the political background of candidates (whether they are tenured politicians or freshmen, without any political experience). Finally, the focus of this study is local: notably, we take into account political scandals involving administrators in local capitals, where the seats of local governments are located, and we study the effect on local elections in all other municipalities.

The paper focuses on Italy. This country constitutes an important instance to be studied

in terms of political corruption for two reasons. First, it represents an important outlier in the international panorama, as it is considered one of the most corrupted nation in Europe. There is international evidence confirming this: Transparency International ranks Italy in position 61 out of 168 in 2015 ranking of corruption perception¹. Domestically, the Italian accounting court, *Corte dei Conti*, classifies corruption as a widespread phenomenon with deleterious and durable effects on economic growth and, in an estimate of 2012, it evaluated *cost of corruption* in 60 billion Euros. Second, corruption played, and currently plays, a crucial role in the Italian social and political life. For instance, the large corruption scandal that took place at the beginning of the 90s, *Mani pulite*, has deeply modified Italian politics and it has significantly contributed to the transition to the so-called *Second republic* (Vannucci 2009). Moreover, corruption is a fundamental component of the political discourse of new populist forces: Caiani, Della Porta (2012) study electoral speeches of extreme right parties in Italy and they underline how corruption plays an essential role, and Biorcio, Natale (2013) document the important role of this element in the recent rise of the Italian *Five Star Movement*, a new populist movement, headed by the former comedian Beppe Grillo.

In order to measure political corruption at the local level, we generate an index by screening newspaper articles of the main Italian press agency, ANSA, which see local politicians involved in corruption cases. We identify an article as dealing with local scandals if a) it talks about corruption (we make use of keywords related to corruption, *e.g.* "bribe", to screen articles in an automatic web search) and b) it mentions the surname of a local politician in charge in the period the article was issued and in the place the article was geo-localized. Moreover, in order to limit endogeneity issues, due to the fact that local newspaper articles may be endogenous with respect to the political background, we limit the data collection to scandals that took place in local capitals, *i.e.* province and region capitals. And we study the effects on the group of municipalities that are not local capitals. With this local/non-local approach, each municipality is treated with corruption recorded in his local capitals and this allows us to interpret the treatment as pure exposure. We obtain a local and high-frequency (daily) measure of corruption for a long time span (1999-2014): we identify and geo-localize 14,614 articles about local corruption, pertaining politicians of different administrative levels and dealing with all phases of judiciary process, from the beginning of investigation to the sentence.

In order to capture the causal effect of corruption exposure on political participation, we rely on an event-study design: we take advantage of the large geographical and temporal variation of scandals as well as the staggered timing of Italian municipal elections. The essence of this identification strategy lies in the comparison of the impact of scandals taking place before elections with those occurred after, with the control group represented by cities that were not exposed to corruption. The key identifying assumption, then, is that the timing of scandals is plausibly exogenous with respect to elections and that it is not correlated with electoral dates. This assumption is reasonable in this setting, given the non-local nature of the treatment. This makes implausible to believe that the treatment may be affected by local unobservables characteristics, and that timing of capital scandals may be tailored on local election dates. Furthermore, we test whether there is manipulation in the number of scandals released by the press around the cutoff represented by election dates: in particular, we conduct a McCrary test checking whether the treatment is discontinuous around that threshold and we can assert that there is no evidence of discontinuity. This disproves the presence of manipulation in corruption news timing and further supports the identifying assumption.

Three main results emerge from the analysis. First, corruption exposure significantly reduces voter turnout in municipal elections: one standard deviation increase in the treatment lowers,

¹The index is a self-reported measure of perception of public corruption in a certain country (<http://www.transparency.org>).

on average, voter turnout by 0.36% after one year and by 0.26% after two years, while the effect vanishes for older scandals. Moreover, the effect turns out to be stronger for municipalities located in the centre-north of Italy, marked by high past turnout, low victory margin and with high levels of newspaper circulation and share of immigrants. Further, ideological affiliation of involved politicians, combined with local preferences, results to be an important mediator. In particular, cities with a left-wing mayor tend to react more, but not only, to scandals involving left-wing politicians and the opposite happens for cities with a right-wing administration. This result can be read in the light of the *retrospective voting model* (Persson, Tabellini 2000) where a piece of news about political corruption is an informative signal on the quality of the entire political class. Voters may update their beliefs on political class quality and this new piece of information could reduce their “confidence in the responsiveness of electoral institutions and public officials” (Ansolabehere et al. 1994). This may induce voters to think that voting will not be beneficial for them, and they could be more likely to abstain in following elections (Chong et al. 2015). In terms of magnitude of the results, it is reasonable to think that the conservative approach, adopted in the construction of the index, leads to an attenuation bias: since ANSA works as a source of news for the press, it is possible that “real” newspapers devote more space to corruption stories and the true effect is likely to be larger than the one found here. As a second result, we find mixed evidence of politicians selection: on the one hand, demographic characteristics of elected mayors and councillors, such as age, education and gender, do not modify substantially after a scandal. On the other hand, there is robust evidence that the elected mayor in treated cities is more likely to be a tenured politician, *i.e.* a candidate who already ran in the past or the incumbent, instead of a freshmen coming from the civic society. This represents an important modification in the local political background, as it leads the old political class to consolidate his control at the local level.

Third, political supply narrows because the effect of exposure to corruption. The number of candidates running for mayor and lists of candidates decrease, as one standard deviation increase in corruption index lowers the number of candidates by 0.05 units and the number of lists by 0.075 lists after two years. The effect reaches its maximum after two years from the scandal and then it reduces. The timing of the effect is coherent with the fact that these choices are taken in advance, and are unlikely to be affected by shocks close to the electoral date. Moreover, we document that the composition of candidates pool modifies substantially because of corruption: indeed, the representation of tenured politicians raises consistently as we observe an increase in the proportion of old runners and in the probability that the incumbent reruns. This implies that freshmen candidates are the ones who leave the competition, suggesting that corruption makes local political career less attractive for them. Furthermore, this modification in the local supply of candidates is related and contributes to the previous result that tenured politicians are more likely to be elected. We could read this second result in the light of the model developed by Caselli, Morelli (2004) as the presence of corruption could modify politician image affecting costs of running for freshmen. This would work as a negative externality, raising entry costs for freshmen without modifying incentives for “old politicians” and this may generate, in equilibrium, a lower number of new-politicians running for mayor.

The main results of the paper suggest that exposure to corruption generates negative effects on political participation in neighbouring cities: on the one hand, citizens are more likely to abstain, on the other hand, fewer lists are presented and fewer candidates run for mayor. This outcome is driven by a consistent reduction in the number of freshmen candidates. This makes the election of tenured politicians more probable, without any renewal in the political class. A general discouragement seems to emerge, leading to a worsening in the quality of the local political scene.

This contribution is related to several papers in Political Economy. First, it is linked with

the literature studying effects of corruption on voter turnout. On the one hand, there is a large set of papers in Political Science on this issue that do not agree upon the direction of the effect: most of these papers find that corruption negatively affects turnout (Anderson, Tverdova 2003; Kostandinova 2003, Bauhr, Grimes 2013), a small group of studies find a positive relation (Stockemer, Calca 2013) and, finally, few papers do not find any relation (Rothstein, Solevid 2013, Stockemer 2013). These studies consider a very general definition of corruption (from political to public) and they mostly focus on cross-country comparison. On the other hand, there is a small group of papers in Economics exploring this relation. Chong et al. (2015) focus on local incumbents in Mexico and find that random dissemination of information on politicians' malfeasance affects electoral support and decreases voter turnout; moreover, Costas-Perez (2013) concentrates on Spain and relies on a dataset of documented cases of corruption to study the effects on political participation. Our study contributes to this literature as it evaluates the impact of corruption taking place in the political background and in local capitals on voter turnout, observing the distinction between short and long-run effects. Moreover, we make use of an original daily and local measure of corruption exposure, directly built from newspaper articles.

Second, this contribution is related to the large literature on determinants of politicians selection. Several factors has been inspected: remuneration/compensation (Gagliarducci, Nannicini 2013), the political competition (Galasso, Nannicini 2011), the electoral system (Mattozzi, Merlo 2008), the presence of gender quotas (De Paola et al. 2010, Baltrunaite et al. 2014) and the activity of organized crime (Daniele, Geys 2015). Moreover, a set of recent studies focus on the endogenous response to corruption of parties or politicians: such as studies on party selection of candidates (Cavalcanti et al. 2017) or loyalty of local politicians to the involved party (Daniele et al. 2017). Our contribution is among the first to study the effect of corruption exposure on selection of politicians and on the endogenous supply of candidates, focusing also on the political background of the pool of candidates.

Third, this study is related to the set of papers on incentives and determinants of politicians choice to run for a seat, in terms of monetary incentives (Mattozzi, Merlo 2008) or reputation gains (Caselli, Morelli 2004). Our paper contributes to this literature as it provides an empirical test on the impact of corruption on entry decisions of local candidates. Finally, this paper pertains to the large Media Economics literature, in particular the studies on the effect of media on political participation (Stromberg 2004, Gentzkow 2006, Della Vigna, Kaplan 2007, Enikolopov et al. 2010, Snyder, Stromberg 2010, Drago et al. 2014, Adena et al. 2015), the ones which inspect media bias on news about corruption (Di Tella, Franceschelli 2009, Puglisi, Snyder 2010, Fiorino, et al. 2012, Rizzica, Tonello 2015) and studies which focus on the importance of the press in mediating the impact of corruption (Muço 2017).

The paper is organized as follows. Section 2 describes data on corruption and on political participation. Section 3 presents the identification strategy and the validity check. Section 4 shows the main results on political turnout, selection of politicians and political supply. Section 5 discusses several robustness checks and Section 6 concludes.

2.2 Data

2.2.1 Political Corruption

Articles about local corruption

The first contribution of this paper is providing a measure of exposure to local corruption in the context of Italian municipalities. We rely on the main Italian press agency ANSA²; this is an important and prestigious institution in the Italian press market and it is not likely to be prone to ideological and geographical bias (as it is probably the case for local/national newspapers).

Local corruption in Italy can take place at different levels of local administration: Italian territory is divided into 20 regions, 103 provinces and 8101 municipalities (in 2004). Each administrative level is characterized by specific competences and many services are administered locally: for instance, healthcare is managed at the regional level, and waste management at the municipal one. Moreover, starting from the last constitutional reform in 2001, local administrations face a certain degree of revenues/expenditures autonomy, which has been considered as a possible determinant for the increasing trend in political corruption. Figure 2.1 shows Italian administrative divisions with regions, delimited by black boundaries and municipalities, delimited by light grey lines. Red dots are local capitals: Italy has 103 province capitals, 20 of them are also regional capitals hosting regional government institutions, such as Milan for the region of Lombardy or Rome for the region of Lazio. We collect corruption scandals involving local politicians for the time span 1999-2014: in order to limit endogeneity issues due to the fact that local newspaper articles may be affected by social and political background, we limit the data collection to scandals that took place in local capitals, and that target politicians at any level of the local administration. The main goal is to construct a non-local treatment and to study effects locally, *i.e.* on municipalities that are exposed to these scandals. Each municipality is then treated with the level of corruption uncovered in its province and region capital; furthermore, we exclude from the sample of cities local capital themselves. With this local/non-local approach we are able to limit direct effects of corruption and we can safely interpret the treatment as exposure to corruption. Moreover, the coverage for news that target small cities would be less accurate and precise, and this further supports our focus on local capitals.

[Figure 2.1 here]

We construct the index following an automatic keyword-based procedure. First, we screen titles and first paragraphs of ANSA articles (using the web portal Factiva) in order to identify articles dealing with political corruption; notably, we collect articles based on two criteria: 1) the presence of at least one keyword related to political corruption, *e.g.* *abuso d'ufficio* (malfeasance in office), 2) the presence of the surname of a local politician in charge in the place where the article was geo-localized, in the period when the article was written³. Figure 2.2 shows one instance: the article contains one corruption term, *indagato* (under investigation), as well as the

²ANSA, *Agenzia Nazionale Stampa Associata*, is the first press agency in Italy and the fifth in the world, it has been established in 1945 and it has 22 offices in Italy and 81 in other countries.

³To be more precise on this first step:

- First, we screen first paragraphs of ANSA articles, making use of keywords related to corruption.
- Second, we geo-localize selected articles based on places mentioned in the article (we exploit the fact that in the standard structure of articles the first word is the name of the place where the piece of news comes from); we use the province as unit of our analysis.
- Third, we further screen selected articles identifying names of local politician. In particular, we search within article text for names of local politicians (*i.e.* all politicians in charge in regions,

surname of a local politician, *Boni (Davide)*, that was in charge in the place (Milan) the article refers to, in the period (march 2012) when the article was written. Moreover, we perform a random check to validate this automatic procedure and we generate monthly articles counts by province. This procedure guarantees that selected articles deal with corruption involving local politicians in charge when the article was issued and in the mentioned locality: this is a very conservative approach which successfully limits the incidence of false positive, but one drawback is that it discards many false negatives, as we are not able to record, for instance, scandals uncovered after that the politician leaves the office. Nevertheless, this is not an issue for the identification strategy, rather this is likely to have a downward effect on estimates. Finally, we generate a second, more stringent, version of the index where we impose a third criterion in order to identify corruption articles, namely the mention of political office title (*e.g.* mayor, alderman, ...) in the text of the article: this additional requirement further improves precision of the method at the price of reducing the sample losing a number of true corruption stories.

[Figure 2.2 here]

The main procedure allows the identification and geo-localization of 14,614 articles about political corruption in local capitals. The 55.2% of these articles involve politicians in the regional administrations, the 23.5% in the province and the 38.6% in the municipal administration; and it is frequent that one scandal involves politicians from different administrative levels. In terms of content, most of these articles deal with corruption or malfeasance in office involving councillors, members of local governments or mayors. The coverage is on all phases of judiciary procedure: from the beginning of the investigation to the sentence and more common misconducts include bribery, extortion, embezzlement, misuse of power, collusive tendering, and infractions in refunds usage. Table 2.1 reports some figures about articles content. First, it emerges that most of the scandals, 74.6%, are discovered in regional capitals, compared to province capitals: these cities are important local centres of power hosting all regional administrative organs such as regional assembly and the local government, moreover it is possible that these scandals are larger, more salient and more likely to be reported in the press. As a second result the south of the country hosts more than half of detected scandals, 53.5%, compared to the 27.2% in the north and the 19.3% in the centre of Italy. This evidence is coherent with the classical dichotomy between centre-north of the country and southern regions, that is characterized by low levels of economic development, poor social capital (Banfield 1958, Putnam 2001) and by the interference of organized crime in public and political affairs. Furthermore, second part of Table 2.1 summarizes characteristics of politicians that are involved in scandals, even if we cannot say anything about their specific involvement in the corruption case. The table focuses on three administrative levels separately: average age of involved administrators and fraction of women are lower than the average values, while average education is slightly higher. Moreover, if we focus on the specific political office of these administrators, it emerges that presidents, mayors and councillors are the ones more represented in this list: presidents and mayors are the heads, respectively, of regional and municipal administrations while councillors are members of local assembly and they represent the majority of local politicians. Nevertheless aldermen are frequently quoted in corruption articles for province administrations.

provinces and municipalities between 1999-2014; this information comes from *Anagrafe degli Amministratori Locali e Regionali*-Italian Ministry of Internal Affairs) and we identify an article whether it mentions one local politician, in charge in the place where the article was geo-localized, in the period when the article was written.

Corruption index

After the collection of articles about corruption in local capitals, we generate the measure to be used in the analysis, which is an index of relative frequency; the index expresses the number of article in ‰ terms, with respect to the total number of articles generated by ANSA in each year. We treat each municipality with corruption level taking place in its province and region capital:

$$corruption_{ity} = \frac{n_{ity}^P + n_{ity}^R}{N_y} * 1000$$

with $corruption_{ity}$ corruption index for municipality i in month t in year y , n_{ity}^P number of articles about political corruption in province capital of i at time ty , n_{ity}^R number of articles in region capital of i and N_y total number of articles of ANSA in year y ⁴.

Figure 2.3 shows political corruption index for four large Italian cities with monthly aggregation; several remarks can be done: first, average frequency of the index increases after 2003, this can be due to a better coverage of Factiva or to an actual deterioration of the phenomenon; second, there are peaks in correspondence of important corruptive events (*e.g.* we can see a huge spike in January 2004 in Milan, corresponding to the Parmalat crac, the biggest bankruptcy scandal of a private company in Europe; the scandal involved national and local politicians and was massively reported in the press. Milan court was in charge of financial crimes). In terms of distribution across time, Figure 2.4 reports the aggregate monthly frequency of the corruption index: it emerges that articles frequency raises starting from 2003⁵. From 2007 on, the index is quite stable and it seems to emerge a weak seasonality⁶. In terms of distribution across geography, the region with the highest average is Lazio (where Rome is located, more than four times higher than the national average), followed by Lombardy (region of Milan) and Sicily. Table 2.2 shows the descriptive statistics of corruption indices, we focus on monthly and yearly aggregation and different versions of the index are presented: "Corruption Region cap." captures scandals taking place in region capitals alone and "Corruption Region & Province cap." captures scandals of region and province capitals, and it is the main index of the analysis, with an average value of .2744 and a standard deviation of .5442. Moreover, "Corruption Region cap. - Office" and "Corruption Region & Province cap. - Office" limit the set of articles to those where also political offices are mentioned and "Corruption left wing - Regional politicians" and "Corruption right wing - Regional politicians" focus on corruption scandals of left or right wing local politicians; these indices only focus on regional politicians given the difficulty to precisely detect political alignment of administrators in lower administrative levels, due to the capillary

⁴We generate another version of the index where we rescale absolute number of articles with the total number of articles of ANSA in the reference month of the reference year, *i.e.* using N_{my} , with m reference month, as denominator of the index. We include the analysis preformed with this index in the robustness checks section.

⁵This weak upward trend in (local) political corruption has been highly discussed in Italian public debate, and some accused constitutional reform of 2001 (*Riforma del Titolo V della Costituzione*) that stated fiscal autonomy (revenues/expenses) of local administration (regions, provinces and municipalities). One advocate of this hypothesis is Raffaele Cantone, president of the Italian Anti-Corruption Association (ANAC-*Associazione Nazionale Anti Corruzione*), claiming that the reform "[...] increases units of expenses in a meaningless fashion leading to situations where local administrations, as regions, spend lot of money." (http://www.rainews.it/dl/rainews/articoli/Cantone-Norma-criminogena-la-riforma-del-Titolo-V-Costituzione-91737009-8668-4d93-8139-74b73cc33138.html?refresh_ce).

⁶In particular, the number of article about corruption is systematically lower during the summer and Christmas periods. This is probably due to the reduced activity of editors, journalists and courts in these periods of the year.

diffusion of civic lists⁷, not officially affiliated to any national party. To identify the ideological affiliation of involved politicians we use their names, as mentioned in the articles, and we trace back this information from the *Anagrafe degli Amministratori Locali e Regionali* (Italian Ministry of Internal Affairs).

[Figure 2.3 here]

[Figure 2.4 here]

Finally, to validate our measures we explore whether high levels of corruption exposure are correlated with peaks in attention toward "corruption related topics" by the audience: in other words, whether supply of corruption topics (in the press) leads to demand of corruption issues. We explore this issue using Google trend⁸ and we measure attention to corruption making use of specific keywords (*corruzione*-corruption, *abuso d'ufficio*-malfeasance in office, and *concussione*-embezzlement); then we study how traffic of these terms evolves after a scandal, on a daily basis. What emerges from this simple analysis is that after an article about corruption comes out in the news, attention devoted to this theme on the web surges, measured with Google traffic on "corruption keywords" (details on this analysis are contained in the Appendix A1).

2.2.2 Political Participation Measures

Italian municipal polls lead to the election of the municipal council (*Consiglio comunale*) and the mayor (*Sindaco*) who is in charge of appointing the local executive council (*Giunta*). The current electoral system implies a single ballot, with plurality rule, for city with less than 15,000 inhabitants and runoff for the others (in the analysis we only focus on first ballot) as well as the direct election of the mayor. Between 1993 and 2000 legislative term was four years, and after 2000 it has been extended to five years and mayors face a two-term limits. Our sample includes municipal elections for all Italian municipalities in the time span 1999-2014⁹.

We want to explore the impact of corruption exposure on all aspects of political participation. First, we study the effect on electoral turnout (ratio between voters and eligible citizens) for municipal polls at the municipal level. Second, we want to shed light on the effects on selection of politicians: background of elected mayors (whether she/he was the incumbent or runs in past elections) and characteristics of elected politicians (age/gender/education of mayors and councillors). Third, we consider the impact on local political supply, *i.e.* characteristics of the pool of candidates; in particular, we look at the number of candidates and their background (*i.e.* whether they run in previous elections or the incumbent runs again) as well as the number of candidates lists. All these data on municipal election come from the Italian Ministry of Internal Affairs and from the *Anagrafe degli Amministratori Locali e Regionali* (published by the Italian Ministry of Internal Affairs). Table 2.3 shows descriptive statistic of the outcome variables.

⁷In Italian local elections a civic list, *lista civica*, is a list of candidates running for mayor or councillor, which is not, officially, affiliated with any national political party. These lists are very important for local politics, they were, for instance, crucial for the early eradication of *Five Star Movements* (born after electoral experiences of lists such as *Amici di Beppe Grillo* and *Liste Civiche a Cinque Stelle* in municipal elections in the early 2000).

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

⁹There are few missing years for the region of Sicily (1999-2006).

2.3 Identification Strategy

2.3.1 Event Study Analysis

We are interested in the causal effect of corruption exposure on political participation, but this link can be problematic to compute. First of all, it could be that places with lower political participation may happen to be those in more corrupted regions. This could be due, for instance, to local unobservable characteristics such as low level of *civic spirit* or *loyalty to institutions*. Moreover, it can be that political participation was already decreasing *before* the scandal takes place. As a result there could be a spurious negative correlation between corruption exposure and political participation.

In order to study this causal relation, the empirical strategy is structured as an event study analysis¹⁰. On the one hand, we take advantage of the large geographical and temporal variation of political scandals, reported in newspaper articles. On the other hand, we exploit the variation in the dates of municipal elections. Timing of local polls is staggered over time in Italy: every year a group of cities votes and the formation of these groups is mostly due to historical factors (Gagliarducci, Coviello 2010, Repetto 2016, Giommoni 2018). The essence of this identification strategy lies in the comparison of the impact of scandals taking place before elections with those occurring after, with cities not exposed to corruption forming the control group. The key identifying assumption is, then, that the timing of scandals is plausibly exogenous with respect to elections, being not correlated with electoral dates. This assumption is plausible in this setting given the non-local nature of the treatment: we focus on scandals that hit politicians of local capitals, as each municipality is treated with corruption that took place in his province and regional capital. First, this local/non-local approach makes implausible that the treatment is affected by local unobservables characteristics, and in particular that timing of capital scandals is tailored on local election dates. Furthermore, this allows us to interpret the treatment as pure media exposure, without any direct effects, as considered scandals do not directly involve local politics, but they are rather in the political background.

Nevertheless, there is still room for possible stories that could violate the main identifying assumption. This could be the case if, for instance, scandals' timing is somehow manipulated by the editors and journalists in order to distort local electoral outcomes¹¹. Moreover, it may also be the case that public attorneys and prosecutors manipulate investigation timing with the aim of affecting political outcomes¹². It is, indeed, plenty of anecdotes that this happens in national politics: one recent case is the reopening of F.B.I.'s investigation into Hillary Clinton's use of an unsecure server eleven days before 2016 United States Elections; this scandal, as argued by Bill Clinton after Hillary's lost, "*costs her the election*". In order to address this issue, we

¹⁰For introduction on event-study see MacKinlay (1997). Other contributions are for example Jacobson, et al. (1993), Kline (2011).

¹¹There is a literature in Communication Science studying how editors/journalists choose news to be published. There are two opposing hypothesis: the one considering editors choosing news based on their professional judgements, the *trustee model*, and the other considering editors following audience interests, the *market model* (Schudson 2003). There are some papers testing empirically these models and findings are that audience revealed preferences on stories (generally measured with views to on-line articles) influence journalists decision-making (Anderson 2011, Welbers et al. 2015). So, it seems that market forces shape journalists' decision to publish a piece of news and this considerably limits space for journalist's discretion, but this is clearly not enough to claim the absence of manipulation in this specific setting.

¹²Some studies document this phenomenon in Italy, where the judges enjoy high levels of formal independence from the political power (Fabri 1997, Guarnieri 2011) and judiciary is strongly politicized (Della Porta 2001). Moreover, there is some evidence that political alignment of judges affects decisions to prosecute members of the parliament from some parties more than others (Ceron, Mainenti 2013).

test whether there is manipulation in the number of scandals released by the press around the cutoff represented by election dates: in particular, we run a McCrary test checking whether the treatment is discontinuous around the threshold (results of this validity check are shown in section 3.2); and we find that the density of scandals immediately before the cutoff (election date) is not statistically different from the one immediately after the cutoff. This disproves the presence of manipulation in corruption news timing. This result is not surprising as these elections are local, involve only a portion of cities in a province or region, most of them take place in very small cities, and they are not very salient events to be covered by national news.

The event-study gives the possibility to exploit the high frequency of the scandal database and to explore how the treatment affects the set of dependent variables and how the effect evolves over time. We will show two different specifications: a classic monthly event-study analysis (to evaluate the effect in the short-run) and a compact version (similar to the one in Bottan, Perez-Truglia 2015) to explore short/long run effects in the same specification.

The first regression specification, the classic event-study, is as follows:

$$y_{i,t} = \sum_{p=1}^{+4} \alpha_p^{lag} C_{j,i,(t-p)} + \sum_{p=-4}^{-1} \alpha_p^{lead} C_{j,i,(t-p)} + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t} \quad (2.1)$$

With $y_{i,t}$ the outcome variable (*e.g.* voter turnout) in municipality i in year/month t ; $C_{j,i,(t-p)}$ is the main explanatory variable and it represents the amount of corruption recorded in local capitals j of municipality i at time $t - p$ ($C_{j,i,(t-1)}$ reports index value in year/month $t - 1$ [lag 1] and $C_{j,i,(t+1)}$ value at year/month $t + 1$ [lead 1]). Lagged variables ($C_{j,i,(t-1)}, \dots, C_{j,i,(t-P)}$) are explanatory variables of the analysis, while forward variables ($C_{j,i,(t+P)}, \dots, C_{j,i,(t+1)}$) are included as a falsification test, to check whether the outcome was already decreasing (or increasing) before the scandal takes place. $X_{i,t}$ is the vector of municipal specific controls, γ_i are municipality fixed effects and δ_t year fixed effects.

The second specification is the compact version:

$$y_{i,t} = \sum_{p=1}^{+2} \alpha_p^S C_{j,i,(t-p)}^{Short} + \alpha^L C_{j,i,t}^{Long} + \alpha^P C_{j,i,t}^{Placebo} + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t} \quad (2.2)$$

With $C_{j,i,(t-p)}^{Short}$ representing the index of corruption exposure in the short-run, *i.e.* number of scandals in years $t - 1$ and $t - 2$. $C_{j,i,t}^{Long}$ represents the index in the long-run, *i.e.* aggregate number of scandals before $t - 2$ years. And $C_{j,i,t}^{Placebo}$ is the placebo test and it is defined as the corruption exposure in year $t + 1$. Finally $y_{i,t}$, $X_{i,t}$, γ_i and δ_t are defined as in the classic specification.

For the analysis on voter turnout all regressions contain following list of controls: municipality and year fixed effects, province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (yearly at the province level), number of candidates, dummy whether incumbent reruns, share of old candidates (political variables refer to past elections) and number of years of *commissariamento* (which takes place in case of municipal government dissolution imposed by the national government, this can be due to several reasons such as Mafia infiltrations). For the analysis on political selection and supply the regressions include the controls: municipality and year/macroregion fixed effects, province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (yearly at the province level) and number of years of *commissariamento*. Finally, in both analysis robust standard errors are clustered at the region level.

2.3.2 Validity Check

The main empirical challenge for estimating models (1) and (2) is to show that there are not confounding factors affecting timing of media coverage of scandals. In this section, we want to deal with this issue by studying whether scandals timing is strategically manipulated close to election dates. In particular, we analyse whether there are systematically more articles about corruption right before elections, compared to following days. This implies to study whether there is manipulation in the corruption index around the threshold represented by the day of municipal elections. If we found that there are typically more articles about corruption above the cutoff this could signal some form of strategic manipulation in articles' timing casting doubts on the presence of potential confounding factors at the local level. To do this, we run a McCrary test (McCrary 2008) on the entire sample of provinces, in a daily analysis where we consider that a province holds municipal elections whether at least one municipality vote in that day; we focus on a time window of 360 days (180 before and 180 after elections)¹³.

Figure 2.5 shows the output of the McCrary test: left sub-figure includes the complete sample of Italian provinces and right sub-figures is limited to region capitals. Both figures allow us to reject the null hypothesis that there is a discontinuity in the density of scandals around the cutoff and they suggest that there is not manipulation around election day in corruption articles. This result is not surprising in the specific setting of Italian municipal elections: these polls are not very salient for the general audience and the great majority of cities involved are small, often very small: for instance, 69.59% of Italian municipalities have less than 5,000 inhabitants and 24% of them have less than 1,000 inhabitants.

[Figure 2.5 here]

Moreover, we raise the issue whether manipulation could emerge in a sub-sample of elections that are more salient: results are similar if we limit the sample to local elections with many municipalities voting (where the number of cities voting is higher than the median number) and to local elections characterized by high levels of voter turnout (where the average turnout of cities voting is higher than the median value). Finally, we run the same check reducing the sample of local elections to those that overlap with national, region or province polls as in these moments politics is particularly salient for the audience: also in these cases we can exclude the presence of manipulation (outputs for these further checks are not shown and are available upon request). We can conclude that there is no evidence of manipulation of scandals timing and this validates the key assumption of the event study analysis.

2.4 Results

2.4.1 Political Turnout

Main Analysis

In the first part of the paper we use political turnout as the dependent variable; it is a direct measure of political participation and it has been employed as a proxy for civic spirit and pro-social behaviour. The outcome of the classic event-study analysis is the event study-graph represented in Figure 2.6, where we inspect the effect of corruption exposure on voter turnout in the short run with monthly aggregation. Coefficients in the right part of the graph are lags, explanatory variables, while coefficients in the left part are leads, placebo variables; the graph includes 95%

¹³We conduct the same test using a time window of 180 or 240 days as well as the optimal bandwidths computed according to McCrary (2008): results are always similar.

confidence intervals and the corresponding table is included in the Appendix A2 (Table 2.10). From the analysis, we can see that the impact of local capital scandals on neighbouring cities is negative and significant: in particular, the effect is sizeable after one month, it remains negative after two and three months and then it vanishes. In terms of magnitude the impact is significant only after one month and one standard deviation increase in corruption exposure lowers turnout by 0.79%. Moreover, the leads are not distinguishable from zero, meaning that voter turnout was not decreasing before scandals and this excludes the presence of pre-trends in voter turnout at the local level. This result suggests that local scandals immediately impact voters behaviour but the effects is highly temporary as it disappears soon¹⁴.

[Figure 2.6 here]

For the compact analysis, we present a single specification where we include short and long run coefficients together (as in Botta, Perez-Truglia 2015). Column (1) of Table 2.4 shows the results for the specification with the complete set of fixed effects and controls. First, the effect in the short run is captured by variables "Short-run effect (1 year)" and "Short-run effect (2 years)", representing the impact of scandals of local capitals taking place one and two years before elections. Observing the table, it emerges that the effect of corruption exposure is negative and significant on voter turnout. Moreover, the magnitude of the effect is not negligible: as corruption index raises by one standard deviation, voter turnout decreases by 0.36% after one year and by 0.26% after two years; this means that between 3.7% and 2.7% of dependent variable average variation is explained by the corruption index. The magnitude of this effect is not trivial if we consider that the impact of the entry of a newspaper increases turnout in Italian municipal elections, on average, by 0.45% (Drago et al. 2014)¹⁵. Second, the effect in the long run is captured by the variable "Log-run effect (3+ years)", representing the aggregate number of scandals of local capitals before two years; we can see that despite being negative, the coefficient is not statistically different from zero. Finally, variable "Pre-scandal effect" represents the placebo test, *i.e.* aggregate number of scandals one year after the elections, and it is evident that there are no pre-trends, as the coefficient is non significant and small; this means that before a scandal voter turnout between treated and control units evolves similarly.

As a second step we raise the issue whether negative impact of scandals is differential in terms of political affiliation of involved politicians and local preferences. Muço (2017) shows that after a scandal voters tend to punish party members of involved politicians and that this effect spreads out geographically across neighbouring municipalities. We want to study whether this effect emerges also for political turnout in our setting and whether it is mediated by local preferences. In particular, we study whether scandals involving left or right-wing politicians affect differently participation across cities administered by mayors with different political alignments. For this analysis we make use of ideological-specific measures of corruption exposure that rely on information of mentioned names and we limit the treatment to scandals of politicians from the regional administration. Table 2.5 shows results for this analysis: the set of explanatory

¹⁴We also conduct the analysis with semester aggregation. The negative effect of corruption is confirmed and a clear seasonality emerges in the marginal effects of the index. In particular, scandals taking place in odd semesters are systematically stronger than those happening in even ones and this is a result of the seasonality of scandals and local elections over the year: on the one hand, there are regularly less scandals during the summer and Christmas holidays, on the other hand municipal elections almost always take place in May or June. This pattern generates an oscillatory behaviour in the marginal effect of the index which disappears in the yearly analysis, this is why we mostly rely on the compact analysis in this study.

¹⁵In particular, Drago et al. 2014 only focus on large municipality, with more than 15,000 inhabitants, while our estimates cover the entire sample.

variables is limited to the effect of corruption after one year, interacted with political alignment of the mayor (civic list, left-wing or right-wing). Column (1) is the benchmark and it shows the impact of a scandal without distinguishing for the alignment of involved politicians; the index used, for consistency with other columns, only captures scandals involving politicians in regional administration. It emerges that the effect is negative and sizeable for cities administered by ideological mayors but it is not different from zero for cities with a civic mayor. Column (2) and (3) show the differential effect by local administration of ideology specific scandals. What emerges is that local political preferences mediate the effect of corruption as cities with a left-wing mayor react more, with a larger drop in turnout, to scandals involving left-wing politicians (column 2), and the opposite happens for cities with a right-wing mayor (column 3), despite the difference being much smaller. This seems to confirm that the effect of corruption is heterogeneous with respect to the "colour" of the scandal and to political preferences locally.

From this analysis we can conclude that corruption exposure significantly reduces voter turnout in neighbouring municipalities, the effect weakens with time and, finally, it vanishes after two years from the scandal. And it does not seem to be pre-trends in voter turnout prior to scandals occurrence. Moreover, it emerges that local political preferences seem to be an important mediator for the effect of corruption.

2.4.2 Political Selection

In this section we are interested in studying whether exposure to scandals of local capitals affects selection of politicians. Characteristics of elected administrators, differently from voter turnout, are the result of a longer and more articulated process involving the evolution of voters' preferences over candidates characteristics and profiles. Given the slow moving nature of these processes, in this section we only perform the compact version of the event-study, without providing the monthly analysis.

We evaluate the impact of corruption on characteristics of elected mayors and councillors: first, age, gender and education. On the one hand, female and young politicians are generally regarded as "underdogs" in Italy (Galasso, Nannicini 2011), on the other hand, education is commonly used as a measure of politicians quality (*e.g.* Merlo et al. 2010, Galasso, Nannicini 2011). Columns 1-6 of Table 2.6 contain results on politicians characteristics. From the table emerges that there are no sizeable effects on characteristics of elected mayors (columns 1-3) as well as councillors (columns 4-6). These results suggest that exposure to corruption does not impact on the selection of politicians, without any substantial effect on characteristics of elected administrators. Furthermore, as a second analysis we focus on the background of elected mayors to explore whether chosen politicians are more likely to be tenured administrators or freshmen, without any political experience; we concentrate on the probability that the incumbent is re-elected and the likely that politicians who already run in the past are elected with a focus on all past election of that city. Results are shown in columns 7-8 of Table 2.6: first, there is evidence, both in the short and in the long run, that the probability that an old candidate is elected raises consistently (column 7), the effect reduces progressively for older scandals but the accuracy is higher in the long run. Second, it emerges that the probability that the incumbent is re-elected raises (column 8) but the effect is weaker and only lasts in the short run. In terms of magnitude, one standard deviation increase in the index raises the probability that an old candidate and the incumbent is elected, respectively, by 1.9% and 1.5%, after one year. These results suggest that, as effect of a scandal in local capitals, tenured politicians tend to be more represented as mayors and this means that freshmen are less likely to be elected.

On the one hand, from this section it emerges that being exposed to local scandals does not lead to the election of politicians with different characteristics, suggesting that the electorate

does not react to perceived corruption trying to improve or renew the political class¹⁶. On the other hand, after a scandal, elected mayors are more likely to be tenured politicians.

2.4.3 Political supply

As a second analysis, we consider how local political actors endogenously responds to the occurrence of scandals. To answer this question we inspect a series of variables. First, we want to explore how candidate pool modifies: number of candidates running in an election for mayor and number of candidates lists; second, similarly to previous section, we want to measure whether political background of candidates modifies, *i.e.* whether they are more likely to be tenured politicians, using as proxy the share of candidates that already run in past elections and the probability the incumbent reruns, or freshmen without any political experience.

Column (1) and (2) of Table 2.7 contain results on number of candidates running for the mayor seat and number of lists. Two main results emerge: first, both outcome variables significantly reduce after a scandal, second the effect has a reverse U-shape, the impact is tiny after one year, it reaches the maximum after two years and then it declines again in the long run: this is coherent with the fact that, differently from voter turnout, the decision to run as a candidate takes time to be made and it slowly evolves over time. In terms of magnitude, one standard deviation increase of the corruption index decreases after two years the number of candidates by 0.05 units and the number of lists by 0.075 lists, explaining respectively 3.8% and 2.1% of these variables standard deviation. On the other hand, if number of candidates reduces, we want to see who is leaving the competition. Columns (3) and (4) of Table 2.7 contain results for the analysis on candidates background: first, scandal exposure increases the probability that old candidates run (columns 3). Moreover, the effect is stronger in the short-run, although it is not significant after two years, and it declines in the long-run. Second, the same result applies if we consider the probability that the incumbent reruns (columns 4), and it declines for older scandals. The magnitude of these effects is not negligible as one standard deviation increase in the index raises the fraction of old candidates by 1.6% and the probability that incumbent runs by 1.4%. This result shows that the relative importance of tenured politicians raises in the local political environment and this implies that, after a scandal, freshmen are more likely to leave the competition. Moreover, it is reasonable that this modification in candidates background, as effect of corruption is, at least partially, driving the result observed in the previous section that tenured politicians are elected with higher probability.

This section suggests that exposure to scandals leads to a consistent reduction in the number of running candidates and lists and it raises the space of tenured politicians. These effects reduce consistently the level of political competition in local elections and may be interpreted as a drop in direct participation of citizens into local politics.

2.4.4 Heterogeneity analysis

In this section we want to explore heterogeneous effects of corruption exposure on two main variables of the analysis, namely voter turnout and number of candidates. We conduct five different analysis: results for voter turnout are shown in Table 2.10 and those for number of candidates in Table 2.11.

¹⁶The eradication of Italian *Movimento Cinque Stelle*, a populist party that recently obtained a large electoral consensus, has been due among other reasons to the reaction to widespread public corruption. We cannot properly observe the impact of the movement in this analysis as it runs in few municipalities within this time span. First municipal elections where the movement obtains consistent results has been the one of June 2016 (where the party wins in Rome).

In the first inspection, we study differences in local political background: columns (2) and (3) of Table 2.10 and 2.11 show the effect for municipalities below/above median turnout in previous elections; focusing on the effect of voter turnout, it is evident that cities with higher past turnout react more than the other group. We can link the result on voter turnout with the social capital literature (Banfield 1958, Putnam 2001) that identifies turnout as a measure of civic spirit; in these regards, places with higher social capital face less vote buying as well as clientelism and political participation is more likely to be motivated by civic engagement. Thus, corruption exposure is more effective where citizens vote according to moral concerns rather than to material interests. Second, it seems that the effect becomes stronger as electoral competition raises, *i.e.* as margin of victory decreases (columns 4-5). This result can be explained by the fact that scandals are more salient when political debate is more intense and this leads people reacting more to perceived corruption. If we focus on the variable number of candidates, instead, there are no differential effects emerging in settings with different degrees of past voter turnout or margin of victory, meaning that local level of political competition is not mediating this relation.

Second, we examine differential effect across geography: Italy is marked by deep differences between macro-areas under many aspects such as economic development, political background and cultural origin. Columns (6) and (7) of Table 2.10 study geographical heterogeneity for the voter turnout: we can see that the effect is strong and significant for centre-north and not distinguishable from zero for centre-south¹⁷. Again this evidence may be motivated quoting the literature on social capital in Italy (Banfield 1958, Putnam 2001) that underline that northern regions are marked by high levels of civic spirit opposed as southern regions. This result, along with previous evidence on turnout, seems to suggest that civic spirit is an important mediator of the effect of exposure to scandals on turnout. On the other hand, column (6) and (7) of Table 2.11 show that the effect on number of candidates emerges in both areas but it is stronger in the centre-south, despite being less accurate, and this suggests that in low social capital regions political supply reacts more to corruption. This may be due to denser political networks in southern regions that leads to direct effect of a capital scandal at the local level.

As a third analysis, we study whether differences in newspaper circulation mediates the effect of exposure to scandals: media economics literature underlines that the presence/entry of newspapers raises electoral participation (Gentzkow et al. 2011) and affects other political aspects such as the incumbency advantage (Snyder, Stromberg 2010). Moreover, Drago et al. (2014) show that local Italian newspapers play an important role in terms of electoral turnout and efficiency of municipal governments. In our setting, we want to see whether newspaper circulation is able to amplify the effect of scandals, focusing on national newspapers. To do this we make use of data on circulation at the province level from *ADS-Accertamenti Diffusione Stampa*, for national newspapers¹⁸ and we run the main specification for the sub-sample of municipalities below/above median level of newspaper circulation, in the time span 2003-2014¹⁹. Columns (8) and (9) of Table 2.10 show the results for voter turnout: municipalities above the median level react more to scandals compared to those below where the effect is much weaker. On the other hand, columns (8) and (9) of Table 2.11 shows the same result for number of candidates and the relation seems to be the opposite as the effect only emerges in cities with lower newspaper circulation: this is likely to depend on the fact the effects on number of candidates are stronger in southern regions that are marked by lower levels of newspaper circulation. We could conclude that diffusion of newspaper is an important mediator for the effect of political scandals on voter

¹⁷We divide Italian territory in two macroareas: centre-north including northern regions as well as Tuscany, Umbria and Marche, and centre-south, including southern regions, islands and Lazio.

¹⁸We include top 18 Italian national newspapers in 2013, according to Wikipedia (https://it.wikipedia.org/wiki/Quotidiani_in_Italia).

¹⁹We were not able to collect circulation data for the period 1999-2002.

turnout and this can be due to the fact that news circulation makes corruption stories more salient or that it is associated to higher interest in politics, or in more pronounced civic spirit. But we cannot say the same for the effect on number of candidates.

Then, we inspect whether the share of immigrants in a municipality may affect the impact of corruption, making use of information on immigrant population at the municipal level from ISTAT (*Istituto Nazionale di Statistica*). From Tables 2.10 and 2.11, Columns (10) and (11), it emerges that the impact of corruption is stronger in cities with a higher share of immigrants, both in terms of turnout and number of candidates. We may interpret this result in the light of the recent literature showing that the presence of immigrants increases consensus of far-right parties and populist forces (Barone et al. 2016, Halla et al. forthcoming, Otto, Steinhardt 2014). In cities characterized by an higher share of migrants, the degree of dissatisfaction to the political class may lead voters to react more to corruption with a decrease in political participation.

Finally, we study whether the dimension of cities mediates the effect of corruption. In order to do this we reduce gradually the sample keeping cities with larger population and we track the evolution of short run coefficients. Figure 2.7 shows results for voter turnout: left sub-figure shows the evolution of the impact of corruption after one year (coefficient "Short run (after 1 year)") and right sub-figure focuses on the effect of corruption after two years (coefficient "Short run (after 2 years)"). Two remarks may be done: first, the impact of corruption on turnout does not seem to modify much as we select samples of cities with larger population; second, the precision of estimates decreases for larger cities mechanically as we are gradually reducing the sample size. On the other hand, Figure 2.8, shows the same heterogeneity analysis for the variable number of candidates: first, the impact of corruption after one year do not modify consistently as it is never different from zero, second the impact of corruption after two years considerably increases with city dimension, for instance the coefficient for cities with more than 20,000 inhabitants is more than four times larger than the one for the complete sample. This suggests that the effects of corruption on number of candidates are much stronger in large cities.

[Figure 2.7 here]

[Figure 2.8 here]

In this section, we shed light on factors mediating the effect of being exposed to corruption. On the one hand, in terms of voter turnout, it emerges that municipalities reacting more are those where people participated more to past polls, where elections are more competitive, with higher levels of newspaper circulation and share of immigrants and located in the centre-north of the country. While there are no heterogeneous effects in terms of city size. On the other hand, in terms of number of candidates, it turns out that cities with larger reductions are those located in the centre-south, with lower newspaper circulation, with higher share of immigrants and with larger population. In general, important heterogeneous effects of corruption emerge from these analysis, and this reflects the large heterogeneity of Italy in economic, political and cultural terms. Nevertheless, we should take these results as simple correlations without any causal interpretation.

2.5 Robustness checks

2.5.1 Voter turnout

In this section, we run several robustness checks relative to the analysis on voter turnout; in particular, we want to show that the result does not depend on how the sample is structured and the index is constructed as well as on the specification we use.

In columns 1-4 of Table 2.8 we run the main analysis modifying the specification and the sample. Column (1) contains the main specification. Column (2) shows the analysis without pre-scandal effect and coefficients are very similar to the ones in the main specification. In column (3) we restrict the sample excluding all municipalities in the province of region capital in order to exclude potential direct effects of the scandals that may spillover in the province of region capitals: the main result holds, the magnitudes of coefficients do not modify significantly and the coefficient associated to the pre-scandal effect is even weaker. Moreover, column (4) contains the output for the analysis performed with province aggregation. In particular, we run the main specification at the province level using as dependent variable the average turnout of municipalities in a province in a certain year/month: the sample shrinks, standard errors raise but the negative effect of corruption emerges in the short-run. In columns 5-6 of Table 2.8 we test other versions of the index to show that the results do not depend on specific choices adopted in the construction of the corruption indicator. Column (5) shows results for the analysis conducted with the index rescaled with monthly amount of articles, instead of yearly: in the main index we rescale the absolute count of articles for the total number of articles in the year the news come out, in this version we are scaling the absolute number of articles with the total number of news in the month/year it appears (using as denominator N_{my} instead of N_y). Coefficients of the analysis are slightly weaker but the negative effect still emerges. Finally, in column (6) we are using the version of the index where we impose a third criterion for the article identification: which is the mention of the political office title (*e.g.* mayor, alderman, ...) in the article text. This further requirement raises precision of the process but it fairly decreases the sample of corruption article, losing many corruption stories: Figure 2.9 shows aggregate frequency of this version of the index over time, and it is clear that the measure starts being reliable from 2003, indeed we limit the sample to the time span 2003-2014 in this analysis. Column (6) shows the main analysis using this version of the index and it confirms main results, with a strong and negative effect after one year (unique exception is the positive, and small, effect in the long run).

[Figure 2.9 here]

From this section it emerges that the negative effect of the treatment on voter turnout persists as we make use of different specifications, samples and different versions of the index.

2.5.2 Number of candidates

In this sections we repeat the set of checks performed in previous section for the analysis on the number of candidates. In columns 1-4 of Table 2.9 we run the analysis modifying the specification and the sample. Column (2) includes the main specification without pre-scandal effect and coefficients are very similar to the main analysis. In column (3) we restrict the sample excluding all municipalities in the province of region capital and negative effects are even stronger in magnitude than for the main specification, moreover the effect of corruption is also significant after one year. Then, the negative effect of corruption emerges in column (4) showing the output for the analysis performed with province aggregation. In columns 5-6 we test different versions of the index: column (5) shows the main specification performed with the index rescaled with monthly amount of articles and in column (6) we are using the version of the index with the further requirement of mention of political office in article text. In both analysis the negative effect of corruption emerges.

Also from this section, it emerges that the negative effect of corruption on number of candidates persists as we make use of different specifications, samples and different versions of the index.

2.6 Concluding Remarks

In this contribution we explore effects of corruption exposure on local political participation. We focus on all aspects of participation: voter turnout, selection of politicians and political supply. Our results suggest that what prevails is a generalized negative reaction after a scandal, both in terms of citizens and politicians participation.

First, we find that municipalities which are exposed to political corruption of their local capitals face lower levels of voter turnout and the effect is sizeable in the short run and it vanishes in the long run, after two years from the scandal. Moreover, we show that this effect is mediated by local political preferences as left-wing cities tend to react more to scandals involving left-wing politicians and the opposite happens for right-wing cities. Furthermore, we find evidence of heterogeneous effects as the relation is stronger for cities located in the centre-north, with higher electoral competition, newspaper circulation levels and share of immigrants. Following Chong et al. (2015), this result can be read in the light of the model by Persson, Tabellini (2000) where corruption works as a signal for the entire political class leading voters to, negatively, update their beliefs and to reduce their participation in equilibrium. Second, we focus on selection of politicians, mayors and councillors: we do not find clues that corruption exposure leads to relevant modifications in the characteristics of politicians. Instead, we find that the background of elected modifies consistently as tenured politicians, incumbents and old candidates, win with higher probability, compared to freshmen. Finally, we study the impact on the political supply. On the one hand we find that as a response of a scandal fewer candidates run and less electoral lists are presented; moreover the effect on the number of candidates raises consistently with cities population. On the other hand, we show that this result is driven by a decrease in the number of freshmen while tenure politicians gain further space as the proportion of old candidates and incumbents who run raises. We could read this last result in the light of the model by Caselli, Morelli (2004) as the presence of corruption could affect payoffs of being politicians, in particular through a reduction of reputation payoffs. This would work as a negative externality raising entry costs for freshmen without modifying incentives for “old” politicians and could generate, in equilibrium, a lower number of non-politicians running for mayor. This study contributes to the large literature focusing on the effects of political corruption in many ways: first, it evaluates the effect of corruption on voter turnout with a daily and local indicator being able to distinguish between short and long-run effects. Second, this is among the first papers to explore the impact of corruption on the selection of politicians and on the political supply at the local level. Third, this paper provides an empirical test to evaluate the impact of corruption on local entry decisions of candidates.

In conclusion, this analysis documents sizeable effects of corruption on political participation. After a scandal in the local capital, citizens of neighbouring cities vote less, fewer candidates run for the seat, less lists are presented and elected administrators are more likely to be tenured politicians. Corruption generate consistent and general negative effects on political participation.

2.7 Figures

Figure 2.1: Italian territory with administrative divisions and local capitals



Notes: The figure shows Italian territory underlying administrative divisions. Regions are delimited by black borders, and municipalities by light grey ones. Red dots are local capitals, *i.e.* 103 province capitals, 20 of them are also region capitals (in 2004).

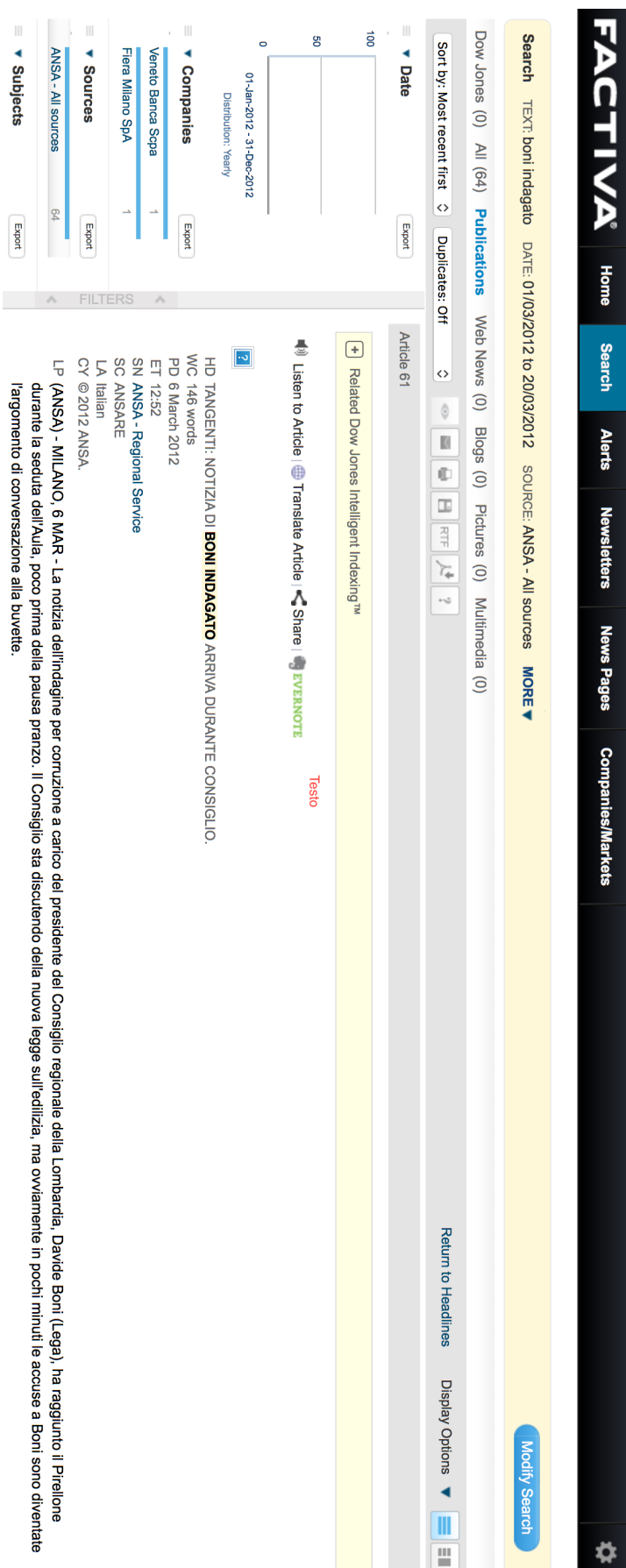
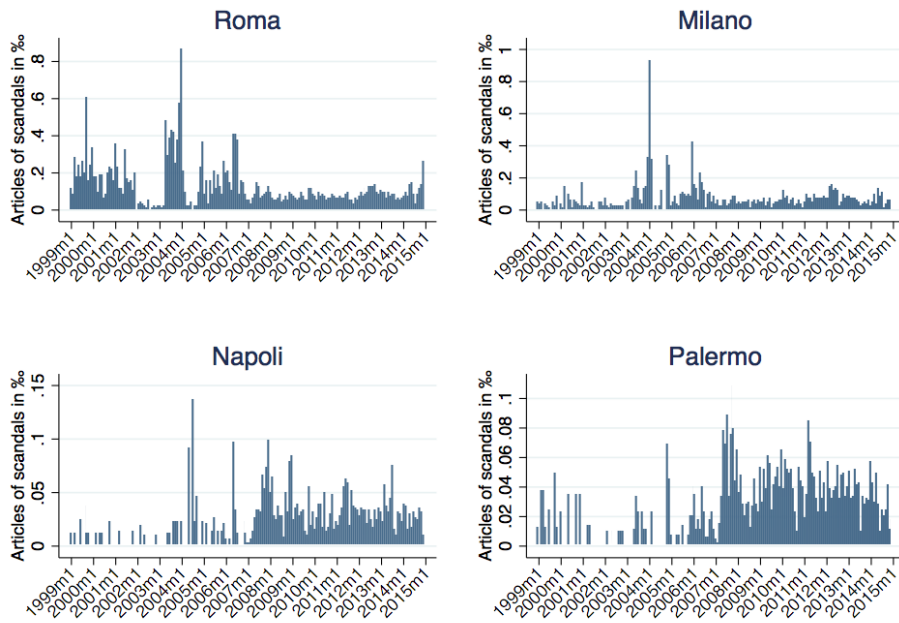


Figure 2.2: Example of Factiva extraction - corruption case in regional council of Lombardy. The text of the article is as follows:

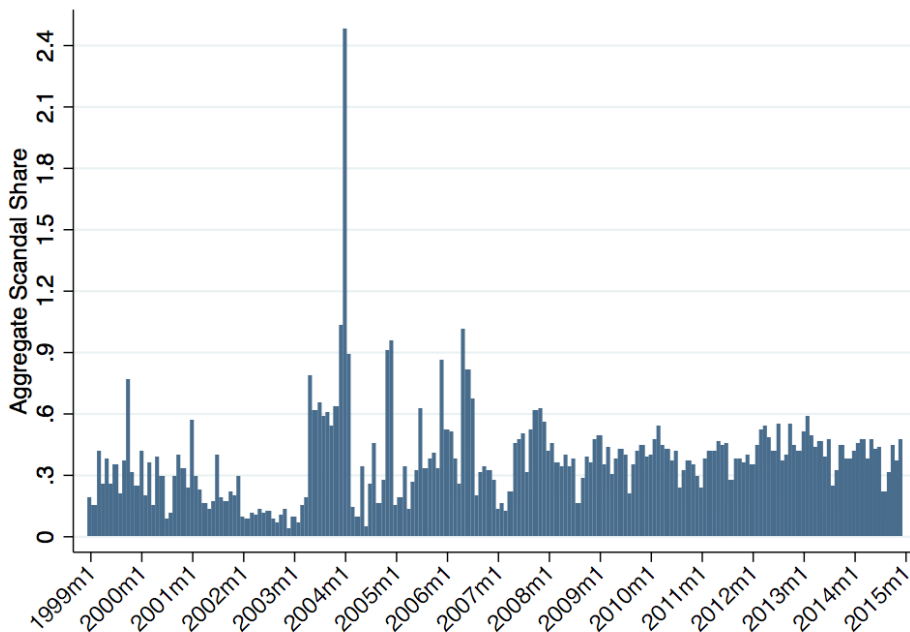
“(ANSA) - Milan, March 6th - The communication about the corruption investigation against the president of the Regional Council of Lombardy, Davide Boni (Northern League), has come during the assembly of the Council, just before lunch break. The Council was debating about housing legislation, but Davide Boni's investigation became soon the main topic of discussion during the lunch break.”

Figure 2.3: Corruption exposure in four Italian large cities



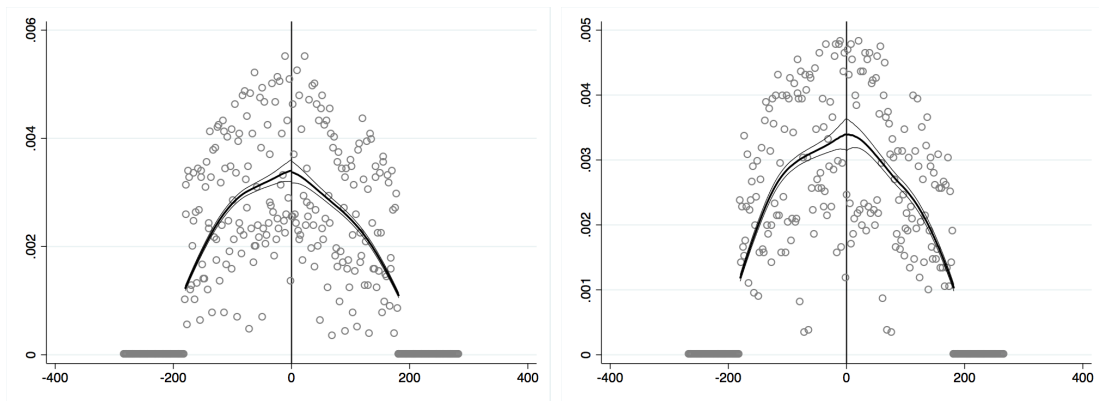
Notes: The figure contains the value of the index "Corruption Region & Province cap.", in per-thousand terms, aggregated over time for four large Italian local capitals, with monthly aggregation.

Figure 2.4: Aggregate corruption exposure



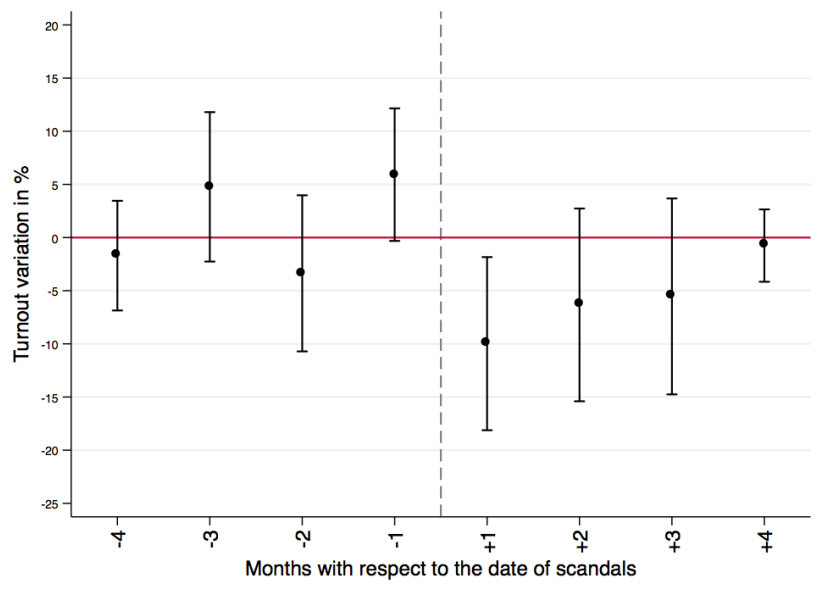
Notes: The figure contains the value of the index "Corruption Region & Province cap.", in per-thousand terms, aggregated over time and over all Italian local capitals, with monthly aggregation.

Figure 2.5: McCrary test



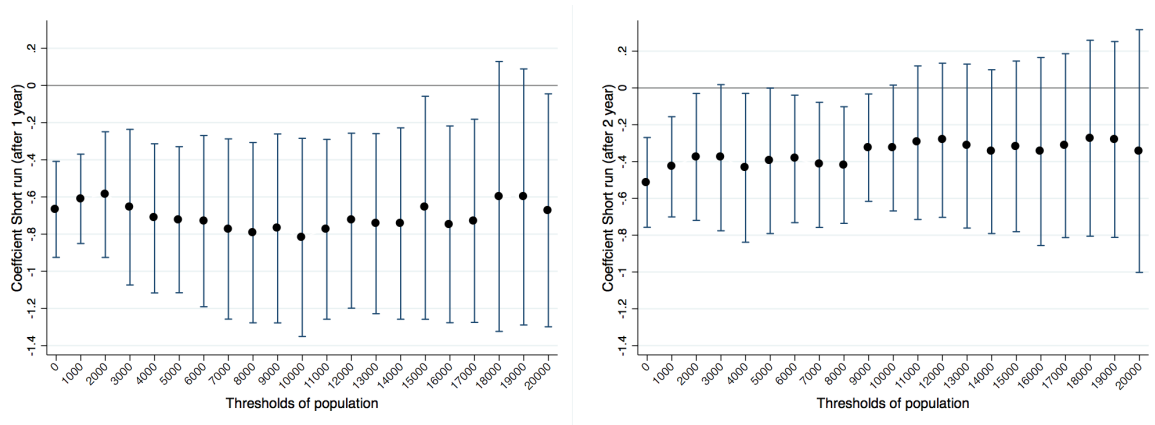
Notes: The plot contains outputs of the McCrary (2008) test on the density of political scandals around the election day. The bandwidth is set in 360 days, with day zero being the election day. The test is performed considering dates of all municipal elections and on the set of all Italian provinces (left sub-figure) and region province alone (right sub-figure).

Figure 2.6: Event study analysis - Turnout



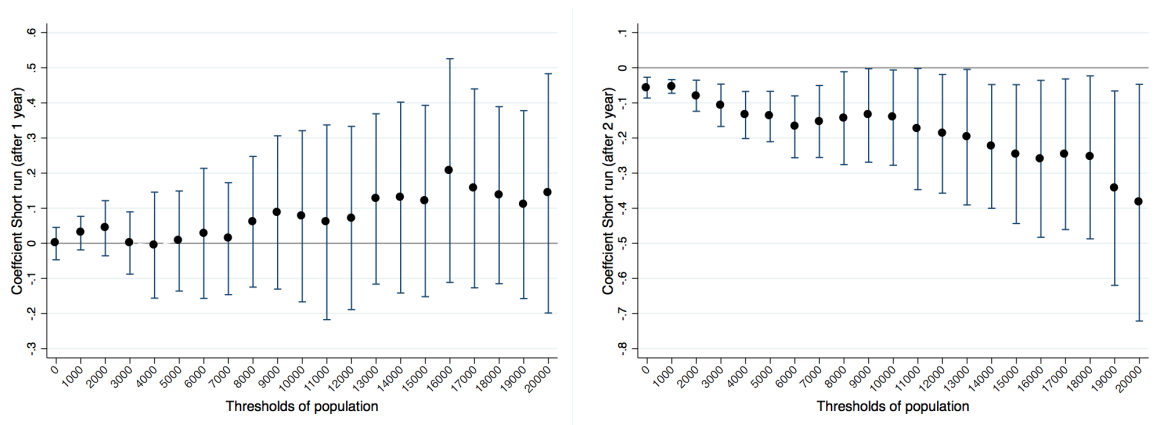
Notes: The figure plots coefficients from the event study conducted with monthly aggregation (Table 2.10). The specification include municipality and year fixed effect as well as province specific time trend, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates [political variables refers to past elections]. Each bracket represent a 95% confidence interval, and the centre of the bracket the point estimate. Confidence intervals are constructed with robust standard errors clustered at the province level. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals.

Figure 2.7: Effect with increasing population - Turnout

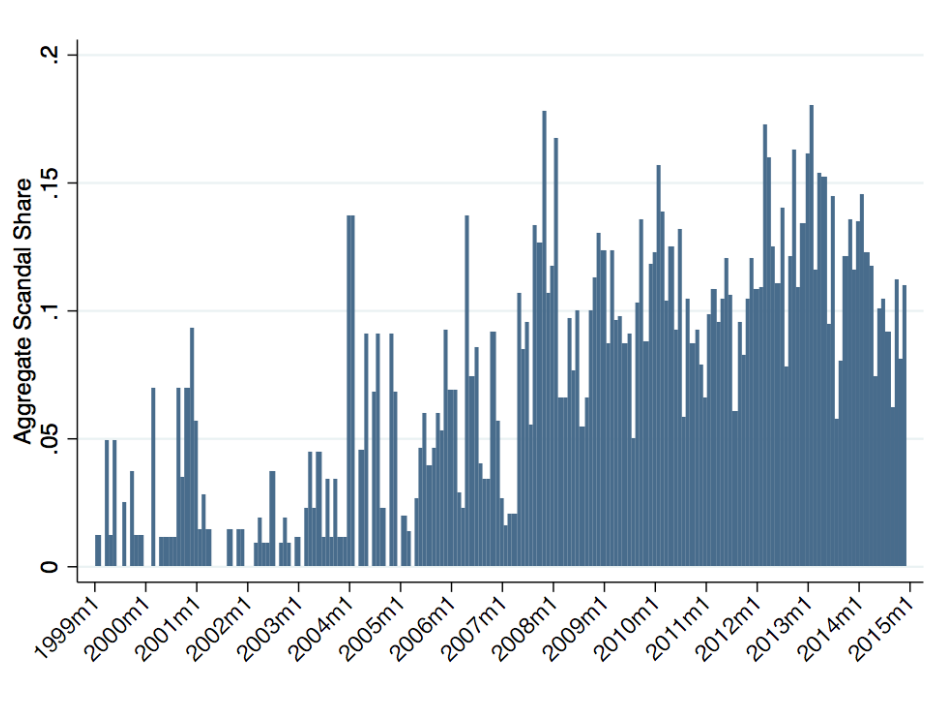


Notes: The figure plots coefficients of the regression $y_{i,t} = \alpha^S C_{j,i,t}^S + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t}$; with $y_{i,t}$ municipal turnout, $C_{j,i,t}^S$ captures the effect of corruption after one year "Short-run (after 1 year)" in left sub-graph, while it captures the effect after two years "Short-run (after 2 years)" in right sub-graph. The specification includes all fixed effects and controls as in the main analysis of section 4.1. The sample reduces gradually: it includes all cities with population larger or equal than the population threshold.

Figure 2.8: Effect with increasing population - Number of candidates



Notes: The figure plots coefficients of the regression $y_{i,t} = \alpha^S C_{j,i,t}^S + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t}$; with $y_{i,t}$ number of candidates, $C_{j,i,t}^S$ captures the effect of corruption after one year "Short-run (after 1 year)" in left sub-graph, while it captures the effect after two years "Short-run (after 2 years)" in right sub-graph. The specification includes all fixed effects and controls as in the main analysis of section 4.3. The sample reduces gradually: it includes all cities with population larger or equal than the population threshold.

Figure 2.9: Aggregate corruption exposure - Version with *political office* mention

Notes: The figure contains the value of the index "Corruption Region & Province cap. - Office", in per-thousand terms, aggregated over time and over all Italian local capitals, with monthly aggregation.

2.8 Tables

Table 2.1: Geography of scandals and characteristics of involved politicians.

	All administrative levels		
Perc. in region capital	74.6%		
Perc. in northern region	27.2%		
Perc. in central region	19.3%		
Perc. in southern region	53.5%		
	Region	Province	Municipality
Av. Age	58.0	58.8	53.7
Av. Education	4.7	4.6	4.3
Perc. of Women	10.7%	14.6%	13.4%
President/Mayor	35.9%	15.6%	16.4%
Alderman	15.8%	23.0%	12.9%
Counsellor	40.6%	46.2%	59.8%
Other	7.7%	15.1%	10.8%

Note: In the second panel outcomes in percentage points are computed over the total number in the reference administrative level. Av. Age is expressed in years, Av. Education ranges from 1 (no education) to 6 (advanced master or PhD).

Table 2.2: Descriptive statistics of corruption exposure measures

	Mean	SD	Minimum	Maximum
Yearly				
Corruption Region cap.	.2691	.5437	0	5.300
Corruption Region cap. - Office	.0400	.0529	0	.357
Corruption Region & Province cap.	.2744	.5442	0	5.300
Corruption Region & Province cap. - Office	.0417	.0537	0	.359
Corruption left wing - Regional politicians	.0132	.0232	0	.157
Corruption right wing - Regional politicians	.0198	.0430	0	.414
Monthly				
Corruption Region cap.	.0238	.0797	0	2.098
Corruption Region cap. - Office	.0034	.0089	0	.262
Corruption Region & Province cap.	.0242	.0798	0	2.098
Corruption Region & Province cap. - Office	.0035	.0090	0	.262
Corruption left wing - Regional politicians	.0011	.0040	0	.087
Corruption right wing - Regional politicians	.0016	.0059	0	.087

Notes: "Corruption region cap." captures scandals of region capitals alone, "Corruption region & province cap." capture scandals of region and province capitals. "Corruption region cap. - Office" and "Corruption region & province cap. - Office" capture the sub-sample of article where political office title is mentioned. "Corruption left wing - Regional politicians" and "Corruption left wing - Regional politicians" captures scandals of, respectively, left-wing and right-wing politicians, only regional administrators are considered.

Table 2.3: Descriptive statistics of outcome variables

	Mean	Standard deviation	Minimum	Maximum
Municipal turnout	76.137	9.724	21.568	100
Number of candidates	2.743	1.313	1	19
Number of lists	3.521	3.634	1	46
Civic lists	.712	.356	0	1
Old candidates All ES (%)	.402	.332	0	1
Old candidates elected All ES (dummy)	.531	.499	0	1
Civic mayor	.679	.466	0	1
Victory margin	.395	.284	0	1
Age candidates (av.)	49.263	7.305	22	85
Gender candidates (female)	.101	.203	0	1
Age mayor	48.957	9.909	19	94
Gender mayor (female)	.102	.302	0	1
Education mayor	4.301	.725	1	6
Age elected (av.)	43.892	4.185	25	77
Gender elected (av. female)	.196	.121	0	1
Education elected (av.)	3.964	.364	2	6
Incumbent reruns All ES (dummy)	.560	.496	0	1
Incumbent elected All ES (dummy)	.474	.499	0	1

Notes: *Municipal turnout* is expressed in percentage points; *Civic lists* is measured as a share (over total number of lists); *Old candidates* is measured as a share (over total number of candidates). *Old candidates elected* is a dummy denoting if the past candidate is re-elected [*Last/All ES* denotes whether they run in last/all past elections]; *Civic mayor* is a dummy denoting if the mayor was supported only by civic lists; *Victory margin* is measured as a share (over the votes of the most voted candidate); *Gender candidates/mayor/elected* denotes proportion of candidates/mayor/elected that are female; *Education mayor/elected* are measured in years (from 1 to 6); *Incumbent reruns/elected* is a dummy denoting if the past incumbent reruns/is re-elected.

Table 2.4: Compact analysis on municipal turnout

Municipal turnout	
	(1)
Short-run effect (1 year)	-0.662 (0.0914)***
Short-run effect (2 years)	-0.469 (0.153)***
Log-run effect (3+ years)	-0.0748 (0.0824)
Pre-scandal effect	-0.279 (0.675)
<i>N</i>	22,282
adj. <i>R</i> ²	0.833
Municipality, Year FE	Yes
Province-specific time trend	Yes
Controls	Yes

Notes: The specification includes municipality and year fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates [political variables refers to past elections] and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variable definitions and descriptive statistics. Robust standard errors clustered at the region level are included. : * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Compact analysis on municipal turnout by ideology

	Municipal turnout		
	(1) Corruption all politicians	(2) Corruption Left-wing politicians	(3) Corruption Right-wing politicians
Short run effect (1 year)*Civic mayor	-5.576 (3.348)	-14.38 (8.414)	-9.105 (5.975)
Short run effect (1 year)*Left-wing mayor	-7.495 (3.340)**	-19.20 (7.359)**	-12.13 (5.938)*
Short run effect (1 year)*Right-wing mayor	-6.612 (3.285)*	-13.96 (8.192)	-12.33 (5.789)**
<i>N</i>	25,490	25,490	25,490
adj. <i>R</i> ²	0.828	0.829	0.829
Municipality, Year FE	Yes	Yes	Yes
Province-specific time trend	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: All specifications include municipality and year fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates [political variables refers to past elections] and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals, involving politicians elected in regional administration only: column (2) and (3) only include scandals involving, respectively, left-wing and right-wing politicians (indices used are, respectively, "Corruption left wing - Regional politicians" and "Corruption right wing - Regional politicians"). See section 2 for dependent variable definitions and descriptive statistics. Robust standard errors clustered at the region level are included. : * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Compact analysis on political selection

	Mayor's characteristics			Elected characteristics			Mayor's background	
	Age	Female	Education	Age (Av.)	Female Prop.	Education (Av.)	Old candidates elected (all ES)	Incumbent elected (all ES)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Short-run effect (1 year)	0.425 (0.246)	0.00150 (0.00733)	-0.0149 (0.0162)	0.103 (0.0777)	0.00132 (0.00143)	-0.00126 (0.00256)	0.0348 (0.00989)***	0.0270 (0.0130)*
Short-run effect (2 years)	0.0317 (0.384)	0.0116 (0.00424)**	0.0122 (0.0241)	0.0551 (0.0745)	0.00117 (0.00329)	-0.00339 (0.00674)	0.00905 (0.0169)	-0.00338 (0.0147)
Long-run effect (3+ years)	0.129 (0.0478)**	0.00160 (0.000977)	-0.00425 (0.00518)	0.0122 (0.0129)	-0.00149 (0.000484)***	-0.00123 (0.00177)	0.00644 (0.00189)***	0.00250 (0.00265)
Pre-scandal effect	1.058 (0.722)	-0.0124 (0.0120)	0.00949 (0.0214)	-0.0739 (0.147)	-0.00840 (0.00969)	0.00443 (0.00856)	-0.0181 (0.0389)	-0.0184 (0.0324)
<i>N</i>	22,096	22,282	21,459	22,154	22,282	22,136	22,282	22,282
adj. <i>R</i> ²	0.266	0.224	0.344	0.489	0.421	0.701	0.117	0.055
Municipality, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All specifications include municipality and year/macoregion fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level) and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variables definitions and descriptive statistics. Robust standard errors clustered at the region level are included. : * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Compact analysis on political supply

	Number of candidates	Number of lists	Old candidate runs (All ES)	Incumbent runs (All ES)
	(1)	(2)	(3)	(4)
Short-run effect (1 year)	-0.0279 (0.0173)	-0.0610 (0.0261)**	0.0298 (0.0101)***	0.0263 (0.0111)**
Short-run effect (2 years)	-0.0955 (0.0202)***	-0.139 (0.0318)***	0.0219 (0.0128)	0.0198 (0.0128)
Long-run effect (3+ years)	-0.0112 (0.00425)**	-0.0170 (0.00702)**	0.00760 (0.00188)***	0.0114 (0.00150)***
Pre-scandal effect	0.00897 (0.0337)	0.118 (0.0773)	0.000280 (0.0263)	-0.000834 (0.0298)
<i>N</i>	22,281	22,282	22,282	22,282
adj. <i>R</i> ²	0.535	0.885	0.265	0.274
Municipality, Year FE	Yes	Yes	Yes	Yes
Province-specific time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: All specifications include municipality and year/macoregion fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level) and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variables definitions and descriptive statistics. Robust standard errors clustered at the region level are included. : * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.10: Voter turnout - Heterogeneity analysis

Dep. var.: Municipal turnout	Main specification			Past Turnout			Margin of Victory			Macro-area			National Newspaper			Share of immigrants		
	Whole sample (1)	Below median (2)	Above median (3)	Below median (4)	Above median (5)	Centre-north (6)	Centre-south (7)	Below median (8)	Above median (9)	Below median (10)	Above median (11)							
Short-run effect (1 year)	-0.662 (0.0914)***	-0.869 (0.266)***	-0.517 (0.201)**	-0.613 (0.186)***	-0.728 (0.199)***	-0.514 (0.130)***	0.0815 (0.302)	-0.532 (1.081)	-0.660 (0.117)**	-0.111 (0.371)	-0.881 (0.308)**							
Short-run effect (2 years)	-0.469 (0.153)***	-0.220 (0.305)	-0.732 (0.197)***	-0.543 (0.284)*	-0.356 (0.278)	-0.377 (0.473)	0.147 (0.301)	1.067 (1.030)	-0.469 (0.262)*	0.744 (0.591)	-0.596 (0.308)*							
Long-run effect (3+ years)	-0.0748 (0.0824)	-0.0995 (0.187)	-0.0789 (0.0789)	-0.109 (0.140)	-0.0488 (0.0848)	0.0170 (0.0433)	0.0798 (0.133)	-0.0714 (0.419)	0.00525 (0.0727)	0.136 (0.230)	-0.0520 (0.0746)							
Pre-scandal effect	-0.279 (0.675)	-1.411 (1.407)	0.408 (0.400)	-0.542 (0.669)	-0.251 (1.280)	-0.0578 (0.348)	0.00487 (0.565)	-0.257 (1.541)	-0.0502 (0.989)	0.0395 (0.936)	0.671 (1.288)							
N	22,282	10,353	11,929	10,103	12,179	13,896	8,386	7,024	15,258	8,925	13,357							
adj. R ²	0.833	0.803	0.601	0.895	0.801	0.778	0.873	0.889	0.809	0.866	0.818							
Municipality, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							
Province-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							

Notes: All specifications include municipality and year fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-35, 66+, net number of firms (province level), number of candidates, dummy whether incumbent renurs, share of old candidates (political variables refers to past elections) and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variable definitions and descriptive statistics. Robust standard errors clustered at the region level are included. ; * p < 0.10, ** p < 0.05, *** p < 0.01

Figure 2.11: Number of candidates - Heterogeneity analysis

Dep. var.: Number of candidates	Main specification			Past Turnout			Margin of Victory			Macro-area			National Newspaper			Share of immigrants		
	Whole sample (1)	Below median (2)	Above median (3)	Below median (4)	Above median (5)	Centre-north (6)	Centre-south (7)	Below median (8)	Above median (9)	Below median (10)	Above median (11)							
Short-run effect (1 year)	-0.0279 (0.0173)	-0.00448 (0.0491)	-0.0216 (0.0168)	-0.0288 (0.0362)	-0.0356 (0.0285)	-0.0374 (0.0149)**	-0.138 (0.193)	-0.207 (0.0764)**	0.0443 (0.0310)	-0.0292 (0.151)	-0.0708 (0.0291)**							
Short-run effect (2 years)	-0.0955 (0.0202)***	-0.103 (0.0463)**	-0.0756 (0.0291)**	-0.0774 (0.0423)*	-0.0985 (0.0345)**	-0.0266 (0.0637)	0.0611 (0.163)	-0.156 (0.0882)*	0.0327 (0.0373)	-0.0745 (0.170)	-0.0380 (0.0437)							
Long-run effect (3+ years)	-0.0112 (0.00425)**	0.00426 (0.0197)	-0.00915 (0.00598)	-0.0118 (0.00892)	-0.0127 (0.00752)	-0.00604 (0.00156)***	-0.113 (0.0465)**	-0.0853 (0.0271)**	0.0397 (0.0117)**	-0.0247 (0.0379)	-0.00550 (0.00689)							
Pre-scandal effect	0.00897 (0.0337)	0.189 (0.141)	0.0147 (0.0733)	-0.104 (0.0563)*	0.0663 (0.0555)	0.0493 (0.0781)	-0.0373 (0.183)	-0.137 (0.148)	0.100 (0.0781)	0.000578 (0.201)	0.0373 (0.0680)							
N	22,281	10,353	11,928	10,103	12,178	13,895	8,386	7,024	15,272	8,925	13,356							
adj. R ²	0.535	0.580	0.543	0.504	0.549	0.547	0.515	0.522	0.463	0.507	0.539							
Municipality, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							
Province-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes							

Notes: All specifications include municipality and year/region fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-35, 66+, net number of firms (province level) and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variables definitions and descriptive statistics. Robust standard errors clustered at the region level are included. ; * p < 0.10, ** p < 0.05, *** p < 0.01

Table 2.8: Turnout - Robustness checks

Dep. var.: Municipal turnout	Main specification	Without leads	No region cap. province	Province variation	Index with monthly scale	Index with political office
	(1)	(2)	(3)	(4)	(5)	(6)
Short-run effect (1 year)	-0.662 (0.0914)***	-0.698 (0.216)***	-0.686 (0.0637)***	-0.688 (0.289)**	-0.520 (0.179)***	-9.254 (4.565)*
Short-run effect (2 years)	-0.469 (0.153)***	-0.511 (0.185)**	-0.528 (0.160)***	-0.178 (0.292)	-0.325 (0.178)*	-1.239 (5.843)
Log-run effect (3+ years)	-0.0748 (0.0824)	-0.0571 (0.0710)	-0.0544 (0.0765)	-0.0727 (0.0763)	-0.0562 (0.0787)	1.912 (1.055)*
Pre-scandal effect	-0.279 (0.675)		0.0259 (0.658)	-0.400 (0.355)	-0.347 (0.541)	0.324 (2.478)
<i>N</i>	22,282	26,132	17,265	1,313	22,189	15,125
adj. <i>R</i> ²	0.833	0.829	0.827	0.643	0.833	0.556
Municipality, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All specifications include municipality and year fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates [political variables refers to past elections] and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. See section 5 for description of difference specification and indices. Robust standard errors clustered at the region level are included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Number of candidates - Robustness checks

Dep. var.: Number of candidates	Main specification	Without leads	No region cap. province	Province variation	Index with monthly scale	Index with political office
	(1)	(2)	(3)	(4)	(5)	(6)
Short-run effect (1 year)	-0.0279 (0.0173)	-0.0251 (0.0196)	-0.0344 (0.0138)**	-12.52 (22.70)	-0.0166 (0.0188)	-0.0931 (0.307)
Short-run effect (2 years)	-0.0955 (0.0202)***	-0.0952 (0.0152)***	-0.0991 (0.0219)***	-36.92 (20.97)*	-0.0761 (0.0141)***	0.348 (0.456)
Log-run effect (3+ years)	-0.0112 (0.00425)**	-0.0117 (0.00320)***	-0.0153 (0.00604)**	-35.55 (14.09)**	-0.00891 (0.00354)**	-0.227 (0.119)*
Pre-scandal effect	0.00897 (0.0337)		0.0254 (0.0402)	-33.70 (26.22)	0.0117 (0.0193)	-0.0824 (0.590)
<i>N</i>	22,281	26,131	17,264	1,453	22,188	15,125
adj. <i>R</i> ²	0.535	0.535	0.510	0.533	0.921	0.556
Municipality, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All specifications include municipality and year/macoregion fixed effect as well as province specific time trend, population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level) and number of years of *commissariamento*. The sample includes all Italian municipalities that are not province or region capitals. See section 5 for description of difference specification and indices. Robust standard errors clustered at the region level are included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

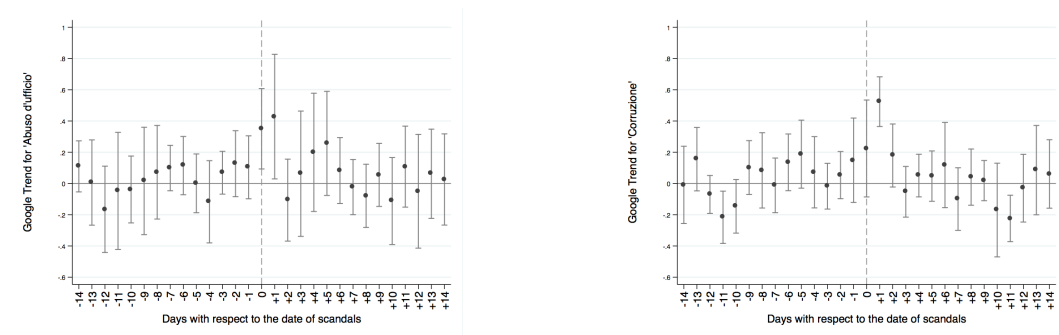
2.9 Appendix A1: Corruption exposure index validation

In this appendix we want to provide evidence that corruption index is a good measure in capturing people exposure to political corruption. In order to do this, we make use of Google trend data as a tool to explore people attention in a certain moment of time, and its evolution (Stephens-Davidowitz, Varian 2015). As already anticipated, we try to capture attention to corruption making use of three keywords, *i.e.* *corruzione* (corruption), *abuso d'ufficio* (malfeasance in office) and *concessione* (embezzlement). The goal of the analysis is to study whether there are peaks in searches of these keywords immediately after an article on local corruption appears in the newspaper. For the analysis, we can exploit the high-frequency nature of our index and we perform a daily analysis, with regional aggregation (the drawback of Google trend data, for Italy, is this very aggregate geographical variation). We run the following specification:

$$K_{r,d} = \sum_{p=0}^{+14} \alpha_p^{lag} C_{r,(d-p)} + \sum_{p=-14}^{-1} \alpha_p^{lead} C_{r,(d-p)} + \gamma_{r,y} + \zeta_w + \epsilon_{r,d} \quad (2.3)$$

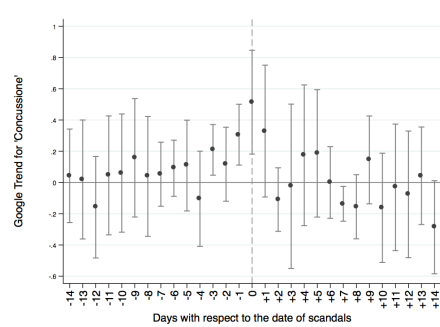
With $K_{r,d}$ Google trend index for selected keyword (either *corruzione* or *abuso d'ufficio* or *concessione*) in region r , in day d . $C_{r,(d-p)}$ main explanatory variable, representing the value of the index in region r , in day $d - p$: $C_{r,(d-1)}, \dots, C_{r,(d-14)}$ are lagged variables while $C_{r,(d+14)}, \dots, C_{r,(d+1)}$ are leads, to test whether attention to corruption was already raising before the article comes out. $\gamma_{r,y}$ is region/year fixed effect and ζ_w is day of the week fixed effect (to account for trends in attention to political topics within the week). Finally, robust standard errors are clustered at the region level.

Figure 2.12: Attention to corruption - Google trend data



(a) Google traffic for *Malfeasance in office*

(b) Google traffic for *Corruption*



(c) Google traffic for *Extortion*

We present the graphical analysis of this regression in Figure 10. We can note two things: first, all keywords generate an attention peak when the article appears in the newspaper, day zero, but this vanishes after two days: the attention spike turns out to be large but highly temporary. Second, lead 1 is positive and sometimes statistically significant, this can be explained by the fact that while newspaper in day d reports news of day $d - 1$, the web updates information in real time and this may explain the weak pre-trend; moreover, there can be anticipation of some news stimulating audience interest before the story is actually published in the newspaper. We can conclude that the index of corruption exposure seems to be a good proxy to measure the phenomenon and that it is evident that people pay consistent but highly temporary attention to corruption stories.

2.10 Appendix A2: Additional tables

Table 2.10: Turnout - Monthly analysis

	Municipal turnout
	(1)
Lag month 1	-9.982 (4.103)**
Lag month 2	-6.336 (4.569)
Lag month 3	-5.538 (4.645)
Lag month 4	-0.756 (1.716)
Lead month 1	5.918 (3.142)*
Lead month 2	-3.366 (3.700)
Lead month 3	4.770 (3.542)
Lead month 4	-1.701 (2.601)
<i>N</i>	26,192
adj. <i>R</i> ²	0.827
Municipality, Year FE	Yes
Province-spec. time trend	Yes
Controls	Yes

Notes: All specifications include municipality and year fixed effect as well as province specific time trend, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates [political variables refers to past elections]. The sample includes all Italian municipalities that are not province or region capitals. Corruption index captures scandals taking place in region and province capitals. See section 2 for dependent variable definitions and descriptive statistics. Robust standard errors clustered at the province level are included. : * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Chapter 3

The Role of Personal Interests in Tax Manipulation

3.1 Introduction

What motivates fiscal manipulation? The strategic decision of taxes has been deeply documented by Political and Public Economics literature in several different aspects and many studies provide evidence of this phenomenon at the national and local level. The main incentives which motivate fiscal manipulation are electoral: politicians may exploit fiscal instruments to maximize their probability to be re-elected. This can lead to the phenomenon of the political budget cycle, the reduction of taxes or the increase in public expenditures or transfers before elections, in order to gain political support. Many papers document the phenomenon at the national (Alesina et al. 1997, Persson, Tabellini 2002, Mink, De Haan 2006, Efthyvoulou 2012) and at the local level (Kneebone, McCKenzie 2001, Akhmedov, Zhuravskaya 2004, Drazen, Eslava 2008). Nevertheless, there can be other reasons, which can explain why administrators manipulate taxes. One of these reasons is personal interest. Despite the fact that there are many anecdotal evidences suggesting that politicians take into account their private and financial interests while taking fiscal decisions¹, there are very few papers that study empirically this topic. The absence of formal analysis on this phenomenon is mainly due to the difficulty to detect politicians personal interests, apart from few notorious cases, and to obtain causal estimates. The goal of this project is to contribute to fill this gap in the Political Economy literature, studying whether personal interests of politicians lead to fiscal manipulation. The objective is to shed light on another possible, and potentially important, source of strategic behaviour, which has remained rather unexplored so far.

In this paper we explore this aspect from an empirical point of view, with a focus on local politicians and we study the setting of Italian municipalities, which are administered by mayors. In particular, we focus on the local personal income tax, which is a surcharge to the national income tax in Italy (Bordignon et al. 2015). This tax is relevant for municipal balance sheets,

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

as it is the third local tax in terms of revenues, and it is a salient fiscal instrument for taxpayers. When the tax was introduced, in 1999, it was flat, and, starting from 2012, it became progressive, allowing mayors to introduce different rates for different income groups, tracing brackets of national income tax. This modification deeply changed the municipal surcharge, and it introduced the possibility, for local politicians, to tax differently distinct portions of population, with different income levels (Italian system is based on five tax brackets €0-15,000, €15,001-28,000, €28,001-55,000, €55,001-75,000 and more than €75,001). Moreover, the introduction of local progressivity has led to an increase in the strategic use of this tax with the emergence of large political budget cycles after 2012 (Giommoni 2018).

We want to study the local surcharge with the aim of exploring whether local administrators manipulate it in order to get personal advantages, *i.e.* in order to face lower rates on their personal wages and to pay lower taxes. Mayors, indeed, are required to pay the income tax, as well as the local surcharge, on their institutional wage. In order to explore this aspect, we exploit an institutional feature of the Italian system. Institutional wages of local administrators are based on population of the city and they sharply varies around several population thresholds² (Gagliarducci, Nannicini 2013). In this unique setting, we can exploit sharp increases in mayors' remuneration and see whether these are associated with manipulation of the surcharge: we want to study whether mayors whose income is placed in a certain tax bracket, tend to tax that bracket less than mayors, of comparable cities, whose incomes are located in the preceding tax bracket. In particular, we want to focus on one of these population thresholds, the 5,000 inhabitants threshold, which is particularly important. First, it is one of the three cutoffs where mayors' wage changes together with the tax bracket, as mayors' income brackets sharply shifts from the second to the third one. Second, it is the threshold where the sample size is large enough to conduct a statistical analysis that involves cities in both sides of the cutoff. The analysis consists in a sharp regression discontinuity design where we compare tax rate chosen for second and third income brackets between cities above and below the 5,000 inhabitants threshold.

One possible concern can be that mayors' income can include other earnings such as rents from financial assets or real estates, as well as wages from their "civic" occupation. There are, indeed, many anecdotal evidence that mayors maintain their civic job while in office³. These additional earnings may raise mayor's taxable income and it may end up in a higher tax bracket than the one assumed. This will probably attenuate our estimates but it won't be a concern for the identification strategy. A similar concern can be whether mayors are eligible for deductions, lowering their taxable incomes. In case of large deductions, it may be possible that treated mayors, those in cities above the 5,000 inhabitants threshold, reach the precedent income bracket. This can generate additional noise in our estimates, further attenuating the effect. In these regards, what we are capturing in this analysis is an intention to treat rather than a local average treatment effect. Also the RDD analysis could be viewed as fuzzy rather than a sharp one.

The main results of the analysis document the presence of consistent fiscal manipulation for personal reasons, in the setting of Italian municipalities. Mayors of cities above the 5,000 inhabitants threshold tend to introduce lower tax rates on third tax bracket compared to mayors of cities below the threshold and the effect is sizeable and statistically significant as it amounts to

²This is not the unique criterion affecting the income of mayors; this depends, indeed, on other factors such as budget performances of the municipality and occupational status of the mayor (whether she/he is a employee or a self-employed worker or whether she/he is in unpaid leave).

³Gagliarducci et al. (2010) focus on Italian members of Parliament and document consistent levels of moonlighting; they also underline a trade-off between the quality of elected officials and the effort exerted in political life, as good politicians tend to keep their outside jobs and are more likely to shrink once elected.

between 39% and 61% of the dependent variable average value, depending on the specification. Moreover, there are no significant differences in other positions of the tax structure between cities around the threshold. In terms of compliers, it turns out that cities that introduce more progressive tax schemes show higher levels of manipulation, with an average effect more than three times larger than the one found in the main specification. As a second result, we document the presence of consistent heterogeneous effects in this relation as manipulation is larger when mayors are young (under-40), male, self-employed workers or highly educated (holding at least a university degree). These outputs seem to suggest that manipulation is stronger when mayors are less likely to have additional incomes (young), can hide or cover part of their revenues (self-employed workers) or are more skilled, informed and aware about mechanisms of the fiscal system (educated). Moreover, manipulation is larger in cities that are located in the south of Italy, with higher fiscal revenues and public expenditures.

Nevertheless, if we translate this manipulation in monetary terms the amounts saved are rather limited, as they vary between €10 and €75 per year, depending on the specification. These small amounts hardly justify the manipulation that emerges from the data. Many explanations may clarify this aspect. First, it is possible that the mayor is not only favouring himself with a decrease in the third tax rate; rather, it is plausible that he wants to reward people in his inner circle, such as friends, relatives or colleagues, who are likely to be part of the same economic group and to pay taxes in the third bracket. With this interpretation the overall return from manipulation is much larger. Second, it is possible that the large measurement error, due to the omitted additional revenues, reduces the baseline effect obtained in the main analysis. Furthermore, the large heterogeneous effects suggest that the average effect is driven by a small group of compliers where the effect is quite consistent. Third, and finally, there may be behavioural reasons under this strategic behaviour: it is possible that the mayor simply wants to keep low his tax rate, but he does not compute the actual fiscal savings, *i.e.*, he may simply focus on the direction and not on the magnitude of the tax cut. Moreover, he may feel uncomfortable to raise his own tax rate, and he may be tempted to postpone, in the tax structure, the tax increase to avoid this self-punishment.

This project is related to several empirical papers in Political and Public Economics. First, there is a large set of studies exploring manipulation of public balance sheets for electoral purposes. These contributions focus on the practice of "political budget cycle" that leads to cyclical manipulation of taxes and public expenditures in order to please voters as elections approach. On the one hand, the literature documents the phenomenon from a cross-country perspective (Alesina et al. 1997) and describes how this practice is affected by electoral and political systems (Persson, Tabellini 2002). On the other hand, there is a large literature studying the issue at the local level. Many papers uncover consistent balance sheets manipulation in several countries involving public finance variables such as taxes (Kneebone, McCKenzie 2001, Alesina, Paradisi 2015), public expenditure (Drazen, Eslava 2008) and public transfers (Akhmedov, Zhuravskaya 2004). In addition, few papers inspect which institutional frameworks promote or weaken the practice (Rose 2006, Benito et al. 2013, Repetto 2016). This large literature underlines that politicians tend to use fiscal instruments to increase their probability of re-elections or to improve their future political career but it does not take into consideration other incentives that politicians may have to manipulate balance sheets. Our project contributes to this literature as it focuses on a new aspect: personal interests of politicians, which lead them to manipulate taxes in order to obtain better fiscal treatments. This is an important aspect to take into account that, however, remained rather unexplored, due to lack of data and proper setting to be studied.

A second literature related with this project is the one that focuses on wages of politicians. These papers explore how wages of elected mayors affects their performances in office and the process of political selections. The main results suggest that higher wages improve politicians

quality, measured with personal characteristics, such as education, previous profession or political experience and lead to better political performances (Ferraz, Finan 2009, Gagliarducci, Nannicini 2013, Fisman et al. 2015). Moreover, another group of studies focuses on moonlighting, the practice of carrying out further jobs in addition to the political mandate. These papers mainly study effects on performances and selection (Eggers, Hainmueller 2009, Gagliarducci et al. 2010, Geys, Maue 2012, Mocan, Altindag 2013, Campbell, Cowley 2015) and the main results suggest that this practice reduces performances of members of parliaments in office. Our project contributes to this literature as it underlines the impact of politicians wage on their incentive to manipulate taxes for personal interests.

Finally, this project contributes to the large literature on the conflict of interest of politicians: these studies focus mostly on members of national Parliaments and explore different aspects. First, there is convincing evidence about private returns from political office: many papers show that members of parliaments may collect consistent post-congressional earnings (Eggers and Hainmueller 2009, Diermeier, et al. 2005, Parker, Parker 2009) and that these extra-revenues raise with political relevance (Parker 1992). Second, there is evidence that firms connected with politicians face improvements in their operational and stock-market performances (Fisman 2001, Ferguson and Voth 2008, Goldman et al. 2009a, Niessen, Ruenzi 2010). Moreover, positive effects on firms have been demonstrated when business owners themselves take a political office (Bunkanwanicha, Wiwattanakantang 2009, Faccio 2006). Third, many contributions find that firms which are connected with politicians happen to have better access to credit (Khywaya, Mian 2005), higher probability to be bailed out by the government (Faccio et al. 2006), larger offering prices in the IPO market (Francis et al. 2009) and to experience an increase in procurement contracts (Goldman et al. 2009b). This project contributes to this literature in two aspects: first, it is one of the few papers in this literature with a local focus as it explores conflict of interest of city mayors. Second, we study a very specific setting where politicians face a conflict of interest in deciding their own tax rate and we can trace fiscal decision given politicians exposure to the tax.

The paper is structured as follows: section 2 discusses institutional settings and the data. Section 3 presents the identification strategy. Section 4 shows the main results and discusses heterogeneous effects and robustness checks and section 5 concludes.

3.2 Institutional setting and data

We focus on the local surcharge to the Italian income tax, the IRPEF (*Imposta sul reddito delle persone fisiche*). This tax is direct and personal with, as taxable income, the sum of all gross incomes of an individual and it is approved yearly by the municipal council (*Consiglio comunale*), upon mayor's proposal. After 2011, the surcharge was reformed and it was introduced the possibility to set it progressively: the mayor could set different tax rates, in the range 0-0.8%, to the different IRPEF wage brackets, as well as introducing an exemption threshold, below which the rate is zero. The surcharge is the third source of local revenues, after the real estate and the waste tax, and its importance grew quickly from its introduction in 1999. Moreover, the tax is a salient fiscal instrument for taxpayers and it is considered a particularly unpleasant tax in Italian municipalities (Giommoni 2018). Table 3.1 shows average rates for the five income brackets: column 2 considers the whole sample of cities, while columns 3 and 4 focus, respectively, on cities that have a surcharge higher than zero and on cities that introduced flexible rates, *i.e.* a multiple rates structure or an exemption threshold. We can note that the tax structure is always progressive as the marginal rate raises as income increases and that progressivity is higher for sub-samples with a positive surcharge and with a flexible rate. Data under analysis cover local surcharge for the time span 2012-2017. We have information on the tax rates associated to each

income bracket as well as exemption thresholds, when included, for all Italian municipalities. Furthermore, we have data on the number of tax-payers and the aggregate amount of income declared in each wage bracket for our time span.

Moreover, we focus on institutional wages of mayors. These depends on population as expressed by Law Bassanini, n. 265 03/08/1999, and, as already discussed in the introduction, they sharply change in correspondence of specific population thresholds, according to the most recent national census. Table 3.2 contains different population groups that determine mayors' wage and the number of cities for each group as well as the associated tax bracket. For cities with less than 5,000 inhabitants, mayors' wage is located in the second bracket, while, in cities with more than 5,000 inhabitants it raises and mayor's income bracket sharply shifts from the second to the third one. Another shift takes place after 100,001 inhabitants, where mayor's wage is located in the fourth bracket and, lastly, after 500,001 inhabitants, the last bracket is reached. In this analysis we are focusing on the 5,000 threshold that is the only one where the sample size is large enough to conduct a statistical analysis. We have data on 2011 census, the most recent one, determining mayors' income in the time span in analysis. Finally we have data on mayors and councillors characteristics (age, gender and education), on local politics (last elections turnout and vote share of most voted candidate) on local public finance (fees, taxes and special tariffs per capita as well as current and capital expenditures per capita) and on cities demographics (logarithm of population, share of immigrants and share of population in age groups 0-14/15-25/26-65/more than 66).

3.3 Identification strategy

In this project we want to explore whether local politicians tend to tax less wage brackets where their own income is located. We could not simply study whether mayors with higher wages tend to tax less rich people than mayors with lower wages, comparing the two groups with OLS, because this could generate biased estimates, due to endogeneity issues: it could be, for instance, that mayor's wage is correlated with local income distribution and this is likely to affect incentives to tax middle and high incomes. Furthermore, remuneration of mayors can be related with city size which could affect the use of the surcharge and the tax structure as well. In order to avoid these issues of endogeneity, we exploit the peculiar Italian setting and we make use of population thresholds that determine institutional wages of mayors and that induce variations in the tax bracket where the mayors pay their own taxes. In particular, we rely for this analysis on the 5,000 inhabitants threshold where mayors' wage jumps from € 26,040 (yearly gross) to € 33,480 (yearly gross) and this rise induces an exogenous shift of mayor's wage from the second to the third income bracket. We set the identification strategy as a sharp regression discontinuity design (RDD) where the forcing variable is city population, the treatment is mayor's income (and associated tax bracket) and the dependent variable is the local difference in tax rates between second and third brackets. The main identifying assumption in this model is that covariates are continuous around the population threshold and in correspondence of the shift from the second and the third bracket. In other words, it is required that cities characteristics vary smoothly around the population cutoff and along the income distribution with particular attention to the passage from the second to the third bracket which is the point in the income distribution that we are studying in this analysis.

Many possible threats to this identification strategy can arise. First, as discussed by Eggers et al. (2015), one pitfall in the use of population-threshold RDD can be that the same threshold is used to determine multiple policies, and this case is not an exception. Indeed, other two policies move sharply around 5,000 inhabitants threshold: on the one hand, wage of executive officers sharply changes from an amount equal to 15% of mayor's wage to 45% of it. We don't think

this can affect fiscal policy decisions as executive officers are not directly involved in decision process of the surcharge, but just in local government of the city. Moreover, it is unlikely that this variation around the population cutoff have an effect on policies which is differential between the second and the third income bracket. On the other hand, balanced-budget rule (partially) changes at this threshold: in particular, Italian stability pact imposing a gradual reduction in municipal deficit applies only to municipalities above 5,000 inhabitants. Nevertheless, this restriction only holds for 2012, as the policy changed from 2013 on and the threshold has been decreased to 1,000 inhabitants. In order to avoid a confounding effect of the stability pact, we exclude the year 2012 from the sample in the empirical analysis. A second threat can be the risk of manipulation around the threshold since cities can self-select in order to get better policies, as pointed out in the European context by Eggers et al. (2015). Nevertheless, this should not be a problem in our case as mayors' wage depends on population data from latest census, taking place in 2011, before the surcharge became progressive. Thus, it is implausible that mayors tried to manipulate figures to get better treatment before the policy took place. Furthermore, we run standard tests for continuity of covariates around the threshold of 5,000 inhabitants across different wage brackets to provide evidence that all relevant factors, but mayors' wage bracket, vary smoothly around the cutoff. Finally, mayors may have additional sources of earning, such as their "civic" occupation or financial and real estate rents, and these may raise their taxable income locating them in a tax bracket that is higher than the one assumed. This will probably attenuate our estimates but it won't be a concern for the identification strategy. A similar concern can be whether mayors are eligible for deductions, lowering their taxable incomes. In case of large deductions, it may be possible that treated mayors, those in cities above the 5,000 inhabitants threshold, reach the precedent income bracket. This can generate additional noise in our estimates, further attenuating the effect.

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

$$y_{it} = \alpha + \beta_1 \hat{Pop}_i + \beta_2 Wage_{it} + \beta_3 \hat{Pop}_i * Wage_{it} + \delta_t + \lambda_r + \zeta X_{it} + \epsilon_{it} \quad (3.1)$$

With y_{it} , the dependent variable capturing the difference in surcharge rates from second to third brackets, $\tau_3 - \tau_2$, in municipality i in year t . \hat{Pop}_i is the forcing variable, $\hat{Pop}_i = Pop_i - 5,000$, and captures the distance between the population in municipality i , according to the last census (2011), and the 5,000 inhabitants threshold. $Wage_{it}$ is the treatment dummy, equal to 1 if the mayor of i ends up into the third income bracket, *i.e.* $P_i \geq 5,000$, and zero otherwise. δ_t are year fixed effects and λ_r are macro-region fixed effects. Finally, X_{it} includes mayors and councillors characteristics (age, gender and education), which are important factors to control for in this setting as there is evidence that mayors and candidates characteristics modify around the cutoff: Gagliarducci, Nannicini (2013) show that, after 5,000 population threshold, mayors and candidates are more educated and more likely to be white collar⁴. Moreover, X_{it} includes controls on local politics (last elections turnout and vote share of most voted candidate), to control for relative power of mayor in the city council. Fixed effects and controls are only included in some specifications and standard errors are always robust and clustered at the municipal level. The coefficient we are interested in is $\hat{\beta}_2$ that represents the effect of the treatment and identifies the

⁴Despite Gagliarducci, Nannicini (2013) show that mayors' characteristics are not continuous around the 5,000 inhabitants threshold, this result is not a problem for our analysis. Indeed, since the dependent variable is the difference between τ_3 and τ_2 we are cleaning for city-specific factors that do not vary along the tax scheme. Thus, this discontinuity may be a concern only if it has a differential effect along the tax structure, which is not likely to happen. Moreover, Giommoni (2018) shows that the education of the mayor affects the propensity to introduce more progressive scheme, and this evidence goes against the main result of the analysis that cities above the threshold have flatter tax schemes (between τ_3 and τ_2).

local average treatment effect. The implementation of the RDD-LLR model is limited to the sub-sample of municipalities in the interval $Pop_{it} \in [h, h]$, with symmetric optimal bandwidths, according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b).

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

$$y_{it} = \sum_{k=0}^p (\beta_k \hat{Pop}_i^k) + Wage_i \sum_{k=0}^p (\gamma_k \hat{Pop}_i^k) + \delta_t + \lambda_r + \zeta X_{it} + \epsilon_{it} \quad (3.2)$$

With \hat{Pop}_i^k , $Wage_i$, δ_t , λ_r and X_{it} defined as in the previous model and with γ_0 capturing the jump between the two regression functions at the cutoff of 5,000 inhabitants. Thus, the local average treatment effect is consistently estimated by $\hat{\gamma}_0$ and we show these results to multiple orders of the polynomial (namely, $p=1$, $p=2$ and $p=3$). Furthermore, standard errors are robust and clustered at the municipal level.

Finally, in order to provide a placebo, we study the effect of the treatment on the gaps in other tax brackets. In particular, we run again the model (3.1) with a different set of dependent variables capturing the rate change from second to first bracket ($\tau_2 - \tau_1$), from fourth to third bracket ($\tau_4 - \tau_3$) and from fifth to fourth bracket ($\tau_5 - \tau_4$). This test guarantees that the effect is local in the tax structure and it only involves the wage groups including mayors' incomes.

3.4 Results

Table 3.3 contains the main results from the analysis of the model (3.1). The table is organized in two panels, focusing on different sub-samples. Panel A considers municipalities with a positive level of surcharge rate, obtained excluding municipalities that do not use the surcharge as a source of revenue. As already discussed, the dependent variable of the analysis captures the rate difference between second and third income brackets, "Second rate gap" ($\tau_3 - \tau_2$), shown in column 2. Moreover, we include all other tax rate differences as placebo: "First rate gap" captures rate difference between first and second brackets ($\tau_2 - \tau_1$), "Third rate gap" between third and fourth brackets ($\tau_4 - \tau_3$) and "Fourth rate gap" between fourth and fifth brackets ($\tau_5 - \tau_4$), respectively column (1), (3) and (4). Presented estimates include the set of controls and fixed effects discussed in the previous section and optimal bandwidths are symmetric and computed according to Calonico et al. (2014a), Calonico, Cattaneo and Farrell (2018), and Calonico, Cattaneo, Farrell and Titiunik (2018). Two main results emerge from the table. On the one hand, it seems that tax rate difference between second and third brackets is significantly lower in cities where the mayor is subject to the third bracket tax rate, compared to similar cities where the mayor is located in the second bracket. In other words, third bracket tax rate raises less, compare to second bracket rate, in cities that are above the cutoff, where mayors' income ends up in the third bracket. The average effect on rate difference is not negligible as it varies between 39% and 61% of the dependent variable average value. On the other hand, it does not emerge any discontinuity in the other tax rate differences around the cutoff: focusing on columns (1), (3) and (4) we can never reject the null hypothesis that average rate gaps are equal around the threshold. Moreover, as tax rates are continuous in every other position of the tax structure, it is implausible that the result is due to some other factors affecting local finance. In Panel B of Table 1.3, we repeat the same analysis with the sample of cities that set a flexible rate, *i.e.* which introduce a multiple rates structure. These cities clearly constitute

the set of compliers as tax rates of single brackets are different. Similar results emerge from the table, moreover the marginal effect on the second rate gap is more than three times larger than in Panel A. These results suggest that municipalities with higher use of flexible rates are those where manipulation for personal interests actually takes place. Moreover, despite the sizeable reduction in the sample, these estimates are more precisely estimated than those of Panel A⁵. These results are shown graphically in Figure 3.1: it emerges clearly the negative discontinuity in rate difference from second to third tax bracket (top-right plot) as well as the continuity in all other rate differences. These results suggest that mayors that pay their own taxes into the third income bracket tend to keep the associated tax rate lower than those mayors that are subject to the second bracket tax rate: these outputs uncover a new form of fiscal manipulation motivated by personal interests.

[Figure 3.1 here]

A related issue to is whether the drop in the second rate difference, for cities above the cutoff, is due to an increase in the tax rate of the second bracket or to a decrease in the third bracket tax rate: only the second case is coherent with the story of manipulation. In order to shed light on this point, we provide a test in Table 3.10, placed in the Appendix. We repeat the analysis of model 3.1, using as dependent variables the single rates: τ_1 , τ_2 , τ_3 and τ_4 . The main goal of this test is to check whether the main result obtained in Table 3.10 implies a reduction in τ_3 or an increase in τ_2 for cities above the 5,000 cutoff. The set of controls and fixed effects is the same as the main specification plus the inclusion of a variable capturing the average surcharge rate in the previous year, to control for the overall fiscal pressure of the city. A caveat for this additional exercise is that the identifying assumption is less conservative: differently from the main specification, indeed, where we use as dependent variable the delta between τ_3 and τ_2 , we are now comparing the absolute tax rate of the third bracket across the population cutoff. And this makes easier violations of the identifying assumption. From Table 3.10, it emerges that the tax rate of the third income bracket is significantly lower for cities above the cutoff, supporting the idea that mayors assign a lower rate to the third bracket once they pay their own taxes in that bracket.

3.4.1 Heterogeneity analysis

Given the fact that mayors actual tax bracket may be different from the one we assume in the analysis, due to additional earning as well as deductions, it is possible that the group of compliers is a sub-sample of the entire set included into the main analysis. Within this sub-sample, indeed, mayors below the 5,000 cutoff pay taxes in the second bracket and those above the cutoff are located in the third bracket. This aspect makes particularly important analysing heterogeneous effects in order to clarify which is the group of mayors reacting to these incentives. We performed two types of heterogeneity analysis, with respect to mayors' and city characteristics. Table 3.4 shows the analysis considering characteristics of the mayor, Panel A shows results without covariates and Panel B those with the inclusion of covariates. As a first test we study the relation dividing between mayors with low and high education level, respectively with and without a

⁵Table 3.9 in the Appendix, shows the same outcomes computed without including controls and fixed effects: similar results emerge from the table, with slightly lower coefficients. Furthermore, Table 3.11 in the Appendix contains the analysis done with the method of the spline polynomial approximation: Panel A and B show the analysis considering the sample of cities that use the surcharge as a source of revenue and Panel C and D focus on cities that introduce a differentiated rate structure, for both versions outputs with and without controls are presented. From the table the main result is confirmed, although sometimes the coefficient is not statistically significant.

university degree. These outputs are shown in columns (1) and (2) of Table 3.4: it emerges that mayors with high education manipulate more the surcharge, compared with those with low education, and the effect is larger than the one in the main specification and statistically significant. This result suggests that being informed and aware about the tax stimulate the manipulation as well as that more skilled mayors are more proficient in exploiting fiscal tools to their own advantage. One possible concern derives from the fact that education level is not balanced around the cutoff, as Gagliarducci, Nannicini (2013) show that above the 5,000 inhabitants threshold mayors are significantly more educated. This does not represent a problem for our identification as it is implausible that education level is correlated with rate jump from second to third bracket, as already discussed in the identification strategy session. If anything, we expect that more educated politicians are more aware about social benefit of redistributive taxation and tend to set steeper tax structures with larger jumps between income brackets: this can represent an attenuation bias in this case.

As a second analysis we study whether the effect changes with the age of mayors and we divide the sample between young and old administrators, respectively under-40 and over-40 years old. This characteristic may be correlated with the possibility of having additional earnings and young mayors are more likely to get their revenues primarily from the institutional wage. Thus, it is more likely that young mayors are compliers of the analysis. Results of this test are shown in columns (3) and (4) of Table 3.4 and confirm this conjectures as the effect is larger for young mayors as well as stronger than those found in the main analysis. Thirdly, we focus on gender of the mayor. There is a large literature suggesting that public misbehaviours such as corruption, bribery and embezzlement are less likely to be committed by females, compared to men. These studies, mostly relying on experimental evidence, acknowledge several possible channels such as risk aversion and preference for reciprocation (see Serra, Wantchekon 2012 for a survey of the literature). I want to study whether differences across genders appear also in this setting and I divide between female and male mayors: columns (5) and (6) of Table 3.4 contain results of this analysis and confirm the classic view as the effect is only significant for male mayors and it is absent for female administrators. Finally, we study whether the type of job of mayors affects his strategic behaviour. It is, indeed, plausible to think that self-employed workers, who directly pay their taxes, are more informed and aware about fiscal mechanisms and more capable to manipulate tax declaration, differently from dependent workers, whose employers are in charge of paying taxes. Moreover, self-employed workers have more chances to hide or cover part of their revenues, and to declare lower incomes; this makes them more likely to be compliers. In columns (7) and (8) of Table 3.4 we split the sample between employees and self-employed mayors and main results confirm this idea as the effect is absent for the former group and it is very large and precise for latter one.

The second block of heterogeneous analysis focuses on whether the manipulation varies with characteristics of the city. On the one hand, we focus on different effects across geography and we divide the sample between Centre-north and South of Italy. This is an important geographical division under many aspects such as economic development, local public finance, civic spirit and social capital (Harari, Tabellini 2009). Results of this test are included in columns (1) and (2) of Table 3.5, , Panel A shows results without covariates and Panel B those with the inclusion of covariates. It emerges that the effect is entirely driven by the southern regions of the country as the coefficient is very large and precisely estimated. On the other hand we focus on local budget variables. Columns (3-8) of Table 3.5 split the sample of cities in terms of fiscal revenues, current expenditures and capital expenditures, all in per-capita terms, and perform the main analysis for cities that are below and above variables median. What emerges is that manipulation is larger for cities that have fiscal revenues, current and capital expenditures above the median level. This suggests that mayors of richer cities are more likely to manipulate taxes for personal interests as

they are probably less fiscally constrained.

3.4.2 Robustness checks

In this section we provide a number of robustness checks for main results of the analysis. As a first test we run the main specification of model 3.1 changing the bandwidth. Table 3.6 contains the output of this check: columns (1) includes the main results, obtained with the optimal bandwidth and columns (2-5) include the outputs with the bandwidth, respectively, reduced by the 10%, increased by the 10%, reduced by the 20% and increased by the 20% (Panel A includes municipalities with a positive level of surcharge rate and Panel B the sample of cities that set a flexible rate). Outputs of the table confirm main results and the effect is almost always negative and significant. Secondly, we test the assumption in the regression discontinuity design methodology requiring that covariates vary smoothly around the cutoff. In this case, we need to check whether there are discontinuities in observables characteristics varying around 5,000 threshold between second and third income bracket of the tax scheme. We perform this analysis in Table 3.7 where we focus on four main variables: gross income declared (Panel A), gross income declared per-capita (Panel B), number of tax-payers (Panel C) and number of tax-payers per-capita (Panel D). From the table it is evident that these factors vary smoothly across income brackets as coefficients are never significantly different between municipalities below/above the population threshold in analysis. This result further confirms that the effect is due to the jump in mayors' income and it is not driven by any other city-specific factors that may have affected city management. The story of manipulation seems supported by this test. Finally, in order to show that the drop in the tax rate between second and third rate is actually due to the change in mayors' income bracket, we repeat the same exercise using different, fake, population thresholds, where income of mayors raises but the income bracket does not modify. This placebo allows us to figure out whether the effect depends solely on the fact that mayors' income raises or, instead, whether this result is due to the shift to an higher income bracket. In particular, we run the main analysis using two different population cutoffs: 3,000 and 10,000 inhabitants. Around these thresholds the income of the mayors raises but it does not move to a higher income bracket: around 3,000 threshold the yearly income raises from €17,400 to €26,040 and around 10,000 threshold it raises from €33,480 to €37,200. Table 3.8 shows results of these placebos: columns (1) and (2) study the evolution of the second rate gap around 3,000 cutoff and columns (3) and (4) focus on 10,000 cutoff, we include both specifications with and without controls. From the table it does not emerge any discontinuity in the second rate gap, in correspondence of both population thresholds. This result seems to suggest that the treatment that really matters is the modification in the income bracket of mayors instead of the mere increase in their salary.

3.5 Concluding remarks

The aim of this project is to uncover a new form of fiscal manipulation motivated by personal interests. There are many anecdotal evidence showing that politicians take their private and financial interests in consideration when deciding policies but few papers document this phenomenon with a causal approach. In this contribution, we exploit the specific Italian setting and we focus on the local personal income tax, a progressive local fiscal instrument, decided annually by each mayor. We make use of sharp discontinuities in mayors remuneration allocating them in different wage brackets. And we study whether mayors whose income ends up in a certain bracket tend to tax that bracket less than mayors, of comparable cities, whose income turns out to be in the preceding tax bracket. We find evidence of consistent manipulation motivated by personal interests: third bracket tax rate results to be consistently lower in cities above the

threshold than in those below, and the average effect is sizeable as it amounts between 39% and 61% of the dependent variable mean. Moreover, the effect is stronger for cities that happen to have mayors that are young (under-40), male, self-employed workers and highly educated and in cities located in the south of Italy and richer in terms of public finance.

Nevertheless, one important caveat of this analysis is the fact that mayors may have additional revenues, such as labour incomes, financial or real estate rents, that we cannot observe. These extra-revenues can move them to another, higher, tax bracket; this could lead some mayors, in principle allocated in the control group, to end up in the treatment one. On the contrary, some mayors in the treatment group could be eligible for deductions lowering their taxable income and placing them in the control group. These potentially large measurement error can consistently bias down our estimates but it is not a concern for the identification strategy.

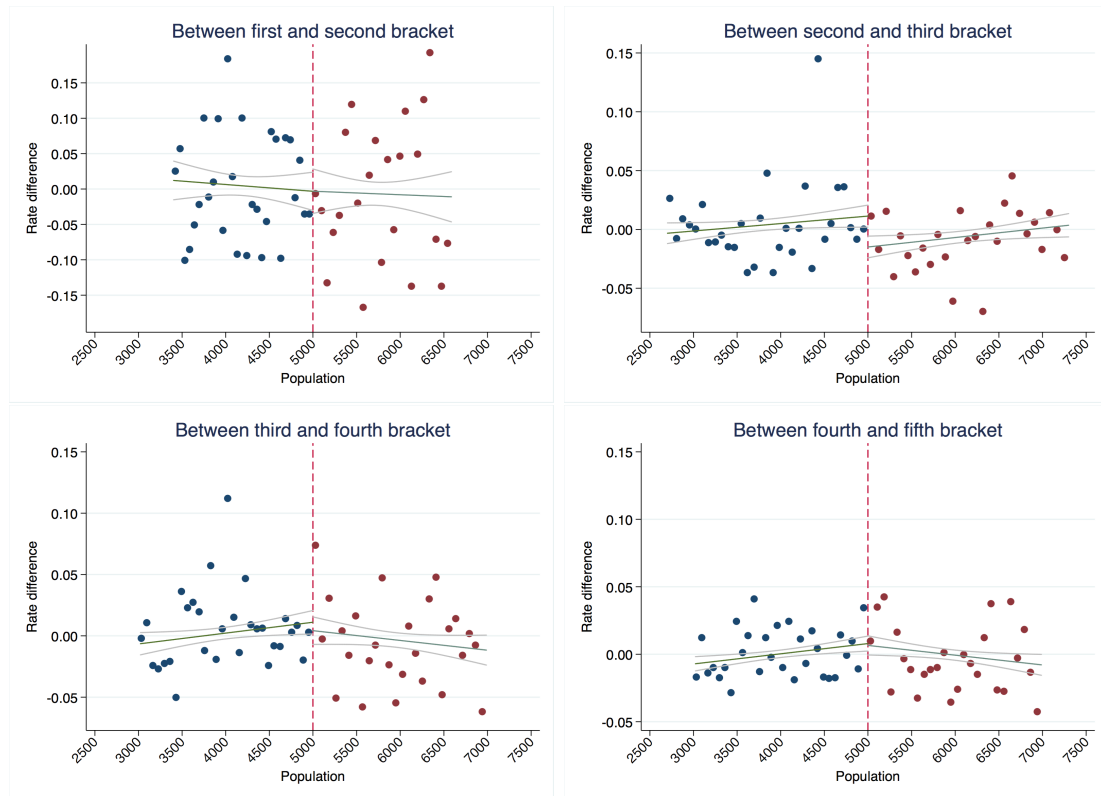
On the other hand, another potential weakness of the analysis is in terms of the magnitude of these estimates. The magnitude is large in terms of the dependent variable average value, but it is limited if we translate the coefficient in monetary terms: average amount saved because of fiscal manipulation by mayors that are above the threshold varies between €10 and €75 per year, depending on the specification⁶. These amounts are limited and may not fully justify the manipulation that emerges from the data. There may be several possible explanations to justify this aspect. First, it is possible that the mayor is not only favouring himself with a decrease of the third tax rate; rather, it is plausible that he also wants to reward people in his inner circle, such as friends, relatives or colleagues, who are likely to be part of the same economic group and to pay taxes in the third bracket. With this interpretation the overall return from manipulation is much larger. Second, it is possible that the baseline effect obtained in the main analysis is artificially low because of the large measurement error, due to omitted revenues. Furthermore, the large heterogeneous effects suggest that the average effect is driven by a small group of compliers where the effect is quite consistent. Third, and finally, there may be behavioural reasons under this strategic behaviour: it is possible that the mayor simply wants to keep low his tax rate, but he does not compute the fiscal savings, *i.e.*, he may simply focus on the direction and not on the magnitude of the tax cut. Moreover, he may feel unconformable to raise his own tax rate, and he may be tempted to postpone, in the tax structure, the tax increase to avoid this self-punishment. Nevertheless, these explanations are plausible but, at the same time, very difficult to test in our setting.

This paper is aimed at exploring the practice of fiscal manipulation, in particular we uncover and document a new form of manipulation motivated by private and personal interests. These results suggest that electoral incentives are not the unique source of fiscal manipulation and that personal interests may also induce politicians to act strategically leading to large distortions and inefficiencies in fiscal policies.

⁶These figures are computed assuming that mayors declare their income in the third income bracket with the application of coefficients that emerged in the analysis.

3.6 Figures

Figure 3.1: Rate differences around 5,000 inhabitants threshold



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

3.7 Tables

Table 3.1: Surcharge rate for each wage bracket

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

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

Table 3.2: Mayors' income and tax brackets

Population	Number of cities (in 2011)	Monthly income of mayor	Yearly income of mayor	Tax bracket of mayor's income
Up to 1,000	2,152	€ 1,290	€ 15,480	II bracket [15,001-28,000]
Between 1,001-3,000	2,525	€ 1,450	€ 17,400	II bracket [15,001-28,000]
Between 3,001-5,000	1,111	€ 2,170	€ 26,040	II bracket [15,001-28,000]
Between 5,001-10,000	1,152	€ 2,790	€ 33,480	III bracket [28,001-55,000]
Between 10,001-30,000	858	€ 3,100	€ 37,200	III bracket [28,001-55,000]
Between 30,001-50,000	161	€ 3,460	€ 41,520	III bracket [28,001-55,000]
Between 50,001-100,000	89	€ 4,130	€ 49,560	III bracket [28,001-55,000]
Between 100,001-250,000	32	€ 5,010	€ 60,120	IV bracket [55,001-75,000]
Between 250,001-500,000	6	€ 5,780	€ 69,360	IV bracket [55,001-75,000]
Over 500,001	6	€ 7,800	€ 93,600	V bracket [over 75,000]

Notes: Amounts are expressed in Euros according to *Law Bassanini, n. 265 03/08/1999* and are gross of taxes.

Table 3.3: RDD analysis on rate differences-with Controls

Panel A: Cities with positive surcharge	(1)	(2)	(3)	(4)
	First rate gap	Second rate gap	Third rate gap	Fourth rate gap
	$\tau_2 - \tau_1$	$\tau_3 - \tau_2$	$\tau_4 - \tau_3$	$\tau_5 - \tau_4$
RD_Estimate	-0.0383 (0.0249)	-0.00978 (0.00514)*	-0.0000416 (0.00560)	0.000469 (0.00433)
Outcome variable mean	.144	.016	.012	.008
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	1,600.4	2,293.5	1,892.8	1,825.9

Panel B: Cities with flexible rates				
RD_Estimate	-0.0326 (0.0465)	-0.0307 (0.0149)**	0.00914 (0.0183)	0.00860 (0.0140)
Outcome variable mean	.298	.077	.062	.043
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	1,600.4	2,293.5	1,892.8	1,825.9

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

Table 3.4: RDD analysis on diff3-Heterogeneity mayors characteristics

Panel A: Cities with flexible rates	Dep. variable: Second rate gap							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low education	High education	Under-40	Over-40	Male	Female	Employees workers	Self-employed workers
RD_Estimate	0.00960 (0.0415)	-0.0267 (0.0146)*	-0.0474 (0.0194)**	-0.0159 (0.0181)	-0.0259 (0.0160)	-0.0168 (0.0316)	-0.0186 (0.0194)	-0.0450 (0.0200)**
Outcome variable mean	.077	.077	.077	.077	.077	.077	.077	.077
Covariates	No	No	No	No	No	No	No	No
Bandwidth (h)	1,981.4	2,332.6	1,480.7	1,941.8	2,447.3	1,984.3	2,309.2	2,347.5

Panel B: Cities with flexible rates								
RD_Estimate	0.0125 (0.0240)	-0.0325 (0.0153)**	-0.0431 (0.0197)**	-0.0248 (0.0192)	-0.0303 (0.0168)*	-0.0278 (0.0209)	-0.0275 (0.0189)	-0.0538 (0.0174)***
Outcome variable mean	.077	.077	.077	.077	.077	.077	.077	.077
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (h)	1,981.4	2,332.6	1,480.7	1,941.8	2,447.3	1,984.3	2,309.2	2,347.5

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

Table 3.5: RDD analysis on diff3-Heterogeneity city characteristics

Panel A: Cities with flexible rates	Dep. variable: second rate gap							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	North of Italy	South of Italy	Fiscal rev. (below median)	Fiscal rev. (above median)	Current Expend. (below median)	Current Expend. (above median)	Capital Expend. (below median)	Capital Expend. (above median)
RD_Estimate	-0.0129 (0.0150)	-0.0835 (0.0568)	-0.0202 (0.0178)	-0.0300 (0.0144)**	-0.0222 (0.0197)	-0.0229 (0.0158)	-0.0138 (0.0167)	-0.0395 (0.0141)***
Outcome variable mean	.077	.077	.077	.077	.077	.077	.077	.077
Covariates	No	No	No	No	No	No	No	No
Bandwidth (h)	1,857.8	1,599.2	2,254.9	2,492.7	1,914.1	1,894.8	2,203.3	2,471.2

Panel B: Cities with flexible rates								
RD_Estimate	-0.0155 (0.0163)	-0.149 (0.0520)***	-0.0295 (0.0191)	-0.0284 (0.0143)**	-0.0304 (0.0206)	-0.0264 (0.0157)*	-0.0150 (0.0173)	-0.0471 (0.0143)***
Outcome variable mean	.077	.077	.077	.077	.077	.077	.077	.077
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (h)	1,857.8	1,599.2	2,254.9	2,492.7	1,914.1	1,894.8	2,203.3	2,471.2

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

Table 3.6: RDD analysis on diff3-sensitivity of the Bandwidth

Panel A: Cities with positive surcharge	Dep. variable: Second rate gap				
	(1) Main specification (h)	(2) $h - h \cdot 10\%$	(3) $h + h \cdot 10\%$	(4) $h - h \cdot 20\%$	(5) $h + h \cdot 20\%$
RD_Estimate	-0.00978 (0.00514)*	-0.00975 (0.00537)*	-0.00912 (0.00493)*	-0.00919 (0.00562)	-0.00867 (0.00474)*
Outcome variable mean	.077	.077	.077	.077	.077
Covariates	Yes	Yes	Yes	Yes	Yes
Bandwidth (h)	2,293.5	2,064.15	2,522.85	1,834.8	2,752.2
Panel B: Cities with flexible rates					
RD_Estimate	-0.0307 (0.0149)**	-0.0300 (0.0154)*	-0.0287 (0.0144)**	-0.0266 (0.0159)*	-0.0273 (0.0139)*
Outcome variable mean	.077	.077	.077	.077	.077
Covariates	Yes	Yes	Yes	Yes	Yes
Bandwidth (h)	2,293.5	2,064.15	2,522.85	1,834.8	2,752.2

Notes: Estimation by RDD-LLR. The analysis covers the period 2013-2017. Panel A includes all cities that introduce the municipal surcharge and Panel B only includes cities that introduce flexible rates. The set of controls includes mayors and councillors characteristics (age, gender, education) and political controls (turnout in last elections and vote share of the most voted candidate) as well as year and macro-region fixed effects. Symmetric bandwidths (column 1) are computed according to Calonico et al. 2014, Calonico et al. (2018a), and Calonico et al. (2018b). Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Continuity of covariates-with Controls (only cities with flexible rate)

Panel A: Gross income declared	(1)	(2)	(3)	(4)
	First rate gap $\tau_2 - \tau_1$	Second rate gap $\tau_3 - \tau_2$	Third rate gap $\tau_4 - \tau_3$	Fourth rate gap $\tau_5 - \tau_4$
RD_Estimate	176652.5 (727343.5)	1170753.0 (1176875.6)	-822078.6 (943287.0)	-79220.8 (190731.3)
Outcome variable mean	1.46e+07	3358706	-2.93e+07	335513.8
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	2,224.8	1,850.1	1,801.0	2,179.6
Panel B: Gross income declared (per-capita)				
RD_Estimate	47.32 (139.5)	174.6 (213.9)	-145.7 (165.5)	-19.34 (36.26)
Outcome variable mean	1,802.1	-437.3	-3,387.4	-27.6
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	1,365.7	1,897.5	2,031.9	1,528.7
Panel C: Frequency of tax-payers				
RD_Estimate	47.03 (82.01)	35.17 (37.63)	-19.82 (27.87)	-0.417 (3.044)
Outcome variable mean	-709.2	-525.2	-902.7	-28.1
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	2,276.9	2,095.1	1,990.3	2,333.2
Panel D: Frequency of tax-payers (per-capita)				
RD_Estimate	0.00680 (0.0138)	0.00750 (0.00883)	-0.0101 (0.00660)	-0.000433 (0.000524)
Outcome variable mean	-.13	-.10	-.11	-.003
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	1,720.1	1,499.3	1,238.2	1,917.1

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

Table 3.8: Placebo RDD analysis on diff3-Fake thresholds

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

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

3.8 Appendix: additional tables

Table 3.9: RDD analysis on rate differences-without Controls

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

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

Table 3.10: RDD analysis on single rate-with Controls

Panel A: Cities with positive surcharge	(1)	(2)	(3)	(4)
	First rate	Second rate	Third rate	Fourth rate
	τ_1	τ_2	τ_3	τ_4
RD_Estimate	0.0243 (0.0194)	0.0000683 (0.00854)	-0.00672 (0.00788)	-0.00284 (0.00879)
Outcome variable mean	.53	.58	.59	.60
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	2,097.4	1,663.4	1,665.3	1,703.8
Panel B: Cities with flexible rates				
RD_Estimate	0.00992 (0.0346)	-0.0209 (0.0229)	-0.0392 (0.0211)*	-0.0163 (0.0157)
Outcome variable mean	.41	.55	.62	.67
Covariates	Yes	Yes	Yes	Yes
Bandwidth (h)	2,097.4	1,663.4	1,665.3	1,703.8

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

Table 3.11: Spline polynomial analysis on second rate gap

Panel A: Cities with positive surcharge	Dep. variable: Second rate gap		
	(1) Spline 1 st	(2) Spline 2 nd	(3) Spline 3 rd
T	-0.00992 (0.00325) ^{***}	-0.00787 (0.00503)	-0.00869 (0.00643)
Outcome variable mean	.016	.016	.016
Covariates	No	No	No
Panel B: Cities with positive surcharge			
T	-0.00992 (0.00325) ^{***}	-0.00787 (0.00503)	-0.00869 (0.00643)
Outcome variable mean	.016	.016	.016
Covariates	Yes	Yes	Yes
Panel C: Cities with flexible rates			
T	-0.0185 (0.00989) [*]	-0.0250 (0.0145) [*]	-0.0251 (0.0177)
Outcome variable mean	.077	.077	.077
Covariates	No	No	No
Panel D: Cities with flexible rates			
T	-0.0185 (0.00989) [*]	-0.0250 (0.0145) [*]	-0.0251 (0.0177)
Outcome variable mean	.077	.077	.077
Covariates	Yes	Yes	Yes

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

Appendix: data sources

Table 3.12: Data sources

	Description	Data source
Municipal surcharge	Information on tax rates, exemption thresholds	<i>Italian Ministry of Internal Affairs</i>
Municipal balance sheets	Data on fiscal revenues, current and capital expenditure	<i>Italian Ministry of Internal Affairs</i>
Income distribution	Data on income distribution and declared income	<i>Italian Ministry of Economics and Finance (Italian Ministry of Internal Affairs)</i>
Local politicians	Demographics (age, gender, education) and profession of mayors and councillors	<i>Anagrafe degli amministratori locali regionali</i>
Local elections	Date, turnout and candidates' vote share	<i>Italian Ministry of Internal Affairs</i>
Population		<i>ISTAT (Italian statistical office)</i>

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