Switch towards tax centralization in Italy: a wake up for the local political budget cycle

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Abstract

Local tax on owner-occupied dwelling in Italy has been replaced by a vertical transfer in 2008. To identify the effect of the reform on policy outcomes we divide the municipalities into two groups: those that held one election before the reform and one election in the year after the reform, and municipalities that held one or two elections before the reform. We show that municipalities that in 2008 were in their pre-electoral year increase the size of their budget, whereas the political cycle is not present in municipalities that experienced their pre-electoral year before the reform.

Keywords: Political budget cycle, Inter-governmental transfers, Local property tax, Local elections, Centralization versus decentralization

JEL classification:C23, H71, H72

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1 Introduction

Taxes on housing properties are often object of a heated political debate. In Italy, at the closing of the electoral campaign for the 2006 parliamentary elections, the candidate for Prime Minister of the right-wing coalition, Silvio Berlusconi, announced that, in case of victory, his government would have abolished the local tax (Imposta Comunale sugli Immobili, ICI) on owner-occupied housing properties.[1]

Thanks to this unexpected announcement, that bought the vote of many home-owners for the right-wing candidate, the forecasted vote margin, in favor of the left-wing candidate, Romano Prodi, throughout the electoral campaign, considerably reduced. Nonetheless, the left-wing coalition won the elections, albeit for a close margin. As a result, the government headed by Romano Prodi, supported by a narrow majority in the Parliament, had to resign in 2008 and immediately afterward new general elections were held. This time, the coalition headed by Silvio Berlusconi won the elections and formed a new government on May 8, 2008. On May 27, the Prime Minister honored his 2006 electoral promise, by exempting taxpayers from the payment of the local property tax levied on owner-occupied dwellings.

From the perspective of Municipal public finances, the main feature of the 2008 local fiscal reform is that it abolished the property tax on owner-occupied dwellings – one of the main sources of revenues for Italian municipalities, bearing high political costs as it directly links the local decision maker to her voters – by substituting it with a compensating transfer from the central government – that, contrary to own tax revenues, bears no political costs for the local decision maker. The impact on the incentives for municipal spending and taxes of this sharp change in the structure of municipal revenues is the primary focus of this work, with particular reference to the strategic incentives to manipulate policy decisions close to elections, as evidenced by the well-known literature on political budget cycles. The classical theoretical framework on political budget cycles is due to Rogoff

[1] According to Corriere della Sera – one of the leading Italian newspaper – the property tax is considered as the most "hated" tax by Italian taxpayers (Corriere della Sera, May 22, 2007).

and Sibert (1988) and Rogoff (1990) who show that, when voters are rational but imperfectly informed about the complexities of the government budget, the incumbent leader has an incentive to bias the pre-election fiscal policy. In these papers, it is assumed that each political candidate has a competence level (high or low), which is only known to the politician and not to the electorate. Before the election, the high-type incumbent will signal his type (and thereby increase her chances of reelection) by engaging in an expansionary fiscal policy (Rogoff and Sibert, 1988), or in a switch from investment expenditure to a more visible consumption spending (Rogoff, 1990). Both actions are less "costly" for the high type incumbent than for the low type, leading to a budget cycle (a pre-election increase in government deficit) when a competent politician is in office. A large literature developed from these works, documenting and seeking to explain whether the electoral budget cycles exist. However, most studies are based on cross-country samples of central government budgets.[2] In fact, few works focus on the local government level, because data at the local level are available for shorter time periods than national data, or because all local elections occur at the same time, which does not allow to identify the election year effect for a specific government layer (Sjahrir et al., 2013). Evidence of local political budget cycles is found by Kneebon and McKenzie (2001), who use data on Canadian provinces over the period 1966-1997, finding that more visible expenditure — as Education, Transportation and Communication, and Recreation and Culture — increases in election years versus non-election years. The same findings are found by Drazen and Eslava (2010), who, relying on data on Colombian municipalities, show that, prior to elections, infrastructure spending, which is considered more attractive to voters, expands significantly. Akhmedov and Zhuravska (2004), by using a

[2] Among others, Alesina et al. (1997), by using a sample of 13 OECD countries for the period 1960-1993, find the presence of the political budget cycle only in the aggregate balance, while, when they split the budget into different components, they do not find any significant results. Persson and Tabellini (2000) investigate whether the budget cycles are driven by the system of government, finding the cycle only for revenues and only in the presidential systems. Other works have shown that budget cycles occur only in certain countries. In particular, Shi and Svensson (2006), using a panel of 123 countries over the period 1975-1995, show that budget cycles exist only in developing countries and Brender and Drazen (2008), using a sample of 106 countries for the years 1960-2001, find the presence of the political budget cycles only in new democracies.

Russian provinces monthly panel data for the period 1998-2003, find significant political cycles in budget spending and its composition. Khemani (2004) considers 14 major states of India over the period 1960-1992 and shows that in election years tax collection from specific producer groups is lower and public investment spending is higher. Finally, a quasi-experimental strategy has been recently exploited by Alesina and Paradisi (2014) in order to test the budget cycle. They use a cross-section of Italian municipalities for the year 2012, at the end of which all municipalities were imposed to deliberate on the new real estate tax rate (IMU) —both on owner-occupied dwellings and on other dwellings, testing the impact on the tax rate deliberation for those municipalities having elections scheduled in 2013. They find evidence of the political budget cycle, since municipalities with elections scheduled in 2013 set lower tax rates for owner-occupied dwellings than those not having elections. Interestingly, they do not find any significant effect for tax rates on other dwellings. However, when they replicate the analysis for tax rates set in 2013, when only the tax on other dwellings was in place, they find that municipalities having elections scheduled in 2014 set significantly lower tax rates than those not having elections.

In our work we rely on a panel data of Italian municipalities and we exploit the exogenous change in their financial system — the replacement of the property tax on owner-occupied dwellings with a compensating vertical transfer, to identify whether this policy shift affected the incentives for strategic manipulation of tax and spending decisions of municipalities close to elections, thereby triggering a kind of policy behavior which is typical of the political budget cycle framework. To identify the effect of the reform, we exploit the staggered structure of the electoral years of Italian municipal elections. In particular, we divide the municipalities, observed in a specific time period (2002-2008), into two groups: (i) those that in that period held one election before the reform and one election in the year after the reform, implying that one pre-electoral year falls before the reform and the other in the year of the reform, and (ii) municipalities that held one or two elections both before the reform, implying that all pre-electoral years fall before the reform. We then compare decisions on expenditures and revenues for the two groups of municipalities. While, before the reform, the policy outcome

decisions in the pre-electoral year should be similar for both groups of municipalities —as the financing system is the same for both groups, after the reform the change in the municipal financing system may show up in different policy outcome decisions for the two groups. The difference between the two groups of municipalities, indeed, is that, for one group the pre-electoral year falls always before the reform —implying a high political cost for the local policy maker as she must tax citizens on owner-occupied dwellings, while, for the other group of municipalities, one pre-electoral year falls in the year of the reform —implying no political cost for the local policy maker, as she must not tax citizens anymore on owner-occupied dwellings given that, for the same amount of revenue, she now receives a compensating transfers from the central government. Hence, we expect that the incentives to strategically manipulate decisions on expenditure and taxes should be more pronounced for those municipalities that are in the pre-electoral year coinciding with the year of the reform. In fact, our results show that municipalities with the pre-electoral year in 2008 increase their expenditure by 3% with respect to the average value of municipal expenditures. Moreover, we find that municipalities with the pre-electoral year in 2008 increase revenues from fees and charges by 10% with respect to the average value, suggesting that the reform prompted incentives to strategically manipulate policy outcome decisions when municipalities are close to elections (political budget cycle), resulting not only in an increase in expenditure, but also in the recourse to a less transparent revenue source such as charges and fees (Bracco et al., 2013).

The rest of the work is structured as follows. Section 2 discusses the fiscal policy reform and provides some institutional information on the finance of Italian municipalities. The identification strategy is illustrated in Section 3. The dataset and some preliminary evidence are presented in Section 4. The empirical analysis, the results and the robustness checks are in Section 5. Section 6 concludes.

2 Institutional framework

Municipalities in Italy are responsible for a large array of important public programs in the field of welfare services, territorial development, local transport,

infant school education, sports and cultural facilities, local police services, as well as infrastructural spending. As regards their share of the general government budget, municipalities account on average for about 8% of total public expenditure during the period 2002-2008, which is the time span we use in the empirical analysis.

On the revenue side, municipalities can rely on transfers from upper levels of government (mainly central and regional governments) and, as a result of a lengthy process of fiscal devolution, they rely on own taxes.

The main local tax revenue is given by the property tax, ICI (*Imposta comunale sugli immobili*, now renamed IMU, *Imposta municipale unica*), introduced in 1992 and applied to real estates. This tax is paid every year by property owners directly to the municipality where the property is located. In particular, the ICI tax base is the cadastral income, which does not vary over time (occasionally, cadastral values are increased by the same proportion, so they do not change in relative terms), and the tax is levied differently on owner-occupied dwellings (the dwellings where owners have their residence) and on other dwellings (rented properties, secondary properties used for holidays, and so on): tax rates are lower on the former, and tax credits are allowed only for the former.

Other important tax revenue sources for municipalities are the tax or tariff on urban waste disposal (*Tarsu*, now renamed *Tari*), and a surtax on the personal income levied by the central government (*Addizionale comunale Irpef*). Additional own revenues can be raised by Italian municipalities through user fees, which are linked to the municipal provision of various services such as parking permits, occupation of public areas, and use of billboards.

The Decree no. 93 of 27 May 2008 abolished the property tax levied on owner-occupied dwellings. For public finances of municipalities the resulting loss of tax yield was partially compensated by a transfer from the central government, thus changing the structure of local finance towards a more centralized system. Hence, from 2008 each municipality received a transfer whose amount was determined by the amount of the lost tax yield, but corrected according to two criteria: a) efficiency in tax collection, measured by the ratio between the average value of

the revenue of the property tax levied on owner-occupied dwellings for the period 2004-2006, measured in cash terms, and the corresponding value measured in accrual terms; b) compliance with the fiscal rules imposed by the central government to each municipality (domestic stability pact) for the year 2007. Furthermore, special provisions applied to municipalities with a population lower than 5,000 inhabitants. Overall, the aggregate amount of compensating transfers received by Italian municipalities in 2008 was about 2.8 billion euro, while the revenue from the property tax on owner-occupied dwellings collected in 2007 was around 3.5 billion euro.

Clearly the fulfillment of the above criteria in determining the amount of compensating transfers, introduced in 2008, were based on decisions taken beforehand, and thus could not be affected by policy maker decisions taken in 2008. Hence, the received per capita transfer was, for the local policy maker, truly exogenous.

2.1 The Italian institutional thresholds

There are two dimensions that need to be carefully considered in order to assess our empirical analysis.

The first one relates to the choice of the time span, since the abolition of the property tax on owner-occupied dwellings is not the only institutional policy reform that took place in Italy during the last 15 years. For Regions ruled by ordinary statutes, starting from 2002, municipalities have been granted access to a fixed share of the personal income tax revenues generated in their territory (with a corresponding reduction in central transfers). Furthermore, in May 2009 it was approved an important law (Law 42/2009), which opened the way to the introduction of "fiscal federalism" in Italy. Hence, from 2009 onwards, as a result of the fiscal federalism process, the local fiscal rules have been frequently changing from one year to another, including a) a set of small taxes devolved at the local level, such as the cadastral taxes on property sales and a fixed municipal share to the VAT (only for 2011), b) modifications of the equalization system and of the structure of vertical transfers from the central government, c) the introduction, in 2012 of a reformed property tax on owner-occupied dwelling (Imposta Municipale

Unica, IMU), with a tax base slightly different from that of ICI and with part of the revenue retained by the central government.

The second dimension regards the cross-section features of the dataset. In particular, the presence of different policy provisions at the municipal level based on population brackets (Gagliarducci and Nannicini, 2013). The salary of the mayor, of the members of the executive committee and of the councilors, the size of the council, the size of the executive committee, the electoral rule, whether or not a municipality can have additional elective bodies in every neighborhood and whether or not a municipality can host hospital facilities or organize a health-care district, are all policy assignments that vary with population size. Moreover, vertical transfers from the central government change proportionally with the size of population (Law 504/1992). Finally, municipalities below 5,000 inhabitants are exempted from a set of rules imposed by the national government to municipalities in order to improve their fiscal discipline (Domestic Stability Pact).

All these policies, based on population brackets, clearly affect fiscal policy decisions at the local level. Gagliarducci and Nannicini (2013) find that better-paid politicians lower per capita tariffs and reduce both current and investment expenditure; Grembi et al. (2016) find evidence that municipalities not constrained by the rules of the Domestic Stability Pact have lower tax revenues and larger fiscal gaps compared to constrained ones. There are also some recent works on the effect of the Italian municipal electoral system on fiscal policy decisions. Bracco and Brugnoli (2012) find that municipalities with runoff electoral systems that are politically aligned with the central government receive, ceteris paribus, more transfers than those that not aligned; Bordignon et al. (2016) find that municipalities just above 15,000 inhabitants (that rely on runoff elections) on average have a larger number of candidates and less volatile tax rates, compared to municipalities just below 15,000 inhabitants (that have single round elections). Ferraresi et al. (2015) show that taxes and expenditure in municipalities where the runoff electoral system holds are lower than those in municipalities with a single round elections, but only if the mayor of the former type of municipalities does not need a broad coalition to be elected.

We finally note that in Italy there are regions with special autonomy, which are

allowed to set their own fiscal rules for their municipal governments.

3 Identification strategy

As we illustrated in the previous section, there are several policies that change at different population thresholds, and between municipalities belonging to ordinary and special statutes, as well as other local structural reforms that took place in Italy in the last 15 years. The presence of these policies might confound the impact on local policy choices of the policy we are interested in, i.e., the replacement of the property tax on owner-occupied dwellings with a vertical transfer, with the result that its effect cannot be properly identified. Hence, we restrict our sample to municipalities belonging to regions ruled by ordinary status, with a population range between 3.000 and 5.000 inhabitants, over the period 2002-2008. Such restrictions assure that there are no other policy changes or structural reforms in the sample apart from the one we focus on.

We aim at estimating the causal effect of upcoming elections on policy outcome decisions of municipalities, by exploiting the following experiment. Imagine that we can observe over a given period, including two pre-electoral years, two municipalities, A and B, that are similar in their demographic, geographic and socio-economic characteristics. Now, suppose to flip a coin to decide the timing of elections and, say, that municipality A holds the election one year after the reform. The key point is that being in an electoral year is as good as randomly assigned, so that the random assignment of the timing of elections generates a random assignment in which municipality the election will be held the year after the reform. Such exogenous variations, in terms of the timing of elections, allows us to define a treated and a control group. In particular, municipality A, which holds one election before the reform and one election the year after the reform —implying that one pre-electoral year falls before the reform and the other pre-electoral year coincides with the year of the reform— is our treated municipality; while municipality B, which holds both elections before the reform —implying that both pre-electoral years fall before the reform— is the control municipality. In this way we can compare the policy outcome, in terms of revenue

and expenditure decisions, of municipality A (treated) with the policy outcome of municipality B (control) before the reform, namely in a period where both municipalities have the same incentives to manipulate the budget in their pre-electoral years, since they rely on the same set of tax instruments. Then, we compare the policy outcome of municipality A with that of municipality B after the reform, namely in a period where the pre-electoral strategic choice of policy outcome variables generated by the reform matters only for municipality A since its pre-electoral year coincides with the year of the reform and, the pre-electoral year of municipality B falls before the reform.

In the absence of the reform, the difference in the policy outcomes in the pre-electoral years of municipality A, before 2008 and in 2008, should be the same as the difference in policy outcomes in the pre-electoral years of municipality B, before 2008 and in 2008. In the former case, in fact, one pre-electoral year falls before 2008 and the other coincides with 2008, while, in the latter case both pre-electoral years fall before 2008, but in absence of the reform this difference in the election timing should not affect fiscal policy decisions, that is, there would be no difference in the differences in policy outcome during pre-electoral years, between treated and control groups. On the other hand, if the abolition of the property tax on owner-occupied dwellings, with the replacement of the lost revenue through a compensating transfer, changes the pre-electoral strategic choice of policy outcomes, we should observe a difference in local tax and spending decisions between municipality A and B, namely the difference in policy outcomes in the pre-electoral years of municipality A, before and in 2008, should be different from the same difference in the pre-electoral years of municipality B. Such difference in the differences represents a causal effect of the reform on the political budget cycles, which in turn affects policy outcome decisions.

4 Dataset and variables

4.1 Dataset

The empirical analysis is based on a dataset of Italian municipalities resulting from a combination of different archives publicly available from the Italian Ministry of the Interior, the Italian Ministry of the Economy and the Italian Statistical Office. It includes a full range of information for each Italian municipality organized into three sections: 1) financial data; 2) electoral data, covering the results of elections in which the mayors in office during the period covered by the dataset were elected; 3) demographic and socio-economic data, such as population size, age structure, average income of inhabitants. In order to avoid overlapping policies, as discussed in section 2.1, we restrict the sample to municipalities belonging to Regions ruled by ordinary status, for the period 2002-2008, with a range of population between 3.000 and 5.000 inhabitants according to 2001 Census population. Also we do not include municipalities with missing values from our dataset and municipalities put under commissioner or municipalities where the majors resigned before the term. With these restrictions, we obtain a sample of 733 municipalities including 5,131 observations from 2002 to 2008.[3]

4.2 Dependent variables

As our dependent variable on the expenditure side, we use the per capita current expenditure (*current expenditure*). On the revenue side, we use the per capita tax instruments that can be set by the local policy maker, like the property tax on other dwellings (*property tax on other dwellings)*, the surtax on the personal income tax (*surtax on personal income*), and users' fees and charges (*fees and*

[3]Over 8,442 (1,206 municipalities for 7 years) potential observations in the range between 3,000 and 5,000 inhabitants, our sample includes 5,131 observations. As a matter of fact, we exclude 1,456 (208 municipalities for 7 years) observations referring to municipalities in Special Statute Regions and Provinces, 630 (90 municipalities for 7 years) observations relative to municipalities put under commissioner, 182 (26 municipalities for 7 years) observations relative to municipalities where the major resigned before the term in the considered period, and 1043 (149 municipalities for 7 years) observations relative to municipalities/years where data are not complete or data are missing.

charges). The reason for using per capita revenues (and not tax rates) is threefold. First, a tax revenue financial variable is coherent and comparable with spending. Second, it would be very difficult to have homogeneous and comparable rates for all kind of revenues we consider (taxes and fees and charges). Third, revenues account for both tax rate effort and effort in tax evasion control, which are both important complementary components of the municipality's fiscal policy.

As a preliminary piece of evidence it is interesting to compare the change in mean of expenditure and revenue variables, before and in 2008, for municipalities when they are in their pre-electoral year, with the same change – before and in 2008 – for municipalities when they are not in their pre-electoral year (Table 1).

The difference in *current expenditure* (Table 1; Panel A), before and in 2008, for municipalities that are in their pre-electoral year is positive (115.20 euro) and statistically significant at 1% (col. 2; Panel A), implying that, on average, current expenditure in the pre-electoral year in 2008 is higher than current expenditure in the pre-electoral year before 2008. The difference in *current expenditure*, before and in 2008, for municipalities that are not in their pre-electoral year is 51.36 euro and statistically significant at 1% (col. 1; Panel A), suggesting that, on average, current expenditure in 2008 is higher than that before2008 also for municipalities that are not in their pre-electoral year. Notice, however, that the difference in *current expenditure* before and in 2008 for municipalities that are in their pre-electoral year (115.20 euro) is higher than the difference, before and in 2008, for municipalities that are not in their pre-electoral year (51.36), and the difference in these differences (63.84 = 115.20 – 51.36) is statistically significant at 1% (col. 3; Panel A).

The same picture emerges for revenues of both the *surtax on personal income* (Table 1; Panel C) and *fees and charges* (Table 1; Panel D). In particular, the difference in the revenue from *surtax on personal income*, before and in 2008, for municipalities that are in their pre-electoral year is positive (27.37 euro) and statistically significant at 1% (col. 2, Panel C), while the same difference for municipalities that are not in their pre-electoral year is 14.66 euro and statistically significant at 1% (col. 1, Panel. C). It is worth noting that the difference in the revenue from *surtax on personal income*, before and in 2008, for municipalities

that are in their pre-electoral year (27.37) is larger than the difference, before and in 2008, for municipalities that are not in their pre-electoral year (14.66), and the difference in these differences (12.71 = 27.37 – 14.66) is statistically significant at 1% (col. 3; Panel C). As it regards revenues from *fees and charges*, the difference, before and in 2008, for municipalities that are in their pre-electoral year is positive (14.19 euro) and statistically significant at 10% (col. 2; Panel D), while the same difference for municipalities that are not in their pre-electoral year is negative (-22.38 euro) and statistically significant at 1% (col. 1; Panel D). The difference in these differences is positive (36.57 = 14.19 + 22.38) and statistically significant at 5% (col. 3; Panel D). Finally, the difference in the revenue from *property tax on other dwelling*, before and in 2008, for municipalities that are in their pre-electoral year is not statistically significant (col. 2; Panel B) as well as the difference in the revenue from *property tax on other dwelling*, before and in 2008, for municipalities that are not in their pre-electoral year (col. 1; Panel B) is not statistically significant. Note also that the difference in these differences is not statistically significant (col. 3; Panel B).

What this simply suggests is that the reform seems to have led to a significant increase in current expenditure and in own revenues (surtax on personal income and fees and charges). Such an increase is more pronounced for those municipalities that are in their pre-electoral year compared to municipalities that are not in their pre-electoral year.

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4.3 Treated and control municipalities

Since 1993, the Italian municipal electoral rule prescribes that elections are held normally every 5 years during the period April-June. However, during years, the process of replacement of the mayor and of the municipal council has not followed the regular cycle in every municipality, leading to different timing of elections. This feature determines a random assignment of municipalities, over the period 2002-2008, into two groups: those with an election held before the

reform and one after it, and those with all elections held before the reform. In other words, since each municipality has its own electoral story, which generates different timing of elections, it is just by "chance" that one municipality has elections scheduled before or after the reform, during the period 2002-2008. This exogenous assignment can be used to define a treated and a control group for the 773 municipalities included in our dataset. Table 2 shows the timing and the frequency of elections. In particular there are 506 municipalities (69% of the total) that held elections in 2004, and given that elections are running every 5 years, these municipalities are also the same that had elections scheduled for 2009.[4] These municipalities represent our treated group since, given the timing of the elections, one pre-electoral year (2003) falls before the reform, while the other pre-electoral year (2008) falls in the year of the reform (Decree no. 93 of 27 May 2008), which was also the same year of the first switch from local taxation on owner-occupied dwellings to a compensating transfer.[5] On the other hand, for the remaining municipalities (227; 31% of the total) the pre-electoral year always falls in a period before the reform; hence these municipalities are the control group.

It is important to note that we do not consider in our dataset municipalities that have not held elections every 5 years;[6] that is, we excluded from the dataset municipalities that had elections scheduled after 2009, but anticipated them in 2009: if these municipalities were included, the treatment would not be exogenous to potential outcomes (Alesina and Paradisi, 2014)[7].

***** insert here TABLE 2 *****

[4]We checked whether all these 506 municipalities actually had the election in 2009.

[5]Details on the timing of the transfers in 2008 can be found at http://finanzalocale.interno.it/docum/studi/varie/soppressione_ici.html

[6]Once we have excluded municipalities put under commissioner and municipalities where data are not complete or data are missing, our dataset contains information on 759 municipalities observed for the period 2002-2008. However, we also excluded 26 municipalities which have not held elections every 5 years and, among these, three municipalities had elections scheduled after 2009, but anticipated them in 2009 because the mayor resigned before the term. Therefore, the final sample includes 733 municipalities that held elections every 5 years.

[7]The same reason is pointed out by Akhmedov and Zhuravskaya (2004) who argue that moving elections away from the originally scheduled date creates concerns about identification.

4.4 Socio-economic and demographic controls

We include a set of time-varying variables which characterize a municipality's demographic and economic situation. In relation to demographic controls we include the population of the municipality (*pop*), the population density (*density*) calculated as the number of citizens per municipal area (measured in square kilometers); these variables can capture the presence of scale economies in the provision of public goods. The proportion of citizens aged between 0 and 5 (*child*) and the proportion aged over 65 (*aged*) can account for some specific public needs (e.g., nursery schools, nursing homes for the elderly).

Regarding economic and financial controls we include the average per capita income of municipalities, proxied by the personal income tax base (*income*) and the per capita value of the transfers from the upper levels of government (*transfers*). Finally, we also set a dummy (*election*) equal to one for each election year during the period 2002-2008, allowing to capture the effect of having an election during the considered period. The summary statistics, data description and data sources of all the variables used in the analysis are reported in Appendix, Tables A1 and A2.

5 Empirical analysis

5.1 Econometric specification

Formally, our estimation approach is based on a difference-in-difference (DiD) framework and the baseline specification can be expressed as following:

$$\begin{aligned} Y_{it} &= \gamma_1 pre\,electoral\,year_{it} + \gamma_2 pre\,electoral\,year_{it} \times reform \\ &+ \beta' X_{it} + \alpha_i + \tau_t + \lambda Trend_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

where Y_{it} is one of the public policy outcomes we consider (i.e., per capita current expenditure, per capita revenue of property tax on other dwellings, per capita revenue of surtax on personal income and per capita revenue of fees and charges) for municipality i at time t; *pre electoral year* is a dummy variable equal to 1 in

the year before the election and 0 otherwise, *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise, when the property tax on owner-occupied dwellings has been abolished and replaced by a compensating vertical transfer; X_{it} is the vector of all the control variables described in section 4.4. To take account of unobserved heterogeneities across municipalities, we include a set of municipalities fixed effects, α_i, and we also control for exogenous shocks that can equally affect both treated and control groups by adding year fixed effects, τ_t. Moreover, $Trend_{it}$, reflects a complete set of municipality-specific time trends. A key identifying assumption of the DiD approach is that the temporal development of each municipality would have been the same in the absence of any treatment. Hence, by including the set of municipality specific time trends we control for any potential temporal pattern independent of the treatment status. Finally, ε_{it} is the error term, clustered at the municipal level.

In this framework, γ_1 accounts for the impact of upcoming elections on the policy outcome before the reform, while γ_2 captures the differential effect, on the policy outcome,with respect to γ_1, of being in a pre-electoral year in the 2008.

5.2 Results

For each outcome variables, we present our DiD estimates as in equation (1). As for the expenditure side of the budget, we find that the coefficient estimate of *pre electoral year* $\times$ *reform* is positive and statistically significant at 5% level (col. 1; Table 3). In terms of the size of the estimated effect, the results suggest that the *current expenditure* of municipalities in the pre-electoral year in 2008 is 19.04 euro higher, *ceteris paribus*, compared to what it would have been in the absence of the reform, and this amount corresponds to about a 3% increase with respect to the average value of expenditure (632.07 per capita euro).

Looking at the revenue side of the budget, we find that the coefficient of *pre electoral year* $\times$ *reform* is not statistically different from zero neither for the revenue from *property tax on other dwellings*, nor for the revenue from the *surtax on personal income* (col. 2 and 3; table 3); while it is positive and statistically significant at 5% level for the revenue from *fees and charges* (col. 4; table 3).

In particular, we find evidence that the revenue from *fees and charges* of municipalities in the pre-electoral year in 2008 is 17.75 euro higher, *ceteris paribus*, compared to what it would have been in the absence of the reform, which corresponds to an approximately 10% increase with respect to the average value of the revenue from *fees and charges* (176.69).

What this suggests is that substituting own municipal revenues with compensating transfers from the central government generates incentives for municipalities to increase both expenditure and revenue from charges and fees the year before elections, indicating the presence of a political budget cycle. The intuition of these results is simple. On the one hand, the political cost of increasing expenditure, after the reform, is lower, given that at least part of the increase in local expenditure is financed by the compensating transfer (which has no political cost for the local decision maker) replacing the property tax on owner-occupied dwellings, which was politically costly because easily related to the local decision maker (Dahlby, 2011). On the other hand, the abolition of a visible fiscal tool, as it was the property tax on owner-occupied dwellings, leads local governments to substitute it with the less visible available revenue source (fees and charges) the year before the election. Fees and charges are, indeed, much less visible to voters with respect to the other available tax instruments, because they are collected several times during the fiscal year and their amount is, generally, relatively small (Bracco et al., 2013).

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5.3 Robustness checks

In this section, we assess the validity of the previous results by performing a set of robustness tests.

Even though we have restricted our analysis to municipalities belonging to the range of 3,000 - 5,000 inhabitants to avoid the presence of other overlapping policies, a source of potential concerns is that the group of treated municipalities might differ in some characteristics with respect to the control group of municipalities, making thus our "random assignment" hypothesis of the treated status

weak. We address this issue by using the matching approach,[8] that consists to match treated and control groups using a set of observable characteristics. In particular, we use data from the 2001 Census and run a logit regression (details are available in the Appendix, Table A3) by using, as control variables, those variables that might affect both the treatment and outcome variable (Sianesi, 2004; Smith and Todd, 2005, which are: population (*population*), a categorical variable (*altimetry zone*) equal to 1 if the municipality is located in plain, equal to 2 if the municipality is located in hills, and equal to 3 if the municipality is located in mountains, the proportion of population over 65 years old (*aged*), the proportion of population less than 5 years old (*child*), the population density (*density*), the per capita income (*income*), the per capita grants from upper levels of government (*transfers*), the proportion of families (*families*), the per capita number of houses (*houses*), the per capita number of firms (*firms*), the unemployment rate (*unemployed*) and the average altitude level of the municipal territory (*altitude*). The fitted values of the previous logit regression let us define the propensity score variable, which is then used to find the common support including all observations lying within the percentiles range of the propensity score variable running from 0.5 to 95.5. Then we match the sample of treated to a comparable sample of non treated, linking each municipality only to its "nearest neighbor" propensity-score distance.[9] Such procedure reduces the sample to 667 municipalities and, within this sample, there are no significant differences, on the observable characteristics included, between the matched group of treated and control municipalities (details are available in the Appendix, Table A4). In addition, the distributions of the estimated propensity score for the treated group and the control group show overlapping (Figure 1), implying that for each treated municipality there is a control with similar characteristics, so it is possible to obtain a valid inference (Wooldridge, 2010). The results in Table 4 replicate the analysis in Table 3 for the subsample of matched municipalities and all the results, in terms of both the size and the statistical significance of the estimated coefficients, are fully confirmed.

[8]The matching approach has been performed by using the Stata command *psmatch2* developed by Leuven and Sianesi (2010).

[9]Four municipalities were found outside of the common support.

***** insert here FIGURE 1*****

***** insert here TABLE 4 *****

As a second check we control whether the results are driven by the amount of compensating transfers that municipalities received from the central government. In fact, as we have described in Section 2, in 2008 and subsequent years, each municipality received a transfer whose amount was determined by some past indicators. Therefore, it can be the case that some municipalities received an amount of compensating transfer very similar to the missing revenue from the abolished property tax on owner-occupied dwellings, while, on the other hand, some municipalities received an amount of compensating transfer by far different (and lower) than the missing revenue from the property tax on owner-occupied dwellings. The difference in the amount of transfers received by the municipality might thus drive our results, so that the effect of the reform is not due to the reform per sé, but, instead, by the higher/lower amount of transfers that the municipality received with respect to the revenue collected from the property tax on owner-occupied dwellings. In order to check for this issue, we build a variable, *icigrants*, containing the per capita revenue of the property tax on owner-occupied dwellings from 2006 to 2007, and the per capita value of the grant compensating municipalities for the corresponding missing revenue on owner-occupied dwellings in 2008.

First, we look at the mean difference of the variable *icigrants*, between control and treated municipalities, before (2006 and 2007) and in the year of the reform (2008). The difference in the variable *icigrants* (Table 5) for control municipalities before and in 2008 (-12.40 euros in per capita terms) is smaller than the same difference for treated municipalities (-17.68 euros), and such differences are statistically significant at 1%, implying that both group of municipalities have, on average, received an amount of compensating transfers lower then the revenue collected through the property tax on owner-occupied dwellings. However, the difference of the differences in the variable *icigrants* between control and treated

municipalities, before and in 2008, leads to an estimate that is not statistically significant, implying that the change in the financial resources of municipalities, due to the switch from the property tax on owner-occupied dwellings to the compensating transfer, for treated municipalities is, on average, the same to that of the control municipalities.

***** insert here TABLE 5 *****

We then replicate the previous regressions of equation (1) by using, as dependent variable, the new variable *icigrants.*[10] Were the coefficient of *pre electoral year* × *reform* significant, it would mean that municipalities in the pre-electoral year in 2008 received a greater /smaller (according to the sign of the coefficient) amount of financial resources with respect to other municipalities, and hence it would be impossible to separate the effect of the reform from the effect of having more (or less) financial resources, in term of the received compensating transfer. The results show that the variable *pre electoral year* × *reform* is not statistically different from zero, both for the whole sample (col. 1, Table 6) and for the sample of matched municipalities (col. 2, Table 6). These results indicate that being in a pre-electoral year in the year of the reform (2008) has no significant effect on the amount of money that municipalities received from the central governments for replacing the missing revenue from the property tax on owner-occupied dwellings. This strongly suggests that the increase in expenditure and revenue from fees and charges observed for municipalities in the pre-electoral year in 2008 (Table 3) is not due to the amount of grants received by municipalities for compensating the missing revenue from the property tax on owner-occupied dwellings.

***** insert here TABLE 6 *****

Finally, the effect of the reform on policy outcomes can be driven by mayors with a binding term limit (the Italian law establishes a limit of no more than two

[10]The variable *icigrants* runs from 2006 to 2008 and contains per capita tax revenue on owner-occupied dwellings in 2006 and 2007 and per capita compensating transfers received by municipalities in 2008, hence, in this specification, the control variable, *transfers*, is net of the compensating grants in the year 2008.

consecutive mandates for the office of mayor), since they might have different incentives to use tax instruments with respect to mayors where the term limit is not binding. To analyze this issue, and so investigate whether there has been any heterogeneous response to the 2008 reform across municipalities with mayors with a binding term limit, we build a *termlim* dummy variable, which is equal to one if the mayor is at her second mandate and zero otherwise, and interact it with both *pre electoral year* and *pre electoral year* × *reform* in a triple-difference model. Therefore the model we estimate is a modified version of the model (1) taking the following form:

$$\begin{aligned} Y_{it} = {} & \gamma_1 pre\,elecotral\,year_{it} + \gamma_2 pre\,elecotral\,year_{it} \times reform \\ & + \gamma_3 pre\,electoral\,year_{it} \times termlim_{it} + \gamma_4 pre\,elecotral\,year_{it} \times reform \times termlim_{it} \\ & + \phi termlim_{it} \times reform + \pi termlim_{it} + \beta' X_{it} + \alpha_i + \tau_t + \lambda Trend_{it} + \epsilon_{it} \end{aligned} \tag{2}$$

where *termlim* is a dummy variable equal to one if the mayor is at her second mandate and zero otherwise. Our variables of interest are *pre electoral year* × *reform* and *pre electoral year* × *reform* ×*termlim* where the former captures the change in the impact of being in the pre-electoral year in the year of the reform (2008) if the mayor is not term limited, and the latter captures the change in the change in the impact of being in the pre-electoral in 2008 when the mayor is lame-duck.

We find that the coefficient of *pre electoral year* × *reform* is positive and statistically significant at 5% for *expenditure* (22.37 euro in per capita terms; col. 1, Table 7) and, that of *pre electoral year* × *reform* ×*termlim* is not statistically significant. Hence, municipalities that are in the pre-electoral year in 2008 increase their current expenditure (22.37 per capita euro), regardless of the status of being a mayor with a binding term limit. The results remain the same when we run regression on the matched sample of municipalities (col. 5, Table 7).

As it regards revenues from *fees and charges,* we find that the coefficient of *pre electoral year* × *reform* is positive and statistically significant at 1% (27.43 per capita euro; col. 4, Table 7) and, that of *pre electoral year* × *reform* ×*termlim* is

negative (-30.02) and statistically significant at 5%. However, the impact of being in a pre-electoral year in 2008 for municipalities which are term limit is 27.43 - 30.02 = -2.59, which is not statistically different from zero (p-value = 0.826), implying, also in this case, that municipalities that are in the pre-electoral year in 2008 increase revenue from fees and charges regardless of the status of being a mayor with a binding term limit. Again, the results remain the same when we run regression on the matched sample of municipalities (col. 8, Table 7)[11].

As for revenue from both *property tax on other dwellings* and *surtax on personal income* we do not find any effect due to the reform, either for municipalities with mayors with a binding term limit or for municipalities with mayors with a no binding term limit (col. 2 and 3, Table 7).

***** insert here TABLE 7*****

6 Conclusion

In this study we investigate the impact on local policy outcome decisions of a very salient fiscal reform, introduced by the Italian government. Since 2008, the local property tax on owner-occupied dwellings was abolished and the corresponding tax yield was replaced for municipal budgets by a compensating transfer from the central government, thus providing a good framework to test for strategic manipulation of policy outcome decisions in anticipation of elections when part of the financial system is switched from decentralized to centralized. We find that the reform impacts on the political budget cycles, leading municipalities that were in the pre-electoral year in 2008 to expand the size of their budget, by increasing current expenditure and fees and charges, compared to municipalities that were in the pre-electoral year before 2008. In addition, the increase in the

[11] In the case where we use all municipalities (col. 4, table 7), the linear combination of the coefficients of *pre electoral year* × *reform* + *pre electoral year* × *reform* ×*termlim* leads to an estimation equals to 27.43 - 30.02 = -2.59, which is not statistically different from zero (p-value = 0.826) and, also in the case where we use the matched sample of municipalities (col. 8, table 7), the linear combination of the coefficients of *pre electoral year* × *reform* + *pre electoral year* × *reform* ×*termlim* leads to an estimation equals to 29.98 - 30.53 = -0.55, which is not statistically different from zero (p-value = 0.964).

expenditure and revenues of municipalities that were in the pre-electoral year in 2008 did not depend on the status of being a mayor with a binding term limit.

These results suggest that the centralization process of the tax system can generate stronger incentives for municipalities to manipulate policy outcome decisions when close to elections, while, on the contrary, under a decentralized tax system, such incentives are weaker.

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Table 1: Mean difference of policy outcomes between municipalities that are in pre-electoral year and municipalities that are not in the pre-electoral year, before and in the year of the reform.

Panel A: Current expenditure				**Panel B: Property tax on other dwellings**			
	No pre-electoral year (1)	Pre-electoral year (2)	Difference (3)		No pre-electoral year (1)	Pre-electoral year (2)	Difference (3)
Before 2008	624.33	600.43	-23.90***	Before 2008	140.70	138.25	-2.45
			(2.70)				(26.09)
2008	675.69	715.62	39.93**	2008	150.42	176.00	25.58***
			(17.69)				(7.93)
Difference (2008 – Before 2008)	51.36***	115.20***	**63.84*** **	Difference (2008 – Before 2008)	9.72	37.75	**28.03**
	(12.27)	(7.98)	**(18.49)**		(6.18)	(26.95)	**(30.24)**
Panel C: Surtax on personal income				**Panel D: Fees and Charges**			
	No pre-electoral year (1)	Pre-electoral year (2)	Difference (3)		No pre-electoral year (1)	Pre-electoral year (2)	Difference (3)
Before 2008	26.33	25.74	-0.59	Before 2008	177.10	174.17	-2.92
			(0.58)				(2.77)
2008	41.00	53.11	12.11***	2008	154.72	188.37	33.65**
			(2.61)				(13.25)
Difference (2008 – Before 2008)	14.66***	27.37***	**12.71*** **	Difference (2008 – Before 2008)	-22.38***	14.19*	**36.57****
	(1.99)	(1.35)	**(2.70)**		(8.16)	(8.08)	**(14.63)**

Notes: Number of observations is 5,131. Period 2002-2008. Municipalities with population between 3,000 and 5,000 inhabitants. For the variable *property tax on other dwellings* data are available only from the 2006 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings has been recorded in Italian municipal budget only from 2006 onwards (2,199 observations). Column (1) of Panel A reports the average per capita *current expenditure* for municipalities that are not in the pre-electoral year before 2008 and in 2008; column (2) of Panel A shows the average per capita *current expenditure* for municipalities that are in the pre-electoral year before 2008 and in 2008; column (3) of Panel A displays the difference of the average per capita *current expenditure* for municipalities that are not in the pre-electoral year and the average per capita *current expenditure* of municipalities that are in the pre-electoral year. Column (1) of Panel B reports the average per capita revenue of *property tax on other dwellings* for municipalities that are not in the pre-electoral year before 2008 and in 2008; column (2) of Panel B shows the average per capita revenue of *property tax on other dwellings* for municipalities that are in the pre-electoral year before 2008 and in 2008; column (3) of Panel B displays the difference of the average per capita revenue of *property tax on other dwellings* for municipalities that are not in the pre-electoral year and the average per capita revenue of *property tax on other dwellings* for municipalities that are in the pre-electoral year. Column (1) of Panel C reports the average per capita revenue of *surtax on personal income* for municipalities that are not in the pre-electoral year before 2008 and in 2008; column (2) of Panel C shows the average per capita revenue of *surtax on personal income* for municipalities that are in the pre-electoral year before 2008 and in 2008; column (3) of Panel C displays the difference of the average per capita revenue of *surtax on personal income* for municipalities that are not in the pre-electoral year and the average per capita revenue of surtax on personal income for municipalities that are in the pre-electoral year. Column (1) of Panel D reports the average per capita revenue of *fees and charges* for municipalities that are not in the pre-electoral year before 2008 and in 2008; column (2) of Panel D shows the average per capita revenue of *fees and charges* for municipalities that are in the pre-electoral year before 2008 and in 2008; column (3) of Panel D displays the difference of the average per capita revenue of *fees and charges* for municipalities that are not in the pre-electoral year and the average per capita revenue of *fees and charges* for municipalities that are in the pre-electoral year. Robust standard errors, clustered at the municipal level, are shown in parentheses. Significance at 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 2: Timing and frequencies of elections

Type of municipality	2002	2003	2004	2005	2006	2007	REFORM 2008	2009
control	E (58)	IV (58)	III (58)	II (58)	I (58)	E (58)	IV (58)	III (58)
control	I (25)	E (25)	IV (25)	III (25)	II (25)	I (25)	E (25)	IV (25)
treated	II (506)	I (506)	E (506)	IV (506)	III (506)	II (506)	I (506)	E (506)
control	III (32)	II (32)	I (32)	E (32)	IV (32)	III (32)	II (32)	I (32)
control	IV (112)	III (112)	II (112)	I (112)	E (112)	IV (112)	III (112)	II (112)

Notes: Period 2002-2008. Municipalities with population between 3,000 and 5,000 inhabitants. Roman letters represent the years to the following election, that is E = election, I = one year to the following election, II = two years to the following election, III = three years to the following election and IV = four years to the following election. The number of municipalities is shown in parenthesis.

Table 3: Policy outcomes baseline results

	current expenditure	property tax on other dwellings	surtax on personal income	fees and charges
	(1)	(2)	(3)	(4)
pre electoral year	-0.42	13.28	0.97	-3.04
	(3.03)	(12.76)	(0.86)	(2.61)
pre electoral year × reform	19.04**	-21.34	1.05	17.75**
	(7.65)	(17.80)	(2.36)	(7.54)
Municipality FE	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	5,131	2,199	5,131	5,131
Number of municipalities	733	733	733	733
Treated municipalities	506	506	506	506
Control municipalities	227	227	227	227
R-squared within	0.66	0.62	0.49	0.56

Notes: Period 2002-2008. Municipalities with population between 3,000 and 5,000 inhabitants. *Pre electoral year* is a dummy variable equals to one in the year before the election; *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise. The number of observations in col. (2) is 2,199 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings has been recorded in Italian municipal budget only from 2006 onwards. In all regression we control for *population*, *density*, *child*, *aged*, *transfers*, *income*, *election*, *municipal effects*, *municipal time trend* and *year effects*. Robust standard errors, cluster at the municipal level, are shown in parenthesis. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 4: Policy outcomes results on a sample of matched municipalities

	current expenditure	property tax on other dwellings	surtax on personal income	fees and charges
	(1)	(2)	(3)	(4)
pre electoral year	-0.00	14.06	1.01	-3.78
	(3.48)	(16.43)	(1.04)	(3.11)
pre electoral year × reform	21.91**	-21.51	-1.55	19.82**
	(8.67)	(21.37)	(2.72)	(8.08)
Municipality FE	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	4,669	2,001	4,669	4,669
Number of municipalities	667	667	667	667
Treated municipalities	502	502	502	502
Control municipalities	165	165	165	165
R-squared within	0.65	0.62	0.49	0.56

Notes: Period 2002-2008. Municipalities with population between 3,000 and 5,000 inhabitants. *Pre electoral year* is a dummy variable equals to one in the year before the election; *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise. The number of observations in col. (2) is 2,001 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings has been recorded in Italian municipal budget only from 2006 onwards. In all regression we control for *population*, *density*, *child*, *aged*, *transfers*, *income*, *election*, *municipal effects*, *municipal time trend* and *year effects*. Robust standard errors, cluster at the municipal level, are shown in parenthesis. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 5: Mean difference estimates of fiscal reform on the variable icigrants

icigrants	control group	treated group	Difference (Treated - Control)
	(1)	(2)	(3)
Pre reform (2006-2007)	53.53	64.85	11.32***
			(3.97)
Reform (2008)	41.14	47.17	6.03***
			(1.85)
Difference (Reform –Pre reform)	-12.40***	-17.68***	**-5.28**
	(2.15)	(2.79)	**(3.52)**

Notes: Period 2006-2008. Municipalities with population between 3,000 and 5,000 inhabitants. Number of observations 2.199. Number of treated municipalities: 506, number of control municipalities: 227. Column (1) reports average per capita revenue of the variable *icigrants* for control municipalities before 2008 and in 2008; column (2) displays average per capita revenue of the variable *icigrants* for treated municipalities before 2008 and in 2008; column (3) shows the average difference of per capita revenue of the variable *icigrants* for control and treated municipalities before 2008 and in 2008. Robust standard errors, clustered at the municipal level, are shown in parentheses. Significance at 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6: Estimates of fiscal reform on the variable icigrants

Dependent variable: *icigrants*	Whole sample	Sample of matched municipalities
	(1)	(2)
pre electoral year	1.78	1.08
	(8.76)	(9.38)
pre electoral year × *reform*	5.85	7.71
	(14.43)	(15.13)
Municipality FE	YES	YES
Municipal time trend	YES	YES
Year FE	YES	YES
Observations	2,199	2,001
Number of municipalities	733	667
Treated municipalities	506	502
Control municipalities	227	165
R-squared within	0.57	0.57

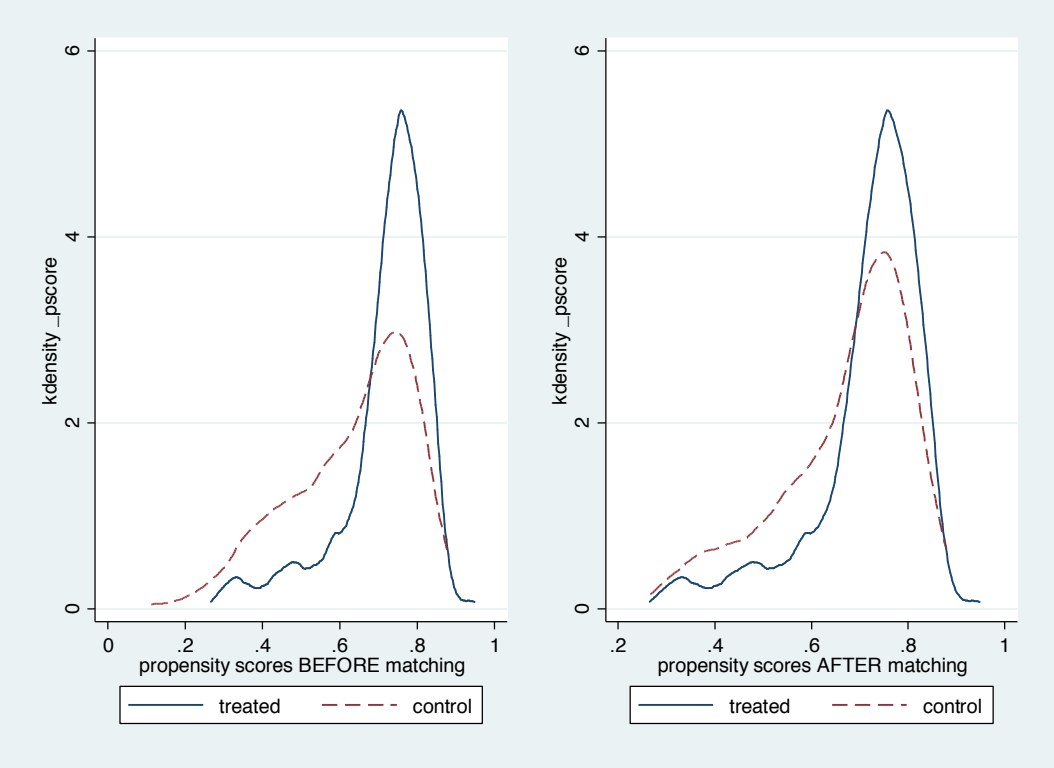
Notes: Period 2006-2008. Municipalities with population between 3,000 and 5,000 inhabitants. *Pre electoral year* is a dummy variable equals to one in the year before the election; *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise. Col. (1) reports the results by using all the sample available, col. (2) displays the results by using the sample of matched municipalities. In all regression we control for *population*, *density*, *child*, *aged*, *transfers (net of compensating transfers for the year 2008)*, *income*, *election*, *municipal effects*, *municipal time trend* and *year effects*. Robust standard errors, cluster at the municipal level, are shown in parenthesis. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 7: Policy outcomes results and term limit

	Whole sample				Sample of matched municipalities			
	current expenditure	property tax on other dwellings	surtax on personal income	fees and charges	current expenditure	property tax on other dwellings	surtax on personal income	fees and charges
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
pre electoral year	0.66	12.66	0.71	-2.15	0.44	8.15	0.82	-2.86
	(4.39)	(13.54)	(1.15)	(4.05)	(4.66)	(18.39)	(1.32)	(4.64)
pre electoral year × reform	22.37**	-14.27	0.95	27.43***	26.96**	-12.91	-2.08	29.98***
	(10.24)	(21.90)	(2.83)	(9.11)	(12.03)	(27.56)	(3.36)	(10.36)
pre electoral year × termlim	-1.94	8.20	0.55	-1.06	-0.58	19.54	0.38	-1.00
	(6.98)	(25.13)	(1.73)	(7.34)	(7.39)	(32.04)	(1.85)	(7.95)
pre electoral year × after × termlim	-10.74	-25.98	0.59	-30.02**	-14.49	-29.85	1.67	-30.53*
	(15.17)	(30.99)	(4.59)	(14.59)	(17.44)	(39.03)	(5.33)	(15.98)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	5,131	2,199	5,131	5,131	4,669	2,001	4,669	4,669
Number of municipalities	733	733	733	733	667	667	667	667
Treated municipalities	506	506	506	506	502	502	502	502
Control municipalities	227	227	227	227	165	165	165	165
R-squared within	0.66	0.63	0.49	0.56	0.65	0.63	0.49	0.56

Notes: Period 2002-2008. Municipalities with population between 3,000 and 5,000 inhabitants. *Pre electoral year* is a dummy variable equals to one in the year before the election; *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise, and *termlim* is a dummy variable equal to one if the mayor is at her second mandate and zero otherwise. Columns (1), (2), (3) and (4) report the results by using all the sample available; columns (5), (6), (7) and (8) display the results by using the sample of matched municipalities. The number of observations in col. (2) and col. (6) is lower because the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings has been recorded in Italian municipal budget only from 2006 onwards. In all regression we control for *termlim×after reform*, *termlim, population*, *density*, *child*, *aged*, *transfers*, *income*, *election*, *municipal effects*, *municipal time trend* and *year effects*. Robust standard errors, cluster at the municipal level, are shown in parenthesis. *** significant at 1%; ** significant at 5%; * significant at 10%.

Figure 1: Propensity score in Treated and control group, before and after implementing the matching procedure



Notes: the figure presents the distribution of the estimated propensity score between treated and control municipalities, before and after the matching procedure. For the matching procedure we use the "nearest neighbor" approach as explained in section 5.3.

Table A1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
current expenditure	5131	632.07	219.17	218.55	2362.66
surtax on personal income	5131	29.53	27.16	0.00	217.10
fees and charges	5131	176.79	183.90	8.19	3408.61
property tax on other dwellings	2199	149.73	115.01	0.00	2101.19
icigrants	2199	56.00	60.47	0.00	1467.49
reform	5131	0.14	0.35	0.00	1.00
population	5131	4028.23	658.18	2269.00	7535.00
child	5131	0.05	0.01	0.02	0.09
old	5131	0.20	0.04	0.09	0.34
density	5131	291.21	333.11	14.18	3304.00
income	5131	11198.29	3296.32	2819.97	28118.87
transfers	5131	193.11	122.08	5.42	1627.43
termlim	5131	0.38	0.49	0.00	1.00
pre electoral year	5131	0.25	0.43	0.00	1.00

Notes: Period 2002-2008. Years before the reform are 2002-2007. Year after the reform is 2008. Municipalities with population between 3,000 and 5,000 inhabitants. For the variable *property tax on other dwellings* data are available only from the 2006 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings has been recorded in Italian municipal budget only from 2006 onwards.

Table A2: Descriptive statistics

Variable	Definition and measure	Available from-to	Source
current expenditure	Current expenditure per resident; 2011 Euros	2002-2008	Italian Ministry of Interior
surtax on national income	Revenue from surtax on personal income per resident; 2011 Euros	2002-2008	Italian Ministry of Interior
fees and charges	Revenue from fees and charges per resident; 2011 Euros	2002-2008	Italian Ministry of Interior
property tax on other dwellings	Revenue from property tax on other dwellings per resident; 2011 Euros	2006-2008	Italian Ministry of Interior
icigrants	Vector containing revenue per resident of property taxes on owner-occupied dwellings from 2006 to 2007 and compensating grants per resident for the corresponding missing revenue on owner-occupied dwellings for 2008; 2011 Euros	2006-2008	Our computation
pre electoral year	Dummy variable equal to 1 in the year before the election	2002-2008	Our computation
reform	Dummy variable equal to 1 for year 2008	2002-2008	Our computation
population	Population of the municipality	2002-2008	ISTAT
child	Share of the population aged between 0-5	2002-2008	ISTAT
old	Share of the population over the age of 65	2002-2008	ISTAT
density	Numbers of citizens per area	2002-2008	Our computation
income	Real personal income tax base per resident; 2011 Euros	2002-2008	Italian Ministry of Economy, Department of Finance
transfers	Total current transfers from the upper level of the government (State and Regions)	2002-2008	Italian Ministry of Interior
termlim	Dummy variable equals to one if the mayor is at her second mandate and zero otherwise	2002-2008	Our computation
election	Dummy variable equal to 1 for each election year of the municipalities and zero otherwise	2002-2008	Italian Ministry of Interior, Department of Internal Affairs

Table A3: Logit Regression

	Treated
	(1)
altitude	0.00
	(0.00)
population	-0.00***
	(0.00)
aged	-3.10
	(3.50)
child	-6.92
	(13.71)
density	-8.66
	(11.61)
income	0.00
	(0.00)
transfers	0.00
	(0.00)
families	6.93*
	(4.10)
houses	-0.82
	(0.61)
firms	6.68
	(6.68)
unemployed	-4.49**
	(1.75)
altimetry zone	-0.12
	(0.12)
Constant	0.95
	(1.97)
Observations	733

Notes: Period 2001. Municipalities with population between 3,000 and 5,000 inhabitants. All the variables, a part from *income* and *transfers*, are from the 2001 Census. Standard errors are shown in parenthesis. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table A4: Difference between the matched set of treated and control municipalities on the characteristics used for the matching procedure.

	Mean		Difference (T-test)	
Variable	Treated	Control	t	p>|t|
population	3,850.500	3,856.600	-0.160	0.870
altimetry zone	1.663	1.673	-0.180	0.861
aged	0.191	0.194	-1.160	0.244
child	0.055	0.054	1.000	0.320
density	0.008	0.007	0.960	0.340
income	11,341.000	11,505.000	-0.720	0.470
transfers	265.430	252.790	1.460	0.146
families	0.384	0.384	-0.040	0.971
houses	0.487	0.484	0.240	0.808
firms	0.064	0.064	0.600	0.547
unemployed	0.067	0.065	0.380	0.702
altitude	263.520	261.940	0.120	0.903

Notes: Period 2001. Municipalities with population between 3,000 and 5,000 inhabitants. All the variables, a part from *income* and *transfers*, are from the 2001 Census.