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# SWITCH TOWARDS TAX CENTRALIZATION IN ITALY: A WAKE UP FOR THE LOCAL POLITICAL BUDGET CYCLE*

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ABSTRACT: The abolition of the municipal property tax on owner-occupied dwellings accomplished in Italy in 2008 offers a quasi-natural experiment that allows for the identification of the presence of political budget cycles - the incentives for municipalities close to elections to manipulate policy outcome decisions. Our empirical analysis shows that the reform impacted on municipalities that in 2008 were in their pre-electoral year, by expanding the size of their budget in the form of an increase of current expenditure and fees and charges, but this did not occurred in municipalities that experienced their pre-electoral year before 2008.

JEL Codes: C3, H71, H72

Keywords: Political budget cycle, transfers, federal budget, property tax, fiscal reform, local elections.

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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 narrow margin. As a result, the government headed by Romano Prodi, supported by a weak 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 most 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 his chances of reelection) by engaging in 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 (pre-election increase in government deficit) when a competent politician is in office. Since then, a large literature has developed, 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 - that 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 revenue 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 in the years 1960-2001, find the presence of the political budget cycles only in new democracies.

Russian provinces monthly panel data in 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 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, in fact 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 – 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 taxes and spending decisions of municipalities close to elections. Hence, interestingly, our setting can let us understand whether the reform triggers the policy maker behavior, typical of the political budget cycle. 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 after the reform, implying that one pre-electoral year falls before the reform and one after 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 expenditure and revenue for the two groups of municipalities during their pre-

electoral years. 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. In particular, we expect that the incentive to strategically manipulate decisions on expenditure and taxes should be more pronounced for those municipalities that are in the pre-electoral year after the reform, as the compensating transfer granted by the central government in replacement of the revenue from the abolished property tax on owner-occupied dwellings bears no political costs for the local decision maker. In fact, our results show that municipalities in the pre-electoral year after the reform increase expenditure by 3% with respect to the average value of the municipal expenditure. Moreover, we find that municipalities in the pre-electoral year after the reform increase revenue 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. Our 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*), introduced in 1992 and applied to real estate. 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 personal central income tax (*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 for parking permits, occupation of public spaces and 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 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 transfer 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 these criteria in determining the amount of compensating transfers, introduced in 2008, is 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 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 set of local devolved small tax - such as cadastral taxes on property sales and a fixed municipal share to the VAT (only for 2011) - , modifications of the equalization system and of the structure of vertical transfers from the central government, the introduction in 2012 of a reformed property tax on principal 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 compensation 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 policies varying with population size. Moreover, vertical transfers from the central government changes proportionally with the population (Law 504/1992). Finally, municipalities below 5,000 inhabitants are exempted from a set of rules imposed by the national government to the 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 el. (2013) 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. These different policies based on population brackets might affect the identification of the impact of the property tax reform on fiscal policy decisions.

Furthermore, regions with special autonomy are allowed to set their own fiscal rules for municipal governments.

# 3 Identification strategy

As we discussed in the previous section, there are several policies that change at different population threshold, as well as other local structural reforms took place in Italy in the last 15 years. The presence of these policies might confound the impact on local policy choices of the replacement of the property tax on owner-occupied dwellings with a vertical transfer, so that the effect of the effect of reform cannot be properly identified. Hence, first, we restricted our sample to municipalities belonging to regions ruled by ordinary status with a population range between 3.000 and 5.000 inhabitants. Such restriction assures that no other policies changes according to population size. Then, we focused on the period 2002-2008 because within this period we do not assist to any other local structural reforms a part that of abolishing the property tax on owner-occupied dwellings.

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 the 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 hold 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 falls after 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 the policy outcome 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 municipality B has already held the election before the reform.

In the absence of the reform, the difference in the policy outcomes in the pre-electoral years between municipality A and municipality B before 2008, would be exactly the same as the difference in the policy outcomes in the pre-electoral year between municipality A and municipality B after 2008. 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 outcome, we should observe a difference in local tax and spending decisions between municipality A and B after the reform: such difference would represent 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 Cen-

sus population. Also we did not include municipalities with missing values from our dataset and municipality put under commissioner or municipality where the majors resigned before the term. Finally we obtain a sample of 733 municipalities including 5,131 observations from 2002 to 2008.[3]

## 4.2 Dependent variables

As our dependent variables 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 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 comparable rates for all kind of revenues we consider (taxes and fees and charges). Third, revenue gives account for both tax rate effort and effort in tax evasion control, which are both complementary important components of the municipality's fiscal policy.

As a preliminary piece of evidence it is interesting to look at the mean difference in expenditure and revenue variables before and after the reform (Table 1). In particular, the average *current expenditure* after the reform is 83.05 euro higher than that before the reform and this difference is statistically significant at 1%. The same difference for both revenue from *property tax on other dwellings* and from *surtax on personal income* is, respectively, of 27.52 (1% significant) and 23.13 euro (1% significant). Note that revenues from *fees and charges* after the reform do not differ from those of before the reform. What this simply suggests is that the reform seems to have led to a significant increase in current expenditure, net of the increase in own revenues.

[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 Province, 1,125 observations relative to municipalities put under commissioner and municipality where the majors resigned before the term in the considered period, and 730 observations relative to municipalities/years where data are not complete or data are missing.

Table 1: Mean difference in expenditure and revenue before-after the reform

| Outcome variables | Before the reform | After the reform | Difference in means |
|---|---|---|---|
| | (1) | (2) | (3) = (2)-(1) |
| current expenditure | 620.21 | 703.25 | 83.05*** |
| | (3.21) | (8.94) | (6.64) |
| property tax on other dwellings | 140.56 | 168.08 | 27.52*** |
| | (3.15) | (3.70) | (4.07) |
| surtax on personal income | 26.23 | 49.36 | 23.13*** |
| | (0.37) | (1.26) | (0.80) |
| fees and charges | 176.59 | 177.95 | 1.35 |
| | (2.72) | (7.50) | (7.34) |

**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.

## 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, since the electoral terms are not perfectly aligned, the timing of elections generates a random assignment of municipalities, in the period 2002-2008, into two groups: those with an election held after the reform and those with all elections held before the reform. 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 have elections scheduled for 2009.[4] These municipalities represent our treated group since, given the timing

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

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 of the first switch from its own tax 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].

Table 2: Timing and frequencies of elections

| | | | | | | | REFORM | |
|---|---|---|---|---|---|---|---|---|
| **Type of municipality** | **2002** | **2003** | **2004** | **2005** | **2006** | **2007** | **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 elections. The number of municipalities is shown in parenthesis.

[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 noy complete or data are missing, our dataset contains information on 758 municipalities observed for the period 2002-2008. However, we also exluded 25 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 control 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 school, 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 level 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:

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

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 equals 1 in the year before the election and 0 otherwise, *after reform* is a dummy variable equal to 1 in the year 2008, when the property tax on owner-occupied dwellings has been abolished and replaced by a compensating vertical transfer; $X_{it}$ contains all the control variables discussed in section 4.4. To take account of unobserved heterogeneities across municipalities, we include a set of municipalities fixed effects, $\alpha_i$, and we also control for exogenous shocks that can equally affect both treated and control group by adding year fixed effects, $\tau_t$. 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 trend we control for any potential temporal pattern independent of the treatment status. Finally, $\varepsilon_{it}$ is the error term, clustered at the municipal level.

In this framework, $\gamma_1$, accounts for the impact of upcoming elections on the policy outcome before the reform, while $\gamma_2$ is the DiD estimator, which captures the differential effect – on the policy outcome – with respect to $\gamma_1$ of being in a pre-electoral year after the reform.

## 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* $\times$ *after 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 after the reform 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 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* $\times$ *after 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 after the reform 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 revenue 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, and so accounting for the presence of the political budget cycles. 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 political 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 other left tax instruments, because they are collected several times during the fiscal year and their amount is, generally, relatively small, so voters do not easily understand how much power a mayor has in setting these fees (Bracco et al., 2013).

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 × after 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 and *after reform* is a dummy variable equals to one after the reform (2008). 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%.

## 5.3 Robustness checks

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

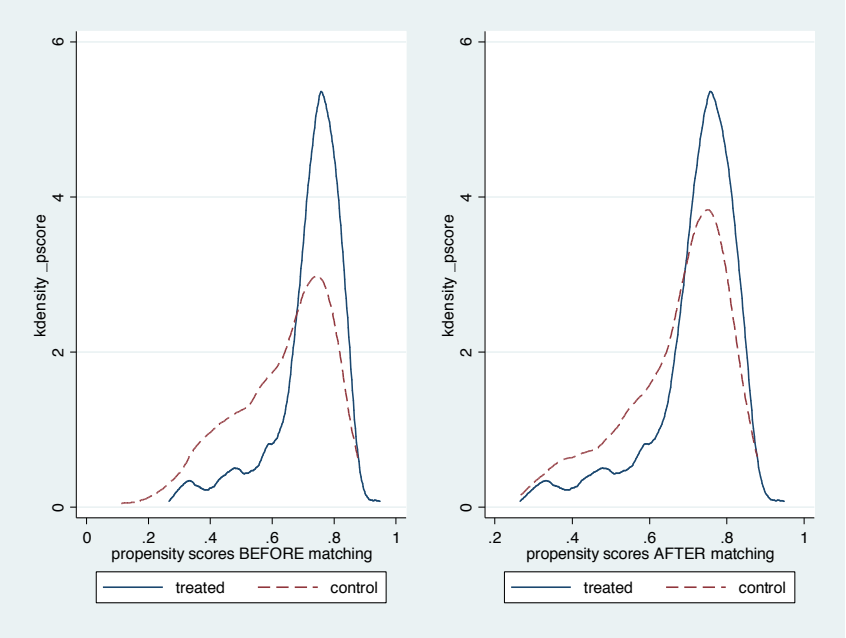
Even though we have restricted our analysis to municipalities belonging to the range of 3,000 - 5,000 inhabitants to avoid the presence of other overlapping policies, one source of potential concerns is that the group of treated municipalities might differ in some characteristics with respect to the control group of municipalities, making thus our "random assignment" hypothesis of the treated status weaker. Therefore, we address this issue by using the matching approach[8], that consists to match treated and control group based on a set of observable char-

[8]The matching approach has been performed by using the Stata command *psmatch2* developed by Leuven and Sianesi (2010). Moreover, we have performed the matching procedure by using the observations lying on the common support, resulting in 4 municipalities outside the common support.

acteristics. In particular, to match treated and control group, we use data from 2001 Census and we ran 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 hill, and equal to 3 if the municipality is located in mountain, 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 level of government (*transfers*), the proportion of families (*families*), the per capita number of houses (*houses*), the per capita number of firms (*firms*), the unemployed rate (*unemployed*) and the average altitude level of the municipal territory (*altitude*). Then we match the sample of treated to a comparable sample of non treated, linking each municipality only to its "nearest neighbor" in terms of municipalities propensity score. 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.

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 6.3.

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 × after 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 and *after reform* is a dummy variable equals to one after the reform (2008). 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%.

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 2.1, in 2008 and subsequent years, each municipality received a transfer whose amount was determined by some past indicators. Therefore, it might be the case that some municipalities received an amount of compensating transfer very similar to the missing revenue from the 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 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 after the reform (2008). The difference in the variable *icigrants* (Table 5) for control municipalities before and after the reform (-12.40 per capita euros) is smaller than the same difference for treated municipalities (-17.68 per capita 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 after the reform, leads to an estimate that is not statistically significant, implying that the change in the financial resources of treated 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.

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) |
| After reform (2008 ) | 41.14 | 47.17 | 6.03*** |
| | | | (1.85) |
| Difference (After -Pre) | -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 and after the reform; column (2) displays average per capita revenue of the variable *icigrants* for treated municipalities before and after the reform; column (3) shows the average difference of per capita revenue of the variable *icigrants* for control and treated municipalities before and after the reform. 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 ***.

Second, we replicate the previous regressions of equation (1) by using, as the dependent variable, the new variable *icigrants*.[9] Were the coefficient of *pre electoral year* $\times$*after reform* significant, it would mean that municipalities in the pre-electoral year after reform would have 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* $\times$*after 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 after the reform 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 after the reform (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.

[9]Since the variable *icigrants* contains the per capita value of the grant compensating municipalities for the corresponding missing revenue on owner-occupied dwellings in 2008, the control variable *transfers*, in this specification, is net of the compensating grants in the year 2008.

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* × *after 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 and *after reform* is a dummy variable equals to one after the reform (2008). 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%.

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 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* × *after reform* in a triple-difference model. Therefore the model we estimate is a modified version of the model (1) taking the following form:

$$Y_{it} = \gamma_1 pre\,elecotral\,year_{it} + \gamma_2 pre\,elecotral\,year_{it} \times after\,reform$$
$$+ \gamma_3 pre\,electoral\,year_{it} \times termlim_{it} + \gamma_4 pre\,elecotral\,year_{it} \times after\,reform \times termlim_{it}$$
$$+ \phi termlim_{it} \times after\,reform + \pi termlim_{it} + \beta' X_{it} + \alpha_i + \tau_t + \lambda Trend_{it} + \epsilon_{it} \quad (2)$$

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* $\times$*after reform* and *pre electoral year* $\times$*after reform* $\times$*termlim* where the former captures the impact for no-term limit municipalities in the pre electoral year after the reform, and the latter captures how such impact changes for municipalities whose mayor is lame-duck.

We find that the coefficient of *pre electoral year* $\times$*after reform* is positive and statistically significant at 5% for *expenditure* (22.37 per capita euro; col. 1, Table 7) and, that of *pre electoral year* $\times$*after reform* $\times$*termlim* is not statistically significant. Hence, municipalities that are in the pre-electoral year after the reform 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* $\times$*after reform* is positive and statistically significant at 1% (27.43 per capita euro; col. 4, Table 7) and, that of *pre electoral year* $\times$*after reform* $\times$*termlim* is negative (-30.02) and statistically significant at 5%. The impact of being in a pre-electoral year after the reform for municipalities which are term limit is 27.43 - 30.02 = -2.59, which is not statistically different from zero (p-value = 0.826)[10], implying, also in this case, that municipalities that are in the pre-electoral year after the reform increase revenue from fees and charges regardless of the status of being a mayor with a binding term limit. Also in this case, the results remain the same when we run regression on the matched sample of municipalities (col. 8, Table 7).

As it concerns 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).

[10]In the case where we use all municipalities (col. 4, table 7), the linear combination of the coefficients of *pre electoral year* $\times$*after reform* + *pre electoral year* $\times$*after reform* $\times$*termlim* leads to an estimation equals to 27.43 - 30.02 = -2.59, which is not statistically different from zero (p-value = 0.826), while 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* $\times$*after reform*+ *pre electoral year* $\times$*after reform* $\times$*termlim* leads to an estimation equals to 29.98 - 30.53 = -0.55, which is not statistically different from zero (p-value = 0.964).

# 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 × after 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 reform × 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; *after reform* is a dummy variable equals to one after the reform (2008) 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%.

# 6 Conclusion

In this study we investigated 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 found that the reform impacts on the political budget cycles, leading municipalities that were in the pre-electoral year after the reform 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 the reform. In addition, the increase in the expenditure and revenues of municipalities that are in the pre-electoral year after the reform does 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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# Appendix

**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 |
| after 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 |
| after 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.

**2012**

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**2013**

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**2015**

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**Fiscal Federalism**

# IEB Working Paper