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

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Abstract
Are the incentives to expand expenditure before local elections affected by the composition of local governments' revenues? We explore this issue by exploiting the Italian government bill that in 2008 replaced the municipal tax on main residence with a vertical transfer. Relying on staggered dates of municipal elections to identify the effect of the reform, we find evidence of a political budget cycle, but only for municipalities that in 2008 were in their pre-electoral year. The result suggests that a lower degree of municipal tax autonomy strengthens the incentives to expand the size of the budget before the elections.

Keywords Political budget cycle · Local elections · Local property tax · Tax autonomy · Fiscal centralization

JEL Classification C23 · H71 · H72

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1 Introduction

Taxes on housing properties are often the 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 municipal tax on owner-occupied housing properties (*Imposta Comunale sugli Immobili*, ICI).[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 by 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 on owner-occupied dwellings.

From the perspective of municipal finance, the main feature of the 2008 reform is that the abolition of the municipal property tax on main residence was paired with the introduction of a transfer from the central government to cover the revenue loss—the so-called compensating transfer. Hence, the reform changed the composition of the revenue side of municipal budgets, reducing the degree of municipal tax autonomy. More importantly, it dispensed municipal administrators from having to rely on a tax bearing high political costs—as its burden falls entirely on residents who are also the voters at the local level—while covering the revenue loss with a compensating transfer, which instead bears no political costs for the local decision maker. The impact on the incentives underlying local budget decisions 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, in line with the well-known literature on political budget cycles.

The classical theoretical framework on political budget cycles is due to Rogoff 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.[2] More specifically, they assume that political candidates have either a high or a low competence level that is known to the politician but not to the electorate. Before the election, a high-type incumbent will signal her 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 'more visible' current spending (Rogoff 1990). Since the signaling action is less costly for a high type than for a low-type

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

[2] Seminal works highlighting the incentives for governments to manipulate public policies for electoral purposes—in particular tax cuts and spending increases—are Nordhaus (1975) and Tufte (1978).

incumbent, the outcome is the emergence of 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.[3] Instead, only few works focus on local governments, because data at the local level are usually available for shorter time periods than national data, and because in most countries local elections generally occur at the same time, so that it is difficult to identify the election year effect for a specific government layer (Sjahrir et al. 2013). Evidence of local political budget cycles is found by Kneebone and McKenzie (2001), who use data on Canadian provinces over the period 1966–1997, finding that more visible expenditure functions—such as education, transportation and communication, recreation and culture—expand in election years versus non-election years. Similar findings are reported by Drazen and Eslava (2010), who show that prior to elections Colombian municipalities significantly expand their spending on public infrastructures, since this type of spending is considered more attractive to voters. Akhmedov and Zhuravskaya (2004) use a monthly panel dataset of Russian provinces over the period 1998–2003, finding significant political cycles both for overall spending and for budget composition. Khemani (2004) considers the 14 major Indian states over the period 1960–1992, showing that in election years tax collection from specific producer groups is lower and public investment spending is higher than in non-electoral years. Finally, a quasi-experimental strategy has been recently exploited by Alesina and Paradisi (2017) to test the budget cycle in a cross section of Italian municipalities for the year 2012, given that at the end of 2011 the central government introduced a new municipal tax on housing properties (*Imposta Municipale Unica*, IMU) to replace the above-mentioned ICI. Focusing on the new tax, applied in 2012 both on main and on additional residences, Alesina and Paradisi (2017) find evidence of a political budget cycle, since municipalities holding elections in 2013 set for year 2012 tax rates on main residence significantly lower than those set by municipalities not having elections. Interestingly, Alesina and Paradisi (2017) do not find any significant effect for the 2012 tax rates on additional residences. However, when they replicate the analysis for 2013, when the tax on main residence was again abolished, they find that municipalities holding elections in 2014 set significantly lower tax rates on additional residences than those not in their pre-electoral year.

[3] Among others, Alesina et al. (1997), using a sample of 13 OECD countries for the period 1960–1993, find the presence of a political budget cycle only for the overall budget; instead, when the budget is split into different components, they do not find any significant result. Persson and Tabellini (2000) investigate whether the budget cycles are driven by the system of government, finding the cycle only in presidential systems and only for revenues. Other works show that budget cycles occur only in certain countries or under specific circumstances, depending on the political environment. In particular, Shi and Svensson (2006), using a panel of 123 countries over the period 1975–1995, find that budget cycles are present only in developing countries. Brender and Drazen (2008), using a sample of 106 countries for the years 1960–2001, document the presence of political budget cycles only in new democracies. Bove et al. (2016)—relying on a panel of 22 OECD countries for the period 1981–2009—find evidence of a switch from military to social spending when elections are getting closer, with an effect that is less pronounced for countries involved in conflicts and in countries supported by a right-wing majority.

In this work, we rely on a panel of Italian municipalities to examine whether the exogenous change in their financial system settled by the central government in 2008—the abolition of the property tax on main residence paired with the allowance of a compensating transfer—affected the incentives for local administrators to strategically manipulate tax and spending decisions before the elections. To identify the effect of the reform we exploit the staggered timing of Italian municipal elections, allowing us to divide the municipalities in our panel—whose observations span from 2002 to 2008—into two groups. The first one includes the municipalities holding 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 another one in the same year of the reform. The second group includes the municipalities holding either one or two elections before the reform, implying that all the pre-electoral years fall before the reform. The comparison of expenditure and revenue decisions across groups enables us to assess the effect of the reform, since the municipal financing system is the same for both groups before the reform while it is different after it, implying that pre-electoral year budget decisions should be similar across groups before the reform while they may differ once the reform is in effect. And this is indeed the case, since our empirical analysis shows that the municipalities that were in their pre-electoral year in 2008 increased expenditure by 3% and revenues from fees and charges by 10% with respect to the corresponding average values, as a clear indication that the reform prompted incentives to strategically manipulate budget decisions in pursuit of electoral goals.

Our interpretation of this result is that the effect of the reform was that of lessening the political costs sustained by local administrators for increasing own revenues to finance expenditure hikes before the elections.[4] Prior to 2008, municipalities relied on three main sources of own revenues: the property tax on main residence, whose burden falls entirely on residents, user fees and charges, whose burden falls almost entirely on residents, and the property tax on additional residence, whose burden falls in part on residents and in part on non-residents.[5] Since, as noted above, taxing the main residence is highly unpopular, the typical practice was to tax the main residences with rates just above the minimum level imposed by the central government, while taxing the additional residences with rates close to the maximum allowed. At the same time, the political cost of increasing revenues from user fees and charges was high, since residents were already taxed on their main residence. Hence, since under these conditions it was quite costly to finance expenditure hikes, local administrators had little room for indulging in strategic political budget cycles.[6] The 2008 reform changed the picture, as municipalities gained room for financing expenditure boosts through user fees and charges, since the substitution of

[4] We formalize our interpretation in a simple local public finance model that we illustrate in an Online Appendix.

[5] In the case of municipalities located in touristic areas, almost all additional housing properties are owned by non-residents.

[6] Debt financing was not a viable option, as the provisions of the Domestic Stability Pact together with other fiscal laws imposed no deficits on current municipal budgets and stringent targets on capital budgets.

the tax on main residences with the compensating transfer reduced the political costs of making leverage on user fees and charges.[7]

The rest of the work is organized as follows. Section 2 illustrates the policy reform and describes the institutional framework about municipal finance in Italy. Section 3 illustrates the identification strategy and Sect. 4 the dataset and some preliminary evidence. The empirical analysis, the results and the robustness checks are in Sect. 5. The possibility of heterogeneous responses, by term-limited and non-term-limited mayors, is addressed in Sect. 6. Section 7 concludes.

2 Municipal finance in Italy

Italian municipalities are responsible for a large array of important public programs in the fields of welfare services, territorial development, local transport, infant schools, sports and cultural facilities, local police services, as well as infrastructural spending. As a share of the general government budget, during the time span covered by our empirical analysis (2002–2008) municipalities account on average for about 8% of total public expenditure.

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, on own revenue sources.

The main municipal tax—introduced in 1992—is ICI (*Imposta Comunale sugli Immobili,* since 2011 renamed IMU, *Imposta Municipale Unica*), due yearly by real estate owners to the municipality where the property is located. The tax base is the cadastral income, which does not vary over time unless, but only rarely, a nationwide uniform increase in all cadastral values is established by law.[8] Another important feature is that the range of tax rates applicable to main residences (the dwellings where owners have their residence) is smaller than that applicable to additional residences (rented properties, secondary properties used for holidays, and so on). Moreover, tax credits (conditional, for instance, on having children in the household) are allowed on main residences but not on additional residences.

Other revenue sources for Italian municipalities are the duty due for waste collection (a tax until 2013, named *TARSU*; a charge since 2014, named *TARI*), the surtax on the central government personal income tax (*Addizionale Comunale all'Irpef*), various types of fees (for parking permits, occupation of public areas, use of billboards, and so on), and charges for the use of municipal services (infant schools, sports facilities, and so on).

As described in the Introduction, on May 27, 2008, the government decree no. 93 abolished the property tax on main residence and introduced a vertical transfer to cover the loss in tax yields. The amount of the transfer, however, was not exactly

[7] Moreover, user fees and charges are a less transparent revenue source than property taxes (Bracco et al. 2013). Recall also, as noted above, that tax rates on additional residences were already close to their maximum level, and that municipalities could not rely on debt financing.

[8] Note that a uniform increase does not change cadastral values in relative terms.

equal to the tax revenue loss for each municipality, since the decree also established that the transfer was to be based on: (a) efficiency in tax collection, expressed by an index equal to the ratio between the average value of the revenue of the property tax on main residence over the period 2004–2006 measured in cash terms and the corresponding value measured in accrual terms; (b) degree of compliance with the fiscal rules imposed by the central government on municipalities for the year 2007 through the Domestic Stability Pact. Furthermore, special provisions applied to municipalities with a population lower than 5000 inhabitants. In the aggregate, the amount of the compensating transfers granted in 2008 was about 2.8 billion Euros, while the revenue from the property tax on main residence collected in 2007 was about 3.5 billion Euros.

Note that the criteria used to determine the compensating transfers, introduced in 2008, were based on decisions taken beforehand by municipalities. Hence, in 2008 municipal administrators had no room to act strategically with the aim of increasing their share of the transfers. For local policy makers, the amount of transfers was truly exogenous.[9]

3 Identification strategy

In order to assess whether and how the 2008 reform affected the incentives for local administrators to expand the size of municipal budgets before the elections, we adopt a binary identification strategy. The first is based on misalignment of the timing of Italian municipal elections. The second is based on restricting the sample to municipalities operating in a ‘uniform’ institutional setting, so as to ensure that over the chosen time span for the analysis there are no other relevant institutional settings or changes in policies, apart from the 2008 reform, bearing heterogeneous impacts on budget decisions of different types of municipalities.

3.1 Staggered municipal elections

In Italy, municipal elections are normally held every 5 years during the period April–June but the timing is not the same for all municipalities. Staggering of electoral dates is the result, over the years, of local governments having to resign before the end of the term because of the impossibility to form a majority in the city council supporting the local government, or because of political scandals or judicial impeachment.

This feature of the timing of municipal elections is the first pillar of our identification strategy. To illustrate the point, suppose that over a given period, including two pre-electoral years, we can observe two municipalities, A and B, that are similar in their demographic, geographic and socioeconomic characteristics. Now suppose a coin flip decides the timing of elections with the result that while municipality A holds one election before the reform and one the year after it, municipality B

9 Exogeneity of the compensating transfers is examined in detail in Sect. 5.3.

holds two elections before the reform. The key point is that being in an electoral year is randomly assigned. Hence, such exogenous variations in terms of the timing of elections allow us to define a treated and a control group. In particular, municipality A, holding one election before the reform and one the year after it—implying that one pre-electoral year falls before the reform and the other in the same year—is our treated municipality. Municipality B, holding both elections before the reform—implying that both pre-electoral years fall before the reform—is the control municipality. We can therefore compare the expenditure and revenue decisions of municipality A (treated) with those of municipality B (control) before the reform; i.e., in a situation where A and B face the same incentives to manipulate their budget in a pre-electoral year, since they both rely on the same set of tax instruments. But we can also make a comparison after the reform; i.e., in a situation where A—which is in a pre-electoral year when the reform is enacted—faces different incentives from B—which has both pre-electoral years falling before the reform is introduced.

Suppose that the 2008 reform was not introduced. In this case, the difference between the pre-electoral years' policy outcomes of municipality A should be the same as that of municipality B. For A, one pre-electoral year falls before 2008 and one in 2008; for B, both pre-electoral years fall before 2008. However, with no reform this difference in the timing of elections should not affect fiscal policy decisions, so that there should be no difference in the differences of pre-electoral years' policy outcomes between treated and control groups. On the other hand, if the reform is introduced, and if it affects the incentives to manipulate budget decisions before the elections, then we should observe a difference between pre-electoral years' tax and spending decisions of treated and control groups. In particular, the difference between the pre-electoral years' policy outcomes of municipality A (one before 2008 and one in 2008) should be different from that of municipality B (for which both electoral years fall before the reform). If significant, such difference in the differences represents the causal effect of the reform on the incentives for political budget cycles at the municipal level.

3.2 Uniform institutional setting

There are two dimensions that need to be carefully considered in order to correctly identify the impact of the 2008 reform. 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, since 2002 municipalities have been granted access to a fixed share of the personal income tax revenues generated within their territory, with a corresponding reduction in central transfers. Furthermore, Law no. 42, approved on May 5, 2009, opened the way to the introduction of 'fiscal federalism' in Italy. Hence, from 2009 onward, the fiscal rules applying to local governments have been frequently changing, 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 of the VAT revenue (only for 2011), (b) modifications of the equalization system and of the structure of vertical transfers from the central government, (c) the introduction in 2011 of a reformed property tax (*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-sectional features of the dataset. Italian municipalities are affected by many legislative thresholds based on population. The salary of the mayor, of the members of the executive committee and of the councilors, the size of the city council and of the executive committee, the electoral rule, whether or not a municipality can have additional elective bodies in its districts, and whether or not a municipality can host hospital facilities or organize a healthcare district. These are all policy assignments that vary with population size, vertical transfers from the central government change proportionally with the size of population (Law 504/1992). Finally, municipalities below 5000 inhabitants are exempted from having to comply with a set of rules imposed by the national government to municipalities in order to improve their fiscal discipline (the Domestic Stability Pact).

The presence of these overlapping policies, differentiated on the basis of population brackets, creates concerns about identification as they affect policy decisions at the local level. To begin with, 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 and those 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, which rely on runoff elections, have on average a larger number of candidates and less volatile tax rates, compared to municipalities just below 15,000 inhabitants, which instead rely on single round elections. Ferraresi et al. (2015) show that taxes and expenditure in municipalities with runoff elections are on average lower than those in municipalities with single elections, but only if the mayor of the former type of municipality does not need a broad coalition to be elected.

Therefore, to clearly identify our effect it is crucial to find a population threshold not influenced by the presence of these overlapping policies. For example, if we used the population threshold from 0 to 5000 inhabitants, our outcome variables would be affected, simultaneously, by the change in the level of transfers from the central government and by the salary of the major, whose effect cannot be dismissed, thereby leading to biased estimates. In a similar vein, if we considered the interval from 3,000 to a threshold greater than 5000 inhabitants, we would have other confounding policies, such as the Domestic Stability Pact, and the change of its rules over the period 2002–2008. Moreover, we would have the change in the electoral system at the 15,000 inhabitant threshold, together with the change in the number of seats in the City council, in the neighbor council and in the reimbursement of council members. Consequently, we restrict our analysis to municipalities with a population ranging from 3000 to 5000 inhabitants, which are not affected by any overlapping policies.

We finally note that in Italy there are Regions and Provinces ruled by special statutes, which in force of their special autonomy are allowed to set their own fiscal rules and transfer policies for their municipal governments.[10]

Summing up, there are several institutional settings and policies that change at different population thresholds, and that differ between municipalities belonging to ordinary and special statutes Regions and Provinces. Since these different institutional settings and policies might confound the empirical assessment of the impact on local policy choices of the 2008 reform, in order to properly identify its effect we restrict our sample to municipalities belonging to Regions ruled by ordinary statutes, with a population ranging from 3000 to 5000 inhabitants, over the period 2002–2008. Such restrictions assure that there are no other policy changes, structural reforms or different institutional settings that are relevant for the municipalities in the sample, apart from the 2008 reform we focus on.[11]

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, the Italian Statistical Office (ISTAT), and the National Association of Italian Municipalities (ANCI). It contains a full range of information for each Italian municipality organized into three sections: (1) financial data from balance sheets and data on property tax rates; (2) electoral data, including the results of the elections in which the mayors in office during the period covered by the dataset were elected; (3) demographic and socioeconomic data. As discussed in Sect. 3.2, in order to correctly identify the impact of the 2008 reform, we restrict the sample to municipalities belonging to Regions ruled by ordinary statutes, over the period 2002–2008, within the 3000–5000 population range (based on the 2001 Census of the population). Furthermore, we do not include municipalities

[10] Italy counts five Autonomous Regions: Sicily, Sardinia, Valle d'Aosta, Friuli Venezia-Giulia and Trentino Alto-Adige (the latter composed of two Autonomous Provinces: Trento and Bolzano). In 2008, of the 8101 Italian municipalities, 1299 belong to Autonomous Regions.

[11] In order to test whether the results are sensitive to the choice of the 3000–5000 population range, we compute separate estimates for samples of municipalities falling in the following ranges: (i) 0–3000 inhabitants; (ii) 5000–10,000 inhabitants; (iii) 10,000–15,000 inhabitants; (iv) 15,000–30,000 inhabitants and (v) above 30,000 inhabitants. The results indicate that our variable of interest (*pre-electoral year* × *reform*, see Eq. 1) is not statistically significant in any of the new population brackets. However, for all municipalities with population lower than 15,000 inhabitants—approximately 92% of the total—the sign of our variable of interest turns out to be the same as that found in the main specification (using the 3000–5000 population bracket) for all dependent variables. What this last evidence seems to suggest is that the sample restriction to the 3000–5000 population range essentially allows the effect to be better identified. Results of this analysis are available upon request.

under the administration of a government commissioner and municipalities whose mayor and city council resigned before the end of the term, thus leading to 'anticipated' elections. With these restrictions, and after excluding municipalities with missing values for some data, we obtain a sample of 733 municipalities including 5131 observations from 2002 to 2008.[12]

4.2 Dependent variables

As dependent variables, we use the per capita current expenditure (*current expenditure*) and, on the revenue side of municipal budgets, the per capita revenues of the three main sources of own revenues (apart from the property tax on main residence, abolished by the reform): 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, instead of tax rates and fares, is threefold. First, revenue is a financial variable expressed in the same units as, and comparable with, spending. Second, it would be very difficult to collect homogeneous and comparable rates for all kind of revenues we consider, especially for fees and charges. Third, revenues account for both tax rate effort and effort in administration and control of tax evasion, which are important complementary components of municipal fiscal policy.

As a preliminary piece of evidence, Table 1 compares, for each dependent variable, the average value in pre-electoral years with the average value in non-pre-electoral years, separately for the period 2002–2007 (before the reform) and for year 2008 (when the reform was introduced). As for *current expenditure*, Panel A shows that in 2008 the average per capita spending is higher in pre-electoral years (715.62 Euros) than in non-pre-electoral years (675.69 Euros), with a difference of 39.93 Euros, statistically significant at 5%. The same comparison for the years before 2008 gives instead a negative difference of − 23.90 Euros, significant at 1%. Notice finally that the difference in the differences (63.84 = 39.93 + 23.90) is statistically significant at 1%, suggesting that the reform introduced in 2008 may have prompted incentives—'dormant' before 2008—to expand expenditure one year ahead of elections.

A similar picture emerges for revenues. In 2008, average per capita revenues in pre-electoral years are higher than average per capita revenues in non-pre-electoral years: by 25.58 Euros (significant at 1%) for the *property tax on other dwellings* (Table 1, Panel B), by 12.11 Euros (significant at 1%) for the *surtax on personal income* (Panel C), by 33.65 Euros (significant at 5%) for *fees and charges* (Panel D). The difference between pre-electoral and non-pre-electoral years is instead non-significant for the years before 2008 for all revenue sources. Note finally that the

[12] Municipalities in the range of 3000–5000 inhabitants potentially provide 8442 observations (1206 municipalities for 7 years). Excluding municipalities belonging to special statutes Regions and Provinces (208), municipalities administered (even if only for a brief period) by a government commissioner (90), municipalities whose council resigned prematurely (26), and municipalities with incomplete or missing data (149), we end up with 733 municipalities in the sample, for a total of 5131 observations in the 7-year time span.

Table 1 Mean difference of dependent variables between pre-electoral and non-pre-electoral years, and between year 2008 (introduction of the reform) and years 2002–2007 (before the reform)

	Non-pre-electoral year (1)	Pre-electoral year (2)	Difference (3)		Non-pre-electoral year (1)	Pre-electoral year (2)	Difference (3)
Panel A: Current expenditure				Panel B: Property tax on other dwellings			
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			
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)

Number of observations: 5131 (733 municipalities with population between 3000 and 5000 inhabitants over the 7-year period 2002–2008). For the variable *property tax on other dwellings* data are available only for the period 2006–2008 (2199 observations) 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 onward. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

difference in the differences—comparing the pre-electoral year effect in 2008 with that before 2008—is statistically significant at 1% for the *surtax on personal income* (12.71 Euros) and at 5% for *fees and charges* (36.57 Euros), while it is not significant for the *property tax on other dwellings* (28.03 Euros).

What this simply suggests is that the reform seems to have activated political budget cycles at the local level. Our aim is to verify through a more rigorous econometric analysis whether this indication emerging from a coarse comparison of average spending and revenues is empirically robust.

4.3 Treated and control municipalities

The staggered timing of the Italian municipal elections—illustrated in Sect. 3.1—determines, over the period 2002–2008, a random assignment of municipalities into two groups: (i) those holding one election before, and one after, the 2008 reform, and (ii) those holding one or two elections before the reform. This exogenous assignment can be used to allocate the 773 municipalities in the dataset to a treated and a control group. Table 2, reporting the frequency of elections at different years, shows that there are 506 municipalities (69% of the total) holding elections in 2004 and 2009.[13] These municipalities represent our treated group, since one pre-electoral year (2003) falls before the reform while the other one (2008) falls in the same year of the reform.[14] All the other 227 municipalities form the control group, since all their pre-electoral years (one or two) fall before 2008.

It is important to note that we have excluded from the dataset not only 23 municipalities holding at least one 'anticipated' election (i.e., two elections within 5 years) over the period 2002–2008, but also three municipalities holding an anticipated election in 2009, because the treatment would not be exogenous to potential outcomes, were these municipalities included in the treatment group together with municipalities holding regular elections in 2009.[15]

4.4 Socioeconomic and demographic controls

The dataset includes also some time-varying control variables that account for differences among municipalities in their population structure and economic conditions. The demographic controls include total population (*population*) and population density (*density*), expressed by population per square kilometer of municipal territory; these variables can capture the presence of scale economies in the provision of public goods. We also include the share of population aged between 0 and 5 (*child*) and the share of population over 65 (*aged*), to account for some specific age-related public needs such as nursery schools, nursing homes, and so on.

[13] We checked that all the 506 municipalities actually held elections in 2009.

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

[15] The same point is made by Akhmedov and Zhuravskaya (2004), who argue that moving elections away from the originally scheduled date creates concerns about identification.

Two additional controls are the municipal per capita income (*income*), proxied by the per capita base of the personal income tax, and the per capita transfers from the upper levels of government (*transfers*). Finally, we set a dummy variable (*election*) equal to one for each election year during the period 2002–2008. The summary statistics, data description and data sources of all the variables used in the analysis are reported in the Online Appendix, Tables A.1 and A.2.

5 Empirical analysis

5.1 Econometric specification

Our estimation approach is based on a Difference-in-Difference (DiD) framework whose baseline specification can be expressed as follows:

$$\begin{aligned} Y_{it} = {} & \gamma_1 \, \textit{pre-electoral year}_{it} + \gamma_2 \, \textit{pre-electoral year}_{it} \times \textit{reform}_t \\ & + \beta' X_{it} + \alpha_i + \tau_t + \lambda \, \textit{Trend}_{it} + \varepsilon_{it}, \end{aligned} \tag{1}$$

where Y_{it} is one of the four dependent variables described in Sect. 4.2 for municipality i at time t. As for the explanatory variables, *pre-electoral year* is a dummy variable equal to 1 in the year before an election and 0 otherwise, *reform* is a dummy variable equal to 1 in the year 2008 and zero otherwise, X_{it} is a vector including the control variables described in Sect. 4.4. To take account of unobserved heterogeneities across municipalities, we include a set of municipality fixed effects, α_i, while to control for exogenous shocks that can equally affect both treated and control groups we add year fixed effects, τ_t. Since 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, we control for any potential temporal pattern independent of the treatment status by including a complete set of municipality-specific linear time trends $\textit{Trend}_{it}$. Finally, ε_{it} is the error term, clustered at the municipal level.

Under the specification in Eq. (1), the coefficient γ_1 accounts for the impact on the policy outcome Y_{it} of being in a pre-electoral year before 2008, while γ_2 captures the differential effect, with respect to γ_1, of being in a pre-electoral year in 2008, when the reform was introduced.

5.2 Results

For each dependent variable, Table 3 presents the DiD estimates of the full specification of our model shown in Eq. (1).[16] As for current expenditure (col. 1), we find that the coefficient estimate of *pre-electoral year* × *reform* is positive and

[16] Panel A of Tables A.3, A.4, A.5 and A.6 of the Online Appendix report, for each dependent variable, a complete set of estimates of Eq. (1), starting with the simple OLS specification and then adding up year fixed effects, municipality fixed effects, municipality-specific time trends, and finally municipal controls, which is the full specification as in Table 3.

Table 2 Timing and frequencies of elections

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

733 municipalities with population between 3000 and 5000 inhabitants over the 7-year period 2002–2008. Roman numbers represent the years to the following election; i.e., E = election, IV = four years to the following election, III = three years to the following election, II = two years to the following election, I = one year to the following election. The number of municipalities is shown in parentheses

statistically significant at 5% level. The value of the estimated coefficient suggests that the per capita *current expenditure* of municipalities that were in a pre-electoral year in 2008 is, *ceteris paribus*, 19.04 Euros higher compared to what it would have been in the absence of the reform, an absolute amount corresponding to about a 3% increase with respect to the average value of 632.07 Euros.

Turning to the revenue side of the budget, we find that the coefficient of *pre-electoral year* × *reform* is not statistically different from zero either for the revenue of the *property tax on other dwellings* or for that of the *surtax on personal income* (Table 3, col. 2 and 3), while it is positive and statistically significant at 5% level for the revenue from *fees and charges* (col. 4). The value of the coefficient shows that the per capita revenue from *fees and charges* of municipalities that were in a pre-electoral year in 2008 is, on average, 17.75 Euros higher compared to what it would have been in the absence of the reform, which amounts to an approximately 10% increase with respect to the average value of 176.69 Euros.

These findings indicate that the structural change of the revenue side of municipal budgets brought about by the 2008 reform—the substitution of own revenues from property taxation with transfers from the central government—switched on political budget cycles at the local level in the form of current expenditure expansions in the year before the elections mainly financed through increases in revenues from fees and charges.

Our interpretation of the result is that the primary channel through which the reform prompted incentives for expanding the budget in pre-electoral years was by

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)
Year FE	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES
Municipal controls	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

733 municipalities with population between 3000 and 5000 inhabitants over the 7-year period 2002–2008. *pre-electoral year* is a dummy variable equal to one in the year before the election and zero otherwise; *reform* is a dummy variable equal to 1 in year 2008 and zero otherwise. In col. (2) the number of observations is 2199 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings is recorded in Italian municipal budgets only from 2006 onward. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

lowering the costs for financing expenditure increases with own revenue sources.[17] Before the 2008 reform, the three main sources of own revenues of Italian municipalities were the property tax on main residence, whose burden is entirely borne by residents who are also voters at the local level, users' fees and charges, whose burden is mostly borne by residents directly benefiting from municipal services, and the property tax on additional residence, whose burden is in some cases mostly borne by non-residents, as in municipalities located in touristic areas.[18] Since taxing the main residence is highly unpopular,[19] before 2008 most municipalities applied tax rates on main residence just above the minimum level imposed by the central government, while they exerted more effort in the taxation of additional residences and in

[17] The interpretation provided in the text is derived from a simple theoretical model that we present in an Online Appendix.

[18] Recall that our analysis considers also a fourth source of own revenues, namely, the municipal surtax on the personal income tax, whose revenues are, on average, one sixth of those of fees and charges. Both before and after the 2008 reform, there is no evidence of political cycling financed with the surtax on the personal income tax, presumably because the surtax is a much more salient levy than fees and charges.

[19] Unfairness is the main argument against taxation of main residence, as the latter is seen as the primary component of a household's wealth acquired after years of savings. Justifications based on the benefit principle—as municipal services contribute to the value of housing properties—or on political accountability—as taxing their voters makes policy makers responsible for how they spend tax revenues—are largely outweighed by the unfairness argument.

the determination of fees and charges, though the room to rely heavily on the latter was narrow, since residents were already taxed on main residence.[20] Under these conditions, local administrators had little capacity of making leverage on own revenues to finance expenditure expansions for electoral purposes.[21] The 2008 reform changed the setting. By relieving local administrators from having to impose a burden on residents on their main property, and by substituting the revenue loss with a compensating transfer, the reform considerably reduced the political costs incurred by local administrators to make leverage on user fees and charges to finance pre-electoral expenditure hikes.[22]

5.3 Robustness checks

In this section, we assess the validity of the previous results by performing a set of robustness tests. First, we verify the hypothesis of random assignment to treated and non-treated groups through the matching approach. Second, we check that the results are not influenced by the amounts of the compensating transfers, which do not match one to one, as described in Sect. 2, the revenue of the abolished tax on main residence. Third, we verify whether the results could be invalidated by municipalities acting strategically in anticipation of the reform with the purpose of influencing the value of the compensating transfer. Fourth, we check that the control variables are not themselves affected by the reform.

Recall from Sect. 3.2 that, as part of our identification strategy aimed at avoiding that other policy interventions overlapping with the reform we focus on might have influenced local budget decisions, we restricted the analysis to municipalities belonging to Regions ruled by ordinary statutes, with population within the 3000–5000 range, over the period 2002–2008. These restrictions notwithstanding, a source of potential concern is that the group of treated municipalities might differ from the control group in some relevant characteristics, thus undermining our 'random assignment' hypothesis of the treated status. To address this issue, we apply the matching approach, which consists of matching municipalities in treated and control groups through a set of observable characteristics. To do so, we select from the 2001 Census a set of variables that might affect both the treatment and outcome variable,

[20] Unfairness is not an issue for the taxation of additional residences for the obvious reason that owners of more than one dwelling are considered richer than single property owners. As for fees and charges, there is a clear link between the payment due and the benefits in terms of services provided, while for the tax on main residence the link is weaker. Moreover, fees and charges are much less visible to voters with respect to property taxes, as they are collected several times during the fiscal year in amounts that are usually relatively small (Bracco et al. 2013).

[21] Italian municipalities cannot rely on debt for financing current expenditure and are subject to strict limitations for financing capital spending.

[22] Note that our analysis is not about the impact of the reform on the overall incentives to expand expenditure as a consequence of the substitution of own revenues with vertical transfers, in the spirit of the well-known literature on the flypaper effect (see, e.g., Dahlby 2011, for a theoretical analysis). Rather, our focus is on the differential impact of the reform in pre-electoral versus non-pre-electoral years, which is about political budget cycles.

and then use them as controls in a logit regression.[23] In particular, the set of controls includes: population (*population*); a categorical variable (*altimetry zone*) equal to 1 if the municipality is located in plain, equal to 2 if it is located in hills, and equal to 3 if it is located in mountains; the share of population aged over 65 (*aged*); the share of population aged below 5 (*child*); population density (*density*); per capita income (*income*); per capita grants from upper levels of government (*transfers*); number of families (*families*), houses (*houses*) and firms (*firms*) over total population; unemployment rate (*unemployed*); average altitude level of the municipal territory (*altitude*).

From the fitted values of the logit regression we obtain the propensity score variable, which is then used to determine the common support, excluding all observations with values lower than the first percentile and higher than the ninety-ninth percentile range of the propensity score variable. Finally, we match the sample of treated to a comparable sample of non-treated municipalities, by linking each municipality only to its 'nearest neighbor'.[24] This matching procedure reduces the sample to 667 municipalities and, within this sample, there are no significant differences between the matched groups of treated and control municipalities in terms of observable characteristics (details in the Online Appendix, Table A.8). Moreover, the distributions of the estimated propensity score for the treated group and for the control group overlap (see Fig. 1), meaning that it is possible to obtain a valid inference, since for each treated municipality there exists a control municipality with similar characteristics (Wooldridge 2010). The results in Table 4, replicating the analysis reported in Table 3 for the subsample of matched municipalities, show that all the results, in terms of both the size and the statistical significance of the estimated coefficients, are fully confirmed.[25]

As a second check, we check whether the results are driven by the amount of compensating transfers received by municipalities from the central government. In fact, as described in Sect. 2, in 2008 and in subsequent years each municipality received a transfer whose amount was determined not only by the revenue loss from the abolished property tax on main residence but also by some indicators of fiscal performance, with the result that there is variation among municipalities in the difference (generally negative) between the amount of the compensating transfer and the amount of the tax revenue loss. It is therefore important to verify that our results are not driven by the different degrees of coverage of tax revenue losses by the compensating transfers.

To deal with this issue, we build the variable *icigrants*, equal to the per capita revenue of the property tax on main residence for the years 2006 and 2007 and to

[23] We follow the approach in Sianesi (2004) and Smith and Todd (2005) and perform the matching analysis by using the Stata command *psmatch2* developed by Leuven and Sianesi (2010). Details are available in the Online Appendix, Table A.7.

[24] Beside the nearest-neighbours matching algorithm, we used the kernel matching algorithm (though recently criticized by King and Nielsen 2016), under which four municipalities lie outside the common support, and found no significant changes in the result (details are available upon request).

[25] Results considering the other specifications listed in footnote 16 are reported for each dependent variable in Panel B of Tables A.3, A.4, A.5 and A.6 of the Online Appendix.

the per capita value of the compensating grant for the year 2008. We then look at the mean difference of this variable between the reform year (2008) and the two pre-reform years (2006 and 2007), finding that (see Table 5) it is equal (in per capita terms) to − 12.40 Euros for the control group and to − 17.68 Euros for the treated group, both statistically significant at 1%, meaning that both groups of municipalities have cashed, on average, compensating transfers of amounts lower than the revenue previously collected through the property tax on main residence. However, the difference of the differences is not statistically significant, indicating that the reform, while not fully compensating municipalities for the tax revenue loss, did not systematically differentiate between our treated and control units.

As a further check, we replicate the estimations of Eq. (1) reported in Table 3 using *icigrants* as the dependent variable.[26] Were the coefficient of *pre-electoral year* × *reform* significant, it would mean that municipalities in a pre-electoral year in 2008 were granted amounts of compensating transfers significantly different from those granted to the other municipalities, making it impossible to separate the impact of the reform on budget decisions due to the structural change in the composition of municipal revenues from that due to the change in the amount of resources to finance expenditure. However, the estimated coefficient of the variable *pre-electoral year* × *reform* is not statistically different from zero, both for the whole sample (Table 6, col. 1) and for the sample of matched municipalities (Table 6, col. 2), strongly indicating that the observed increase in expenditure and revenue from fees and charges for municipalities in a pre-electoral year in 2008 (Table 3) is not due to the amount of grants received from the central government as a compensation of the missing revenue from the property tax on main residence.[27]

Another element that needs careful consideration to test the robustness of our results is the possibility that local administrators changed their behavior in anticipation of the reform. Since it was clear, in 2007, that the left-wing government headed by Romano Prodi—supported by a tiny and unstable majority in the Parliament—would not have lasted until the end of the legislature, and that in case of anticipated elections the right-wing coalition guided by Silvio Berlusconi would have taken power and honored the promise of abolishing the tax on main residence, it is possible that municipalities with elections scheduled in 2009, anticipating the reform, could have strategically decided in 2007 to increase taxation on main residences in order to obtain a larger compensating transfer from 2008 onward. Clearly, if this is the case, our results could be seriously biased, since the observed increase in the 2008 expenditure of municipalities holding elections in 2009 would not be due to the reform itself but, instead, to its anticipation by local administrators.

To test for anticipatory effects of the reform, we collect data about the tax rates applied by municipalities on main and on additional residence for the years 2006

[26] Since the variable *icigrants*, running from 2006 to 2008, contains the per capita tax revenues on main residence for years 2006–2007, and the per capita compensating transfers for year 2008, to avoid duplications the control variable *transfers* is redefined net of the compensating transfers for year 2008.

[27] Results considering the other specifications listed in footnote 16 are reported in Table A.9 of the Online Appendix.

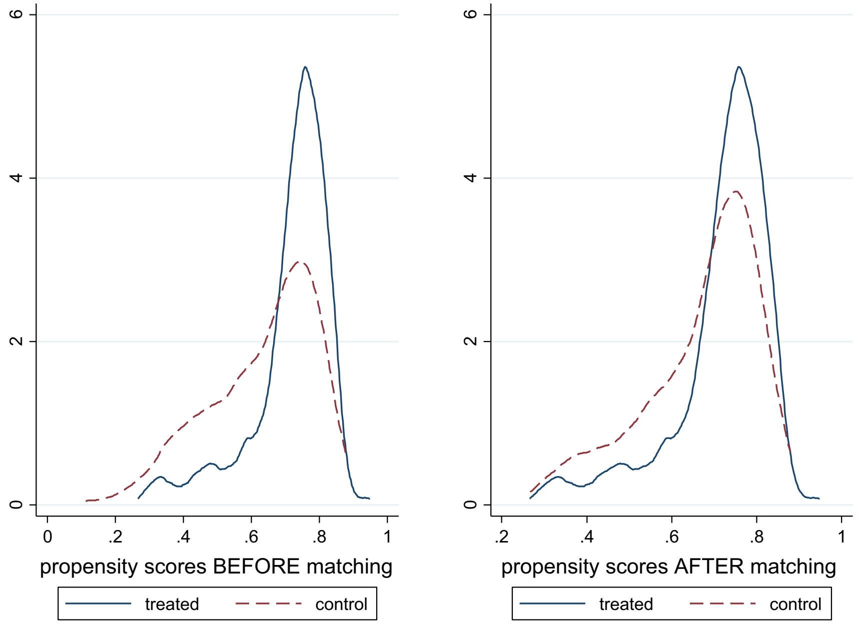

Fig. 1 Propensity score in treated and control group, before and after implementing the matching procedure. The figure presents the distribution of the estimated propensity score between treated and control municipalities, before and after the matching procedure. For the matching procedure, we use the "nearest neighbor" approach as explained in Sect. 5.3

and 2007.[28] We then compare the average change from 2006 to 2007 of the *tax rate of the property tax on owner-occupied dwellings* of municipalities in the treatment group with that of municipalities in the control group. Were the difference of these differences positive and significant, there would be a clear indication that municipalities holding elections in 2009 inflated their property tax in 2007 in anticipation of the 2008 reform with the purpose of cashing a more generous compensating transfer in 2008, thus invalidating the conclusion that the 2008 expenditure expansion by municipalities in their pre-electoral year is due to a political budget cycle. The results of the analysis on tax rates show that the difference (between treated and control municipalities) of the differences (year 2007 less year 2006) in the *tax rate of the property tax on owner-occupied dwellings*, equal to – 0.02, is not statistically significant (see Table 7, Panel A), suggesting that we can rule out strategic anticipations of the reform. The same result emerges for the *tax rate of the property tax on other dwellings* (Table 7, Panel B).

As a final robustness test, in the spirit of the work by Pei et al. (2017), we estimate our model putting, in turn, each control variable as the dependent variable, in order to verify that—by not being affected by the reform—they are truly exogenous. Indeed, this is what emerges from the results shown in Table 8, since the estimated

[28] Data on tax rates are missing for 168 municipalities, resulting in 336 missing observations.

Table 4 Policy outcomes results in 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)
Year FE	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES
Municipal controls	YES	YES	YES	YES
Observations	4669	2001	4669	4669
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

733 municipalities with population between 3000 and 5000 inhabitants over the 7-year period 2002–2008. *pre-electoral year* is a dummy variable equal to one in the year before the election and zero otherwise; *reform* is a dummy variable equal to 1 in year 2008 and zero otherwise. In col. (2) the number of observations is 2001 since the distinction between revenue from property tax levied on owner-occupied dwellings and revenue from property tax levied on other dwellings is recorded in Italian municipal budgets only from 2006 onward. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

Table 5 Mean differences 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)

2199 observations: 733 municipalities with population between 3000 and 5000 inhabitants over the 3-year period 2006–2008. Number of treated municipalities: 506; number of control municipalities: 227. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

coefficient of *pre-electoral year* × *reform* is not statistically significant for all control variables, both in the whole sample (Panel A) and in the sample of matched municipalities (Panel B), with the exception of *income* in the whole sample, where it is negative (−78.13) and significant at 5%. However, the fact that the sign of the

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)
Year FE	YES	YES
Municipality FE	YES	YES
Municipal time trend	YES	YES
Municipal controls	YES	YES
Observations	2199	2001
Number of municipalities	733	667
Treated municipalities	506	502
Control municipalities	227	165
R-squared within	0.57	0.57

733 municipalities with population between 3000 and 5000 inhabitants over the 3-year period 2006–2008. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

corresponding estimated coefficient in the regressions with expenditure and revenues as dependent variables is positive—and thus pointing to the opposite direction—reassures that there is no serious endogeneity problem for the control variable *income*.

6 Heterogeneous effects

Politicians with stronger re-electoral concerns might have incentives to set different fiscal policies (see, among others, List and Sturm 2006; Bordignon et al. 2017). Thus, the 2008 reform might have exerted a different impact on budget decisions taken by second-term term-limited mayors and first-term mayors.[29] To investigate whether there has been a heterogeneous response we build the *termlim* dummy variable, equal to one if the mayor in office is at her second mandate and zero otherwise, and then interact it with both *pre-electoral year* and *pre-electoral year* × *reform* in a triple-difference model. Hence the model we estimate, which is a generalized version of that in Eq. (1), takes the following form:

[29] The Italian municipal electoral system establishes a limit of no more than two consecutive mandates for the office of mayor.

Table 7 Estimates of fiscal reform on tax rates

Average tax rates							
Panel A: Property tax on owner-occupied dwellings				Panel B: Property tax on other dwellings			
	Treated group	Control group	Difference (Treated − Control)		Treated group	Control group	Difference (Treated − Control)
	(1)	(2)	(3)		(1)	(2)	(3)
2006	5.29	5.27	0.02	2006	6.32	6.16	0.16**
			(0.01)				(0.22)
2007	5.23	5.23	0.00	2007	6.36	6.21	0.15**
			(0.06)				(0.22)
Difference (2007 − 2006)	− 0.06***	− 0.04***	− 0.02	Difference (2007 − 2006)	0.041***	0.05***	− 0.01
	(0.01)	(0.01)	(0.012)		(0.01)	(0.02)	(0.02)

Number of observations: 1130 (565 municipalities) over the period 2007–2008. Municipalities with population between 3000 and 5000 inhabitants. Treated municipalities (395) are those holding the election in 2009. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

Table 8 Placebo test

Variables	Panel A: Whole sample						Panel B: Sample of matched municipalities					
	Pop	Child	Density	Income	Aged	Transfers	Pop	Child	Density	Income	Aged	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
pre-electoral year	1.57	0.00	0.31	23.69*	0.00*	4.34	1.95	0.00	0.41	20.34	0.00	2.39
	(1.53)	(0.00)	(0.23)	(13.32)	(0.00)	(3.01)	(1.78)	(0.00)	(0.27)	(15.17)	(0.00)	(3.58)
pre-electoral year ×*reform*	− 0.20	− 0.00	0.16	− 78.13**	− 0.00	3.38	2.66	− 0.00	0.22	− 24.02	− 0.00	− 3.46
	(4.56)	(0.00)	(0.75)	(31.21)	(0.00)	(8.70)	(5.08)	(0.00)	(0.94)	(33.55)	(0.00)	(10.83)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipal controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	5131	5131	5131	5131	5131	5131	4669	4669	4669	4669	4669	4669
Number of municipalities	733	733	733	733	733	733	667	667	667	667	667	667
Treated municipalities	506	506	506	506	506	506	502	502	502	502	502	502
Control municipalities	227	227	227	227	227	227	165	165	165	165	165	165
R-squared within	0.96	0.87	0.94	0.82	0.87	0.64	0.95	0.87	0.93	0.82	0.88	0.66

733 municipalities with population between 3000 and 5000 inhabitants over the seven-year period 2002–2008. *pre-electoral year* is a dummy variable equal to one in the year before the election and zero otherwise; *reform* is a dummy variable equal to 1 in year 2008 and zero otherwise. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

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

Our variables of interest are *pre-electoral year* × *reform* and *pre-electoral year* × *reform* × *termlim*. The former captures the differential impact, for first-term mayors, of having a pre-electoral year in 2008 with respect to having it in other years. The latter accounts for the differential impact, for second-term mayors, of having a pre-electoral year in 2008 with respect to first term mayors.

Our estimates in Table 9 show that, for the whole sample, the coefficient of *pre-electoral year* × *reform* is positive (22.37 Euros, in per capita terms) and statistically significant at 5% with *current expenditure* as the dependent variable, while that of *pre-electoral year* × *reform* × *termlim* is not statistically significant. This means that municipalities having a pre-electoral year in 2008 increased their current expenditure, regardless of the status—first or second term of office—of their mayor. The result is the same for the sample of matched municipalities. As for revenues from *fees and charges*, we find that, for the whole sample, the coefficient of *pre-electoral year* × *reform* is positive (27.43 Euros) and statistically significant at 1% while that of *pre-electoral year* × *reform* × *termlim* is negative (– 30.02 Euros) and statistically significant at 5%, indicating that municipalities governed by first-term mayors behave differently from municipalities governed by second-term mayors. Note, however, that the impact of having a pre-electoral year in 2008 for municipalities with second-term mayors amounts to 27.43 – 30.02 = –2.59, which is not statistically different from zero ($p = 0.826$). What these results simply reveal is that municipalities that were in a pre-electoral year in 2008 increased revenues from fees and charges only if the mayor was at her first term of office, while there is no effect on revenues from fees and charges for municipalities led by mayors in their second term of office. Again, the results are the same for the sample of matched municipalities.[30]

Finally, we do not find any significant impact of the reform on the revenues from the *property tax on other dwellings* and the *surtax on personal income*, both for municipalities with term-limited mayors and for municipalities with non-term-limited mayors.[31]

[30] The impact of having a pre-electoral year in 2008 for municipalities with second-term mayors amounts to 29.98 – 30.53 = –0.55, which is not statistically different from zero ($p = 0.964$).

[31] Results considering the other specifications listed in footnote 16 are reported for each dependent variable and each sample in Tables A.10, A.11, A.12 and A.13 of the Online Appendix.

Table 9 Policy outcomes results and term limit

	Panel A: Whole sample				Panel B: 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)	(1)	(2)	(3)	(4)
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 × *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)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal time trend	YES	YES	YES	YES	YES	YES	YES	YES
Municipal controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	5131	2199	5131	5131	4669	2001	4669	4669
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

733 municipalities with population between 3000 and 5000 inhabitants over the 7-year period 2002–2008. *pre-electoral year* is a dummy variable equal to one in the year before the election and zero otherwise; *reform* is a dummy variable equal to 1 in year 2008 and zero otherwise; *termlim* is a dummy variable equal to one if the mayor is at her second mandate and zero otherwise. In all regression we control for *termlim*× *reform* and *termlim*. Robust standard errors, clustered at the municipal level, are shown in parentheses: ***Significant at 1%; **Significant at 5%; *Significant at 10%

7 Conclusion

In this study, we investigate the impact of the reform introduced by the Italian government in 2008, which abolished the municipal tax on main residence and replaced it with a compensating vertical transfer. Our analysis shows that the reform gave incentives to municipalities having a pre-electoral year in 2008 to expand their current expenditure and revenues from fees and charges. That is, the reform prompted incentives for political budget cycles at the local level which were absent before the reform. This finding suggests that in a centralized setting—the reform changed the structure of the revenue side of municipal budgets, increasing the share of vertical transfers and decreasing that of own revenues—local governments have stronger incentives for political budget cycles than in decentralized settings. Our explanation is that centralization lessens the political costs faced by local administrators to finance pre-electoral expenditure hikes through own revenues.

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