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EVIDENCE FROM ITALIAN CITIES

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ABSTRACT

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Political Budget Cycles: Evidence From Italian Cities*

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Abstract

The introduction of a new real estate tax in Italy in 2011 created a well designed natural experiment to test the strategic choice of fiscal variables (a tax rate) in relation to elections. We find substantial evidence of "political budget cycles", with municipalities choosing lower tax rates when close to elections. The evidence on political budget cycles is stronger in localities in the South of Italy. The well documented lower level of "social capital" in this region may account for less attention and lower control of politicians. Cities with large preexisting deficits did not set lower rates before elections, presumably because the deficit was a salient political problem and incumbents did not want to look as aggravating it.

JEL Codes: E62, H3, H7.

1 Introduction

Do governments strategically manipulate fiscal policy tools in order to win elections, for instance reducing taxes and/or increasing spending close to elections? The empirical evidence is far from a clear cut. At the national level certainly one can observe celebrated specific episodes (see Tufte (1978)). For other countries work by Persson and Tabellini (2002) and

Brender and Drazen (2005), as we discuss below, show that political budget cycles occur only in certain countries under special circumstances. At the local level there is some evidence of budget cycles on the spending side, in particular for what concerns public investments (Khemani (2000) and Drazen and Eslava (2003)).

In this paper we present evidence on Italian cities. A recent policy development in Italy allows for a very clean test of strategic electoral choice regarding fiscal policy and elections. In late 2011 the national government introduced a new real estate tax allowing cities some flexibility on the rate. This new policy was very salient, discussed at length in the Parliament and on the press. In fact, given the large share of wealth in real estate held by Italians, it was a major policy issue. Italian cities for a variety of reasons have staggered terms of elections.[1] Thus every year there is a group of municipalities holding elections. This feature allowed us to test whether the tax rate chosen was affected by the distance from the next election.

Our results show clear evidence of "political budget cycles", namely municipalities chose lower tax rates when close to elections. The result is robust to a host of controls.

Furthermore, we find evidence of stronger cycles in the South of Italy and in smaller cities. The result about the South is intriguing. A vast literature (Putnam et al (1993), Guiso et al. (2103) Tabellini (2008) amongst others) has pointed out how the level of trust, civicness, participation in social and political activities is substantially lower in Southern Italy than in the rest of the country. Nannicini et al (2013) argue that lower social capital implies lesser control of politicians. Therefore, the latter may feel less constrained in pursuing strategic manipulation of policies. These results confirm the early intuition by Banfield (1958) in his study of a Southern village in the fifties in Italy. Our results are consistent with these arguments.

The political budget cycle is however smaller in cities which had larger deficits. We also find that left wing government set a lower tax than right wing one, particularly on the main residence. Also in the year 2012, when some localities could choose to decide the level of the tax before or after the election which took place in May, we find a tendency to choose the tax before. This may be consistent with an attempt to convince the voter that a non excessive tax rate would be chosen immediately after the election. In line with this view, we find that the tendency to anticipate the deliberation is even stronger in contested municipalities (i.e. those

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[1] The reason is that all Italian cities voted for the first time in 1946 . However, in many municipalities the process of renewal of the municipal councils did not follow the regular five-year cycle. The most common reasons were the fall of the government and political scandals that forced the governments of some municipalities to resign prematurely.

where previous elections were won with low margins of majority).

The literature on political budget cycles is vast. The theoretical underpinning is due to Rogoff and Sibert (1988) and Rogoff (1990) in particular; they show how these cycles may persist even with rational voters if the latter are imperfectly informed about the complexities of the government budget and if they learn with a delay the full set of fiscal variables in play. As for the empirical evidence, at the national level the literature includes both papers on cross-country samples and papers focusing only on one country. In the first group Alesina et al. (1997) find evidence of cycles in the aggregate balance, but no evidence of cycles in single budget components for a sample of 13 OECD economies over the period 1960-1993. Shi and Svensson (2002) employs a panel of 123 countries in the years 1975-1995 and shows some evidence of budget cycles in developing countries. Persson and Tabellini (2002) in a sample of 60 democracies over the period 1960-1998 find a revenue cycle, but no political cycle on spending and transfers. They also find that, while all democratic systems display cycles before elections, only presidential systems show evidence of fiscal adjustments after elections. Brender and Drazen (2005) find political budget cycles only in new democracies.[2] Several papers also look at national level cycles for specific countries.[3]

Our work is closer to another branch of literature that focuses on the subnational level, which has some advantages. First, the study of local governments in the same country allows to hold constant a series of cultural and institutional characteristics that can potentially threaten the identification of causal effects in a cross-country analysis. Second, local governments can easily target policies to special interest groups or voters and this makes political budget cycles more likely to arise. Third, as we show in this paper, policies designed by the central government and imposed on all local governments can be a good natural experiment.

Akhmedov and Zhuravskaya (2004) test the theory of political budget cycles in Russian provinces showing evidence of an increase of monetary transfers to voters before elections. In order to achieve identification they exploit the orthogonality of election timing to implemented policies. However, as they point out, part of their sample moves elections away from the originally scheduled date creating concerns about the identification of the causal effect of interest.

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[2]Other works on a cross-section of countries are Ames (1987) that shows the presence of budget cycles in 17 Latin American countries, Block (2002) finding evidence of political budget cycles in both monetary and fiscal policy in 44 Sub-Saharan countries and Schuknecht (1996) that focuses on 35 developing countries over the years between 1970-1992.

[3]Ben-Porath (1975) has evidence on budget cycles on the revenue side in Israel; Krueger and Turan (1993) in Turkey and Gonzales (1999) for Mexico. Keech and Pak (1989) find a cycle for veteran benefits in the United States and Alesina Cohen and Roubini (1992) show a cyclical behavior of transfers over GNP in the United States between 1961 and 1985.

Khemani (2000) show that electoral budget cycles affect the composition of local budgets.[4] Evidence of local political budget cycles is also found in the work of Reid (1998) and Kneebone and McKenzie (2001) about Canadian provinces. Drazen and Eslava (2003) brings descriptive evidence of a significant increase of investments prior to elections in local governments in Colombia, an increase which is only partially compensated by a decrease in government consumption.[5]

Our work has some potential advantages. Unlike previous papers we focus on actual policy changes by looking at a specific tax rate. All past works focused on changes in aggregate quantities (tax revenues or total spending) that could likely be influenced by the cycle or many other unobservable factors. Obviously previous paper try to control for some cycle-linked variables to isolate the discretionary change, but the identification is always debatable.[6]

The present paper is organized as follows. Section 2 briefly describes the institutional setting. Section 3 describes our identification strategy, and section 4 the data. Section 5 and 6 present our results on the effect of elections on the tax rate and on the timing of choices relative to elections. The last section concludes.

2 Institutional Background

The IMU ("Imposta municipale unica", unique municipal tax) is a property tax introduced in Italy in late 2011 on all 8092 municipalities. It was very salient and followed an acrimonious debate in Parliament which had an extensive coverage in the press. Unlike the preexisting real estate tax (called ICI), the IMU applied also the main house. About 50% of the IMU tax revenue was transferred to the central government, while the remainder represented the main revenue source in municipal budgets, in addition to the transfers from the central government. IMU represented around the 50% of the total revenues of municipalities. Moreover, the property tax is a very important issue in public debate given that in Italy the 61.9% of households own their

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[4]For the reasons that threaten the identification in Akhmedov and Zhuravskaya (2004) his identification had to rely on instruments.

[5]Eslava (2005) confirms these findings underlining the change in the composition of public spending in local budgets, that is not followed by an increase in the total deficit.

[6] Another paper has already used the "natural experiment" of a property tax in Italy to investigate other political economy model. Bordignon and Turati (2014) show that after the introduction of a previous real estate tax in Italy in 1993, which allowed municipalities some degree of fiscal independence, the quality of politicians improved for rich municipalities.

residence[7] and real estate investments represented in 2010 the 52% of total Italian households' assets with a ratio of housing wealth over income of 5.6 against the 5.1 of France and United Kingdom and the 2.1 of United States (Neri and Monteduro (2010) and Banca d'Italia (2011)).

The national government set a default rate for each category of houses, (main residence, additional ones and rural property) with a range of discretion for the municipalities. The default rate for main residences is 0.4% with a window between 0.2% and 0.6%; rural buildings have a default tax rate of 0.2% that can be decreased to zero; additional residences have a default rate of 0.76% which can be moved in the interval 0.46%-1.06%. The tax base on which rates are applied depends on a formula based upon the book value of the property. Each municipality had to define tax rates for 2012 through a municipal council deliberation. In case a municipality did not deliberate, the default rates applied. The central government imposed a deadline for the deliberation that was first scheduled for September 30th and then moved to October 31st; the deadline for tax payment was set on the 17th of December.[8] The total tax revenue raised with IMU in 2012 was 23.7 billion euros, paid by 25.8 million of taxpayers.

3   Identification Strategy

We want to estimate the causal effect of having elections on an outcome that we call Y and it will be tax rates. Our treatment is denoted by E and takes value 1 in cities where elections are held. We denote with Y_i and $Elec_i$ respectively the outcome and treatment of a generic city i. The potential outcomes for each i in the control and treatment group are $Y_{i,0}$ and $Y_{i,1}$ respectively. We are interested in the estimation of the average treatment effect, which is $E(Y_{i,1} - Y_{i,0})$. In order to identify the latter, we need orthogonality between treatment and potential outcomes. We achieve it thanks to the institutional design of IMU. We exploit the fact that the central government imposes the tax on all cities and sets a deadline for the deliberation of tax rates within the year, avoiding any strategic behavior of municipal governments in choosing the year of the introduction of taxes. In light of this, the fact of being in an electoral year is as good as randomly assigned. On the contrary, suppose there was not a deadline, the occurrence of deliberations could be dependent on the presence of elections in the year. Thus, we argue the following:

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[7]Computations on OECD Housing Market Questionnaire.

[8] The process of deliberation was characterized by a back and forth of information. This led to multiple deliberations in many municipalities. However, by the 31st of October all rates for the year 2012 were defined.

Assumption 1: The imposition of a deadline for the deliberation, set by the end of 2012, guarantees that $Y_{i,0}, Y_{i,1} \perp E_i$.

We must also assume that the treatment of each city i does not affect the potential outcomes of other cities. In other words, there must be no externalities. Thus, we assume the Stable Unit Treatment Value Assumption:

Assumption 2: $E_i \perp Y_{j,0}, Y_{j,1}$ for every i and j .

We do not see an obvious strategic interaction between municipalities. Tie bout sorting given the very short period which we analyze and the widespread uncertainty about the status of this tax is not worrisome.

Once the two assumptions hold, we can identify the effect of interest by running the following model:

$$Y_{p,i} = \beta Elec_{p,i} + \gamma X_{p,i} + \lambda_p + \varepsilon_{p,i} \tag{1}$$

where $Elec$ gets value 1 when elections are held, p denotes the province of each municipality and λ_p are provincial fixed-effects. There are 109 provinces in Italy, they can be thought as large counties in the US.

We perform two tests using 2012 deliberation data. Our natural experiment generates a complication for the estimation of political budget cycles in 2012 using the strategy defined above. In fact, the deadline imposed by the national government for deliberations was the 31st of October, after elections were held on the 6th and 7th of May. The consequence is that we cannot take election in 2012 as our treatment variable. Indeed, municipalities were free to deliberate in any moment before the deadline and could have easily moved the deliberation after elections. Thus, if we used as a treatment the fact of having deliberated before elections, being an electoral municipality in 2012, we would introduce endogeneity in the model. Municipalities would be selected into treatment. However, we can assess whether having elections in 2012 had an effect on the probability of moving the deliberation after the election date.

We then take the characteristic of having elections scheduled for 2013 as our treatment and we assess whether this has an effect on tax rates set in 2012. In this case municipalities are not selected into treatment since the fact of being in an electoral year is reasonably orthogonal to every unobservable factor. Thus, *Assumption 1* and *2* are satisfied and we can achieve

identification.[9]

The second experiment we implement regards those municipalities where there we no elections in 2012 . We want to assess whether the number of periods lasting to next elections can have an effect on the deliberated tax rates.[10] When we restrict our sample, the number of years lasting to next elections in each municipality is as good as randomly allocated. We thus estimate the following:

$$Y_{p,i} = \beta NTE_{p,i} + \gamma X_{p,i} + \lambda_p + \varepsilon_{p,i} \tag{2}$$

where NTE counts the number of years lasting to next elections for municipalities that do not elect in 2012.

4 Data

We collected the dates (day, month, year) in which each municipality council deliberated the specific tax rates.[11] These data cover both 2012 and 2013 and have been provided by IFEL foundation and the Italian Ministry of Economy and Finance.

Since deliberation was not mandatory, not all the municipalities decided to formally fix these parameters: this is why not all the municipalities have at least one deliberation in 2012, when the tax has been introduced. Therefore the standard rates (0.4%, 0.76%, 0.2% with a deduction of 200 euros)[12] are applied to this group and to those which explicitly decided to set the standard rates. In the following year, 2012, rates would have applied if municipalities had not changed them through a new deliberation. Thus, in 2013, local governments deliberated only if they wanted to change the status quo established the previous year. As we can see in Table 1, the share of municipalities which deliberated in 2013 is lower (69% against 97%) than the previous year.[13] Table 2 shows that in 2012 local governments preferred to use the additional

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[9]We construct the dummy for elections in 2013 by looking at the information set of local governments in 2012 classifying as municipalities electing in 2013 those that were expected to have the end of the term in 2013 at the beginning of 2012. Thus, we exclude the threat to identification coming from cases in which municipalities could have anticipated elections in 2013 because of a collapse linked to the deliberated tax rates. In that case in fact the treatment would not be orthogonal to potential outcomes.

[10]The reason why we restrict the sample to municipalities that do not elect in 2012 is that if we included them, we would generate endogeneity. We would in fact treat those deliberating after elections as if they were at the beginning of the term and the municipalities deliberating before elections as if they were in the election year, letting the variable be endogenously determined.

[11]The law included also some tax credits are excluded from our analysis since, as we will explain later, only a few municipalities moved the level of deductions away from the default rate.

[12]The tax credit is applied only to the main residence and is complemented with 50 euros of additional credit for every non economically-independent child with less than 26 years for a maximum of 400 euros.

[13] Since municipalities were allowed to change their decision within 2012 and 2013, we can see in both years

residence tax rate rather than the main residence one as a source of additional funding. Indeed, a majority of them (65%) maintained the Standard rate on the main residence, while 43%, did not change the rate on additional ones. The municipalities increasing the latter are almost as twice as those increasing the former (fourth column). Only 137 of them, out of a total of 8092, decided to move away from the standard tax credits while the 32.5% of them lowered the rate on rural properties.

There are three types of government in italian cities: independent ones with local political groups not linked to national parties, center left and center right parties, the same coalitions as at the national level. Table 3 shows that governments of both left and right tend to set higher rates than Independent ones, but left wing tends to set higher rates than right. Furthermore, a higher share (35%) of left wing municipalities set a main house rate higher than the standard one with respect to right wing (27.6%). This is consistent with the ideological stands of the two sides regarding the taxation of wealth.

Recent economic and demographic data for each municipality, such as population, unemployment rate, number of active firms are from the 2011 Census of Population and Houses and the 2011 Census of Industry and Services are provided by ISTAT, the National Statistic Institute of Italy. Data about number of resident and non-resident held houses, altitude and municipal area extensions are taken from 2001 Census of Population and Houses and Statistical Atlas published by ISTAT as well. Other economic controls, such as per-capita taxable income, overall income and taxpayer number come from 2013 IRPEF data held by the Italian Ministry of Economic and Finance. Deficit data for all municipalities and other measures included in 2011 balance sheets are from the database of the Italian Ministry of the Interior. Data regarding the whole Region of Trentino-Alto Adige, a region with a "special status" of autonomy are missing. Political data about the governing coalitions in municipalities come from the Italian Ministry of the Interior, as well. Table A1 in Appendix summarizes all the data used in this paper and their sources. Descriptive statistics for all the variables employed are provided in Tables 4 and 5.

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multiple deliberations in the same municipality. In these cases, the actual tax payed by citizens was computed using the rates and credits chosen with the last deliberation of the year.

5 The Effects of Elections on Tax Rates

Table 6 reports the result of the estimation of model (1). Our regressions include provincial fixed-effects and clustered standard errors at the provincial level. The dependent variable is the main house tax rate set in the last deliberation of 2012.[14] In column 1 we show the effect of having elections in 2013 estimated without including any additional control. The coefficient is significantly estimated and negative, suggesting that municipalities with elections in 2013 set lower tax rates in 2012. The result does not change and its significance is robust to the inclusion of economic, demographic and geographic controls as displayed in column 2. In particular, we control for population in 2011, municipal area (and its square), altitude (and its square), the share of people having a high school or University degree, the number of firms per capita in 2011, the income per capita, deficit per capita, the number of main habitations per capita and the unemployment rate in 2011.

The inclusion of province fixed-effects guarantees that the result is not driven by cultural aspects that might not be balanced between different geographical areas. Column 3 and 4 show the estimated coefficients for regressions having as dependent variable the tax rate on additional residence set in the last deliberation of 2012. The effect has the expected sign, but is not statistically significant.

In Table 7 we replicate the experiment with 2013 deliberation data. The IMU on the main residence was abolished in August 2013, after a harsh political and mediatic debate, and the tax on additional residences became even more salient and obviously more important for local finances. Thus, we can only estimate the model using the additional residence tax rate as dependent variable. The coefficient is significant and robust to the inclusion of controls.

All results in Table 6 and 7 are robust to the exclusion of one region at a time and the inclusion of regional fixed-effects instead of provincial fixed-effects. Moreover, since we do not have data on deficit for some municipalities, the number of observations drops when we include controls. Therefore, as a further robustness check we run the regressions in column 1 and 3 of Table 6 and the one in column 1 of Table 7 on the restricted sample for which we have all controls. Results do not change.

We then run the analysis separately on North, Center and South of Italy.[15] For what

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[14] The tax rates fixed in these last deliberations are those employed in the computation of the tax levy in December 2012.

[15] The regions included in the North are: Friuli-Venezia Giulia, Liguria, Lombardia, Piemonte, Valle d'Aosta, Trentino Alto Adige and Veneto. The Center includes: Emilia Romagna, Toscana, Umbria, Marche and Lazio.

concerns the tax rate on the main residence (see Table 8), we find little evidence of the presence of cycles in the North that does not remain significant after the inclusion of controls, no evidence in the Center where the coefficient is positive but not significant and a strong presence of cycles in the South where the coefficient is negative, significant and higher than the one estimated for the entire sample. For other residence tax rates we find evidence of political budget cycles in the South and an increase of tax rates for electing cities in the Center (see Table 9). We interpret the result for the Center as probably depending on the political orientation (left) of the regions included in this geographical area. In Table 10 we find evidence of budget cycles in 2013 for all of the geographical areas with a stronger effect of elections for cities in the Center.

As we mentioned in the introduction, the fact that the strategic manipulation of the tax rate is especially evident mostly in the South is consistent with the literature on the effect of civicness on control of politicians as in Nannicni et al (2012). Lower civicness in Southern Italy[16] is presumably associated with less control of politicians who can strategically manipulate policies to their advantage since they are less checked.[17]

We then distinguish between cities with more and less than 15000 inhabitants. We chose this threshold because above and beyond it cities have a different electoral system. In particular, smaller cities have only one ballot where the candidate gaining the relative majority becomes mayor and forms the local government. On the other hand, in cities with more than 15000 inhabitants if an absolute majority (i.e. more than 50%) is not reached in the first ballot, the elections are decided by a two-candidates ballot where the candidates who got the highest shares of votes in the first ballot run for becoming the mayor. We expect that in small cities the IMU tax is more salient in the public debate given the limited range of issues that small city governments have to deal with. On the contrary, in bigger cities where the administration is more complex, we expect voters to base their electoral decision on other aspects (e.g. the efficiency of public transports, the cleanness of streets, the efficiency of bureaucracy or health services). If the incumbents anticipate this, in small cities we should observe a higher degree of political budget cycles. This is indeed the case as we show in Table11 and 13. We find that political budget cycles on the main residence tax rate in 2012 and on additional residence rates in 2013 are present only for small municipalities, while there is no such an evidence in big cities. We also find no significant cycle for additional residences in 2012 (Table 12).

The South includes: Abruzzo, Molise, Campania, Calabria, Basilicata, Puglia, Sicilia and Sardegna.

[16]In recent years a multitude of newspaper articles underscored many episodes of waste and corruption in ,local governments in the South of Italy.

[17]A similar result is found by Akhmedov and Zhuravskaya (2004) that find a stronger presence of political budget cycles in regions with lower freedom of press and lower government transparency. The explanation is again that the lack of transparency reduces political accountability.

We then conduct a second experiment on municipalities that did not vote in 2012. We assess whether their deliberated tax rates are influenced by the distance from next administrative elections measured in number of years. Table 14 reports results from the estimation of the model in (2) and shows that the coefficient on the number of years lasting to next elections is positive and strongly significant for both tax rates. In particular, we estimate that being at the beginning of the term leads to a main residence tax rate 0.12 higher than the one that would be deliberated in the year before elections, and an additional residence tax rate 0.36 higher. Both results are robust to the inclusion of controls and to the same robustness checks explained above.

We conduct our analysis on geographical areas and municipalities below and above the 15000 inhabitants for model in (2), as well. The results are consistent with the ones found in the previous analysis and are available upon request.

6   Deficit and Political Budget Cycles

As previously pointed out, the IMU is the main source of revenue for local governments. This implies that its importance in keeping a balanced fiscal stance depends on the level of deficit each municipality inherited from 2011. We try to understand whether the initial level of deficit (i.e. the one in 2011) interacts with elections in determining the extent to which we observe budget cycles. Table 15 reports the results for the estimation including an interacted term between the electoral dummy and the level of deficit per capita. We find in column 1 and 2 that when the dependent variable is the main residence rate in 2012, the interaction gets a significant and positive coefficient, suggesting that among the cities voting in 2013, those with high level of deficit were more incline to set higher tax rates for the main residence. We interpret the result saying that when deficit is high, it is more likely to become a salient issue in the electoral debate. Therefore, municipalities running high deficits have a higher incentive to reduce it and appear more judicious from voters' perspective.[18] When there are high levels of deficit, this effect can more than compensate the budget cycle observed in the coefficient of the electoral dummy. In other words, we observe political budget cycles on the main residence only for municipalities with low levels of deficit. In columns 3 to 6 we find no evidence of an influence of deficit on budget cycles for the additional residences' tax rates.

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[18]The view that large deficits are seen as problematic by the voters and that deficit reducing governments are not systematically punished on election day is consistent with the evidence by Brender and Drazen (2008) and Alesina et al (2013).

The Italian government enforces the European Stability and Growth Pact on the local governments by means of an Internal Stability Pact which every year designs new fiscal targets that become mandatory for local governments. The municipalities on which the Pact is enforced have changed several times in the last ten years. In particular, the central government is progressively enlarging the number of cities that must fulfill the requirements of the Pact. In 2012 the Pact was enforced on all municipalities with more than 5000 inhabitants. We run our model above and below the threshold of 5000 for the data in 2012. Results are reported in Table 16 and show that the effects highlighted above hold only for the sample of municipalities with less than 5000 inhabitants, while they disappear on the sample over 5000 inhabitants. We confirm the absence of any evidence of political budget cycles for the additional residence tax (Table **??**). Our interpretation for these results is that in municipalities with more than 5000 inhabitants the voters are aware of the existence of deficit constraints and therefore evaluate a deficit reduction as a simple comply of the rule. On the other hand, voters in smaller municipalities that are not subject to the Pact may regard a deficit reduction in the presence of a high net borrowing as a sign of the effort of the government towards a sustainable fiscal stance. As a consequence, the governors anticipating this fact could decide to increase taxes when the level of deficit is high.

7 The timing of deliberations

In this section we present a test conducted on the time of deliberation of municipalities in 2012. We are interested in understanding whether the presence of elections in 2012 led to a different timing in the deliberation administrative process. We run the same model as before, but we employ as dependent variable a dummy taking the value of 1 when the deliberation is made before elections take place. We find that having elections in 2012 leads to an increase of about 8% in the probability of anticipating the deliberation before the election date and the effect is robust to the inclusion of controls: results are presented in Table 18. Moreover, as a further check we report in Table 16 the results obtained including election 2013 as treatment variable. In line with our expectations, we find no evidence of an effect of elections in 2013 on the probability of deliberating before elections in 2012. We then look at the deliberated tax rates before and after the elections for the municipalities electing in 2012 and we find that those deliberating before fixed on average lower tax rates. However, the difference is almost negligible and we cannot reject the hypothesis of zero difference (see Table 19).Moreover, when we introduce in column 4 province fixed-effects the coefficient changes in sign and is still not

significant. Thus, we cannot conclude that there is evidence of differences in the tax rates fixed before and after elections among the municipalities having elections in 2012. Given these results, we think the most plausible explanation is that municipalities facing election wanted to dispel from the voters the fear that they would set high tax rates after the election. In line with this hypothesis, we show that deliberations were more likely to be anticipated before elections in cities electing in 2012 where last elections were won with a small margin of majority. In other words, the more competitive and contested a city was, the more amplified is the strategic choice of the timing of deliberations. Results on this point are available upon request.

8   Conclusion

The introduction in 2011 of a new real estate tax in Italy provides an excellent natural experiment to test the prevalence of strategic manipulations of fiscal instruments in anticipation of election. Two features of this experiment are especially useful for identification: city governments had some discretion in the choice of the level of this tax rate on the main residence of a family and on additional residences, and the election date in cities is staggered for reason completely unrelated to this tax rate. We find substantial evidence of this strategic manipulation. The closer the local governments were to a new election, the lower the tax rate chosen; the results are robust to a host of controls. However, the size of the cycle is smaller for cities in which deficits in 2011 were higher suggesting that budget concerns made it more difficult for cities to chose lower tax rates in that situation. This effect is stronger for cities with more discretion on their level of deficit.

We also find that the evidence on cycles is especially strong in the South of Italy. This is consistent with models which suggest that lower levels of civicness are associated with less controls of politicians who can then engage in strategic manipulations of policies.

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Tables and Figures

Table 1: Multiple Deliberations

Maximum Number of Deliberations	Municipalities in 2012	Municipalities in 2013
1	7268	5321
2	599	230
3	4	14
4	0	1

Table 2: 2012 Rates Statistics

Type of Rate	Standard Rate (%)	Average Rate (%)	Above Standard (%)	Below Standard (%)
Main Residence	0.40	0.42	28	7
Additional Residence	0.76	0.844	55	2
Rural Property	0.20	0.14	0	32

Table 3: Rates and Politics in 2012

	rw	lw	Independent
Main Residence Av.	4.21	4.29	4.20
Main Residence Av. Distance	0.013	0.097	0
Additional Residence Av.	8.67	8.89	8.33
Additional ResidenceDistance	0.33	0.56	0
Subsample Size	894	790	5934

RIGHT-WING Munic.	Main Home>Standard	Main Home<Standard
Additional Residence>Standard	25.2%	6.4%
Additional Residence<Standard	0%	0.2%
Additional Residence=Standard	2.5%	2.0%
Total	27.6%	8.4%

LEFT-WING Munic.	Main Home>Standard	Main Home<Standard
Additional Residence>Standard	33.3%	2.7%
Additional Residence<Standard	0%	3.0%
Additional Residence=Standard	1.6%	2.0%
Total	35.0%	6.5%

Table 4: Dependent Variables' Statistics

(a) Rates 2012

2012	Obs	Mean	Std. Dev.	Min	Max
Main Residence first	8092	4.195	0.692	0	6
Main Residence last	8092	4.206	0.709	0	6
Additional Residencelast	8092	8.683	1.294	4.6	10.6
2013	Obs	Mean	Std. Dev.	Min	Max
Additional Residence last	8092	8.686	1.124	4.6	10.6
Additional Residence yearly change	8092	0.236	0.909	-4.6	41.4

Variable	Obs.	Mean	Std. Dev	Min	Max
Deliberations before Elections	8092	0.254	0.435	0	1

(b) Fiscal and Electoral Timing

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Table 5: Controls' Statistics

Variable	Obs	Mean	St. Dev	Min	Max
Population[a]	8092	7,344.753	39,741.76	30	2,617,175
Territory size[b]	8089	0.373	0.500	0.002	13.077
Altitude[c]	8089	0.358	0.298	0.00	2.035
Education[d]	8084	0.259	0.061	0.0508	0.706
Active firms[e]	8091	0.064	0.022	0.007	0.307
Taxable income[e]	8092	16,189.29	3.455,785	6,491	44270.7
Deficit[e]	6222	-60.182	245.90	-4,941.209	7,466.19
First property[f]	8086	0.390	0.082	0.123	1.053
Unemployment rate (%)[a]	8064	10.2	6.3	0.64	.42.2

[a] *2011 data*

[b] *hundreds of km^2*

[c] *thousands of m*

[d] *share of graduated people on local population in 2001*

[e] *per capita*

[f] *per capita houses held by residents: it is a proxy of the number of people who hold their main habitation*

Table 6: The effects of elections in 2013 on 2012 tax rate

	(1)	(2)	(3)	(4)
	M R	M R	A R	A R
Election 2013	-0.0840**	-0.0871***	-0.0498	-0.0554
	(0.0326)	(0.0328)	(0.0514)	(0.0572)
Controls		X		X
Observations	8,091	6,199	8,091	6,199
R-squared	0.001	0.015	0.000	0.076
Number of prov_cod	110	108	110	108

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.

Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.

Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when the municipality has election in 2013.

The dependent variable for columns (1) and (2) is the main residence rate set at the end of 2012, while for column (3) and (4) it is the additional residence rate at the end of 2012.

Table 7: The effects of elections in 2014 on additional residence tax rates in 2013

| | (1) | (2) |
	A R	A R
Election 2014	-0.233***	-0.170***
	(0.0225)	(0.0254)
Controls		X
Observations	8,091	6,199
R-squared	0.011	0.085
Number of prov_cod	110	108

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.

Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.

Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2014* equals 1 when the municipality has election in 2014.

The dependent variable is the additional residence rate set at the end of 2013.

Table 8: The effects of elections on the main residence rate in 2012 by geographical areas

	(1)	(2)	(3)	(4)	(5)	(6)
	North	North	Center	Center	South	South
Election 2013	-0.0978***	-0.0692	0.0629	0.0505	-0.113**	-0.147**
	(0.0360)	(0.0434)	(0.0707)	(0.0770)	(0.0540)	(0.0565)
Controls		X		X		X
Observations	4,191	3,437	1,343	1,051	2,557	1,711
R-squared	0.001	0.021	0.001	0.013	0.003	0.034
Number of Provinces	38	36	31	31	41	41

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when there are elections in 2013.
The dependent variable is the main residence rate set at the end of 2012.

Table 9: The effects of elections on the additional residence rate in 2012 by geographical areas

	(1)	(2)	(3)	(4)	(5)	(6)
	North	North	Center	Center	South	South
Election 2013	0.0750	0.0524	0.294***	0.309***	-0.224***	-0.266***
	(0.0694)	(0.0621)	(0.0826)	(0.0955)	(0.0679)	(0.0933)
Controls		X		X		X
Observations	4,191	3,437	1,343	1,051	2,557	1,711
R-squared	0.000	0.067	0.005	0.082	0.005	0.156
Number of Provinces	38	36	31	31	41	41

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when there are elections in 2013.
The dependent variable is the additional residence rate set at the end of 2012.

Table 10: The effects of elections on the additional residence rate in 2013 by geographical areas

	(1)	(2)	(3)	(4)	(5)	(6)
	North	North	Center	Center	South	South
Election 2014	-0.252***	-0.187***	-0.274***	-0.187***	-0.170***	-0.133**
	(0.0279)	(0.0339)	(0.0487)	(0.0617)	(0.0554)	(0.0559)
Controls		X		X		X
Observations	4,191	3,437	1,343	1,051	2,557	1,711
R-squared	0.017	0.077	0.016	0.082	0.004	0.161
Number of Provinces	38	36	31	31	41	41

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2014* equals 1 when there are elections in 2014.
The dependent variable is the additional residence rate set at the end of 2013.

Table 11: The effects of elections on the 2012 main residence rate by municipality population

	(1)	(2)	(3)	(4)
	< 15000	< 15000	> 15000	> 15000
Election 2013	-0.0920**	-0.109***	-0.0269	0.0413
	(0.0396)	(0.0350)	(0.0898)	(0.0909)
Controls		X		X
Observations	7,368	5,644	723	555
R-squared	0.001	0.018	0.000	0.041
Number of Provinces	110	108	108	101

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when there are elections in 2013.
The dependent variable is the main residence rate set at the end of 2012.

Table 12: The effects of elections on the 2012 additional residence rate by municipality population

	(1)	(2)	(3)	(4)
	< 15000	< 15000	> 15000	> 15000
Election 2013	-0.0380	-0.0510	-0.256	-0.275*
	(0.0509)	(0.0561)	(0.157)	(0.165)
Controls		X		X
Observations	7,368	5,644	723	555
R-squared	0.000	0.065	0.008	0.091
Number of Provinces	110	108	108	101

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when there are elections in 2013.
The dependent variable is the additional residence rate set at the end of 2012.

Table 13: The effects of elections on the 2013 additional residence rate by municipality population

	(1)	(2)	(3)	(4)
	< 15000	< 15000	> 15000	> 15000
Election 2014	-0.185***	-0.133***	-0.114	-0.0495
	(0.0227)	(0.0270)	(0.0847)	(0.0953)
Controls		X		X
Observations	7,368	5,644	723	555
R-squared	0.007	0.069	0.003	0.073
Number of Provinces	110	108	108	101

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2014* equals 1 when there are elections in 2014.

The dependent variable is the additional residence rate set at the end of 2013.

Table 14: The effects of elections proximity on tax rates in 2012

	(1)	(2)	(3)	(4)
	M R	M R	A R	A R
NTE	0.0335***	0.0325***	0.0941***	0.0706***
	(0.00844)	(0.0100)	(0.0166)	(0.0174)
Controls		X		X
Observations	6,835	5,308	6,835	5,308
R-squared	0.002	0.015	0.008	0.076
Number of prov_cod	110	108	110	108

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.

Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.

Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *NTE* counts the number of years to the end of the term for each municipality.

The dependent variable for columns (1) and (2) is the main residence rate set at the end of 2012, while for column (3) and (4) it is the additional residence rate at the end of 2012.

Table 15: The effects of elections on tax rates: interaction with deficit

	(1)	(2)	(3)	(4)	(5)	(6)
	M R 12	M R 12	A R 12	A R 12	A R 13	A R 13
Deficit*Election2013	0.000495**	0.000498**	0.000293	0.000292		
	(0.000230)	(0.000234)	(0.000361)	(0.000340)		
Election2013	-0.0697**	-0.0689**	-0.00477	-0.0448		
	(0.0338)	(0.0335)	(0.0600)	(0.0570)		
Deficit	0.0000637	0.0000627	0.000177**	0.000176**	0.000257***	0.000249***
	(0.0000441)	(0.0000429)	(0.0000730)	(0.0000688)	(0.0000955)	(0.0000785)
Deficit*Election2014					-0.0000105	0.0000009
					(0.000131)	(0.000120)
Election2014					-0.233***	-0.170***
					(0.0275)	(0.0261)
Controls		X		X		X
Observations	6,222	6,199	6,222	6,199	6,222	6,199
R-squared	0.003	0.015	0.003	0.076	0.016	0.085
Number of Provinces	108	108	108	108	108	108

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.

Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis. Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* and *Election 2014* equal 1 when there are elections in 2013 and 2014 respectively. *Deficit* is the level of deficit per capital in 2011, while *Deficit*Election2013* and *Deficit*Election2014* are the interacted terms.

The dependent variable is the main residence rate set at the end of 2012 in column 1 and 2, the 2012 additional residence rate in column 3 and 4 and the 2013 additional residence rate in column 5 and 6.

Table 16: The effects of elections on main residence tax rates in 2012: interaction with deficit in big and small municipalities

	(1)	(2)	(3)	(4)
	< 5000	< 5000	>5000	> 5000
Deficit*Election2013	0.000598**	0.000602**	-0.000178	-0.000094
	(0.000255)	(0.000258)	(0.000400)	(0.000391)
Election2013	-0.130***	-0.122***	-0.0330	-0.0283
	(0.0424)	(0.0414)	(0.0511)	(0.0506)
Deficit	0.000038	0.000039	0.000385**	0.000262
	(0.000043)	(0.000042)	(0.000152)	(0.000163)
Controls		X		X
Observations	4,351	4,328	1,871	1,869
R-squared	0.005	0.020	0.004	0.029
Number of Provinces	106	106	107	107

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.

Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis. Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2013* equals 1 when there are elections in 2013. *Deficit* is the level of deficit per capital in 2011, while *Deficit*Election2013* is the interacted term.

The dependent variable is the main residence rate set at the end of 2012. In column 1 and 2 we report results for municipalities with less than 4000 inhabitants, while in 3 and 4 there are municipalities with more than 5000.

Table 17: The effects of elections on additional residence tax rates in 2013: interaction with deficit in big and small municipalities

	(1)	(2)	(3)	(4)
	< 5000	< 5000	>5000	> 5000
Deficit*Election2014	-0.000153	-0.000184	0.000211	0.000171
	(0.000158)	(0.000168)	(0.000185)	(0.000162)
Election2014	-0.146***	-0.131***	-0.237***	-0.171***
	(0.0398)	(0.0421)	(0.0351)	(0.0328)
Deficit	0.000318**	0.000317***	0.000088	0.000168*
	(0.000126)	(0.000119)	(0.000133)	(0.000100)
Controls	1,487	1,466	4,735	4,731
Observations	0.015	0.043	0.015	0.075
R-squared	90	90	108	108
Number of Provinces	106	106	107	107

($*$) $p < 0.1$; ($**$) $p < 0.05$; ($***$) $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis. Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2014* equals 1 when there are elections in 2014. *Deficit* is the level of deficit per capital in 2011, while *Deficit*Election2014* is the interacted term. The dependent variable is the additional residence rate set at the end of 2013. In column 1 and 2 we report results for municipalities with less than 4000 inhabitants, while in 3 and 4 there are municipalities with more than 5000.

Table 18: The effects of elections on the deliberation date

	(1)	(2)	(3)	(4)
	BeforeElec	BeforeElec	BeforeElec	BeforeElec
Election 2012	0.0764***	0.0811***		
	(0.0138)	(0.0162)		
Election 2013			0.00358	0.0105
			(0.0165)	(0.0194)
Controls		X		X
Observations	8,091	6,199	8,091	6,199
R-squared	0.004	0.010	0.000	0.006
Number of prov_cod	110	108	110	108

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
Fixed-effects at the province level are included. Clustered standard errors at the province level are in parenthesis.
Controls included are: population 2011, municipal area, municipal area squared, altitude, altitude squared, share of people with high school or University degree, number of firms per capita 2011, income per capita, deficit per capita, number of main habitations per capita, unemployment in 2011. The variable *Election 2012* and *Election 2013* are equal to 1 when elections are held in 2012 and 2013 respectively.
The dependent variable is the dummy *BeforeElec* that equals 1 when the deliberation date for the first deliberation of 2012 is prior to May 6th 2012.

Table 19: Differences in Main Habitation Deliberated Tax Rates Before and After 2012 Elections

	(1)	(2)	(3)	(4)
Before Elections	-0.0063	-0.0063	-0.0063	0.043
	(0.055)	(0.056)	(0.07)	(0.069)
N. Obs	954	954	954	954

$(*)$ $p < 0.1$; $(**)$ $p < 0.05$; $(***)$ $p < 0.01$.
We show, among municipalities electing in 2012, the difference in mean in the first deliberated Main Habitation tax rate between those that deliberated before and after elections. Column 1 reports the homoskedastic standard error, column 2 the robust one and column 3 the one clustered at province level. Column 4 shows the difference when we introduce province fixed-effects and we cluster standard errors at the province level.

A positive difference means a higher tax rate for those deliberating before elections.