# Term Limits for Mayors and Fiscal Policy: Evidence from Italian Municipalities

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**Abstract**

This paper investigates the influence of term limits for executives on fiscal policy at the municipal level. Estimates are based on a dataset comprising 106 Italian cities over the 1998-2010 period. In order to credibly identify the influence of term limits, the estimations include mayor fixed effects. We provide evidence that electoral incentives distort rather than discipline incumbent mayors' behaviour, because both pork and spending is higher before an election with an eligible incumbent, in line with the political budget cycle literature. Term limits do not always appear to matter, which raises the question whether in local governments with two-term term limits electoral incentives are completely absent in the second term. Southern cities are found to behave rather differently from Northern ones; in the former pork is higher in first term, in the latter spending is higher.

*JEL classification:* D72, H11, H72

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## 1. Introduction

The effect of term limits on fiscal policy has been investigated both by political scientists and economists. However, theoretical models still widely differ in their predictions and empirical evidence has not come to definite conclusions by now. Empirical contributions mainly draw on datasets from the US and focus on the state tier only, which makes it difficult to draw general conclusions. One important contribution that this paper makes is the investigation of term limits at the local level of government. In fact, we use data from Italian cities.

There is a relevant dimension in which local governments are, in most cases, different from central governments or the states of a federation: a considerable share of their fiscal resources are transfers from higher levels of government. When the size of these transfers is not set according to a strict pre-defined rule and may depend on lobbying efforts by individual municipalities, the relationship between central and local governments is similar to the one between legislatures and their electoral districts. Aidt and Shvets (2012) have recently addressed term limits in the context of legislatures where fiscal policy is decided upon by state-level legislators, each representing a voting district. In this setting, legislators' re-election incentives may have a perverse effect: legislators are likely to exert more effort and bring home a greater amount of pork if they can run in the next elections. This causes an increase in pork-barrel spending, which exacerbates the well-known common pool problem (Weingast, Shepsle and Johnsen, 1981).

Empirically, whether term limits have a different overall impact at the *local* level of government is an open question. It is not clear whether the effect that Aidt and Shvets isolate is quantitatively relevant. The impact of term limits on legislatures is generally not well documented in the empirical literature, either. Aidt and Shvets (2011) conduct an empirical analysis that focuses on pork; their dependent variable is the size of targeted transfers from state budget to legislative districts. We do not only focus on transfers but also consider spending. In our view, what is relevant is the overall impact of the presence/absence of re-election incentives, i.e. the ultimate consequences of term limits for government size.

Another important contribution of this study is that it combines features of the empirical literature on term limits and the empirical literature on political budget cycles. In particular, we investigate in a first step whether fiscal policy differs between mayors' first and second term in office and in a second step we investigate the specific pattern of fiscal policy over the two terms. We contribute to the empirical literature on political budget cycles at the local level of government (Galli and Rossi, 2002, Akhmedov and Zhuravskaya, 2004, Veiga and Veiga, 2007, Sjahrir et al., 2013).

Our dataset consists of a panel of Italian cities over the 1998-2010 period. Italian mayors have been subject to a term limit since 1993. The 106 large Italian municipalities included in the dataset are suited to our research question as they are characterized both by a high variance in per capita spending and by the fact that, in the period we consider, their fiscal resources have consisted partly of revenues and partly of transfers from the central government. We use both static and dynamic panel data

methods in our investigation. To control for cities' and mayors' time-invariant unobservable characteristics we include mayor fixed effects in all estimations[1]. This approach draws on Aidt and Shvets (2012) who include legislator fixed effects.

Our results show that electoral incentives impact on pork and spending in different ways at the local level of government. There seems to be a prevailing phenomenon of higher pork and spending whenever an incumbent may be re-elected; however, differences emerge in the second term. Our evidence also points to a clear divide between the Centre-North part of the country, where there appears to be a classical electoral cycle whenever incumbents may be re-elected, and the South, where pork is in line with the predictions in Aidt and Shvets (2012).

The remainder of this paper is structured as follows: Section 2 provides an overview of the existing theoretical and empirical literature on the fiscal effects of term limits. Section 3 describes in detail the institutional setup for Italian municipalities as well as the construction of our dataset. Section 4 describes the empirical strategy and provides the baseline estimation results. Section 5 provides some robustness checks. Section 6 concludes and identifies avenues for future extensions.

## 2. Literature review

This section reviews the existing literature on the effect of term limits on fiscal policy. We take into account both the predictions by theoretical models as well as the existing empirical evidence.

### *2.1 Theoretical predictions*

Term limits have been analyzed by both political scientists and economists; the latter have often focused on two-term term limits, i.e. no possibility of re-election after two terms. Since such an institutional arrangement entails a sequence of elections characterized by different incentives for an incumbent, this stream of literature is intertwined with the literature on the effects of re-election incentives on a policymaker's conduct. There are two main classes of models in the political economy literature on the effects of re-election incentives.

The first comprises models in which elections induce an incumbent to deviate from benchmark fiscal policy because this increases, by different mechanisms, her chances to be re-elected. Nordhaus (1975) and Rogoff and Sibert (1988) analyze an opportunistic cycle in an adaptive expectation and rational expectation context, respectively; in the latter case, a signaling game takes place between the incumbent, who may be either competent or not, and the voters, who do not observe competence but policy outcomes. This stream of literature has witnessed a steady growth in time; recent contributions are referred to as the literature on the conditional rational business cycle. These models analyze under which particular conditions and specific political institutions the phenomenon is observed (Persson and Tabellini, 2003; Brender and Drazen, 2005; Shi and Svensson, 2005). Most of the empirical evidence reveals that the political cycle is primarily observable with respect to public spending. Aidt

[1] In our sample period, the same mayor does not hold office in more than one municipality.

and Shvets (2012) consider the effects of electoral incentives on distributive politics, and show that the common pool problem (Weingast, Shepsle and Johnsen, 1981) may be exacerbated by legislators' re-elections incentives if there are differences in legislators' ability to bring home pork and voters cannot directly observe it. This implies that, whenever a legislator may be re-elected, one should expect, on average, higher pork in her constituency. On the contrary, a term-limited office-holder makes no costly effort to bring home more pork.

The second class of models provides an alternative view on the effects of elections. Elections guarantee political accountability, hence they have a disciplining role on an incumbent's behavior. Models along these lines were first developed by Barro (1973) and Ferejohn (1986), who suggested the voter-incumbent relationship should be investigated as an agency problem in which voters base their voting strategy on the incumbent's behavior, as they have no information on either her competence and/or her desire to extract rents at their expense. Besley and Case (1995) show that in a two-term limit institutional setup, there is room for reputation building in the first term, while in the second the office-holder maximizes her own payoff subject to no political constraints. It is usually understood that a politician's preferences are for higher expenditure, so that, in view of an election she will compete in, she tends to spend less than she would if she had no re-election concerns.

Notice that the two classes of models have different empirical predictions. With specific reference to public expenditure, the former expects an office-holder to spend more before the elections/in the first term, while for the latter higher spending characterizes incumbents' second terms. However, in the second case, the prediction is the consequence of both the model and the assumption that incumbents like spending more than voters. With voters more spending-prone than their incumbent, the two classes of models both predict more spending in the first term.

Recent contributions to the literature on term limits are based on signaling games. Smart and Sturm (2013) introduce the possibility of having public-spirited incumbents in addition to self-interested politicians that were the exclusive focus of the prior literature. They predict no difference between a policymaker's first-term and second-term conduct. This is because, with respect to a context where there are no term limits, imposing that an eventual second term in office will be the last one lowers the value of being re-elected, thus making re-election incentives small also in the first term. In a similar signaling game context, however, Alt et al. (2011) claim that, if the costs of signaling are not too high, there is a disciplining effect on a competent incumbent's behavior in the first term. These two recent contributions also stress the importance of the selection effect elections entails: in equilibrium, voters only retain the incumbents who are public-spirited or competent.[2] Smart and Sturm predict a difference between the average fiscal policy of all first terms and of all second terms: the latter is more in line with voters' preferences. In their model, this happens only because second-term office holders are a subset of first-term incumbents and this subset is made up by public-spirited incumbents, as these are more likely to get re-elected. In the model of Alt et al., instead, the presence

[2] In the Smart and Sturm model a pooling equilibrium prevails when there is no term limit.

both of a political accountability effect and of a selection effect makes it difficult to draw a conclusion on how one should expect first term policies to be different from second term policies. The authors suggest an empirical strategy for disentangling the two effects, and they show that they can offset each other, so that first terms and second terms may be observationally equivalent.

Some common features of the models focusing on term limits weaken their interpretation and predictive power. First of all, they all refer to the choice between different generic policies. While other political economy models investigating the effects of elections on fiscal outcomes have considered the specific traits of this domain of governmental action, this is not the case here. There is no benchmark to make reference to, so that welfare analysis is seldom addressed, and the link between the fiscal policies of different points in time is disregarded: there is no consideration for the intertemporal budget constraint. Finally, it is often understood that all years of the first term are similarly characterized in terms of fiscal policy. While in models with reputation building this is quite reasonable, in signaling games it is not so; their translation into the simplified prediction of an inflated expenditure or pork throughout the first term is sometimes justified by the fact that the incumbent's type may change and/or the signal is very noisy (Bernhardt, Dubey and Hughson, 2004). Yet, whether these claims are relevant has never been empirically assessed.

*2.2 Empirical evidence*

Term limits are particularly suitable for analyzing the presence and nature of electoral budget cycles in contexts where party discipline is weak and an incumbent's professional future is not dependent on last-term political decisions.[3] In this perspective, a number of contributions have investigated contexts where this institutional arrangement exists. However, no definite conclusion as to the dominance of the distorting or the disciplining role of elections has been reached so far.

Using data for 48 US states from 1950 to 1986, Besley and Case (1995) estimate the effect of term limits on taxes and expenditures, controlling for state per capita income, demographic composition and state population, plus year- and country-dummies. They find that there is a positive and significant effect of term limits on taxes and expenditures. Besley and Case (2003) update their previous results using data from 1950 to 1997. Again, they find that term-limited governors tend to significantly increase state spending, thus confirming the prevalence of the disciplining role of elections. However, the estimated coefficients are smaller, and the evidence of lower per-capita taxes in first terms is not confirmed.

Alt, Bueno de Mesquita and Rose (2011) argue that the different evidence in Besley and Case (1995) and Besley and Case (2003) is due to the fact that the second contribution considers a longer time span: in the last decade there has been a switch in many US states from one-term to two-term term limits. In fact, when a one-term limit is prevailing in the sample, there is a weak selection effect at work, while with a prevalence of two-term limits it may well be the case that the political

[3] The alternative scenario would make incentives in the last term similar to those in the first term.

accountability effect on first term policies co-exists with a selection effect that make second term governors, on average, more competent. They use first-term lame ducks as the reference group and find that both first term eligible and second term lame ducks spend significantly less than them; the similarity in the size of the difference in spending is said to be the reason why, in two-terms institutional set ups, spending in the two terms may be similar.

List and Sturm (2006) found that governors in the last term of office spend significantly less on environmental protection. They use data covering the 1960–1999 period. However, the term limit effect is weaker in states where a large fraction of citizens support environmental groups. Also, the term limit effect is smaller if the margin of victory in the gubernatorial race is larger.

Johnson and Crain (2004) consider a panel of 48 democracies, some of which characterised by either one term or two term limits applied to their presidential office, over the period from 1972 to 1990. Their results closely resemble those of Besley and Case (1995). A term limit rule leads to both higher government expenditure and revenue. They also look at possibly different effects of a one- and two-term limit. It appears that executives subject to the former constraint are even more prone to engage in higher government expenditure and tax revenue, which is consistent with the predictions of models in which elections play a disciplining role.

Dalle Nogare and Ricciuti (2011) investigate a similar sample but use a dynamic empirical strategy. When non-presidential democracies are included, term limits applied to the presidential office do not seem to impact on government spending and deficit, while when only presidential democracies are considered, deficit is still unaffected, while expenditures are found to be significantly lower in the second term, which contradicts previous evidence and is compatible with models where elections distort an incumbent's behaviour.

Aidt and Shvets (2012) investigate the impact of term limits on per capita transfers from the U.S. state budgets to 598 legislative districts over the 1993-2004 time span. Their approach introduces an innovation to this literature: they use legislators' fixed effects. Their finding confirms their model's predictions: since last term's effect is significantly negative, this points to an incumbent's incentive to bring home more pork whenever an election is in sight, i.e. in the first term.

De Janvry, Finan and Sadoulet (2012) also use incumbents' fixed effects when considering the case of Brazilian municipalities' implementation of a national schooling programme aimed at reducing dropout rates. Though not strictly related to the issue considered here (the programme is in fact financed by central government), we mention it because of the rich model it is complemented by, in which the difference between first-term and second-term performance is assumed to depend not just on different electoral incentives, but also a mayor's experience and ability.[4] They deal with the former by selecting a sample of similarly experienced mayors, and with the latter, which introduces a

[4] While ability is an innate quality, experience is learning while being in office. There are different opinions as to whether experience leads to more or to less spending. Dick and Lott (1993) is an example of a model in which experience causes higher spending (seniority trap): they advocate term limits because they reduce average tenure. However, deBacker (2011) finds that, empirically, seniority has little impact on spending.

selection effect if re-election is more likely when an incumbent is more able, by excluding from the analysis those mayors who were not re-elected. They provide evidence of a better performance of first-term mayors, and conclude that this is because policy makers exert more effort when subject to re-election incentives. This finding hints at a disciplining role of elections.

A limitation of the empirical literature on term limits is that existing studies never consider that if the distorting effect of elections dominates, it may be that not all years of the first term are affected in a similar way. In particular, a mayor that seeks to be re-elected only needs to spend more in the election year or one or two years before as voters' memory is likely to be limited to the most recent fiscal decisions. While empirical tests of the Rational Business Cycle models often include a term limit dummy as a control, empirical research on the effects of term limits never include pre-, post- and electoral dummies. Recent empirical research on electoral budget cycles confirms that electoral incentives may affect each year of a term differently (Akhmedov and Zhuravskaya, 2004; Aidt et al., 2011; Sjahir et al, 2013; Aidt and Mooney, 2014). In this study, we extend the existing literature by taking into account specific fiscal patterns during the two terms that mayors are in office.

## 3. Data description

### *3.1. Institutional setup*

Italy has four main levels of government: the central, regional, provincial and municipal tiers.[5] There are more than 8000 municipalities which provide a variety of services: local police, public transport and road maintenance, street lighting, waste management and a number of social (care for children and the elderly) and cultural services (theatres, libraries, museums). Spending at the municipal level amounts to about 10% of the general government's primary expenditure and 4% of national GDP.

Since the mid-nineties municipalities have funded their expenditure through own taxes (mainly property taxes),[6] charges on specific services (such as waste disposal) and grants they receive from central and regional government, which were prevalent before a reform of local finance took place. Transfers from the central government are allocated only partially according to pre-determined demographic, socio-economic and fiscal indicators, as Carozzi and Repetto (2013) show. If one aggregates municipal spending at the national level, each of these sources of revenues makes up about a third of the total. Transfers often slightly dominate the other two items, but there is a lot of geographical variance in this respect. Northern municipalities rely much more on own revenues than Southern municipalities (Bordignon, Gamalerio and Turati, 2013). Municipalities have limited autonomy to accumulate debt. Since 1999 they are subject to the Domestic Stability Pact (DSP), a

[5] Metropolitan areas were introduced as a fourth level by a constitutional change in 2001, but they are not operative, yet.

[6] Municipalities are not completely free to set tax rates, because central government decides a range of possible values.

numerical fiscal rule. DSP is the same for all municipalities,[7] but it has changed every year since its introduction. In most years the rule is on the size of (a proxy of) deficit, but in some years it consisted of a spending cap. The very fact that the rule changes every year has clearly undermined its credibility, and enforcement of these rules are a debated issue. Some authors find that the introduction of the DSP has had, in general, the effect of sounder municipal finances (Grembi, Troiano and Nannicini, 2011).[8]

A law reforming municipal governments' institutional setup was introduced in 1993. This law represented an innovation in many respects.[9] One of the most important institutional features introduced by this reform was the direct election of mayors and the introduction of a term limit rule by which a mayor cannot stand for re-election after two *consecutive* terms.[10] Under the new system, Italian mayors have a large influence on local politics and public finances: the mayor presents the budget for approval to the municipal council (which is elected distinctly on the same day as the mayor), and if the budget plan is not approved in time, new elections for both political bodies are required.[11] The very composition of the council is such that this happens quite rarely. In fact, there is a majority premium for the lists supporting the winning candidate. For municipalities with more than 15.000 inhabitants a double-ballot majoritarian electoral mechanism applies.[12] If a candidate wins the absolute majority of votes at the first ballot, the lists supporting him get 60% of the seats in the council.[13] The eventual second ballot requires voters to choose between the two candidates who collected the largest number of votes in the first round. Lists previously supporting the candidates now excluded from electoral competition may declare their support to either candidate. The same majority premium applies as in the case of a victory at the first ballot, but now 60% of the seats are assigned to all lists supporting the elected mayor, including those which started supporting him after the first ballot. This makes the case of a misalignment between a mayor and a municipal council not so common.[14]

A number of economists have investigated the relationship between this institutional arrangement and fiscal performance in Italian municipalities. Some of them have considered term limits as a control variable. Bordignon, Cerniglia and Revelli (2004) find that local tax rates are

[7] Smaller municipalities have been exempted since 2001. Our dataset only comprises cities, so all are subject to the DSP.

[8] Recent contributions by Mink and de Haan (2005) and Bartolini and Santolini (2009) find evidence that when there is a fiscal rule in force, there is a stronger electoral budget cycle.

[9] This law has been changed only once since its adoption and only in a marginal manner: the term lasted four years before 1999, five afterwards.

[10] Since 1999 the rule does not apply if an early termination due to reasons different from voluntary resignation occurs before half term has expired.

[11] The mayor always sits in the municipal council, but in municipalities with population greater than 15.000 he is not the agenda setter. He has no veto powers.

[12] For smaller municipalities there is a single ballot majoritarian system.

[13] Since it is possible for voters to split the vote, the law requires for the majority premium to be assigned to have both the candidate's victory at the first ballot and at least 40% of the votes in the council election given to the supporting lists, with no other group of lists with absolute majority.

[14] If a mayor is supported by just one list, clearly the case of a majority within the council qualifying as a coalition is ruled out. However, this is not a common case because of the proportional rules about the distribution of seats to the lists supporting the mayor in the council.

positively auto-correlated among neighboring jurisdictions when the mayors run for re-election, but not when mayors face a term limit. Depalo and Messina (2011) also investigate whether there is spatial interdependence in tax rate determination: they find that tax rates are reduced when elections are approaching, and more so when term limits are not binding. Rizzo and Zanardi (2012) consider the difference in the number of lists in municipal councils of small and large municipalities, due to the presence of different electoral rules. They find no evidence of an effect on the level of spending, but an effect on its composition, which is more dispersed under the double ballot regime. This happens only when mayors are not term-limited. Coviello and Gagliarducci (2010) show that public procurement decisions have become more objective ever since the two-term limit is applied in Italy. Boetti, Piacenza and Turati (2012) use DEA and SFA techniques to investigate the relationship between tax decentralisation and spending efficiency in the municipalities of the Turin province in 2005. More autonomous municipalities exhibit less efficient behaviours, and excess current spending is particularly high close to elections years, but there is no evidence of a term limit effect. Gennari and Messina (2012) test the "flypaper effect" on a large number of Italian municipalities between 1999 and 2006, controlling for political variables. They find evidence of an impact of the revenue structure on total and capital spending, which are also found to be characterised by an electoral cycle, and more so when a mayor is in his first term.

There are only few contributions focusing on electoral incentives and the level of expenditures. Bartolini and Santolini (2009) consider current spending in the municipalities of the region Marche in 1994-2003 but do not make a distinction between mayors' first and second term. They find that the introduction of the DSP reduced spending, but it increased its dependence on the timing of elections, in accordance to the PBC literature. Cioffi, Messina and Tommasino (2011) use a much broader sample (all municipalities, 1998-2006) and concentrate on capital expenditures. They identify an electoral cycle by which a mayor spends more before an election, but *only* if he does not belong to a national party (i.e. he is at the top of an independent list), and more so if they are term-limited. In a paper on municipal cultural spending, Dalle Nogare and Galizzi (2011) also report estimates of the impact of political determinants on *total* current spending. Their sample comprises 106 cities and the time span is 1998-2005. Spending is not found to be higher in election years, while the term limit dummy is significantly negative.

All in all, most of these contributions seem to point to the fact that in Italy elections may have a distortionary effect on the behaviour of incumbents eligible for running in the next elections, but not all of them find a difference between first- and second-term incumbents.

### *3.2. Construction of the dataset*

We consider 106 Italian municipalities, in particular the cities that are provincial administrative centres. These cities have a population size between 20,000 and 2.5 million inhabitants and are the biggest towns in their respective areas. We choose this subsample for a number of reasons. First, these

municipalities are homogeneous with respect to two relevant dimensions: they all must respect the DSP, whereas since 2001 municipalities smaller than 5.000 inhabitants have been exempted, and they all have the same electoral rule, which applies to all municipalities with more than 15.000 inhabitants (counted at the last census). Second, since these municipalities are geographically distant from each other, this mitigates possible spatial interdependence. Finally, electoral candidates for a mayoral position in administrative centres are almost always members of national parties, while in most other municipalities independent lists are quite common. Mayors belonging to independent lists have been found to behave quite differently from partisan mayors (Cioffi et al., 2012). This means that our dataset is more homogeneous and avoids variation in the degree of partisanship of candidates, which may be a confounding factor if a larger sample of municipalities is considered.

Our dataset starts in 1998 due to the introduction of the Domestic Stability Pact in 1999. According to the DSP, every municipality has to report the Italian Home Office its budget in detail to make monitoring feasible.[15] We consider the official 'certificati consuntivi' (final budget balances) made available by the Italian Home Office and concentrate on transfers from central government and capital expenditures (investment).

The transfers we consider are so-called '*central government current transfers'*.[16] These are totally non-earmarked, partly but not exclusively formula-based transfers and constitute the bulk of revenues coming from higher levels of government.[17]

As far as capital expenditures are concerned, we consider '*impegni*' which represent expenses that have actually been decided upon in the year of interest. Capital spending is highly visible to the electorate and easily targeted to groups of voters; besides, in some of the years we consider it was exempted from the DSP numerical rule. This makes it more discretionary than current expenditures, especially considering that the latter is dominated by municipal employees' wages which are quite rigid. On the other hand, capital expenditures are usually decided upon over a time horizon longer than one year, because a great part of it is about the building of infrastructure, which takes time. This makes it difficult to draw a direct connection between the level of capital spending in a year just after elections and that year's incumbent mayor. Yet, it may as well be that mayors speed up investment before the next elections so that infrastructure is ready by then.

As for political data, we construct both a term limit dummy and electoral year dummies using information provided by the Home Office. Municipal elections are staggered over time in our sample, as shown in Table (1).

(Table 1 about here)

[15] Since the DPT in 1999 set targets on the increase of fiscal aggregates, municipalities had to report both 1998 and 1999 data.

[16] We exclude capital transfers since they are mostly earmarked, and their average size is much smaller.

[17] This is not the case only in some autonomous regions. We show regressions results on samples both including and excluding the cities of autonomous regions.

Note that this implies that, in the 13 years of our sample, some cities witness two full mayoral terms and some witness one full term and two portions of terms. This is the case, for instance, for a city where elections were held in the years 2001 and 2006. The picture is complicated by the presence of terms that ended prematurely for different reasons (death of mayor, criminal conviction or resignation). Sometimes mayors step down before the end of the term to compete in higher levels of government elections. We take account of these cases in the construction of our electoral cycle dummies: they are leads of the actual electoral year dummy.

Elections usually take place in June, sometimes in autumn and rarely at other times of the year. Central and regional government elections have a different schedule.[18] The timing of elections within the electoral year is also important for the matching between political and fiscal data in our analysis. The fact that elections (almost) never take place in the first months of the year means that the fiscal policy of an electoral year can safely be attributed to the mayor in office before the elections are held. So in our dataset the year of an election is coded as belonging to the ending term.

A relevant feature of our sample is the distribution over time of the political variable that we are most interested in, i.e. the term-limit dummy. As Table 2 illustrates, this variable has no particular concentration in any single year.

(Table 2 here)

This pattern is induced by two facts: first, not all mayors choose to run again for office and are re-elected and second, elections are staggered.[19] Figure 1 shows the comparison between eligible and non-eligible incumbents in each year.

(Figure 1 about here)

As will be made clear in the next paragraph, our use of mayor fixed effects makes it unnecessary to consider a large number of controls; particularly, we can do away with both mayors' and cities' time-invariant characteristics such as partisan affiliations, regional location, etc. We do, however, use provincial GDP, population density and the shares of the population up to 14 and over 65 as covariates, as these may vary over a mayor's time in office. The inclusion of real income per capita accounts for Wagner's law. The inclusion of population density may capture possible economies of

[18] Regional elections are staggered as well. Matches between central government and or regional elections on one hand and municipal elections on the other occasionally happen, but they are not the rule. Provincial elections take place at the same time as those of their administrative centres. However, this level of government is almost irrelevant, especially as far as its spending power is concerned; in fact, many are advocating its abolition.

[19] In counting the numbers of term-limited mayors in the first years of the sample we took account of the fact that the 1993 law introducing term limits prescribed that the mayors currently in office would be counted as first term mayors, no matter their past appointments.

scale in public investment production. The demographic structure is included in the estimations to control for the different needs of a city in terms of public spending. In particular, we believe that the old and the young need more dedicated services, implying more current expenditures. So we expect that more old and young people are likely to decrease investment, because in those years when the DSP was a numerical rule on spending, or on a measure of deficit comprising capital expenditures, larger current expenditures would constrain the resources available for investment.

## 4. Empirical results

### *4.1. Empirical strategy*

The first empirical models that we use for our estimations can be represented as follows:

$$Y_{it} = \alpha_i + \delta TL_{it} + \beta X_{it} + \mu_t + \varepsilon_{it} \quad (1)$$

and

$$Y_{it} = \alpha_i + \delta TL_{it} + \beta X_{it} + \gamma Y_{it-1} + \mu_t + \varepsilon_{it} \quad (2)$$

where i = 1, 2…, 297 is a mayor and t = 1998, 1999, …, 2010 is a year. *Y* is either the log of central government transfers or the log of capital expenditures, both in real per capita terms.[20] *TL* is a term limit dummy taking on the value 1 in all years of a second term. *X* is the set of economic and socio-demographic controls described in section 3, and $\mu_t$ represents a set of time dummies, accounting for the specific context in which all mayors have to set fiscal policy in a specific year. The context includes the actual DSP in force in that year and the ideology of the ruling central government.

Cross-sectional fixed effects are represented by $\alpha_i$ in equations (1) and (2). They refer to mayors as the relevant cross-sectional unit, and their inclusion when estimating the effect of term limits is a quite novel approach. To our knowledge, the only other studies adopting it are Aidt and Shvets (2012) and de Janvry et al (2012). The use of mayor fixed effects rather than municipality fixed effects is preferable because it allows to control for unobserved characteristics of mayors. In traditional city fixed effects an omitted variable problem may be present because data on mayors' characteristics are not observable: only partisan affiliation and sometimes gender are included. Moreover, given that, in our sample, we only have mayors that ruled in just one municipality, we automatically control for time-invariant municipality characteristics by using mayor fixed effects. The standard errors that we report are clustered at the mayor-level and they are robust to heteroscedasticity.

Our first specification of the empirical model is static, the second is dynamic and accounts for the sluggishness usually observed in fiscal policy. Due to the inclusion of both cross-sectional fixed

[20] As a deflator, we use the OECD government consumption deflator.

effects and a lagged dependent variable in equation (2), the within-estimator to dynamic models yields biased estimates (Nickell, 1981). While the Nickell-bias is negligible when T is large, this bias may be a serious concern in panels with a small time dimension. Since T is on average smaller than 10 in our dataset (due to the presence of mayors who only served for one term, and those who stepped down prematurely), the empirical analysis necessitates more sophisticated estimation methods.

Several IV and GMM estimators are suggested in order to deal with the bias in dynamic panel data models. For models where it cannot be assumed that disturbances are spherical, the Arellano–Bond Difference GMM and Blundell–Bond System GMM estimators outperform their alternatives (Roodman, 2009a). Between these two options, the choice of the appropriate estimator depends on the persistence of the dependent variable. With a persistent endogenous variable the Difference-GMM estimator gives rise to finite sample biases and therefore the System-GMM estimator is recommended (Blundell and Bond 1998, 2000). Since current expenditures are likely to be persistent, we apply the robust one-step System-GMM estimator. In addition, we collapse "GMM-style" instruments set to deal with the instrument proliferation problem (Roodman, 2009b).

As a second step, we use a more detailed specification that allows us to identify within-term cycles in the fiscal variables of interest, which may be different in first and second terms. For this purpose, we use the following empirical models:

$$Y_{it} = \alpha_i + \lambda \sum_{j=0}^{3} FTelectyear_{it-j} + \rho \sum_{j=0}^{4} STelectyear_{it-j} + \beta X_{it} + \mu_t + \varepsilon_{it} \quad (3)$$

$$Y_{it} = \alpha_i + \lambda \sum_{j=0}^{3} FTelectyear_{it-j} + \rho \sum_{j=0}^{4} STelectyear_{it-j} + \beta X_{it} + \mu_t + \gamma Y_{t-1} + \varepsilon_{it} \quad (4)$$

*FTelectyear* takes on the value 1 in year 5 of the first term (the year when elections take place) and *STelectyear* takes on the value 1 in year 5 of the second term.[21] The reference year is the first year of a first term, and all *FTelectyear* and *STelectyear* coefficients are therefore to be interpreted as an indication of the difference in the fiscal policy of a given year compared to this reference year.

This empirical strategy allows for more flexibility as it does not require the effect of the electoral incentives in first terms to be constant throughout the term, allowing for the detection of a possible electoral cycle effect in line with Rogoff and Sibert (1988). We also explore the second term as we cannot rule out ex ante that party discipline or mayors' career perspectives after stepping down may play a role, which would make second terms similar to first terms.

[21] In constructing these dummies, we take account of the fact that, as pointed out in par. 3.2, not all terms last 5 years. Most of the terms ending before the fifth year do so because of either they started before 1999, when a law passed making term length change from 4 to 5 years, or the mayor resigned to compete for a seat at higher levels of government. So we assume that incumbents start their term knowing how long it will last.

## 4. Results

*4.2 Baseline estimations*

In this section, we report the estimation results that use the empirical specifications in equations (1) to (4) applied to the whole of the sample as well as geographic subsamples.

*4.2.1 Transfers from the central government*

Table 3 reports the results obtained when applying the OLS-FE estimator to transfers from central government; in models 1-4 we consider all provincial administrative centers, while in model 5, following Carozzi and Repetto (2013), we exclude those of the regions Valle d'Aosta, Trentino Alto Adige and Friuli Venezia Giulia (7 cities in all), because given the more prominent role of regional governments in these autonomous regions, the share of central government transfers in total transfers from higher levels of governments is small.

(Table 3 about here)

Whether controls are used or not (models 1 and 3), we find weak evidence of a decrease in transfers in the second term: the estimated coefficient of the TL dummy is negative and significant at the 10% significance level. Its size hints at a possible decrease in transfers during the second term of about 10%. Though the effect is not significant at standard levels, this result is in line with the evidence in Aidt and Shvets (2012) on district transfers in the US; also the magnitude of extra spending in the first term, about 10%, is comparable. Notice however that we are considering an accounting item comprising a relatively large share which is predetermined (i.e. transfers based on census population); as a consequence, our evidence indirectly shows that discretionary transfers are much higher in the first term, which may be due to first-term incumbents' higher effort. This contradicts the common view by which these transfers may be taken as exogenous (Bartolini and Santolini, 2009; Gennari and Messina, 2012).

Models 2 and 4-5 investigate possible differences between the amount of transfers in the first year of the first term and all subsequent years. Apparently, no clear cyclical pattern emerges. All controls are insignificant, except GDP.[22]

Since, as noticed, a relatively large part of these transfers are predetermined and depend on census population, we expect the series to be autocorrelated. This leads us to consider dynamic specifications, such as (2) and (4), and to adopt a dynamic panel estimation technique. The results are reported in Table 4 .

[22] This is due to the fact that in most years of the sample transfers include a portion dependent on how much revenues from central government's tax on income has been raised locally.

(table 4 about here)

As expected, the lagged dependent variable turns out to be highly significant and the coefficient is quite large. Controlling for it makes our previous evidence on the effect of term limits much stronger. Even in specifications where all years of the terms (except the first) are introduced, there is evidence that in the first term, after the first year, transfers are higher, while second term years do not differ, in terms of the size of transfers, from the baseline year.

All in all, we find that first term's transfers from central government are higher, and this is especially the case after the first year.

*4.2.2 Baseline estimations: capital expenditure*

Table 5 reports the results obtained when applying OLS-FE using capital spending as the dependent variable.

(Table 5 about here)

While among the controls both GDP and old population appear to be significant and have the expected sign, when the term limit dummy is introduced, it appears to be statistically insignificant (models 1 and 3). In models 2 and 4 we investigate this further and find that while in first term there seems to be a typical electoral cycle pattern, in which, however, the rise in expenditure is in the two years before the electoral one, not in the electoral year itself. Considering that in most cases elections take place in June, it is reasonable to think that mayors do not increase capital spending in the last 6 months of their electoral campaign, also because we consider the accounting item "impegni", and it may take some months between the decision to spend and actual expenditure.[23] In model 4 the electoral year dummy, though not significant, even presents a negative sign. This, again, is not in contradiction with PBC, because we are investigating investments, so that a possible slump in spending does not mean that voters observe a decrease in the number or quality of the public services provided. What a capital spending slump in an election year signals is simply the stop of a multi-year expenditure aimed at improving, say, street lighting or museums' premises just in time for voters to test the increase in municipal capital stock in the months previous to the elections.

The insignificant term limit dummy is however at odds with electoral cycle models' predictions: how do we reconcile the findings in models 1 and 3 with those in models 2 and 4? The clue is to look at what happens in second term. Capital expenditures are higher than that of the baseline year, occasionally (and erratically) in a statistically significant way. The standard errors of the second-term dummies are all quite high, apart from the second term's final year. All in all, first and

[23] This is the case, for instance, when a municipality does not spend in-house, but it must sign procurement contracts.

second terms do not appear to be too different. This may be the consequence of the fact that, since mayors may aspire to political positions at other levels of government after the end of their career as mayors and/or party discipline may play a role, electoral incentives are present in the second term as well as in the first.

Before drawing any definite conclusion, we must however check the robustness of our results to the inclusion of a lagged dependent variable among the regressors. We show the results in Table 6.

(table 6 about here)

First of all, we observe that the estimated coefficients of the lagged dependent variable are not so high,[24] and insignificant when economic and socio-demographic controls are included. This questions the validity of equations (2) and (4) in treating capital spending. It is therefore with caution that we comment on these results. Apparently, the first term electoral cycle shows up here, too, though shorter and not so strong. The electoral year's slump is here present not just in the first, but also in the second term. Second term capital expenditure is found to be higher than the level in baseline year up to the last year but one, though never in a significant way.

*4.3 Discussion*

The joint consideration of our analyses of transfers and capital expenditure levels is problematic. While both seem to highlight that elections distort incumbents' behavior rather than induce more discipline, when we deal with transfers there is a difference between first term and second term, while when capital expenditure is considered this difference fades away.

Before trying to reconcile these findings, we wish to say that this difference is interesting in itself. Generally speaking, our results point to the fact that the evidence on pork is not sufficient to conclude that term limits matter, in terms of spending. Identifying pork with district or lower levels of governments' spending is not correct, and concentrating just on pork may be misleading.[25]

The reason why pork and spending may have different reactions to the presence/absence of electoral incentives may be due to the fact that local governments also collect own revenues and, to the extent that the institutional context allows it, resort to debt. Boetti et al. (2010) highlight that the revenue structure in the South and the Centre-North part of Italy are quite different. In our sample, the average per capita level of current transfers is € 310 in the South, while in the Centre-North it is

[24] That municipal investment is not so autocorrelated may depend on the type of competences municipalities have in Italy. Building a hospital may take ten years, but that is regional, not municipal spending. Municipal investment typically has shorter horizons.

[25] This is not to say that Aidt and Shvets (2012) model is deficient. The focus on transfers could still be justified if voters cared not just about public spending, but also about pork in itself. There is often local media coverage on the pork an incumbent brings home, and this may be interpreted as a sign of competence regardless of the consequences on spending.

€176,50. Such a big difference leads us to believe that the two parts of the country should be considered in separate regressions, which we present in Table 7. Perhaps the puzzling second term differences in the trends of transfers and spending are just the result of the merging of two distinct contexts.

(table 7 about here)

As a matter of fact, the Centre-North and South appear to be subsamples in which the presence/absence of electoral incentives impact quite differently.

As far as transfers are concerned, in the South there is strong evidence of a clear electoral cycle throughout the first term; second terms dummies are never significant (but have sometimes large standard errors). On the contrary, in the Centre-North the electoral dummies never reach statistical significance, though the standard errors of the positive coefficients associated with the two years previous to the elections at the end of the first term are large; negative signs prevail in second term.

The different degrees of statistical significance are coherent with the different role of transfers in municipal budgets: exerting effort to increase transfers makes sense only if transfers are already an important revenue item.

The estimations that use capital expenditures as dependent variable suggest that in the North a clear electoral cycle emerges in the first term, and (unlike in the case of the whole sample) not in the second. Clearly, given the previous analysis on transfers, this cycle has little to do with pork. So term limits do matter in the Centre-North, as they mitigate the distorting effect of elections on incumbents' spending policy; however, the evidence shows that the underlying mechanism is probably more in line with Nordhaus (1977) and Rogoff and Sibert (1988) than with Aidt and Shvets (2011). In the South one would expect higher pork to be immediately translated into higher expenditure, but our evidence denies such a direct transmission. We find marginal significance only for the positive coefficient the first year of the second term, though in all years except the electoral ones the estimates are positive and have large standard errors. A possible explanation is that pork partially substitutes for higher taxes there.

**5. Robustness checks**

In our previous analysis we have disregarded the fact that a possible explanation for the observational equivalence between first term and second term that sometimes emerges may be due to the presence of a selection effect induced by voters' equilibrium strategy in the elections after the first term, by which they reconfirm an incumbent only when he signals himself as competent (Alt et al., 2011; Smart and Sturm, 2013). De Janvry et al. (2012) suggest that, whenever an empirical strategy uses mayor fixed effects, a simple way to control for the selection effect is to consider a subsample only comprising

mayors who have served two terms. We therefore follow them and check the robustness of our previous results accordingly.

(Table 8 about here)

All in all, also considering that the sample is smaller here, there does not seem to be a lot of difference between the regression results in Table 8 and those of the same models in Table 3 and 5, which suggests that the selection effect is not relevant in this context. Possibly pork and spending are just two of the relevant items in electoral campaigns, and there are multiple signals that voters consider.

## 6. Conclusions

The details of electoral systems differ across countries for historical and cultural reasons. This is also true with regard to the use of term limits either for legislators or executives at different levels of government. The extant empirical and theoretical literature analyzing the effects of term limits is vast, and as far as research on the effects on fiscal policy are concerned, it does not reach a clear verdict as to whether the use of term limits is desirable. This normative question is difficult to answer even if the effects on fiscal policy were clear, as its answer depends on a country's objectives. We do not question desirability here; we adopt a purely positive approach. Our goal is to provide evidence that helps us answer the question whether and how term limits influence fiscal policy.

The existing empirical evidence is limited to the US and is based on data at the national or state level. We make a contribution to this literature by focusing on the Italian municipal level. Our dataset covers 106 Italian cities over the time period from 1998 to 2010. In terms of the empirical methodology, we use mayor fixed effects to control for many sources of time-invariant unobserved heterogeneity at the municipality and mayor level.

We provide evidence that the impact of electoral incentives on pork and spending may be different at the local level of government, so that looking at pork only does not seem appropriate. In both cases, however, there seems to be a prevailing distorting role played by elections whenever an incumbent may be re-elected. In this sense, our contribution is, with reference to spending, in line with previous works using Italian municipal data (Bartolini and Santolini, 2009, Dalle Nogare and Galizzi, 2011) and, since the cities we consider are all subject to a numerical fiscal rule, our contribution may also be viewed as an addition to the recent evidence on the surviving of political budget cycles when numerical fiscal rules are in force (Mink and de Haan, 2005). Our evidence also points to a clear difference between the Centre-North part of the country, where there appears to be a classical electoral cycle whenever incumbents may be re-elected, and the South, where pork is in line with the predictions in Aidt and Shvets (2012).

Our results survive an important robustness check, namely the control for a possible selection effect. Selection may lead second terms to be characterized by more competent mayors; in order to disentangle the effects of electoral incentives and selection we follow the innovative empirical strategy suggested by de Janvry et al. (2012).

One important avenue for future research concerns the dependent variables that we use: overall current transfers and capital spending per capita. While these variables are a good starting point, the availability of more public accounting data will make our empirical tests more focused. In particular, we need a finer proxy for pork, i.e. central government transfers that are (almost) purely discretionary whereas our data refers to total transfers that are only partially discretionary. Also, in order to have a better understanding of the interaction between the Domestic Stability Pact prescriptions and the spending dynamics, we intend to consider deficits as well.

A second extension we will consider is accounting for the role played by experience. A mayor who is in his second term may not only differ from himself being in office during his first term because of the absence of electoral incentives, but also with respect to his experience in office. The effect of experience on fiscal performance is not a-priori clear, and the existing literature provides different predictions. Dick and Lot (1993) associate longer tenure with higher spending. On the other hand, other authors stress the importance of office-holders' learning-by-doing (Padrò i Miquel and Snyder, 2006). The relevance of experience for fiscal policy is questioned in the recent empirical literature (deBacker, 2011). Aidt and Shvets (2012) claim that a more experienced legislator should be better able to bring pork home. As a consequence, their evidence of higher pork during legislators' first terms is said to be an underestimation of the effect that electoral incentives have on incumbents' behavior. The same might be argued with respect to our findings: our current estimates should be regarded as conservative estimates. Further investigation is however necessary to disentangle the effects of electoral incentives and experience.

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**Tables.**

**Table 1: Number of municipal elections in Italian administrative centres per year**

| Year | No. of elections |
|---|---|
| 1998 | 33 |
| 1999 | 33 |
| 2000 | 13 |
| 2001 | 24 |
| 2002 | 28 |
| 2003 | 10 |
| 2004 | 32 |
| 2005 | 16 |
| 2006 | 26 |
| 2007 | 27 |
| 2008 | 13 |
| 2009 | 33 |
| 2010 | 13 |
| ALL | 301 |

**Table 2: Number of term-limited mayors in Italian cities per year**

| Year | No. of term-limited mayors |
|---|---|
| 1998 | 18 |
| 1999 | 31 |
| 2000 | 45 |
| 2001 | 47 |
| 2002 | 41 |
| 2003 | 42 |
| 2004 | 41 |
| 2005 | 36 |
| 2006 | 34 |
| 2007 | 36 |
| 2008 | 39 |
| 2009 | 33 |
| 2010 | 33 |
| ALL | 476 |

**Figure 1: eligible and term-limited mayors in office in each year**

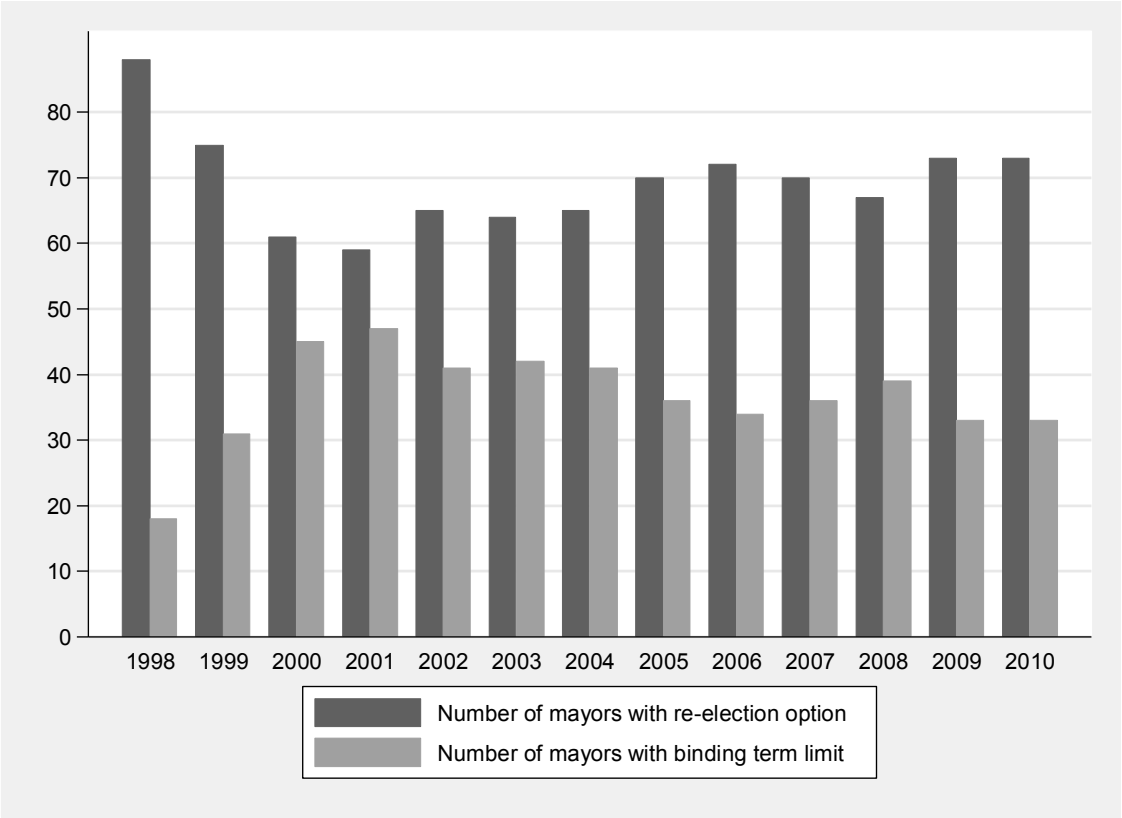

**Table 3. Term limits of mayors and log of real transfers per capita over the electoral cycle, 106 municipalities, 1998 - 2010, FE estimations**

| | Model1 | Model2 | Model3 | Model4 | Model5 |
|---|---|---|---|---|---|
| **Second term (term-limited mayor)** | -0.107* | | -0.105* | | |
| | (-1.753) | | (-1.729) | | |
| **Election year - 3 in 1st term** | | 0.006 | | 0.005 | -0.020 |
| | | (0.113) | | (0.108) | (-0.420) |
| **Election year - 2 in 1st term** | | 0.057 | | 0.051 | 0.020 |
| | | (1.029) | | (0.894) | (0.369) |
| **Election year - 1 in 1st term** | | 0.099 | | 0.096 | 0.050 |
| | | (1.432) | | (1.399) | (0.802) |
| **Election year in 1st term** | | 0.131* | | 0.122 | 0.063 |
| | | (1.757) | | (1.625) | (1.018) |
| **Election year - 4 in 2nd term** | | 0.045 | | 0.035 | -0.005 |
| | | (0.493) | | (0.377) | (-0.069) |
| **Election year - 3 in 2nd term** | | 0.045 | | 0.028 | -0.004 |
| | | (0.370) | | (0.228) | (-0.041) |
| **Election year - 2 in 2nd term** | | -0.029 | | -0.051 | -0.052 |
| | | (-0.243) | | (-0.426) | (-0.452) |
| **Election year - 1 in 2nd term** | | -0.097 | | -0.118 | -0.121 |
| | | (-0.657) | | (-0.809) | (-0.866) |
| **Election year in 2nd term** | | -0.117 | | -0.148 | -0.186 |
| | | (-0.676) | | (-0.860) | (-1.134) |
| **Real GDP per capita** | | | 0.038* | 0.041* | 0.045** |
| | | | (1.739) | (1.911) | (2.433) |
| **Population density** | | | -0.000 | -0.000 | -0.000 |
| | | | (-0.164) | (-0.105) | (-0.264) |
| **Share of old population** | | | 0.016 | 0.015 | 0.017 |
| | | | (0.972) | (0.882) | (1.000) |
| **Share of young population** | | | -0.054 | -0.044 | -0.034 |
| | | | (-1.306) | (-1.050) | (-0.809) |
| **Mayor fixed effects** | YES | YES | YES | YES | YES |
| **Year fixed effects** | YES | YES | YES | YES | YES |
| **Observations** | 1366 | 1366 | 1366 | 1366 | 1279 |
| **Number of mayors** | 297 | 297 | 297 | 297 | 280 |
| **R-squared** | 0.495 | 0.505 | 0.499 | 0.510 | 0.604 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***) [2] t-statistics in parentheses [3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level [4] Model 5 excludes seven provinces (Aosta, Bolzano, Trento, Udine, Pordenone, Gorizia, Trieste) because they belong to autonomous regions where central government transfers are a small fraction of transfers from higher levels of government.

**Table 4. Inclusion of lagged log of real transfers per capita, 106 municipalities, 1998 - 2010, System GMM estimations**

| | **Model3** | **Model4** |
|---|---|---|
| *Dependent variable* | *Transfers* | |
| **Lagged dep. var.** | 0.735*** | 0.749*** |
| | (7.699) | (9.017) |
| **Election year - 3 in 1st term** | 0.152* | 0.155* |
| | (1.727) | (1.771) |
| **Election year - 2 in 1st term** | 0.194** | 0.192** |
| | (2.173) | (2.173) |
| **Election year - 1 in 1st term** | 0.192** | 0.181** |
| | (2.569) | (2.468) |
| **Election year in 1st term** | 0.188** | 0.185** |
| | (2.156) | (2.158) |
| **Election year - 4 in 2nd term** | 0.009 | 0.016 |
| | (0.124) | (0.223) |
| **Election year - 3 in 2nd term** | 0.090 | 0.095 |
| | (1.108) | (1.186) |
| **Election year - 2 in 2nd term** | 0.051 | 0.058 |
| | (0.703) | (0.813) |
| **Election year - 1 in 2nd term** | 0.063 | 0.065 |
| | (0.759) | (0.788) |
| **Election year in 2nd term** | 0.045 | 0.054 |
| | (0.544) | (0.667) |
| **Other control variables** | NO | YES |
| **Mayor fixed effects** | YES | YES |
| **Year fixed effects** | YES | YES |
| **Observations** | 1065 | 1065 |
| **Number of mayors** | 243 | 243 |
| **Chi-square** | 2575.252 | 4701.515 |
| **Hansen-test (p-value)** | 0.657 | 0.627 |
| **AR(1)-test (p-value)** | 0.000 | 0.000 |
| **AR(2)-test (p-value)** | 0.831 | 0.809 |
| **Number of instruments** | 30 | 34 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***)

[2] t-statistics in parentheses

[3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level

[4] Instrument set is collapsed and two-step Windmeijer correction of standard errors for small sample bias is used

**Table 5. Term limits of mayors and log of real capital expenditures per capita over the electoral cycle, 106 municipalities, 1998 - 2010, FE estimations**

| | Model1 | Model2 | Model3 | Model4 |
|---|---|---|---|---|
| **Second term (term-limited mayor)** | 0.026 | | 0.017 | |
| | (0.419) | | (0.277) | |
| **Election year - 3 in 1st term** | | 0.034 | | 0.032 |
| | | (0.610) | | (0.580) |
| **Election year - 2 in 1st term** | | 0.119** | | 0.115** |
| | | (2.042) | | (1.977) |
| **Election year - 1 in 1st term** | | 0.167*** | | 0.165*** |
| | | (2.695) | | (2.640) |
| **Election year in 1st term** | | 0.002 | | -0.007 |
| | | (0.025) | | (-0.089) |
| **Election year - 4 in 2nd term** | | 0.177** | | 0.151* |
| | | (2.066) | | (1.714) |
| **Election year - 3 in 2nd term** | | 0.188 | | 0.154 |
| | | (1.594) | | (1.278) |
| **Election year - 2 in 2nd term** | | 0.256* | | 0.214 |
| | | (1.887) | | (1.545) |
| **Election year - 1 in 2nd term** | | 0.228 | | 0.181 |
| | | (1.329) | | (1.036) |
| **Election year in 2nd term** | | 0.079 | | 0.024 |
| | | (0.394) | | (0.115) |
| **Real GDP per capita** | | | 0.037** | 0.039** |
| | | | (2.470) | (2.585) |
| **Population density** | | | -0.001 | -0.000 |
| | | | (-0.798) | (-0.703) |
| **Share of old population** | | | -0.044** | -0.044** |
| | | | (-2.499) | (-2.411) |
| **Share of young population** | | | 0.056 | 0.053 |
| | | | (1.292) | (1.227) |
| **Mayor fixed effects** | YES | YES | YES | YES |
| **Year fixed effects** | YES | YES | YES | YES |
| **Observations** | 1370 | 1370 | 1370 | 1370 |
| **Number of mayors** | 297 | 297 | 297 | 297 |
| **R-squared** | 0.138 | 0.159 | 0.149 | 0.169 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***) [2] t-statistics in parentheses [3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level

**Table 6. Inclusion of lagged real capital expenditure per capita, 106 municipalities, 1998 - 2010, System GMM estimations**

| | Model1 | Model2 |
|---|---|---|
| *Dependent variable* | *Capital expenditures* | |
| **Lagged dep. var.** | 0.219** | 0.172 |
| | (2.062) | (1.622) |
| **Election year - 3 in 1st term** | 0.072 | 0.101 |
| | (0.747) | (1.075) |
| **Election year - 2 in 1st term** | 0.083 | 0.122 |
| | (0.911) | (1.335) |
| **Election year - 1 in 1st term** | 0.088 | 0.130* |
| | (1.140) | (1.707) |
| **Election year in 1st term** | -0.091 | -0.059 |
| | (-1.133) | (-0.753) |
| **Election year - 4 in 2nd term** | 0.107 | 0.116 |
| | (1.312) | (1.489) |
| **Election year - 3 in 2nd term** | 0.022 | 0.039 |
| | (0.254) | (0.468) |
| **Election year - 2 in 2nd term** | 0.094 | 0.105 |
| | (1.019) | (1.183) |
| **Election year - 1 in 2nd term** | 0.046 | 0.065 |
| | (0.429) | (0.639) |
| **Election year in 2nd term** | -0.106 | -0.084 |
| | (-0.957) | (-0.794) |
| **Other control variables** | NO | YES |
| **Mayor fixed effects** | YES | YES |
| **Year fixed effects** | YES | YES |
| **Observations** | 1069 | 1069 |
| **Number of mayors** | 243 | 243 |
| **Chi-square** | 236.768 | 294.132 |
| **Hansen-test (p-value)** | 0.479 | 0.572 |
| **AR(1)-test (p-value)** | 0.000 | 0.000 |
| **AR(2)-test (p-value)** | 0.121 | 0.159 |
| **Number of instruments** | 30 | 34 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***)

[2] t-statistics in parentheses

[3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level

[4] Instrument set is collapsed and two-step Windmeijer correction of standard errors for small sample bias is used

**Table 7. Subsamples of Centre-Northern and Southern Italian municipalities, 106 municipalities, 1998 - 2010, FE estimations**

| | **Model1** | **Model2** | **Model3** | **Model4** |
|---|---|---|---|---|
| *Dependent variable* | *Capital expenditure* | | *Transfers* | |
| *Subsample* | *North* | *South* | *North* | *South* |
| **Election year - 3 in 1st term** | -0.016 | 0.125 | -0.007 | 0.082* |
| | (-0.299) | (1.105) | (-0.115) | (1.733) |
| **Election year - 2 in 1st term** | 0.111* | 0.121 | 0.036 | 0.132** |
| | (1.703) | (1.118) | (0.493) | (2.593) |
| **Election year - 1 in 1st term** | 0.159** | 0.125 | 0.112 | 0.151** |
| | (2.401) | (1.009) | (1.210) | (2.410) |
| **Election year in 1st term** | 0.025 | -0.082 | 0.130 | 0.209*** |
| | (0.324) | (-0.560) | (1.236) | (3.172) |
| **Election year - 4 in 2nd term** | 0.069 | 0.314* | 0.061 | 0.112 |
| | (0.701) | (1.762) | (0.507) | (1.524) |
| **Election year - 3 in 2nd term** | 0.050 | 0.332 | 0.068 | 0.137 |
| | (0.393) | (1.382) | (0.417) | (1.394) |
| **Election year - 2 in 2nd term** | 0.075 | 0.459 | -0.048 | 0.131 |
| | (0.529) | (1.654) | (-0.338) | (1.068) |
| **Election year - 1 in 2nd term** | 0.069 | 0.331 | -0.127 | 0.108 |
| | (0.377) | (0.943) | (-0.710) | (0.751) |
| **Election year in 2nd term** | -0.120 | 0.232 | -0.184 | 0.214 |
| | (-0.549) | (0.555) | (-0.874) | (1.161) |
| **Other control variables** | YES | YES | YES | YES |
| **Mayor fixed effects** | YES | YES | YES | YES |
| **Year fixed effects** | YES | YES | YES | YES |
| **Observations** | 907 | 463 | 904 | 462 |
| **Number of mayors** | 188 | 109 | 188 | 109 |
| **R-squared** | 0.197 | 0.206 | 0.582 | 0.495 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***) [2] t-statistics in parentheses [3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level

**Table 8. Robusness check: Subsample without mayors who only served one term, 106 municipalities, 1998 - 2010, FE estimations**

| | Model1 | Model2 | Model3 | Model4 |
|---|---|---|---|---|
| *Dependent variable:* | *Capital expenditures* | | *Transfers* | |
| **Second term (term-limited mayor)** | -0.003 | | -0.087 | |
| | (-0.049) | | (-1.533) | |
| **Election year - 3 in 1st term** | | 0.037 | | -0.001 |
| | | (0.517) | | (-0.012) |
| **Election year - 2 in 1st term** | | 0.164** | | 0.049 |
| | | (2.297) | | (0.722) |
| **Election year - 1 in 1st term** | | 0.184*** | | 0.103 |
| | | (2.729) | | (1.338) |
| **Election year in 1st term** | | -0.020 | | 0.143* |
| | | (-0.258) | | (1.827) |
| **Election year - 4 in 2nd term** | | 0.140 | | 0.038 |
| | | (1.472) | | (0.398) |
| **Election year - 3 in 2nd term** | | 0.137 | | 0.034 |
| | | (1.043) | | (0.263) |
| **Election year - 2 in 2nd term** | | 0.187 | | -0.054 |
| | | (1.232) | | (-0.416) |
| **Election year - 1 in 2nd term** | | 0.140 | | -0.115 |
| | | (0.741) | | (-0.726) |
| **Election year in 2nd term** | | -0.011 | | -0.153 |
| | | (-0.051) | | (-0.809) |
| **Other control variables** | YES | YES | YES | YES |
| **Mayor fixed effects** | YES | YES | YES | YES |
| **Year fixed effects** | YES | YES | YES | YES |
| **Observations** | 966 | 966 | 962 | 962 |
| **Number of mayors** | 148 | 148 | 148 | 148 |
| **R-squared** | 0.161 | 0.187 | 0.495 | 0.507 |

[1] Stars indicate significance levels at 10% (*), 5% (**) and 1%(***) [2] t-statistics in parentheses [3] Standard errors are robust to heteroscedasticity and clustered at the mayor-level