Political budget cycles with informed voters: Evidence from Italy

Luca Repetto

Political budget cycles with informed voters: evidence from Italy

Luca Repetto

Political budget cycles with informed voters: evidence from Italy [*]

Luca Repetto[†]

First version: November 2014
This version: May 2016

Abstract

I exploit a reform that required Italian municipalities to disclose their balance sheets before elections to study whether having more informed voters affects the political budget cycle. To start, investment spending in the year before elections is 28.5% higher than in the election year and this increase is mainly financed with new debt and sales of public assets. Taking advantage of the staggered timing of municipal elections, I estimate that the reform reduced this pre-electoral spending increase by around one-third. I also study the role of local newspapers in disseminating municipal financial information to voters and I find that the reduction in spending after the reform is twice as strong in provinces with above-median local newspapers sales per capita. I interpret these results as evidence that mayors react to more informed voters by reducing spending manipulation.

Keywords: Information, Political budget cycles, accountability, Italian municipalities
JEL codes: D72, E62, P16

1 Introduction

Understanding why and to what extent politicians manipulate public spending for electoral purposes is important to design policies that ensure accountability and limit opportunism. Political budget cycles have been studied extensively at different levels of government and the most convincing evidence is found at the local level (see, e.g. Alesina and Perotti 1995 Akhmedov and Zhuravskaya 2004, Drazen and Eslava 2010). The typical theoretical explanation for why budget cycles arise even with rational voters is that politicians enjoy an informational advantage over their citizens (Rogoff 1990, Persson and Tabellini 2002). For example, politicians may borrow more before elections to finance an increase in the provision of public goods. If borrowing can be kept hidden from voters before elections, they may mistake this increase in expenditures for a signal of the incumbent's ability to provide more public goods. Politicians can then exploit this informational advantage and increase spending before elections in order to gain votes. A direct implication of this mechanism is that spending manipulation should decrease with the level of information of voters. Although the asymmetry of information is crucial in explaining budget cycles, evidence on this mechanism is remarkably scarce. This is likely due to the difficulty in finding exogenous variation in voters' information in most settings.

In this paper I use variation in voters' information induced by a reform carried out in Italy in 2008 to study how budget cycles are affected by information. I start by showing that the budget cycle in Italian municipalities is substantial. Investment expenditures fluctuate significantly during the term and reach, in the year before elections, a level that is 28.5% higher than in the election year. This cycle is most evident in the types of expenditures that are most visible to voters, such as roads, parks and public housing and is mainly financed with borrowing and sales of public assets.

I then turn to the question of how voters' information affects the budget cycle by exploiting a reform that, as of 2008, required Italian municipalities to disclose their balance sheet before elections. The balance sheet is the main accounting document of a municipality and contains detailed information on expenditures, revenues and debt of the previous year. It is a rich source of information that can be used by the opposition and the local media as an accountability device for the incumbent. Before the 2008 reform voters did not have access to the balance sheet before local elections. The reform changed the deadline for approval and required all municipalities to approve and disclose their balance sheet before elections, therefore providing voters with a new source of information.

The over 8,000 Italian municipalities can be divided into five groups, each on a different, 5-year long, election schedule. The staggered timing of local elections is due to historical reasons and is particularly useful for estimating the effect of the reform because every year there are municipalities in different years of the term.[1] Using a difference-in-differences approach, I compare spending in different groups before and after the reform for each year

---

[1]The staggered timing of elections allows the inclusion of time dummies in estimation and is crucial for separating the budget cycle from other fluctuations due to, for example, changes in macroeconomic conditions.

of the term, while controlling for municipality and time effects. Results show that, in the post-reform years – when the balance sheet is made public before elections – the magnitude of the cycle decreases substantially. In particular, the pre-electoral year increase in spending is reduced by about one-third. Using a simple model, I interpret this result as suggesting that mayors react to more informed voters by reducing spending manipulation.

To investigate how information is conveyed to voters, I consider the impact of local media on the budget cycle. Local media decrease the cost of information for voters by providing summarized information on local matters at a low cost.[2] Using data on sales of local newspapers, I test whether the effect of the reform varied with the availability of local newspapers. Indeed, in provinces where newspaper sales were above the median, the effect of the reform is almost twice as strong as the baseline estimate. On the contrary, in other provinces the impact of the reform is almost negligible. Overall, these results strengthen the evidence on the information hypothesis and suggest that the presence of more informed voters weakens the incentives for politicians to strategically raise spending before elections.

In additional analyses, I study whether increasing spending before an election is an effective way to gain votes. To this end, I estimate how the probability of being re-elected (conditional on running again) depends on a series of spending variables measured in the last year of the term. Results suggest that doubling investment expenditures in the pre-election year is associated with a 2% higher probability of re-election. This effect appears to be rather large, considering that investment expenditure figures vary significantly from one year to the other and even a single large project may raise per-capita investment expenditures by a sizeable amount. Consistently with the main results, the electoral reward of additional spending is reduced by about one-quarter after the reform, although coefficients are imprecisely estimated.

The analysis in this paper contributes to a growing literature on the importance of information for political accountability. Recent studies show that the timely disclosure of information on politicians' performance has large effects on the actions of both voters and politicians. Publishing negative corruption audits before elections, for example, reduces re-election rates (Ferraz and Finan, 2008) and turnout is higher when voters are made aware of the incumbent's activities through information cards (Banerjee et al., 2011). Politicians, on the other hand, appropriate less public money if they know that they will be audited (Olken, 2007) and increase relief expenditures in areas with higher newspaper circulation and where voters are more informed (Besley and Burgess 2002, Stromberg 2004). This work contributes to this literature by showing that the effects of a simple change in the disclosing policy of an already existing accounting document on politicians' behaviour is substantial. Also, given that similar types of accounting documents are used in several other countries, these results are arguably easier to generalize to other settings than those from small-scale randomized experiments.

---

[2] In Italy, local newspapers play a key role in disseminating municipal financial information to voters and, by monitoring politicians' behaviour while in office, they increase accountability (Drago, Nannicini and Sobbrio, 2014).

This paper is also a formal test of information-based models of budget cycles.[3] Papers on the effect of information on the budget cycle typically rely on cross-country data and uses an indirect measure of information. Gonzalez (2002) uses indices for the level of democracy as measures of transparency in Mexico and shows that the budget cycle is stronger in more democratic times. Shi and Svensson (2006), instead, measure information with an index based on the number of radios per capita and a freedom of press indicator and show that cycles are reduced in countries with more informed voters. This paper overcomes two important drawbacks of this literature: first, by exploiting quasi-experimental variation in voters' information it provides more credible estimates. Second, the use of a direct measure of voters' information - the availability or not of the balance sheet - mitigates concerns on measurement error and endogeneity that usually arise when a proxy is used instead.

Budget cycles have recently been brought back to the attention of academic research by Alesina and Paradisi (2015), who use the introduction of a new real estate tax in Italy to show that municipalities that are in their pre-election years set a rate lower than others. Estimation uses the staggered election timing and essentially assumes that municipalities in the pre-election year at the time the tax was introduced are comparable with the others. However, there are good reasons to believe that the grouping of municipalities by the year of election is not entirely the result of pure chance, so simple comparison of average outcomes is unlikely to yield unbiased estimates. In this paper I consider this issue in detail, and propose alternative specifications and robustness checks to ensure that the results are not driven by differences in spending trends between groups.

2 Conceptual framework

Although there is little debate on the existence of political budget cycles, it is intuitively difficult to reconcile their existence with rational voters. To guide the empirical analysis, I describe in this section the key ingredients and implications of a simple moral hazard model of electoral competition based on Shi and Svensson (2006), leaving a complete formal presentation for the Appendix. The main feature of the model is the incumbent's ability to manipulate a particular policy instrument, for example borrowing, in order to bias the voters' inference process before elections in her favour.

Voters derive utility from a consumption good, a public good g_t, and from being informed on the municipal government's activities. The preference for being informed is randomly distributed across voters. Voters will incur the cost of information only if the utility they derive from being informed exceeds the cost they must bear. For this reason, only a fraction

---

[3]The first formal model of opportunistic pre-electoral manipulation is Nordhaus (1975). Most models postulate that budget cycles arise from asymmetries of information. While Rogoff and Sibert (1988) and Rogoff (1990) emphasize the role of adverse selection, more recent papers by Persson and Tabellini (2002) and Shi and Svensson (2006) propose the alternative view that fluctuations are a consequence of a moral hazard problem: incumbents have the possibility to increase spending by manipulating policy instruments observable to voters only with a delay.

π of the electorate decides to become informed.

Politicians set the level of taxes τ_t and borrowing d_t at the beginning of each period. The final amount of public good provided, however, also depends on the incumbent's competence level η_t^j in the following way:

$$g_t = \tau_t + d_t - R(d_{t-1}) + \eta_t^j,$$

where $R(d)$ is a convex cost function of public borrowing. In a given year, competence is the combination of the current competence shock and the shock in the previous year. Voters, hence, can learn something about the future competence of the incumbent by observing the level of public good provided today.

At the beginning of period t, the incumbent sets the level of taxes and borrowing without observing her competence level.[4] Then, the current competence shock is realized and the amount of public good g_t is residually determined. Taxes τ_t and aggregate spending g_t are always observed by all voters before the election. Additionally, a fraction π of voters also observes d_t and, therefore, can infer the competence level. At the end of period t, elections take place. Voters re-elect the incumbent if the expected utility they derive from doing so is higher than the utility they would obtain from electing the challenger. In $t + 1$, the timing is the same as in t except for the fact that no elections take place. New elections are called at the end of period $t + 2$, in which everything is the same as in t.

The fact that a fraction of the population is not informed creates incentives for the incumbent to increase the supply of the public good before elections, and to finance this increase by borrowing. The larger the fraction π of uninformed voters, the larger the spending increase in the pre-election year will be. However, since non-informed voters are rational agents, they know the incumbent's strategy and, in equilibrium, correctly infer the amount of borrowing and, hence, the competence level. As a consequence, the incumbent chooses in equilibrium a positive level of borrowing and uses it to finance a boost in public good spending, but cannot fool voters into believing that this increase is due to competence alone.

In this model, the reform that requires municipalities to disclose the balance sheet before elections can be interpreted as a decrease in the price of information. As this price decreases, a larger fraction of the electorate decides to incur the cost of being informed. Since the equilibrium level of borrowing (and, consequently, of public good provision) decreases with the fraction of informed voters, one should observe that, in the years following the reform, pre-electoral borrowing and spending boosts are attenuated.

---

[4]Notice that the fact that neither politicians nor voters observe competence before choosing the level of taxes and borrowing implies that the optimal choices are the same for politicians of all levels of ability. Differently from (Rogoff and Sibert, 1988), in which politicians observe their type, the only equilibrium of the game is pooling.

3 Background information

3.1 Municipalities

Municipalities are the smallest administrative unit in Italy and are headed by a mayor. The mayor appoints the local government (*Giunta*) and is also part of the *Consiglio Comunale*, the town council, with limited legislative powers.

Italy had 8,109 municipalities as of 2010, although this number changes slightly over the years because of merges and separations. Municipal governments' revenues come from taxes, transfers from the central or regional government or the European Union, revenues from fees (e.g. building permits, provision of public services, museums) or fines, capital transfers and sales of public assets or, finally, by borrowing. Municipalities are in charge of providing public goods and services to citizens, such as public transportation, welfare - for example, assistance to elderly people, nursery schools and public housing - and manage public utilities (Gagliarducci and Paserman, 2012). Municipalities have only limited freedom in setting the local real estate tax rate (called ICI until 2012, then IMU) and, although taxes are their most important source of income, they are still very dependent on transfers, mostly from the central and regional governments (Carozzi and Repetto, 2016).

Municipalities are grouped into 110 provinces and 20 regions. Regions are the most important sub-national administrative units and have substantial legislative, political and fiscal autonomy. Five regions are granted additional autonomy for being home to language minorities or for being islands: Valle d'Aosta, Trentino-Alto Adige, Friuli-Venezia Giulia, Sardegna and Sicilia.

Since 1999, Italian municipalities are subject to the Domestic Stability Pact, a set of rules the central government established in order to comply with the EU convergence criteria. The specific rules changed during the years and include expenditure caps and a ceiling on municipal revenues and debt, as well as the requirement that only investment expenditures can be financed with debt. While in 1999 and 2000 all municipalities were subject to the pact, starting from 2001, small municipalities (those with less than 5,000 inhabitants) were exempted. The effects of the Stability Pact on local finances have been widely studied.[5] Overall, the rules of the Stability Pact affect the municipal governments' policy decisions and may therefore also affect the political budget cycle. I show that the possibility that the Pact is driving the results is unlikely in a robustness check in section 6.5.

3.2 Budgets and balance sheets

Every December, the municipal government prepares a draft of the *budget*, a planning document that details both the total amount and distribution of the municipal expenditures in the

---

[5]Bartolini and Santolini (2009) conduct a panel data analysis on the current expenditures of 246 Italian municipalities and show that the Pact reduced current expenditures but strengthened the opportunistic behaviour of mayors in pre-electoral years. Gregori (2014) investigates how the composition of the municipal budget reacts to variation in the fiscal rules of the Pact over the years.

year to come and how they will be financed. The budget is discussed in the council and must be approved by the end of the year. The *balance sheet* is instead the ex-post document that records the effective amounts spent and received by the municipality in the year before. The revenues side is disaggregated into taxes, transfers, non-tax, disposal of public assets, loans and third-party services. Expenditures, on the other hand, are classified into current, investment, loan reimbursement and third-party services. The balance sheet is publicly available and, since 2008, must be approved by April 30.

3.3 The reform

In October 2008, a government *decreto*, later transformed in law in December, required municipalities, starting from 2009, to approve and disclose the balance sheet two months earlier, from June 30 to April 30.[6] The lemma that changed the approval date was a small part of a large text that dealt with general accounting principles for local governments (including regions and provinces) and extraordinary measures to contain the increase in health care expenditures regions were facing at the time. One of the members of the Parliament who discussed the law confirmed, in a personal conversation with the author, that the change in the deadline was not the main purpose of the law and that it was motivated by the necessity, for the central government, to have more timely figures on the financial conditions of Italian municipalities. Information on the financial status of the municipalities is crucial for drafting the central government budget law, which contains, among other things, the allocations of municipal transfers for the following year. Given both the marginal role the change in the deadline played in the law as a whole and the fact that legislators introduced the change for reasons other than affecting mayors' choices, it is reasonable to assume that the reform was unexpected to mayors and voters. In the empirical analysis, however, I also consider the possibility that mayors anticipate the effect of the reform and resign strategically to avoid its effects. Results from an instrumental variables estimation provide evidence that endogenous resignations are not driving the results.

## 3.4 Balance sheets as a source of information for voters

Balance sheets contain information on the financial status, such as the municipality debt level, the amount and distribution of investment and current expenditures, and the level of deficit. Voters might find this information useful for assessing the incumbent's performance as an administrator. The presence of the opposition in the town council facilitates the diffusion to both the media and voters of irregularities or anomalies and enhances the role of the balance sheet as an accountability device. Local media, either on newspapers or online, are those typically covering these issues. Browsing online and in the archive of a few local newspapers, one often finds headlines quoting a member of the opposition, (e.g. "They

---

[6]The *decreto legge* in question is number 154, approved on October 7, 2008. The *decreto* was later transformed in law 189/2008 (the full text is available at http://www.parlamento.it/parlam/leggi/08189l.htm).

[the municipal government] cancelled public safety funding") or figures about the deficit or some important expenditures category ("€25,000 for social spending"). These articles, naturally, appear more frequently in the weeks immediately before and after the approval and disclosure of the balance sheet.

In order to obtain more systematic evidence on the interest the balance sheet sparks in voters, I searched jointly the words *Bilancio Consuntivo* (Italian for "balance sheet") in Google Trends. Google Trends gives a 0-100 index of interest over time of a given word or phrase, compared to the total number of Google searches done during that time. Plotting the Trends index in figure 1 confirms that interest in the balance sheet among Google users rises substantially in the month of approval or around it and fades in other months. Although there could be several factors generating this cyclical pattern (for instance, town accountants might be more actively looking for information on the balance sheet during the approval month), it is reasonable to assume that a large fraction of it corresponds to the rise in voters' interest.

FIGURE 1

GOOGLE TRENDS SEARCH OF THE WORDS "BILANCIO CONSUNTIVO"
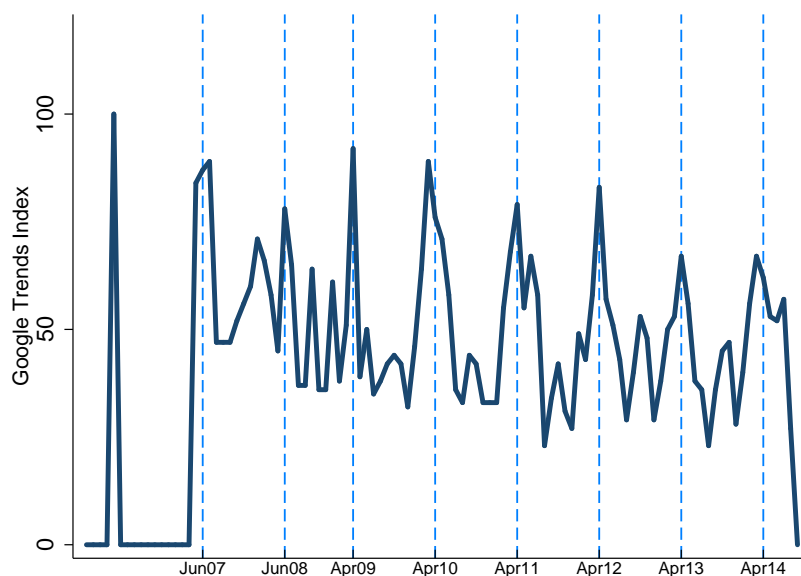
Notes: Google Trends interest over time index of the search "bilancio consuntivo", 2006-2014. Google Trends analyses a percentage of Google web searches to determine how many searches have been done for a specific word or phrase compared to the total number of Google searches done during that time. Dashed lines correspond to months of balance sheet approval (June until 2008 and April afterwards). Google searches reach their yearly peak in balance sheet approval months, and fall in other months. Notice that Google Trends data are available only from 2004 and, for our search, are noisy until 2007.
Source: http://www.google.com/trends/explore

The availability of the balance sheet before elections would not have a first order effect on information if voters could rely on estimates from the municipal budget. However, budget quantities are often unreliable: in figure 6 in the Appendix one can see that budget quanti-

ties are excellent predictors for realized current expenditures, with a correlation of 0.96, but not for investment expenditures. The correlation between the budget forecast and what is effectively spent in investment project is, in the sample, only 0.40. Also, budget quantities are much larger, on average, that realized values. Conversations with local politicians confirmed to the author that this "overshooting" is due to the fact that, while there is no penalization in forecasting a high amount and then lower estimates, in case expenditures exceed those planned in the budget the council approval is required. The balance sheet, then, acquires additional relevance as an information device as a consequence of the fact that budgets do not provide an accurate picture of how much is spent in investment projects in each year.

In order to know with certainty if voters have access to the balance sheet information before elections, one needs the exact date of actual approval in the council. Unfortunately, this piece of information is not included in the original data sources, as municipalities are not required to communicate the exact approval date to the Ministry of Internal Affairs. An assumption implicit in the estimation procedure is that the municipal balance sheet was never available to voters before the reform and always after. This assumption rules out the possibility that, before the reform, some municipal government may decide to approve the balance sheet before elections even if the deadline would allow them to postpone it. However, if early approval were prevalent, the reform should have no impact on the information level of voters. In this sense, the estimated effect of the reform should be interpreted as a lower bound.

4 Data

4.1 Data Sources

The final dataset is obtained by combining several sources. First, balance sheets for all municipalities are gathered using publicly available data from the Ministry of Internal Affairs' website. This dataset contains data on revenues and expenditures categories for each year since 1999. Those data are complemented with information on mayors and on the election results. For each election and for each candidate, the dataset includes votes obtained by each candidate and vote share, supporting party, birth town and date of birth. Finally, data from the Italian Statistical Office (ISTAT) are also used for geographical characteristics and population of municipalities. Finally, data on local newspaper diffusion are gathered from a private agency called ADS (*Accertamenti Diffusione Stampa*). Further details on sources and a description of the variables used in the empirical analysis are available in the Appendix.

4.2 Sample

The sample consists of 6,705 municipalities (out of the 8,109 existing municipalities in 2010) for the years 1999-2012 years. The autonomous regions of Trentino-Alto Adige, Friuli-Venezia Giulia, Valle d'Aosta, Sicily and Sardinia are excluded because they have different accounting

and electoral rules, and municipalities are financed via different channels. I also drop 23 municipalities that held special elections in days other than the one fixed by the Ministry (usually because of early dissolutions of the council for *mafia* presence). Finally, I replace as missing some outliers that have investment expenditures per capita 100 times above the median (see the Appendix for more details). These are most likely coding errors or cases in which a large emergency transfer was required. Then, I replace as missing the expenditures that exceeded 10 times the sample standard deviation.[7] I do the same for outliers in the revenue categories. Among these municipalities with unusually large variables are *enclaves* like Campione d'Italia and towns hit by the 2009 earthquake. In order to select the sample as little as possible, I keep in the analysis all terms that ended prematurely for a government crisis, resignation of the council or the mayor or other causes. In the empirical analysis, I include an indicator for such terms; dropping them altogether is another possibility and leaves results virtually unchanged.

4.3 Summary Statistics

Figure 2 gives an overview of the financial status of municipalities over the sample period. Municipalities had, in 2005, revenues and expenditures for €80 billion Euros (roughly 4.8 percent of the GDP), an amount that started to decline since then until reaching about 60 billion Euros in 2012. On the revenues side, disposal of public assets and taxes account for more than half of the total, whereas transfers contribute for 10-25 percent. Expenditures are heavily concentrated in current expenditures and investment projects, with services and loans accounting for a much smaller small fraction. Investment expenditures have started decreasing both as a fraction of the total and in absolute terms starting in 2005 and reached a minimum in 2012, while current expenditures are relatively stable, with their share of the total even slightly increasing with time. Being mostly running and maintenance costs, current expenditures are generally considered much harder to manipulate (Aidt, Veiga and Veiga, 2011).

---

[7]Using as trimming threshold 5 or 15 percent does not significantly alter any of the results in the following.

FIGURE 2

EVOLUTION OF REVENUES AND EXPENDITURES

(A) REVENUES BY CATEGORIES

TOTAL IN BILLION €   FRACTION OF TOTAL

(B) EXPENDITURES BY CATEGORIES

TOTAL IN BILLION €   FRACTION OF TOTAL
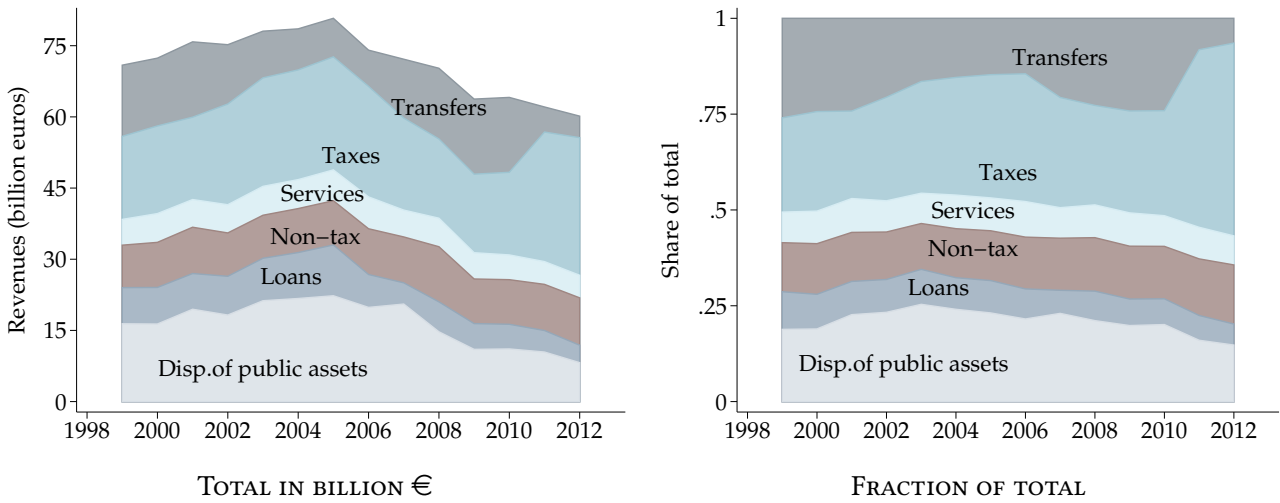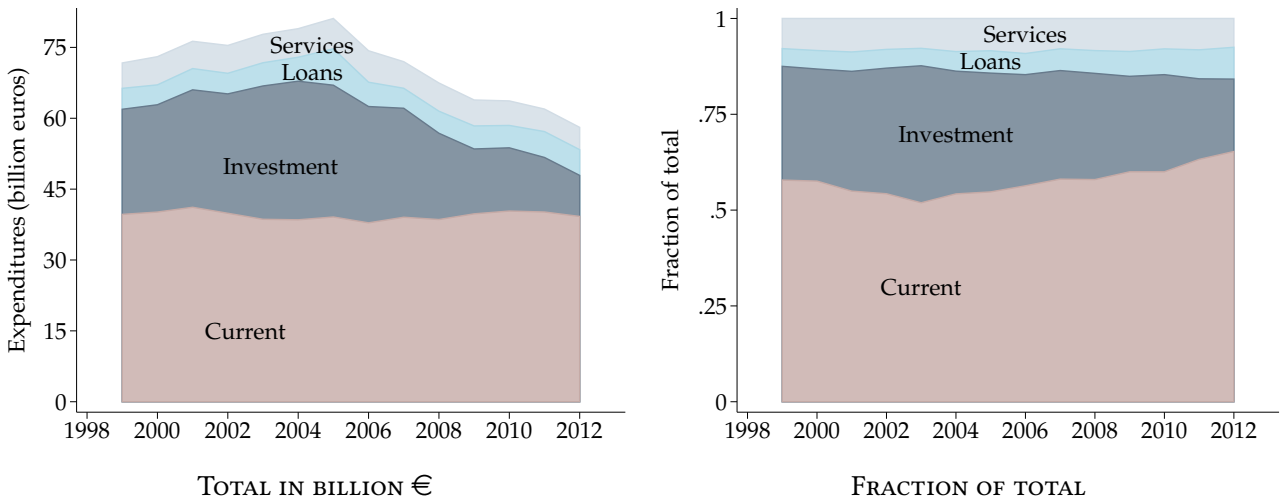
Notes: Figures are in 2005 euros, deflated using the St. Louis FED GDP deflator. Sample is composed of 6,705 municipalities and excludes municipalities from special regions. The upper panels plot total revenues for municipalities both in absolute terms and as a fraction of the total. The lower panels show, instead, total expenditures. The discrepancy between revenues and expenditure is due to the presence of balance sheet deficits or surpluses that are not plotted.

11

Table 8 in the Appendix shows some descriptive statistics for the sample used throughout. Municipalities before and after the 2008 reform spend roughly the same in current expenditures, but there are differences in capital expenditures due to the general declining trend described in figure 2. Correspondingly, on the revenue side disposals of assets and new loans decreased after the reform, as well as services and transfers. Increases in tax and non-tax only partially made up for the overall decrease in revenues. The pattern is qualitatively the same even looking at budget quantities, as shown in table 9 in the Appendix.

The second panel shows that, geographically, Italian municipalities tend to be small on average, with an average population of around 7,500 and have a density of approximately 320 inhabitants per square kilometre. Mayors are, on average, about 50 years old and predominantly male, well educated and, in our sample, more than one third of them is term-limited.

4.4 Election timing

Municipal elections are held every five years (they were four until 2000) to replace the mayor, the municipal government and the council. Mayors, since 2000, are term-limited after two consecutive terms.[8] In case the mayor or at half of the councillors resign before the end of the term, new elections are called without the possibility of forming a new coalition.[9] Mayors, upon winning, obtain a large majority premium (two-thirds or, for large municipalities, 60 per cent) of the council seats that ensures government stability.

FIGURE 3

MUNICIPALITIES HOLDING ELECTIONS IN EACH YEAR
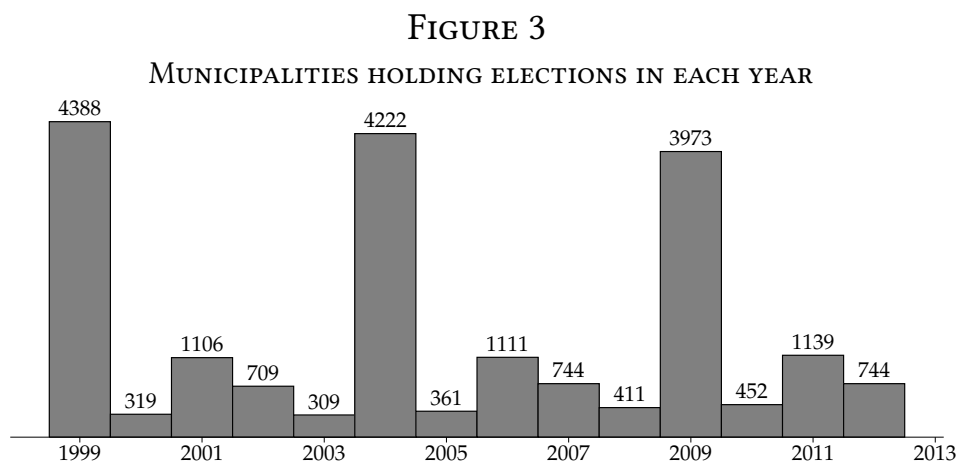
Notes: Frequency of Italian municipalities holding elections, 1999-2012. Special regions are excluded.

Figure 3 shows the timing of elections. The exact day of the election is chosen each year by decree of the Minister of Internal Affairs among all Sundays in the period April 15 to

---

[8]Before 2000 the maximum was three. The term limit only applies to consecutive terms, and it is not uncommon to see a mayor stepping down as vice-mayor for one legislature and then running again.

[9]Early termination can be due not only to a government crisis but also to dissolution for suspected *mafia* presence in the council, commissioner intervention, merging with other municipalities or violations of the law. In the sample, 11.5 per cent of legislatures ended prematurely. In the empirical analysis, I include a dummy for terms ended prematurely, and as a robustness check I also run all specifications excluding those terms. Results are not significantly affected.

June 15, and is the same for all municipalities. More than half of the municipalities in the sample had elections in 1999 (and, subsequently, 2004 and 2009). Of the remaining ones, 319 voted in 2000, 1106 in 2001, 709 in 2002 and 309 in 2003. The presence of these five groups of municipalities has historical reasons since, after the Second World War in 1946, all the ruling war councils had to be substituted. However, in the subsequent decades several cities - among which Rome in 1947 - underwent government crises and new elections were called prematurely. Early terminations for other reasons and modifications in the law also changed the length of the term and the exact timing of elections, inducing more towns to enter their own electoral cycle.[10]

In table 10, in the Appendix, I report summary statistics for municipalities divided according to the year of first election. The group of municipalities voting in 1999 includes those that never experienced an early termination, and might therefore be a special group. I deal with some of the concerns from using a potentially selected group as the control group in the next section.

5 Empirical analysis

To estimate the effect of voters' information on mayors' decisions, one could imagine a randomized experiment in which a randomly chosen group of municipalities - the treatment group - is required to approve and disclose the balance sheet before elections. The remaining municipalities are, instead, allowed to approve the balance sheet after elections and serve as control group. Randomization ensures that treatment and control group are comparable in the sense that they differ, on average, only in the level of information voters dispose of. The information level of voters is therefore uncorrelated with any other determinant of mayors' decisions, and a comparison of the mean outcome in the two groups would give an estimate of the effect of interest.

The difference-in-differences approach exploits the quasi-experimental variation in the information level of voters induced by the 2008 reform to mimic this experiment. The "treatment" of the reform affects municipal governments in different years of the term, so that municipalities in other years of the term can serve a control.

5.1 Empirical model

Let y be the outcome of interest (for instance, investment expenditures), i a municipality and t a year, and consider the following baseline model:

$$y_{it} = \alpha + \beta_1' \mathbf{d} + \beta_2' \mathbf{d} \cdot Post_t + \gamma' X_{it} + \delta_t + \mu_i + \lambda_r \cdot \delta_t + \epsilon_{it}, \tag{1}$$

---

[10]For a brief discussion on the exogeneity of election dates in Italy, see Coviello and Gagliarducci (2010).

where **d** is a set of dummies for each year in the term defined as follows:

$$
\mathbf{d} = \begin{cases}
d_{\tau-3} = 1 & \text{three years before election} \\
d_{\tau-2} = 1 & \text{two years before election} \\
d_{\tau-1} = 1 & \text{one year before election} \\
d_{\tau+1} = 1 & \text{one year after election}
\end{cases}
$$

and zero otherwise, where the indicator for an election year, d_τ, is excluded from estimation to avoid multicollinearity and acts as reference group. The year in term indicators collected in **d** capture the fluctuations in spending due to the political cycle and vary cross-sectionally by group, because municipalities in different groups are in different points of the electoral cycle.[11] To estimate the effect of the reform on the political cycle, those variables are interacted with an indicator $Post_t$ that equals one in 2008 and in the following years. The variable $Post_t$ is one since 2008 because, although the first balance sheets affected by the reform are those approved in 2009, they refer to spending decisions made in 2008. The implicit assumption is that, although the *decree* was approved in October 2008, the reform already had an effect on the spending of 2008. The baseline effect of the reform is subsumed in the year effects δ_t and therefore not included.

The vector X_{it} includes municipality, mayor-level and political controls: to control for determinants of spending connected to size or geographical characteristics, I include a cubic polynomial in population, population density, altitude and surface in km^2, and an indicator for being a province capital. Mayor-specific traits are controlled for by years of education, gender and age. Besides the level of education of the mayor, I include per capita yearly spending in education as an attempt to proxy for differences in the level of education of voters. To account for possible endogenous resignations I include a dummy for terms that ended early or in which a government commissioner was in power.[12] Furthermore, I control for the vote share obtained in the last election – to account for differences in the freedom to choose the level of spending among mayors –, for the mayor being term-limited or not and, finally, for the turnout in the last municipal elections. These variables are meant to control for differences in the political participation – both in terms of voters' interest and in the strength of the incumbent government – across municipalities. Unobserved determinants of y that are fixed at the municipality level are controlled for by the municipality fixed effect μ_i whereas the year effects δ_t absorb common shocks. Region-year interactions, $\lambda_r \cdot \delta_t$ control for possible trends in spending in different areas of Italy. Last, all unobserved variables fall into the error term, ϵ_{it}, which, as usual, is assumed to be uncorrelated with the variables of interest at all leads and lags.

---

[11]Early terminations of the term, due for instance to the resignation of the mayor, lead to early elections and cause some municipalities to change group. In these cases, the dummies **d** also vary between municipalities.

[12]Excluding terms that, for any reason, terminated prematurely (10.9% of the total), leaves results unaffected. The issue of endogenous resignations is further investigated in section 6.5.

5.2   Identification

Estimation of model 1 relies on both cross sectional variation, by comparing municipalities in different years of the cycle, and time variation, by comparing the same municipality in different points in time. To estimate the budget cycle parameters, municipalities in a particular year of the term act as control group for those in other years. In each year, then, treatment and control groups change. To estimate the effect of the reform, the difference-in-differences approach exploits the fact that municipalities are affected by the reform in different years of the term. Municipalities is the same group are first compared with other municipalities in different years of the term and then with themselves before the reform, to obtain the difference-in-differences estimator.

The inclusion of fixed effects controls for any time-invariant difference across municipalities. If variation in the political cycle indicators were only at the group level, the inclusion of municipality effects would not affect the estimation of β_1 and β_2. However, in some cases, premature terminations of the term cause municipalities to change group, so that the indicators **d** varies, in these cases, not only across the five groups but also across municipalities. Given that in each year only a group of municipalities holds elections, it is also possible, and indeed very desirable, to include time dummies in estimation. In fact, if the electoral schedule were the same for everybody it would not be possible to separate the effect of the reform from that of other shocks common to all municipalities like, for instance, changes in the economic conditions or a generalized decline in municipal resources caused by the economic downturn.

5.3   Assessing the difference-in-differences model

The critical identifying assumption in the difference-in-differences model is that, in absence of the reform, the budget cycles in the five groups of municipalities would be comparable, so that municipalities in different years of the cycle could serve as valid control groups for each other. Figure 3 in section 4 shows that groups are of different size and a large fraction of municipalities holds elections in 1999, 2004 and 2009. As discussed in section 4, this clustering is due various factors - such as early resignations, crises, changes in the law - that made some municipalities change group. Most of the determinants of group membership date back several years and probably do not affect spending trends today. If this were the case, the "parallel cycles" assumption implicit in the difference-in-differences approach should hold. However, if those differences cause one group to evolve differently from others across time, between group comparisons in different points in time would not only capture the effect of the reform but also that of different group trends.

To obtain evidence for parallel trends, I plot in figure 4 investment spending per capita, in yearly averages, for the five groups in all years of the term. In the left panel one can see that spending rises in the years before election, usually peaking in the pre-election year, and then drops in the year of the elections. Although the individual cycles show some heterogeneity,

FIGURE 4

BUDGET CYCLE IN INVESTMENT SPENDING PER CAPITA IN THE FIVE GROUPS
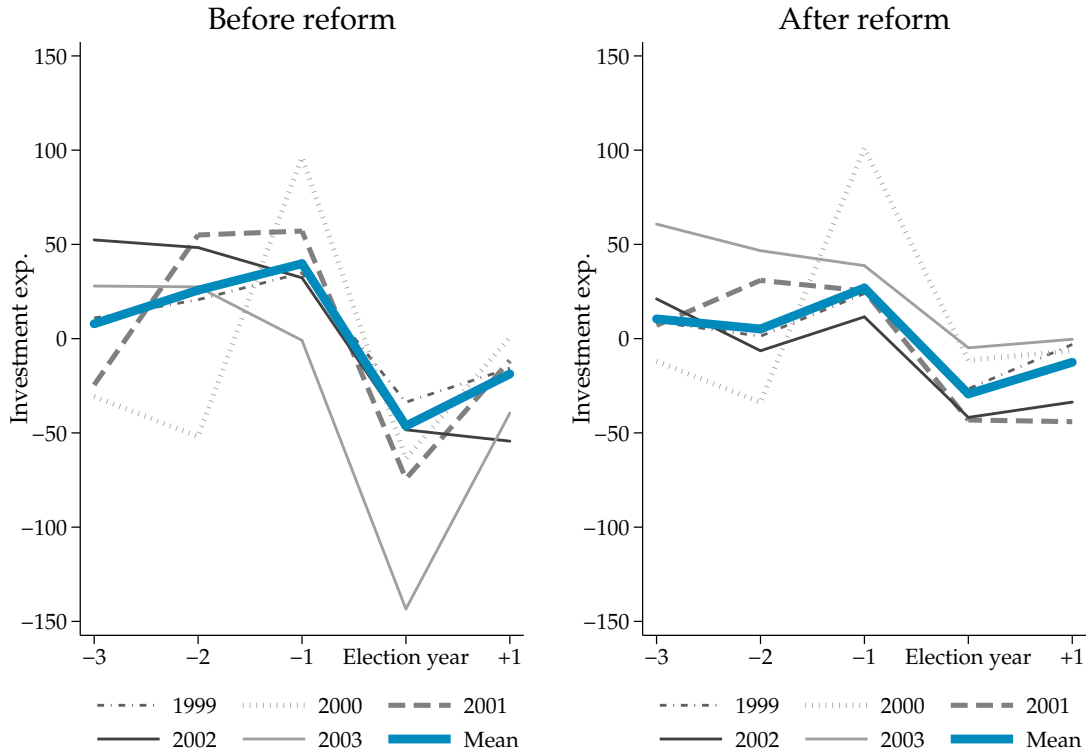
Notes: Municipalities are grouped according to their first year of election in the sample. The y-axis variable is average investment spending in each group, in 2005 Euros per capita after removing time and municipality effects. The left panel plots averages for each year of the term only for years up to 2007, whereas the right panel plots averages for years from 2008 onwards. The average across groups, weighted by the number of municipalities belonging to each group, is highlighted.

spending in all five groups follows a similar cyclical pattern.[13] The rightmost panel plots the same variable for years after the reform, that is from 2008 to 2012. Comparing the two panels shows that cycle fluctuations after the reform are reduced in all groups, and the same pattern emerges by inspecting group means. Although this evidence broadly support the difference-in-differences model, I will further challenge its validity in section 6.5 by controlling for different configuration of municipality specific trends and excluding from estimation observations from each one of the five groups.

---

[13]The somewhat erratic behaviour of the 2000 and 2003 groups might be due to the fact that they are the smallest: together, they account for less than one-tenth of the sample.

6 The effect of information on mayors' decisions

6.1 The budget cycle and the effect of the reform

In table 1 I report results for the difference-in-differences model given by equation 1 (coefficients for controls are omitted here and reported in table 11 in the Appendix). The first column shows estimates from the specification with controls and year dummies. In the second column I add municipality fixed-effects, which control for fixed differences across municipalities. Municipal spending fluctuates strongly during the term: taking the election year as the baseline and concentrating on column 2 estimates, expenditures three years before are roughly €86 per capita higher. Compared to the sample mean of €487.6, this amounts to a 17.5% increase. Spending further increases two years before elections and peaks in the pre-election year, when it is 28.5% higher than in election years. In the year after election the cycle begins again, with a more moderate increase over the baseline of about 10%.

After the 2008 reform, the magnitude of the fluctuations in each year of the term decreases substantially, especially two years before elections and in the pre-electoral year, where fluctuations are reduced by, approximately, one-half and one-third. In the third column of table 1 I show that results are robust to excluding all controls but municipality and year effects. Finally, the last column of table 1 shows the importance of including time effects when estimating the political budget cycle: the point estimates for both coefficients are much larger because they also capture the nation-wise declining trend in municipal spending common to all municipalities. Figure 5 represents in a graph the results in column 2 of table 1, by plotting the estimated coefficients for the year of the term indicators and how they change after the reform. From the figure, the negative effect of the reform on the deviations from the electoral year - and, therefore, the variance of the fluctuations - is apparent and sizeable.

Disaggregating investment expenditures in categories reveals differences in the cycle fluctuations: as table 12 in the Appendix shows, there is strong evidence of pre-electoral spending increases in investment in roads and transportation, social, sport, culture and parks and public housing (both grouped under the "territory" category). Roads and territory are the largest categories in terms of total spending and are also arguably the most visible to voters. The fact that the largest fluctuations are found in visible categories is in line with results in, e.g., Drazen and Eslava (2010). Interestingly, the pre-electoral spending increase in these categories is also the one that drops the most after the reform.

TABLE 1

THE EFFECT OF THE REFORM - BASELINE RESULTS

|	Baseline specification		W/o controls	W/o year effects
	(1)	(2)	(3)	(4)
	Invest. exp.	Invest. exp.	Invest. exp.	Invest. exp.
3 years before election	88.9***	85.6***	81.6***	107.9***
	(9.50)	(9.91)	(9.67)	(7.14)
2 years before election	105.1***	104.7***	103.4***	108.2***
	(9.02)	(9.25)	(8.89)	(6.94)
1 year before election	141.8***	138.8***	122.1***	211.4***
	(11.80)	(12.20)	(10.96)	(9.22)
1 year after election	50.8***	52.0***	51.4***	54.2***
	(9.34)	(9.79)	(9.40)	(6.43)
3 years before elect.*Post	-40.4***	-40.1***	-33.0**	-174.8***
	(15.12)	(15.30)	(14.98)	(9.76)
2 years before elect.*Post	-63.9***	-70.8***	-68.2***	-196.6***
	(14.62)	(15.01)	(14.36)	(9.26)
1 year before elect.*Post	-54.9***	-55.1***	-46.4***	-193.1***
	(16.19)	(16.72)	(15.21)	(11.11)
1 year after elect.*Post	-16.2	-21.7	-19.9	-109.1***
	(15.00)	(15.65)	(15.03)	(8.98)
Mean of dep. var.	487.6	487.6	485.0	487.6
Controls	Y	Y	N	Y
Year Effects	Y	Y	Y	N
Year-Region Effects	Y	Y	Y	N
Municipality Effects	N	Y	Y	N
R^2	0.16	0.41	0.40	0.12
Obs.	85385	85385	90279	85385

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. *Post* is an indicator for years from 2008 onwards. All columns but the last include year dummies. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 The effect of the reform on revenues

With the disclosure of the balance sheet before elections, voters obtain access not only on the level and composition of expenditures, but also to how those are financed. If voters prefer certain types of financing over others, it is reasonable to expect that, after the reform, municipal governments substitute unpopular financing means such, for instance, local taxes, with those that voters consider less costly. In table 2 I estimate the same baseline model of equation 1 but using, as dependent variables, various categories of the revenue side of the balance sheet in 2005 Euros per capita.

FIGURE 5

DEVIATIONS IN INVESTMENT EXPENDITURES RELATIVE TO THE ELECTION YEAR
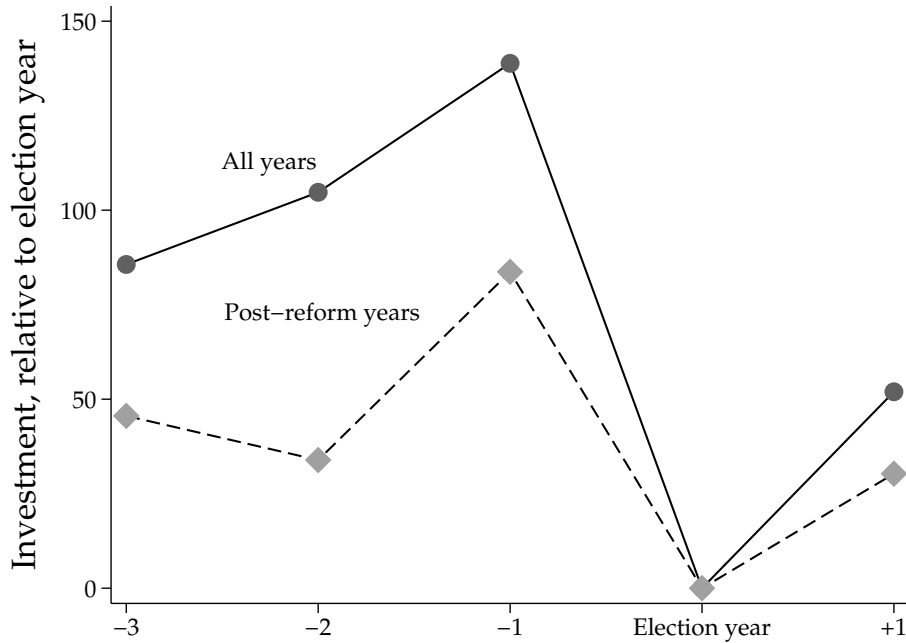
Notes: This graph is based on the estimated coefficients in column 2 of table 1. Both lines are estimated deviations from the election year in average investment expenditures. The "All years" line plots budget cycle estimates relative to all years in the sample ($\hat{\beta}_1$), whereas the "Post-reform years" line reports the estimated budget cycle in post-reform years ($\hat{\beta}_1 + \hat{\beta}_2$).

Municipalities can finance expenditures by selling public assets (including land, buildings and construction permits), new loans, tax and non-tax revenues, fees from the provision of services, and transfers, including funds from the national and regional governments and the European Union. The shares of revenues coming from taxes, disposal of public assets and transfers are the largest and together account for more than half of the total. Interestingly, table 2 shows that much of the political cycle activity appears in disposal of public assets, which increases roughly by 20% of the sample mean in the year before elections and in loans (+32%). In other categories such as, for instance, taxes, services and non-tax revenues, spending fluctuations are much smaller and below 2% of the mean. After the reform, the cycle in disposal of public assets and in loans is reduced, and the increase in the pre-electoral year is about one-third lower after the reform. Consistent with the hypothesis that those means of financing are among the least preferred by voters, the reduction in pre-election years is large in both categories. Given that the total size of the balance sheet decreases after the reform, the fact that transfers do not decrease and even exhibit a small increase in the pre-election years after the reform suggests that they may have at least in part taken the place of loans and asset sales as a way to finance additional spending. Overall, these results show that mayors not only change the total amount of investment spending after the reform, but also modify

19

TABLE 2

BASELINE RESULTS FOR REVENUES, BY CATEGORY

	(1) Disposals	(2) Loans	(3) Non-tax	(4) Services	(5) Tax	(6) Transf.	(7) Revenues
3 years bf. elect.	46.5***	24.8***	2.44**	0.69	5.78***	0.35	79.2***
	(8.64)	(3.32)	(1.08)	(1.30)	(0.85)	(0.90)	(11.46)
2 years bf. elect.	60.9***	30.1***	2.99***	0.34	6.05***	0.95	100.0***
	(7.88)	(3.12)	(1.14)	(1.20)	(0.80)	(0.96)	(11.04)
1 year bf. elect.	72.8***	38.3***	2.89**	1.27	2.35***	1.80*	131.7***
	(9.89)	(3.75)	(1.24)	(1.39)	(0.86)	(0.98)	(14.02)
1 year aft. elect.	31.9***	15.2***	1.09	0.47	3.35***	0.38	49.0***
	(8.43)	(3.19)	(0.83)	(1.22)	(0.72)	(0.85)	(11.63)
3 years bf. elect.*Post	-14.2	-12.9***	0.046	-1.71	5.64***	-6.93***	-36.3*
	(13.48)	(4.90)	(1.81)	(1.89)	(2.10)	(2.26)	(19.76)
2 years bf. elect.*Post	-46.5***	-14.3***	1.31	-2.05	3.82*	-5.37**	-88.5***
	(12.71)	(4.75)	(1.96)	(1.80)	(1.96)	(2.32)	(19.25)
1 year bf. elect.*Post	-22.5	-12.5**	2.28	0.70	2.79*	0.68	-30.1
	(14.15)	(5.31)	(1.79)	(1.93)	(1.66)	(1.97)	(22.12)
1 year aft. elect.*Post	-15.0	-8.55*	1.48	-3.03*	1.08	-0.11	-29.2
	(13.19)	(4.52)	(1.64)	(1.80)	(1.68)	(1.87)	(20.15)
Mean of dep. var.	358.0	120.1	181.7	109.7	365.3	245.2	1430.9
Controls	Y	Y	Y	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	Y	Y	Y	Y
Municipality Effects	Y	Y	Y	Y	Y	Y	Y
R^2	0.38	0.33	0.77	0.49	0.87	0.85	0.60
Obs.	85292	86154	87188	86212	87278	87340	86765

Notes: In each column the dependent variable is a different category of revenues in per capita 2005 Euros. Sample sizes differ slightly because of missing values in some of the categories. Controls, year and municipality dummies are included in all specifications. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the sources of financing.

6.3 Local newspapers and the effect of the reform

The Google trends data described in section 3.4 suggest that voters actively look for information on the balance sheet after approval. The baseline results show that the large fluctuations in spending across the term are significantly reduced after the reform. A possible explanation is that mayors, knowing that the balance sheet will be of public domain before elections, have less incentives to manipulate spending. A large part of the information voters receive comes through the active role of local media. Local newspapers usually either directly report news

on spending decisions or interview members of the opposition or the ruling party in order to comment the main figures on the balance sheet after approval. Overall, local media helps the diffusion of information by decreasing the cost of information, hence increasing the number of informed voters and the quality of the information they have.

The impact of news coverage on political outcomes has been shown to be significant. Politicians that are under less media scrutiny tend to work less and transfer less resources to their constituency (Stromberg 2004, Snyder and Stromberg 2010). Local newspapers in Italy cover extensively political matters at the municipality level and still play a mayor role as a source of information to citizens. Drago, Nannicini and Sobbrio (2014) show that the presence of local media has large effects on several political outcomes: the entry of newspapers providing local news increases turnout in municipal elections, the re-election probability of the incumbent and the efficiency of the municipal government. If the reduction in the budget cycle after the reform is due to mayors being concerned about information reaching voters and if newspapers facilitate this flow of information, one should observe the effect of the reform to be stronger in areas with relatively many readers.[14]

To test this hypothesis, I gather data on newspaper sales per capita from ADS (*Accertamenti Diffusione Stampa*), an agency that certifies sales and circulation of the most sold newspapers in Italy at the province level.[15] Among the 63 available newspapers, I consider national press, and therefore exclude, 18 newspapers that in 2008 were sold in more than 10 (out of 110) provinces.[16] I then use the number of copies of local newspapers per 100 inhabitants (yearly averages of daily sales) as a variable that captures the diffusion of local media at the province level. Equation 1 is then estimated for two samples: the first sample contains all provinces where local newspaper sales are above the national median, whereas the second contains those below.[17] Results are reported in table 3 and show that the effect of the reform is indeed much stronger in the group of municipalities with higher access to newspapers, both for expenditures and for new loans, and weaker, and in some year even statistically indistinguishable from zero, in provinces with low sales. Overall, these results support the hypothesis that the effect of the reform is strengthened by the presence of local newspaper that, by covering key issues on municipal matters, facilitate the access to the information contained in the balance sheet.

---

[14]Another possibility arises if voters in areas with high readership rates are more informed on the financial status of the municipalities before the reform. In this case, we would observe the opposite effect, that is in areas with high readership the reform would have little or no effect. We find no evidence in favour of this hypothesis in the data.

[15]Data for the sample of regions used in this paper are represented in a map in figure 7 of the Appendix.

[16]As an exception, I consider *La Stampa*, a Turin-based newspaper as local press although it is available everywhere in Italy. This is because more than half of its sales are concentrated in Piedmont and, importantly, the newspaper is bundled with local editions, different for each provinces, that deal extensively with local matters.

[17]Estimating by splitting the sample in two yields results that are similar to including a dummy for sales above the median, interactions of this dummy with the *Post* dummy and the years in term, as well as the interaction of the three. By splitting the sample, however, I am not restricting the coefficients on the controls (and the fixed effect) to be the same in the two samples.

TABLE 3

THE EFFECT OF LOCAL NEWSPAPERS

	Investment expenditures		Loans revenues	
	(1) Local sales > median	(2) Local sales < median	(3) Local sales > median	(4) Local sales < median
3 years before election	116.7***	62.6***	32.9***	18.5***
	(14.03)	(13.69)	(5.15)	(4.36)
2 years before election	110.8***	99.8***	34.5***	26.3***
	(12.68)	(13.07)	(4.77)	(4.13)
1 year before election	145.5***	135.3***	40.7***	36.9***
	(16.22)	(17.29)	(5.64)	(5.03)
1 year after election	58.8***	46.1***	21.4***	10.5***
	(13.25)	(13.73)	(5.25)	(4.02)
3 years before elect.*Post	-102.6***	7.14	-18.8***	-8.58
	(21.31)	(21.51)	(7.07)	(6.79)
2 years before elect.*Post	-88.1***	-55.8***	-21.4***	-8.39
	(22.50)	(20.37)	(6.67)	(6.66)
1 year before elect.*Post	-95.0***	-26.7	-23.4***	-4.36
	(23.29)	(23.34)	(7.80)	(7.25)
1 year after elect.*Post	-61.3***	6.74	-12.8*	-5.65
	(22.85)	(21.18)	(6.69)	(6.11)
Mean of dep. var.	453.6	522.7	105.2	135.2
Controls	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	Y
Municipality Effects	Y	Y	Y	Y
R^2	0.42	0.40	0.29	0.36
Obs.	43321	42058	43483	42665

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in the first two columns and revenues from new loans in the last two. The sample is split in two parts: in the first and third column results are for provinces where local newspapers sales per capita in given year are above the national median, whereas results for provinces below the median are reported in the second and fourth column. Controls, year, year-region and municipality dummies are included in all specifications. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Given that newspaper readership can be correlated with several unobservables that also affect spending, one needs to take these results with caution. One example arises if, as it is indeed the case, newspaper coverage is higher in the north than in the south, or if municipalities differ in the level of social capital, political participation or education of their voters. To control for these possibilities, region-time interactions, per capita municipal expenditures in education, and the years of education of the mayor are, as usual, included in all specifications. The inclusion of voters' turnout as a control in all specifications, instead, helps controlling for another confounding factor, namely for the possibility that more responsive and active voters may generate a reaction in mayors' spending decisions.

6.4 The budget cycle and the probability of re-election

The presence of a strong budget cycle suggests that mayors put considerable effort in choosing both the timing and the scale of investment projects, possibly as an attempt to improve the probability of being re-elected. Obtaining evidence on the causal effect of spending on the probability of re-election is problematic in absence of an instrument because of the presence of many confounding factors that are correlated with spending but unobservable to the econometrician. It is possible, however, to investigate if there is at least a positive correlation between different types of expenditures and re-election chances. To this end, I concentrate only on terms in which the incumbent ran for re-election and estimate a Probit model for the probability of being re-elected (conditional on running for re-election) on a series of spending variables measured in the last year of the term. The probability of being re-elected conditional on running again is quite high: in the 6,466 terms of this sample, the incumbent is re-elected 76% of the times.[18] To control for possible size effects and municipality specific characteristics, I include a cubic polynomial in population, surface, density, altitude, and indicator for province capitals, and an indicator for early termination of the term.[19] I also control for the vote shares of both the incumbent and the runner-up, as well as the vote shares of both candidates in the runoff election (if there is one, otherwise both variables are set to zero). Finally, I include total expenditures, calculated as the sum of all expenditures over all years of the term, as a measure of the aggregate size of investment projects over the whole term.

To measure pre-electoral spending, I include in estimation current, investment, loans and services expenditures in the year preceding elections. In table 4 I report both the Probit coefficients and the elasticities (evaluated at the sample means) with and without controls, region and election year effects. The incumbent advantage is evident by looking at the estimated elasticity of the previous election vote share of roughly 0.3%. For what regards spending measured in pre-election year, we notice that neither current expenditures nor loans or services appear to be correlated with re-election. Investment expenditures instead have a positive elasticity of around 0.02, indicating that doubling expenditures in pre-election years is associated with a 2% higher probability of re-election. This effect is quite strong, especially considering that investment expenditure figures vary significantly from one year to the other, and a single large project may raise the per-capita investment expenditures for a municipality by a sizeable amount.

Interacting the pre-electoral spending variables with an indicator for years after the reform yields a negative overall estimate of the baseline reform indicator and also negative coefficients for investment expenditures and loans. However, although the signs seem to suggest that voters penalize in the polls additional spending after the reform, none of these coefficients is statistically significant. Overall, the results in this section provide suggestive evidence that investment spending helps re-election chances. Because of the difficulty to control for all possible determinants of re-elections that are correlated with spending, however,

---

[18]Clearly, the re-election variable is missing for the last term of all municipalities.
[19]Results are robust to excluding terms that did not end regularly altogether.

TABLE 4

| | Dependent variable: 1 if incumbent was re-elected | | | | | |
| | (1) | | (2) | | (3) | |
	β / SE	Elasticity	β / SE	Elasticity	β / SE	Elasticity
Incumbent vote share	1.460***	0.308***	1.350***	0.282***	1.361***	0.284***
	(0.171)		(0.172)		(0.172)	
Post	-0.120	-0.024	-0.152*	-0.031*	-0.070	-0.014
	(0.093)		(0.091)		(0.145)	
Exp. in pre-election year						
Current	0.000	0.013	-0.000	-0.021	-0.000	-0.026
	(0.000)		(0.000)		(0.000)	
Investment	0.000**	0.022**	0.000***	0.023***	0.000***	0.022***
	(0.000)		(0.000)		(0.000)	
Loans	-0.000	-0.002	-0.000	-0.002	-0.000	-0.001
	(0.000)		(0.000)		(0.000)	
Services	-0.000*	-0.008*	-0.000	-0.007	-0.000	-0.007
	(0.000)		(0.000)		(0.000)	
Exp. in pre-election year (Post)						
Current*Post	0.000	0.004	0.000	0.010	0.000	0.012
	(0.000)		(0.000)		(0.000)	
Investment*Post	-0.000	-0.006	-0.000	-0.006	-0.000	-0.005
	(0.000)		(0.000)		(0.000)	
Loans*Post	-0.000	-0.002	-0.000	-0.002	-0.000	-0.003
	(0.000)		(0.000)		(0.000)	
Services*Post	0.000	0.004	0.000	0.004	0.000	0.004
	(0.000)		(0.000)		(0.000)	
Expenditures over term						
Total exp. in the term	-0.000	-0.006	-0.000	-0.009	-0.000	-0.009
	(0.000)		(0.000)		(0.000)	
Total exp. in the term*Post	-0.000	-0.004	-0.000	-0.003	-0.000	-0.004
	(0.000)		(0.000)		(0.000)	
Mean of dep. var	0.76		0.76		0.76	
Controls	Y		Y		Y	
Region Effects	N		Y		Y	
Electoral year Effects	N		N		Y	
Pseudo-R^2	0.08		0.09		0.09	
Obs.	6466		6466		6466	

Notes: The dependent variable is one if the incumbent ran again for mayor and was re-elected, and zero if the incumbent ran but lost to the challenger. Each of the three specifications is a probit with different configurations of controls, region and election year effects. Both the marginal effects and the elasticity (calculated at the sample mean of all variables) are reported. Current, investment, loans and services expenditures are in hundreds of 2005 Euros per capita and are measured in the year before elections. *Post* is a dummy for post-reform years. Total expenditures in the term are obtained as the sum of total expenditures over the term. Standard errors are clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

caution is needed in giving these coefficients a causal interpretation.

6.5   Robustness analysis

In this section I consider several possible "threats to identification" (Meyer, 1995) that would bias the baseline estimates. First, spending trends may evolve differently over time in the five groups. Second, mayors may anticipate the effect of the reform and resign in advance, self-selecting into some of the groups. Finally, there might be some other factor that, at the same time as the reform, affects spending in each year of the term differently. In the following I discuss each threat to identification in turn.

Heterogeneous trends in spending

Even after controlling for observables, time, and municipality effects, it is possible that there are still other factors that cause spending to evolve differently in the five groups. For instance, the differences in the level of population, density and the some mayor traits reported in table 10 might be the result of group-specific trends related to those characteristics that also affect spending.[20]

In order to rule out this concern, I first include in estimation characteristics of municipalities measured in a baseline year (2007) interacted first with a time trend and then with a time dummy (Duflo 2001, Bhuller et al. 2013). This procedure helps ruling out the possibility that differences in spending after the reform are due to municipality-specific trends related to some pre-determined characteristics by directly controlling for these trends in estimation. Columns 1 and 2 of table 5 report results for those two models and show that coefficients are very similar to the baseline point estimates.[21]

Next, I estimate municipality-specific trends using only data from the pre-reform period (1999-2007) to estimate ϕ_{1i} and ϕ_{2i} in the following quadratic trend model:

$$y_{it} = \phi_{1i}t + \phi_{2i}t^2 + u_{it},$$

and include the estimated coefficients in the main specification as follows, therefore "projecting" pre-reform trends in the post-reform years:

$$y_{it} = \alpha + \boldsymbol{\beta}'_1\mathbf{d} + \boldsymbol{\beta}'_2\mathbf{d} \cdot Post_t + \boldsymbol{\gamma}'X_{it} + +\theta_1\hat{\phi}_{1i}t + \theta_2\hat{\phi}_{2i}t^2\delta_t + \mu_i + \lambda_r \cdot \delta_t + \epsilon_{it}$$

In this way, I control for municipality-specific trends that were in place before the reform and that may cause spending patterns to be different across groups.[22] Finally, I include a municipality-specific linear trend $\nu_i t$ directly in the baseline specification (eq. 1). This model

---

[20]Notice that differences in the levels – as opposed to trends – among groups would not bias the estimates.

[21]The loss of observations is due to some missing values in the covariates in 2007. Using 2008 as an alternative baseline year does not change the results significantly.

[22]Notice that including municipality-specific trends allows for more heterogeneity than just including group-specific trends.

TABLE 5

ROBUSTNESS I - UNOBSERVABLE MUNICIPAL-SPECIFIC TRENDS

	Baseline char. interactions		Individual trends	
	(1)	(2)	(3)	(4)
	Trend	Dummies	Pre-estimated	Controls
3 years before election	89.9***	78.5***	87.1***	82.8***
	(10.1)	(10.2)	(10.1)	(12.3)
2 years before election	105.5***	93.1***	105.3***	104.0***
	(9.40)	(9.50)	(9.39)	(13.2)
1 year before election	142.8***	140.6***	139.9***	139.3***
	(12.5)	(12.5)	(12.3)	(14.4)
1 year after election	57.5***	55.9***	52.4***	53.8***
	(10.0)	(10.3)	(9.83)	(11.4)
3 years before elect.*Post	-43.7***	-26.0	-36.6**	-22.9
	(15.7)	(16.1)	(15.5)	(21.2)
2 years before elect.*Post	-71.7***	-53.6***	-67.8***	-42.1*
	(15.3)	(15.9)	(15.1)	(24.3)
1 year before elect.*Post	-63.0***	-61.7***	-55.4***	-42.5**
	(17.1)	(17.2)	(16.7)	(20.3)
1 year after elect.*Post	-27.7*	-31.1*	-18.2	-16.5
	(16.4)	(16.5)	(15.7)	(20.2)
Controls	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	N
Municipality Effects	Y	Y	Y	Y
R^2	0.41	0.40	0.44	0.01
Obs.	82524	82524	83489	69034

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in all columns. Controls, year and municipality dummies are included in all specifications. Standard errors are robust to heteroskedasticity and clustered at the municipality level. Columns 1-3 are estimated by within-groups whereas column 4 is estimated by OLS on twice differenced data.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

can be estimated by OLS on data differenced twice. To see this, collect all regressors in a vector $\mathbf{Z}$ so that $y_{it} = \beta' \mathbf{Z}_{it} + \delta_t + \nu_i t + \mu_i + \epsilon_{it}$. Then, by first differencing, remove the fixed effect μ_i and obtain the following model (dropping the region-time effects for simplicity):

$$\Delta y_{it} = \beta' \Delta \mathbf{Z}_{it} + \Delta \delta_t + \nu_i + \Delta \epsilon_{it},$$

where the municipality-specific trend in levels is now a fixed effect in differences. This model can be estimated by within-groups or by OLS after a second differencing. Columns 3 and 4 of table 5 show that the estimated coefficients are similar to the baseline results, although the

effect of the reform appears to be slightly weaker. Notice that the R^2 in column 4 is much smaller because the program used for estimation (Stata 14) gives as output the R^2 of the model in double differences.

A final check is devoted to the possibility that, of the five groups of municipalities, there is one that behaves differently from others and is driving the results. Given that the 1999 group is the largest one and also appears to be the most different one in terms of observable characteristics (see table 10 in the Appendix), a possibility is to exclude all municipalities that held elections in 1999 and estimate the model again. In table 14 in the Appendix I exclude each group of municipalities one at a time to ensure that none of them is driving the results: remarkably, results are stable and are not affected by the removal of any of the groups.

Selection into groups

Another possible concern is that mayors resign before the end of the term to strategically avoid the effect of the reform. Belonging to one group of municipalities or another would then depend on the decision of the mayor, so that groups might not be comparable anymore. I construct an artificial, deterministic election cycle for all municipalities as follows: municipalities holding elections in 1999 are automatically assumed to repeat in 2004 and 2009. I repeat the same procedure for municipalities that voted in 2000 (but did not vote in 1999), assuming they vote again in 2005 and 2010, and similarly for the cycles starting in 2001, 2002 and 2003. Using these theoretical electoral schedules I then construct the equivalent of the year in term indicators in equation 1 and their interactions with the post-reform indicator and use them either as regressors in the main specification or as instruments for $d_{\tau-3}, ..., d_{\tau+1}$. Results for either possibility are presented in table 6 and are quite reassuring: both specifications go in the same direction as the baseline results.[23]

Other confounding factors

The presence of unobserved factors that affect spending in the treatment and control group differently at about the same time as the reform would bias the baseline diff-in-diffs coefficients. A natural concern given the approval date of the reform is that comparing the budget cycle before and after 2008 would simply capture the effect of the financial crisis or of some other reform that also affected the spending decisions of Italian municipalities. Since the crisis presumably raised the cost of financing for municipalities and reduced the amount of resources they could spend, it is reasonable to expect a decrease in investment expenditures with respect to pre-crisis years.[24]

The effect of the crisis - be it to reduce government transfers and tax income or increase

---

[23]Since the instruments are naturally strongly correlated with the endogenous regressors by construction, the instruments are relevant, with F-statistics of over 1,000 for all instruments. First-stage results are reported in the Appendix.

[24]In fact, as figure 2 shows, the decline in investment expenditures indeed took place but it started earlier, in 2005.

Table 6
Robustness II

	Using exogenous elections	
	(1) As regressors	(2) As IV
3 years before election	72.4***	78.3***
	(9.93)	(10.7)
2 years before election	96.0***	103.7***
	(9.77)	(10.0)
1 year before election	127.7***	137.9***
	(12.8)	(13.0)
1 year after election	43.9***	47.4***
	(11.2)	(11.1)
3 years before elect.*Post	-34.6**	-29.8
	(15.6)	(18.3)
2 years before elect.*Post	-64.5***	-62.1***
	(15.3)	(17.7)
1 year before elect.*Post	-69.4***	-62.1***
	(17.5)	(19.8)
1 year after elect.*Post	-15.8	-11.2
	(17.2)	(19.4)
Controls	Y	Y
Year Effects	Y	Y
Year-Region Effects	Y	Y
Municipality Effects	Y	Y
R^2	0.41	0.06
Obs.	85093	85091

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in all columns. Controls, year, year-region, and municipality dummies are included in all specifications. Column 1 uses the panel IV estimator, using as instruments for the years of the term dummies indicators constructed ignoring early terminations, whereas column 2 uses those instruments directly and is estimated by within-groups. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the cost of debt - will be captured by the time dummies only if it affects municipalities in different years of the term in the same way. If, in turn, municipalities in the pre-election year, that are those raising more debt to boost spending, are more affected by the crisis than municipalities in other years of the term, then the latter will not be a suitable control group

anymore.

To control for a possible deterioration in the access to credit, in column 1 of table 7 I add as controls both the yearly amount of payments for past loans and the accumulated stock of debt at the beginning of the year, both in 2005 Euros per capita. The stock of debt is available only since 2003 so the sample size is, in this exercise, reduced. Both variables are negatively correlated with spending although the point estimates are very small, but the estimated effect of the reform remains strongly negative. Alternatively, in column 2 I drop all the years from 2010 to 2012, in order to minimize the impact of post-crisis years in estimation, but again the effect of the reform remains negative although it is estimated with less precision.

Another possible confounding factor is the Domestic Stability Pact (see section 3) that, by requiring stricter accounting rules that limited investment, may also have affected differently the budget cycle in municipalities in different years of the term. Since only larger municipalities, with population above 5,000, are subject to this Pact, in column 3 I show that results are also robust to excluding them from estimation.

Finally, I add a lag of the dependent variable to the baseline model. The specification with one or more lags is commonly used in the budget cycle literature and is generally estimated by GMM using the instruments suggested by Arellano and Bond (1991). Estimation results using the Arellano-Bond estimator are in reported in column 4 and show that point estimates are basically unaffected by the inclusion of a lag of investment expenditures.

7   Conclusions

Politicians are traditionally thought to behave strategically before elections, yet the empirical evidence on this matter is rather mixed. This paper starts by showing that the budget cycle in Italian municipal spending is sizeable, as investment in the year before elections is almost one-third higher on average than in election years. This increase is concentrated in visible categories of spending such as roads, parks and public housing and is financed with new borrowing and sales of public assets. In theory, this behaviour should not yield any electoral advantage to politicians in presence of perfect information because voters can internalize the cost of this additional investment in terms of more debt or higher taxes in the future. If politicians have an informational advantage, however, they can trick voters into believing that a larger provision of public good is due to their superior competence even when it is not.

To quantify how giving voters information affect the strategic manipulation of spending, I use a reform that induced quasi-experimental variation in the availability to voters of a particular source of information, the balance sheet of their municipality. In the years after this reform, when the balance sheet is made public before elections, the magnitude of the budget cycle decreases. In particular, the pre-election year increase is reduced by one-third. To investigate the impact of local media as means of circulating information on the behaviour of mayorsin office, I then test whether the reform had a differential impact in provinces where local newspaper sales are above the national median, and find that, in such areas, the effect

TABLE 7

ROBUSTNESS III

	(1) Controls for debt	(2) Drop > 2009	(3) Stability Pact	(4) Adding a lag
3 years before election	68.4***	86.6***	98.6***	86.7***
	(13.8)	(10.2)	(15.5)	(10.9)
2 years before election	99.7***	103.2***	123.6***	107.8***
	(13.4)	(9.49)	(14.2)	(11.2)
1 year before election	132.3***	137.5***	173.8***	126.7***
	(15.2)	(12.5)	(19.7)	(12.7)
1 year after election	52.7***	51.9***	69.6***	70.4***
	(13.6)	(9.93)	(15.9)	(11.7)
3 years before elect.*Post	-23.9	-18.9	-32.6	-43.3***
	(17.6)	(24.7)	(24.3)	(16.6)
2 years before elect.*Post	-67.9***	-56.8**	-76.6***	-71.1***
	(18.0)	(24.8)	(24.0)	(17.6)
1 year before elect.*Post	-50.8***	-41.8*	-70.6***	-42.7**
	(18.9)	(21.5)	(26.8)	(17.5)
1 year after elect.*Post	-22.5	-49.4**	-29.7	-30.1*
	(18.2)	(22.1)	(25.5)	(17.4)
Loans payments p.c.	-0.077			
	(0.050)			
Accumulated debt p.c.	-0.0039			
	(0.0035)			
Controls	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	Y
Municipality Effects	Y	Y	Y	Y
R^2	0.46	0.43	0.39	
Obs.	60897	67557	59863	75641

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in all columns. Controls, year, region-year and municipality dummies are included in all specifications. In the first column yearly payment for past loans and accumulated debt at the beginning of the year, both in 2005 euros per capita, are included as controls. In the second column observations for years 2010-2012 are dropped. In column 3 the sample is restricted to municipalities with more than 5,000 inhabitants. Column 4 includes a lag of the dependent variable and is estimated with Arellano-Bond's estimator (the R^2 is not available). Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of the reform is almost twice as strong as the baseline estimates. I interpret these results as suggesting that mayors react to more informed voters by reducing spending manipulation and that local newspapers help disseminating information among voters.

References

Aidt, Toke S, Francisco José Veiga, and Linda Gonçalves Veiga. 2011. "Election results and opportunistic policies: A new test of the rational political business cycle model." *Public Choice*, 148(1-2): 21–44.

Akhmedov, A, and Ekaterina Zhuravskaya. 2004. "Opportunistic Political Cycles : Test in a Young Democracy Setting." *The Quarterly Journal of Economics*, 119 (4): 1301–1338.

Alesina, Alberto, and Matteo Paradisi. 2015. "Political Budget Cycles: Evidence from Italian Cities." National Bureau of Economic Research NBER Working Papers 20570.

Alesina, Alberto, and Roberto Perotti. 1995. "The Political Economy of Budget Deficits." *Staff Papers (International Monetary Fund)*, 42(1): 1–31.

Arellano, Manuel, and Stephen Bond. 1991. "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations." *Review of Economic Studies*, 58(2): 277–97.

Banerjee, Abhijit V., Selvan Kumar, Rohini Pande, and Felix Su. 2011. "Do Informed Voters Make Better Choices? Experimental Evidence from Urban India." MIT Working Paper.

Bartolini, David, and Raffaella Santolini. 2009. "Fiscal Rules and the Opportunistic Behaviour of the Incumbent Politician: Evidence from Italian Municipalities." CESifo Group Munich CESifo Working Paper Series 2605.

Besley, Timothy, and Robin Burgess. 2002. "The Political Economy Of Government Responsiveness: Theory And Evidence From India." *The Quarterly Journal of Economics*, 117(4): 1415–1451.

Bhuller, Manudeep, Tarjei Havnes, Edwin Leuven, and Magne Mogstad. 2013. "Broadband Internet: An Information Superhighway to Sex Crime?" *Review of Economic Studies*, 80(4): 1237–1266.

Carozzi, Felipe, and Luca Repetto. 2016. "Sending the pork home: birth town bias in transfers to Italian municipalities." *Journal of Public Economics*, 134: 42–52.

Coviello, Decio, and Stefano Gagliarducci. 2010. "Tenure in Office and Public Procurement." Tor Vergata University CEIS Research Paper 179.

Drago, Francesco, Tommaso Nannicini, and Francesco Sobbrio. 2014. "Meet the Press: How Voters and Politicians Respond to Newspaper Entry and Exit." *American Economic Journal: Applied Economics*, 6(3): 159–88.

Drazen, Allan, and Marcela Eslava. 2010. "Electoral manipulation via voter-friendly spending: Theory and evidence." *Journal of Development Economics*, 92(1): 39–52.

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review*, 91(4): 795–813.

Ferraz, Claudio, and Frederico Finan. 2008. "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes." *The Quarterly Journal of Economics*, 123(2): 703–745.

Gagliarducci, Stefano, and M. Daniele Paserman. 2012. "Gender Interactions within Hierarchies: Evidence from the Political Arena." *Review of Economic Studies*, 79(3): 1021–1052.

Gonzalez, Maria de los Angeles. 2002. "Do changes in democracy affect the political budget cycle? Evidence from Mexico." *Review of Development Economics*, 6(2): 204–224.

Gregori, Wildmer Daniel. 2014. "Fiscal Rules and Public Spending: Evidence from Italian Municipalities." Dipartimento Scienze Economiche, Universita' di Bologna Working Papers wp923.

Meyer, Bruce D. 1995. "Natural and Quasi-experiments in Economics." *Journal of Business & Economic Statistics*, 13(2): 151–61.

Nordhaus, William D. 1975. "The political business cycle." *Review of economic Studies*, 42(2).

Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy*, 115: 200–249.

Persson, Torsten, and Guido Tabellini. 2002. *Political Economics: Explaining Economic Policy.* Vol. 1 of *MIT Press Books*, The MIT Press.

Rogoff, Kenneth. 1990. "Equilibrium Political Budget Cycles." *American Economic Review*, 80(1): 21–36.

Rogoff, Kenneth, and Anne Sibert. 1988. "Elections and macroeconomic policy cycles." *The Review of Economic Studies*, 55(1): 1–16.

Shi, Min, and Jakob Svensson. 2006. "Political budget cycles: Do they differ across countries and why?" *Journal of Public Economics*, 90(8-9): 1367–1389.

Snyder, James M., and David Stromberg. 2010. "Press Coverage and Political Accountability." *Journal of Political Economy*, 118(2): 355–408.

Stromberg, David. 2004. "Radio's Impact on Public Spending." *The Quarterly Journal of Economics, MIT Press*, 119(1): 189–221.

Appendix I - Conceptual framework

Budget cycles with uninformed and informed voters

Model setup

In this simple framework, based on Shi and Svensson (2006), politicians belong to either party a or party b, and derive utility from consumption c_t and from a public good g_t. While in office, they also obtain "ego rents" X in each period. The utility function of candidate $j \in \{a, b\}$ is, therefore,

$$V_t^j = \sum_{s=t}^{T} [g_s + u(c_s) + X],$$

Politicians set optimally the level of taxes τ_t and borrowing d_t at the beginning of each period. The final amount of public good provided, however, also depends on the incumbent's competence level η_t^j in the following way:

$$g_t = \tau_t + d_t - R(d_{t-1}) + \eta_t^j,$$

where $R(d)$ is a continuous cost function of public borrowing with $R(0) = 0$, $R'(0) = 1$ and $R''(d) > 0$ for all positive d. η_t can be interpreted, for instance, as the ability to secure government transfers. More competent politicians are able to provide more units of public good because they obtain more transfers than less competent ones. Assume that ability follows a zero mean MA(1) process with finite variance, that is,

$$\eta_t = \mu_t + \mu_{t-1},$$

where each μ_t is *iid* with density $f(u)$ and cdf $F(u)$. Also, assume that past competence is known by all agents.[25]

The economy is populated by many voters, each deriving utility from consumption c_t and a public good g_t. Voter i's utility function in period t is:

$$U_t^i = \sum_{s=t}^{T} [g_s + u(c_s) + \theta_i z_s + \epsilon_i - \gamma \epsilon_i^2],$$

where z_t takes value -1/2 if a is in power and 1/2 if b is. θ_i makes voters heterogeneous and reflects, for instance, ideological or personal preferences for candidates. Voters with a negative realization of θ_i, which is assumed to be uniformly distributed over $[-1/2, 1/2]$, have a positive contribution to their utility from electing a, whereas voters with a positive realization prefer b.

Voters also differ in their preference for being informed about the choices of the politician

---

[25]Serial correlation is needed so that voters are able to know something useful about the incumbent's competence in the year after the election by observing her behaviour before.

in office. Informed voters obtain non-negative utility ϵ_i with c.d.f. G and $G(0) = 0$, but incur a convex information cost $\gamma \epsilon_i^2$, where γ is the "price" of information, assumed to be greater than one. Non-informed voters only observe the level of public good g_t and of taxes τ_t, whereas informed ones also observe d_t and can residually determine the ability η_t.

Voters will incur the cost of information only if $\epsilon_i - \gamma \epsilon_i^2 \geq 0$, that is, only if $0 \leq \epsilon_i \leq \frac{1}{\gamma}$. The fraction of informed voters is, therefore, decreasing in the price of information γ and is defined as follows:

$$\pi \equiv Pr\left(0 \leq \epsilon_i \leq \frac{1}{\gamma}\right) = G\left(\frac{1}{\gamma}\right).$$

Timing of events

At the beginning of period t, the incumbent sets τ_t and d_t without observing her competence shock μ_t^j. Then, μ_t^j occurs and the amount of public good g_t is determined. Taxes τ_t and aggregate spending g_t are always observed by all voters before the election and. Additionally, a fraction π of voters also observes d_t and, therefore, can infer the competence level. At the end of period t, elections take place. Voters re-elect the incumbent if the expected utility they derive from doing so is higher than electing the challenger.

In $t + 1$, the timing is the same as in t except for the fact that no elections take place. New elections are called at the end of period $t + 2$, in which everything is the same as in t.

Equilibrium

Under the simplifying assumptions that competence is MA(1) and preferences are quasi-linear, the problem can be broken down into a sequence of two-period maximisations. Consider the post-election period $t + 1$. Given the competence process, the incumbent has no incentive to manipulate spending, since the expected competence in period $t + 3$, which is what determines election outcomes in $t + 2$, is independent of the competence shock η_{t+1}, that is, $E_{t+1}[\eta_{t+3}|\eta_{t+1}] = E[\eta_{t+3}] = 0$. Also, given that borrowing is costly and the marginal utility of g_t is constant, there is no borrowing in $t + 1$, and the government will instead run a surplus to pay back the debt accumulated in the election period:

$$g_{t+1} = \tau_{t+1} - R(d_t) + \eta_{t+1}.$$

I will show later that the level of taxes is the same regardless of who wins and in both election and non-election years and always equals $\tau_{t+1} = \tau^* = y - u_c^{-1}(1)$. Assume there is no borrowing in $t-1$ so that in t there is no debt to be repaid and the budget constraint is simply

$$g_t = \tau^* + d_t + \eta_t.$$

Define the optimal borrowing (yet to be determined) as d_t^*. Assume without loss of generality that the incumbent is from party a. Voters, since they have no way to infer the competence level of the challenger, expect the following level of taxes and public good if they elect the

challenger:

$$\tau^b = \tau^*$$

$$E_t[g^b_{t+1}] = \tau^* - E_t R(d^*_t),$$

since, for the challenger, $E_t(\eta^b_{t+1}) = 0$ whereas, if the incumbent is re-elected,

$$\tau^a = \tau^*$$

$$E_t[g^a_{t+1}] = \tau^* - E_t R(d^*_t) + E_t(\mu^a_t).$$

Voter i chooses the incumbent if $E_t U^a_{t+1} > E_t U^b_{t+1}$, that is, if

$$E_t(g^a_{t+1}) + Eu(c_{t+1}) - \frac{1}{2}\theta_i \geq E_t(g^b_{t+1}) + Eu(c_{t+1}) + \frac{1}{2}\theta_i.$$

Substituting and simplifying one obtains that voter i chooses a if

$$E_t[\mu^a_t] - \theta_i \geq 0.$$

The incumbent's expected share of voters is, therefore,

$$Pr(E_t[\mu^a_t] - \theta_i \geq 0) = E_t[\mu^a_t] + \frac{1}{2}.$$

A fraction π of voters observe, besides spending g_t and taxes τ^*, also borrowing d_t before elections. They can therefore determine competence as

$$\mu^a_t = g_t - \tau^* - d_t - \mu^a_{t-1},$$

while the remaining $1 - \pi$ need to make an estimate $\hat{d}_t$, based on the observable level of taxes and public good and knowing the equilibrium strategy of the incumbent, so that

$$\hat{\mu}^a_t = g_t - \tau^* - \hat{d}_t - \mu^a_{t-1} = \mu^a_t + (d_t - \hat{d}_t).$$

The probability that the incumbent stays in power (as perceived by the incumbent) is then equal to the probability that she obtains more than half the votes:

$$P_t = Pr\left[\pi\left(\mu^a_t + \frac{1}{2}\right) + (1 - \pi)\left[\mu^a_t + d_t - \hat{d}_t + \frac{1}{2}\right] \geq \frac{1}{2}\right]$$

$$= Pr\left[\mu^a_t \geq (1 - \pi)(\hat{d}_t - d_t)\right]$$

$$= 1 - F[(1 - \pi)(\hat{d}_t - d_t)]. \tag{2}$$

The probability of being re-elected, thus, depends on the level of borrowing and on the probability of disclosure of the balance sheet. At the beginning of period t, the incumbent

sets τ_t and d_t to maximise her total expected utility over the next two periods. Write the maximisation problem as follows:

$$\max_{d_t, \tau_t} \quad E_t[\tau_t + d_t + \eta_t^a + u(y - \tau_t) + X]$$
$$+ E_t \left[1 - F[(1 - \pi)(\hat{d}_t - d_t)] \right] \left[\tau_{t+1} - R(d_t) + \eta_{t+1}^a + u(y - \tau_{t+1}) + X \right]$$
$$+ E_t \left[F[(1 - \pi)(\hat{d}_t - d_t)] \right] \left[\tau_{t+1} - R(d_t) + \eta_{t+1}^b + u(y - \tau_{t+1}) \right].$$

The first order condition with respect to τ_t yields

$$E_t[1 - u_c(y - \tau_t^*)] = 0,$$

which implies that $\tau_t^* = \tau^* = y - u_c^{-1}(1)$ is constant for each t.[26] Differentiating with respect to d_t and equalling the result to zero gives the other first order condition, which implicitly defines the equilibrium level of new borrowing d_t^*:

$$1 + (1 - \pi)F'[(1 - \pi)(\hat{d}_t - d_t)]X = R'(d_t^*),$$

which equals the marginal utility of the public good in t, equal to one, plus the enhanced probability of re-election times the value of being re-elected (the second term) with the marginal cost of borrowing, equal to $R'(d_t)$. In equilibrium, the incumbent's choice must be consistent with what voters expect, so $d_t^* = \hat{d}_t$.

Assuming, for simplicity, that μ_t^j is uniform over $[-1/2, 1/2]$, so that $F'(u) = 1$ for $u \in [-1/2, 1/2]$ and zero otherwise, the first order condition becomes

$$1 + (1 - \pi)X = R'(d_t^*),$$

which yields $d_t^* = R'^{-1}(1 + (1 - \pi)X) > 0$. Combining this result with the probability of re-election (eq. 2) and the definition of $\pi = \pi(\gamma)$, one obtains the following result.

Proposition I The equilibrium level of borrowing in election years is increasing in the price of information that is,

$$\frac{\partial d_t^*}{\partial \gamma} > 0.$$

Intuitively, the larger the price voter have to pay for information, the larger the fraction of uninformed voters in the population will be. Thus, the expected gain of boosting spending increases and so does pre-electoral equilibrium borrowing. Since $g_t^* = \tau^* + d_t^* + \eta_t$, the optimal level of public spending in election years is also increasing in γ.

Interpreting the 2008 reform as a decrease in the price of information γ, theory then predicts that, after the reform, fluctuations in both borrowing and public good spending should decrease.

---

[26] In off-election periods, the tax choice is the same so that also $\tau_{t+1}^* = \tau^*$.

Appendix II - Data Documentation

Data sources

Data on geographical characteristics and population are taken from the Italian Statistical Office (ISTAT, http://demo.istat.it/ and http://ottomilacensus.istat.it/download-dati/) and the Italian Agency for Energy (ENEA). The names and numerical identifiers of municipalities are from the Ministry of Internal Affairs' *Rilevazione del corpo elettorale*, for all years 1992-2013 (June) http://amministratori.interno.it/semestrale/html/archivio.htm.

Data on expenditures and revenues are contained in the balance sheets and available in html format on the Ministry's web page, so a Python script was used to obtain them (http://finanzalocale.interno.it/apps/floc.php/in/cod/4).

Election results are publicly accessible from the web page of the Italian Ministry of Internal Affairs (http://elezionistorico.interno.it/). In the paper, however, I make use of a more detailed dataset, which also includes some information on the mayoral candidates I obtained after getting in touch with the Ministry's offices.

Elected politicians characteristics for each year after 1985 are readily downloadable from the Census of Local and Regional Administrators (http://amministratori.interno.it/). Finally, data on local newspaper diffusion are taken from ADS (*Accertamenti Diffusione Stampa*, *Dati Territoriali Dichiarati e Certificati* http://www.adsnotizie.it).

Outliers

The balance sheet data present some large outliers, that are the result of miscoding in original the balance sheet (for example, by entering an additional zero by mistake), or that arise from an exceptional year. For example, areas affected by the 2009 earthquake received a substantial relief fund in the following years. To avoid the possibility that abnormally large observations affect the estimates, I follow a simple procedure to identify outliers and re-code the corresponding spending or revenue variable as missing. Specifically, for each variable, I look for municipalities for which the variable is 100 times larger that the sample median (calculated using all observations) in at least one year of the sample and I replace all observations for that variable in those municipalities with a missing. Then, I calculate the standard deviation of the variable of interest (using the remaining municipalities) and code as missing observations those exceeding 10 times the standard deviation. This procedure is carried out for all revenues variable and for the main dependent variable of interest, investment expenditures, after they have been transformed in 2005 Euros per-capita. 5.9% of the sample is affected, in one or more of these variables, by this outlier detection procedure. The procedure is not carried out for disaggregated investment expenditures because those variables vary substantially from zero to very large numbers, even for the same municipality over the years so, in this case, is less clear how to identify outliers.

Main variables definitions

Variable	Definition or description
Electoral cycle variables	
$d_{\tau-3}, ..., d_{\tau+1}$	Indicators for years until next election ($d_{\tau-3}$, $d_{\tau-2}$ and $d_{\tau-1}$) or since last election ($d_{\tau+1}$).
Controls	
Population	Resident inhabitants.
Male	Indicator equal to one if mayor in office is a man.
Age	Age of elected mayor
Years of schooling of mayor	Years of schooling of elected mayor, constructed by imputing years from education categories as follows: primary education is considered equivalent to 5 years of schooling, junior high school is equivalent to 8 years, high school to 13 and college or more to 18 years.
Education expenditures p.c.	Yearly, per capita, current expenditures in education.
Surface	Surface of the municipality, in km^2
Province capital	Indicator for the municipality being a province capital.
Altitude	Average altitude of the municipality, in meters over sea level.
Density	Population density of the municipality, in inhabitants per km^2.
Political Controls	
Term not ended regularly	Indicator for the term having ended either with an early government termination, reintegration of the mayor, term suspension or with the dissolution of the municipality.
Term limited	Indicator for the mayor being term limited this term.
Incumbent share	Vote share of the winning candidate in the first round of the previous election.
Commissioner in office	Indicator for the presence of a commissioner in power.
Turnout	Number of voters in the previous municipal election/Number of eligible voters.
Other variables	
Local newspaper sales	Per-capita sales of local newspapers, measured at the province level. Local newspapers are, among the 63 most sold newspapers in Italy, those that are sold in less than 10 provinces. *La Stampa* is considered local because it is bundled with local editions.

Appendix III - Additional figures and results

FIGURE 6

EXPECTED (BUDGET) AND REALIZED (BALANCE SHEET) PER CAPITA EXPENDITURES
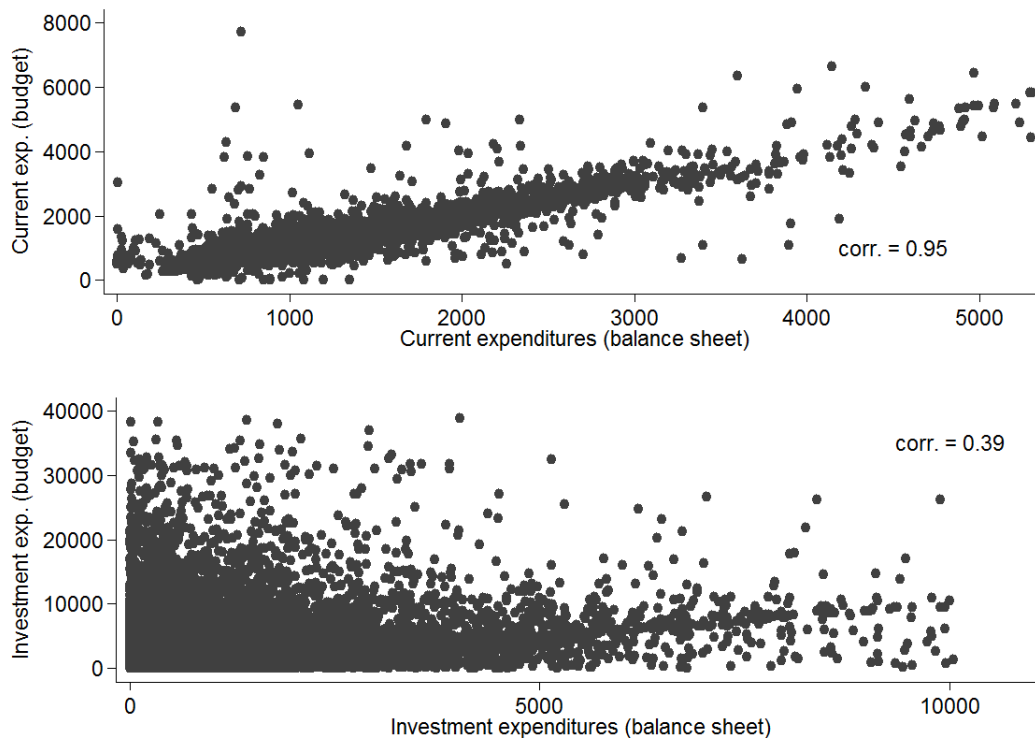
Notes: The upper panel plots in the x-axis current expenditures as recorded in the balance sheet, and in the y-axis expected current expenditures from the budget for all municipalities and for each year. The lower panel reports the same comparison for investment expenditures. All quantities are in 2005 Euros per capita.

FIGURE 7

LOCAL NEWSPAPERS SALES, COPIES PER ONE HUNDRED INHABITANTS
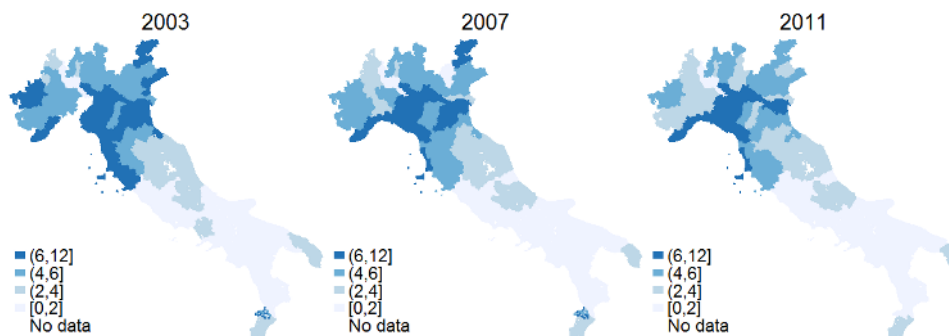
Notes: Darker provinces are those where sales per one hundred inhabitants are higher. A local newspaper is defined as a newspaper that is distributed in 10 provinces or less. Figures are for 2003, 2007 and 2011. *Source: Accertamento Diffusione Stampa.*

TABLE 8

	1999-2007	2008-2012	1999-2012
Balance sheet expenditures			
Current expenditures	747.0	762.2	752.4
	(425.6)	(513.4)	(458.9)
Investment expenditures	625.2	450.0	562.7
	(1374.3)	(1100.4)	(1286.1)
Loans expenditures	77.3	100.4	85.5
	(150.5)	(188.4)	(165.4)
Services expenditures	134.3	109.3	125.4
	(660.5)	(189.8)	(541.9)
Balance sheet revenues			
Non-tax revenues	189.8	199.1	193.1
	(313.3)	(380.4)	(338.8)
Disposal of assets	478.2	359.4	435.8
	(1289.5)	(1006.4)	(1197.6)
Loans revenues	143.3	111.9	132.1
	(304.4)	(289.6)	(299.6)
Services revenues	134.3	109.3	125.4
	(660.6)	(190.2)	(542.0)
Tax revenues	354.8	406.1	373.1
	(258.5)	(263.7)	(261.5)
Transfers revenues	267.6	223.7	251.9
	(245.7)	(308.8)	(270.7)
Geographic characteristics			
Population	7280.9	7600.0	7394.8
	(42178.4)	(43631.9)	(42703.2)
Surface (km2)	33.9	33.9	33.9
	(47.6)	(47.6)	(47.6)
Altitude (mt.)	339.1	339.1	339.1
	(277.8)	(277.8)	(277.8)
Pop. density (inh./km2)	307.4	324.7	313.6
	(665.1)	(676.7)	(669.3)
Characteristics of the mayor			
Number of terms in our sample	1.60	1.57	1.59
	(0.73)	(0.76)	(0.74)
Term limited	0.40	0.33	0.37
	(0.49)	(0.47)	(0.48)
Age of mayor	49.89	51.78	50.56
	(9.61)	(10.01)	(9.80)
Male	0.91	0.89	0.90
	(0.28)	(0.32)	(0.30)
Years of schooling of mayor	14.33	14.58	14.42
	(3.58)	(3.44)	(3.53)
Observations	60314	33506	93820

Notes: Averages taken over the periods specified in the column headings (standard deviations in parentheses). Balance sheet quantities are in per capita 2005 euros. Number of terms in our sample is the average experience, in terms, of mayors within our sample period. Term limited is a dummy equal to one if the mayor is term-limited.

TABLE 9

DESCRIPTIVE STATISTICS FOR MUNICIPALITIES - BUDGET QUANTITIES

	1999-2007	2008-2012	1999-2012
Budget expenditures			
Current expenditures	772.0	786.1	777.0
	(1128.6)	(580.4)	(969.0)
Investment expenditures	1309.4	1267.6	1294.5
	(4045.6)	(3806.7)	(3961.8)
Loans expenditures	137.9	174.4	150.9
	(323.0)	(239.5)	(296.4)
Services expenditures	225.4	215.7	221.9
	(1023.0)	(290.0)	(838.1)
Budget revenues			
Non-tax revenues	194.7	204.7	198.3
	(372.6)	(364.9)	(369.9)
Disposal of assets	1055.5	1091.4	1068.4
	(3634.6)	(3480.2)	(3580.2)
Loans revenues	329.1	277.5	310.5
	(835.6)	(590.6)	(756.6)
Services revenues	225.5	215.7	222.0
	(1024.5)	(290.0)	(839.3)
Tax revenues	351.7	376.4	360.5
	(617.3)	(242.8)	(515.8)
Transfers revenues	277.5	263.7	272.6
	(759.0)	(1114.2)	(902.2)
Observations	59196	32934	92130

Notes: Averages taken over the periods specified in the column headings (standard deviations in parentheses). Budget quantities are in per capita 2005 Euros.

TABLE 10

TABLE 10

DESCRIPTIVE STATISTICS FOR MUNICIPALITIES GROUPED BY YEAR OF FIRST ELECTION

	1999	2000	2001	2002	2003
Balance sheet expenditures					
Current expenditures	754.6	734.8	759.3	752.2	703.1
	(1.6)	(5.9)	(3.2)	(9.1)	(5.4)
Investment expenditures	569.0	537.2	576.9	522.7	516.8
	(5.3)	(16.4)	(9.8)	(13.1)	(23.8)
Loans expenditures	82.0	86.8	93.7	97.5	82.6
	(0.7)	(2.5)	(1.3)	(2.0)	(3.2)
Services expenditures	122.3	115.4	124.4	157.0	115.8
	(1.5)	(1.9)	(1.8)	(15.4)	(3.2)
Balance sheet revenues					
Non-tax revenues	196.5	165.0	190.9	197.0	162.4
	(1.0)	(3.1)	(1.9)	(8.8)	(3.1)
Disposal of assets	441.1	422.2	448.3	398.8	391.9
	(5.0)	(15.7)	(8.8)	(12.2)	(21.0)
Loans revenues	125.1	135.0	148.5	150.3	141.3
	(1.2)	(3.9)	(2.5)	(3.3)	(7.0)
Services revenues	122.4	115.3	124.5	157.0	115.7
	(1.5)	(1.9)	(1.8)	(15.4)	(3.2)
Tax revenues	377.6	359.1	367.5	358.5	372.3
	(0.9)	(4.0)	(2.0)	(4.7)	(5.1)
Transfers revenues	248.1	267.6	262.7	260.9	231.3
	(1.1)	(3.5)	(1.9)	(2.9)	(3.3)
Geographic characteristics					
Population	4835.0	12336.8	12530.0	13224.5	11520.2
	(54.8)	(367.7)	(809.8)	(345.3)	(395.1)
Surface (km2)	29.9	47.7	38.5	47.9	34.4
	(0.1)	(1.1)	(0.6)	(0.6)	(0.6)
Altitude (mt.)	343.9	314.7	350.1	320.5	279.5
	(1.1)	(4.4)	(2.4)	(3.1)	(4.8)
Pop. density (inh./km2)	249.5	525.2	350.3	525.5	543.5
	(1.9)	(17.9)	(5.8)	(11.5)	(18.0)
Characteristics of the mayor					
Number of terms in our sample	1.6	1.5	1.5	1.5	1.5
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Term limited	0.4	0.3	0.3	0.3	0.4
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Age of mayor	50.6	49.9	50.5	50.6	49.4
	(0.04)	(0.16)	(0.08)	(0.10)	(0.17)
Male	0.9	0.9	0.9	0.9	0.9
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Years of schooling of mayor	14.1	15.3	14.8	15.2	15.2
	(0.01)	(0.05)	(0.03)	(0.04)	(0.06)
Observations	62903	4172	14867	8846	2951

Notes: Municipalities are grouped according to the year of the first election in the sample and group averages are reported (standard errors in parentheses). Balance sheet quantities are in per capita 2005 euros. Number of terms in our sample is the average experience, in terms, of mayors within our sample period. Term limited is a dummy equal to one if the mayor is term-limited.

<center>

TABLE 11

<small>BASELINE RESULTS - FULL TABLE</small>

</center>

	Baseline specification		W/o controls	W/o year effects
	(1) Invest. exp.	(2) Invest. exp.	(3) Invest. exp.	(4) Invest. exp.
3 years before election	88.9***	85.6***	81.6***	107.9***
	(9.50)	(9.91)	(9.67)	(7.14)
2 years before election	105.1***	104.7***	103.4***	108.2***
	(9.02)	(9.25)	(8.89)	(6.94)
1 year before election	141.8***	138.8***	122.1***	211.4***
	(11.80)	(12.20)	(10.96)	(9.22)
1 year after election	50.8***	52.0***	51.4***	54.2***
	(9.34)	(9.79)	(9.40)	(6.43)
3 years before elect.*Post	-40.4***	-40.1***	-33.0**	-174.8***
	(15.12)	(15.30)	(14.98)	(9.76)
2 years before elect.*Post	-63.9***	-70.8***	-68.2***	-196.6***
	(14.62)	(15.01)	(14.36)	(9.26)
1 year before elect.*Post	-54.9***	-55.1***	-46.4***	-193.1***
	(16.19)	(16.72)	(15.21)	(11.11)
1 year after elect.*Post	-16.2	-21.7	-19.9	-109.1***
	(15.00)	(15.65)	(15.03)	(8.98)
Male	-7.47	5.03		15.5
	(11.94)	(11.31)		(12.43)
Age of mayor	-0.40	-0.45		-0.54
	(0.38)	(0.38)		(0.40)
Years of schooling of mayor	-4.60***	0.75		-0.60
	(1.16)	(1.14)		(1.19)
Education expend. p.c.	0.90***	0.51**		0.43***
	(0.17)	(0.22)		(0.15)
Population	-0.0025***	-0.0014		-0.0025***
	(0.00)	(0.00)		(0.00)
Population squared	5.7e-09***	-5.5e-09		5.7e-09***
	(0.00)	(0.00)		(0.00)
Population cube	-1.8e-15***	1.5e-15*		-1.8e-15***
	(0.00)	(0.00)		(0.00)
Surface (km2)	-0.59***			-0.30**
	(0.14)			(0.13)
Pop. density (inh./km2)	-0.075***	-0.045		-0.046***
	(0.01)	(0.05)		(0.01)
Altitude (mt.)	0.69***			0.69***
	(0.03)			(0.03)
Province capital	242.4***			192.3***
	(45.99)			(43.92)
Term not ended regularly	-18.2*	-8.88		16.5
	(10.96)	(10.62)		(11.25)
Term limited mayor	-19.5***	6.68		-18.1***
	(6.21)	(5.93)		(6.23)
Incumbent share (%)	320.8***	15.2		343.6***
	(30.04)	(28.06)		(30.50)
Commissioner in office	5.11	-46.8		-95.1
	(61.55)	(52.98)		(61.62)
Turnout (voters/eligible)	-366.7***	-35.9		-259.0***
	(83.46)	(101.94)		(77.68)
Mean of dep. var.	487.6	487.6	485.0	487.6
Controls	Y	Y	N	Y
Year Effects	Y	Y	Y	N
Year-Region Effects	N	Y	Y	N
Municipality Effects	0.16	0.41	0.40	0.12
R^2	85385	85385	90279	85385

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. *Post* is an indicator for years from 2008 onwards. All columns but the last include year dummies. Standard errors are robust to heteroskedasticity and clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 12

BASELINE RESULTS FOR INVESTMENT EXPENDITURES, BY CATEGORY

	(1) Educ.	(2) Police	(3) Admin.	(4) Roads	(5) Devel.	(6) Tourism	(7) Social	(8) Sport	(9) Services	(10) Culture	(11) Justice	(12) Territory
3 years bf. elect.	7.93***	0.018	11.0***	20.7***	2.04	8.10	5.82***	4.86*	4.55	6.45***	0.29	27.2**
	(1.31)	(0.07)	(3.42)	(5.66)	(2.48)	(6.73)	(1.58)	(2.83)	(2.78)	(2.14)	(0.36)	(13.22)
2 years bf. elect.	9.65***	0.097	16.9***	37.7***	1.31	8.45	6.87***	6.33	4.26	6.96**	-0.13	40.7***
	(1.33)	(0.08)	(4.19)	(5.63)	(2.31)	(9.69)	(1.62)	(4.02)	(2.90)	(3.02)	(0.24)	(10.57)
1 year bf. elect.	7.92***	0.076	12.4***	50.4***	7.35**	0.58	9.38***	8.61**	3.27	6.22***	-0.054	55.9***
	(1.71)	(0.08)	(4.22)	(7.86)	(3.48)	(4.43)	(2.02)	(4.05)	(2.53)	(2.29)	(0.37)	(12.91)
1 year aft. elect.	6.75***	0.098	2.24	11.3**	3.76	0.068	3.25**	1.69	2.25	4.55**	0.093	33.1**
	(1.26)	(0.08)	(3.42)	(4.76)	(2.65)	(5.37)	(1.53)	(3.29)	(2.73)	(2.13)	(0.30)	(13.22)
3 years bf. elect.*Post	0.15	0.098	-5.16	-0.16	-11.1**	-9.46	-1.52	-0.65	-8.66	-3.87	-0.43	-2.44
	(2.69)	(0.12)	(5.07)	(8.60)	(4.76)	(7.11)	(2.25)	(3.28)	(6.37)	(2.44)	(0.39)	(20.42)
2 years bf. elect.*Post	-3.04	0.026	-14.8***	-15.2	-10.4**	-9.48	-3.74*	1.73	0.17	-2.72	0.30	-36.1**
	(2.41)	(0.11)	(5.24)	(9.52)	(4.04)	(9.65)	(2.22)	(8.42)	(2.98)	(3.39)	(0.29)	(15.46)
1 year bf. elect.*Post	1.06	0.017	-4.49	-9.33	-12.1***	-5.96	-1.75	-2.96	14.9	-1.78	-0.072	-18.7
	(3.27)	(0.18)	(5.65)	(10.14)	(4.62)	(5.80)	(3.07)	(4.86)	(11.83)	(2.81)	(0.42)	(17.16)
1 year aft. elect.*Post	-3.16	0.16	6.20	-0.87	-13.0***	2.92	1.12	-2.49	1.88	0.60	0.14	-22.0
	(2.46)	(0.17)	(5.15)	(8.34)	(4.50)	(5.40)	(2.46)	(4.62)	(3.77)	(3.07)	(0.49)	(17.54)
Mean of dep. var.	33.0	0.92	79.4	131.1	15.7	17.5	29.2	28.1	12.2	16.5	0.46	202.0
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.14	0.16	0.30	0.30	0.16	0.18	0.14	0.16	0.19	0.20	0.11	0.29
Obs.	87306	87306	87306	87306	87306	87306	87306	87306	87306	87306	87305	87306

Notes: In each column the dependent variable is a different category of investment expenditures in per capita 2005 Euros. *Post* is an indicator for years from 2008 onwards. Sample sizes slightly differ because of missing values in some of the categories. Controls, year, year-region and municipality dummies are included in all specifications. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 13
ROBUSTNESS - FIRST STAGES OF THE IV ESTIMATION

	(1) 3 years bf. election	(2) 2 years bf. election	(3) 1 year bf. election	(4) 1 years aft. election	(5) 3 years bf. *Post	(6) 2 years bf. *Post	(7) 1 year bf. *Post	(8) 1 years aft. *Post
3 years bf.election (theor.)	0.86***	0.02***	0.00*	0.06***	-0.00***	-0.00***	-0.00***	0.00***
	(178.7)	(6.3)	(2.5)	(15.6)	(-3.5)	(-8.1)	(-14.3)	(5.7)
2 years bf.election (theor.)	-0.02***	0.92***	0.00	0.02***	-0.00***	-0.00***	-0.00***	0.00***
	(-7.4)	(256.3)	(0.2)	(9.0)	(-5.7)	(-9.3)	(-15.0)	(8.3)
1 year bf.election (theor.)	-0.04***	-0.01***	0.94***	0.04***	-0.00***	-0.00***	-0.00***	0.00***
	(-13.1)	(-4.9)	(273.1)	(12.0)	(-9.0)	(-7.9)	(-9.9)	(7.3)
1 years aft.election (theor.)	-0.03***	0.00	-0.00	0.98***	-0.00***	-0.00**	-0.00***	0.00***
	(-10.7)	(0.7)	(-1.7)	(270.5)	(-5.2)	(-3.3)	(-7.7)	(6.9)
3 years bf*Post (theor.)	-0.08***	0.00	-0.01	-0.07***	0.78***	0.02***	0.00	-0.02***
	(-11.4)	(0.8)	(-1.4)	(-13.3)	(102.2)	(4.2)	(0.6)	(-3.4)
2 years bf*Post (theor.)	0.00	-0.14***	0.00	-0.03***	-0.01*	0.79***	0.01*	-0.01*
	(0.7)	(-20.7)	(0.8)	(-6.6)	(-2.2)	(99.6)	(2.2)	(-2.3)
1 year bf*Post (theor.)	0.02***	0.00	-0.15***	-0.05***	-0.01*	-0.00	0.80***	-0.01*
	(3.5)	(0.6)	(-22.5)	(-8.2)	(-2.2)	(-0.4)	(105.2)	(-2.0)
1 year aft.*Post (theor.)	0.03***	0.00	-0.00	-0.19***	0.00	0.01	-0.01	0.79***
	(5.8)	(0.6)	(-1.2)	(-25.1)	(0.2)	(1.1)	(-1.2)	(91.0)
Observations	85091	85091	85091	85091	85091	85091	85091	85091

Notes: First stage results for instrumenting each year in term indicator with the variables constructed using the election dates obtained by assuming 5-year terms with no early terminations. Year, year-region and municipality dummies are included in all specifications. Standard errors are robust to heteroskedasticity and clustered at the municipality level. t-statistics in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 14

Robustness - excluding groups from estimation

	Drop 1999	Drop 2000	Drop 2001	Drop 2002	Drop 2003
3 years before election	81.8***	90.3***	83.9***	85.9***	86.3***
	(15.5)	(10.4)	(13.4)	(10.8)	(10.5)
2 years before election	88.2***	103.7***	111.5***	109.7***	104.7***
	(14.4)	(9.65)	(11.7)	(10.9)	(9.81)
1 year before election	117.0***	136.5***	135.1***	148.8***	145.3***
	(15.6)	(12.5)	(15.6)	(14.7)	(13.2)
1 year after election	51.9***	47.6***	49.6***	62.5***	57.5***
	(13.7)	(10.7)	(11.6)	(11.4)	(10.8)
3 years before elect.*Post	-30.8	-46.1***	-42.7**	-42.0**	-37.9**
	(24.6)	(16.0)	(20.2)	(16.7)	(16.1)
2 years before elect.*Post	-52.8**	-70.7***	-90.1***	-71.1***	-70.1***
	(23.7)	(15.7)	(19.2)	(17.7)	(15.8)
1 year before elect.*Post	-34.7	-59.2***	-43.1**	-63.8***	-57.2***
	(23.7)	(17.4)	(20.6)	(19.4)	(17.9)
1 year after elect.*Post	-48.2**	-19.7	-12.6	-33.0*	-22.0
	(20.3)	(16.9)	(18.4)	(18.2)	(17.1)
Mean of dep. var	485.5	487.8	483.4	490.0	489.8
Controls	Y	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y	Y
Year-Region Effects	Y	Y	Y	Y	Y
Municipality Effects	Y	Y	Y	Y	Y
R^2	0.42	0.41	0.42	0.41	0.41
Obs.	27277	81884	72100	77373	82750

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in all columns. *Post* is an indicator for years from 2008 onwards. Controls, year, region-year and municipality dummies are included in all specifications. Municipalities are divided in five groups according to their year of first election in the sample and, in each column, estimation is run dropping one group from the sample at a time. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

WORKING PAPERS[*]
Editor: Nils Gottfries

2013:15 Helena Svaleryd, Self-employment and the local business cycle. 28 pp.

2013:16 Tobias Lindhe and Jan Södersten, Distortive Effects of Dividend Taxation. 22 pp.

2013:17 Jonas Poulsen, After Apartheid: The Effects of ANC Power. 42 pp.

2013:18 Magnus Gustavsson, Permanent versus Transitory Wage Differentials and the Inequality-Hours Hypothesis. 11 pp.

2013:19 Lovisa Persson, Consumption smoothing in a balanced budget regim. 35 pp.

2013:20 Linuz Aggeborn, Voter Turnout and the Size of Government. 50 pp.

2013:21 Niklas Bengtsson, Stefan Peterson and Fredrik Sävje, Revisiting the Educational Effects of Fetal Iodine Deficiency. 48 pp

2013:22 Michihito Ando, How Much Should We Trust Regression-Kink-Design Estimates? 69 pp.

2013:23 Bertil Holmlund, What do labor market institutions do? 25 pp.

2014:1 Oscar Erixson and Henry Ohlsson, Estate division: Equal sharing as choice, social norm, and legal requirement. 45 pp.

2014:2 Eva Mörk, Anna Sjögren and Helena Svaleryd, Parental unemployment and child health. 35 pp.

2014:3 Pedro Carneiro, Emanuela Galasso and Rita Ginja, Tackling Social Exclusion: Evidence from Chile. 87 pp.

2014:4 Mikael Elinder och Lovisa Persson, Property taxation, bounded rationality and housing prices. 38 pp.

2014:5 Daniel Waldenström, Swedish stock and bond returns, 1856–2012. 51 pp.

2014:6 Mikael Carlsson, Selection Effects in Producer-Price Setting. 34 pp.

2014:7 Ranjula Bali Swain and Fan Yang Wallentin, The impact of microfinance on factors empowering women: Differences in regional and delivery mechanisms in India's SHG programme. 28 pp.

2014:8 Ashim Kumar Kar and Ranjula Bali Swain, Competition, performance and portfolio quality in microfinance markets. 33 pp.

---

[*] A list of papers in this series from earlier years will be sent on request by the department.

2014:9   Shyam Ranganathan, Ranjula Bali Swain and David J.T. Sumpter, A DYNAMICAL SYSTEMS APPROACH TO MODELING HUMAN DEVELOPMENT. 42 pp.

2014:10 Shyam Ranganathan and Ranjula Bali Swain, Analysing Mechanisms for Meeting Global Emissions Target - A Dynamical Systems Approach. 40 pp.

2014:11 Chuan-Zhong Li and Ranjula Bali Swain, Growth, Water Resilience, and Sustainability: A DSGE Model Applied to South Africa. 21pp.

2015:1   Evelina Bonnier, Jonas Poulsen, Thorsten Rogall and Miri Stryjan, Preparing for Genocide: Community Work in Rwanda. 40 pp.

2015:2   Linna Martén, Political Bias in Court? Lay Judges and Asylum Appeals. 38 pp.

2015:3   Daniel Waldenström, The National Wealth of Sweden, 1810–2014. 31 pp.

2015:4   Daniel Waldenström, Wealth-income ratios in a small, late-industrializing, welfare-state economy: Sweden, 1810–2014. 41 pp.

2015:5   Mattias Nordin, Local Television, Citizen Knowledge and Political Accountability: Evidence from the U.S. Senate. 45 pp.

2015:6   Glenn Mickelsson, Estimation of DSGE models: Maximum Likelihood vs. Bayesian methods. 51 pp.

2016:1   Selva Bahar Baziki, Rita Ginja and Teodora Borota Milicevic. Trade Competition, Technology and Labor Re-allocation. 83 pp.

2016:2   Matz Dahlberg, Kevin Mani, Mattias Öhman and Anders Wanhainen, Health Information and Well-Being: Evidence from an Asymptomatic Disease. 32 pp.

2016:3   Federico Belotti, Edoardo Di Porto and Gianluca Santoni, The effect of local taxes on firm performance: evidence from geo-referenced data. 37 pp.

2016:4   Edoardo Di Porto and Henry Ohlsson, Avoiding taxes by transfers within the family. 35 pp.

2016:5   Eva Mörk and Mattias Nordin, Voting, Taxes and Heterogeneous Preferences: Evidence from Swedish Local Elections. 30 pp.

2016:6   Luca Repetto, Political budget cycles with informed voters: evidence from Italy. 46 pp.