Political budget cycles with informed voters: evidence from Italy *

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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. Municipal investment in the year before elections is 28.5% higher than in electoral years, and the reform reduced this pre-electoral spending increase by one-third. I then study the role of local newspapers in disseminating municipal financial information to voters and find that the effect of the reform is twice as large in areas with relatively many newspaper readers, suggesting that mayors react to more informed voters by reducing spending manipulation.

Keywords: Information, Political budget cycles, accountability, Italian municipalities
JEL codes: D72; H72; P16

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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 this borrowing can be kept hidden until the elections, voters may mistake the 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 expenditures in roads, parks and public housing and is mainly financed with borrowing and sales of public assets.

Then, I consider 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 by April 30, that is, two months earlier than before. Given that municipal elections are typically held after this date, the reform had the unintended consequence that the balance sheet was made available to voters before the election date. 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.

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, in each calendar year, there are municipalities in different years of the term.[1] The empirical strategy consists in comparing the budget cycle in different groups before and after the reform for each year 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

[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.

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 then consider the impact of local media on the budget cycle. Local media decrease the cost of information for voters by providing summarised information on local matters at a low cost.[2] Using data on sales of local newspapers, I test whether the effect of the reform varies with the availability of local newspapers. In provinces where newspaper sales were above the median, the effect of the reform is almost twice as large as the baseline estimate, while in other provinces the impact of the reform is almost negligible. To disentangle the impact of newspapers from other related factors, I extend this sample-split approach and further divide the sample according to each of four municipal-level variables meant to measure, respectively, social capital, human capital, ethnic homogeneity, and political participation. Once I control for newspapers readership, further splitting the sample based any of these variable does not unveil any additional substantial difference.

I also test two indirect implication of the information mechanism that arise from the presence of term limited mayors and from the fact that larger municipalities are subject, as of 2001, to a set of fiscal rules contained in the Stability Pact. In municipalities where the mayor is term limited, the cycle is smaller and the effect of the reform is reduced and not statistically significant. Similarly, municipalities subject to fiscal rules have much smaller budget cycles and are less affected by the reform. 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 2.6% 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 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,

[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).

2007) and increase relief expenditures in areas with higher newspaper circulation and where voters are more informed (Besley and Burgess 2002, Stromberg 2004). This paper contributes by showing that there are large effects on politicians' behaviour of a simple change in the disclosing policy of an already existing accounting document. Also, given that similar types of accounting documents are used in several other countries, these results are arguably easier to generalise to other settings than those from small-scale randomised experiments.

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.

1 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

[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) emphasise 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. In these models, incumbents have the possibility to increase spending by manipulating policy instruments observable to voters only with a delay.

a particular policy instrument, in this case borrowing, without revealing it to voters before the election. By raising borrowing, the incumbent can finance an increase in the provision of public goods and bias the voters' inference process before elections.

Voters derive utility from a consumption good, from 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 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 t. 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 $R(d_t)$ 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 realised 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.

The reform that requires municipalities to disclose the balance sheet before elections can be interpreted, in this model, as a decrease in the price of information. As this price decreases,

[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. Hence, differently from Rogoff and Sibert (1988) – in which politicians observe their type – the only equilibrium of the game is pooling.

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, the manipulation of pre-electoral borrowing and spending is attenuated.

2 Background information

2.1 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 municipal council (*Consiglio Comunale*), 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; transfers from 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 the elderly, 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 established by the central government 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 may constrain the municipal governments' policy decisions and, hence,

[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. Coviello et al. (2016), instead, consider its effect on firms.

also affect the political budget cycle. I study the effect of the Pact on the budget cycle in section 5.4, and show that the possibility that it is driving the results is unlikely in a robustness check in section 5.6.

2.2 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 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, and is the object of interest here. 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. Municipalities that fail to respect the deadline are sanctioned. A commissioner may be appointed in order to execute the approval process; government transfers are interrupted; and the municipality cannot ask for new mortgages and becomes object of special controls. In some cases, the prefect can even dissolve the council and call new elections.[6]

2.3 The reform

In October 2008, a government decree, 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.[7] The lemma that changed the approval date was a small part of a large text that dealt with general accounting principles for local governments. Given that the law in question contained several provisions in additional to the one of interest of this paper, it is crucial to investigate it in detail to rule out the existence of other, contemporaneous policy changes that might also affect municipal expenditures and, therefore, invalidate my interpretation of the results.

To this end, I read carefully the text of the law (together with the reports of the two spokesman at the *Camera* and *Senato*), and summarised and discussed the provisions contained in each Section in Appendix D. While I refer to the Appendix for details, it is useful to note here that, of the 7 Sections in the law, only Section 2 (and its annexes) is relevant to municipal financing and accounting, with the rest being provisions meant to reduce health care spending in regional governments or in modifying the allocation of some government resources across different categories. Among the dispositions in Section 2, some simply prorogue for an additional year some provisional dispositions approved in previous years; others

[6] These are the generic sanctions that are applied in case the local governments "delay or omit an action that is provided for by law", and also apply in this case (see D.Lgs. 18 August 2000, n.267).

[7] 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).

are ad-hoc measures for Rome and other province capitals, or minor changes in who is the person responsible for signing certain accounting documents. The balance sheet deadline change is contained in Section 2-quater.

To further understand the reasons of the legislators who drafted the law, I also contacted one of the members of the Parliament who discussed the law who confirmed, in a personal conversation, 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.

A final comment is devoted to possible anticipation effects. 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 in section 5.6 provide evidence that endogenous resignations are not driving the results.

2.4 Balance sheets as a source of information for voters

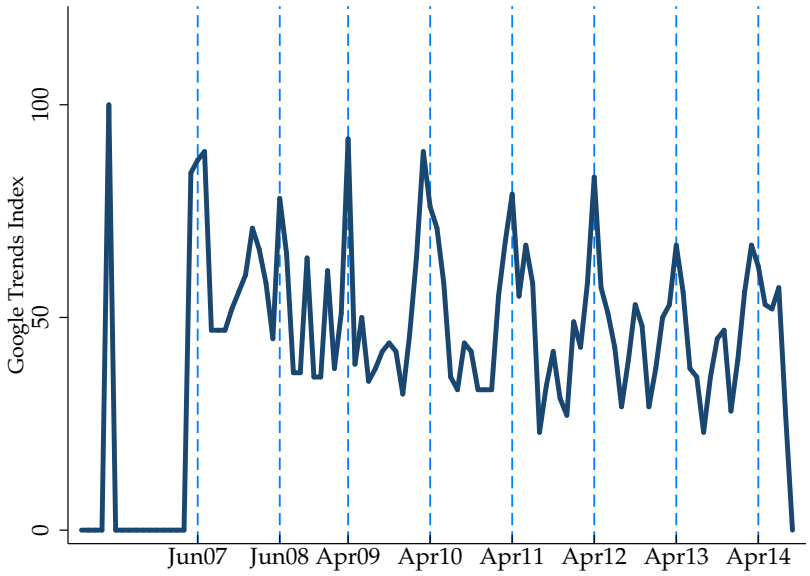
Balance sheets contain information on the financial status of each municipality, such as the level of outstanding debt, the amount and composition of investment and current expenditures, and deficit. Voters might find this information useful to assess the incumbent's performance as an administrator. The presence of the opposition in the municipal 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 [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 among voters, I searched jointly the expression "*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. Also, we can notice that, in line with the expected effect of the reform, the peak in interest shifts from June to April after 2008. Although there could be several factors generating this cyclical pattern (for instance, town accountants might be more actively looking for informa-

tion 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. Interesting, the same pattern arises when using search results from *Factiva*, a database containing past articles from newspaper from all over the world. Figure 2 shows that the frequency of articles in local newspapers discussing matters related to balance sheets over the years 2006-2014 follows the same seasonal pattern observed in the Google trends data, suggesting that the local press covers such issues, and that it does so especially in the month of approval of the balance sheet.

FIGURE 1

GOOGLE TRENDS SEARCH OF THE WORDS "BILANCIO CONSUNTIVO"



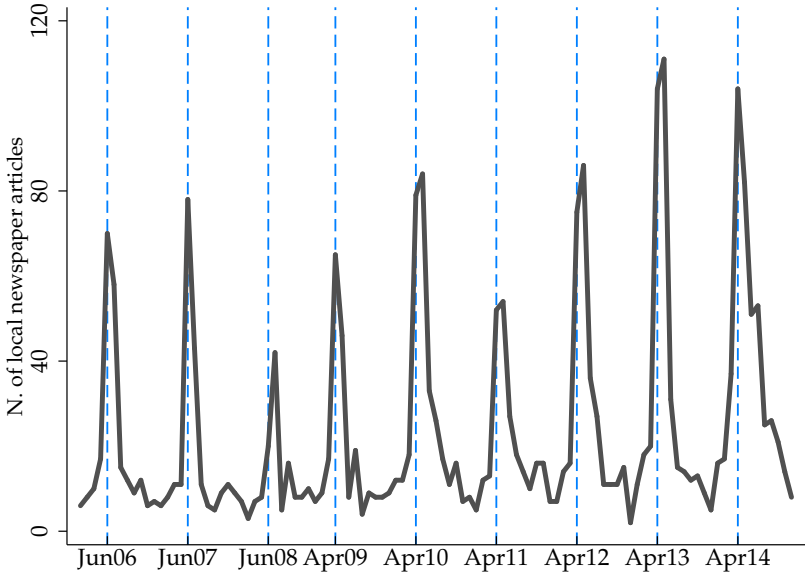
Notes: Google Trends interest over time index of the search "bilancio consuntivo", 2006-2014. Google Trends analyses a percentage of Google web searches to determine how many searches have been done for a specific word or phrase compared to the total number of Google searches done during that time. Dashed 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 should 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 8 in the Appendix one can see that budget quantities are excellent predictors for realised current expenditures, with a correlation of 0.94, but not for investment expenditures, which are the object of interest of this paper.[8] The correlation between the budget forecast and what is effectively spent in investment project is, in the sample, only 0.42. Also, budget quantities are much larger, on average, that realised values.

[8] As figure 3 in the next section shows, current expenditures represent more than half of total municipal expenditures. They are mostly meant to cover ordinary expenses such as salaries and maintenance costs, and they exhibit no budget cycle behaviour.

Figure 2

Factiva search of the words "Bilancio consuntivo" on local newspapers



Notes: Number of articles containing the words "bilancio consuntivo" or "rendiconto comunale" from a *Factiva* search among local newspapers only. Result of the search: 2,476 articles between 2006 and 2014. The search is based on all the 45 local newspapers used in section 5.3, of which only 6 had article entries in *Factiva*.

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 became always available 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 approvals were prevalent, the reform should have little or no impact on the information level of voters. In this sense, the estimated effects should be interpreted as a lower bound.[9]

[9]The evidence from the *Factiva* search results in figure 2 shows that newspaper coverage peaks exactly around the deadline date for approval, indirectly confirming that municipalities tend to abide by the law.

3 Data

3.1 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 Census, 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 Appendix B.

3.2 Sample

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 their 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.[10] 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. Finally, I drop municipalities in the years in which a commissioner is in office in place of a mayor (1,793 cases). The final sample consists of 6,702 municipalities (out of the 8,109 existing municipalities in 2010) for the years 1999-2012.

3.3 Summary Statistics

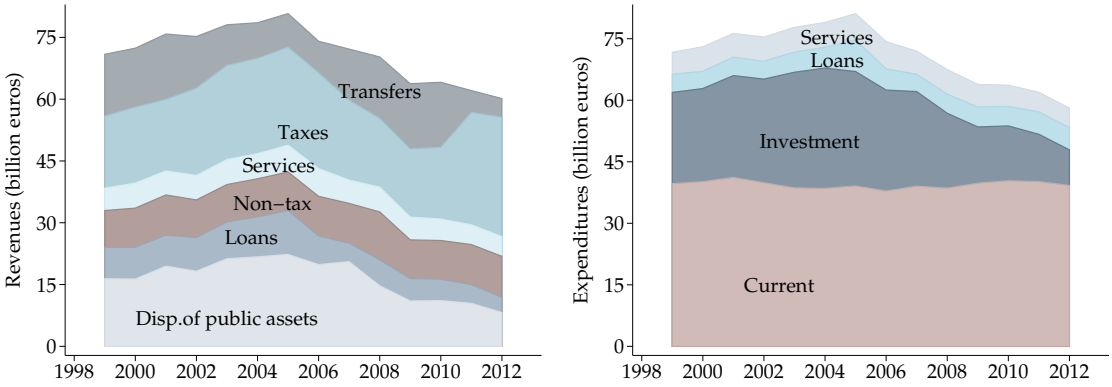
Figure 3 gives an overview of the financial status of municipalities over the sample period. Municipalities had, in 2005, revenues and expenditures for €80 billion Euros (roughly 4.8% of the GDP), an amount that started to decline since then until reaching about 60 billion Euros in

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

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%. Expenditures are heavily concentrated in current expenditures and investment projects, with services and loans accounting for a much smaller small fraction. Investment expenditures started decreasing (both as a fraction of the total and in absolute terms) in 2005, reaching 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).

FIGURE 3

EVOLUTION OF REVENUES AND EXPENDITURES



REVENUES IN BILLION € EXPENDITURES IN BILLION €

Notes: Figures are in 2005 Euros, deflated using the St. Louis FED GDP deflator for Italy. Sample is composed of 6,702 municipalities and excludes municipalities from special regions. The left panel plots total revenues as recorded in the balance sheet, while the right panel shows total expenditures. The small discrepancy between revenues and expenditures is due to the presence of balance sheet deficits or surpluses that are not plotted.

Table 11 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 3. 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 third panel shows that, geographically, Italian municipalities tend to be small on average, with an average population of around 7,400 and have a density of approximately 313 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.

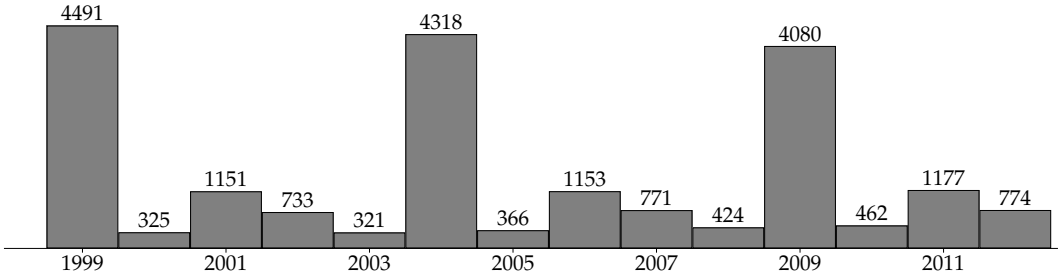
3.4 Election timing

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.[11] In case the mayor, or at least half of the councillors, resign before the end of the term, new elections are called, without the possibility of forming a new coalition.[12] 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 4

Municipalities holding elections in each year



Notes: Frequency of Italian municipalities holding elections, 1999-2012. Special regions are excluded.

Figure 4 shows that municipalities follow different election schedules. 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 June 15, and is the same for all municipalities that are in an election year. Whereas elections always fall in the same period of the year (April 15 to June 15), rules on the disclosure of the balance sheet have changed after 2008. By moving forward the deadline for approval of the balance sheet of all municipalities from June 30 to April 30, the reform had a substantial effect on the amount of information available to voters, who are today more likely to have access to the balance sheet before voting than before 2008.

More than half of the municipalities in the sample had elections in 1999 (and, subsequently, 2004 and 2009). Of the remaining ones, 325 voted in 2000, 1151 in 2001, 733 in 2002 and 321 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 a different electoral cycle.[13]

In table 12 in Appendix C, I report summary statistics for municipalities divided according to the year of first election. The group of municipalities voting in 1999 includes those that

[11] 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.

[12] Early termination can not only be due 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, 10.9 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.

[13] For a brief discussion on the exogeneity of election dates in Italy, see Coviello and Gagliarducci (2017).

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.

4 Empirical analysis

To estimate the effect of voters' information on mayors' decisions, one could imagine a randomised 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 budget cycles in the two groups would give a consistent estimate of the effect of interest. In absence of such an experiment, we have to resort to quasi-experimental methods. The difference-in-differences approach exploits the variation in the information level of voters induced by the 2008 reform to mimic this experiment.

4.1 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:

$$y_{it} = \alpha + \boldsymbol{\beta}_1' \mathbf{d}_{it} + \boldsymbol{\beta}_2' \mathbf{d}_{it} \cdot Post_t + \boldsymbol{\gamma}' X_{it} + \mu_i + \lambda_r \cdot \delta_t + \epsilon_{it}, \tag{1}$$

where $\mathbf{d}_{it}$ is a set of four dummies for each year in the term defined as follows:

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

and zero otherwise, where the indicator for an election year, d_{it}^{τ}, is excluded from estimation to avoid multicollinearity. Hence, all coefficients should be interpreted as deviations from the election year. The year in term indicators collected in $\mathbf{d}_{it}$ 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.[14] To estimate the effect of the reform on the political cycle, those variables are interacted with an indicator $Post_t$ that equals

[14] 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 $\mathbf{d}_{it}$ also vary between municipalities.

one in 2008 and in the following years. The variable $Post_t$ is 1 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.[15] 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, 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. To account for possible endogenous resignations I include a dummy for terms that ended early.[16] Furthermore, I control for the mayor being term-limited or not. 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.

4.2 Identification

Estimation of model 1 relies on both cross-sectional variation, by comparing municipalities in different groups, given by the year of the cycle in which they are, and time variation, by comparing the same municipality in different points in time. Estimation of the budget cycle indicators rely on comparing municipalities in different years of the term, while controlling for common time effects. To estimate the effect of the reform, municipalities in the same group are first compared with other municipalities in different years of the term and then with themselves before the reform.

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 legislature would cause municipalities to reset their electoral cycle and, hence, to change group. In these cases, that the indicators $\mathbf{d}_{it}$ vary 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 generalised decline in municipal resources caused by the economic downturn.

[15] In the following section I will also alternatively define $Post_t$ to be one in 2009 and afterwards.

[16] 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 5.6.

5 The effect of information on mayors' decisions

5.1 The budget cycle and the effect of the reform

In table 1 I report results for the baseline equation 1 (coefficients for controls are omitted here and are reported in table 13 in Appendix C). The first column shows estimates from the specification with controls and year-region dummies. In the second column I add municipality fixed-effects in order to control for time-invariant differences across municipalities. As usual, control variables that are fixed over time – such as surface, altitude and the province capital dummy are dropped when including fixed effects. 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 €488.1, 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% of the sample mean 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%.[17]

After the 2008 reform, the magnitude of the fluctuations in each year of the term decreases substantially, with the largest effects found two years before elections and in the pre-electoral year. 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 the effect of the reform. From the figure, the negative effect of the reform on the deviations from the electoral year - and the variance of the fluctuations - is apparent and sizeable. Results using total expenditures instead of investment are qualitatively analogous and reported in table 14 in the Appendix. Comparing the two tables shows that, interestingly, most of the budget cycle fluctuations are due to investment, while the other components of municipal expenditures (current, service and loans repayments) are essentially stable over the term.

Given that the reform was approved in October 2008, it could be the case that the first effects on mayors' behaviour did not materialise until 2009. In this case, defining the *Post* dummy as one in 2008 and afterwards would induce some measurement error. A possible solution would be to change the definition of *Post* as being one from 2009 on (as opposed

[17] Investment expenditures could also exhibit a cyclical behaviour for reasons other than strategic manipulation. New investments planned at the beginning of a term may start with some delay and take a few years to be completed, peaking towards the end of the term. The fall in the electoral year could then be explained by new incumbents slashing previous investments in order to start their own. Although this hypothesis is difficult to rule out entirely, if one restricts the sample to terms in which the incumbent wins the subsequent election (and, therefore, has no incentives to interrupt previous investment plans), the cycle in expenditures is still visible and, if anything, even bigger (results not reported).

Table 1

The Effect of the Reform - Baseline Results

	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	85.9***	85.5***	81.6***	104.0***
	(9.51)	(9.79)	(9.67)	(6.90)
2 years before election	103.4***	107.4***	103.4***	106.5***
	(8.74)	(9.02)	(8.89)	(6.68)
1 year before election	137.8***	139.0***	122.1***	202.7***
	(11.58)	(11.88)	(10.96)	(8.87)
1 year after election	51.6***	53.8***	51.4***	51.6***
	(9.18)	(9.60)	(9.40)	(6.21)
3 years before elect.*Post	-37.0**	-40.5***	-33.0**	-170.0***
	(15.15)	(15.18)	(14.98)	(9.56)
2 years before elect.*Post	-63.6***	-75.7***	-68.2***	-193.1***
	(14.39)	(14.77)	(14.36)	(8.95)
1 year before elect.*Post	-50.5***	-57.2***	-46.4***	-181.9***
	(15.99)	(16.37)	(15.21)	(10.94)
1 year after elect.*Post	-14.7	-22.5	-19.9	-103.8***
	(14.86)	(15.34)	(15.03)	(8.73)
Mean of dep. var.	488.1	488.1	485.0	488.1
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	0.11
Obs.	86157	86157	90279	86157

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.

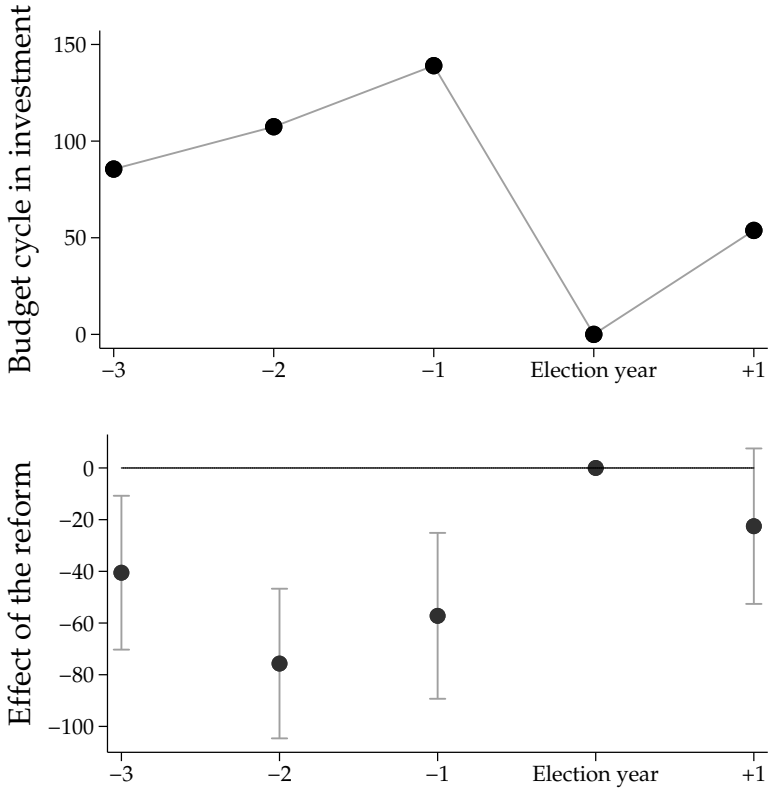
* p < 0.1, ** p < 0.05, *** p < 0.01.

to 2008). Reassuringly, doing so leaves results in this and the following sections substantially unchanged. Baseline results using this alternative definition, together with additional robustness checks, are shown in section 5.6.

Disaggregating investment expenditures in categories reveals differences in the cycle fluctuations: as table 15 in the Appendix shows, there is evidence of pre-electoral spending increases in investment in roads and transportation, social, sport, and parks and public housing (grouped under the "territory" category). Roads and territory are the largest categories in terms of total spending and are also arguably among the most visible to voters. The fact that the largest fluctuations are found in visible categories is in line with results in Kneebone and McKenzie (2001), who also find cycles in roads and sports expenditures. Interestingly, the pre-electoral spending increase in these categories is also the one that drops the most after

Figure 5

The budget cycle in investment and the effect of the reform



Notes: This graph is based on the estimates from equation 1 from the specification in column 2 of table 1. The upper panel shows $\hat{\beta}_1$, the estimated difference in investment expenditures, for each year of the legislature, relative to the election year (in per capita Euros). The lower panel plots $\hat{\beta}_2$, the estimated effect of the reform on investment expenditure, with 95% confidence intervals.

the reform.

One final note is devoted to the interpretation of the results in light of the model. The theoretical framework described in section 1 predicts that the reform should reduce the spending manipulation in the pre-electoral year only.[18] However, as figure 5 shows, the reduction in spending starts in earlier years, apparently in contradiction with the model and with the hypothesis that mayors are reacting to the presence of informed voters in the proximity of elections. But investment projects take time from planning to completion, and the total cost has to be distributed over the whole term, so that mayors who wish to take credit before elections might have to start investing a few years in advance in order to complete the works on time. In this case, it is reasonable to assume that mayors face costs when significantly increasing investment (because of the cost of raising debt) and reducing it (as this implies

[18]Notice that, strictly speaking, the model predicts that the spending should increase in the election year and not, as we observe in the data, in the pre-electoral year, but this is simply an issue of definition.

halting undergoing works). In Appendix A I show that a simple way to model this idea is to include an adjustment cost that is increasing in the amount of borrowing relative to the previous period. In presence of adjustment costs, politicians willing to take electoral credit for providing a public good must increase spending gradually and therefore have to recur to borrowing also in non electoral periods. As a consequence, the political budget cycle in both borrowing and spending has a smoother shape and the reform has an effect on all years of the term.

5.2 The effect of the reform on revenues

With the disclosure of the balance sheet before elections, voters obtain access not only to the level and composition of expenditures, but also to how these 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.

Municipalities can finance expenditures by selling public assets (including land, buildings and releasing construction permits), borrowing, 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 by 20% of the sample mean in the year before elections and in loans (+34%). 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 borrowing is reduced, with the pre-electoral year increase being about one-third lower. This reduction is large in both categories, suggesting that they might either be the easiest to manipulate or the least preferred by voters. 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 the sources of financing by especially reducing some categories such as sales of public assets and borrowing.

TABLE 2

BASELINE RESULTS FOR REVENUES, BY CATEGORY

	(1) Disposals	(2) Borrowing	(3) Non-tax	(4) Services	(5) Tax	(6) Transf.	(7) Revenues
3 years bf. elect.	46.9***	24.9***	1.88**	0.48	5.55***	0.62	80.3***
	(8.19)	(3.15)	(0.95)	(1.30)	(0.81)	(0.80)	(9.14)
2 years bf. elect.	61.8***	31.3***	2.32**	0.17	5.71***	0.72	101.9***
	(7.34)	(3.05)	(1.00)	(1.15)	(0.74)	(0.86)	(8.31)
1 year bf. elect.	67.6***	40.0***	3.17***	1.48	2.55***	1.17	116.0***
	(9.26)	(3.54)	(1.11)	(1.31)	(0.81)	(0.84)	(10.51)
1 year aft. elect.	32.5***	16.2***	1.63**	0.41	3.63***	0.14	54.5***
	(7.83)	(3.08)	(0.78)	(1.19)	(0.70)	(0.79)	(8.94)
3 years bf. elect.*Post	-22.1*	-14.9***	0.94	-0.96	5.83***	-6.87***	-38.0***
	(12.88)	(4.67)	(1.64)	(1.77)	(1.97)	(2.07)	(14.41)
2 years bf. elect.*Post	-50.3***	-17.4***	3.36*	-0.94	4.18**	-5.88***	-66.9***
	(12.23)	(4.67)	(1.82)	(1.69)	(1.85)	(2.10)	(13.99)
1 year bf. elect.*Post	-25.9*	-16.3***	1.44	0.19	1.73	1.90	-37.0**
	(13.55)	(4.95)	(1.60)	(1.77)	(1.51)	(1.80)	(15.15)
1 year aft. elect.*Post	-20.4	-11.2**	1.43	-2.76	0.47	-0.52	-33.0**
	(12.43)	(4.43)	(1.53)	(1.70)	(1.53)	(1.66)	(13.89)
Mean of dep. var.	341.3	117.4	172.2	104.1	359.4	231.8	1326.2
R^2	0.38	0.34	0.75	0.43	0.87	0.85	0.60
Obs.	83287	83287	83287	83287	83287	83287	83287

Notes: In each column the dependent variable is a different category of revenues in per capita 2005 Euros. Controls, year, municipality and region-year fixed effects 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.

5.3 The differential impact of the reform: newspapers, social and human capital

The role of local newspapers

The baseline result shows 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, refrain from manipulating spending. An important source of the information voters receive comes from the active role of local media. As mentioned in section 2, local newspapers usually either directly report news on spending decisions or interview politicians in order to comment the main figures on the most recent balance sheet. By doing so, they lower the cost of information, and increase both 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.

For example, politicians that are under less media scrutiny tend to work less and transfer fewer resources to their constituency (Stromberg 2004; Snyder and Stromberg 2010). In Italy, local newspapers cover political matters at the municipality level extensively, and still play a major role as a source of information for 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.[19]

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. 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.[20] I 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.[21] Equation 1 is then estimated for two samples: the first sample contains all provinces where local newspaper sales are above the 2007 median – defined for each region separately – whereas the second contains those below. I define the median for each region separately because using the national level median would essentially amount to divide the sample in north and south, as it is clear from figure 9 and table 16 in the Appendix. Since region-time dummies are included, estimation relies on cross-sectional variation in the newspaper sales dummy in a given region and year. Results for the alternative model in which, instead of splitting the sample in two, this variable is interacted with the cycle indicators and with the post-reform dummy are analogous and not reported.

Given that newspaper readership can be correlated with several unobservables that also affect spending, omitted variables could be biasing the results. 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, in addition to the controls and fixed effects used in the baseline equation, in all specifications in this subsection and in the next I include, besides the year of

[19] 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. I find no evidence in favour of this hypothesis in the data.

[20] The data from *Factiva*, used to construct figure 2, could also potentially be useful to complement the ADS dataset. Unfortunately, of the 45 local newspapers for which diffusion data are available from ADS, only 6 have entries in *Factiva*, making those data unfit for any statistical analysis.

[21] Figure 9 of the Appendix shows that this variable varies geographically especially along the north-south dimension. As an exception, *La Stampa*, a Turin-based newspaper, is consider 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.

TABLE 3

THE EFFECT OF LOCAL NEWSPAPERS

	Investment expenditures		Borrowing	
	(1) Local sales > median	(2) Local sales < median	(3) Local sales > median	(4) Local sales < median
3 years before election	117.4***	92.0***	41.2***	28.9***
	(20.00)	(15.93)	(7.87)	(5.47)
2 years before election	135.0***	106.7***	43.8***	31.4***
	(21.26)	(12.68)	(7.52)	(5.31)
1 year before election	170.2***	117.2***	40.1***	43.3***
	(21.85)	(19.01)	(7.89)	(6.04)
1 year after election	91.0***	36.8**	27.4***	22.7***
	(20.24)	(14.41)	(8.38)	(5.42)
3 years before elect.*Post	-127.1***	-36.5	-27.8***	-12.0
	(35.53)	(22.28)	(10.59)	(7.75)
2 years before elect.*Post	-119.2***	-72.3***	-37.2***	-9.54
	(35.09)	(23.58)	(10.02)	(8.24)
1 year before elect.*Post	-136.1***	-48.9*	-25.9**	-18.2**
	(33.32)	(27.76)	(11.42)	(8.52)
1 year after elect.*Post	-106.5***	-16.2	-27.1***	-9.55
	(36.13)	(23.64)	(10.18)	(6.95)
Mean of dep. var.	480.4	493.0	121.6	119.5
R^2	0.41	0.42	0.33	0.33
Obs.	33679	52472	33866	53057

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in the first two columns and borrowing 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 2007 median – defined for each region separately – whereas the second and fourth column restricts the sample to those below. Controls, year, year-region, and municipality dummies are included in all specifications. Also, interactions between the cycle indicators, macro-region indicators, and the *Post* dummy are included. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

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

the term indicators and the post-reform dummy, also their interactions with macro-region dummies (north, centre and south).

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 borrowing, and weaker, and in some years even statistically indistinguishable from zero, in provinces with low newspaper readership. Overall, these results support the hypothesis that the effect of the reform is strengthened by the presence of local newspaper which, by covering key issues on municipal matters, facilitate the access to the information contained in the balance sheet.

Social and human capital

While the interactions with macro-region dummies and the post-reform indicators should control for a possible differential impact of the reform in the north, centre and south of Italy, there could still be differences within these regions in terms of social capital, human capital, or the political participation of voters. In this case, the results in table 3 might be biased, and interpreting the differences in the coefficients between columns 1 and 2 as the effect of newspaper readership would be misleading. For instance, if provinces with relatively many newspaper readers are also more likely to have higher social capital, finding a larger effect of the reform in these areas might reflect, in part or completely, the impact of social capital. Indeed, both social and human capital in Italy has been shown to vary substantially both over time and across regions (see e.g. Bertola and Sestito 2011 and Guiso, Sapienza and Zingales 2004).

Although disentangling the impact of newspapers from other factors is challenging, in this section I try to tackle this issue by extending the sample-split idea used previously. Specifically, I further divide the sample in two according to each of four municipal-level variables meant to capture, respectively, social capital, human capital, ethnic homogeneity, and political participation.[22]

I start, in table 4, by using the median number of non-governmental organizations per capita (henceforth NGOs, as recorded in the 2001 Census), calculated for each region of Italy separately, to divide municipalities in those with a number of NGOs above the regional median and those below. Inspecting column 1 reveals that, in municipalities where there are both relatively more newspaper readers and more NGOs, the effect of the reform is the largest and essentially amounts to eliminating the budget cycle fluctuations in expenditures altogether. The effect of the reform is still present but much smaller in municipalities with relatively more newspaper readers but a number of NGOs below the median (column 2). Columns 3 and 4, where the sample is restricted to municipalities with relatively less newspaper readers, show that the effect of the reform is attenuated.

Table 5 uses the same approach to see whether the effect of newspaper readers changes with human capital, measured as the percentage of adults aged 30-34 with a college degree. A comparison of columns 1-2 and 3-4 suggests again that municipalities with many newspaper readers are more affected by the reform, but that, conditioning on this, human capital does not appear to have any significant additional effect, as evident from the similar coefficients in columns 1 and 2, and 3 and 4, respectively. In table 6 I use the percentage of foreign residents as a measure of ethnic composition of the municipality, and find that the effect of the reform is stronger in areas where less foreigners are present, even conditioning on newspaper readership. This might be related to the fact that racially mixed areas tend to show a lower level of participation in social activities and social trust (Alesina and La Ferrara, 2000, 2002).

[22] It is worth noting that, except for turnout (which is the variable used to measure participation) all variables used in this subsection are taken from the 2001 Census and may therefore contain some measurement error.

TABLE 4

THE EFFECT OF LOCAL NEWSPAPERS - BY NUMBER OF NON-GOVERNMENTAL ORGANIZATIONS

	Local sales > median		Local sales < median	
	(1) NGOs p.c. > median	(2) NGOs p.c. < median	(3) NGOs p.c. > median	(4) NGOs p.c. < median
3 years before election	132.9***	90.8***	107.8***	79.9***
	(34.99)	(15.25)	(32.91)	(12.71)
2 years before election	169.7***	94.7***	132.2***	86.7***
	(36.46)	(17.68)	(24.56)	(11.83)
1 year before election	217.8***	107.0***	117.1***	116.4***
	(38.38)	(16.65)	(39.49)	(16.54)
1 year after election	123.2***	48.9***	31.7	45.1***
	(35.46)	(13.72)	(29.58)	(12.17)
3 years before elect.*Post	-166.9***	-65.4***	-44.1	-30.2*
	(63.57)	(21.53)	(46.48)	(16.93)
2 years before elect.*Post	-166.5***	-60.4**	-108.9**	-38.2**
	(62.50)	(24.28)	(50.04)	(18.00)
1 year before elect.*Post	-199.6***	-57.8**	-31.2	-61.5***
	(58.64)	(25.98)	(56.90)	(23.40)
1 year after elect.*Post	-161.4**	-31.1	-15.6	-15.0
	(65.97)	(21.62)	(50.17)	(18.69)
Mean of dep. var.	570.3	356.5	591.3	392.0
R^2	0.41	0.37	0.41	0.42
Obs.	17898	15102	24040	27477

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. The sample is split in four parts: in the first two columns the sample is restricted to provinces where local newspapers sales per capita in given year are above the 2007 median – defined for each region separately – whereas the third and fourth column restricts the sample to those below. I then further split the sample in two according to the median of the number of non-governmental organizations in the municipality (in per capita terms). Controls, year, year-region, and municipality dummies are included in all specifications. Also, interactions between the cycle indicators, macro-region indicators, and the *Post* dummy are included. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

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

Finally, in table 7 I investigate whether municipalities with higher political participation (measured by the turnout in the previous municipal election) are more affected by the reform. The evidence points towards a stronger effect in areas of relatively low turnout, but once again the effect appears to be more strongly related to newspaper readership rather than political participation.

Overall the results in this section suggest that, of all factors considered, the impact of the reform is strongest in areas with relatively more local newspapers readers. Specifically, once

Table 5

The Effect of Local Newspapers - by Percentage of College Educated

	Local sales > median		Local sales < median	
	(1) College educated > median	(2) College educated < median	(3) College educated > median	(4) College educated < median
3 years before election	102.6***	131.7***	54.1***	137.7***
	(27.90)	(28.88)	(18.06)	(28.27)
2 years before election	138.3***	129.7***	77.4***	145.7***
	(27.94)	(32.22)	(13.62)	(23.12)
1 year before election	172.3***	166.2***	99.0***	142.2***
	(34.34)	(26.94)	(20.13)	(35.56)
1 year after election	89.5***	90.5***	39.4***	38.1
	(24.97)	(32.64)	(14.65)	(27.43)
3 years before elect.*Post	-87.0**	-163.3***	-17.8	-58.4
	(40.32)	(59.65)	(24.12)	(40.42)
2 years before elect.*Post	-111.9***	-126.7**	-43.1*	-112.5**
	(39.79)	(58.32)	(24.30)	(44.10)
1 year before elect.*Post	-125.3***	-148.4***	-55.1*	-42.6
	(43.87)	(50.40)	(32.69)	(47.98)
1 year after elect.*Post	-49.8	-158.8***	-21.2	-14.6
	(45.69)	(56.34)	(21.74)	(46.45)
Mean of dep. var.	455.5	502.4	465.4	522.9
R^2	0.40	0.42	0.43	0.42
Obs.	15825	17835	27307	25165

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. The sample is split in four parts: in the first two columns the sample is restricted to provinces where local newspapers sales per capita in given year are above the 2007 median – defined for each region separately – whereas the third and fourth column restricts the sample to those below. I then further split the sample in two according to the median of the percentage of college educated in the municipality. Controls, year, year-region, and municipality dummies are included in all specifications. Also, interactions between the cycle indicators, macro-region indicators, and the *Post* dummy are included. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

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

controlling for newspapers readership by splitting the sample at the median, the additional effect of social and human capital is small. However, the effect of the reform appears to be larger in ethnically homogeneous municipalities, even after controlling for newspapers readership.

TABLE 6

THE EFFECT OF LOCAL NEWSPAPERS - BY PERCENTAGE OF FOREIGN RESIDENTS

	Local sales > median		Local sales < median	
	(1) Foreign residents > median	(2) Foreign residents < median	(3) Foreign residents > median	(4) Foreign residents < median
3 years before election	117.3***	116.7***	84.8***	99.0***
	(26.75)	(29.56)	(17.18)	(28.53)
2 years before election	123.6***	144.6***	113.0***	101.3***
	(32.67)	(27.17)	(15.33)	(21.15)
1 year before election	149.4***	192.2***	136.0***	89.3***
	(32.91)	(28.97)	(21.41)	(33.37)
1 year after election	80.5***	97.5***	43.6***	31.5
	(25.07)	(32.28)	(16.08)	(25.02)
3 years before elect.*Post	-57.9	-196.2***	-39.8*	-33.9
	(37.85)	(59.69)	(24.00)	(39.74)
2 years before elect.*Post	-74.7*	-158.6***	-47.1*	-108.8**
	(40.15)	(56.74)	(24.53)	(42.43)
1 year before elect.*Post	-63.1	-207.9***	-55.3*	-36.4
	(43.22)	(49.92)	(31.47)	(47.90)
1 year after elect.*Post	-40.1	-164.0***	-0.95	-36.6
	(32.26)	(63.40)	(22.45)	(44.23)
Mean of dep. var.	449.0	508.9	450.7	538.0
R^2	0.37	0.44	0.42	0.42
Obs.	16048	17612	27073	25399

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. The sample is split in four parts: in the first two columns the sample is restricted to provinces where local newspapers sales per capita in given year are above the 2007 median – defined for each region separately – whereas the third and fourth column restricts the sample to those below. I then further split the sample in two according to the median of the percentage of foreign residents in the municipality. Controls, year, year-region, and municipality dummies are included in all specifications. Also, interactions between the cycle indicators, macro-region indicators, and the *Post* dummy are included. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

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

5.4 Term limits and fiscal rules

Theoretical explanations for why budget cycles exist implicitly assume that politicians have re-election motives. In the framework of this paper, the change in cycle after the reform is attributed to mayors seeking re-election reacting to voters having better information. A testable implication of this mechanisms comes from the fact that several mayors in the sample are term-limited and are therefore not eligible for re-election. In municipalities where the

Table 7

The Effect of Local Newspapers - by Political Participation

	Local sales > median		Local sales < median	
	(1) Turnout > median	(2) Turnout < median	(3) Turnout > median	(4) Turnout < median
3 years before election	56.9*	140.4***	44.7	104.8***
	(33.12)	(24.76)	(34.95)	(19.37)
2 years before election	102.2**	144.8***	103.2***	100.0***
	(41.00)	(26.14)	(26.09)	(16.45)
1 year before election	212.3***	154.2***	90.0**	132.8***
	(50.92)	(22.93)	(39.79)	(22.00)
1 year after election	47.0	108.7***	4.61	40.2**
	(36.22)	(24.14)	(28.30)	(17.80)
3 years before elect.*Post	-13.8	-162.0***	-1.61	-43.3
	(69.69)	(44.54)	(51.86)	(26.74)
2 years before elect.*Post	-14.8	-151.3***	-36.5	-88.9***
	(53.86)	(47.48)	(42.03)	(32.27)
1 year before elect.*Post	-159.3**	-127.0***	-9.24	-70.7**
	(65.29)	(40.45)	(53.41)	(35.77)
1 year after elect.*Post	-41.3	-131.9***	30.5	-37.7
	(59.76)	(47.51)	(35.69)	(33.55)
Mean of dep. var.	454.3	503.6	459.2	528.3
R^2	0.44	0.40	0.41	0.43
Obs.	15787	17818	27189	25027

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. The sample is split in four parts: in the first two columns the sample is restricted to provinces where local newspapers sales per capita in given year are above the 2007 median – defined for each region separately – whereas the third and fourth column restricts the sample to those below. I then further split the sample in two according to the median turnout in the municipality (measured in the 2007 municipal election or in the closest previous election). Controls, year, year-region, and municipality dummies are included in all specifications. Also, interactions between the cycle indicators, macro-region indicators, and the *Post* dummy are included. Standard errors are robust to heteroskedasticity and clustered at the municipality level.

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

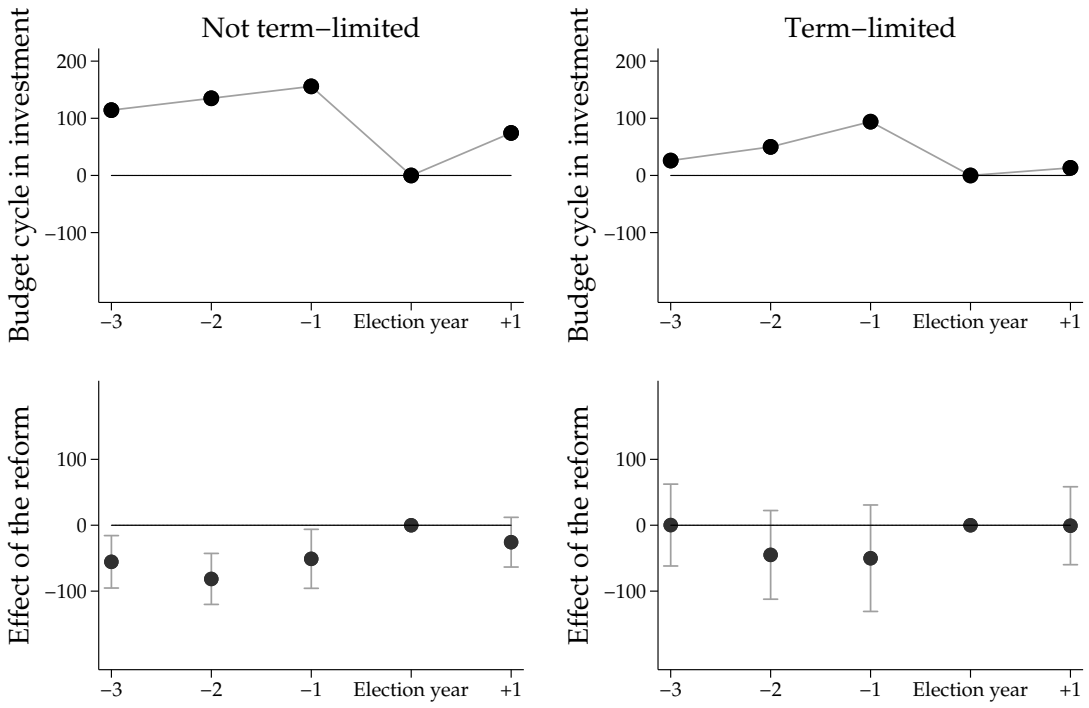
incumbent is term-limited – and, hence, has no re-election pressure – we should observe that first, the budget cycle should be absent, or at least much reduced; and second, the reform should have little or no effect. To investigate this, I re-estimate the baseline model of equation 1 by first restricting the sample to municipalities with a mayor who is eligible for re-election, and then to those with a term-limited mayor in office, respectively.

To ease interpretation, in this section I only report a graphical representation of the results, leaving the regression output for the Appendix. As figure 6 shows, the estimated budget

cycle is substantially smaller in term-limited municipalities, suggesting that when electoral incentives are absent the cycle fluctuations diminish. One possible explanation for why the cycle does not vanish is that term-limited politicians still retain some electoral incentives. In fact, they are allowed to run again after waiting out one term (and they sometimes do so), and they may want to help their party's candidate win the election. Regarding the effect of the reform in term limited municipalities, shown in the lower right panel, it is negative and relatively small, suggesting that term-limited mayors do not react significantly to the disclosure of the balance sheets before elections.[23]

Figure 6

The budget cycle and the reform effect in term-limited municipalities



Notes: The upper panels reports budget cycle estimates from baseline model of eq. 1 in municipalities with non term-limited and term-limited mayors, respectively. The lower panels plot the estimated effect of the reform in the corresponding group together with 95% confidence intervals. Full results are in table 21.

Another testable implication of the information hypothesis comes from the fact that, as mentioned in section 2, after 2001 only municipalities with more than 5,000 inhabitants are subject to the budgeting constraint of the Stability Pact. Municipalities that violated the Pact were subject to sanctions, such as a cut in government transfers or a hiring ban (see, e.g., Grembi, Nannicini and Troiano 2016). The specific rules changed from one year to the other,

[23] To be precise, the coefficients for the reform effect one and two years before elections are negative and of some magnitude. However, none of them is statistically significant at the 10% level, as one can also appreciate in table 21 in the Appendix.

and most of the details are outside the scope of this paper.

In municipalities subject to the Pact, margins for manipulation of expenditures for electoral purposes are arguably smaller. As a consequence, in such municipalities, there should be a smaller cycle and, given the limited margin of action, a smaller effect of the reform. To see if municipalities subject to the Pact behave differently, I estimate the baseline model separating municipalities that are subject to the Pact from the rest.[24] Figure 7 shows that, to start, municipalities subject to the pact have smaller budget cycle fluctuations. Then, with respect to municipalities not subject to the Pact, the effect of the reform is reduced in all years, both in absolute terms and with respect to the sample mean.

Taken together, these results lend support to the information hypothesis. Coherently with the model, where budget cycles are a mean to help with re-election, mayors that are subject to fiscal rules or that are term-limited should have less incentives to manipulate spending to begin with and, at the same time, the new information available after the reform should have little effect on their behaviour. Both the term-limit and the fiscal rule results described before provide some additional evidence that is consistent with the information mechanism.

5.5 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, likely 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. Specifically, to measure pre-electoral spending, I include in estimation current, investment, borrowing, and services expenditures in the year preceding the election. To control for possible size effects and municipality specific characteristics, I use a cubic polynomial in population, surface, density, altitude, an indicator for province capitals, and an indicator for early termination of the term.[25] Finally, I include region fixed effects and 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.

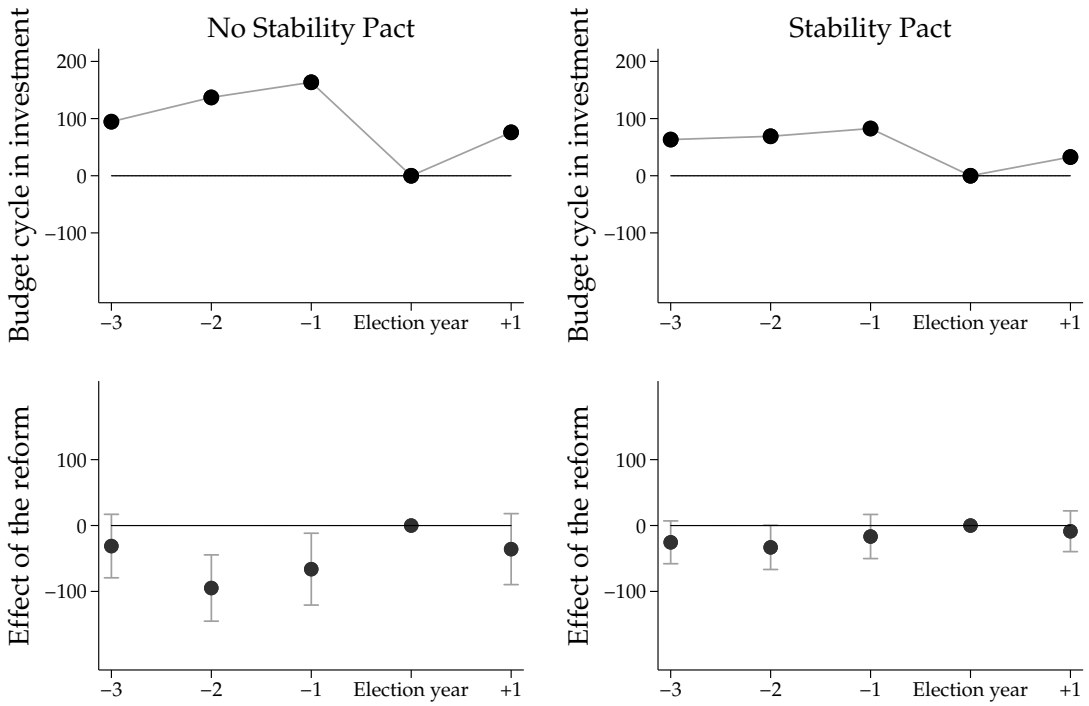
In order to see how spending affects re-election before and after the reform, in Table 8 I report estimates for different groups of municipalities separately, starting from those holding elections in 2007 and before (in column 1) and then showing results for the 2008, 2009, 2010,

[24] The population to use in order to calculate the threshold is the number of residents registered two years earlier (Chiades and Mengotto, 2015). The sample is restricted to 2001-2012 because before 2001 all municipalities were subject to the Pact.

[25] Results are robust to excluding terms that did not end regularly altogether.

Figure 7

The budget cycle and the reform effect in Stability Pact municipalities



Notes: The upper panels reports budget cycle estimates from baseline model of eq. 1 in municipalities subject or not to the Stability Pact, respectively. Only municipalities with less than 10,000 inhabitants are considered. The lower panels plot the estimated effect of the reform in the corresponding group together with 95% confidence intervals. Full results are in table 21.

and 2011 separately in columns 2 through 5. To help with the interpretation, I show estimated elasticities (evaluated at the sample mean of all variables) instead of the Probit coefficients.

The probability of being re-elected – conditional on running again – is quite high: in the 6,722 terms in this sample, the incumbent is re-elected 76% of the times. As the lower panel of Table 8 shows, re-election rates are quite stable over the years, with a slight drop in 2008 and a recovery afterwards.

In pre-reform years, neither current expenditures nor borrowing or services are correlated with re-election. However, investment expenditures have a positive elasticity of 0.026, indicating that doubling expenditures in pre-election years is associated with a 2.6% 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 per-capita investment expenditures by a sizeable amount.

In post-reform elections, however, the positive correlation between investment and re-election appears to weaken, and even turns negative in the 2010 election. At the same time, the coefficient on borrowing appears to become negative over the years, although never being

TABLE 8

EFFECT OF EXPENDITURES ON RE-ELECTION PROBABILITY

	<=2007	2008	2009	2010	2011
	(1) Elast./SE	(2) Elast./SE	(3) Elast./SE	(4) Elast./SE	(5) Elast./SE
Exp. in pre-election year					
Current	-0.033	0.039	-0.010	0.100	0.040
	(0.027)	(0.201)	(0.036)	(0.187)	(0.050)
Investment	0.026***	0.063	0.017	-0.050*	0.009
	(0.009)	(0.055)	(0.012)	(0.030)	(0.026)
Borrowing	-0.001	0.019	-0.008	-0.028	-0.019
	(0.005)	(0.024)	(0.006)	(0.019)	(0.020)
Services	-0.005	-0.022	-0.000	-0.010	0.025
	(0.005)	(0.080)	(0.014)	(0.015)	(0.029)
Expenditures over term					
Total exp. in the term	0.004	-0.030	-0.014	-0.064	-0.034
	(0.009)	(0.114)	(0.018)	(0.077)	(0.049)
Mean of dep. var	0.77	0.71	0.78	0.73	0.74
Controls	Y	Y	Y	Y	Y
Region Effects	Y	Y	Y	Y	Y
Pseudo-R^2	0.10	0.12	0.05	0.19	0.09
Obs.	3024	184	2089	181	465

Notes: The dependent variable is equal to 1 if the incumbent ran again for mayor and was re-elected, and 0 if the incumbent ran, but lost to the challenger. In column 1 we restrict the sample to elections before 2008. In columns 2 through 5 we restrict to elections in a particular year, as indicated in the column titles. Controls and region fixed effects are included in all specifications. To help with the interpretation, the estimated elasticities (calculated at the sample mean of all variables) and their s.e. are reported in place of Probit parameter estimates. Current, investment, borrowing and services expenditures are measured in the year before elections. 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.

statistically significant at conventional levels. Overall, the results in this section provide suggestive evidence that investment spending helped re-election chances before the reform, but not afterwards, when voters are exposed to information. This interpretation of the results is consistent with the prediction of the model that the probability of re-election is decreasing in the fraction of informed voters. However, because of the difficulty to control for all possible determinants of re-elections that are correlated with spending, caution is needed in giving these coefficients a causal interpretation.

5.6 Robustness analysis

In this section I consider several robustness checks and address possible issues related to identification that could bias the baseline estimates (Meyer, 1995). I start by redefining the indicator for post-reform years to be equal to one in 2009 and afterwards (as opposed to

2008). Then, I consider different possible threats to identification. 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.

Redefinition of the *Post* variable

Given that the government decree was approved in October 2008 and converted in law in December of the same year, it is possible that its effects on mayors' behaviour (and, hence, on the 2008 expenditures) were minimal. To explore whether the results are driven by the definition of *Post*, the indicator for post-reform years, in table 9 I redefine it as being one in 2009 and afterwards (as opposed as using 2008 as reform year as in the rest of the paper). Reassuringly, although the estimated effect of the reform is slightly reduced when using this definition, results are qualitatively very similar.

Heterogeneous trends in spending

Even after controlling for observables, time, and municipality effects, it is possible that there are 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 12 might be the result of group-specific trends related to those characteristics that also affect spending.

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 10 report results for those two models and show that coefficients are very similar to the baseline point estimates.[26]

Next, I estimate municipality-specific trends using data from the pre-reform period only (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:

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

[26] 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.

TABLE 9

THE EFFECT OF THE REFORM - BASELINE RESULTS REDEFINING THE Post VARIABLE

	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	84.8***	85.8***	81.8***	98.2***
	(8.99)	(9.23)	(9.11)	(6.61)
2 years before election	100.6***	102.0***	98.1***	104.7***
	(8.37)	(8.70)	(8.58)	(6.57)
1 year before election	129.0***	132.3***	117.9***	147.3***
	(10.08)	(10.47)	(9.72)	(7.06)
1 year after election	45.3***	47.7***	45.7***	46.3***
	(8.70)	(9.06)	(8.88)	(6.08)
3 years before elect.*Post	-37.5**	-44.7***	-35.9**	-184.1***
	(16.10)	(15.98)	(15.71)	(9.83)
2 years before elect.*Post	-60.1***	-65.5***	-58.3***	-199.7***
	(14.96)	(15.09)	(14.62)	(9.08)
1 year before elect.*Post	-33.3*	-50.6***	-44.9***	-140.3***
	(19.10)	(18.83)	(16.90)	(15.65)
1 year after elect.*Post	6.03	-3.47	-2.85	-99.1***
	(15.74)	(16.28)	(15.96)	(9.24)
Mean of dep. var.	488.1	488.1	485.0	488.1
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	0.11
Obs.	86157	86157	90279	86157

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. *Post* is an indicator for years from 2009 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.

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. Finally, I include a municipality-specific linear trend directly in the baseline specification (eq. 1). This model can be estimated by OLS on data differenced twice.

Columns 3 and 4 of table 10 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 15) 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

Table 10

Robustness I - Unobservable Municipal-Specific Trends

	Baseline char. interactions		Individual trends	
	(1) Trend	(2) Dummies	(3) Pre-estimated	(4) Controls
3 years before election	87.9***	77.0***	87.0***	82.4***
	(10.0)	(10.0)	(9.91)	(11.5)
2 years before election	107.2***	95.2***	107.7***	102.0***
	(9.16)	(9.32)	(9.14)	(12.0)
1 year before election	140.4***	137.3***	141.1***	135.2***
	(12.1)	(12.3)	(12.0)	(13.3)
1 year after election	58.2***	56.4***	54.7***	55.2***
	(9.83)	(10.1)	(9.64)	(10.7)
3 years before elect.*Post	-43.4***	-24.5	-37.1**	-21.3
	(15.6)	(16.0)	(15.4)	(21.0)
2 years before elect.*Post	-76.4***	-56.2***	-72.1***	-45.3*
	(15.0)	(15.6)	(14.9)	(23.7)
1 year before elect.*Post	-64.2***	-59.7***	-58.2***	-45.4**
	(16.9)	(17.2)	(16.4)	(19.6)
1 year after elect.*Post	-29.1*	-30.1*	-18.2	-16.9
	(16.1)	(16.3)	(15.4)	(19.8)
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.	83478	83478	84239	70039

Notes: The dependent variable is investment expenditures per capita in 2005 Euros in all columns. Controls, year-region 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.

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 different in terms of observable characteristics (see table 12 in the Appendix), a possibility is to exclude all municipalities that held elections in 1999 and estimate the model again. In table 19 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 any more. I construct an artificial, deterministic election cycle for all municipalities as follows: municipalities holding elections in 1999 are automatically assumed to vote again 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 18 in the Appendix and are quite reassuring: both specifications go in the same direction as the baseline results.[27]

Other confounding factors

A natural concern given the timing 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 macroeconomic factors 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.[28] The effect of the crisis - be it to reduce government transfers and tax income or increase the cost of borrowing - 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 any more.

To control for a possible deterioration in the access to credit, in column 1 of table 20 in the Appendix 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 specification, reduced. Both variables are negatively correlated with spending, although their 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 minimise the impact of post-crisis years in estimation, but again the effect of the reform remains negative although it is estimated with less precision.

[27] 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 also reported in the Appendix.

[28] In fact, as figure 3 shows, the decline in investment expenditures indeed took place but it started earlier, in 2005.

Another possible confounding factor is the Domestic Stability Pact (see section 2) 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 after 2001 only larger municipalities, with population strictly 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.

6 Conclusions

Politicians are traditionally thought to behave strategically before elections, yet direct empirical evidence on this matter is rather scarce. 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 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 internalise the cost of additional spending in terms of more debt or higher taxes in the future. If politicians have an informational advantage, however, they can mislead 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 affects 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 significantly. To investigate the impact of local media as means of circulating information, I then test whether the reform had a differential impact in provinces where local newspaper sales are relatively high, and find that, in such areas, the effect of the reform is almost twice as strong as the baseline estimates. The evidence in this paper informs the discussion on the importance of information for political accountability. Following a large and unexpected increase in voters' information, local politicians react by reducing spending manipulation, and they appear to do so especially in areas where voters are relatively more exposed to local news.

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Online Appendix

A Appendix - Conceptual framework

A.1 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.[29]

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.

[29] 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.

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

A.2 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:

$$\begin{aligned} \tau^b &= \tau^* \\ E_t[g_{t+1}^b] &= \tau^* - E_t R(d_t^*), \end{aligned}$$

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

$$\begin{aligned} \tau^a &= \tau^* \\ E_t[g_{t+1}^a] &= \tau^* - E_t R(d_t^*) + E_t(\mu_t^a). \end{aligned}$$

Voter i chooses the incumbent if $E_t U_{t+1}^a > E_t U_{t+1}^b$, that is, if

$$E_t(g_{t+1}^a) + E_t u(c_{t+1}) - \frac{1}{2}\theta_i \geq E_t(g_{t+1}^b) + E_t u(c_{t+1}) + \frac{1}{2}\theta_i.$$

Substituting and simplifying one obtains that voter i chooses a if

$$E_t[\mu_t^a] - \theta_i \geq 0.$$

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

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

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

$$\mu_t^a = g_t - \tau^* - d_t - \mu_{t-1}^a,$$

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

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

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:

$$\begin{aligned} P_t =& Pr\left[\pi\left(\mu_t^a + \frac{1}{2}\right) + (1-\pi)\left[\mu_t^a + d_t - \hat{d}_t + \frac{1}{2}\right] \geq \frac{1}{2}\right] \\ =& Pr\left[\mu_t^a \geq (1-\pi)(\hat{d}_t - d_t)\right] \\ =& 1 - F[(1-\pi)(\hat{d}_t - d_t)]. \end{aligned} \tag{2}$$

The probability of being re-elected, thus, depends on the level of borrowing and on the fraction of informed voters. 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:

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

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.[30] 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 1 *The equilibrium level of borrowing in election years is increasing in the*

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

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.

A.3 Introducing adjustment costs

In equilibrium, the model predicts that borrowing should be positive in election years and zero in off-election years. This is a consequence of the fact that borrowing resources is costly and that when election are far away it yields no benefit to the incumbent. This dynamic of borrowing translates into a similar pattern in spending. In practice, however, public borrowing and spending fluctuations are usually smoother over the term. One example arises with the provision of an investment good (such as a school or a park), which may take several years to complete. The decision of how much to spend more in a given period will thus necessarily have to take into account investment started previously that will in general be hard to slash. One simple way to explicitly allow for this possibility in the model is to include a quadratic adjustment cost in the budget constraint:

$$g_t = \tau_t + d_t - R(d_{t-1}) + \eta_t^j - \alpha(d_t - d_{t-1})^2. \tag{3}$$

Now, providing a high level of public good is more expensive the greater the increase in borrowing (and spending) from the previous period.[31]

The utility function of voters and politicians, and the timing of the model do not change. The decision of the incumbent, however, now becomes dynamic because, for example, the optimal choice of debt in t depends on the previous amount of debt through the adjustment cost and the repayment $R(d_{t-1})$. To develop the intuition, however, it is sufficient to focus on the simplified case in which there are only two periods: $t-1$ (an off-election year), t, when elections take place and $t+1$. I assume that the incumbent in $t-1$ inherits the repayment of an amount of debt d_{t-2}, that I assumed to be equal to 0 for simplicity. The last period is $t+1$, where either the incumbent or the challenger is elected and when I assume that no debt is raised, so $d_{t+1} = 0$. Given these assumptions, the only decisions the incumbent has to take are in t and $t-1$.

[31] An analogous possibility is to include a quadratic cost in g_t instead of d_t.

Incumbent's choices of d_t and τ_t

The incumbent at time t chooses τ_t and d_t optimally by maximizing the expected utility:

$$\begin{aligned}\max_{d_t,\tau_t} \quad & E_t[\tau_t + d_t + \eta_t^a - \alpha(d_t - d_{t-1})^2 + u(y - \tau_t) + X] \\ & + E_t P_t \left[\tau_{t+1} + d_{t+1}^a - R(d_t) + \eta_{t+1}^a - \alpha(d_{t+1}^a - d_t)^2 + u(y - \tau_{t+1}) + X\right] \\ & + E_t(1 - P_t) \left[\tau_{t+1} + d_{t+1}^b - R(d_t) + \eta_{t+1}^b - \alpha(d_{t+1}^b - d_t)^2 + u(y - \tau_{t+1})\right].\end{aligned}$$

The first order condition with respect to τ_t yields, as before, that $\tau_t^* = \tau^* = y - u_c^{-1}(1)$ is constant for each t. Differentiating with respect to d_t and equalling the result to zero gives the other first order condition:

$$\begin{aligned}1 - 2\alpha(d_t - d_{t-1}) + \frac{\partial P_t}{\partial d_t} E_t(U_{t+1}^a) + E_t P_t[-R'(d_t) + 2\alpha(d_{t+1}^a - d_t)] \\ - \frac{\partial P_t}{\partial d_t} E_t(U_{t+1}^b) + E_t(1 - P_t)[-R'(d_t) + 2\alpha(d_{t+1}^b - d_t)] = 0\end{aligned}$$

where P_t is the probability of being re-elected as perceived by the incumbent (which, under the assumption that $d_{t+1}^j = 0$, is exactly the same as eq. 2 in the baseline model), and U_{t+1}^j is the politician's utility in $t+1$ in case j is elected. Manipulating the f.o.c. one obtains:

$$1 - 2\alpha(d_t - d_{t-1}) + \frac{\partial P_t}{\partial d_t} E_t(U_{t+1}^a - U_{t+1}^b) + E_t P_t[2\alpha(d_{t+1}^a - d_{t+1}^b)] - R'(d_t) + 2\alpha(d_{t+1}^b - d_t) = 0$$

Replacing $d_{t+1}^j = 0$ and noticing that, under this assumption, $E_t(U_{t+1}^a - U_{t+1}^b) = X$, the f.o.c. becomes

$$1 - 2\alpha(d_t - d_{t-1}) + \frac{\partial P_t}{\partial d_t} X - R'(d_t) - 2\alpha d_t = 0$$

From the definition of P_t one can calculate $\frac{\partial P_t}{\partial d_t}$ and replace it to obtain

$$1 - 2\alpha(d_t - d_{t-1}) + (1 - \pi)F'[(1 - \pi)(\hat{d}_t - d_t)]X - R'(d_t) - 2\alpha d_t = 0$$

In equilibrium, it must be that $\hat{d}_t = d_t$ so that the optimal borrowing choice is implicitly defined by:

$$4\alpha d_t^* + R'(d_t^*) = 1 + 2\alpha d_{t-1} + (1 - \pi)F'(0)X \tag{FOC1}$$

where the dependence on d_{t-1} is due to the adjustment cost.

Incumbent's choice of d_{t-1}

A time $t-1$ the incumbent chooses the level of debt by maximizing expected utility. Since the incumbent knows that in the next period he will still be in power, there is no election

uncertainty in t so that the problem becomes (omit the decision of τ since it is always constant and equal to τ^*):

$$\begin{aligned}\max_{d_{t-1}} \quad & E_{t-1}[\tau^* + d_{t-1} - R(d_{t-2}) + \eta^a_{t-1} - \alpha(d_{t-1} - d_{t-2})^2 + u(y - \tau^*) + X] \\ & + E_{t-1}\left[\tau^* + d_t - R(d_{t-1}) + \eta^a_t - \alpha(d_t - d_{t-1})^2 + u(y - \tau^*) + X\right]\end{aligned}$$

The f.o.c. is

$$1 - 2\alpha(d_{t-1} - d_{t-2}) - R'(d^*_{t-1}) + 2\alpha(d_t - d_{t-1}) = 0 \tag{FOC2}$$

Rearranging and using the assumption that $d_{t-2} = 0$:

$$4\alpha d^*_{t-1} + R'(d^*_{t-1}) = 1 + 2\alpha d_t$$

To obtain a closed-form solution, one can further assume that the borrowing is charged a constant interest r each period, so that $R(d_t) = (1 + r)d_t$ and $R'(d_t) = 1 + r$, yielding

$$d^*_{t-1} = \frac{1}{2}d_t - \frac{r}{4\alpha} \tag{O1}$$

Replacing in (FOC1) one obtains

$$\begin{aligned}4\alpha d^*_t + 1 + r &= 1 + \left(\alpha d^*_t - \frac{r}{2}\right) + (1 - \pi)F'(0)X \\ d^*_t &= \frac{1}{3\alpha}\left[(1 - \pi)F'(0)X - \frac{3}{2}r\right].\end{aligned} \tag{O2}$$

This expression, together with (O1), reveals that the optimal borrowing is larger in the election year and smaller in the year before elections when, differently from the case of no adjustment costs, borrowing is positive. Additionally, the larger the parameter α, that determines how important the adjustment costs are, the smaller the optimal borrowing in both periods. The effect of the reform is to increase the fraction of informed voters π. As a consequence, in presence of more informed voters, the incumbent should decrease borrowing (and, consequently, the amount of public good provided) *both* in the electoral year and in the previous year.

B Appendix - Data Documentation

B.1 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 download them one at a time (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).

B.2 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. This procedure is not carried out for disaggregated investment expenditures because the substantial variability over time of this type of expenditures makes the definition of outliers too arbitrary.

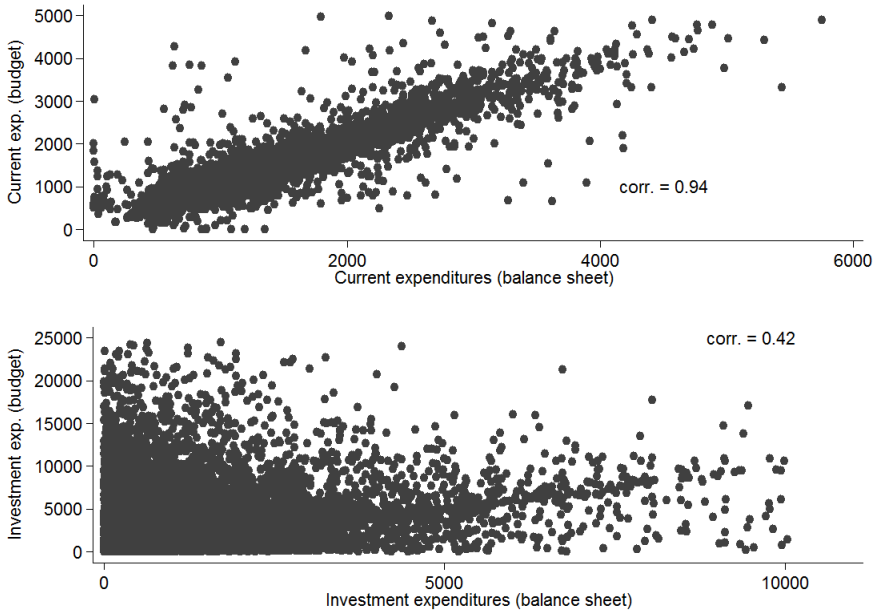
Main variables definitions

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.
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.
Stability pact	Indicator for the municipality satisfying the population criterion for being subject to the stability pact.
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.
Ethnic composition measure	Percentage of foreign residents over municipal total (2001 Census)
Human capital measure	Percentage of individuals aged 30-34 with college degree in the municipality (2001 Census)
Social capital measure	Per capita number of non-profit organizations in the municipality (2001 Census)
Political participation measure	Turnout, defined as the number of voters in the municipal election/Number of eligible voters (measured in 2007 or in the closest previous election).

C Appendix - Additional figures and results

Figure 8

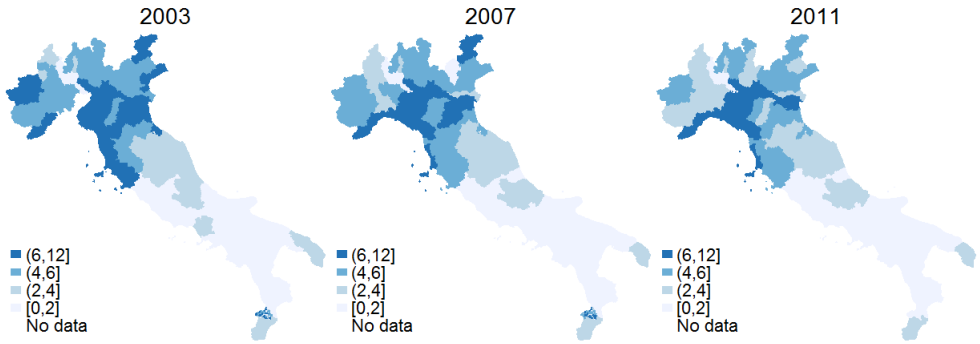
Expected (budget) and realised (balance sheet) per capita expenditures



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 9

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 11

Descriptive Statistics for Municipalities

	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	538.3	388.5	484.9
	(742.2)	(646.1)	(713.0)
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	179.3	183.8	180.9
	(160.9)	(174.8)	(166.0)
Disposal of assets	390.6	294.7	356.4
	(632.3)	(528.7)	(599.2)
Loans revenues	130.3	102.0	120.2
	(220.5)	(211.4)	(217.7)
Services revenues	114.2	101.5	109.7
	(105.1)	(85.7)	(98.8)
Tax revenues	347.1	396.5	364.7
	(181.2)	(208.1)	(192.7)
Transfers revenues	262.3	215.7	245.7
	(187.9)	(186.0)	(188.5)
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	0.34	0.38
	(0.49)	(0.48)	(0.49)
Age of mayor	49.89	51.78	50.56
	(9.61)	(10.01)	(9.80)
Mayor is a 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)	(3.43)	(3.53)
Observations	59988	33316	93304

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 12

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	487.0	479.7	506.1	460.0	415.1
	(2.9)	(12.3)	(6.1)	(7.7)	(10.6)
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	186.8	156.5	179.2	162.1	157.6
	(0.7)	(2.3)	(1.4)	(1.6)	(2.7)
Disposal of assets	356.2	347.4	380.1	337.6	310.1
	(2.4)	(10.1)	(5.4)	(6.4)	(9.8)
Loans revenues	112.3	129.9	139.1	138.5	124.3
	(0.8)	(3.6)	(2.0)	(2.6)	(3.9)
Services revenues	109.1	111.8	113.4	109.1	103.9
	(0.4)	(1.5)	(0.8)	(1.0)	(1.7)
Tax revenues	370.9	345.8	356.8	343.5	365.4
	(0.8)	(3.0)	(1.7)	(2.0)	(3.7)
Transfers revenues	242.0	257.3	256.8	253.3	231.3
	(0.8)	(2.9)	(1.6)	(2.1)	(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)	(0.01)	(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)
Mayor is a 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	62627	4137	14755	8790	2937

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.

Table 13

Baseline Results - Full table

	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	85.9***	85.5***	81.6***	104.0***
	(9.51)	(9.79)	(9.67)	(6.90)
2 years before election	103.4***	107.4***	103.4***	106.5***
	(8.74)	(9.02)	(8.89)	(6.68)
1 year before election	137.8***	139.0***	122.1***	202.7***
	(11.58)	(11.88)	(10.96)	(8.87)
1 year after election	51.6***	53.8***	51.4***	51.6***
	(9.18)	(9.60)	(9.40)	(6.21)
3 years before elect.*Post	-37.0**	-40.5***	-33.0**	-170.0***
	(15.15)	(15.18)	(14.98)	(9.56)
2 years before elect.*Post	-63.6***	-75.7***	-68.2***	-193.1***
	(14.39)	(14.77)	(14.36)	(8.95)
1 year before elect.*Post	-50.5***	-57.2***	-46.4***	-181.9***
	(15.99)	(16.37)	(15.21)	(10.94)
1 year after elect.*Post	-14.7	-22.5	-19.9	-103.8***
	(14.86)	(15.34)	(15.03)	(8.73)
Mayor is a male	-5.71	2.19		15.4
	(12.09)	(11.24)		(12.55)
Age of mayor	-0.76*	-0.47		-0.92**
	(0.39)	(0.38)		(0.40)
Years of schooling of mayor	-5.24***	0.78		-1.62
	(1.18)	(1.12)		(1.21)
Population	-0.0024***	-0.0027		-0.0026***
	(0.00)	(0.00)		(0.00)
Population squared	5.8e-09***	-4.7e-09		6.0e-09***
	(0.00)	(0.00)		(0.00)
Population cube	-1.8e-15***	1.3e-15		-1.9e-15***
	(0.00)	(0.00)		(0.00)
Surface (km2)	-0.63***			-0.38***
	(0.15)			(0.14)
Pop. density (inh./km2)	-0.082***	-0.048		-0.055***
	(0.01)	(0.05)		(0.01)
Altitude (mt.)	0.75***			0.74***
	(0.03)			(0.03)
Province capital	258.1***			220.6***
	(47.29)			(45.62)
Term not ended regularly	-22.3**	-9.46		12.0
	(11.03)	(10.50)		(11.28)
Term limited mayor	9.85*	8.32		12.6**
	(5.65)	(5.41)		(5.57)
Mean of dep. var.	488.1	488.1	485.0	488.1
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	0.11
R^2	86157	86157	90279	86157

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 14

Baseline Results - Total Expenditures

	Baseline specification		W/o controls	W/o year effects
	(1)	(2)	(3)	(4)
	Total exp.	Total exp.	Total exp.	Total exp.
3 years before election	81.3***	85.7***	78.9***	113.3***
	(11.68)	(11.26)	(11.06)	(8.58)
2 years before election	107.0***	114.4***	109.8***	101.8***
	(10.86)	(10.82)	(10.53)	(8.52)
1 year before election	158.0***	149.1***	125.6***	213.8***
	(13.92)	(13.35)	(12.38)	(11.46)
1 year after election	54.0***	56.6***	55.2***	47.7***
	(11.06)	(11.38)	(11.13)	(7.86)
3 years before elect.*Post	-44.6**	-48.6**	-39.0**	-179.0***
	(20.57)	(19.61)	(18.56)	(12.12)
2 years before elect.*Post	-80.1***	-91.8***	-85.0***	-186.9***
	(19.85)	(19.60)	(18.43)	(11.22)
1 year before elect.*Post	-51.5**	-42.8**	-31.1	-161.3***
	(21.81)	(21.27)	(19.64)	(14.66)
1 year after elect.*Post	-25.6	-32.4	-31.9*	-92.6***
	(19.93)	(19.94)	(19.28)	(11.83)
Mean of dep. var.	1443.4	1443.4	1440.4	1443.4
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	0.19	0.60	0.59	0.16
Obs.	87527	87527	91732	87527

Notes: The dependent variable is total 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 15

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.	8.05***	0.029	11.6***	19.2***	2.14	8.81	5.63***	4.86*	5.04*	6.17***	0.29	23.8*
	(1.28)	(0.07)	(3.39)	(5.51)	(2.32)	(6.42)	(1.58)	(2.77)	(2.84)	(2.08)	(0.35)	(12.92)
2 years bf. elect.	9.67***	0.11	18.4***	37.2***	1.66	10.0	6.60***	6.57*	4.91*	6.65**	-0.16	38.2***
	(1.29)	(0.08)	(4.12)	(5.57)	(2.22)	(9.23)	(1.63)	(3.93)	(2.94)	(2.84)	(0.24)	(10.36)
1 year bf. elect.	7.75***	0.081	13.1***	46.7***	7.13**	1.74	8.65***	8.77**	4.30*	5.48**	-0.077	50.2***
	(1.64)	(0.08)	(3.99)	(7.23)	(3.17)	(3.86)	(1.92)	(3.58)	(2.51)	(2.14)	(0.33)	(11.76)
1 year aft. elect.	6.66***	0.11	3.23	11.1**	3.81	0.50	2.94*	1.62	2.97	4.09*	0.068	31.2**
	(1.25)	(0.08)	(3.38)	(4.49)	(2.55)	(5.04)	(1.54)	(3.21)	(2.81)	(2.09)	(0.29)	(12.77)
3 years bf. elect.*Post	0.089	0.12	-6.19	-1.08	-11.0**	-9.36	-1.38	-0.72	-8.70	-4.38*	-0.46	-4.00
	(2.67)	(0.12)	(5.06)	(8.43)	(4.73)	(7.13)	(2.20)	(3.23)	(6.37)	(2.46)	(0.39)	(20.28)
2 years bf. elect.*Post	-2.86	0.052	-16.1***	-18.5*	-9.09**	-10.7	-3.47	1.62	-0.47	-3.31	0.29	-39.0**
	(2.41)	(0.11)	(5.30)	(9.46)	(4.06)	(10.21)	(2.20)	(8.23)	(2.95)	(3.45)	(0.28)	(15.82)
1 year bf. elect.*Post	1.20	0.050	-4.92	-9.86	-10.9**	-5.76	-0.85	-2.65	13.5	-1.92	-0.11	-20.3
	(3.23)	(0.17)	(5.53)	(9.90)	(4.40)	(5.75)	(3.03)	(4.72)	(11.72)	(2.79)	(0.40)	(16.62)
1 year aft. elect.*Post	-3.09	0.17	5.01	-3.17	-12.1***	3.03	1.59	-2.86	1.15	0.40	0.11	-21.0
	(2.48)	(0.17)	(5.12)	(8.11)	(4.49)	(5.50)	(2.43)	(4.39)	(3.70)	(3.08)	(0.46)	(17.14)
Mean of dep. var.	33.1	0.92	79.3	131.1	15.7	17.5	29.1	28.2	12.2	16.6	0.46	202.7
R^2	0.14	0.16	0.30	0.29	0.16	0.18	0.13	0.16	0.19	0.20	0.11	0.29
Obs.	88100	88100	88100	88100	88100	88100	88100	88100	88100	88100	88100	88100

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 16

Municipalities with Newspaper Sales Above and Below the National Median

	< median	> median	Total
Piemonte	3458	13426	16884
Lombardia	6729	14901	21630
Veneto	1694	6440	8134
Liguria	0	3290	3290
Emilia Romagna	448	4354	4802
Toscana	546	3472	4018
Umbria	1288	0	1288
Marche	3416	0	3416
Lazio	5290	0	5290
Abruzzo	4270	0	4270
Molise	1904	0	1904
Campania	7714	0	7714
Puglia	3612	0	3612
Basilicata	1834	0	1834
Calabria	5348	378	5726
Total	47551	46261	93812

The table reports, for each region in the sample, the number of municipalities in which local newspaper sales per capita are above the 2007 national median (equal to 3.59). Observations are municipality-year pairs.

Table 17

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***	0.01***	0.05***	-0.00***	0.00**	-0.00***	0.00
	(181.2)	(8.1)	(5.4)	(14.9)	(-4.4)	(2.7)	(-4.7)	(1.5)
2 years bf.election (theor.)	-0.01***	0.93***	0.01***	0.02***	-0.00***	0.00	-0.00***	0.00***
	(-6.5)	(268.3)	(4.3)	(7.6)	(-6.8)	(0.9)	(-5.0)	(5.2)
1 year bf.election (theor.)	-0.03***	-0.00**	0.95***	0.03***	-0.00***	0.00	-0.00**	0.00***
	(-12.1)	(-2.6)	(302.9)	(10.2)	(-9.8)	(0.1)	(-2.8)	(5.0)
1 years aft.election (theor.)	-0.03***	0.00	-0.00	0.97***	-0.00***	0.00	-0.00***	0.00***
	(-10.2)	(1.4)	(-0.4)	(275.5)	(-5.1)	(0.0)	(-5.9)	(6.5)
3 years bf.*Post (theor.)	-0.07***	0.01	-0.01	-0.07***	0.79***	0.03***	0.00	-0.02***
	(-10.8)	(1.4)	(-1.7)	(-13.2)	(102.0)	(5.0)	(0.7)	(-3.7)
2 years bf.*Post (theor.)	0.00	-0.13***	0.00	-0.04***	-0.01*	0.80***	0.01**	-0.02**
	(0.8)	(-19.2)	(1.0)	(-7.1)	(-2.0)	(101.2)	(2.8)	(-3.1)
1 year bf.*Post (theor.)	0.02**	0.01	-0.15***	-0.04***	-0.01	0.00	0.80***	-0.02*
	(3.1)	(1.8)	(-22.5)	(-7.8)	(-1.8)	(0.7)	(106.2)	(-2.5)
1 year aft.*Post (theor.)	0.03***	0.01	-0.00	-0.19***	0.00	0.01	-0.00	0.79***
	(5.7)	(1.3)	(-0.8)	(-25.1)	(0.3)	(1.7)	(-0.6)	(90.0)
Observations	85629	85629	85629	85629	85629	85629	85629	85629

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 18

Robustness II - Instrumenting the Budget Cycle Indicators

	Using exogenous elections	
	(1) As regressors	(2) As IV
3 years before election	73.4***	78.4***
	(9.81)	(10.6)
2 years before election	99.3***	106.2***
	(9.55)	(9.77)
1 year before election	129.2***	138.1***
	(12.4)	(12.6)
1 year after election	46.7***	49.8***
	(10.9)	(10.9)
3 years before elect.*Post	-40.3***	-36.9**
	(15.6)	(18.2)
2 years before elect.*Post	-72.0***	-71.4***
	(15.2)	(17.4)
1 year before elect.*Post	-75.4***	-69.5***
	(17.2)	(19.4)
1 year after elect.*Post	-24.8	-22.0
	(16.7)	(18.8)
Controls	Y	Y
Year Effects	Y	Y
Year-Region Effects	Y	Y
Municipality Effects	Y	Y
R^2	0.41	0.06
Obs.	85633	85629

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.

Table 19

Robustness III - Excluding Groups from Estimation

	Drop 1999	Drop 2000	Drop 2001	Drop 2002	Drop 2003
3 years before election	80.1***	89.4***	85.0***	85.9***	85.7***
	(15.7)	(10.3)	(13.3)	(10.7)	(10.4)
2 years before election	90.3***	106.4***	114.0***	112.8***	107.1***
	(14.7)	(9.43)	(11.6)	(10.5)	(9.52)
1 year before election	118.1***	134.8***	137.1***	149.0***	145.1***
	(15.1)	(12.4)	(15.2)	(14.1)	(12.8)
1 year after election	50.8***	49.0***	51.1***	64.6***	59.4***
	(13.7)	(10.6)	(11.4)	(11.1)	(10.5)
3 years before elect.*Post	-30.2	-45.3***	-44.9**	-41.0**	-37.7**
	(24.5)	(15.8)	(19.9)	(16.7)	(16.0)
2 years before elect.*Post	-54.7**	-75.7***	-95.1***	-75.6***	-75.2***
	(23.6)	(15.5)	(18.8)	(17.4)	(15.5)
1 year before elect.*Post	-39.2*	-58.8***	-46.3**	-64.5***	-58.9***
	(22.8)	(17.2)	(20.1)	(19.1)	(17.5)
1 year after elect.*Post	-47.7**	-19.9	-13.0	-31.0*	-22.8
	(20.0)	(16.6)	(18.0)	(17.8)	(16.8)
Mean of dep. var	487.7	488.0	483.5	490.4	490.3
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	0.41	0.41	0.41
Obs.	27759	82429	72655	78137	83488

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.

Table 20

Robustness IV - Additional Checks

	(1) Controls for debt	(2) Drop > 2009	(3) No Stability Pact	(4) Adding a lag
3 years before election	68.6***	87.2***	94.6***	92.9***
	(13.2)	(9.97)	(16.2)	(10.7)
2 years before election	102.8***	106.6***	137.0***	119.9***
	(13.1)	(9.16)	(17.0)	(11.1)
1 year before election	132.8***	139.0***	163.5***	135.3***
	(14.9)	(12.1)	(21.0)	(12.9)
1 year after election	54.4***	54.1***	75.9***	73.3***
	(13.5)	(9.73)	(19.1)	(11.6)
3 years before elect.*Post	-24.6	-19.7	-31.2	-48.5***
	(17.4)	(24.1)	(24.6)	(17.1)
2 years before elect.*Post	-72.6***	-61.9***	-94.8***	-83.3***
	(18.0)	(23.9)	(25.7)	(18.3)
1 year before elect.*Post	-52.7***	-42.7**	-66.2**	-50.3***
	(18.7)	(21.4)	(27.8)	(18.4)
1 year after elect.*Post	-22.9	-48.3**	-35.8	-32.7*
	(18.0)	(21.9)	(27.5)	(18.1)
Loans payments p.c.	-0.078			
	(0.050)			
Accumulated debt p.c.	-0.0037			
	(0.0033)			
Mean of dep. var.	471.3	523.4	582.7	487.3
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	0.41	
Obs.	61058	68437	51684	71434

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 not subject to the Stability Pact. 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.

Table 21

The Effect of Term Limits and Fiscal Rules

	Term-limits		Stability Pact	
	(1)	(2)	(3)	(4)
	Not term-limited	Term-limited	No stability pact	Stability pact
3 years before election	114.2***	26.1	94.6***	63.3***
	(12.64)	(18.75)	(16.15)	(13.06)
2 years before election	135.0***	50.0***	137.0***	69.0***
	(12.12)	(18.12)	(17.04)	(12.95)
1 year before election	155.9***	94.1***	163.5***	82.7***
	(15.65)	(24.20)	(21.05)	(13.05)
1 year after election	74.4***	13.3	75.9***	32.7***
	(11.95)	(19.06)	(19.13)	(12.15)
3 years before elect.*Post	-55.5***	0.26	-31.2	-25.5
	(20.31)	(31.70)	(24.58)	(16.58)
2 years before elect.*Post	-81.5***	-45.0	-94.8***	-33.1*
	(19.75)	(34.33)	(25.67)	(17.14)
1 year before elect.*Post	-51.0**	-50.1	-66.2**	-16.7
	(22.84)	(41.22)	(27.82)	(17.09)
1 year after elect.*Post	-25.7	-0.74	-35.8	-8.67
	(19.24)	(30.17)	(27.48)	(15.79)
Mean of dep. var.	478.8	500.8	582.7	277.0
R^2	0.45	0.50	0.41	0.43
Obs.	52226	33419	51684	11127

Notes: The dependent variable is investment expenditures per capita in 2005 Euros. *Post* is an indicator for years from 2008 onwards. The sample is restricted as indicated in each column. The sample in columns 3 and 4 is restricted to municipalities with less than 10,000 inhabitants. Controls, year, municipality and region-year fixed effects 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.

D Appendix- Detailed discussion of law 189/2008

D.1 Main references

Especially useful references to understand the law (in Italian) can be found at the *Camera dei Deputati*'s website: *Discussion in the Commission and in the Chambers* (http://leg16.camera.it/126?pdl=1891).[32]

Of particular interest are the spokesmen's reports at the *Camera* and *Senato*:

- *Camera* (http://leg16.camera.it/126?tab=4&leg=16&idDocumento=1891&sede=&tipo= , see particularly the part *Esame e rinvio*).

- *Senato* (http://www.senato.it/leg/16/BGT/Schede/Ddliter/comm/32464_comm.htm , *Seduta* number 55).

D.2 Discussion

The law 198/2008 (http://www.parlamento.it/parlam/leggi/08189l.htm) has seven Sections, some of which are further divided into sub-Sections. The main objectives of the law were, as the title suggests ("Urgent arrangements to contain health expenditures and regarding accounting principles of local governments") to reduce the expenditure boom in health care (which, in Italy, is mostly financed by regional governments), and to change a few aspects of the accounting process of municipalities. The part that changes the balance sheet deadline is contained in Section 2-quater.

Below follow some comments for each article, based on a reading of the law and on the explanations contained in the reports of the spokesmen during the discussions in the *Camera* and *Senato* cited above.

Section 1 (*Arrangements regarding the carrying out of plans to reduce health financial deficits*)

This Section changes some aspects of the financing of health care provision by regional governments, and thus it is not directly relevant to this paper as it does not affect municipalities. The objective is to reduce the heavy deficits that some regions were running at the time. To this aim, the central government is authorised to appoint special commissioners (at the expense of regional governments) and transfer additional resources to regions in distress.

Section 2 (*Arrangements to preserve the financial stability of local authorities*)

This Section prolongs the applicability of the dispositions made in 2007 to 2009. These dispositions were concerned with compensating municipalities for the missing income deriving from abolishing the Property Tax (ICI) with a corresponding increase in government transfers.

Section 2 - bis (*Transfers to former unions of municipalities in mountainous areas*)

[32] All the links were accessed on May 31, 2017. Although the translation, done by the author, aims at being the most accurate possible, some terms of the Italian legislative jargon presented some difficulties and might be imprecise.

Establishes that the municipalities that replace the mountain communities will receive the corresponding transfers.

Section 2 - ter (*Arrangements regarding fiscal aid for gasoline retailers*)

Regulates the subsidies to gasoline pumps close to Switzerland (to avoid Italian drivers to go to Switzerland because of lower prices).

Section 2 - quater (*Arrangements regarding local authorities*)

This Section is divided in several subsections and contains some modifications of the accounting rules for municipalities, including the one that changes the balance sheet approval deadline.

1. Extends, for 2009, the rule establishing that the councils of municipalities that fail to approve the balance sheet within the deadline could be disbanded.

2. Extends, for 2009, the applicability of previously established criteria for allocation of transfers.

3. Extends the duration of a measure that established how much of the municipal income tax revenues is given to the municipality.

4. States that a bill (to be approved in the future by the Parliament) will be proposed in order to approve the guidelines and format of the municipal balance sheet and other accounting documents.

5. Changes the person in charge to approve and sign some accounting documents in the municipality.

6. Changes the approval deadline of the balance sheet **from June 30 to April 30**.

7. Establishes a deadline and additional requirements to present a summary document on the Property Tax to the Minister of Internal Affairs.

Section 3 (*Arrangements regarding schools, limited to the part that is of competence of local authorities*)

This Article establishes the creation of a "round table" between the Minister of Finance and the Ministry of Education in order to reduce spending on education, in cooperation with the regional governments.

Section 4 (*Extension of deadlines for local authorities*)

Postpones a couple of deadlines (from September 2008 to January 2009 and from December 2008 to December 2009, respectively) for those municipal governments that decide to become a "consorzio" or "unione di comuni" (union of municipalities) by changing articles in pre-existing laws.

Section 5 (*Reallocation of the resources allocated in the CIPE deliberation of September 30, 2008*)

Assigns additional 500 million Euros to Rome and dictates how they can be used. Finances a Commission on fiscal reform with 2 million Euros). Allows Rome and Catania to use part of the funds they already have to cover a deficit.

Section 6 (*Final dispositions*)

Part of the resources destined to the "Fund for underutilised areas" (*Fondo per le aree sottoutilizzate*, created by the 2003 Budget law) are shifted towards another entity, the "Fund for structural interventions of political economy" (*Fondo per interventi strutturali di politica economica*). At the same time, a new entity (*Fondo per la compensazione degli effetti finanziari non previsti a legislazione vigente conseguenti all'attualizzazione di contributi pluriennali*) is created and endowed with 610 million euros for 2010 and 2011.

Section 7 (*Coming into force*)

Sets the date from which the document becomes law to one day after publication (that is, December 7, 2008).