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Elections and project delivery in public work procurement: A red ribbon effect?

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Abstract

In this paper, we investigate the electoral cycle empirically using data on public work procurement in 1,634 French municipalities between 2005 and 2007. Using a difference-in-difference approach, we compare public work procurement of municipalities whose mayor was a candidate in the 2007 legislative elections with those of municipalities whose mayor did not run. We find that public work contracts are more likely to end in the years preceding the legislative election in municipalities whose mayor ran in the election. We interpret these observations as indications that electoral considerations influence the conduct of public procurement. In particular, our results suggest that mayors running for legislative elections in France attempted to enhance their election perspective by influencing the timing of project delivery in public work procurement. We consider several potential mechanisms to explain our results.

Keywords: *Public procurement, Elections, Political Budget Cycle, Construction, Timing of Projects.*

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1 Introduction

Since the pioneering studies of Kramer (1971) and Tufte (1975), it has been argued that the formulation of economic policy may be subject to politicians' electoral considerations. In particular, incumbent politicians may be tempted to adopt policies, such as monetary policy or fiscal policy, that can artificially lead to good economic conditions to please electors. This, in turn, allows them to enhance their election or reelection perspective. According to these strands of literature, electoral considerations can therefore induce macroeconomic cycles. Such electoral considerations also imply that economic policies adopted by politicians can be socially inefficient.

Economists and political scientists have devoted much effort in the past few decades following the seminal contribution of Kramer (1971) and Tufte (1975), to understand theoretically and to search for empirical evidence on the presence of such cycles in the economy. The political budget cycle literature investigates cycles related to fiscal instruments that are induced by electoral cycles (Rogoff (1990)). At the empirical level, evidence on political budget cycles has been mixed (Drazen (2001)). Interestingly, the more recent empirical studies have detected the presence of such cycles in developing countries—where political competition may not be particularly strong—while no such cycles have been detected in developed countries (see e.g. Shi and Svensson (2006); Eslava (2011)).

In this paper, we intend to empirically investigate the basic question formulated by the political budget cycle literature: Do electoral considerations drive the formulation of economic policies? In contrast with previous existing work, we base our investigation by considering one particular policy instrument—public work procurement—, and consider its use in a relatively homogeneous institutional environment, that of France. More specifically, we use data on public work procurement in 1,634 French municipalities between 2005 and 2007. Using a difference-in-difference approach, we compare public work procurement of municipalities whose mayor was a candidate in the 2007 *legislative* elections with those of municipalities whose mayor did not run in these elections. In doing so, we depart from the bulk of existing studies in the political budget cycle literature by our use of micro-data (on procurement contracts) in a more homogeneous context. While micro-data can avoid the issues related to confounding effects often associated with aggregated data, we are unable to consider the whole palette of policy instruments available to politicians. Hence, we see our paper as complementary to existing ones in the literature. The context that we consider is interesting in that our control group are municipalities or mayors who are not subjected to electoral consideration for membership of parliament. In contrast, most existing studies identify political budget cycles by comparing the same municipalities over time.

We believe that public work procurement is an area that is propitious to political budget cycles. Indeed, public works often consist of the construction of infrastructure

such as roads or schools. These infrastructures provide economic services to the general population, and their presence is often quite visible. Hence, politicians may wish to signal their competency and/or the fact that they hold the public interest at heart by engaging in public work (and therefore public work procurement). Public work procurement is one of the main policy instruments for politicians working in municipalities. Lastly, in France, there is a "golden rule" that allows deficit financing only for investments. This latter is directly related to public work procurement. This implies that, if a politician intends to increase spending to enhance his election perspectives, public work procurement is a very likely tool.

Our empirical analysis shows that a public work procurement contract is more likely to be delivered (in expectations) during the pre-election years in municipalities where the mayor is running for legislative elections, compared to those where the mayor did not. This effect disappears in the post-election year in our sample. This supports the idea that public procurement is used strategically before elections. Our empirical results also hold when we consider the share of public work procurement contract that terminates during the pre-election period at the municipality level. Indeed, we show that municipalities whose mayor runs for member of parliament[1] (MP) have a larger share of procurement contracts (measured in terms of value) ending during the pre-election years.

Our results suggest the presence of a political budget cycle in public work procurement. In addition, our results also suggest that the nature of these cycles is different from the one considered in the existing literature: in French municipalities, what seems to matter is not only the absolute level of spending, but also *when a project is completed*. In a way, our results seem to suggest that mayors running for election favoured projects that will be completed before the election takes place. We refer to this pattern as a *red ribbon effect* as it emphasizes the fact that politicians try to deliver public projects before elections and that the budget itself is only a means to this end. This observation may help to explain some existing empirical results in the which do not find any political budget cycle (Brender and Drazen, 2005)[2]: in developed countries, where institutional environment is stronger and monitoring of politicians is more elaborated, politicians may be more subtle in how they influence policy instruments to enhance their election perspectives instead of relying on straightforward increases in consumption spending. In our case, the results suggest the time at which a project is expected to be completed serves such a role.

Theoretically, our results could be driven by (i) politicians trying to signal competence through visible projects or (ii) politicians trying to sway swing voters through pork-barrel projects. Our investigation of these channels shows that our results are consistent with both of these explanations: on the one hand, the electoral cycle effect is stronger for visible projects and on the other hand, running mayors award more contracts

[1]This corresponds to the French *dÃ©putÃ©s*.

[2]Some recent studies have nevertheless cast doubt on the inexistence of political budget cycle in developed countries, see Foucault et al. (2008) or Katsimi and Sarantides (2012).

to local firms.

To the best of our knowledge, our paper is the first study to consider the use of micro-data in public work procurement to understand political budget cycles, and to show that such cycles can arise in terms of the timing of procurement projects. We also believe that our paper contributes to a small but burgeoning strand of literature trying to understand the interplay between politics and (efficient) public procurement. In particular, existing studies in this area considered the effects of ideology (political partisanship) (Hyytinen et al., 2009), of mayors' tenure in office (Coviello and Gagliarducci, 2013), and of third party opportunism (Chong et al., 2011) on the conduct of public procurement. To our knowledge, the channel considered in this paper has yet to be explored in this particular strand of literature.

Our paper is organised as follows: in the following section, we summarise the existing literature on political budget cycles and argue how public procurement can be affected by election considerations. Section 3 then provides an overview of the institutional context in France, on the organisation of legislative elections and on public expenditures at the municipal level. Section 4 describes our data and discusses our empirical strategy. Our results are presented in Section 5. In Section 6 we discuss the potential mechanisms that may drive our results. Concluding remarks follow.

2 From the nature and origins of political budget cycles to spendings through procurement

2.1 Budget cycles and elections

In recognition of the influence of economic conditions on voting outcomes, initiated by Kramer (1971) and Tufte (1975), economists and political scientists have been interested in understanding how electoral considerations by policy-makers shape macroeconomic policies and economic variables. The underlying idea is quite simple: if economic outcomes have an incidence on electoral outcomes, then elected politicians will have incentives to manipulate macroeconomic policies to favour their reelections. This gave rise to the literature on political business cycles, starting with the seminal work of Nordhaus (1975). Nordhaus (1975) shows theoretically that a policy maker will have an incentive to opportunistically orchestrate an economic boom through expansionary monetary policy to enhance his reelection perspectives if voting were based on economic performance in the recent past, and under the assumption that expectations are backward-looking. Hibbs (1977) focuses on on partisanship and initiated a literature trying to understand how politics and ideology drive business cycles.

However, these studies find relatively weak empirical support (see, for instance,

Drazen (2001) for a review).[3] Consequently, the literature shifted the focus towards the effect on electoral considerations on policy instruments rather than on the outcomes in the real economy (Shi and Svensson, 2003). More specifically, this political budget cycle literature seeks to understand if and how fluctuations in a government's fiscal policy can be explained through politicians' electoral concerns. The political budget cycle literature now focuses on the changes in government spending, deficits or taxes in the election year. The basic underlying intuition is based on the idea that in the year preceding an election politicians may be induced to manipulate policy instruments in order to enhance their (or their party's) re-election perspectives.

Rogoff and Sibert (1988) and Rogoff (1990) are among the first to show that fiscal expansion during pre-election years can be compatible with forward-looking rational voters. The authors consider informational problems between politicians and voters. In their setting, fiscal expansion is a means for an incumbent to signal their competence to voters, since the pursuit of such policies is less costly for more competent politicians than for less competent ones. In these models, a politician is assumed to be more competent when he is capable of providing a higher level of public good or government programs at a given cost. In Rogoff (1990), incumbent politicians can also signal their competence by shifting investment spending to consumption spending, assumed to be more visible in the short run. Recent developments rely rather on a "moral hazard" type of argument as the source of political budgetary cycles (Persson and Tabellini (2000); Shi and Svensson (2006)). In these recent developments, politicians can exert a hidden effort to use a policy instrument unobservable to the voters, thereby increasing government programs and leaving voters with the impression that they are more competent. In contrast with theories based on adverse selection, models based on "moral hazard" predict that a budget cycle can arise regardless of the politician's competence. In any case, a budget cycle compatible with rational voters can arise.

These theoretical developments share a common prediction that points to an expansionary fiscal policy in periods near an election. In other words, politicians will choose to exert more effort when the election is close. This prediction hinges on the assumption that the incumbent politician's recent performance is more informative. Martinez (2009) relaxes this assumption, and shows that political budget cycles can arise when past performance is also informative (although less than recent performance). In doing so, he takes into account an incumbent politician's reputation, which should be related to his competence. In contrast with previous literature, he shows that more recent effort can in fact be less effective in manipulating voters' belief, so that budget cycles due to electoral considerations can also arise between elections (and not necessarily only in the period directly preceding an election).

Tufte (1978) provides a first empirical analysis of budget cycles and electoral con-

[3] Although there has been some empirical support on the partisan approach to business cycles (see Alesina (1987); Alesina et al. (1997)).

siderations. He documented, for instance, changes in governmental transfers in the US during pre-electoral periods. Alesina (1988) also investigates the US setting. He found a significant election-year increase in net transfers (over GNP) between 1961 and 1985. However, it appears that the electoral effect is stronger in the US prior to 1980 (Drazen, 2001). Alesina et al. (1997) also provide some empirical evidence on political budget cycles in various industrialized countries. In their empirical investigation, they found support that government deficits are influenced by election years in countries between 1961 and 1993. Interestingly, developing countries also experience election-induced policy cycles (Kraemer, 1997; de los Angeles Gonzalez, 2002; Khemani, 2004). Moreover, according to Schuknecht (2000), public investments are particularly prone to electoral cycles in developing countries. His result is based on a sample of 24 countries between 1973 to 1992. Likewise, Shi and Svensson (2006) show, using a sample of 85 countries from 1979 to 1995, that balance deficits tend to deteriorate and public expenditures tend to increase in pre-electoral periods in developing countries. In comparison, political budget cycles seem to be less pronounced in developed countries. Taken together, this recent empirical evidence suggests that electoral cycles in fiscal policies tend to take place in environments in which voters are unable to monitor effectively the choices of incumbent politicians (Eslava, 2011).

To sum up, recent developments in the political budget cycle literature point out that politicians can have an incentive to manipulate policy instruments to enhance their re-election perspectives. This is conceptually compatible with forward looking rational voters when there are some information asymmetries between voters and politicians. For manipulation of policy instruments to have an impact, they should translate into visible outcomes for voters and may start before an election year. Political budget cycles vary empirically in size and according to a country's politico-institutional environment.

2.2 From public spending to local public procurement

In this paper, we empirically investigate the issue of political budget cycles at the local level (municipality) using public procurement data for public works. Local governments are represented by politicians in most modern democracies, which means that they care about re-elections. As such, they may also have an incentive to manipulate policy instruments in order to enhance their re-election perspectives. At the empirical level, there is evidence of electoral cycles at the local level (Petry et al., 1999; Baleiras and da Silva Costa, 2004; Binet and Pentecôte, 2004; Foucault et al., 2008).

We believe that looking into public procurement data offers the following advantages over more aggregated expenditures or tax rates. Firstly, public procurement is one of the many means by which spendings are realised. Hence, an examination of how public procurement is influenced by electoral considerations, if such a link exists, can enhance our understanding of how manipulation of the public budget is realised. Moreover, in

industrialized countries, local public finances are generally subjected to some regulations.[4] This means that budgetary manipulation for electoral concerns may be more limited. On the other hand, while public procurement is also a highly regulated activity, a politician has discretion over the choice of projects and how they can be implemented. Lastly, Foucault et al. (2008) have shown that a budgetary cycle exists on capital expenditure in French municipalities (as compared to operational expenditure). This prompts us to consider public work procurement, as it is directly related to capital expenditure. Hence, it seems to us that investigating procurement data on public work is relevant and can be useful in uncovering how capital expenditure cycles come into being. Moreover, investment on the sub-national level accounts for 2/3 of total public investment in the OECD countries, and even slightly more in France (Charbit, 2011). Therefore, the stakes involved seem substantial.

Furthermore, public procurement is an important subject. It accounts for a large part of economic activity (more than 13% of GDP on average in OECD countries in 2011 according to OECD (2013)). Efficient public procurement can lead to substantial savings to the general public. To the extent that politicians are decision-makers in public procurement, it is useful to understand how political considerations may affect choices made in public procurement. To our knowledge, existing empirical studies investigating politics and public procurement tend to focus more on favouritism by politicians in public procurement (Hyytinen et al., 2009; Coviello and Gagliarducci, 2013). We believe therefore that electoral considerations can also provide an interesting perspective on how politics and public procurement may interact with each other.

Based on our literature review, we believe that if there is a budget cycle, then procurement patterns in a municipality with a candidate running for election will be different from those without a candidate running for election. Moreover, given that budget cycles due to electoral considerations may arise to the extent that fiscal policy translates into observable results for voters, we expect that procurement projects will be timed to finish close to elections. Since public works are often investments which take time to be realised, we expect that these contracts are more likely to finish during an election year in those municipalities where the mayor is running for election. We believe that this prediction is consistent with the general political budget cycle in that past decisions may matter to signal to voters a politician's competency in a world where politicians have reputational capital (Martinez, 2009). Hence, we expect politicians to time public procurement projects in such a way that projects are realised during election years, and therefore can be visible to voters. In the following, we take this prediction to the data.

[4]See below for the French case.

3 Municipality spendings and legislative elections in France

France has over 36,000 municipalities. They have responsibilities over a wide variety of areas including, but not limited to, education, culture, social and sanitary, and sports and leisure. There are operational and capital expenditure in a municipality. The former notably includes wages of municipal employees and supplies, while the latter consists of equipment expenses and debt payments. A municipality's revenue stems from different types of taxes as well as subsidies from central and regional governments. French municipalities have to follow a "golden rule": over a given year, deficit financing is only allowed for capital expenditures. Similar rules apply, for example, to Germany, Japan, Sweden or the Netherlands (Sutherland et al., 2005). As a consequence of the rule, the leeway over capital expenditures is larger. Therefore, it is likely that, if a political cycle is to be observed in French municipalities, the cycle will be more important in capital expenditures (e.g. public work procurement as it is the case in this paper) than in operational expenditures.

An important feature of the French system is the so-called "*cumul des mandat*", i.e. the fact that French elected representatives often hold more than one office at the same time (François, 2006; Bach, 2009; Navarro et al., 2013). According to Rouban (2012), 87.3% of the members of parliament that were elected in 2007 held at least one other mandate, the most frequent additional office held being mayor (for 22% of the MPs). For this, we can expect that legislative elections to have an incidence on policies at the municipality level. In particular, for electoral reasons, mayors who run for MPs may have an incentive to behave differently than those who do not.

The legislative elections (i.e. elections for members of parliament sitting in the National Assembly) are direct elections held every 5 years at the *circonscription* level, a subdivision of the *département*.[5,6] Each of the 577 *circonscriptions*, elects one MP for a mandate of 5 years according to a majority voting system in two rounds. During the first round, electors vote for one of the candidates. If one candidate gets 50% of the votes, representing 25% of all registered voters, he is elected. Otherwise, a second round is organised for those candidates who received more than 12.5% of the registered voters' votes. The candidate who gets the most votes during this second round wins the election.

Table 1 shows some statistics on the 2007 legislative election both at the *circon-*

[5]France has a bicameral system. Legislative power is divided between the Senate and the National Assembly. The 348 members of the former are elected indirectly (i.e. by local or regional representatives) for 6 years with elections held every three years to replace half of the senators. These elections are held at the *département* level and the number of senators per *département* is a function of its population (currently between 1 and 12 senators). *Départements* are administrative divisions of the French territory. There are currently 101 French *départements* including 5 located in overseas territories.

[6]The number of *circonscriptions* per *département* is also a function of its population.

scription and at the municipal levels.[7] There were, on average, 76,298 voters per *circonscription*. The average number of municipalities per *circonscription* is 64. While a large majority of municipalities belong to a sole *circonscription*, 86 municipalities were split between several *circonscriptions*. This is mainly the case for large municipalities (there were, on average, 77,916.81 voters in municipalities that were split into several *circonscriptions*, while the average number of voters for municipalities that were not splitter into several *circonscriptions* was 1,007.06), with the largest city Paris being split up into 21 different *circonscriptions*.

[Table 1 here]

There were 7587 candidates for the 571 *circonscriptions*, coming from more than fifteen different political parties.[8] On average, each *circonscription* had in excess of 13 candidates, with a minimum of 6 and a maximum of 20.

4 Data and Empirical Strategy

4.1 Data

For our empirical analysis we use information on public work contracts awarded by French municipalities between 2005 and 2007. Our data come from the "*Observatoire Economique de l'Achat Public*", a department of the French Ministry of Economy. The sample contains the full set of public work contracts over 90,000€ awarded by French municipalities.[9]

To identify municipalities where the mayor ran for MP in 2007, we complete our data on public procurement with information on the 2007 legislative election, collected from the website of the Ministry of Home Affairs.[10] We restrict our sample to municipalities that belong to a unique *circonscription*. This is mainly due to the fact that we do not observe the spatial location of a given project. Therefore, we cannot allocate the projects to the 'correct' *circonscription* within a municipality.

Table 2 describes our data and provides summary statistics both at the contract and

[7] Statistics reported in this subsection were computed using a dataset on the results of the 2007 legislative election taken from data.gouv.fr, the official French open data website. Although there were 577 *circonscriptions* overall, in the following we report statistics on 571 *circonscriptions* due to missing information (at the municipal level) on 6 overseas *circonscriptions*.

[8] Source: http://www.cnccfp.fr/index.php?art=783#P1T2ch2_2.

[9] The 90,000€ threshold corresponds to the obligation, for public buyers, to publish the call for tenders at the national level. Contracts below this threshold are not collected by the "*Observatoire Economique de l'Achat Public*."

[10] See `http://www.interieur.gouv.fr/Elections/Lesresultats/Legislatives/elecresult__legislatives_2007/(path)/legislatives2007/index.html`.

the municipal level. In our final sample we observe 1,634 municipalities from 2005 to 2007[11], which procured a total of 13,262 contracts. In 12% municipalities in our sample, mayors were candidates for the 2007 election and were responsible for slightly more than 20% of the tendered contracts between 2005 and 2007. This gap is mainly due to the fact that mayors who run for legislative elections often come from larger cities. Contracts have a mean duration of slightly more than 9 months. Unsurprisingly, most contracts are tendered through open auctions (about two thirds of the contracts), while negotiations account for 15.7% of contracts awarded in this period.

We aggregate the information at the contract level to the municipality level in the following way. $Y2006$, $Y2007$, and $Y2008$ measure the share of contracts (in terms of value) that started in a municipality in each year between 2005 and 2007 and are expected to terminate in 2006 (the variable $Y2006$), 2007 (the variable $Y2007$), or 2008 (the variable $Y2008$). If a municipality's contracts concluded in 2006 are all expected to terminate in 2006, then our share variables will take on value 1 for $Y2006$, and 0 for $Y2007$ and $Y2008$. As we observe contracts starting in 2005 to 2007, we ended up with about 3 observations per municipality.[12] In a similar way, contracts started in a given year cannot be expected to terminate in the years preceding their signature. Therefore, we consider the share of contracts signed in 2007 expected to terminate in 2006 (our $Y2006$ variable) as undefined. Because of this, there is a lower number of observations for our variable $Y2006$. On the other hand, the share of contracts that terminated in 2007 or 2008 may have started in 2005, 2006 or 2007. This is reflected in a higher number of observations for these variables.

[Table 2 here]

In our data we do not observe the exact date at which contracts were procured. In other words, we know in which year a given contract was awarded but we have no information on the precise day or month. This is likely to be an important drawback given that we are primarily interested in looking at the timing of the procured contracts for the 2007 election. In order to circumvent this obstacle, we use three different assumptions to approximate the starting date of projects. Our default assumption, which is used to report the descriptive statistics shown in Table 2, postulates that, on average, all projects in a given year will start in the middle of that year (i.e. all projects start in July). In that case, any project with a duration equal to or lower than 5 months will finish in the same year it started. Projects with a duration between 6 and 17 months will terminate in the following year etc. To test the robustness of our results, we also use two alternative assumptions. Our first alternative assumption postulates that all contracts in a given year will start in January, while our second assumption assumes that all contracts start in December. In the former (latter) case, all contracts with duration equal to or lower than 11 months (0 month) will terminate in the same year it started

[11] At the municipality level, this yields a total number of 3,830 municipality-year observations.

[12] Since some municipalities do not procure public work in all 3 years between 2005 and 2007, we actually ended up with less than 3 observations per municipality.

and contracts with duration between 12 and 23 months (1 and 11 months) will end in the following year, and so forth.[13] We further discuss this issue in the next subsection.

4.2 Empirical strategy

In order to analyse the potential effect of the French legislative elections on public procurement we compare contracts between municipalities with mayors running for MP and those with mayors not running for MP. The most basic prediction from the PBC literature is that politicians seeking re-election will try to signal good performance before an election. Mayors running for legislative elections will tend to start projects early enough so that they can be delivered before the election (in expectation). In the absence of an election, we would not expect a strategic manipulation of the delivery date of a project and the non-candidate municipalities therefore serve as the control group.

The main idea of the empirical strategy is that contracts written by candidate municipalities in 2005 and 2006 should exhibit a higher probability of finishing in 2007 than non-candidate municipalities. Consequently, the basic relationship that we are estimating is represented by the following linear probability model:

$$\begin{aligned} finish\_2007_{jit} = & start\_2005_t + start\_2006_t + \beta_{2005}(start\_2005_t \times Cand_i) + \\ & \beta_{2006}(start\_2006_t \times Cand_i) + Controls'_{jit}\gamma + \alpha_i + \epsilon_{jit} \end{aligned} \quad (1)$$

with α_i corresponds to municipality fixed-effects, and $start\_2005_t$ and $start\_2006_t$ correspond to time fixed effects for the contract starting in years 2005 and 2006. $start\_2005_t \times Cand_i$ and $start\_2006_t \times Cand_i$ are the interaction terms between the time effects and $Cand$, the dummy indicator showing if a mayor is running for MP (=1) or not (=0). The dependent variable $finish_2007_{jit}$ represents a dummy indicating whether contract j of municipality i starting in year t finishes in 2007, the election year.

Our hypothesis of strategic timing of the delivery leads us to expect a positive effect for the interaction terms, indicating that a project started in 2005 and 2006 is more likely to be delivered in 2007 in municipalities where the mayor is candidate in the legislative election in 2007.

As for the controls, we consider a number of project and municipality specific variables. On the project level, we add contract duration (*duration*) and different award procedures used (*proced_1*, *proced_2* , *proced_3*, and *proced_4*).[14] On the municipal level, municipal debt per capita *debt* and total municipal revenues per capita *rev* are included. Because larger contracts are more important than smaller contracts, the regressions are weighted by the value of a contract.

[13] Note that since the minimum duration in our sample is one month, our second alternative assumption postulates that no contract starts and finishes in the same year.

[14] In the estimations *proced_4* is omitted to avoid perfect collinearity.

In addition to these controls and to account for unobserved heterogeneity between the two types of municipalities, e.g. candidate municipalities are substantially larger than non-candidate municipalities, all estimations will contain municipality fixed effects. Also for this reason we are using a linear probability model (LPM) instead of a Probit model in order to keep the fixed effects to account for municipal heterogeneity.[15] In addition to the municipality fixed effects we are using cluster robust standard errors (on the municipal level) to account for within municipality error correlation and heteroscedasticity in all estimations.

It is important to note that with our empirical strategy, identification of the election cycle effect therefore rests exclusively on the different probabilities to finish a contract in a certain year that we observe for candidate and non-candidate municipalities over time. Instead of considering the period when the contract is concluded (which is typically also the period when public expenditure increases), we consider the finalisation of the construction project as the relevant anchor.

In addition to this basic test, two additional models are estimated:

$$finish\_2006_{jit} = start\_year_t + (start\_year_t \times Cand_i)\beta + Controls_{jit}\gamma + \alpha_i + \epsilon_{jit} \tag{2}$$

$$finish\_2008_{jit} = start\_year_t + (start\_year_t \times Cand_i)\beta + Controls_{jit}\gamma + \alpha_i + \epsilon_{jit} \tag{3}$$

with $finish\_2006_{jit}$ and $finish\_2008_{jit}$ representing a dummy that indicates if a project finished in 2006 and 2008. These additional specifications are helpful to show that the previous findings are actually the result of the political cycle. The potential problem of $finish\_2007_{jit}$ lies in the fact that the French legislative elections take place already in June 2007 and it is therefore unclear if projects finished in 2007 are early enough. In order to be sure that politicians try to finish projects before the election, the estimations with $finish\_2006_{jit}$ should again show a positive, if maybe somewhat smaller, interaction effect.[16]

Regarding $finish\_2008_{jit}$, in contrast to 2006 and 2007 the regressions should show an insignificant or negative coefficient for β. We would typically expect a non significant interaction term for the regressions with $finish\_2008_{jit}$ as the dependent variable because both types of municipalities have the same incentive to target contract termination into 2008. A negative effect may arise, however, if those contracts directed towards 2007 by candidate municipalities are at the expense of contracts that finish in 2008. The regressions using $finish\_2008_{jit}$ can also be interpreted as a placebo test where a positive significant finding would shed doubt about the difference-in-difference strategy that we

[15] There is no consistent estimator for unconditional fixed effects Psrobit model.

[16] As contracts from 2007 cannot possibly be finished in 2006, all those contracts are excluded when using $finish\_2006_{jit}$ as the dependent variable in order to avoid artificial results driven by zero inflated observations.

are applying. For instance, finding a consecutively higher β for candidate municipalities may simply mean that they have a longer average duration of contracts or a different trend than non-candidate cities.

As discussed in Section 4, a problem of the database is that we do not know the exact date when the contract was signed and therefore there is some uncertainty regarding the finalisation of a project. The default choice we are using is to assume that contracts are uniformly distributed over the year and hence the average project start will be just between June and July. As a consequence of this arguably naive assumption, durations between 1 and 5 months will finish in the current year, 6 to 17 will finish in the consecutive year and so forth. To gauge how sensitive our results are to this assumption, we will use different assumptions about the average starting date that is used to calculate the $finish\_year$. More specifically, we consider our results assuming that all contracts start in January or in December. In the former case, any duration between 1 and 11 months will lead us to consider that the project is expected to finish in the current year. In the latter case, no contract can by definition finish in the same year, since the minimum duration in the database is 1 month. This is a fairly extreme assumption.[17]

Finally, given the underlying nature of our data, where the candidate variable does not vary across contracts within a municipality, we also consider estimations on the municipal level. While we lose some information in the process of aggregating the information at the project level to the municipal level, the main advantage is that it allows us also to consider cases where municipalities do not start contracts every year. Ignoring this information could potential lead to a form of selection bias, where municipalities, who do not start a project, self-select out of the sample. Therefore, when aggregating the data to the municipal level all the cases where municipalities did not procure a contract in a given year were taken into account and coded as yielding a 0 contract value.

$$Y2007_{it} = \eta_t + (\eta_t \times Cand_i)\beta + \alpha_i + \epsilon_{it} \tag{4}$$

with α_i and η_t corresponding to municipality fixed effects and time fixed effects for the contract starting years 2005, 2006 and 2007. Again, $\eta_t \times Cand_i$ is the interaction between the time effects and the candidate dummy. The dependent variable $Y2007_{it}$ represents the share of expenditures by municipality i in year t that finishes in 2007, the election year. Our hypothesis of strategically timing project to be delivered close to

[17] In more technical terms, our basic assumption on the expected delivery date of a project considers the following formulation:

$$finish\_year = round(start\_year + (duration/12))$$

The two extreme alternative assumptions to rounding the expression $start\_year + (duration/12)$ is to use the floor and the ceil functions:

$$finish\_year = floor(start\_year + (duration/12))$$
$$finish\_year = ceil(start\_year + (duration/12))$$

election period would lead us to expect a positive interaction effect, indicating that the share of contracts stemming from 2005 and 2006 that end in 2007 is larger for candidate municipalities.

To check for the robustness of the results, the corresponding models for the years 2006 and 2008 are also estimated:

$$Y2006_{it} = \eta_t + (\eta_t \times Cand_i)\beta + \alpha_i + \epsilon_{it} \quad (5)$$

$$Y2008_{it} = \eta_t + (\eta_t \times Cand_i)\beta + \alpha_i + \epsilon_{it} \quad (6)$$

As before, we would expect the interaction term in 2006 and 2007 to be positive and negative or non-significant in 2008.

5 Results

The results from our baseline models in equations 4, 5, 6 for 2006, 2007 and 2008 are exhibited in Table 3. The model of main interest is displayed in Column 2 and tests whether the probability that a contract signed in 2005 and 2006 and finishes in 2007 is different for candidate and non-candidate municipalities. The coefficients on the overall time dummies is negative for 2005 and positive for 2006 (η_{2005} and η_{2006}), which shows that most contracts that finish in 2007 have been launched already before in 2006. This result shows already that analyzing the finishing dates instead of starting dates may lead to very different conclusions about the investment dynamics before elections. More to the point, the interaction terms indicate that there is a significant difference between the two groups of municipalities. Compared to non-candidate municipalities, contracts from municipalities where the mayor runs for MP have a higher probability to finish in 2007. The size of the effect is substantial and suggests that in candidate municipalities the probability for a contract signed in 2005 and 2006 to finish in 2007 is 24% and 20% higher, respectively.

[Table 3 here]

Looking at the differences between candidates and non-candidates in other years, the results are as expected. There is evidence that contracts written in 2005 from candidate municipalities have a higher probability to finish in 2006 than when a non-running mayor is present, at approximatively 13%. On top of that, the pre-election differences we find disappear when looking at projects finishing in 2008 (Model 3). Here the probability of finishing in 2008 coming from previous years 2005 and 2006 is negative but not statistically significant. These two results lend further credibility to the idea that projects are more likely to be delivered (in expectations) close to election period in municipalities where a mayor ran as a candidate in the legislative election in 2007.

Without going into detail for the covariates, it should be stressed that their inclusion in our specification has little effect on the results. This is shown in 4 where the models from Table 3 have been re-estimated without covariates. The coefficient estimates are basically unchanged and standard errors are only slightly higher, which is to be expected as the covariates increase the precision of the estimates by capturing some of the noise in the data.

[Table 4 here]

As noted before, an important sensitivity test relates to the assumption of when the contracts start during the year, which also affects the expected finishing year. Tables 3 and 4 were estimated under the assumption that all contracts begin in the middle of the year (July) and therefore only contracts with a duration of less than 6 months are finished in the same year. While without further information this is probably the most reasonable assumption in terms of expected timing of the start of the contract, alternative assumptions and their effect on the results should be considered.

The two polar assumptions of all contracts beginning in January (floor function) and all contracts beginning in December (ceil function) are displayed in Tables 5 and 6. If we assume all contracts start in January and therefore any contract with a duration of less than 12 months finishes in the current year, we still observe a significant difference between candidate and non-candidate municipalities. The results are even more pronounced than in Table 3. Contracts signed in 2005 and 2006 from mayors running for MP are 22% and 26% more likely to finish in 2007, respectively. Moreover, there is no statistically significant difference in 2008, after the election. Applying the floor function also strongly increases the estimated effect of the interaction term in the first column of Table 5, which is also consistent with the idea of targeting project finalisation before the election.

[Table 5 here]

[Table 6 here]

When using the ceil function, all contracts are assumed to start in December, the difference between contracts from candidates and non-candidates reduces strongly. Most importantly, however, also our the placebo test in Column three of Table 6 fails. Hence if we assume that all contracts written in t finish later than t (all contracts have a duration of at least 1 month), we find a significant and positive interaction term in 2008, where we should find (a negative or) a non significant effect. It is therefore clear that the assumption of all contracts starting in December is too strong.

To see how far we can go with the assumption of when the contracts were written, we successively reduced the months until the placebo test for 2008 is satisfied. As these tests show, our results are robust so long as we assumed projects started in any months

before October. Should all projects start after in October, or after, our baseline results do not hold anymore.[18] The evolution of the coefficients for the $finish\_2007$ regressions under different assumptions of when contracts are written is shown in Table 7.

[Table 7 here]

The bottom line of this robustness exercise is that our results hold for a wide range of assumptions regarding the starting date of the contracts. Without further information our best guess remains to assume that contracts are equally distributed over the year and therefore start on average in June or July. As the previous regressions show, however, the results are still consistent with the assumption that the contracts have been written at some point between January and October during a year. Our baseline results will fail to hold as soon as we assume that all contracts were written in the last three months of the year.

Finally, we analyse the hypothesis of strategic manipulation of expected project delivery before an election at the municipal level. Instead of the probability that a contract finishes in a given year, we use the share of contracts in terms of value that will finish in year t as our dependent variable. The estimations for these models are exhibited in Tables 8. [Table 8 here]

Investigating at the municipal level, the results in Table 8 strongly support our previous results at the contract level. The share of public work procurement contract value that terminates both in 2006 and 2007 are substantially higher for candidate municipalities. On top of that, the results for 2008 in Model 27 suggest again that the documented effect relates to elections, and does not arise due to some other reasons specific to the municipalities where there were candidates during the legislative elections, because there is no statistically significant difference between the two types of municipalities after the election.

6 Discussion

In the previous section, we have provided evidence that there is an electoral cycle in French construction public procurement. Specifically, the uncovered patterns suggest that politicians tend to favor projects which are expected to be delivered before elections. A remaining major issue is the motivation behind this behavior. Indeed, several stories of the political cycle literature are consistent with our findings.

On the one hand, a frequent and recurring explanation for the existence of political

[18] The assumption was formalised as follows: $finish\_year = ceil(start\_year + ((duration - X)/12))$ where X is the number of months. The assumption of all contracts written in December equals $X = 0$ and October would be $X = 2$.

budget cycle relates to salience. Systematic differences in visibility between various types of public expenditure are at the heart of many theoretical models of the political budget cycle. For instance in his early contribution, Rogoff (1990) argues that some types of spending are more visible to the electorate because their benefits are more immediately visible. This feature is a source of asymmetric information between politicians and voters and therefore creates a systematic bias towards more visible spending in order to signal higher competence.

On the other hand, and in line with the literature on white elephants, electoral cycles may arise in order to target spending towards specific groups of the electorate. Drazen and Eslava (2010) considers such a pork-barrel type of explanation, where a cycle arises because of a politician's attempt to win elections through targeted expenditure. Building on a probabilistic voting model, this model predicts that spending will be tailored towards expenditure types that can be targeted easily towards swing voters. This perspective contrasts with that of Rogoff (1990), since politicians are not trying to signal competency but to buy votes through pork-barrel spending.

We run a set of additional tests in order to explore both avenues of explanation. First, the visibility argument would predict a compositional change in the type of public projects. Importantly, we would expect to find that the cycle effects are driven by 'visible' projects. As suggested by the literature on electoral cycles on the sub-national level (see Kneebone and McKenzie (2001), Veiga and Veiga (2007), Khemani (2004)), we classify projects as visible if they involve streets or various types of public buildings (sports, recreational, schooling, and social). In contrast, infrastructure projects, which like water or telecommunications are typically subsurface, and various other types of construction works (e.g. housing, isolation/insulation) are therefore considered as non-visible. A detailed list of the spending categories considered as visible is given in Table 9.

[Table 9 here]

Second, while different pork-barrel channels are conceivable, the most straightforward method would be to award public contracts to local enterprises. This may not only benefit local contractors but also create jobs locally. Hence, in order to test the pork-barrel idea, we have matched contract winners with firm register data. While we are able to match 11,808 contracts for which we know whether the winner is a local firm or not, our panel approach leaves us with 8414 observations for our estimations. In order to be consistent with pork-barrel as the origin of the electoral cycle, we expect that candidate mayors will attribute a higher share of contracts to local firms.

The results for these additional regressions are shown in Table 10. As before, for presentation purposes we focus on our baseline model with a dummy indicating whether a contract finishes in 2007 as the dependent variable. To ease comparison, column 1 replicates the baseline results. Columns 2 and 3 in Table 10 show the results when

we split the sample into visible and less visible types of projects. The coefficients on the interaction terms are larger for visible types of projects. Moreover, for less visible projects, the coefficients are not only smaller, but no longer statistically significant. Our results are therefore in line with the idea that the cycle is driven by politicians trying to signal competence by procurement more visible projects.

[Table 10 here]

It is important to note that this preliminary finding, while consistent with the visibility view, does not automatically rule out the pork-barrel story of electoral cycles. To explicitly address this second potential mechanism behind the cycle, we now distinguish whether projects are award to local or non-local firms. To account for the fact that we could match only about 90% of our dataset with firm register data, column 4 re-estimates the baseline model with all projects we were able to match. This serves as the benchmark for columns 5 and 6, which feature the estimates for local and non-local contract awards.

Comparing the coefficients on the interaction terms in Table 10, we find that the cycle effect appears more pronounced for projects awarded to local companies. While the coefficients on the reduced sample are generally somewhat higher, the estimates for local projects are substantially higher. Despite their substantially reduced sample size, the coefficients in the regressions using only locally awarded contracts are still highly statistically significant.

To summarize, the additional tests were unable to rule out either of the potential mechanisms behind the cycle. While this might seem unfortunate, it may simply suggest that in reality a combination of both is driving our results. The findings of this paper are therefore consistent with politicians that try to target visible spending towards swing voters.

7 Concluding remarks

In this paper, we sought to detect the presence of a political budget cycle by investigating data on public procurement in French municipalities. We use a difference-in-difference approach to compare public work procurement of municipalities whose mayor was a candidate in the 2007 legislative elections with those of municipalities whose mayor did not run in these elections. We find that the probability that a contract finishes before the elections in 2006 and 2007 is consistently and considerably larger in municipalities whose mayor also ran for legislation elections. We interpret these results as evidence that mayors running for elections privileged projects that will be completed before election takes place. We refer to this pattern as a red ribbon effect, thus emphasising the fact that mayors try to deliver the projects before elections and that the budget itself is

only a means to this end. This observation may help to explain a puzzling result in existing empirical literature on political budget cycle, namely political budget cycles are detected only in developing countries. Our results suggest that political budget cycle may also be present in developed countries, albeit in a different form. More specifically, in countries where institutional environment is stronger and monitoring of politicians is more elaborated, politicians may be more subtle in how they influence policy instrument to enhance their election perspectives. They may prefer some other means to signal their competence and/or their motivation than straightforward increases in consumption spending. In our case, our results suggest that the politicians rely on the timing of public work projects. In addition, while most of the empirical literature on political business cycles finds that cycles start one year before the election, we find evidence from a cycle that starts at least two years before the election date. The longer length of the cycle is due to the time that public work projects need to be completed.

We believe that our paper is the first study to consider the use of micro-data in public work procurement to understand political budget cycles, and to show that such cycles can arise in terms of timing of procurement projects instead of increased public spending. It also shows how politics may influence the conduct of public procurement and how this influence may arise at least two years prior to the election. We are further able to show that our results are consistent with two distinct theoretical channels: the projects affected by the cycle are both more visible and have a higher probability of being awarded to local firms.

Nevertheless, our analysis also raises several questions. In particular, we are unable to determine the consequences of the observed behaviour of candidate mayors: Are projects anticipated to finish in a certain period privileged over alternative (and, maybe more socially useful) ones by politicians? Do politicians delay and launch in advance projects that are to be implemented anyway so that their completion can coincide with electoral periods? What are the welfare consequences? Further research should thus focus on identifying the consequences of electoral cycles in public procurement.

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Appendix

Table 1: **Summary statistics on the 2007 election**

Variable	**N**	**Mean**	**Std. Dev.**	**Min**	**Max**
At the *circonscription* level					
Number of voters	571	76,298.14	16,896.76	22,318.00	163,122.00
Number of municipalities	571	64.52	63.80	1.00	344.00
Number of candidates	571	13.29	2.11	6.00	20.00
At the municipal level					
Number of voters in the complete sample of municipalities	36,693	1,187.32	8,350.35	8.00	1,247,925.00
Number of voters in municipalities with one *circonscription*	36,607	1,007.06	2,756.78	8.00	83,859.00
Number of voters in municipalities with more than one *circonscriptions*	86	77,916.81	144,416.30	245.00	1,247,925.00

Table 2: **Summary statistics on our dataset**

Variable	Definition	N	Mean	Std. Dev.	Min	Max
At the contract level						
$Cand$	Equals 1 if the mayor of the municipality is a candidate to the 2007 election, 0 otherwise	13,262	0.203	0.402	0	1
η_{2005}	Equals 1 if the contract started in 2005, 0 otherwise	13,262	0.384	0.486	0	1
η_{2006}	Equals 1 if the contract started in 2006, 0 otherwise	13,262	0.335	0.472	0	1
η_{2007}	Equals 1 if the contract started in 2007, 0 otherwise	13,262	0.280	0.449	0	1
$Y2006$	Equals 1 if the contract finished in 2006, 0 otherwise	13,262	0.340	0.474	0	1
$Y2007$	Equals 1 if the contract finished in 2007, 0 otherwise	13,262	0.304	0.460	0	1
$Y2008$	Equals 1 if the contract finished in 2008, 0 otherwise	13,262	0.150	0.357	0	1
$duration$	Duration of the contract (in months)	13,262	9.239	9.965	1	180
$proced\_1$	Equals 1 if the awarding procedure is an open auction, 0 otherwise	13,262	0.682	0.466	0	1
$proced\_2$	Equals 1 if the awarding procedure is a negotiated procedure, 0 otherwise	13,262	0.157	0.363	0	1
$proced\_3$	Equals 1 if the awarding procedure is an adapted procedure ($MAPA$), 0 otherwise	13,262	0.123	0.329	0	1
$proced\_4$	Equals 1 if the awarding procedure is another procedure, 0 otherwise	13,262	0.038	0.192	0	1
rev	Per capita revenue of a municipality (resources that a municipality can use) in a given year (in thousands of euros)	13,262	1.999	1.747	0.44	26.8
$debt$	Outstanding per capita debt of a municipality in a given year (in thousands of euros)	13,262	1.016	0.979	0	14.1
At the municipal level						
$Cand\_muni$	Equals 1 if the mayor of the municipality is a candidate to the 2007 election, 0 otherwise	3,830	0.120	0.325	0	1
η_{2005}_muni	Equals 1 if year is 2005, 0 otherwise	3,830	0.363	0.481	0	1
η_{2006}_muni	Equals 1 if year is 2006, 0 otherwise	3,830	0.351	0.477	0	1
η_{2007}_muni	Equals 1 if year is 2007, 0 otherwise	3,830	0.286	0.452	0	1
$Y2006\_muni$	Share of contract in a given year (in value) expected to terminate in 2007	2,734	0.485	0.431	0	1
$Y2007\_muni$	Share of contract in a given year (in value) expected to terminate in 2007	3,830	0.307	0.413	0	1
$Y2008\_muni$	Share of contract in a given year (in value) expected to terminate in 2008	3,830	0.137	0.307	0	1
rev_muni	Per capital revenue of a municipality (in thousands of euros)	3,830	1.745	1.404	0.44	26.8
$debt\_muni$	Per capital outstanding debt in a municipality (in thousands of euros)	3,830	0.888	0.866	0	14.1

Table 3: **Baseline regression results on the probability that a contract ends in a given year (Linear Probability Model)**

	Model 1 LPM *finish_2006*	Model 2 LPM *finish_2007*	Model 3 LPM *finish_2008*
η_{2005}	0.102***	-0.345***	-0.348***
	(0.030)	(0.033)	(0.027)
η_{2006}		0.088**	-0.314***
		(0.038)	(0.035)
$\eta_{2005} * Cand$	0.129**	0.235**	-0.055
	(0.058)	(0.094)	(0.069)
$\eta_{2006} * Cand$		0.195***	-0.050
		(0.068)	(0.071)
duration	-0.013***	-0.005***	0.001
	(0.001)	(0.001)	(0.001)
proced_1	0.015	-0.046	-0.047
	(0.058)	(0.048)	(0.049)
proced_2	0.019	-0.003	0.005
	(0.066)	(0.052)	(0.057)
proced_3	-0.053	-0.059	-0.129**
	(0.064)	(0.050)	(0.051)
rev	0.010	-0.015	-0.001
	(0.046)	(0.021)	(0.018)
debt	-0.061	-0.101	0.111
	(0.124)	(0.115)	(0.077)
_cons	0.539***	0.618***	0.328***
	(0.121)	(0.168)	(0.116)
R^2	0.4479	0.4251	0.4750
Adj. R^2	0.3337	0.3438	0.4007
Nb. obs.	9546	13262	13262

Note: Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 4: **Baseline regression results without covariates on the probability that a contract ends in a given year (Linear Probability Model)**

	Model 4 LPM *finish_2006*	Model 5 LPM *finish_2007*	Model 6 LPM *finish_2008*
η_{2005}	0.084***	-0.328***	-0.364***
	(0.031)	(0.030)	(0.026)
η_{2006}		0.110***	-0.325***
		(0.039)	(0.034)
$\eta_{2005} * Cand$	0.124*	0.246**	-0.073
	(0.065)	(0.120)	(0.085)
$\eta_{2006} * Cand$		0.208**	-0.061
		(0.083)	(0.079)
_cons	0.355***	0.347***	0.440***
	(0.015)	(0.023)	(0.019)
R^2	0.3925	0.4074	0.4679
Adj. R^2	0.2674	0.3239	0.3929
Nb. obs.	9546	13262	13262

Note: Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 5: **Sensitivity analysis assuming all contracts are started in January (floor function) on the probability that a contract ends in a given year (Linear Probability Model)**

	Model 7 LPM *finish_2006*	Model 8 LPM *finish_2007*	Model 9 LPM *finish_2008*
η_{2005}	-0.466***	-0.650***	-0.142***
	(0.034)	(0.036)	(0.024)
η_{2006}		-0.428***	-0.148***
		(0.036)	(0.030)
$\eta_{2005} * Cand$	0.325***	0.219**	-0.060
	(0.100)	(0.109)	(0.069)
$\eta_{2006} * Cand$		0.256***	-0.015
		(0.084)	(0.063)
duration	-0.009***	-0.005***	0.003***
	(0.001)	(0.001)	(0.001)
proced_1	-0.015	-0.027	-0.040
	(0.055)	(0.053)	(0.037)
proced_2	-0.020	0.006	-0.006
	(0.060)	(0.065)	(0.041)
proced_3	-0.009	-0.067	-0.053
	(0.064)	(0.056)	(0.037)
rev	0.018	-0.028	-0.003
	(0.051)	(0.024)	(0.015)
debt	-0.079	-0.103	0.103
	(0.144)	(0.140)	(0.087)
_cons	0.814***	0.900***	0.098
	(0.117)	(0.206)	(0.129)
R^2	0.5264	0.5175	0.4576
Adj. R^2	0.4284	0.4493	0.3809
Nb. obs.	9546	13262	13262

Note: Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 6: **Sensitivity analysis assuming that all contracts are started in December (ceil function) and the probability that a contract ends in a given year (Linear Probability Model)**

	Model 10 LPM *finish_2006*	Model 11 LPM *finish_2007*	Model 12 LPM *finish_2008*
η_{2005}	0.789***	0.134***	-0.698***
	(0.021)	(0.023)	(0.033)
η_{2006}		0.755***	-0.541***
		(0.026)	(0.036)
$\eta_{2005} * Cand$	-0.148***	0.135**	0.135*
	(0.053)	(0.065)	(0.070)
$\eta_{2006} * Cand$		-0.072	0.134*
		(0.065)	(0.072)
duration	-0.015***	-0.004***	-0.003***
	(0.002)	(0.001)	(0.001)
proced_1	0.014	-0.023	-0.021
	(0.047)	(0.045)	(0.045)
proced_2	0.013	-0.017	-0.021
	(0.052)	(0.050)	(0.057)
proced_3	0.003	-0.008	-0.059
	(0.048)	(0.046)	(0.048)
rev	0.037	-0.018	-0.018
	(0.030)	(0.014)	(0.019)
debt	-0.074	-0.098	-0.009
	(0.079)	(0.068)	(0.073)
_cons	0.141*	0.226**	0.826***
	(0.074)	(0.107)	(0.110)
R^2	0.8197	0.6393	0.5744
Adj. R^2	0.7824	0.5883	0.5142
Nb. obs.	9546	13262	13262

Note: Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 7: **Sensitivity analysis assuming different starting months**

	Model 13 LPM *finish_2007*	Model 14 LPM *finish_2007*	Model 15 LPM *finish_2007*	Model 16 LPM *finish_2007*	Model 17 LPM *finish_2007*	Model 18 LPM *finish_2007*	Model 19 LPM *finish_2007*	Model 20 LPM *finish_2007*	Model 21 LPM *finish_2007*	Model 22 LPM *finish_2007*	Model 23 LPM *finish_2007*	Model 24 LPM *finish_2007*
η_{2005}	-0.650***	-0.612***	-0.579***	-0.544***	-0.491***	-0.449***	-0.345***	-0.266***	-0.145***	-0.024	0.062**	0.134***
	(0.036)	(0.036)	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)	(0.030)	(0.028)	(0.026)	(0.025)	(0.023)
η_{2006}	-0.428***	-0.372***	-0.296***	-0.233***	-0.131***	-0.045	0.088**	0.229***	0.396***	0.587***	0.682***	0.755***
	(0.036)	(0.037)	(0.037)	(0.036)	(0.039)	(0.041)	(0.038)	(0.036)	(0.034)	(0.033)	(0.032)	(0.026)
$\eta_{2005} * Cand$	0.219**	0.214**	0.234**	0.253**	0.232**	0.220**	0.235**	0.203**	0.212**	0.158**	0.116	0.135**
	(0.109)	(0.108)	(0.102)	(0.103)	(0.102)	(0.101)	(0.094)	(0.090)	(0.086)	(0.077)	(0.073)	(0.065)
$\eta_{2006} * Cand$	0.256***	0.245***	0.253***	0.265***	0.214***	0.190**	0.195***	0.119*	0.074	-0.033	-0.095	-0.072
	(0.084)	(0.083)	(0.082)	(0.078)	(0.074)	(0.074)	(0.068)	(0.066)	(0.067)	(0.064)	(0.066)	(0.065)
duration	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
proced_1	-0.027	-0.045	-0.045	-0.047	-0.073*	-0.044	-0.046	-0.042	-0.031	-0.019	-0.029	-0.023
	(0.053)	(0.053)	(0.043)	(0.042)	(0.042)	(0.043)	(0.048)	(0.047)	(0.048)	(0.048)	(0.045)	(0.045)
proced_2	0.006	-0.009	-0.002	-0.004	-0.026	0.007	-0.003	0.012	-0.003	-0.003	-0.015	-0.017
	(0.065)	(0.065)	(0.049)	(0.048)	(0.048)	(0.049)	(0.052)	(0.051)	(0.052)	(0.053)	(0.051)	(0.050)
proced_3	-0.067	-0.086	-0.085*	-0.091**	-0.120***	-0.081*	-0.059	-0.054	-0.035	-0.032	-0.014	-0.008
	(0.056)	(0.055)	(0.044)	(0.043)	(0.043)	(0.044)	(0.050)	(0.049)	(0.050)	(0.049)	(0.047)	(0.046)
rev	-0.028	-0.026	-0.026	-0.025	-0.018	-0.014	-0.015	-0.014	-0.009	-0.016	-0.019	-0.018
	(0.024)	(0.024)	(0.022)	(0.023)	(0.023)	(0.022)	(0.021)	(0.020)	(0.021)	(0.019)	(0.017)	(0.014)
debt	-0.103	-0.082	-0.081	-0.093	-0.090	-0.123	-0.101	-0.090	-0.089	-0.105	-0.110	-0.098
	(0.140)	(0.139)	(0.133)	(0.134)	(0.132)	(0.125)	(0.115)	(0.111)	(0.097)	(0.086)	(0.075)	(0.068)
_cons	0.900***	0.853***	0.817***	0.793***	0.760***	0.723***	0.618***	0.525***	0.410***	0.342***	0.305**	0.226**
	(0.206)	(0.204)	(0.192)	(0.193)	(0.190)	(0.182)	(0.168)	(0.162)	(0.144)	(0.132)	(0.119)	(0.107)
Placebo test rejects	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
R^2	0.5175	0.4972	0.4823	0.4634	0.4481	0.4376	0.4251	0.4416	0.4705	0.5438	0.5906	0.6393
Adj. R^2	0.4493	0.4261	0.4091	0.3876	0.3700	0.3580	0.3438	0.3626	0.3956	0.4793	0.5327	0.5883
Nb. obs.	13262	13262	13262	13262	13262	13262	13262	13262	13262	13262	13262	13262

Note: Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 8: **Share of contract (in value terms) in a municipality terminating in pre and post legislative election years and mayors' candidacy in the legislative elections**

	Model 25 Y2006	Model 26 Y2007	Model 27 Y2008
η_{2005}	-0.078***	-0.478***	-0.367***
	(0.021)	(0.017)	(0.015)
η_{2006}		-0.097***	-0.359***
		(0.020)	(0.015)
$\eta_{2005} * Cand$	0.121**	0.166***	-0.031
	(0.048)	(0.042)	(0.040)
$\eta_{2005} * Cand$		0.203***	-0.035
		(0.051)	(0.039)
debt_muni	-0.158	-0.001	0.004
	(0.108)	(0.034)	(0.029)
rev_muni	0.014	-0.011	-0.004
	(0.040)	(0.012)	(0.011)
R^2	0.015	0.330	0.376
Adj. R^2	-0.977	-0.172	-0.092
Nb. obs.	2200	3830	3830

Note: The LSDV (Least Square Dummy Variables) is used. Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.

Table 9: **Projects considered as visible**

Public Buildings:
Sport facilities (swimming pools, stadiums, tennis courts, boating centres, tracks, boules pitches, etc.)
Cultural buildings (theatres, (media) libraries, concert halls, museums, village halls, etc.)
Schools (preschools, elementary and secondary schools, high schools)
Religious buildings (chapels, cathedral, churches, etc.)
Various buildings (community centres, hospitals and health related centres, day-nurseries, etc.)
Street Work:
Street work (roads, squares, crossroads, road signs, etc.)
Civil engineering (bridges and tunnels)

Table 10: **Visibility and pork-barrel channels**

	Model 28 Y2007 All projects	Model 29 Y2007 Visible	Model 30 Y2007 Non-Visible	Model 31 Y2007 All Projects	Model 32 Y2007 Local	Model 33 Y2007 Non-Local
η_{2005}	-0.345***	-0.410***	-0.288***	-0.384***	-0.577***	-0.386***
	(0.033)	(0.021)	(0.052)	(0.033)	(0.083)	(0.036)
η_{2006}	0.088**	0.053	0.090*	0.012	-0.185*	-0.010
	(0.038)	(0.059)	(0.053)	(0.046)	(0.107)	(0.049)
$\eta_{2005} * Cand$	0.235**	0.267*	0.118	0.290***	0.523***	0.270**
	(0.094)	(0.137)	(0.086)	(0.109)	(0.178)	(0.117)
$\eta_{2006} * Cand$	0.195***	0.304**	0.089	0.324***	0.578***	0.306***
	(0.068)	(0.120)	(0.088)	(0.076)	(0.188)	(0.087)
duration	-0.005***	-0.003***	-0.007***	-0.007***	-0.005*	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
proced_1	-0.046	0.041	-0.078	-0.153**	0.082	-0.175**
	(0.048)	(0.065)	(0.060)	(0.066)	(0.189)	(0.070)
proced_2	-0.003	0.133*	-0.062	-0.126*	0.154	-0.144**
	(0.052)	(0.077)	(0.67)	(0.068)	(0.242)	(0.072)
proced_3	-0.059	0.055	-0.074	-0.135**	0.075	-0.158**
	(0.050)	(0.073)	(0.630)	(0.065)	(0.210)	(0.070)
debt_muni	-0.015	-0.330*	0.071	-0.166	0.087	-0.191
	(0.021)	(0.179)	(0.070)	(0.146)	(0.166)	(0.161)
rev_muni	-0.101	-0.046	0.021	-0.018	-0.005	-0.023
	(0.115)	(0.031)	(0.014)	(0.026)	(0.053)	(0.029)
R^2	0.4252	0.5160	0.4612	0.4216	0.4840	0.4348
Adj. R^2	0.3438	0.4277	0.3610	0.3183	0.3230	0.3267
Nb. obs.	13262	5719	5470	8414	531	7626

Note: The LSDV (Least Square Dummy Variables) is used. Cluster robust standard errors in parentheses. Significance stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions weighted by the value of the contract.