The Political Cycle of Public-Private Contract Renegotiations:

Evidence from the French car park sector

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Abstract

Recent research in contract theory suggests that public-private and private-private agreements are inherently different. This paper studies empirically the intrinsic differences between these two types of contracting. We focus in particular on the different impact local elections have on the execution of public-private and of private-private agreements. In order to do so, we investigate the occurrence of renegotiations of each type of contract prior to local elections. We believe that, as public-private contracts belong to the public sphere, their renegotiations should be affected by the electoral calendar, while renegotiations of private-private contracts should not. To test this, we use an original dataset comprising every renegotiation of the exhaustive set of public-private and private-private contracts signed by the French car park leader between 1968 and 2008. We use a difference-in-difference methodology to show that, compared with private-private contract renegotiations, public-private renegotiations significantly increase before local elections. In particular, renegotiations aiming at modifying the tariffs or the financial side of the contract (*i.e.* the remuneration of one of the parties) increase before an election, whereas all other types of renegotiation do not. Possible explanations for these results are considered.

Keywords: *Public-private agreements, Contracts, Renegotiations, Political cycle, Car parks.*

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1. INTRODUCTION

Private sector participation in public investment has dramatically increased in the past decades: local and national governments have more and more contracted the construction and/or exploitation of public infrastructures out to private operators through public-private contracting. Besides the benefits from competition, bringing the private party into sectors in need of large investments may be a solution when public funds are rare or devoted to other urgent investments, for instance in the social sectors (Guasch et al. 2008).

This upsurge of public-private contracting gave birth to a new strand of literature in contract theory, which investigates the inherent differences between public-private and private-private contracts. Spiller (2008) argues that public-private contracts differ from private-private contracts because of their public nature: as soon as a public authority is part of a contract, the latter is subject to additional kinds of opportunism, namely governmental and third-party opportunism. This may explain why public-private contracting is generally perceived as less flexible and *"requiring more frequent formal renegotiations"*. Yet, in spite of a large literature on each type of contracting, Spiller asserts that there is a lack of studies comparing public-private and private-private contracts on such issues.

This paper is among the first empirical works to investigate the intrinsic differences between public-private and private-private contracts. In order to do so, we focus on electoral cycles: we explore how the timing of local elections differently affects the relationships between the contracting parties in each type of contract. We believe that, because public-private contracts belong to the public sphere, their execution should be affected by elections (muller1989public). Incumbent governments could indeed try to impact economic outcomes prior to elections in order to influence the voters, using their public-private contracts. We choose to focus on contractual amendments in order to capture the impact of elections on the

execution of each type of contract. This choice is motivated by contract theories, which suggest that renegotiations stand at the core of the relationship between the contracting parties (Williamson, 1979; Hart and Moore, 1988). Moreover, studying public-private contracts, Guasch et al. (2003) highlight that *"political cycles are likely to have consequences on the occurrence of renegotiations"*. By contrast, private-private contracts are concluded between private partners who do not have electoral concerns, and thus should not be affected by the electoral calendar. Consequently, we believe that as local elections approach, the number of public-private renegotiations may increase, whereas the number private-private renegotiations may not.

Susarla (2012) notes that research on contract renegotiations has been hampered by the lack of appropriate data. Our study relies upon an original and exhaustive dataset comprising every renegotiation from the entire set of contracts (public-private and private-private) signed by the French car park leader between 1968 and 2008. Our set of public-private contracts consists of every contract signed by this firm with a municipality, while we exploit the fact that this firm also concludes agreements for similar services (*i.e*. construction, exploitation or renovation of car parks) with private parties (for instance shopping centers, private pool complexes, or amusement parks) to constitute our set of private-private contracts. For each contract and for every year, we have information on the total number of renegotiations, and we know for each amendment which aspect of the contract was renegotiated (tariffs, work, finance and others).

We use a difference-in-difference methodology to show that public-private and private-private contracts are differently affected by electoral periods. Indeed, we find that compared with private-private contract renegotiations, public-private renegotiations significantly increase before local elections. In particular, public-private renegotiations aiming at modifying end user tariffs or the financial dimensions of the contracts (*i.e*. the remuneration of one of the parties) increase before an election, whereas all other renegotiations do not.

Our study offers several innovations and advantages compared with previous work. We are among the first to empirically study the differences between public-private and private-private contracts. We thus believe that our results contribute to the growing literature studying these differences. Moreover, to our knowledge, the impact of local political cycles on contract renegotiations has never been considered. We believe this investigation is all the more relevant given that most of public investment is made at the local level, often through public-

private agreements. Third, we circumvent the issue of the lack of available data on renegotiations by using an exhaustive dataset consisting of 650 public-private and private-private contracts with 1,110 amendments over a 50 years period. Finally, we test our results using a range of robustness checks including the use of placebo elections to confer credibility to the common trend assumption.

We believe that this paper contributes to a small but growing literature that compares public-private and private-private agreements. Moszoro et al. (2013) use data on firms in the United States to show that public-private contracts are more rigid than private-private contracts. Beuve et al. (2014) also explore this question using a similar dataset than ours, consisting of contracts signed by the leader of the French car park sector. They find that public-private contracts are more rigid than private-private contracts, and their renegotiation is formalized more frequently in amendments. Our paper differs from these two studies, because we propose not only to investigate the contractual differences between public-private and private-private agreements, but also the different impact local political cycles can have on the two types of contracting.

Section 2 presents the two strands of literature on renegotiations and political cycles; Section 3 provides details about the French car park sector and the data we use. This section also depicts the variables used in the empirical investigation. Section 4 presents the empirical methodology and our results. A last section discusses these results.

2. RENEGOTIATIONS AND THE LOCAL POLITICAL CYCLE

Renegotiations and local political cycles have been extensively studied in the literature. On the one hand, renegotiations stand at the core of the relationship between contracting parties, determining or reflecting the success of a transaction. On the other hand, local political cycles have been showed to have an impact on several local variables, such as budget, tax or investment decisions by municipalities. This section briefly presents these two strands of literature.

2.1. RENEGOTIATION

2.1.1. Sources and Consequences of Renegotiation

Contract renegotiations are one of the key determinants of the efficiency of transactions. The now substantial literature on renegotiations in various sectors and transaction types sheds light

on some crucial questions: why are contracts renegotiated? Do renegotiations harm the relationship between the contractors? Under which conditions can renegotiations improve the efficiency of transactions? How can the parties take into account this phenomenon in order to write more efficient arrangements? Renegotiations arise because contracts are incomplete, yet the transaction cost theory and the incomplete contract theory differ on their analysis of the sources of this incompleteness. According to the transaction cost theory (Williamson, 1979), contracts are incomplete because of the bounded rationality of the parties, who cannot foresee all possible future contingencies at the contracting stage. According to the incomplete contract theory,[1] the parties are unable to describe the states of the world in enough detail that third parties, in particular courts, can verify *ex post* which state has occurred. In other words, contracts are not enforceable. Consequently, regardless of the theoretical framework, parties have to revise the terms of the initial agreement as the states of nature realize.

Previous work has shown that the link between amendments and efficiency is double-edged. On the one hand, renegotiations can be costly for the parties, both because of the indirect and direct costs they induce. The main indirect cost induced by renegotiations is the risk of underinvestment in relation-specific assets, which may be due either to the fear of *ex post* opportunistic renegotiations where parties are rent-seeking (Klein et al., 1978; Williamson, 1979), or to the fact that information is unverifiable (Hart and Moore, 1988). Furthermore, *ex post* renegotiations induce significant direct adaptation costs (Bajari and Tadelis, 2001), namely renegotiation costs. These additional direct costs can result in less efficient transactions, because sellers anticipate them and increase their bids accordingly (Bajari et al., 2014). On the other hand, renegotiations can be Pareto-improving as they make up for the inherently incompleteness of the contract. They can indeed permit to adapt the terms of the contract to a changing environment and thus be mutually advantageous. In other words, renegotiations can enhance the quality of the transaction by incorporating contingencies revealed *ex post* (Masten and Saussier, 2000; Susarla, 2012). In this regard, Arino and Reuer (2004) explain that alliance contracts should be renegotiated if one of the partners' strategy or the environment changed in a way that significantly impacted the contract; and Beuve et al. (2013) find an optimal frequency of renegotiation in the French car park sector. Guasch (2004) summarizes this equivocal nature of renegotiations, arguing they are desirable when they address the incomplete nature of contracts, but can jeopardize the

[1] See *e.g.* the seminal studies of Grossman and Hart (1986), Hart and Moore(1988), Hart (1988, 1995), Tirole (1999) and Fares and Saussier (2002) on incomplete contract theory.

arrangement when they indicate opportunistic behaviors. As a consequence, Crocker and Masten (1991) demonstrate that the parties choose the optimal degree of contractual incompleteness at the designing stage of the contract. This optimal level results from a trade-off between marginal cost (resources spent in *ex ante* design) and marginal revenue (reduced potential for opportunistic *ex post* renegotiation) of the level of completeness of contracts (Crocker and Reynolds, 1993). Therefore, the parties should consider these issues when designing their contracts: they can write more efficient contracts if they anticipate this renegotiation phase, and include in the initial contract clauses that permit to monitor their bargaining power in case of renegotiation (Aghion et al., 1990; Green and Laffont, 1992). As soon as renegotiations can be anticipated through contractual mechanisms, the latter have to be studied with particular attention (Quélin, 2003; Laffont, 2003; Guasch, 2004; Guasch et al., 2006).

Since renegotiations are at the core of the success of transactions, it is essential to study their determinants. This is especially true concerning public-private contracting, where the efficiency of the transaction does not only affect the parties, but society as a whole. Public-private contracting, because of its "public" nature, is also subject to additional kinds of opportunism than private-private contracting – beyond the partners' opportunism, it is also exposed to governmental opportunism, and third-party opportunism (Spiller, 2008; Moszoro and Spiller, 2012) – which makes studying its renegotiations even more challenging.

2.1.2. Renegotiation of Public-Private Contracts

In the past decade, detailed work has been provided on the determinants of public-private renegotiations, using Latin America data. Engel et al. (2009) use Chilean data to show the government uses renegotiations in order to increase spendings and shift the burden of payments to future administrations. Guasch et al. (2003, 2008) use Latin America data and find that firm-led renegotiations of concession contracts are impacted by the regulatory policy, institutional features, economic shocks, and the characteristics of the contracts. They also found some relevant differences between the determinants of firm-led and government-led renegotiations (Guasch et al., 2007). For instance, the presence of investment requirements from the concessionaire in initial agreements, or the fact that the project is entirely financed by private funds have negative impacts on government-led renegotiations but positive impacts on firm-led renegotiations. These variables indeed affect the *statu quo*

payoffs of the parties. Guasch and Straub (2009) find that corruption has a positive impact on firm-led renegotiations, but a negative impact on government-led renegotiations. They argue that government-led renegotiations are less frequent in more corrupt environments where governments are able to strike *ex ante* agreements: they are then less eager to renegotiate these agreements *ex post*. The authors also find a significant impact of the national political cycle on firm-led renegotiations (Guasch et al., 2003) and government-led renegotiations (Guasch et al., 2007): they both significantly increase after national elections. Indeed, changes of political majority are considered as shocks for concessionaires, and freshly elected governments may offer to renegotiate past agreements.

This last strand of literature therefore establishes evidence that public-private contract renegotiations are subject to political cycles. This study will focus exclusively on that question, looking at local political cycles. To our knowledge, the impact of municipal elections on renegotiations has not been considered yet, despite the fact that most of the national investment is undertaken by local authorities: in France, municipalities are the first public investor, carrying out about 70% of total public investment, which represents 3% of French GDP[2]. We thus believe it is relevant to study the impact of municipal elections on public-private contract renegotiations. The question of the efficiency of these renegotiations, stimulated by the proximity of local elections, will not be formally addressed in this paper, but will be discussed in Section 5.

2.2. POLITICAL CYCLES

Political cycles have been extensively studied in the literature, both from a theoretical and an empirical point of view. The theoretical research showed that politicians have incentives to manipulate economic variables in order to enhance their reelection perspectives. Several empirical studies confirmed these predictions, both and the national and the local level.

2.2.1. National Political Cycles

The theoretical literature on political budget cycles has built upon different sets of assumptions to establish that policy makers have incentives to use economic policy to increase their reelection chances. The idea is that voters will base their electoral choice on

[2] Source: OECD

recently observed economic outcomes. First, the seminal work of Nordhaus (1975) shows that the political tradeoff between inflation and unemployment is impacted by policy makers' electoral concerns, if voters' expectations are backward looking. This last theoretical assumption being contrary to the paradigm of rational expectations, a more recent wave of literature reached the same conclusions with rational expectations and asymmetric information. Rogoff and Sibert (1988) and Rogoff (1990) build an adverse selection model to argue that efficient incumbent governments use taxes, spendings and money growth to signal their type to voters before elections. The assumption is then that voters are rational and forward looking, but imperfectly informed about the incumbent governments' competence level. Policy manipulation is used as a signal by the more competent governments to indicate their type to voters before elections. Persson and Tabellini (2002) and Shi and Svensson (2006) use the same assumptions within a moral hazard framework and show that incumbents still engage in pre-electoral policy manipulations before elections. These moral hazard models contrast with the adverse selection ones in the sense that all governments (*i.e.* high and low competence level) manipulate the budgets before elections. Martinez (2009) introduces politicians' reputation concerns in his model to explain why politicians have stronger incentives to influence election results when elections get closer. Finally, Baleiras and da Silva Costa (2004) construct a model of public budget cycles with ultra-rational agents and full symmetric information. Political cycles still arise when policy makers maximize a utility function which takes into account the income they could earn in the private sector in case of electoral defeat.

A large number of empirical studies has attempted to test these theoretical predictions. A first strand of literature investigated the occurrence of policy manipulation at the national level. In industrialized countries, some evidence of political budget cycles has been found, for instance by Tufte (1980) and Alesina et al. (1992). Tufte (1980) detects some fiscal manipulation before election years in the United States, and Alesina et al. (1992) use a sample of 18 OECD countries to assess some monetary policy manipulation in election years and some fiscal manipulation prior to elections. Political cycles have also been investigated in developing countries. Schuknecht (1996) studies a sample of 35 developing countries and finds evidence of pre-electoral expansionary fiscal policies. He then uses a sample of 24 developing economies over the period 1973-1992 to show this expansionary fiscal policy is mainly due to increased expenditures, rather than lowered taxes (Schuknecht, 2000). Kraemer

(1997) focuses on 21 countries in Latin America and the Caribbeans and finds that budget surpluses are lower in the pre-electoral periods. Gonzalez (2002) shows the Mexican government used public spending in infrastructure and government transfers to enhance its re-election prospects between 1957 and 1997. Finally, some empirical work has been provided to compare the political cycle in different economies. Shi and Svensson (2006) establish that political cycles are stronger and statistically more robust in developing than in developed countries, and Brender and Drazen (2005) find stronger political deficit cycle in newly established democracies.

2.2.2. Local Political Cycles

Since local authorities also have incentives to influence election results, and because a large part of the national investment is carried out by municipalities, another strand of literature has sought evidence of political cycles at the local level. Blais and Nadeau (1992) and Petry et al. (1999) find that electoral cycles impact the spendings of Canadian provinces. Blais and Nadeau (1992) thus measure an increase in total provincial spending in election years of a magnitude of 1%, resulting in higher deficits rather than tax increases. On the same subject, Baleiras and da Silva Costa (2004), and Veiga and Veiga (2007), assess the presence of pre-electoral increases in local expenditures before Portuguese municipal elections. Veiga and Veiga (2007) not only show that total municipal expenditure increases before elections, but also that its composition changes, favoring items that are highly visible. Likewise, several studies have been conducted on French municipal data. Binet and Pentecôte (2004) and Foucault et al. (2008) find that French local governments increase their spendings prior to municipal elections.

We can thus establish that the existence of political cycles relies on solid theoretical ground, and has been empirically found both at the national and the local levels: expenditure, tax and budget decisions of (local) governments are partly determined by their re-election prospectives. These economic variables are manipulated to convince the electorate prior either to national or to local elections. But the budget is not the only variable that governments are prone to influence. Hence, Mayer (1995) finds that before presidential elections, contract awards significantly increase in the United States. In France, Chong et al. (2014) assess that electoral considerations of mayors influence the timing of public procurement. Importantly,

Chong et al. (2014) find that electoral cycles are stronger for projects that are highly visible to the voters.

Local political cycles are then proved to impact several variables at the local level. The decisions of local governments in matter of budget, expenditure, investment, and even public contracting are affected by the proximity of elections. However, the impact of elections on the "life" of the contracts, and on the relationships between the contracting parties, has not been studied so far. Yet, if local authorities are prone to influence the voters before elections, it is reasonable to presume that they will use their public-private contracts to do so.

In this paper, we use renegotiations of public-private contracts to investigate whether local political cycles can impact the conduct of relationships between mayors and private operators. The literature on renegotiations suggests that the occurrence of amendments of public-private contracts could be impacted by elections, and the literature on political cycles suggests that when elections get close, politicians try to impact visible economic outcomes in order to influence the electorate. By contrast, we have no reason to think that the execution of contracts that are concluded between two private parties should be affected by the electoral calendar.

This literature review then suggests two propositions, that we propose to test empirically in this paper. Our first proposition is that public-private contract renegotiations should increase when elections get close, and our second proposition is that these renegotiations should relate to items that are visible to voters.

Proposition 1 *The number of public-private contract renegotiations should increase before local elections, while the number of private-private contract renegotiations shall not.*

Proposition 2 *The renegotiations of public-private contracts caused by the proximity of elections should relate to items that are visible to voters, in particular the end-user tariffs.*

3. SECTOR AND DATA

In order to test these two propositions, we have collected data about amendments of both public-private and private-private contracts in the French car park sector. As our concern is to compare public-private and private-private agreements, we will present in the following subsection the specificities of the public and the private sides of the sector. We will then give some details about the data in a second subsection.

3.1. THE FRENCH CAR PARK SECTOR

Publics car parks. In France and in most European countries, the public parking policy is a responsibility of local authorities. French jurisprudence considers parking as an *"industrial and commercial public service"*: it is under the responsibility of local governments to administer both off-street and on-street parking. Municipalities can choose either to directly manage this service, or to contract it out. Local governments can then conclude contracts with private operators for the construction and/or the exploitation phases, but also only for works or renovations. The contribution of the private sector is far from being anecdotal: since the first car park concession, which was awarded to a private company in 1962, the outsourcing of car park services has been continuously increasing. Between 1960 and 1980, thousands of car parks were constructed by private operators under public-private contracts which generally included the right of exploitation. In 2011, more than 70 % of French car parks were operated by semi-public companies or private operators.[3] Moreover, Baffray and Gattet (2009) describe the car park market as a mature and competitive one, which faces an increasing competitive pressure from both national and international companies.

Furthermore, car parks are highly visible infrastructures which partly determine the satisfaction of the electorate. First, it must be emphasized that an adequate parking policy has many valuable implications. A sufficient number of car parks, placed in judicious locations and with adapted pricing, spares drivers from cruising for parking spaces in crowded areas, which reduces traffic congestion. This does not only impact the ease of the drivers but also of all the citizens, since it reduces air pollution. Moreover, car parks also have economic externalities in the sense that they contribute to the development of commercial activities. In each city, car parks have to exist in sufficient number, be placed in judicious locations, present appropriate prices, but also have to be secure, clean, and accessible to all (in particular to disabled people). In other respects, the contribution of the parking sector to the economic and social development is undeniable. In 2010, it was estimated that the parking sector employed a total of 17,500 persons, creating revenues estimated at 1.3 billion euros.[4]

Reading the French press during municipal elections provides useful lessons about the drivers of the voters' satisfaction concerning car parks: the electorate asks for cheap parking.[5].

[3] In 2011, 55% of the car parks were operated by private operators and 18% by semi-public companies (data from the French national federation of parking activities (FNMS, "Fédération Nationale des Métiers de Stationnement")).
[4] Data from the FNMS.
[5] See for instance the press articles from the French daily newspapers "Libération" (February 25th, 2014) and "Le Figaro" (March 17th, 2014).

Despite the fact that it may not be optimal to maintain low prices,[6] the increase of press publications about the presumed high price of public parking in election periods is striking. We thus assume that the tariffs charged to end users will be an important variable in our analysis.

Since car parks are highly visible structures, which are under the responsibility of municipalities, and whose management directly impacts the satisfaction of voters, we believe that it is relevant to study car park contracts to test for the existence of political cycles upon renegotiations.

Private car parks. Private car park operators do not only conclude contracts with public authorities. They are also led to conclude agreements for similar services (*i.e.* construction, exploitation, or renovation) with private parties. These private parties can for instance be shopping centers, private pool complexes, amusement parks, and so forth. Private car parks share strong similarities with public car parks. In particular, it is worth noting that private car parks also constitute a strategic resource for private co-contractors as the number of parking spaces as well as their location, quality, and price will contribute to the satisfaction of customers. These similarities comfort us in the fact that public-private and private-private contracts are comparable and thus validate our approach which empirically compares of these two types of contracts.

3.2. THE DATA

Our analysis requires highly precise data about the renegotiations of car park contracts. This type of data is yet very difficult to collect. We thus take advantage of having access to every contract – and their amendments – signed by the French leader of the sector between 1968 and 2008. This private operator holds 42% of market shares among private operators, corresponding to 30.6% of total market shares. Moreover, our database has a considerable advantage: it is exhaustive. Thus, we study *every* contract renegotiation of this operator, referring to public-private as well as private-private contracts.

We exploit the opportunity of having an exhaustive database in our empirical methodology. We will indeed use a difference in difference (DiD) methodology and compare

[6] See Pierce (2013) for a discussion about the optimal pricing of car parks.

renegotiations of contracts signed with municipalities (treatment group) to renegotiations of contracts signed with private parties (control group).

Table 1: Types of contract and renegotiations

Co-contracting partner	Number of contracts	Number of contracts renegotiated at least once
Municipalities	557/676 (82,4%)	438/557 (78,6%)
Private	119/676 (17,6%)	88/119 (73,9%)

Table 1 presents some general descriptive statistics about our database (number of contracts of each type and number of contracts renegotiated at least once). It is important to note that we consider all public-private contracts signed with municipalities in our analysis. We do not distinguish between traditional procurement contracts and concession contracts, despite the fact that these types of contracts may present different characteristics.[7] Nonetheless, we will include fixed effects by contract later in our analysis to absorb the specificities of these different types of contracting. In addition, we will exclude concession contracts of the dataset as a robustness check. The following section will describe our dependent and independent variables.

3.2.1. Dependant variables

To test for the differentiated impact of local political cycles on public-private and private-private renegotiations, we constructed several renegotiation variables. Indeed, we do not only seek to show how the overall number of renegotiations of public-private contracts will change before elections compared to private-private renegotiations, but we also want to explore which contractual dimensions are more renegotiated. We thus studied the 1,110 amendments of our database to determine which dimensions of the contracts were renegotiated.

We distinguished between (i) renegotiations on the financial aspects of the contracts, (ii) renegotiations on construction or renovation work and (iii) other renegotiations. Financial renegotiations include amendments aiming at modifying the remuneration of one of the parties, amendments that modify the tariffs charged to end users, and changes of the duration of the contracts. Amendments are coded as work renegotiations as soon as additional work, which was not foreseen by the initial contract, is required. This work can consist in quality improvements (*e.g.* building an elevator shaft, building an access ramp for disabled, or the installation of new parking meters for on-street car parks), renovations (*e.g.* painting), or

[7] For instance, we expect public procurement contracts to be shorter and to relate to less complex transactions.

increases of the size of parks. Finally, other renegotiations concern changes of the name of our private operator.[8] We computed the number of each type of renegotiation per year and per contract. We also derived the total number of amendments per year and per contract; in our sample, contracts are renegotiated from 0 to 5 times per year.

It is essential to notice that it is not unusual for amendments to modify several dimensions of the initial contract at the same time. In other words, one unique amendment can include a "financial" renegotiation, as well as "work" and "other" modifications. Finally, our categories are exhaustive: all amendments were coded as one (or several) of the 3 renegotiation categories. Table 2 presents summary statistics on the total number of each type of renegotiation per year, for public-private and private-private contracts. These statistics show that public-private contracts are on average more frequently renegotiated than private-private contracts, and on all dimensions, which suggests a first visible difference between the two types of agreements.

Table 2: Types of Renegotiations per Year

	Public Contracts (N = 6672)				**Private Contracts** (N = 834)			
Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Tot_Reneg	0.193	0.471	0	5	0.107	0.317	0	2
Financial_Reneg	0.131	0.387	0	4	0.071	0.261	0	2
Work_Reneg	0.031	0.185	0	3	0.013	0.114	0	1
Other_Reneg	0.033	0.183	0	2	0.012	0.109	0	1

3.2.2. Independent variables

Political cycle variables. We now have to define the political cycle variables, in order to investigate the different impact elections can have on the two groups of contracts. Most of the studies on local political cycles define the pre-election period as the election year and the year before (Baleiras and da Silva Costa, 2004; Binet and Pentecôte, 2004; Chong et al., 2013). By contrast, we opt for pre-electoral periods of three years. The reason is that we coded the dates of amendments as the dates of signature. Nonetheless, the amendments most often do not apply immediately. To test for the impact of political cycles on renegotiations, it would then be more relevant to consider their implementation date. As this information is sometimes missing and quite difficult to extract, we chose to keep the date of signature in our analysis, and to include an extra year in our pre-electoral periods. Indeed, amendments concluded two

[8] Every time the name of the operator changes, an amendment called "changement de dénomination sociale" has to be drawn up.

years before local elections can be implemented just the year before elections in order to influence the voters' decisions.

Between 1968 and 2008, seven municipal elections took place in France. As all elections except one were held in March, we consider the "election years" to be the calendar years preceding the elections. The "pre-election years" are then defined as the calendar years before the "election years", etc. Table 3 sums up all the years of the dataset, considered as "pre" or "post" elections, knowing that elections took place in March 1971, 1977, 1983, 1989, June 1995, and March 2001 and 2008.[9]

Table 3: Election Cycles

Pre	y-2	1968	1974	1980	1986	1992	1998	2005
	y-1	1969	1975	1981	1987	1993	1999	2006
	y	1970	1976	1982	1988	1994	2000	2007
Post	y+1	1971	1977	1983	1989	1995	2001	2008
	y+2	1972	1978	1984	1990	1996	2002	-
	y+3	1973	1979	1985	1991	1997	2003	-
	y+4	-	-	-	-	-	2004	-

Control variables. Our empirical strategy includes four control variables. The first set of control variables is used to control for "contract cycles". The first variable, *Ct_Cycle* is defined as the ratio between the current year and the total duration of the contract. The second variable, *Ct_Cycle2* is the square of *Ct_Cycle*. These two variables allow us to control for the linear and non linear effects of the period of the contract life on renegotiations. Indeed, we expect contracts to be differently renegotiated at the beginning or at the end of their life. A second set of control variables is defined in order to test for *partisan* effects, *i.e*. the fact that the ideology of incumbent governments could impact the conduct of renegotiations. We thus define *Right* as a dummy variable which equals 1 if current mayor belongs to a right-wing party, and 0 otherwise. In addition, *Right* * *Public* is a crossed variable indicating whether the mayor is right-wing and the contract is a public-private agreement. This last variable will allow us to investigate whether right-wing parties are more prone to renegotiate public-private (compared to private-private) contracts. Table 4 presents some summary statistics for these control variables.

[9] The municipal elections of 2008 were initially programmed in 2007. However, in reason of an overloaded electoral calendar, this election was deferred in 2008. Therefore, we take 2004 as a post-electoral year of the 2001 election.

Table 4: Summary statistics for control variables

	Public Contracts (N = 6672)				Private Contracts (N = 834)			
Variable	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
Ct_Cycle	0.449	0.322	0.011	2	0.541	0.377	0.013	2
Ct_Cycle2	0.305	0.458	0	4	0.435	0.595	0	4
Right	0.618	0.486	0	1	0.415	0.493	0	1
Right * Pub	0.618	0.486	0	1	0	0	0	0

In the following, the empirical methodology and results will be exposed.

4. METHODOLOGY, RESULTS AND ROBUSTNESS CHECKS

4.1. EMPIRICAL METHODOLOGY

As discussed in the previous sections, we want to empirically investigate the inherent differences between public-private and private-private contracts, and we expect in particular these two types of agreements to be differently impacted by electoral cycles. We thus need to compare our two sets of contracts, and to determine whether the proximity of elections differently impacts the conduct of their renegotiations. To do so, we use a difference-in-difference (DiD) method where we compare public-private contracts (treatment group) to private-private contracts (control group) before and after municipal elections (treatment).

Despite the fact that public-private contracts apply in the area of administrative law whereas private-private contracts are governed by private law, we claim that our two groups are comparable, and especially as regards their executive phase and renegotiation process. We rely upon Seube (2006) who argues that regardless their legal regimes, these arrangements share the same notion of contract, and tend to adopt the same modification rules.[10] We thus consider the pre-election periods to estimate the following equation:

$$Type\_Reneg_{it}=\beta_1 Pre_t+\beta_2(Pre_t*Public_i)+Controls_{it}Z+\alpha_i+\gamma_t+\varepsilon_{it} \quad (1)$$

Where $Type\_Reneg_{it}$ is alternatively Tot_Reneg_{it}, $Financial\_Reneg_{it}$, $Work\_Reneg_{it}$, and $Other\_Reneg_{it}$, *i.e.* the number of *total*, *financial*, *work*, or *other* renegotiations that contract *i*

[10] The main difference between public-private and private-private renegotiation is the extent to which the initial contract can be modified. Public-private renegotiation is indeed allowed when it does not substantially modify the initial contract (see Beuve et al. (2015) for a discussion on public-private contract renegotiation).

occurred in year t.[11] Political cycles are identified by the variable Pre_t, which is a dummy that equals 1 for the three years preceding municipal elections. Our variable of interest is $Pre_t{*}Public_i$, which is the interaction term of Pre_t with a binary variable indicating whether the contract is a public-private contract (=1) or a private-private contract (=0). The coefficient β_2 will then indicate whether public-private contracts exhibit different renegotiation patterns (than private-private contracts) before local elections.

$Controls_{it}$ is a matrix of control variables related to contract i and to year t. This matrix includes four variables: *Ct_Cycle*, *Ct_Cycle2*, *Right*, and the interaction term (*Right * Public*). As described in previous section, the first two variables are meant to account for a potential renegotiation cycle in the life of contracts (*e.g.* there may be more renegotiations when reaching the end of a contract), while *Right* and the interaction term *Right * Public* are meant to determine whether there exists a partisan effect for renegotiations.

α_i corresponds to contract fixed effects. These fixed effects are used to absorb the specificities of each contract. For instance, the statistics on contract duration in Table 5 indicate that public-private contracts last on average longer (17.91 years) than private-private contracts (10.05 years). If these contracts differ on observable factors such as duration, they are also likely to differ on unobservable factors. Thus, to account for observed and unobserved heterogeneity between contracts and, in particular, between public-private and private-private contracts, we use contract fixed effects.[12] Moreover, because there may also be unobserved heterogeneity in time, we include the variable γ_t to our specification, which is a set of dummies identifying each political cycle. These variable are equal to one for the three years preceding and the three years following municipal elections (see Table 3). Finally, ε_{jt} is the error term.

[11] Note that as a first robustness check, we also include regressions of $Dum\_Tot\_Reneg_{it}$, $Dum\_Financial\_Reneg_{it}$, $Dum\_Work\_Reneg_{it}$, or $Dum\_Other\_Reneg_{it}$, dummy variables indicating whether or not *total*, *financial*, *work*, or *other* renegotiations occurred for contract i in year t.

[12] Note that these contract fixed effects absorb the dummy $Public_i$ that does not vary within contracts.

Table 5: Statistics for duration; with and without concession contracts

Type of contract	Nb Obs	Mean	Min.	Max.
Public-private	557	17.91	0	91
Private-private	119	10.05	0	78
Public-private (concessions excluded)	413	11.49	0	76
Private-private (concessions excluded)	118	10.08	0	78

Bertrand et al. (2004) argue that DiD estimations with a substantial number of years such as ours may be plagued with serially correlated outcomes and thus inconsistent standard errors. We report the results of Wooldridge (2002)'s test for serial correlation in our specifications. Two of Bertrand et al. (2004)'s suggested corrections are applied. First, all our regressions are computed using cluster robust standard errors at the contract level to allow for within contract error correlation and heteroscedasticity. Second, we collapse our data into a "pre" and "post" period and re-estimate equation 1.

This aggregation of our dataset was made in the following way. First, to avoid over (or under) representing any observation, we eliminated all years that could not be associated with two other years to form a complete pre- or post-election period. This resulted in eliminating the years 2004 (because of the 7 years between the 2001 and 2008 elections) and 2008 (because we lack data from 2009 and 2010 to form a complete "post" period for the 2008 election).[13] Second, for each contract, we averaged all variables over one "pre" and one "post" period.[14] Aggregating our data in such a way has a second advantage as we can now consider "sub-sub" categories of renegotiations. In other words, rather than limiting our disaggregation of data to financial renegotiations, we can now distinguish between *Tariff_Reneg* and *Remun_Reneg*. *Tariff_Reneg* represents the mean number of renegotiations on the tariffs charged to end users, and *Remun_Reneg* compiles all other financial renegotiations (*i.e.* renegotiations aiming at modifying the remuneration of one of the parties or the duration of the contract). These sub-categories could not been considered at the disaggregated level because of their relative rare occurrence. The descriptive statistics for this aggregated dataset are displayed in Table 6.

[13] See Table 3.
[14] This type of aggregation has a drawback. Indeed, whether the initial contract covered one or more "pre" (or "post") periods, the aggregation always yields one "pre" (and one "post") period. This is likely to put relatively more weight on observations from contracts with few periods. To avoid this bias, our regressions will be weighted by the number of "pre" and "post" periods covered by the contract in our initial dataset.

Table 6: Summary statistics, aggregated

Variable	Mean	Std. Dev.	Min.	Max.	N
Pre	0.562	0.496	0	1	1052
Public	0.835	0.372	0	1	1052
Pre * Public	0.466	0.499	0	1	1052
Tot_Reneg	0.182	0.287	0	2.333	1052
Financial_Reneg	0.124	0.236	0	2	1052
Work_Reneg	0.057	0.141	0	1	1052
Other_Reneg	0.033	0.112	0	1	1052
Types of *Financial_Reneg*:					
Remun_Reneg	0.089	0.204	0	2	1052
Tariff_Reneg	0.052	0.129	0	1.111	1052

Our hypothesis of strategic manipulation of public contracts before elections leads us to expect an increase in "visible" renegotiations before elections. That is, compared with private-private contracts, public-private contracts are suspected to be significantly more renegotiated in the years preceding municipal elections (**Proposition 1**), and on aspects that will be visible to voters (**Proposition 2**). We thus expect a positive and significant coefficient associated with our interaction term *Pre*Public* for *financial* renegotiations, since this category includes renegotiations of end-user tariffs. Moreover, we suppose that the total number of renegotiations of public-private contracts will also increase before elections. Indeed, *work* and *other* renegotiations are not expected to exhibit particular renegotiation patterns before elections for public-private contracts, and Table 2 shows that about 67% of renegotiations relate to *financial* ones. The coefficient associated with *Pre*Public* for the total number of renegotiations should then also be positive. Finally, it is important to note that since *other* renegotiations are not visible at all by the electorate, these latter could not be subject to political manipulations before elections. Our regressions on the number of *other* renegotiations may then be seen as placebo tests, where significant coefficients associated with *Pre*Public* would shed doubt on our empirical strategy. Table 7 summarizes our expected results.

Table 7: Expected impact of *Pre*Public*

	Tot_Reneg	Financial_Reneg	Work_Reneg	Other_Reneg
Expected impact	+	+	0	0

4.2. RESULTS AND ROBUSTNESS CHECKS

The OLS regressions on our initial sample are shown in the first four models of Table 8. In line with our predictions, we find a positive and significant coefficient associated with the interaction term *Pre*Public* when considering the number of financial renegotiations (Model 2). As expected, this leads to an increase in the total number of renegotiations: the coefficient of *Pre*Public* is also positive and significant for Model 1. In other words, public-private contracts are significantly more renegotiated than private-private contracts overall and on financial aspects in pre-election periods, compared with post-election years. The coefficient associated with our interaction term is found not to be statistically different from zero for the number of work renegotiations (Model 3) and our placebo test shown in Model 4 holds, yielding support to our empirical methodology. Finally, we find some differences in renegotiation patterns in right wing municipalities, yet these patterns are not found to be

Table 8: Regressions of the number of renegotiations, non aggregated

	Model 1 Tot_Reneg OLS FE	Model 2 Financial_Reneg OLS FE	Model 3 Work_Reneg OLS FE	Model 4 Other_Reneg OLS FE	Model 5 Dum_Tot_Reneg LPM FE	Model 6 Dum_Financial_Reneg LPM FE	Model 7 Dum_Work_Reneg LPM FE	Model 8 Dum_Other_Reneg LPM FE
Pre	-0.043	-0.033	-0.003	-0.001	-0.044*	-0.030	-0.005	-0.002
	(0.027)	(0.021)	(0.019)	(0.009)	(0.026)	(0.020)	(0.018)	(0.009)
Pre * Public	0.077***	0.066***	0.014	0.002	0.069**	0.059***	0.013	0.000
	(0.028)	(0.022)	(0.020)	(0.010)	(0.027)	(0.021)	(0.020)	(0.010)
Right	0.061*	0.052*	0.011	-0.016*	0.058*	0.043	0.013	-0.015*
	(0.034)	(0.028)	(0.010)	(0.009)	(0.032)	(0.027)	(0.010)	(0.009)
Right * Public	-0.036	-0.032	0.007	0.014	-0.038	-0.025	0.005	0.014
	(0.037)	(0.030)	(0.013)	(0.010)	(0.034)	(0.029)	(0.012)	(0.010)
Ct_Cycle	0.030	0.103	-0.028	-0.042	-0.017	0.056	-0.021	-0.041
	(0.096)	(0.081)	(0.043)	(0.030)	(0.068)	(0.057)	(0.040)	(0.029)
Ct_Cycle2	0.044	0.013	0.024	0.018	0.060*	0.031	0.015	0.018
	(0.053)	(0.047)	(0.021)	(0.016)	(0.036)	(0.030)	(0.019)	(0.015)
Constant	0.211***	0.114***	0.095***	0.037***	0.183***	0.100***	0.086***	0.037***
	(0.042)	(0.032)	(0.022)	(0.013)	(0.029)	(0.024)	(0.020)	(0.013)
Nb Obs	7506	7506	7506	7506	7506	7506	7506	7506
Adj. R2	0.125	0.108	0.070	0.029	0.108	0.095	0.062	0.028
AC tests	0.895	0.021	0.989	0.357	0.392	0.008	0.651	0.320

Note: Cluster robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. UndeAC tests, we report the p-values of the Wooldridge tests for autocorrelation in panel data (H0: no first-order autocorrelation).

different in public-private contracts. As a robustness check, we reported in Models 5 to 8 regressions of dummies indicating whether a type of renegotiation occurred in the period (=1) or not (=0), using a linear probability model. These results are qualitatively similar to that of Models 1 to 4. We also reported the p-values of Wooldridge (2010)'s test for serial correlation in the table. As feared, the outcomes of the regressions appear to be serially correlated (see Models 2 and 6). This justifies aggregating our data into one "pre" and one "post" period.

We report the regressions using our aggregated sample, which was constructed by averaging all variables into one "pre" and one "post" period for each contract, in Table 9.[15] Our results are qualitatively similar to that of the previous table. Indeed, we find that compared with post-election periods, public-private contracts are significantly more renegotiated than private-private contracts in pre-election periods, overall and when looking only at financial renegotiations. Again, our placebo test displayed in Model 4 holds. As discussed in the previous subsection, this level of aggregation enables us to further investigate this result by looking at sub-types of financial renegotiations. The results from these regressions are reported in Table 10. We find positive and significant coefficients for the interaction terms when analyzing the two sub-types of financial renegotiations, indicating that both the remuneration of the parties and end user tariffs are statistically more renegotiated in public-private contracts in pre-election periods.

Table 9: Regressions of the mean number of renegotiations, aggregated

	Model 1 Tot_Reneg OLS FE	Model 2 Financial_Reneg OLS FE	Model 3 Work_Reneg OLS FE	Model 4 Other_Reneg OLS FE
Pre	-0.028	-0.022	-0.008	-0.003
	(0.034)	(0.023)	(0.021)	(0.016)
Pre * Public	0.063*	0.058**	0.017	0.001
	(0.037)	(0.026)	(0.023)	(0.017)
Constant	0.165***	0.104***	0.059***	0.033***
	(0.007)	(0.006)	(0.004)	(0.003)
Nb Obs	2508	2508	2508	2508
Adj. R2	0.709	0.686	0.620	0.545

Note: Cluster robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Our regressions are weighted by the number of election periods represented by each observation.

Table 10: Regressions of the types of financial renegotiations, aggregated

	Model 1 Remun_Reneg OLS FE	Model 2 Tariff_Reneg OLS FE
Pre	-0.025	-0.008
	(0.024)	(0.010)
Pre * Public	0.051**	0.030**
	(0.026)	(0.013)
Constant	0.075***	0.046***
	(0.005)	(0.004)
Nb Obs	2508	2508
Adj. R2	0.660	0.675

Note: Cluster robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Our regressions are weighted by the number of election periods represented by each observation.

[15] Note that these regressions do not include the set of control variables as these latter lose their meaning at this aggregated level.

A common robustness check after performing a DiD is to focus on the common trend assumption. This assumption postulates that after controlling for covariates included in equation (1), no other force should differently affect our control and treatment groups in pre- and post-treatment periods. Here, we believe that including both contract fixed effects and time dummies helps satisfying this assumption by removing the effects of time invariant and time variant characteristics. In addition, the fact that our placebo test always holds is reassuring. Nonetheless, we conducted two additional robustness checks.[16] First, we performed placebo elections in t-2 and t+2.[17] In all our specifications, we find that the coefficients associated with the interaction term are not statistically different from zero. These placebo tests thus lend further credibility to our fulfillment of the common trend assumption.

As a second robustness check, we ran the initial regressions on a restricted dataset which excludes concession contracts. Indeed, as argued before, the DiD methodology relies on the assumption that the treatment and control groups share a common trend. A mean to meet this assumption could then be to assure that the contracts of our two groups are similar *ex ante*. As concession contracts are over represented among public-private contracts (they represent 144 public-private contracts and 1 private-private contract), we decide to exclude them from the analysis. This could help satisfying the common trend assumption as concession contracts last on average longer and relate to more complex transactions. Table 5 supports this argument as when we exclude concessions, the average duration of public-private (11.49 years) and private-private (10.08 years) agreements are much more comparable. A Student test confirms that these average durations are not statistically different. The results are again similar than the ones of Table 9, except that the coefficient associated with the crossed variable *Pre*Public* is not significant anymore for the total number of renegotiations. This result confers additional support to our **Proposition 2**. Indeed, even when the increase of renegotiations is not observable on the total number of renegotiations, visible renegotiations (*i.e. financial* ones) still increase in pre-election periods for public-private contracts.

As argued in section 3.2.2, because we use the signature dates instead of the application dates of amendments, we rely on a three year pre-electoral period (y-2, y-1 and y) where most previous studies on local political cycles opt for a two year period (y-1 and y). To show that

[16] To save space, the results of these robustness checks are not included in this document, but are available upon request to the authors.
[17] Since we perform these robustness check on an average number of renegotiations over three years (for our pre and post periods), delaying the elections by a sole year may not be enough to perform a satisfying robustness check. We thus choose a delay of two years.

our results are in line with these studies and to confer more credibility to the fact that our results attest of a political cycle, we now consider a two year pre-electoral period. Yet, to take into account the delay between signature dates and application dates, we define our pre-electoral period as years y-2 and y-1. Again, the results are qualitatively similar to that of Table 9.

5. CONCLUDING REMARKS

Our results show that elections differently affect public-private and private-private relationships. Indeed, public-private contracts are statistically more renegotiated than private-private contracts in pre-election periods compared with other periods. Among the dimensions that can be renegotiated, financial renegotiations appear to be the main drivers of this difference. Moreover, our investigation of a potential partisan effect in the conduct of renegotiations in public-private contracts did not uncover significant differences between right and left wing municipalities.

We believe that our results show that having a public entity among the contracting parties changes the contractual relationship. Indeed, politicians are subject to elections and may be looking to enhance their electoral perspectives. Public-private contracts thus differ from private-private contracts as the former are subject to a political cycle. Private managers taking part in public-private arrangements should approach them differently from private-private contracting by anticipating that pre-electoral periods are likely to be subject to opportunistic changes by the public side. Similarly, public managers engaging in public-private contracting should also approach pre-election periods with caution as private parties also have an incentive to take advantage of the elections to propose opportunistic changes in the contractual relationship. As suggested by the literature, the parties should anticipate renegotiations at the contracting stage. This may then be particularly relevant for these renegotiations initiated by the proximity of elections.

As of today, we are unable to assess whether these opportunistic renegotiations negatively impact social surplus and/or the quality of the relationship between the parties. Our future research will try to tackle these issues. Furthermore, our work is among the first empirical studies to consider the differences between public-public and public-private contracting. As argued by spiller2008institutional, there remains a great number of studies to be conducted to grasp the extent of the singularity of public contracting. Our future research will also try to tackle these issues.

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